

EDMOND MALINVAUD'S CRITICISMS OF THE NEW CLASSICAL ECONOMICS: RESTORING THE NATURE AND THE RATIONALE OF THE OLD KEYNESIANS' OPPOSITION

BY
MATTHIEU RENAULT

Unlike standard accounts, recent research in the history of macroeconomics has given increasing attention to the Old Keynesians' criticisms of the New Classical Economics. In this paper, I address the case of Edmond Malinvaud, who began opposing the New Classical Economics from the early 1980s and did so throughout the following thirty years. This study shows that his opposition was radical, i.e., multi-dimensional and systematic, and owes to the methodology and the practice of macroeconometric modeling. In turn, this twofold result sheds light on the nature and the rationale of the Old Keynesians' opposition to the New Classical Economics from the 1970s onwards, which can be interpreted along the same lines.

The theory of the 1960s did not fail because it was wrong but too elementary. The risk that is threatening us by now, and to which some of our colleagues badly resist, would be replacing this theory with a more elementary one and professed with much more dogmatism.

—Malinvaud, “Où en est la théorie macroéconomique?”¹

Matthieu Renault: Université Côte d'Azur. I am grateful to Pedro Duarte for his careful readings and sound advice. I benefited from the helpful comments of Richard Lipsey (who dropped his anonymity during the refereeing process), Mauro Boianovsky, Marcel Boumans, Antonella Rancan, Michaël Assous, Aurélien Goutsmedt, Francesco Sergi, Erich Pinzón-Fuchs, Matthieu Ballandonne, Adrien Villa, and participants at the HES (Chicago 2018), the ESHET (Madrid 2018), and the ANPEC conferences (Rio 2018), and seminars at the University São Paulo (FEA-USP) and Paris 1. Comments and suggestions by two anonymous referees, as well as by *JHET*'s editor Jimena Hurtado, helped improve the quality of this paper. Special thanks are due to Helena Ciorra for her excellent research assistance. Research funding from the Fundação de Amparo à Pesquisa do Estado de São Paulo (FAPESP) is gratefully acknowledged. The usual caveat applies.

¹Malinvaud (1982, p. 21). This and all subsequent translations of Edmond Malinvaud's publications in French are my own.

I. INTRODUCTION

At the turn of the 1980s, Edmond Malinvaud embarked on his campaign against the New Classical Economics without knowing that it would last thirty years. In doing so, he followed suit of most macroeconomists of his generation who are usually associated with the “Neoclassical Synthesis”—if that term ever meant something.² To be sure, however, they adhered by and large to the “Keynesian consensus” composed of the IS-LM model, the Phillips curve, the large-scale macroeconometric models, and a belief in the efficiency of stabilization policies (Mankiw 1990). This group of macroeconomists was in no way comprised of minor figures, but included James Tobin, Robert Solow, Franco Modigliani, Lawrence Klein, Richard Lipsey, Otto Eckstein, and others. Alternatively, this group could be labeled as the “Old Keynesians,” thus following the term that Tobin coined for himself to mark his distance from the New Keynesian Economics. Interestingly, he introduced himself that way at the symposium “Keynesian Economics Today” in 1993, organized by Gregory Mankiw likely to promote the label “New Keynesian” in modern macroeconomics: “In this symposium I shall play the role in which I was cast, *the unreconstructed old Keynesian*. Time was when I resisted labels and schools, naively hoping that our fledging science was outgrowing them. ... Considering the alternatives, I do not mind being billed as a Keynesian, an old Keynesian at that” (Tobin 1993, pp. 45–46; italics added).

The Old Keynesians’ opposition to the New Classical Economics has long been neglected in the history of macroeconomics. This is in great part due to the prevalence of the “standard narrative,” i.e., the historical account provided by contemporary practitioners (e.g., Mankiw 1990; Woodford 1999; Blanchard 2000).³ This standard narrative does not report any of the criticism made by the Old Keynesians. Instead, it asserts that Robert Lucas and his New Classical followers took the upper hand after the Neoclassical Synthesis had failed on both the empirical and theoretical grounds. Indeed, the latter had never been able to account for stagflation and was proven to lack sound microfoundations. The Neoclassical Synthesis then broke down, as if without provoking resistance within the profession. If the standard narrative insists on the crisis that macroeconomics went through since then and for the two subsequent decades, it concerns primarily the New Keynesian Economics’ fierce opposition to the New Classical Economics and the Real Business Cycles. According to this narrative, the New Keynesian Economics has thus been New Classical Economics’ actual opponent from the mid-1970s on, and the only approach capable of taking up the torch of Keynesianism and of reintroducing such concerns in rigorous modern macroeconomics.

In contrast to the standard narrative, recent works in the history of macroeconomics have dedicated increasing attention to the Old Keynesians’ criticisms and put into question

²The “Neoclassical Synthesis” is usually defined as the juxtaposition of Keynesian analysis in the short run and Solow’s neoclassical growth model in the long run. However, the exact meaning of that synthesis remained notoriously unclear, long after Samuelson coined it in the third edition of *Economics* (1955). Accordingly, it could refer to the never-achieved theoretical project to reconcile Keynesian and Walrasian analyses (De Vroey and Duarte 2013), or to a cover that merely served Samuelson to not be intimidated for his being Keynesian in the then critical US context (Giraud 2014). Noteworthy, a clearer meaning for that synthesis arose over the 1960s through the sixth edition of *Economics* (1964), along with the correspondence between Solow and Sen (Assous 2017).

³For a critical presentation of the standard narrative, see Duarte (2012), Hoover (2012), and Sergi (2020).

the mere vanishing of the Neoclassical Synthesis following New Classical Economics' assaults. First, it has been stressed that the Old Keynesians were among Lucas's early critics (De Vroey 2016, ch. 12; Da Silva 2017). Second, it has been documented that the Old Keynesians opposed the New Classical Economics from the early 1970s onwards, whether this criticism concerned the Lucas Critique and the reliability of the large-scale models or the Phillips curve (Goutsmedt et al. 2015, 2019; Goutsmedt 2019; Goutsmedt and Rubin 2018). Third, it has been shown that the Old Keynesians opposed the New Classical Economics for methodological concerns more than for ideological ones. This was notably the case of Modigliani (Rancan 2020), and was also quite clearly the case of Solow, whose opposition was primarily motivated by his "non-Walrasian" methodological position and his long-lasting concern for the development of "medium-run macroeconomics" (Ballandonne and Rubin 2020; Assous 2015).

