Barro, Grossman, and the domination of equilibrium macroeconomics

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15 April 2021

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Abstract

Under which conditions did Robert Lucas's microfoundational program come to dominate the field? My article sheds new light on this question. The focus is on why models incorporating rational expectations and market-clearing seduced macroeconomists. My case study is Robert Barro and Herschel Grossman. Drawing on Grossman's archives, I define a framework for explaining their modeling choices. I show that methodological principles, tractability constraints, and research strategies explained why, at the end of the 1970s, Barro and Grossman preferred equilibrium over disequilibrium macroeconomics.

JEL Codes: B21, B22, B23, E1, E3.

Keywords: fluctuations, non-neutrality of money, fixed-price equilibrium models, contract theory, disequilibrium macroeconomics, equilibrium macroeconomics.

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I would like to thank Alain Béraud, Michel de Vroey, Paul Dudenhefer, Kevin Hoover, Jan Horst Keppler, Edward Nelson, Goulven Rubin, Francesco Sergi, and Roy Weintraub for their comments on earlier drafts of this essay. I am also indebted to the participants of the HOPE seminar of Duke University, to the participants of the 2018 Summer Institute on the History of Economics, and to the participants of the nHice workshop. My final thanks go to the staff of the John Hay Library (Brown University) for their help with the Herschel Grossman Papers.

Introduction

In preparation for the 1978 meeting of the American Economic Association (AEA), Robert Solow contacted Herschel Grossman to organize "a session on non-market-clearing macroeconomics."[2] Grossman accepted while indicating a problem. American economists had lost interest in the macroeconomics initiated by Don Patinkin (1956), Robert Clower (1965), and Axel Leijonhufvud (1968). According to Grossman:

> A good sampling of recent work along these lines includes the [fixed-price equilibrium models of] Bénassy, Grandmont, Peisa, Siven, and Varian in the latest issue of the *Scandinavian Journal of Economics*, No. 2/1977 […] The authors doing this work are European.[3]

Europe was the place where the development of disequilibrium macroeconomics took place. However, it did not last. Just like their U.S. counterparts, European macroeconomists eventually lost interest in models incorporating rationing constraints on markets.

Almost simultaneously, on both sides of the Atlantic, there was a growing interest in Robert Lucas's macroeconomics (1972, 1975). It was particularly rapid and strong in the U.S. Whether the goal was to study the effectiveness of monetary policy, fluctuations, or economic growth, more and more macroeconomists used models incorporating rational expectations and market-clearing (e.g., Sargent and Wallace, 1975; Barro, 1976; Kydland and Prescott, 1982). In less than 15 years, notably under the influence of Real Business Cycle theory, rational expectations and market-clearing became widely accepted postulates.[4] Under which conditions did this modeling practice come to dominate the field? My article sheds new light on this question. The focus is on why Lucas's macroeconomics seduced macroeconomists. My case study is Barro and Grossman.

Until the mid-1970s, Barro and Grossman contributed to the development of disequilibrium macroeconomics. In 1971, they elaborated the seminal fixed-price equilibrium model. In 1976, they wrote the first book on disequilibrium macroeconomics, *Money, Employment, and Inflation*. However, at the end of the 1970s, Barro and Grossman advocated

[2] Excerpt from Solow's letter, sent on 19 October 1977. Grossman's papers, Box 3 OF-IUF-G5, John Hay Library Special Collections. Thereafter, I will use the following code: GP X OF-IUF-G5, with X the box number.
[3] Letter from Grossman to Solow, 31 October 1977, Box 3 OF-IUF-G5. Note that Hal Varian was American.
[4] The same applies to the representative agent. Kevin Hoover (2012) explained why, by tracing the history of the microfoundations of macroeconomics.

for models incorporating rational expectations and market-clearing. It was the highlight of the AEA session, "Macroeconomics: An Appraisal of the Non-Market Clearing Paradigm" (Chicago, 08/30/1978).[5] Barro (1979: pp. 55-56) and Grossman (1979a: p. 68) claimed that Lucas had identified the good approach to macroeconomics. Why did they prefer equilibrium over disequilibrium macroeconomics?

According to Roger Backhouse and Mauro Boianovsky (2013), Barro and Grossman deplored that market price stickiness, and the resulting disequilibria on markets, did not result from optimizing behavior. Moreover, while working on contract theory, Barro and Grossman concluded that market-clearing was consistent with microeconomics and a good approximation of reality. Therefore, they eventually preferred equilibrium over disequilibrium macroeconomics (2013: p. 101). Kevin Hoover (2012) agrees with Backhouse and Boianovsky's interpretation (p. 38).[6] However, he adds "a more important reason" for the "disappearance of general disequilibrium microfoundations" (p. 38). According to Hoover, fixed-price equilibrium models *à la* Barro and Grossman (1971) "were built to display certain principles." They did not "have any claim on the real world" (p. 38). Therefore, unlike equilibrium models, they offered "a poor basis for econometrics" (p. 39). Finally, Michel de Vroey (2016) considers that Barro and Grossman defended equilibrium macroeconomics on rigorous grounds. On one side, like Backhouse and Boianovsky, De Vroey mentions the problem with the exogenous restrictions on transactions (2016: p. 140). On the other side, he explains that there is no room for involuntary unemployment in Walrasian theory and that Lucas's macroeconomics "allowed engaging in dynamic analysis in a more serious way than before, while [fixed-price] equilibrium models remained basically static" (p. 141).

Without focusing on Barro's and Grossman's cases, macroeconomists have also attempted to explain equilibrium macroeconomics' success. Whether the development of disequilibrium models is considered (e.g., Blanchard, 2000; Mankiw, 1990) or not (e.g., Blanchard, 2003; Phelps, 1990; Woodford, 1999), the story is always the same. "New Classical Economists" would have triumphed over "Keynesians" because they would have offered

[5] In a letter sent to Grossman on 3 November 1977, Solow claimed: "I feel obliged to appear as a discussant and defend the Barro-Grossman tradition against these turn-coats. It sure would be a switch. Terrific" (Box 3 OF-IUF-G5). The session eventually included the presentation of three papers, later published under the titles "Second Thoughts on Keynesian Economics" (Barro, 1979), "Why Does Aggregate Demand Fluctuates" (Grossman, 1979a), and "Evaluating the Non-Market-Clearing Approach" (Howitt, 1979). Joseph Ostroy and Solow were the discussants.

[6] Hoover referred to two unpublished articles, written in 2005, and that Backhouse and Boianovsky later used to write *Transforming Modern Macroeconomics: Exploring Disequilibrium Microfoundations* (2013).

models based on sound foundations, and able to explain stagflation.[7] Macroeconomists and historians, therefore, agree on the importance of rigor. For instance, Blanchard (2000) and Mankiw (1990) also consider that fixed-price equilibrium models were abandoned because market price stickiness reflected no one's optimal behavior. However, in their view, the failure to explain stagflation was another reason why Lucas beat the competition.

The problem is that every explanation is, to varying degrees, hard to reconcile with Barro and Grossman's positions.[8] In 1974, Grossman claimed that fixed-price equilibrium models could explain the simultaneous increase in U.S. inflation and unemployment rates over 1969-1973. The conditions were to address the dynamic of market prices and incorporate adaptative expectations (1974: p. 404). Then, following the same logic, Barro and Grossman explained stagflation (1976: pp. 204-210). Lastly, and more generally, we will see that according to Barro and Grossman, market price stickiness was instrumental in actual fluctuations. Therefore, it is hard to believe that their problem with disequilibrium macroeconomics concerned the relationship between theory and facts.

On the other hand, in "A General Disequilibrium Model of Income and Employment" (1971), Barro and Grossman explained:

> The inability of a firm to sell its desired output at the going price violates an assumption of the perfectly competitive model. Kenneth Arrow [1959] has stressed this inconsistency of perfect competition with disequilibrium. Essentially, he argues that economic units which act as perfect competitors in equilibrium must (at least in certain respects) perform as monopolists in disequilibrium. In this paper, we focus on the reaction of economic units to given (equilibrium or disequilibrium) price levels. If, in addition, one wished to analyze explicitly the dynamics of price adjustments, it would be necessary to

[7] For a systematic criticism of the school-of-thought framework, see Duarte (2012).

[8] When developing fixed-price equilibrium models, an allegedly "Keynesian" framework, Barro and Grossman explained that "the cases of general excess supply and general excess demand [arose] directly as a consequence of inappropriate monetary and fiscal policies" (1976: p. 40). Moreover, Barro and Grossman supported the implementation of policy rules. For instance, during a conference "On the Stability of Contemporary Economic System" (1975), Grossman claimed that "if we could somehow devise a system where monetary creation [was] stabilized, we could then have a stabilized rate of inflation […] The immediate policy prescription [was] to remove monetary and fiscal policy from the discretion of government and to transfer the power to some highly level of constitutional authority" (1975: p. 457). Therefore, neither the "Keynesian" nor the "New Classical" label fit Barro and Grossman's works. Instead of using such a categorization, the present article focuses on how Barro and Grossman built and assessed models.

> discard the perfectly competitive paradigm of the producer as a price taker (1971: p.85).

Thus, as early as 1971, Barro and Grossman acknowledged that a price theory was missing from their analysis. If this lack of rigor was such a problem, why did they continue to develop the same type of model (1974; 1976)? In particular, why did they not study the dynamic of non-clearing markets in an imperfect competition framework? The following analysis will show that what Barro and Grossman considered as acceptable theorizing or not mattered here.

It will also nuance the view that contract theory led Barro and Grossman to justify Lucas's macroeconomics. Their work on contract theory did matter, but more because it contributed to disqualify disequilibrium macroeconomics. All this suggests that rigor and realism criteria are not enough to explain Barro's and Grossman's modeling choices. Drawing on Grossman's archives, I offer a different framework. I show that methodological principles, tractability constraints, and research strategies explained why, at the end of the 1970s, Barro and Grossman preferred equilibrium over disequilibrium macroeconomics.

1.First round (1971-1976)

In the early 1970s, economists did not necessarily distinguish between disequilibrium and equilibrium macroeconomics. Edmund Phelps was a case in point (Backhouse and Boianovsky, 2013: p. 91). In *Microeconomic Foundations of Employment and Inflation*, the seminal book on equilibrium macroeconomics, Phelps welcomed the formation of "an economics of disequilibrium" (1970: p. vii). What about Barro and Grossman?[9]

1.1 Disequilibrium vs. Equilibrium macroeconomics

In October 1971, Grossman engaged with Leijonhufvud about *On Keynesian Economics and the Economics of Keynes* (1968). The focus was on the microfoundations of disequilibrium macroeconomics. To explain involuntary unemployment, Leijonhufvud (1968) proposed to use search theory. He relied in particular on Armen Alchian's.[10] Even if Alchian addressed

[9] Backhouse and Boianovsky considered that until at least the mid-1970s, Barro did not establish a competition between disequilibrium and equilibrium macroeconomics. According to Backhouse and Boianovsky, "the appearance of this work [on equilibrium macroeconomics] alongside his continuing work on disequilibrium macroeconomics with Grossman suggests that [Barro] was simply using different models for different problems" (2013: p. 101).

[10] While writing *On Keynesian Economics and the Economics of Keynes*, Leijonhufvud had access to "Unemployment and the Cost of Information," an unpublished paper written by Alchian (1968: p. 69). Alchian later published a revised version of this paper in *Microeconomic Foundations of Employment and Inflation* (1970).

"frictional unemployment," Leijonhufvud was convinced that his "analysis [was] applicable to the explanation of individual behavior in a state of 'involuntary' unemployment" (1968: p. 81). In both cases, workers would have to look for jobs without knowing the equilibrium wage level.[11] Then, Leijonhufvud considered that "the initial 'inflexibility' of reservation prices that [Alchian's] analysis [implied was] a necessary condition for the emergence of [involuntary unemployment]" (1968: p. 81). Leijonhufvud concluded that an incomplete information framework was necessary to explain how non-clearing markets worked. Grossman disagreed. In a letter sent on 15 October 1971, he indicated to Leijonhufvud:

> I am currently working on a paper which contrasts what I take to be two alternative approaches to employment theory – the 'inelastic-expectations' approach of Alchian, Phelps, et. al., and the 'market-failure' approach of Clower, Barro and myself, et. al. In this paper, I disagree with your suggestion that the Alchian and Clower analyses are logically complementary.[12]

Grossman's paper was "Aggregate Demand, Job Search, and Employment" (1973). It is helpful to clarify the disagreements between Leijonhufvud and Grossman. Grossman (1973) indicated that individuals had perfect information about market prices in "A General Disequilibrium Model of Income and Employment" (1971). An incomplete information framework was therefore not necessary to explain involuntary unemployment. Starting from Walrasian theory, what was necessary was to reject the tâtonnement hypothesis (1973: p. 1356; p. 1368). Then, Grossman indicated that labor income was not an argument in Alchian's (1970) or Phelps' (1970) frameworks (1973: p. 1362). But it was in Clower's (1965) or Barro and Grossman's (1971). Grossman concluded that Phelps et al.'s (1970) microeconomics was incompatible with disequilibrium macroeconomics (1973: p. 1362).

According to Grossman, this incompatibility was due to the assumptions underlying the determination of prices and quantities. In Phelps et al.'s (1970) macroeconomics, the "levels of wages, prices, employment, and production [were supposed to] always satisfy the conditions for general-market clearing" (1973: p. 1368). It explained why "household simultaneously

[11] Leijonhufvud considered an economy without Walrasian auctioneer. When "this hypothesis is relinquished, the generation of the *information* needed to coordinate economic activities in large systems where decision-making is decentralized is seen to take time and to involve costs. Alchian has shown that the emergence of unemployed resources is a predictable consequence of a decline in demand when traders do not have perfect information on what the new market-clearing price would be. No other assumption, we argue, need to be relinquished in order to get from the Classical to Keynes' Theory of Markets" (1968: p. 38).

[12] Box 2 OF-IUF-G5.

[chose] both the quantity of employment to accept thereby determining its own income, and the quantity of consumables to buy" (1973: p. 1362). However, in the macroeconomics developed by Clower (1965) or Barro and Grossman (1971), labor income imposed a constraint on the demand for goods since economic activities occurred under non-market clearing conditions (1973: p. 1368). As a result, in the early 1970s, Grossman already distinguished between disequilibrium and equilibrium macroeconomics.

This distinction can also be found in *Money, Employment, and Inflation* (1976). Barro and Grossman (1976) developed two frameworks for analyzing the determination of output and employment. The first one, associated with the disequilibrium program of microfoundations, was explored from chapters 2 to 6. The second one, associated with the incomplete information program of microfoundations, was discussed in chapter 7.[13] Barro and Grossman explained:

> Chapter 2 departs from the Walrasian framework to consider output and employment under non-market-clearing conditions. The crucial assumption is that wages and prices respond sluggishly to shift in demand […] Chapter 7 returns to the basic model of chapter 1 and considers an alternative departure from the Walrasian framework – namely that economic units have incomplete information regarding the spatial distribution of wages and prices (1976: p. 4).

There was a common analytical structure to all chapters of *Money, Employment, and Inflation* (1976). It was a perfect competition model where firms, households, and a government interacted through two markets.[14] The labor market, where labor services were exchanged against money, and the market for goods, where consumable commodities and public services were exchanged against money. In this context, the government had to collect tax or to supply money balances to offer public services; firms demanded labor and supplied both consumable commodities and public services to maximize profits; and households aimed to maximize their utility by choosing the quantity of goods to demand, the quantity of labor to supply, and the quantity of real balance to transfer from one period to another. In chapter 1, these economic

[13] All the results presented in chapter 7 were already formulated by Grossman in "Aggregate Demand, Job Search, and Employment" (1973). However, his model was different. Grossman (1973) assumed that firms set wages and prices. In *Money, Employment, and Inflation*, market prices were parametric (1976: p. 239).

[14] Firms' and households' behaviors were analyzed through representative units: "when analyzing the behavior of firms, working households, and retired households, we consider the 'representative' unit; that is, a unit whose behavior, expect for its atomistic scale, is identical to the behavior of the aggregate of such units. The representative unit is essentially an average unit. Consequently, we are able to move freely between the individual and aggregate, and we use the same notation to represent both" (1976: p. 9).

units evolved in a "frictionless system of markets." They had perfect information about market prices, and the "privilege of recontracting" ensured that exchange took place only under market-clearing conditions (1976: p. 31). From chapters 2 to 6, Barro and Grossman developed a general equilibrium model where, by assumption, prices and wages responded sluggishly to discrepancies between supply and demand. It followed that economic activities took place under non-market-clearing conditions. By contrast, disequilibrium transactions were excluded in the general equilibrium model presented in chapter 7 (1976: p. 238). This model departed from the Walrasian framework because individuals did not have perfect information market prices' spatial distribution (1976: p. 239).

Under non-market-clearing conditions, individuals no longer behaved as if they could buy and sell as much as they wanted given market prices: "the failure of a market to clear [implied] that actual quantities transacted [diverged] from the quantity supplied or from the quantities demanded. From the standpoint of the individual, these divergences [appeared] as constraints, to be taken into account when formulating behavior in other markets" (1976: p. 40). To model the behavior of firms under quantity constraints, Barro and Grossman rested on Patinkin's ([1956] 1965) spill-over effect (1976: p. 43). They explained that in situations of excess supply in the market for goods, firms would reduce their demand for labor services by considering the constraints on their sales (1976: p. 42). The same logic applied to situations of excess demand in the labor market. Firms would reduce their output by considering the constraint on their purchases of labor services (1976: p. 69). In parallel, Barro and Grossman used Clower's (1965) dual-decision hypothesis to model how households behaved out of equilibrium (1976: p. 50). In situations of excess supply in the labor market, workers would reduce their demand for goods and their demand for money balances by considering the constraints on their labor income (1976: p. 50). However, in situations of excess demand in the market for goods, they would reduce their supply of labor services and increase their demand for money balances (forced saving) by considering the constraints on their purchases of goods (1976: pp. 70-71).

When revising their plans, individuals expressed "effective" supplies and "effective" demands. These functions provided the basis for explaining the determination of output and employment and the change in market prices. Barro and Grossman assumed that "actual transactions [equaled] the smaller of the quantities supplied and demanded" (1976: p. 40). To be more specific, in situations of general excess supply, "effective demands for labor services and commodities [determined] both employment and output" (1976: p. 55). By contrast, the

level of economic activity was determined by "effective supplies of commodities and labor services" in situations of general excess demand (1976: p. 79). After transactions were completed, market prices varied according to effective excess demands (1976: p. 95). For instance, prices and wages decreased when the effective supply was higher than the effective demand in the labor market and the market for goods. Otherwise, prices and wages increased.

In Chapter 7, Barro and Grossman "[considered] a framework in which both the labor market and the commodity market [involved] a large number of spatially distinct marketplaces" (1976: p. 238). They also assumed a random distribution of individuals across markets (1976: p. 238). Individuals could therefore pay and receive different wages and prices for the same labor services and commodities. It followed speculation on the distribution of market prices over space.