In this paper, I study the case of Edmond Malinvaud to keep assessing the nature and the rationale of the Old Keynesians' opposition. From the early 1980s onwards, he criticized the New Classical Economics in a considerable number of publications, many of which were devoted entirely to this purpose (Malinvaud 1984, 1985a, 1990, 1991a, 1997a). Like most Old Keynesians, he did not make special distinction between the New Classical Economics and the Real Business Cycles, but considered them as parts of an identical doctrine. However, his most acerbic comments concerned this second approach, especially during the 1980s; this cannot be pure coincidence. Arguably, he realized at this very moment that the disequilibrium approach had little chance to take roots in the United States, hence, to influence the further development of modern macroeconomics. He said, "While my reflections during these past fifteen years were concentrating on the research program in disequilibrium macroeconomics, I witnessed with a good deal of dissatisfaction the main trends in macroeconomic theory that were occurring in American universities" (Malinvaud 1987a, p. 231).

The study of Malinvaud's criticisms reveals that his opposition to the New Classical Economics was radical, i.e., multi-dimensional and systematic. Indeed, he criticized many dimensions of this approach and did so systematically in opposing their postulates (i.e., market clearing and rational expectations) as well as several of the developments based on them. The systematic nature of Malinvaud's criticisms provides the main structure for this paper. Accordingly, section II shows his opposition to not only the market-clearing postulate but also the resulting dismissal of involuntary unemployment and the Real Business Cycles' model. Section III shows that he opposed not only the rational-expectations postulate but also the resulting Lucas Critique and the method of calibration. Section IV shows that Malinvaud opposed New Classical Economics' conception of policy making that resulted from the same postulates, namely the promotion of policy rules and structural policies.

In opposing the New Classical Economics, Malinvaud also disclosed his conception of macroeconomics. Beyond his attachment to the Neoclassical Synthesis and the "Keynesian consensus" (Malinvaud 1989a, 1989b, 2001), his conception is based on the methodology and the practice of macroeconometric modeling. This point is worth stressing, for Malinvaud is usually associated with the disequilibrium theory (Backhouse and Boianovsky 2013, pp. 136–140; De Vroey 2016, pp. 136–138). Early committed to the econometric methodology since his stay at the Cowles Commission (1950–51), he introduced this methodology through his teachings in

France over the 1950s and the 1960s. His textbook *Statistical Methods of Econometrics* is the result of those years of teaching (Malinvaud 1966). Attached to the Institut National de la Statistique et des Etudes Economiques (INSEE) throughout his career, he took part in setting up and developing large-scale structural macroeconomic models from the mid-1960s onward, for which he then became responsible as head of the INSEE (1974 to 1987).⁴ Arguably, this experience made Malinvaud endorse how the methodology of macroeconomic modeling evolved over the years, i.e., less dependent on an a priori model and opened to ad hoc specifications. Likewise, he endorsed the prevailing practice to rely on various empirical tools coming from the business studies (Armatte et al. 2017; Renault 2019). This conception of macroeconomics shall surface throughout the paper as being the rationale for his criticisms of the New Classical Economics.

The study of Malinvaud's criticisms also gives way to a by-product, as it sheds light on the nature and the rationale of the Old Keynesians' opposition to the New Classical Economics from the 1970s onwards. Accordingly, section V suggests that their opposition can be similarly characterized as radical and connected in various degrees to the methodology and the practice of macroeconomic modeling.

II. AGAINST MARKET CLEARING AND ITS FURTHER DEVELOPMENTS

The Market-Clearing Postulate

At the 1989 IEA (International Economic Association) conference, Malinvaud (1991a) dedicated his entire communication ("Incomplete Market Clearing") to discussing the issues raised by the market-clearing postulate in macroeconomics, which illustrates, at best, the way he opposed one of the two pillars of the New Classical Economics. To begin with, he notes that the market-clearing postulate raises both empirical and theoretical issues, which must be handled separately.⁵

From an empirical point of view, Malinvaud unequivocally rejects the market-clearing postulate, for it leaves aside most of the short-run phenomena made of disequilibria and various adjustments. He makes that statement in most papers in which he criticizes the New Classical Economics, and his book *Mass Unemployment* is entirely devoted to surveying and to discussing evidence on short-run phenomena (Malinvaud 1984). To support his statement, he regularly relied on the set of evidence raised by the New Classical Economics' critics over the 1970s: real wages vary little with unemployment, prices of manufactured goods are fairly insensitive to demand, or labor supply is nearly inelastic to real wages.

However, Malinvaud's privileged source of evidence for discarding market clearing stems from business survey data that provided raw information on producer and consumer experience in markets, gathered into business, consumer, and labor

⁴For more biographical details on Malinvaud's career, see Renault (2016, pp. 2–9).

⁵"Do markets clear? Should economic theories assume that markets clear? To these two questions a small minority of economists answer: 'Always.' Others think that non-market-clearing occurs and plays a significant role" (Malinvaud 1991a, p. 179).

force surveys.⁶ The INSEE implemented this method by the mid-1950s and kept developing it afterwards. From the early 1970s onwards, the practice of macroeconomic modeling relied on this business survey data through the “indicators of tensions.” In addition to this qualitative information, these indicators comprised quantitative information not reported in national accounts (such as the unemployment rate, the capacity utilization rate, or the number of vacant jobs) to assess the extent of disequilibria on markets.⁷ Relying on business survey data, Malinvaud argued that consumers regularly experienced queues, delivery dates, and spillover effects on substitute or imported goods in the short run. As for producers, they often declared suffering from either involuntary accumulation of stocks or underutilization of capacities. As a result, Malinvaud (1984, 1989b, 1991a, 1991c) concluded that the available data in macroeconomics were more than enough to discard the empirical relevance of the market-clearing postulate in the short run altogether.⁸

However, this conclusion did not imply that macroeconomics had better rely on any other kind of postulate based on the non-clearing market, which brings us to the theoretical issue raised by market clearing. In this respect, Malinvaud (1991a, p. 182) recalls that any attempt to replace this postulate is challenging, for its substitute is required to be logically consistent with the rest of the model and, in particular, with the rationality of agents. By these remarks, Malinvaud shows he was perfectly aware of consistency issues posed by the New Classical Economics’ “equilibrium discipline” (Lucas and Sargent 1979). In a nutshell, postulating the rationality of agents requires the market-clearing postulate since no mutually advantageous exchanges could be left unexploited by rational individuals. Faced with the equilibrium discipline, Malinvaud did not let himself get trapped and instead claimed his preferences for the methodology that previously prevailed in macroeconomics.