When facing a relative price different from their estimation of the mean rate of exchange, the change in individuals' estimation was less than proportionate (1976: p. 240). Then and more importantly, individuals completed transactions only when the actual rate of exchange was better than the estimation of the mean rate of exchange. For instance, a "household [accepted] an actual wage offer which [was] high relative to its subjective estimate of the mean wage rate" (1976: p. 240). Otherwise, it refused employment and engaged in other job search (1976: p. 241). The same logic applied when speculative behavior entered into the determination of consumption demand, labor demand, and output supply. Output and employment were determined when the commodity and labor markets cleared.

1.2 Two competing approaches to economic fluctuations

Equilibrium and disequilibrium macroeconomics competed in *Money, Employment, and Inflation* (1976).[15] The most obvious proof is in Chapter 7. Barro and Grossman (1976) claimed:

> At the prevailing wages, laid-off workers would prefer to be employed, but they are forced off their employment-acceptance schedule by a deficiency of demand. This nonwage rationing of jobs impose a constraint on the household choice problem. This constraint does not arise if employment acceptances govern employment, but becomes a central consideration if labor markets fail to clear. On this score, the analysis of exchange under non-market-clearing conditions

[15] See also "Aggregate Demand, Job Search, and Employment" (1973).

> seems more satisfactory than does the model of speculative household behavior (1976: p. 250).

The challenge is to determine what was the competition's goal, and what were the criteria for determining the winner.

The competition's goal is clarified by how Barro and Grossman (1976) compared the two models. In Chapter 7, they assessed their capacity to match four stylized facts. The first one was the non-neutrality of money. According to Barro and Grossman, a change in the money supply affected production and employment levels (1976: p. 248). The second fact concerned unemployment. While referring to a statistical study from François Sellier and Claude Zarka (1966), Barro and Grossman indicated that "firms rarely cut wages and [induced] workers to leave voluntarily […] Layoffs [accounted] for about two-thirds of total separation in industrialized western countries" (1976: p. 249). The third fact concerned the evolution of the real wage. Barro and Grossman relied on Edwin Kuh's (1966) and Ronald Bodkin's (1969) statistical studies to stress "the absence of any consistent cyclical movement of W/P" (1976: p. 250). Finally, Barro and Grossman indicated that consumption moved procyclically (1976: p. 252).

According to Barro and Grossman, it was possible to assess the role played by the sluggishness of market prices and by imperfect information in fluctuations. The less a model matched the facts mentioned above, and the less the associated friction explained actual fluctuations. This logic appeared when Barro and Grossman studied the implications of the incomplete information model:

> Speculative household behavior probably plays a relatively small role in the determination of the actual cyclical behavior of output and employment. This argument is based on the observation that certain qualitative implications of speculative behavior are difficult to reconcile with actual experience (1976: p. 245).

Therefore, Barro and Grossman's goal was to determine which framework was the most appropriate for explaining fluctuations.[16]

[16] Barro and Grossman's concerns for economic fluctuations can already be found in "A General Disequilibrium Model of Income and Employment" (1971: pp.83-84, p. 87).

There were two evaluation criteria. On one side, there was a realism criterion. It consisted in determining which model was the most consistent with the stylized facts aforementioned. On the other side, there was a rigor criterion. It consisted in assessing the level of "rationalization" offered in each framework – i.e., to what extent the functioning of the model could be explained "in terms of the motivation of the individuals involved and the constraints which they [faced]" (1976: p.1). In the disequilibrium model, the focus was on the capacity to "rationalize" the introduction of quantities into the supply and demand functions. Were the sluggish variations in market prices a result of optimizing behavior or an "ad-hoc" assumption (1976: p. 6)? In the equilibrium model, however, the focus was on the capacity to "rationalize" the introduction of subjective estimates of wages and prices into the supply and demand functions (1976: p. 239). What was the choice-theoretic basis for search activities (1976: pp. 240-241)?

1.3 No ranking

According to Barro and Grossman (1976), the disequilibrium model met the criterion of realism. First, it was possible to establish a causal relationship between monetary and real variables. A decline in the stock of money resulted in an excess supply in the output and labor markets.[17] Since market prices did not adjust instantaneously, Barro and Grossman showed that employers and workers had to revise their production and consumption plans downwards. As a result, a fall in money stock caused a decline in output and employment (1976: pp. 56-57). Second, Barro and Grossman stressed the possibility to account for layoffs. Faced to an excess supply in the market for goods, firms did not cut wages to induce some voluntary quits. They curtailed their demand for labor and, in turn, forced households off their notional supply of labor (1976: p. 42; p. 45). Third, it was possible to replicate the absence of counter-cyclical variations in the real wage. In Barro and Grossman's disequilibrium model, employment variations could occur while the real wage remained fixed. It was the case when, for instance, a decline in aggregate demand caused a generalized excess supply (1976: pp.56-57). Fourth and finally, Barro and Grossman stressed that consumption expenditures varied procyclically in their disequilibrium models. When there was an excess supply in the labor market, the

[17] In the following discussion, the variation in the stock of money is supposed to be compensated by an opposite variation in taxes. It follows that when the stock of money is reduced, the nonwage wealth of the representative household falls. This induces an increase in the supply of labor and a decrease in the demand for goods.

employment quantity entered into the consumption function. Accordingly, a decline in the employment level induced a decline in consumption expenditures (1976: pp. 49-51).[18]

However, the disequilibrium model did not meet the criterion of rigor. Barro and Grossman explained why in the introduction of *Money, Employment, and Inflation*:

> We provide no choice-theoretic analysis of the market-clearing process itself. In other words, we do not analyze the adjustment of wages and prices as part of the maximizing behavior of firms and households. Consequently, we do not really explain the failure of markets to clear, and our analyses of wage and price dynamics are based on ad hoc adjustment equations (1976: p. 6).

Barro and Grossman built their disequilibrium model from the assumption that market prices responded sluggishly to shifts in aggregate demand (1976: p. 2). The problem was that it was not deduced from optimizing behavior. Market prices were fixed exogenously by a market authority in the Walrasian auctioneer spirit (1976: p. 31; p. 95).

By contrast, the equilibrium model met the criterion of rigor. On one side, the "employment-acceptance" decision could be explained by "focusing on the cost associated with the obtaining of information about alternative wage offers" (1976: p. 240). From a worker's perspective, it was rational to take time before accepting a job since "more and better information [could] be obtained more easily and quickly if [he was] not currently employed" (1976: p. 240). On the other side, the existence of spatially distinct marketplaces required individuals to estimate the distribution of prices and wages over space. Therefore, speculation was rational because of the spatial constraint (1976: p. 238).

However, the equilibrium model did not meet the criterion of realism. Not because money was neutral (1976: p. 247), but because the causal relationship between monetary and real variables was inconsistent with facts. First, it was not possible to account for layoffs. Barro and Grossman recalled that when transactions took place under market-clearing conditions, the employment level was chosen by households (1976: p. 248). Therefore, any decline in employment resulted from an optimal decision to quit or to keep looking for a job. Second, Barro and Grossman showed that the real wage varied counter-cyclically in their model. When

[18] The value of the multiplier depended on workers' expectations. If workers expected the employment constraint to last, the reduction in consumption demand was high. The opposite was true, provided that workers had a sufficient stock of money balances (1976: pp. 50-51).

employment increased, the real wage decreased. It was because the demand for labor was inversely related to the real wage, and that exchange took place when markets cleared (1976: p. 250). Third and finally, Barro and Grossman showed that consumption expenditures dampened fluctuations in their model. This effect was due to the speculative component of the consumption demand. By assumption, households compared current prices with their estimate of the distribution of prices over space. When current prices decreased, any offer looked more attractive. Accordingly, households consumed more.

To conclude, Barro and Grossman considered that equilibrium macroeconomics achieved rigor at the sacrifice of realism. The opposite was true for disequilibrium macroeconomics. As a result, Barro and Grossman chose not to establish a ranking. This situation contrasts sharply with their defense of equilibrium macroeconomics at the end of the 1970s. The next section explains why Barro and Grossman changed their mind.

2. Second round (1977-1982)

After 1976, Barro and Grossman continued to compare equilibrium and disequilibrium macroeconomics. However, the competition changed.

2.1 Changes

During the first round, the models did not have to be intertemporal. The proof is that individuals did not make intertemporal choices in the equilibrium model of *Money, Employment, and Inflation* (1976). Moreover, Barro and Grossman (1976) were opened to several forms of expectations. Their disequilibrium model incorporated "static expectations" (Chapters II and III), and then "adaptative expectations" (Chapters IV, V, and VI).[19] By contrast, during the second round, the competition was reserved for models in which individuals formed rational expectations. It was because Barro and Grossman had a new modeling criterion. In "Recent developments in monetary theory" (1976), an article written with Stanley Fisher, Barro explained that the rational expectations hypothesis "[was] a more persuasive starting point than

[19] In a section of Chapter 6, "Adaptative expectations" (1976: p. 223-230), Barro and Grossman indicated that "the analysis of chapter 3, which is serving as a point of departure for the present discussion, assumed expectations regarding output and income, real wages, and the rate of return to be static. Specifically, the representative household expected the level of profits and the constraint on the amount of employment obtainable to remain at its current level until date $\widehat{N}$, and the representative firm expected the constraint on the level of sales to remain at its current level until date $\widehat{N}$. In addition, the representative household and the representative firm expected no change in either the real wage rate or the rate of return over time. This section considers some implications of relaxing these assumptions about expectations (1976: p. 224).

the alternative of using a rule of thumb" (1976: p. 163). Consequently, the best modeling "strategy" was to incorporate rational expectations into macroeconomic models (1976: p. 163). Grossman was on the same page. In a letter sent on 3 February 1978 to Solow, he claimed:

> It probably would be more appropriate to refer to rational expectations as a postulate rather than a hypothesis […] The assumption about efficient [collection and use of information], i.e., rational behavior is not testable, and is really a postulate like the assumption of utility maximization.[20]

Therefore, only macroeconomic models incorporating rational expectations became acceptable.

It follows a second change. The competition involved new models. On the one hand, disequilibrium macroeconomics was represented by a model involving rational expectations and contractual arrangements. It is clear from the published versions of the articles presented in 1978, at the AEA's annual meeting. In "Second Thoughts on Keynesian Economics" (1979), Barro explained:

> My view in the early 1970s of Keynesian, non-market-clearing-type models was that the soundness of their theoretical structure hinged on an as yet absent theory of the stickiness of wages and prices. The application of contracting theory to macro analysis seemed promising in this respect (p. 54).

Likewise, in "Why does Aggregate Employment Fluctuate?" (1979a), Grossman linked disequilibrium macroeconomics with contractual theory. Just like Barro, he thought that the microeconomics developed by Martin Baily (1974) and Costas Azariadis (1975) could ground disequilibrium macroeconomics (1979a: p. 65).[21]

Three features of the new disequilibrium model deserve mention. First, it dealt with uncertainty. Due to random shocks on productivity and on the quantity of money, workers and

[20] Letter from Grossman to Solow, Box 3 OF-IUF-G5.

[21] In 1973, Grossman had already considered the use of contract theory to ground disequilibrium macroeconomics. In a letter sent to Baily on 26 April 1973, he claimed: "I would like to suggest an important extension to your analysis. Your firm announces at time zero a strategy with respect to wages and employment which, if optimal, involves a fixed wage. My question concerns the absence of provisions for revision of this strategy. [This implies] permanent wage rigidity, which is surely too strong a result. However, at the other extreme, if you were to assume that the strategy could be revised costlessly at any time, you would be led to the uninteresting conclusion that the firm would reset the wage each period *after* the state variable becomes known. Thus, what seems necessary to complete your analysis in an interesting way is the introduction of an explicit and finite cost of revising the existing wage and employment strategy and the derivation of optimal criteria for undertaking such revisions" (GP 3 OF-IUF-G5). Grossman sent almost the same letter to Azariadis on 26 April 1973 (GP 1 OF-IUF-G5).

employers could fail to anticipate the level of economic activity. The rational expectations just prevented forecasting errors from being systematic. Second, the determination of market prices was endogenous. It was the result of contracts between employers and workers. Third and finally, disequilibrium transactions occurred because of the existing contracts. One type of labor contract particularly attracted Barro's and Grossman's attention. It distinguished between a situation in which economic agents were successful forecasters and a situation in which they were not. In the former case, workers and employers would agree to set the market-clearing level of employment and a fixed level of nominal wage. In the latter case, however, the nominal wage would remain the same, but the employment level would be determined along the labor demand curve.[22] It explained why there could have an excess supply or an excess demand in the labor market.[23]

On the other hand, equilibrium macroeconomics was no longer represented by a model involving search across markets. During the second round, the references to Lucas (1972, 1975) or Sargent and Wallace (1975) eclipsed the references to Phelps (1970) and Alchian (1970). It is striking in the correspondences between Barro and Grossman. In the mid-1970s, Barro started addressing fluctuations in a model inspired by Lucas (1972). It resulted in a theoretical study, "Rational Expectations and the Role of Monetary Policy" (1976), and in two empirical studies, "Unanticipated Money Growth and Unemployment in the United States" (1977a) and "Unanticipated Money, Output, and the Price Level in the United States" (1978). Grossman commented on each paper. He was particularly interested in how to test the new equilibrium model. In a letter sent on 2 September 1977, he indicated to Barro:

> My student, Robert King, and I have been discussing your interesting paper on 'Unanticipated Money, Output, and the Price Level in the United States.' We have a few tentative comments to offer […] An empirical test that would suggest whether the causation from unanticipated money to output is direct or through

[22] This employment determination rule was used by Azariadis (1975) and Baily (1974) in a partial equilibrium framework, and by Jo Anna Gray (1976) and Stanley Fisher (1977) in a general equilibrium framework.

[23] Contract theory changed the approach to disequilibrium macroeconomics. Azariadis explained why, in a letter sent to Lucas on 1 February 1977. Azariadis explained: "The voluntary or otherwise nature of unemployment is not quite an exercise in semantics because it involves a crucial distinction between two different notions of equilibrium: ex-post (markets clear in every state of nature) equilibrium, which you seem to have in mind; and ex-ante (markets clear before the state of nature is known), which is implied by the work on labor contracts. This would still be a semantic distinction were it not for the fact (which I think I have established) that ex-ante labor market equilibrium yields a systematically different functional dependence on price level of employment and output than does ex-post equilibrium under the same stochastic structure. In other words, the methodological choice facing us now is not between classical equilibrium analysis and disequilibrium of the original Patinkin-Clower type but between two types of equilibria (whole-hog and half-hog)" (Box 1 OF-IUF-G5).

> unanticipated prices would be to use the residuals from the price equation as additional explanatory variables in the output equation.[24]

Just like Barro, Grossman thought that Lucas had identified a promising avenue to explain fluctuations. It just had to be proved in the light of empirical tests.

Three features of the new equilibrium model deserve mention. First, there were random shocks on productivity, the quantity of money, and relative supplies and demands. Second, individuals could not distinguish monetary from real disturbances at once. The correct perception of shocks implied a lag period. Third and finally, fluctuations resulted from unanticipated and misperceived shocks. Barro and Grossman mainly focused on the fluctuations generated by an unanticipated increase in the stock of money. Their explanation was analogous to Lucas's (1972). When "surprised" by monetary authorities, individuals had to determine whether the increase in prices reflected an increase in demand or an increase in the stock of money. Their best course of action was to attribute a fraction of observed price movements to real disturbances. It explained why output and employment could fluctuate after a monetary shock.[25]

Finally, the last change concerned the criteria for discriminating equilibrium and disequilibrium macroeconomics. On the one hand, Barro and Grossman amended the rigor criterion. During the second round, it also included the rationalization of economic policy (Barro, 1979: pp. 54-56; Grossman, 1979a: p. 68). Barro elaborated on what it meant:

> The theoretical case for activism should, as in areas like industrial organization and the production of 'public goods', require as a first step some serious analysis of private market failure. […] The nature of the formation of expectations seems to be an important issue within the general context of the efficiency of private arrangements relative to governmental actions, but it is this general concept of

[24] Letter from Grossman to Barro, Box 1 OF-IUF-G5.

[25] When comparing disequilibrium and equilibrium macroeconomics, neither Barro nor Grossman addressed the persistence of fluctuations. I found only one reference to this issue, in "Why does Aggregate Demand Fluctuate?" (1979a). Grossman argued: "A frequent objection to the incomplete information paradigm is that it cannot readily account for observed persistence in the effect of shifts in aggregate demand on aggregate employment. However, as Lucas and Thomas Sargent stress, an absence of serial correlation in misperceptions of potential gains from trade does not preclude serial correlation in the effects of such misperceptions. Moreover, the alternative non-market-clearing paradigm does not seem to have any basic advantage with respect to explaining persistence. Specifically, assuming that wages and prices adjust gradually so that excess supply is persistent seems no less heroic than assuming that information disperses gradually or, as Lucas and Sargent suggest, that demands for labor services or physical capital adjust gradually" (1979a: p. 66).

> relative efficiency that seems to be crucial in evaluations of policy activism (1979: p. 56).

Each model had to be judged on its capacity to show whether the free functioning of markets led to sub-optimal outcomes and whether public authorities were relatively more efficient than private agents to coordinate economic activities.

On the other hand, Barro and Grossman amended the realism criterion. During the second round, the models' capacity to account for layoffs and for the lack of cyclical variations in real wages became secondary.[26] In "Long-Term Contracting, Sticky Prices, and Monetary Policy" (1977b), Barro claimed:

> Some frequently discussed aspects of labor markets are a façade with respect to employment fluctuations. In this category, we can list sticky wages, layoffs versus quits, and the failure of real wages to move countercyclically (p. 316).

Barro's (1977b) list did not include the non-neutrality of money and the procyclical variations in consumption expenditures. However, the later stylized fact also became secondary. During the second round, Barro and Grossman mainly focused on the models' capacity to establish a causal relationship between monetary and real variables.

To conclude, the competition between equilibrium and disequilibrium macroeconomics changed after 1976. There were a new eligibility condition, new models, and new rules of the game. The question is whether one change was particularly instrumental in the victory of equilibrium macroeconomics.

2.2 The superiority hypothesis

During the first round, equilibrium macroeconomics was rigorous but not realistic. Disequilibrium macroeconomics, on the other side, was realistic but not rigorous. As a result, Barro and Grossman could not rank the two approaches to macroeconomics. How did this change during the second round? Did the new version of equilibrium macroeconomics score better than the new version of disequilibrium macroeconomics?

[26] It is true to a lesser extent for Grossman. Unlike Barro, Grossman kept judging the models' capacity to depict layoffs and the lack of cyclical variation in the real wage. In a similar way, the rationalization of activism seemed more important for Barro than for Grossman. Section 2.2 elaborates on that.