Such a stance forces anyone to take sides: to be for New Classical Macroeconomics and then to reject the earlier methodology, or to believe that the principle of this earlier methodology remains appropriate and to reject New Classical Macroeconomics. Being faced with such a dichotomy I believe that the 1960 line of attack on policy analysis is still the proper one. (Malinvaud 1994a, p. 10)

Malinvaud proposed three alternative ways to get rid of the market-clearing postulate. Noteworthy, he never relied on the common argument of the disequilibrium theory, according to which market clearing was not a sound microfoundation, since it hardly ever results from individual optimization in general equilibrium theory. Instead, he argues, first, that sub-optimal exchanges could be consistent with rationality, referring to ordinary explanations for the rigidity of wages and prices (menu costs, wage efficiency, implicit contracts, insiders-outsiders, and monopolistic competition) (Malinvaud 1984, 1991a, 1991c, 1994a). Second, he suggests softening the rationality of agents in stating that there is no valuable reason to postulate rationality in macroeconomics (Malinvaud 1989b, 1991a, 1995). The third way to get rid of market clearing illustrates at best his

⁶These data were obtained through interviews after the method developed by George Katona (Dechaux 2017).

⁷For more details on the use of business survey data in macroeconomic modeling, see Malinvaud (2000a).

⁸The term “available” is worth being emphasized since he regularly complained about the profession’s reluctance for considering these data as sound statistics (Malinvaud 1991a, 1991b, 1991c).

conception of macroeconomics and the one to which he most regularly refers (Malinvaud 1984, 1985b, 1985a, 1987a, 1989b, 1991a, 1991b, 1991c). In other words, he asserts that an empirical observation, if firmly established, should always dominate a theoretical proposal contradictory with data, even though its theoretical rationalization is still lacking.⁹ Noteworthy, Malinvaud justified the fixed-prices hypothesis in disequilibrium theory in similar terms. As we shall see, this argument primarily serves him to defend the methodology and the practice of macroeconometric modeling.

The Dismissal of Involuntary Unemployment

Besides its being unfounded, Malinvaud considered the market-clearing postulate applied to the labor market as “scandalous” (Malinvaud 1984, 1985b, 1991a). Indeed, this postulate leads to discarding any involuntary dimension in current unemployment. No need to say that this perspective made little sense to him, especially in a context of mass unemployment. He noted thus: “In countries affected by high unemployment, such as ours, we do not have to discuss much this point [the market-clearing postulate]” (Malinvaud 1998a, p. 336). However, he embarked on criticizing this perspective only because the New Classical Economics had started challenging mainstream macroeconomics in this regard.

What is important to us here is that the hypothesis of disequilibrium on the labor market is not unanimously accepted. Indeed, some economists believe that available statistical data are consistent with the equilibrium hypothesis.... The choice between these hypotheses is of great importance for modeling economic policies. For this reason, to decide which one of these two hypotheses is valid is crucial. (Malinvaud 1991c, p. 342)

Malinvaud also did not take the (statistical) rate of unemployment for an exact measurement of involuntary unemployment. In this respect, he used to recall that macroeconomics had long recognized some empirical relevance to “frictional unemployment.” However, the real novelty with the New Classical Economics consisted of the claim that unemployment could be reduced to frictional unemployment, job search included. To discredit that claim, he begins by accumulating evidence (Malinvaud 1984, 1985a, 1985b, 1991c). For instance, he brings the case of unskilled workers to the forefront and points out that their employment has steadily declined since the mid-1970s, whereas the labor supply elasticity of these workers is very likely the lowest of all, for they are paid the minimum legal wage no matter the job. Additionally, Malinvaud relies on the labor force survey to provide specific information on searched jobs.¹⁰ In a paper, he scrutinizes a series of factors usually taken as being responsible for frictional unemployment, such as an increase in unemployment benefits or upward legal protection for unskilled labor. To conclude, it would be complete nonsense to claim that these

⁹“The question is to know whether the [market-clearing] postulate is imperative or not: should we stick to it no matter what else? The only possible answer is no. The force of the postulate lies in observation and disappears as soon as the postulate is proved to be rejected by observation” (Malinvaud 1989b, p. 212).

¹⁰“The present French labour force survey permits us to follow a somewhat similar characteristic: to a question addressed to people looking for jobs concerning the kind of job sought, one entry for the answer is ‘anything’; between 1982 and 1985 the number of people choosing this entry exactly doubled, a fact that would be difficult to reconcile with the idea that the increase from 7.8 to 10.2 per cent in the unemployment rate could have resulted from increased claims of unemployed workers” (Malinvaud 1991b, p. 111).

factors, even when combined, are enough to explain the rise in unemployment in Western societies throughout the 1970s (Malinvaud 1985a).

Over the 1980s, Malinvaud regretted that the supporters of the New Classical Economics were not even trying to develop applications so that their notions of both frictional unemployment and job search could be subject to quantification (Malinvaud 1984, 1985b, 1985a). When this situation changed, thanks to Christopher Pissarides's achievements from the mid-1980s on, Malinvaud jumped on the case and looked into the new results (Malinvaud 1987b, 1994a, pp. 62–115). However, he was not more convinced, and he continued arguing that the rise in frictional unemployment, now captured by an upward shift in the Beveridge curve, was unable to explain the rise in unemployment since the 1970s (e.g., Malinvaud 1994a, p. 112). Interestingly enough, Malinvaud also embarked on elaborating an alternative modeling of the Beveridge curve to make room for involuntary unemployment in addition to frictional unemployment (Malinvaud 1987b, 1994a, 1994b). For this purpose, he poses an alternative set of definitions in which the rate of unemployment is composed of both disequilibrium and frictional unemployment. He defined the latter as the part of unemployment that cannot be reduced by stimulating the demand for labor. This part, in turn, is composed of both job search and “structural unemployment,” which results in differences in the composition of the labor supply and demand.