Concerning the realism criterion, the answer is no. To address the empirical validity of equilibrium macroeconomics, Barro (1977a) tested whether unanticipated money movements affected production and employment. His statistical study showed that they did. Monetary surprises had significant real effects. However, could one conclude that Lucas's framework explained actual fluctuations? In 1977, Barro suggested that his econometric test was not specific enough. It was because "the proposition that only unanticipated money movements [had] real effects [was] clearly more general than the specific setting of [Lucas's (1972) or Sargent & Wallace's (1975)] models" (1977a: p. 101). It also underlined the new version of disequilibrium macroeconomics. The difference was that the causal relationship between monetary and real variables involved the failure of markets to clear, not agents' inability to perceive an ongoing monetary policy correctly. That situation led Barro and Grossman to consider new tests of equilibrium macroeconomics. They were presented in "Money Stock Revisions and Unanticipated Money Growth" (Barro & Hercowitz, 1980) and in "Tests of Equilibrium Macroeconomics Using Contemporaneous Monetary Data" (Boschen & Grossman, 1982). According to Barro and Hercowitz (1980), the formulation of a test adapted to equilibrium macroeconomics required to proxy the unperceived money growth. Their idea was to use the revisions of the money stock data published by the Federal Reserve. Thus, their test consisted of determining whether output and employment could be explained by the discrepancy between the initial and the final reports on money growth. On their side, Boschen and Grossman (1982) conditioned their empirical study upon a modification of equilibrium macroeconomics. They developed a model in which individuals processed information on current monetary policy and considered the revisions of monetary data. On that basis, they tested whether a perceived monetary policy was neutral and whether revisions of monetary data were non-neutral (1982: p.311).

It resulted in three econometric tests. All three questioned the empirical validity of equilibrium macroeconomics. On the one hand, Barro and Hercowitz showed that "the discrepancy between initial and final reports on money growth rates [had] no explanatory power for unemployment and output" (1980: p. 266). It suggested that incomplete information was not central to business fluctuations (1980: p. 266). On the other hand, Boschen and Grossman showed that a perceived monetary policy could have significant real effects and that revisions of monetary data might be neutral (1982: p. 311). It followed that the "two tests [provided] strong evidence against the reality of the equilibrium approach to modeling macroeconomic fluctuations" (1982: p. 311).

At the same time, Barro and Grossman stressed the significance of market price stickiness in fluctuations. In the preface to the Japanese edition of *Money, Employment, and Inflation*, Grossman reflected on "the present state of the theory of macroeconomic fluctuations."[27] He claimed that "to fit the facts, we seem to have to use models that [involved] the failure of markets to clear and/or ad-hoc expectations." Barro, on his side, did not discuss the empirical validity of disequilibrium macroeconomics in the preface. However, he did in *Money, Expectations, and Business Cycles* (1981). While reflecting on the results obtained in "Money Stock Revisions and Unanticipated Money Growth" (1980), Barro addressed the empirical validity of the unperceived vs. unanticipated approaches to fluctuations.[28] He argued that "if the initial reports on the money stock [were] viewed as observable with a negligible time lag, [the econometric result obtained with Hercowitz] would support the view that unanticipated, rather than unperceived, money were the important stimulus for output" (1981: p. 73). It was tantamount to considering that market price stickiness was the source of actual fluctuations. As a result, Barro and Grossman considered that disequilibrium macroeconomics was more realistic than equilibrium macroeconomics.

Under these circumstances, Barro and Grossman did not have a basis for discriminating between the two approaches to macroeconomics. Although equilibrium macroeconomics was considered to be more rigorous than disequilibrium macroeconomics, its superiority did not follow.[29] Therefore, neither the change in models nor the change in evaluation criteria was central equilibrium macroeconomics' victory.

This claim is consistent with two other observations. First, Barro and Grossman considered that the new version of equilibrium macroeconomics lacked microfoundations. In the preface to the Japanese edition of *Money, Employment, and Inflation*, Barro claimed:

> It would not be fair presently to describe the equilibrium approach as providing a complete theoretical and empirical picture of business fluctuations.

[27] Barro and Grossman wrote two separate prefaces. In a letter sent to Hirotaka Kato (the translator of *Money, Employment and Inflation*), Grossman explained that "Barro [preferred] this arrangement of two separate prefaces, [which did not reflect any] major disagreement" (8 October 1980, GP 3 OF-IUF-G5). I found the English version of these prefaces in Grossman's papers. Thanks to Yutaka Furuya, I could check the potential differences with the Japanese's. There is nothing to report.

[28] As a reminder, Barro and Hercowitz pointed out that "purely nominal shocks [could] influence real behavior not because these shocks [were] contemporaneously unperceived, but rather because these shocks were unpredictable at earlier dates. The imposition of unanticipated, but not necessarily contemporaneously unperceived, money movements on an economy with long-term nominal contracts is viewed as a source of business fluctuations in models constructed by Gray (1976) and Fisher (1977)" (1980: p. 258).

[29] For a discussion of the microfoundations of disequilibrium macroeconomics, see 2.3.

> Explanations for the short-run non-neutrality of money are especially troublesome within this setting.[30]

Barro did not elaborate on the theoretical gaps of Lucas's framework. But Grossman did. On the one hand, Grossman criticized the *ad hocness* of the assumption concerning general and relative disturbances. While discussing with Lucas about "An Equilibrium Model of the Business Cycle" (1975), Grossman claimed:

> An important problem with your analysis, and Barro's [1976] analysis, seems to be that the assumption of specific differences in the intertemporal characteristics of the stochastic process that generate general and relative disturbances has an unappealing ad hoc appearance.[31]

In Lucas's (1972; 1975) and Barro's (1976) models, nothing explained why the relative disturbances were transitory while monetary and real disturbances were permanent. This assumption was yet central to money's non-neutrality (Barro and Fisher, 1976: p. 161). On the other hand, Grossman indicated that the lag in the transmission of information was not rationalized. Nothing explained why individuals could observe the general level of prices only with a delay of one period. It was problematic not only because this assumption was instrumental in the non-neutrality of money. But also because, according to Grossman, it was incompatible with the rational expectation hypothesis. This incompatibility problem was stressed in the article written with Boschen:

> Since the early 1950s, the Federal Reserve Board has issued preliminary monetary data with a lag of no more than one or two months. Since 1965, this lag has been only eight days […] The classic equilibrium models abstract from both the existence of contemporaneous preliminary monetary data and the process of gradual accumulation of revised monetary data. The neglect of contemporaneous data implies that private agents act as if they ignore readily available and apparently relevant information, an implication that seems inconsistent with the idea of rational expectations (Boschen & Grossman, 1982: p. 310).

[30] GP 1 OF-IUF-G5.
[31] Letter from Grossman to Lucas, 20 January 1977 (GP 2 OF-IUF-G5).

The rigor of equilibrium macroeconomics was, therefore, more questioned during the second round than during the first round. It reinforces the view that the change in models was not central to its victory.

The second observation concerns the role played by the change in evaluation criteria. This change was significant in Barro's case. After 1976, policy activism's rationalization became central to his comparative analyses of disequilibrium and equilibrium macroeconomics. "Second Thought on Keynesian Economics" (1979) is a case in point. The article's goal was to show that equilibrium macroeconomics offered the best framework for evaluating policy activism (1979: p. 54; p. 56). Moreover, during the second round, Barro was no longer concerned with the models' capacity to depict layoffs, procyclical consumption expenditures, and the lack of cyclical variations in real wages. His focus was on the causal relationship between monetary and real variables.

By contrast, Grossman never stopped referring to the other stylized facts. For instance, in "Why does Aggregate Demand Fluctuate" (1979a), Grossman stressed the relevance of contract theory to explain layoffs and the lack of cyclical variations in the real wage (pp. 67-68). It led him to defend the "paradigm of incomplete information extended to take account of implicit contractual arrangements for mitigating risk" (1979a: p. 68). Moreover, Grossman rarely addressed the rationalization of policy activism. It was mentioned only in passing in the conclusion of "Why Does Aggregate Demand Fluctuate?" (1979a: p. 68). It implies that the change in evaluation criteria was more significant in Barro's than in Grossman's case. Nevertheless, the two economists reached the same conclusion. The change in evaluation criteria was thus not central to the victory of equilibrium macroeconomics.

2.3 The compliance hypothesis

Through a process of elimination, one can conclude that the introduction of rational expectations was the game-changer. However, how could compliance with this new modeling standard affect the outcome of the competition? Two effects are identified.

In the new version of disequilibrium macroeconomics, Barro and Grossman focused on one type of labor contract. The contract specified that when individuals failed to anticipate an expansionist monetary policy (monetary contraction), the real wage decreased (increased), and exchange took place while there was an excess demand (supply) in the labor market. According to Barro, this implied the non-realization of exchange opportunities:

> Positive money shocks imply that the marginal product of labor, as calculated from the [labor demand] curve, is below the marginal value of time, as calculated from the [labor supply] curve – and the reverse for negative money shocks. Whenever there is a departure of the marginal product of labor from the marginal value of time there is, ex post, an unexploited opportunity for mutual gains from trade. Namely, any movement of [the quantity of labor] towards [the market-clearing level of employment] (accompanied by appropriate side payments) would make both firms and workers better off (1977b: p. 311).

The problem, Barro argued, was that firms and workers could perceive such exchange opportunities. In the absence of incomplete information about monetary and real shocks, the rational expectation hypothesis implied that individuals had full information about the model's structure. Thus, the two parties would have no reason to "agree, ex-ante, to a form of contract that imposes these sorts of ex-post dead-weight losses" (1977b: p. 311). According to Barro, it was not rational. Grossman agreed (1979a: p. 68).[32] Barro and Grossman concluded that the new version of disequilibrium macroeconomics broke with the principle of individual rationality (effect 1).

Then, Barro and Grossman generalized the result (effect 2). In "Long-Term Contracting, Sticky Prices, and Monetary Policy" (1977b), Barro claimed "that wage/price stickiness [was] not, per se, fundamental to Keynesian models. Rather, the crucial element –and the aspect that accurately [marked] this approach as 'non-market-clearing' analysis – [was] the nonexecution of some *perceived* mutually advantageous trades (p. 315). Likewise, Grossman argued that the "essential aspect of the non-market-clearing paradigm [was to consider] situations in which *perceived* gains from trade [were] foregone because buyers and sellers [were] limited to transacting at a wage-price vector that [did] not equate quantities supplied and demanded" (1979a: p. 65). The non-realization of perceived exchange opportunities had therefore become a feature of disequilibrium macroeconomics. It implied that disequilibrium macroeconomics

[32] At first, Grossman was not convinced by Barro's (1977b) argument. In a letter sent to Barro on 31 December 1975, he claimed: "when [there is a negative shock on the money supply], your proposed [market-clearing] employment rule, together with the contractually fixed nominal wage rate, can imply negative profits for the firms. […] I would argue that the cost to the firm of financing negative profits probably rules out contracts with both fixed [wages] and [equilibrium in the labor market]" (GP 1 OF-IUF-G5). However, Barro eventually convinced Grossman that he was right. In the process, he relied on the rational expectation hypothesis. In a letter sent on 6 January 1976, Barro explained: "one would always prefer a more variable income stream with a higher income (utility) in every state of the world than a less variable stream. Attitude toward risk is irrelevant here. Any contract with [disequilibrium in the labor market] in some state of the world is stochastically dominated by a contact with [equilibrium in the labor market]" (GP 1 OF-IUF-G5).

could not be based on microfoundations and that, in turn, the only relevant research strategy was to improve equilibrium macroeconomics.

Barro conveyed this message in the preface to the Japanese edition of *Money, Employment, and Inflation*:

> Explanations for the short-run non-neutrality of money are troublesome within [equilibrium macroeconomics]. However, these difficulties reflect the model's requirement that the major propositions can be deduced from an internally consistent framework in which individual rationality prevails. The power of this general economic approach has been demonstrated in many areas, including various aspects of macroeconomics. It seems pointless to return to disequilibrium macroanalysis.[33]

Grossman was on the same page. In a letter sent to the translator of *Money, Employment, and Inflation*, he claimed:

> Barro and I agree, except in whatever subtleties might be reflected in our choice of words. Specifically, [Barro] writes that models that involve failure of markets to clear and/or ad-hoc expectations are 'pointless', whereas I write that 'reliance on such devices is not satisfactory'.[34]

Grossman concluded his preface by identifying an avenue to improve equilibrium macroeconomics. Just like Barro, therefore, he considered that it was the only relevant research strategy.

Barro and Grossman's logic was criticized. Several economists questioned their characterization of disequilibrium macroeconomics and the resulting conclusion. It was notably the case of Robert Gordon and Peter Howitt. During correspondence with Grossman, Gordon claimed:

> My problem with your 'Why Does Aggregate Demand Fluctuate?' involves the flat statement, repeated several times, that the non-market-clearing paradigm involves 'a failure to realize perceived gains from trade.' This statement, and indeed your entire paper, indicates a disregard for the entire microeconomic

[33] GP 1 OF-IUF-G5.
[34] Letter from Grossman to Kato, 5 January 1981 (GP 3 OF-IUF-G5).

> literature on non-Walrasian equilibrium (NWE) surveyed in the attached paper by Drazen [1980]. There seems to be little dispute that it is theoretically possible to imagine a NWE in which there are no perceived (or 'conjectured') gains from trade […] Second, the entire Lucas-Sargent literature rests on an *ad hoc* asymmetry between suppliers and demanders […] Thus Lucas has no greater claim to rigor than Barro-Grossman.[35]

In the same spirit, Howitt claimed a few years later:

> I am puzzled by your characterization of NMC [non-market-clearing] models as assuming that unexploited gains from trade are 'expected'. If you add the assumption that workers and firms know exactly who should trade what with whom to make everyone better off, and that they could carry these trades out at no cost, then it is a genuine puzzle to explain why they don't carry them out. But surely the spirit of this kind of model is that this information is not universally known, that the process by which potential trading partners contact one another and communicate offers takes time, and that this process is somehow captured by assuming that people communicate only through the auctioneer, who operates at a finite speed. […] My conclusion is that the NMC approach does not contradicts any fundamental tenet of neoclassical analysis.[36]

Under these circumstances, the question is why Barro and Grossman did not consider other approaches to disequilibrium macroeconomics. For instance, why did they not develop the conjectural equilibrium models of Frank Hahn (1977; 1978) and Takashi Negishi (1979)? There was no failure to act on perceived gains from trade in this framework. The same applied to Howitt's. However, economic agents did not form rational expectations in these models. It might explain why Barro and Grossman ignored them and stuck to the view that disequilibrium macroeconomics implied a break with individual rationality.[37] The consequence was to end the competition by disqualifying disequilibrium macroeconomics.

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[35] Letter from Gordon to Grossman, 31 July 1978 (GP 2 OF-IUF-G5).

[36] Letter from Howitt to Grossman, 29 December 1982 (GP 3 OF-IUF-G5).

[37] Leland Yeager made a similar conjecture in a letter sent to Grossman, on 22 January 1986. According to Yeager, "monetary disequilibrium theorists recognize that people act rationally and purposefully, striving for the most satisfactory outcomes for themselves, which includes trying to reap gains from trade. But although people desire optimum results, they have no way of achieving them instantly and costlessly […] Despite what Barro says in the passage you quote on pages 6-7 and despite what you yourself seem to be suggesting throughout your whole comment, I myself do not perceive any trouble in reconciling monetary-disequilibrium theory with purposeful

3. The defense of equilibrium macroeconomics

Barro and Grossman disqualified disequilibrium macroeconomics. However, it does not imply that equilibrium macroeconomics was a default choice. To explain why, it is useful to elaborate on their methodology.

3.1 Methodological principles

There was tension in how Barro and Grossman modeled economic fluctuations. On one side, they required to start from the Walrasian framework (e.g., 1971: p. 84; 1973: p. 1355; 1976: p. 10). It resulted in the development of general equilibrium models with competitive markets and where agents' decisions were optimal, consistent, and based on real factors. On the other side, Barro and Grossman considered that Walrasian theory could not explain fluctuations. It was because a change in the money supply did not affect production and employment levels (1973: p. 1354; 1976: p. 23). Consequently, Barro and Grossman had to design a strategy to reconcile theory with facts.

Their strategy was to introduce "frictions" into the Walrasian framework. More specifically, Barro and Grossman developed either models in which market prices responded sluggishly to shocks, or models in which information was incomplete. However, neither Barro nor Grossman considered a model in which agents could base their decisions on nominal or monetary factors. Nor did they consider relaxing agent's rationality, the compatibility between optimizing plans, or perfect competition. It never was an option to reject the core principles of Walrasian theory.

Then, there is the issue of how Barro and Grossman evaluated models. Previous sections showed that they used realism and rigor criteria.[38] However, these were only the most visible. Barro and Grossman also considered a criterion of analytical tractability (i.e., capacity to solve the model) and a criterion of research potential (i.e., capacity to address new issues and/or

and rational behavior. Any problem in recognizing the reconciliation must hinge – so I conjecture – on preconceived notions about what counts as a reconciliation or, more generally, about what counts as acceptable theorizing" (GP 3 OF-IUF-G5).

[38] Barro and Grossman sometimes discussed the realism of assumptions. For instance, they explained that exchange took place under non-market-clearing conditions in the actual world (e.g., 1976: pp. 38-39). However, the realism of assumptions was secondary in the evaluation of models.

provide fresh perspectives on old ones).[39] Both criteria mattered when they started to work on disequilibrium macroeconomics. In a letter sent to Donald P. Tucker, Grossman explained:

> I am not satisfied with 'Non-Tâtonnement Models of Market Disequilibrium' [1969], and I have put it aside for the moment. The problem involves the working out of a tractable formulation of the dynamic of the formation of anticipated transaction constraints. I am not satisfied with the simultaneous formulation which I used in 'Money, Interest, and Prices in Market Disequilibrium'. But I am not sure that further work along these lines would be worth the effort. I suspect that development of the practical implications of less general models may be more valuable, as in my 'General Disequilibrium Model of Income and Employment' [1971], with Bob Barro, which will be appearing in the *AER*.[40]

In the 1971 model, quantities and transactions were determined simultaneously. Moreover, the analysis was carried out at an aggregated level. This implied differences with the disequilibrium models that Grossman had developed earlier. For instance, Grossman (1969) had built a disaggregated model where agents needed to anticipate their rationing levels on markets. It follows that when starting their collaboration, Barro and Grossman had a choice between several disequilibrium models. According to Grossman, the 1971 model was "less general" than others, and the formation of anticipated constraints was not satisfactory. However, it was "tractable" and opened up the possibility to discuss "practical" issues, including involuntary unemployment, forced saving, and the multiplier effects. It explained why Barro and Grossman chose to develop aggregated fixed-price equilibrium models.

In a nutshell, two methodological principles can be highlighted. The first principle concerns the relationship between theory and facts. According to Barro and Grossman, facts had to conform to the Walrasian worldview. The second methodological principle is related to the assessment of models. What is important (at this stage) is that Barro and Grossman used analytical tractability and research potential criteria.

[39] For a systematic analysis of the issue of tractability in disequilibrium macroeconomics, see Plassard (2021). My article is part of a project initiated by Béatrice Cherrier. Information can be found on her blog, https://beatricecherrier.wordpress.com/.

[40] Letter from Grossman to Tucker, 24 June 1970 (GP 3 OF-IUF-G5).

3.2 On the right track

According to Barro and Grossman, the first methodological principle had proved successful.[41] In his review of *Disequilibrium Dynamics: A Theoretical Analysis of Inflation and Unemployment* (Iwai, 1981), Grossman claimed:

> Iwai fails to recognize that abstractions that are not appropriate for the objectives of management science or behavioral psychology can be valuable for economics, and vice versa. Specifically, when our objective is to develop models that explain and predict aggregate economic outcomes, and we want to avoid becoming sidetracked or bogged down by the details of individual behavior, the neoclassical abstractions frequently have proven to be both theoretically convenient and empirically useful (1983: p. 344).