It is worth noticing that Malinvaud's attitude towards involuntary unemployment is in no way theoretical. In particular, his definition is very different from the disequilibrium theory, i.e., an excess of labor supply in a context of low real wages. Instead, he adopts a sort of statistical approach in which involuntary unemployment is reduced to the remaining part, not due to variations in frictional unemployment caused by changes either in job search rates or in the degree of mismatch on the labor market. However, this approach, Malinvaud claims, was closer to the one adopted by statisticians and applied macroeconomists, who are focused on measurable phenomena (e.g., Malinvaud 1994b). His ultimate goal was to address the latter's difficulties when dealing with the causes of unemployment. Accordingly, he provides tractable specifications for econometric applications to improve identification for frictional and involuntary unemployment phenomena (*ibid.*, pp. 257–259).¹¹ Malinvaud's peculiar defense of involuntary unemployment illustrates at best his confidence that theoretical disagreements can be resolved on the statistical ground in applied macroeconomics.

The Real Business Cycles Model

From the early 1980s on, Malinvaud opposed another development based on the market-clearing postulate, namely the Real Business Cycles (RBC) model. However, he did not blame their attempt to unify the short and long run in macroeconomics, because he regarded the Neoclassical Synthesis as equally unsatisfying in this respect. He even praised the achievement to account for business fluctuations, thanks to random shocks of

¹¹In this respect, he makes it clear elsewhere that new statistical conventions will be required: “the dividing line between frictional and disequilibrium unemployment and the corresponding precise definitions of the supply of labor and demand for it are not a priori clear, especially when one stands at the level of aggregate observations and analysis. Conventional rules become necessary” (Malinvaud 1991b, p. 110).

technical progress. Now, the very reason for his reluctance was in regard to the relevance given to this theoretical framework concerning actual macroeconomic phenomena.

This comment applies in particular to the group of articles dealing with the ‘real business cycles’ generated by exogenous shocks to technology under permanent and full market clearing, a representative of this group being that by Kydland and Prescott (1982). But one must decide on the weight to be given to results coming from this approach when one wants to understand actual macroeconomic phenomena. (Malinvaud 1991b, p. 107)

Furthermore, Malinvaud did not accept that the unification of the temporal dichotomy of Neoclassical Synthesis consists in folding up the long-run properties on the short-run ones (Malinvaud 1987a, 1989a, 1989b, 1991a, 1991b, 1998a). This point echoes his own understanding of growth theory after Solow (1956), which he considered valid for no other purposes than studying and measuring economic growth (e.g., Malinvaud 1991b). In this respect, he argues that a theory must provide sufficient approximation to the phenomenon to give way to empirical application. Accordingly, the Real Business Cycles and Solow models could hardly serve another purpose than studying the growth in the long run.¹² In particular, Malinvaud deplores that the former model bypassed all disequilibrium phenomenon occurring in the short run, whether real or monetary. He then recalls the same set of evidence bound to discard the empirical relevance of the market-clearing postulate in the short run (Malinvaud 1991a, 1991b, 1991c, 1997a, 1998a, 2000a, 2004).

For these reasons, Malinvaud regarded the Real Business Cycles model as hardly better suited than the apparatus it aimed at replacing, namely the temporal dichotomy of the Neoclassical Synthesis. Instead, he suggested two alternative ways of going beyond this apparatus, which are both connected with macroeconometric modeling. The first way is disequilibrium dynamics (Malinvaud 1984, 1989a, 1991c), which had started developing at the end of the 1970s. Unlike the Real Business Cycles, this approach took for granted that the short run was made of various disequilibria (though theoretically caused by fixed prices). Beyond the short run, the evolution of the economy resulted from changes in prices and quantities. Many dynamic ways of modeling were investigated, such as excess demand and supply, monetary assets, or investments and productive capacities. The hot question then was to determine whether and how such economies would converge towards the Walrasian stationary state or Solow’s growth path in the long run. In other words, the challenge was to refine (not to get rid of) the temporal dichotomy of the Neoclassical Synthesis by developing an analysis of “medium run” in macroeconomics. Noteworthy, disequilibrium dynamics not only was a theoretical project but also paved the way to econometric applications. Arguably, Malinvaud’s contributions to disequilibrium dynamics throughout the 1980s aimed at improving the performance of current macroeconomic models (Renault 2019; Plassard, Renault, and Rubin *forthcoming*).

The second way suggested by Malinvaud for going beyond the temporal dichotomy of the Neoclassical Synthesis is in close connection with macroeconometric modeling. Indeed, he proposes nothing but relying on what he calls the “adjustment laws”

¹²Note that Solow made it quite clear that his modeling of growth left aside many imperfect phenomena that matter in the short run (Solow 1956, pp. 91–94).

(Malinvaud 1982, 1984, 1987a, 1989b, 1991c, 1995, 1997b). This term refers to the empirically based relationships used to give a dynamic profile to the large-scale models.¹³ Malinvaud's typical illustration was the Phillips curve. In this respect, he often recalls the very empirical nature of this curve, regarded as such by most of the practitioners no matter the various attempts for rationalizing its theoretical underpinnings (Malinvaud 1987a, 1991b, 1991c, 1997b).¹⁴ Conversely to the behavioral laws or any theoretically deduced relationship, the adjustment laws result from observation and, therefore, cannot pretend to have the same explanatory character. For this reason, Malinvaud taunted New Classical Economics' statement that the adjustment laws were lacking microfoundations (Malinvaud 1997b). For him, the recourse to adjustment laws primarily resulted from many theoretical proposals, such as ones from general equilibrium theory, that were unreliable for the purpose of macroeconomic modeling. In this context, Malinvaud asserted that the practice had good reason to rely on such adjustment laws, and thus to ascribe a secondary role to economic theory.¹⁵

III. AGAINST RATIONAL EXPECTATIONS AND ITS FURTHER DEVELOPMENTS

The Rational-Expectations Postulate

As with market clearing, Malinvaud opposed the New Classical Economics' attempt to impose the rational-expectations postulate in macroeconomics. His reluctance did not concern the concept itself, which he described as a step forward, for it helped endogenize expectations. However, he urged the macroeconomist community to be vigilant and more critical of the whole literature this concept relied on (Malinvaud 1981, 1982). In particular, he fought against the spontaneous tendency to make the rational-expectations postulate, instead of market clearing, responsible for New Classical Economics' destructive results for economic policy. Noteworthy, Malinvaud's critical attitude towards rational expectations was motivated by its detrimental effects if introduced into disequilibrium models. Arguably, this point was not a matter of concern or division among the community involved in the disequilibrium theory.¹⁶ Malinvaud had thus no special reason to oppose rational expectations. He did, however, keep doing it long after the early 1980s (Malinvaud 1989b, 1990, 1991c, 1995, 2004). As we shall see, his long-standing opposition makes sense only with respect to the methodology and the practice of macroeconomic modeling.

¹³Malinvaud often recalls that large-scale macroeconomic models were comprised of three kinds of relationships: accounting identities, behavioral laws, and adjustment laws.

¹⁴Briefly, he confessed that he had always been impressed by the ability of the Phillips curve in describing—without explanation—the coevolution of wages and unemployment until the end of the 1960s. As this curve became unstable later on, his reaction was to encourage new empirical researches (Malinvaud 1991c, 1997b).

¹⁵“There is no shame in recognising this situation and in stating that the justification lies in observed regularities. The wrong thing to do, when one cannot fully explain a complex phenomenon, is to pretend the phenomenon is different so as to be able to easily explain it by maximising the behaviour” (Malinvaud 1987a, p. 236). For more details on this point, see Renault (2019).

¹⁶In particular, it has been showed that rational expectations do not put into question the existence of non-Walrasian equilibria and even enhance the efficacy of the government policy (e.g., Neary and Stiglitz 1983).

Interestingly, Malinvaud brings to the forefront the twofold nature of this concept, which is a statement about the actual behavior as much as it is a device, once introduced into a model. In the latter case, expectations are rational if consistent with the underlying theoretical model. Therefore, agents with rational expectations are using at best all available information, which includes not only present and past values of all relevant variables of the model but also the model itself. From there, Malinvaud notices that postulating rational expectations into a model implies that: (i) all agents share the same representation of the economy, provided they have the same model in mind; and (ii) the model is a good representation of the economy. As for (i), he asserted that claiming that agents have the same model in mind is highly disputable, given that even economists do not succeed in agreeing on a single model. As for (ii), he claims that New Classical Economics models portrayed the working of the economy as being so simplified that only a few macroeconomists would be willing to adhere to them.¹⁷

As for the other way to look at rational expectations—that is, as a claim on the actual behavior of agents—Malinvaud relied on a distinction between “pure theory” and “applied economics.” This distinction results from the purpose given to macroeconomics of helping to guide policy makers, giving rise to the development of “applied economics.” The latter branch became equivalent to the large-scale macroeconomic models in France from the mid-1960s onwards, which served for both forecasting and studying the effects of alternative policies. As they were in charge of helping to guide policy makers, practitioners used to test the realism of both hypotheses and results of any proposal prior to their implementation into the large-scale models. This methodology adopted in “applied economics” was in contrast with both the Cowles Commission approach and “pure theory,” for it conferred a crucial role to inductive validation. On this basis, Malinvaud claimed that rational expectations cannot be regarded in “pure theory” or “applied economics” in a similar way (Malinvaud 1990, 2004). While postulating rational expectations is legitimate and could be enlightening in mathematical economics, this postulate does not make any sense in the context of macroeconomic modeling. In such modeling, rational expectations are nothing but a hypothesis in which realism and reliability need to be tested, like any other proposal coming from “pure theory.”

There is no categorical imperative requiring us to give rationality a privileged place. In other words, from a methodological point of view, the true difficulty is to know how a hypothesis can be validated empirically or can be founded on the collective judgment of economic scholars. It is not to know what kind of special treatment should be given to hypotheses of rationality; the answer then is simple: none. (Malinvaud 1990, p. 11)

In line with his distinction, Malinvaud stated that discussions on the proper specification for expectations should be left to applied macroeconomists (Malinvaud 1982, 1987a). However, he kept an eye on the econometric literature dedicated to relative performances of adaptive and rational expectations throughout the 1980s, which he

¹⁷This claim is obviously rhetorical and reflects Malinvaud’s surprise that many macroeconomists ended up rallying under the New Classical Economics. The comments he provides on Lucas’s (1972) paper illustrate that point: “Why was such a result so often quoted as supporting the proposition that anticipated monetary policy was ineffective? Probably not because the model would have been found realistic in its representation of the economic structures or in its representation of economic behaviour. On both accounts it was obviously unrealistic. But the proposition was intuitively appealing” (Malinvaud 2004, p. 132).

reviewed later on (Malinvaud 1998b, pp. 1560–1593). Now, it is worth emphasizing that Malinvaud personally pushed for the adaptive expectations hypothesis because it caters well to the pragmatic attitude prevailing in macroeconomic modeling. First, he noted that adaptive expectations are flexible enough to cope with various contexts and can be amended if needed. Second, this hypothesis was consistent with adjustment processes at play in large-scale models (Malinvaud 1982, 1991c, 1995, 2004). Moreover, Malinvaud deemed that rational expectations are relevant only in troubled times marked by considerable shortages (e.g., wars and revolutions) (Malinvaud 1982, 1991c, 2004). Such an assessment, he claimed, resulted from his personal experience of the economic crisis that occurred in the 1970s.¹⁸

The Lucas Critique

In his article, Lucas (1976) argues that the large-scale models were unable to predict the effects of alternative economic policies correctly, due to their unsatisfying treatment of expectations. Relying on rational expectations, he showed through a few examples how a change in the policy rule induces a shift in the structural parameters of macroeconomic models. From the early 1980s onwards, Malinvaud regularly reacted to the Lucas Critique (Malinvaud 1980, 1987a, 1991c, 1997b, 1998a, 2007a, 2007b). At first, he conceded that large-scale models did not treat expectations in an appropriate manner at the time, but usually assumed them to be extrapolative (Malinvaud 1987a, 1991c, 2007b). He also paid tribute to the Lucas Critique, for it stimulated thinking of the theory of economic policy in less simplistic terms (Malinvaud 1991c, 1997b, 1998a, 2004). He even contemplated that a change in economic policies could modify expectations altogether in specific circumstances (Malinvaud 2007a). However, despite its growing influence in macroeconomics over the years, he never took the Lucas Critique for granted. If this critique is correct in principle, he claimed, its empirical relevance (i.e., its scope) is dubious in practice. “At the time, many macroeconomists, especially [me], were not convinced of the scope of [the Lucas Critique], although they recognized the correctness of the remark that inspired it. Indeed, the small illustrative models presented by Lucas and others showed no more than a possibility and were in no way tested as to their empirical validity” (Malinvaud 1997b, p. 21).