Grossman did not elaborate on the explanatory power of "neoclassical abstractions." His comment was only suggestive. Since the "neoclassical" method allowed to explain a wide range of macroeconomic phenomena, why would equilibrium macroeconomics fail to explain fluctuations?

Due to the results of their empirical tests, Barro and Grossman used to leave the issue open. Grossman even indicated that his "review [was] not meant to be a general defense of equilibrium macroeconomic models or even of neoclassical economics as a framework for the study of macroeconomic fluctuations" (1983 p. 344). However, Barro and Grossman were convinced that equilibrium macroeconomics could meet the realism criterion. This conviction resulted from the first methodological principle. Since theory had the upper hand on facts, Barro and Grossman downplayed their empirical tests' importance. It is striking in "Money Stock Revisions and Unanticipated Money Growth" (1980). Barro and Hercowitz (1980) refused to conclude that Lucas had failed to explain fluctuations. It was because "the strongest theoretical arguments for real monetary effects [depended] on confusions between relative and absolute changes, which [required] the underlying money stocks to be temporarily unperceived" (1980: p. 266).

According to Barro and Grossman, the theory had shown that incomplete information was the critical factor in fluctuations. In "Long-term contracting, sticky prices, and monetary

[41] See *supra*, in the excerpt from Barro's preface to the Japanese edition of *Money, Employment, and Inflation* (section 2.3).

policy" (1977b), Barro explained that incomplete information was necessary to explain fluctuations. His trick was to use a model with contractual arrangements. Barro assumed a labor contract setting a fixed nominal wage level and a market-clearing employment rule (1977b: p. 311). Within this framework, an expansionary monetary policy would decrease the real wage below its equilibrium value. However, employment level would not increase since it was determined under market-clearing conditions (1977b: p.312). However, the conclusion changed once firms could exploit a lack of information about monetary and real disturbances. When engaging in contracts, employers and workers knew that a productivity shock changed the equilibrium level of employment while a monetary shock did not. But unlike workers, firms were directly affected by a productivity shock. Accordingly, they could (temporarily) "misrepresent this value to the workers" when the economy was subjected to monetary disturbances (1977b: p. 314). For instance, firms could overstate their perception of a productivity shock when the general price level raised. It would "substantiate a claim that [the equilibrium level of employment] had increased and, in turn, an increase in economic activity" (1977b: p. 314). Then, in "Rational Expectations and the Role of Monetary Policy (1976), Barro explained fluctuations in a model involving only incomplete information about monetary and real disturbances. Barro and Grossman concluded that incomplete information was not only necessary but also sufficient to generate fluctuations. Hence why they defended equilibrium macroeconomics. Barro and Grossman were convinced that they were on the right track to explain fluctuations.

3.3 Tractability and research perspectives

Neither Barro nor Grossman discussed the tractability of Lucas's macroeconomics. But there are reasons to believe that it mattered. First, Barro and Grossman highlighted the tractability of the rational expectations and the market-clearing assumptions. It is what Grossman did, implicitly, when claiming that "neoclassical abstractions [had] proved to be theoretically convenient" (1983: p. 344). Barro, on his side, was more explicit. In "Recent Developments in Monetary Theory" (1976), he explained:

> A fundamental difficulty with theories of expectations that are not based on the predictions of the relevant economic model (rational expectations) is that they require a theory of systematic mistakes. Such theories are inherently more difficult to formulate than those based on rational behavior, and it seems to be a reasonable strategy to try to do without them (1976: p. 163).

In the same spirit, Barro also justified the use of the market-clearing assumption on tractability grounds. While commenting on "Unemployment, Employment, and Exports in British Manufacturing: A Non-Clearing Markets Approach" (Muellbauer and Winter, 1980), Barro claimed that "the concept of equilibrium [was] a useful way of organizing economic analysis" (1980: p. 412).

Second, during the first round, Barro and Grossman had failed to incorporate uncertainty into macroeconomics. It had been a project when starting to work on *Money, Employment, and Inflation*. In a letter sent to Barro on 13 May 1971, Grossman claimed: "in general, households have some subjective possibility of future constraints on both their sales of labor and purchases of consumables. Perhaps what we should do is to work out the theory of the consumer choice under uncertainty. This material would make a good chapter. How does this strike you?"[42] Barro replied: "in the excess demand model, the problem is that there exists a [constraint] which is known with certainty to limit purchases. Therefore, the substitutability between leisure and consumption is short-circuited […] Anyway, I'm glad you suggested doing something with this for the book."[43] However, Barro and Grossman eventually put aside uncertainty (1976: p. 5). The fixed-price method was part of the problem.[44] In a letter sent to Grossman on 10 May 1972, Barro claimed: "there is little prospect that we will actually incorporate the theoretical results on expectations in any substantive way in our basic model."[45] No explanation was given. But like Barro and Grossman, Antoine D'Autume, Jean-Pascal Bénassy or Edmond Malinvaud later tried to incorporate uncertainty into fixed-price equilibrium models (Plassard et al, 2021). It turned out to be challenging, notably because the "multiplication of [anticipated] regimes made the analysis intractable" (D'Autume, 1985: p. 189). By contrast, Lucas offered a tractable approach to uncertainty in macroeconomics.

Third and finally, economists with whom Barro and Grossman had contacts stressed the tractability of equilibrium macroeconomics. It is the case, for instance, of Thomas Sargent. In "Beyond Demand and Supply Curves in Macroeconomics" (1982), Sargent claimed:

[42] GP 3 OF-IUF-G5.
[43] Letter from Barro to Grossman, 18 May 1971 (GP 3 OF-IUF-G5).
[44] In a letter sent to Grossman on 10 May 1972, Barro claimed: "Frankly, at this point, I am very eager to get the book completed – and I am definitely aiming at the end of the summer for this. Without the expectation-adjustment chapter, I think this is feasible" (GP 3 OF-IUF-G5). On 17 May 1972, Grossman replied: "I go along with your thoughts on the chapter about the theory of expectations and adjustments […] Other material should take higher priority and we should get the book finished" (GP 3 OF-IUF-G5).
[45] GP 3 OF-IUF-G5.

> The basic insight about the cross-equation nature of the restrictions delivered by dynamic economic theory applies in dynamic 'disequilibrium' as well as in equilibrium contexts. [However] things get much more complicated in disequilibrium contexts because agents' decisions rules inherit additional parameters from the dynamic stochastic rationing rules confronting individual agents (p. 383)

Therefore, there are good reasons to believe that Barro and Grossman defended equilibrium macroeconomics on tractability grounds.

On the other hand, Barro and Grossman insisted on the research potential of equilibrium macroeconomics. In "Rational Expectations, Business Cycle, and Government Behavior" (1980), Grossman emphasized the "vitality" of a "research program that [focused] on the relations between various information problems, monetary and fiscal policies, and the nature of business cycles" (p. 10). In the same spirit, Barro (1981) listed the new insights provided by equilibrium macroeconomics. According to Barro, "the equilibrium approach [had] revitalized macro policy analysis, especially by shifting consideration from the impact of one-time realizations of variables like money stock or government spending to a study of different methods for operating policy in relation to the economy. [It had also] shown how models that [exhibited] business cycle characteristics [could] also be consistent with the irrelevance of systematic policy feedback and with the harmful effects of unpredictable government behavior" (1981: p. 74). Finally, Barro indicated that "the new macroeconomic approach [had] produced econometric insights concerning expectations that will remain even if other aspects of the theory [were] questioned" (1981: p. 74).[46] All this suggests that Barro and Grossman also defended equilibrium macroeconomics because it opened up new research perspectives.

4. Conclusion: The domination of equilibrium macroeconomics

When Barro and Grossman defended equilibrium macroeconomics, it did not only affect their research. It also affected their teaching. While they were colleagues at Brown University, Barro and Grossman taught disequilibrium macroeconomics. During the spring of 1971, Barro

[46] Barro particularly referred to the "methods for testing natural rate hypotheses and the interplay between expectations and policy shifts" (1981: p. 74). See Hoover (1988: pp. 165-209) for a detailed analysis of the econometric techniques used in the New Classical macroeconomics.

introduced students of "208" to the analysis of non-market-clearing dynamics.[47] Next academic year, Barro and Grossman used drafts of *Money, Employment, and Inflation* in their macroeconomics courses.[48] In a letter sent on 12 February 1972, Grossman informed Barro: "I am going to be talking in 213 (and 208 eventually) about the interplay between aggregate demand, employment, and inflation. This discussion will involve the results of Chapter II as well as the implications of analysis of wage and price adjustments including the role of expectations. I shall send you my notes and you can indicate how that ties in with your ideas on Chapter VI."[49]

However, Barro and Grossman eventually stopped teaching disequilibrium macroeconomics. By the mid-1970s, when discussing teaching programs or students with Barro (who had left for the University of Chicago), Grossman no longer mentioned disequilibrium macroeconomics. The focus was instead on equilibrium macroeconomics.[50] During the spring of 1975, for instance, Grossman informed Barro that he "[had] just gone over in class [his] 'Rational Expectations and the Role of Monetary Policy.'[51] Barro, on his side, also taught ongoing developments in the new classical macroeconomics. Based on this teaching experience at the University of Chicago and, later, at the University of Rochester, he wrote *Macroeconomics* (1984). *Macroeconomics* later became a reference textbook for several generations of macroeconomists.[52]

This shift in teaching programs affected research. In the early 1970s, several Brown students chose to write a dissertation on disequilibrium macroeconomics. It was the case, for instance, of Barry Carin and Robert F. Lucas. On 24 May 1971, Grossman explained to Barro: "I want [Carin] to concentrate on working out the dynamic implications of alternative exchange rate regimes, using my 'Money, Interest, and Prices in Market Disequilibrium' [1971] as a guide

[47] In a letter sent to Grossman on 18 May 1971, Barro proposed to "send a copy of [his] 208 notes […] to write the first draft on the lags and short-run dynamics chapter [of *Money, Employment, and Inflation*]" (GP 3 OF-IUF-G5).