Accordingly, Malinvaud regarded the Lucas Critique as nothing but a theoretical proposal, which had to prove its empirical relevance. In this regard, he thought that the burden of the proof was on the shoulders of Lucas and his New Classical fellows. As none of them sought to evaluate the empirical relevance of the Lucas Critique in the aftermath, he then concludes that they were satisfied with that sole theoretical result (Malinvaud 1989b, 1991c, 1998a). He points out that the history of Western economies had already provided many examples of drastic changes in economic policies that could have served for this purpose. He thus mentioned the implementation of the European

¹⁸“Fanatics of rational expectations should think about the long delay which is necessary for Western public opinion to understand the new petroleum context and its consequences” (Malinvaud 1981, p. 1369n10). This statement got Malinvaud some acerbic comments from Lucas in the preliminary version of the preface for the Japanese edition of his *Models of Business Cycles*. “I suppose those who were expecting to hear a representative of what Edmund [sic] Malinvaud has referred to the ‘rational expectations fanatics’ were a little disappointed at the lectures’ rather technical tone, and the absence of any very new or startling recommendations for economic policy” (Lucas, Box 13, Folder: Models of Business Cycles 1985–1987).

Monetary System (1979), the deindexation of prices and wages in France (1983), and the shift of the control of money supply to the targeting of interest rates in several central banks throughout the 1980s.

More to the point, Malinvaud stood up against the claim that the Lucas Critique implies getting rid of the large-scale models, or that the rational-expectations postulate is the only way to make these models “immune” (Malinvaud 1991c, 1998a, 2007b). Conversely, he noted that assuming rational expectations was nothing but Lucas’s (1976) “theoretical solution” to his proper critique. Besides that, he recalled that Christopher Sims (1980) proposed an “empirical” solution in replacing the large-scale models by his VARs models. As for Malinvaud, he claimed that the Lucas Critique in no way implied getting rid of the large-scale macroeconomic models (Malinvaud 2007b). He thus kept looking at this critique as a theoretical statement empirically ill-founded. At the very best, the Lucas Critique could be interpreted positively as a word of caution while estimating in macroeconomic modeling. He also claimed that there was no particular reason to prefer rational expectations over adaptive expectations, unless this choice had not been supported by evidence (Malinvaud 1998a, 1998b, 2007b).¹⁹ In line with his assessment of rational expectations, he was also dubious that agents could take into account changes in monetary or fiscal policies before these changes affected them (Malinvaud 1998a).

The Method of Calibration

The method of calibration introduced by Finn Kydland and Edward Prescott (1982) did not directly result from the rational-expectations postulate. However, it emerged arguably as an a priori solution in the aftermath of Thomas Sargent’s difficulties for estimating rational expectations through structural econometrics methods (see, e.g., De Vroey 2016, pp. 278–279). Along this line, Malinvaud interpreted the emergence of calibration as a trick that allowed for preserving New Classical Economics’ postulates while confronting the model with empirical data. For this reason, he did not regard Real Business Cycles as better empirically grounded than New Classical Economics. Also, he portrayed the method of calibration as a step back for econometric theory: “Such practices represent a decline in requirements, when compared to what has brought progress in econometric theory for a half-century. It would be irresponsible of me to plead against the lessons that have been learned so far from advances in applied macroeconomics” (Malinvaud 1998ba, p. 337).

From the early 1990s on, Malinvaud took a stand against the method of calibration and its dissemination in macroeconomics (Malinvaud 1991c, 1997b, 1998a, 1998b, 2004, 2007b). Referring mainly to Peter Hansen and James Heckman (1996), he claimed that a few econometricians could reasonably promote calibration. In particular, Malinvaud opposed microeconomic estimations picked up in the literature and used to calibrate some microeconomic parameters of the RBC model, such as the elasticity of labor supply. According to him, these

¹⁹In his discussion on relative performances of both rational and adaptive expectations, Malinvaud dedicates a section to the empirical relevance of the Lucas Critique (Malinvaud 1998b, pp. 1563–1567).

microeconomic estimations are not only unlikely to be transferable to the macroeconomic level but also often too vague and disputable to be mobilized (Malinvaud 2004). Among others, he mentions the value of the elasticity of intertemporal substitution for labor supply.

More generally, Malinvaud deplores the lack of an explicit methodology for calibration to prevent this practice from generating perverse effects in applied macroeconomics (Malinvaud 1991c, 1997b, 2007b).²⁰ At first, he regrets that calibration does not frame the selection of values in econometric samples, which could deeply bias (intentionally or not) final results. Second, he fears that calibration focuses attention only on the model's performance and no longer on its parameter values. Third, he points out the risk that calibration depreciates any rigorous econometric investigations and possibly any empirical contribution. To prevent such perverse effects from occurring in applied macroeconomics, Malinvaud then promotes methodological rules for framing the practice of calibration. For instance, he suggests highlighting calibrated parameters, displaying all econometric sources and justifying the values then retained, and systematically testing the sensitivity of simulations to the parameter values of the model (Malinvaud 1991c, pp. 377–378).

Besides calibration, Malinvaud criticized the validation procedure of Real Business Cycles that consisted of comparing simulations to the so-called stylized facts. For him, the stylized facts, including time series, hardly summarized the empirical knowledge in macroeconomics (Malinvaud 1991c, 1997b, 1998a). On many occasions, Malinvaud fired against the tendency to focus on time series, whereas these data are not at all conclusive to discriminate between alternative theories. Instead, he pleaded for expanding the set of data in macroeconomics and, in particular, for including business survey data. Let us recall here that, for Malinvaud, this data was the most secure way to discard New Classical Economics' postulates.²¹

IV AGAINST THE NEW CLASSICAL APPROACH TO POLICY MAKING

Malinvaud also opposed the policy-making conception of New Classical Economics, namely the promotion of rules and structural policies. Instead of both time-inconsistent and underoptimal (if agents endowed with rational expectations) discretionary policies, the New Classical Economics had been promoting policy making based on fixed rules since Kydland and Prescott (1977). For instance, they promoted the monetarist rule reducing the money supply to a constant growth rate. In line with the application of the market-clearing postulate on the labor market, they had also promoted the natural rate of unemployment, since Lucas (1972), along with the idea that unemployment can be reduced substantially only by stamping out the labor market rigidities.

²⁰“[M]any supporters of this movement [RBC] substituted ‘calibrations’ for econometric estimates or relied on distorted tests that do not honor mathematical statistics” (Malinvaud 2007b, p. 420).

²¹“Although fairly rare now direct use of business survey results is valuable for business cycle research, so valuable that it should be more frequent. This is so because of the importance of the two aspects . . . namely the nature of market disequilibria and the formation of expectations or intentions of market participants” (Malinvaud 2000a, p. 13).

A Mere Return to Traditional Liberalism

In the early 1980s, Malinvaud confessed being puzzled by the New Classical claim that policy makers had better adopt rules over discretionary economic policies, regardless of business cycles (Malinvaud 1982). Remarkably, he did not change his mind while becoming more familiar with the literature on temporal inconsistency, optimal taxation, or the effectiveness of the monetary policy (Malinvaud 1987a, 1991c, 1997a, 1998b, 2004). He kept claiming that the government, thanks to their experts, had valuable knowledge of economic phenomena for regulating the economy. In a paper dedicated explicitly to the New Classical Economics' conception of policy making, he strove to identify the kind of regulation this school of thought was developing (Malinvaud 1997a). If he mocked their commitment to the Walrasian framework while analyzing economic policy issues, he noted that no one of this school ventured so far as to recommend implementing the perfect competitive equilibrium.

Few, if any, would go as far as saying that the Walrasian competitive equilibrium provides the perfect reference to the real world for the purpose and that economic policy should aim only at implementing the conditions required for this equilibrium. This is so even though, on the surface, it seems that the hinted conditions for good performance of market economies refer to Walrasian theory. (Malinvaud 1997a, p. 159)

In that context, Malinvaud concluded that the New Classical Economics' conception of economic policy marked a mere return to the *laissez-faire* that prevailed before WW II, besides undermining the state intervention.²² In this regard, he recalled that the superiority of this specific organization of markets had never been demonstrated in theory (Malinvaud 1997a). Moreover, he emphasized that the role of macroeconomics is to study market failures, not to convey the doctrinal idea that the market economy maximizes social well-being (Malinvaud 1991b, 1991c, 1997a, 1998a).

Structural Policies in Europe

Malinvaud not only opposed the New Classical Economics' approach to policy making, he also fired against the set of reforms to which this approach gave rise in Europe, known as "structural policies." From the mid-1990s on, Malinvaud indeed became more critical of European economic policies while the influential OECD (Organization for Economic Cooperation and Development) promoted the implementation of structural policies onto the labor market. Indeed, in the famous *Jobs Strategy* OECD report, five out of eight policies aimed to increase the flexibility of the labor market (OECD 1994). Highly critical of the radical shift in the strategy adopted by the OECD, Malinvaud continued scrutinizing their recommendations over the years (Malinvaud 2000b, 2003, 2009). In particular, he criticized the fact that the OECD based its recommendations on the concept of structural unemployment, inspired by New Classical Economics. In this

²²“The logic of policy intervention in modern market economies was also re-examined, starting from the so-called ‘Lucas critique’, or the so-called ‘Ricardian equivalence’, or still the so-called ‘Public Choice theory’” (Malinvaud 1997a, p. 161). Noteworthy, he regarded the Public Choice theory and the New Classical Economics as parts of a similar academic coalition that aimed at knocking down state intervention. In this regard, let us just recall that both promoted an economic constitutionalism.

respect, he argued that the econometric studies supporting the beneficial effects of structural policies on employment are neither complete nor conclusive to be transmitted to policy makers, referring in particular to Richard Layard, Stephen Nickell, and Richard Jackman (1991).²³ Furthermore, he pointed out that the OECD estimations of the natural rate of unemployment are highly disputable, probably no more relevant than a smoothing of the current unemployment rate corrected from short-run fluctuations.²⁴ In that context, he came to wonder whether the OECD had not merely converted itself into the “efficiency of markets” (Malinvaud 2000b, 2003). His impression would become even more accurate a few years later after this institution published two further reports on the benefits of competition in market economies.

[OECD reports] express a belief, maybe not dogmatic but well anchored, in the benefits of competition.... It sounds curious for an economist to see it is nowhere suggested that additional conditions are required for the competition to be beneficial. After all, competition is certainly not perfect ‘everywhere.’ ... [L]et me address a related question. Wouldn't there be an anti-state bias in some OECD corridors? (Malinvaud 2009, p. 32)

The most explicit mark of Malinvaud's opposition to liberal economic policies in Europe is the call he broadcast with Jacques Drèze, entitled “Growth and Employment: The Scope of a European Initiative,” also co-signed by many other European macroeconomists (Drèze and Malinvaud 1994). This call aimed to promote an alternative economic program in Europe and was addressed to both policy makers and European macroeconomists (*ibid.*, pp. 489–490). This program placed the return to full employment as the utmost priority and recommended a set of economic policies supporting both supply and demand sides. Noteworthy, the measures supporting the demand side included a public investment program enabled by a substantial reduction of real interest rates, which would be maintained as long as economic activity remained stagnant.

V WHAT ABOUT THE OLD KEYNESIANS?

The study of Malinvaud's criticisms sheds light on both the nature and rationale of the Old Keynesians' opposition to the New Classical Economics from the 1970s onward. Similarly, this opposition can be outlined as radical as well as connected in various degrees to the methodology and the practice of macroeconomic modeling. Furthermore, this twofold claim applies, though to a lesser extent, to a few figures of the younger generation who were all trained by Old Keynesians, such as: Alan Blinder (Solow); Robert J. Gordon (Solow); Stanley Fisher (Franklin Fisher); Benjamin Friedman (?); Willem Buiters (Tobin); Ray Fair (Solow); and Robert Shiller (Modigliani).

²³Malinvaud made a similar conclusion after reviewing the econometric literature dedicated to the role of unemployment insurance in the increase of the current unemployment (Malinvaud 1985c).

²⁴“Let's have enough bravery to face our doubts. With that in mind, I state that I do not know if today the French structural unemployment rate is 9 or 8, or maybe 5 percent” (Malinvaud 2003, p. 26).