[48] *Money, Employment, and Inflation* was supposed to be part of a series of textbooks published by Basic Books. Unable to commission other textbooks, Basic Books cancelled its publication (Backhouse and Boianovsky, 2013: p. 73). In the end, Barro and Grossman's book was published by Cambridge University Press.

[49] GP 3 OF-IUF-G5.

[50] An interview conducted with D'Autume (03/2019) confirms this shift in teaching programs. In the early 1980s, Grossman invited him to visit Brown University. D'Autume accepted and spent one semester in Providence. While there, he remembered that students were not familiar with disequilibrium macroeconomics. Models incorporating market-clearing and rational expectations were what interested them.

[51] Letter from Grossman to Barro, 9 April 1975 (GP 1 OF-IUF-G5).

[52] *Macroeconomics* was reissued on five occasions (1984; 1987; 1990; 1993; 1998), and translated into Chinese, French, German, Japanese, Italian, Polish, and Spanish.

to framework."[53] Carin followed Grossman's advice and wrote a dissertation, "Dynamics of Disequilibrium in Alternative Exchange Rate Regimes" (1972). Lucas chose a similar topic. In a letter of recommendation written in December 1974, Grossman summarized: "Lucas is in the process of completing his studies for the PhD degree in Economics […] His particular specialization has been the theory of general disequilibrium. His dissertation involves the application of this theory to the effects of international disturbance on an economy under non-market-clearing conditions."[54]

However, after Lucas, Barro and Grossman no longer mentioned dissertations on disequilibrium macroeconomics. By the mid 1970s, they mainly supervised dissertations on equilibrium macroeconomics. For instance, Grossman supervised Boschen's and King's. Boschen's "thesis [addressed] the empirical relevance of rational-expectations models that [attempted] to explain business cycles by appealing to assumptions about incomplete information."[55] The same applied to King, who defended a PhD thesis on "Asset Markets and the Neutrality of Money" (1980). After the PhD, both macroeconomists continued to work on equilibrium macroeconomics. King notably participated in the emergence of real business cycle models (Young, 2014). Therefore, Barro and Grossman contributed to redirecting research from disequilibrium to equilibrium macroeconomics by changing their teaching programs.

Second, Barro and Grossman stimulated new research on equilibrium fluctuations. Their empirical studies (1980; 1982) forced macroeconomists to consider models different from Lucas's (1972, 1975). King is a case in point. In collaboration with a student (Brahat Trehan) and a colleague (Charles Plosser) from the University of Rochester, King developed real business cycle models incorporating money (1984a; 1984b). The models presented at least two distinctive features. Instead of being exogenous, the money supply responded to changes in the nominal rate of interest, and to unobserved shocks on money's velocity. Moreover, "positive comovements in money and output [could] be obtained even if the current money stock [was] known to agents" (1984a: p. 393). Fluctuations occurred because monetary statistics provided signals about unobserved real aggregate shocks.

While elaborating these models, King sought to offer a way around the criticisms raised by Barro and Grossman. In a letter sent to Grossman on 1 October 1981, King explained:

[53] GP 3 OF-IUF-G5.
[54] Letter from Grossman to "whom it may concern," 4 December 1974, (GP 2 OF-IUF-G5).
[55] Excerpt from a letter of recommendation written by Grossman, on 15 February 1979 (GP 1 OF-IUF-G5).

> I have given some further thought to tests of Lucas-type business cycle theories that employ contemporaneous monetary data […] Suppose that monetary measures were a signal about some contemporaneously (aggregate) determinant of real economic activity. Then, agents could respond to monetary statistics as an information variable. That is, in a manner analogous to Lucas, agents might be 'fooled' (by a particular monetary event) into believing that a shift in real economic conditions had occurred. Examples of this type are i) contemporaneously unobservable changes in government financing mechanisms; and ii) contemporaneously unobservable shocks to real demand/supply that are corelated with money through the banking system. At present, I've talked some of these ideas through with Brahat Trehan and we aim to develop this topic further.[56]

On the other hand, King and Plosser (1984b) claimed: "our explanation of the correlation between money and business fluctuations stands in sharp contrast to traditional theories that stress market failures" (p.363) – i.e., "macroeconomic theories [based] on implausible wage and price rigidities [and the] analyses of monetary non-neutrality that rely on an apparent failure in the market for information (Lucas, 1977, provided a summary of this viewpoint)" (p. 363). King and Plosser recalled that "Boschen and Grossman (1982) rejected [Lucas's] proposition by the data" (p.363). They concluded that it was "worthwhile to consider alternative hypotheses" (p.363). Hence the development of real business cycle models with or without money.

Third and finally, Barro and Grossman might have induced defections from disequilibrium macroeconomics. It is what suggests an interview with D'Autume (03/2019).[57] Like Barro and Grossman, D'Autume developed fixed-price equilibrium models. His doctoral dissertation, "Équilibres non-walrassiens et macro-économie" (1980), was devoted to the analysis of economic growth under non-market-clearing conditions. After his PhD, D'Autume focused on the relationship between income distribution and capital accumulation (1983), on the valuation of capital under anticipated constraints (D'Autume and Michel, 1984), and on the long-run dynamics of non-clearing markets (D'Autume and Michel, 1985a; 1986). That research culminated in the publication of *Monnaie, Croissance, et Déséquilibre* (1985).

[56] GP 2 OF-IUF-G5.

[57] Goulven Rubin and I conducted D'Autume's interview on 26 March 2019, at Paris School of Economics.

However, like Barro and Grossman, D'Autume eventually abandoned fixed-price equilibrium models. He kept addressing economic growth (e.g., D'Autume and Michel, 1993a; 1993b; 1994), but within endogenous growth models *à la* Romer (1986).[58]

When asked whether Barro and Grossman influenced this decision, D'Autume replied "yes, but it was more general" (11/2019). D'Autume mentioned three problems with fixed-price equilibrium models. Their lack of rigor was the first one. Market price stickiness did not result from optimizing behavior. Then, D'Autume explained that fixed-price equilibrium models were not tractable. He particularly insisted on the difficulties to study the dynamic of non-clearing-markets. The multiplication of anticipated regimes forced to make heroic assumptions. For instance, to solve the model developed with Michel (1986), it was necessary to assume that the real wage and the interest rate were constant over time. Last but not least, D'Autume questioned the research potential of fixed-price equilibrium models. On one side, he indicated that while working on *Monnaie, Croissance, et Déséquilibre* (1985), he developed an interest in rational expectations. He eventually wrote a review, "Les anticipations rationnelles dans l'analyse macro-économique" (1986). D'Autume even tried to incorporate rational expectations into a fixed-price equilibrium model (D'Autume and Michel, 1985b). But it did not result in further developments. On the other side, D'Autume contrasted the dynamic properties of models with or without market-clearing. According to D'Autume, the instability of Walrasian equilibrium, obtained in several fixed-price equilibrium models (Plassard et al, 2021), was not "fruitful." Not only because the result lacked generality, but also because it was hard to build on it. By contrast, D'Autume claimed that endogenous growth models opened up new research perspectives. Thanks to the use of unit roots, it was possible to consider various equilibrium growth paths and explain why an economy could switch from one to another. This allowed addressing economic growth with a different perspective, which fascinated D'Autume. All this explained why he decided to abandon fixed-price equilibrium models. Barro and Grossman's criticisms of disequilibrium macroeconomics only provided an extra incentive.

D'Autume did not elaborate on Barro and Grossman's effect. However, it is possible to trace its origins. D'Autume just ended his PhD when Barro and Grossman spread the word that disequilibrium macroeconomics was a dead end. It might be important since one is probably more open to new modeling techniques at the beginning of a career than after spending a life working on one type of model. Moreover, at the time, Barro was the associated editor of the

[58] D'Autume's research interests went beyond economic growth. For more details, see Bertrand Wigniolle's "Tribute to Antoine d'Autume" (2019).

Journal of Monetary Economics (1976-1980), of *Econometrica* (1978-1981), and the editor of the *Journal of Political Economy* (1983-1985). Grossman, on his side, was in the editorial board of the *Journal of Monetary Economics* (1977-1983), and of the *American Economic Review* (1980-1983). On the other hand, a strong critic of fixed-price equilibrium models, Clower, was *American Economic Review* editor (1981-1985).[59] Under these conditions, it made sense for D'Autume to consider another approach to macroeconomics. It increased the chance of publishing in top American journals and, in turn, to boost his academic career. Therefore, more or less directly, Barro and Grossman contributed to the marginalization of disequilibrium macroeconomics and, simultaneously, to the domination of equilibrium macroeconomics.

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[59] In the afterword of *Money and Markets: Essays by Robert W. Clower* (Walker, 1984), Clower claimed: "Imagine my astonishment when a virtually distinct branch of economic theory began to develop from the dual-decision hypothesis and from the surprisingly similar Patinkin model of constrained supply. I refer, of course, to the fix-price models of Barro and Grossman, Drèze, Negishi, Grandmont, Benassy, Malinvaud, Varian, and other writers. Although I am an acknowledged 'grandfather' of all these 'babies', I disowned them at the 1980 Aix-en-Provence World Conference of the Econometric Society" (1984, p. 267). See Plassard (2017, 2018) for a systematic analysis of Clower's approach to disequilibrium macroeconomics.

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