Table 1. Malinvaud's Criticisms Raised by Old and Younger Keynesians

Edmond Malinvaud	Old Keynesians	Younger Keynesians
Market Clearing	Tobin; Solow; Modigliani; Klein; Lipsey	Blinder; Fischer; Friedman
Rational Expectations	Tobin; Solow; Modigliani; Klein; Lipsey; Eckstein	Blinder; Friedman; Fischer; Buiter; Schiller
Involuntary Unemployment	Tobin; Solow; Klein; Modigliani	Blinder
RBC Model	Tobin; Solow; Klein; Modigliani; Lipsey	Blinder
Lucas Critique	Tobin; Solow; Klein; Eckstein	Blinder; Fischer; Fair; Gordon; Buiter
Phillips Curve	Solow; Klein; Lipsey; Modigliani	Blinder; Gordon; Friedman
Calibration	Solow; Klein	Fair
Rules	Tobin; Solow; Klein; Modigliani	Blinder; Buiter
Natural Unemployment / Structural Policies	Tobin; Solow; Lipsey; Modigliani	Blinder; Gordon; Fair

Table 1 displays how much the Old Keynesians' criticisms overlap with Malinvaud's.²⁵ In each particular case, at least four Old Keynesians opposed the New Classical Economics on similar terms (except for calibration). Alike, they criticized the latter approach on several aspects. In this respect, let us stress that Solow expressed all of Malinvaud's criticisms, while three other figures expressed almost all of them (Tobin, Modigliani, and Klein). Also, many Old Keynesians also opposed the New Classical Economics on two additional aspects. Indeed, they fired against the representative agent used in Real Business Cycles (e.g., Solow), and the spurious claim that they had not been able to account for the stagflation phenomenon throughout the 1970s.²⁶ As a result, it is not an undue generalization to argue that the Old Keynesians' opposition to the New Classical Economics was radical, i.e., multi-dimensional and systematic. The same set of

²⁵This table is mainly based on recent research in the history of macroeconomics (Assous 2015; Backhouse and Cherrier 2019; Ballandonne and Rubin 2020; Da Silva 2017; De Vroey and Duarte 2013; De Vroey 2016; Goutsmedt 2019; Goutsmedt et al. 2015, 2019; Goutsmedt and Rubin 2018; Rancan 2020). To complete it, a few pieces of primary literature have been considered (Buiter 1980; Blinder 1986, 1987, 1988, 2001; Fair 1992; Klein 1992; Lipsey 2001, 2000, 2016; Tobin 1972, 1977, 1980a, 1980b, 1984, 1992, 1993, 1995; Solow 1978, 1983, 1984, 1985, 2008).

²⁶Arguably, the Old Keynesians proposed ways to explain stagflation from the mid-1970s, whether through the AS-AD model or through the introduction of additional variables such as oil prices into the macroeconomic models (see Goutsmedt 2019). In the same way, Malinvaud suggested ways to account for stagflation from the 1970s onward. In *The Theory of Unemployment Reconsidered*, he already stressed that there was no paradox for a simultaneous occurrence of unemployment and inflation, since a rise in both wages and prices can result from expectations and social tensions without any excess demand on the labor market (Malinvaud 1977, pp. 104–107).

observations applies to a few figures of the younger generation, though to a lesser extent, since only Blinder's criticisms closely overlap with Malinvaud's.

The second claim that the rationale of the Old Keynesians' opposition takes roots in the methodology and the practice of macroeconomic modeling is more delicate to substantiate. This claim is not disputable for the Old Keynesians involved in the setting up of the large-scale models while they were criticizing the New Classical Economics (Rancan 2020; Goutsmedt 2019; Goutsmedt et al. 2015, 2019). This was the case of Klein, involved in Project LINK since 1968. This was also the case of both Eckstein and Modigliani, who were, respectively, developing the DRI model (1969 to 1979) and the FRB-MIT-Penn model (1966 to 1970).²⁷ By contrast, this claim is more disputable for the Old Keynesians who were not involved in the setting up of such models from the 1970s on, namely Tobin, Solow, and Lipsey. In this respect, let us note that at least Tobin and Solow had been very close to the Cowles Commission in their early years. More to the point, they both stood up for defending the large-scale macroeconomic models while under the fire of New Classical Economics. While Tobin (1984) strongly dismissed the so-called irreparable damages caused by these attacks, Solow saw no reason to get rid of these models: "I share Franco Modigliani's view that the alarmism ... simply doesn't square with what in fact actually happened. If you give grades to all the standard models, some will get a B and some a B minus on occasion, especially for wage equations, but I don't see anything in that record that suggests suicide" (Solow 1978, p. 204).²⁸

This second claim also applies to the younger generation of Keynesians, though to a lesser extent. In this respect, we can argue that fewer of them had been directly involved in the development of large-scale models (Fair, Fisher, and Shiller). However, they strikingly opposed New Classical Economics in a very Old Keynesian fashion throughout the 1970s and 1980s. This is particularly the case of Blinder, who long claimed his attachment to the Neoclassical Synthesis, of which he stressed the crucial role of the macroeconomic models (Blinder 1986, 1987, 1988). In this respect, he claimed that most significant contributions to macroeconomics up to the 1970s were related to the large-scale models, such as Klein-Goldberger's model of the US economy (1955), or Tobin's work on the demand for money (1958) (Blinder 2001, p. 111).

The table above hardly illustrates this second claim, however. Therefore, some criticisms are more detailed to display how they are connected to the methodology and the practice of macroeconomic modeling. First, let us note that the Old Keynesians never opposed the market-clearing and rational-expectations concepts. They merely refused to consider them as postulates, arguing that macroeconomics dealt only with hypotheses selected after their empirical relevance.²⁹ For this very reason, none of them took for granted New Classical Economics' results on economic policy (Assous 2015;

²⁷In his 1977 AEA Presidential Address, Modigliani (1977) opposed New Classical Economics' main statements by relying on the results of the FRB-MIT-Penn model.

²⁸Solow concludes this talk in being confident that the large-scale models could still get improved in the future. Noteworthy, he also expressed in his obituary for Eckstein his sympathetic (though more balanced) view on the large-scale models, especially about the DRI model to which he confessed having been addicted (Solow 1985).

²⁹Solow's statement on rationality and rational expectations illustrates at best the Old Keynesians' attitude. "The assumption of conventional rationality has to earn its wings every day, as Mr. Bormann of Eastern Airlines would say; and if it doesn't earn its wings it is not entitled to fly [laughter]" (Solow 1984, p. 141).

Ballandonne and Rubin 2020; Rancan 2020). As for rational expectations, it is worth emphasizing that most Old Keynesians recognized the treatment of agents' expectations as unsatisfying in large-scale models. Still, this observation did not make them more willing to adopt the rational expectations postulate. Instead, they saw good reason for moving forward, and some even embarked on evaluating the actual state of agents' expectations after George Katona's method (Goutsmedt et al. 2015; Goutsmedt 2019).

Second, many Old Keynesians opposed the RBC model in a similar way as Malinvaud. The most significant one was Solow, who often stated that this model and his own growth model alike were relevant only for studying growth in the long run (Assous 2015; Ballandonne and Rubin 2020). Interestingly, Solow was not satisfied by the temporal dichotomy of the Neoclassical Synthesis either and strove to develop dynamic connections between short- and long-run modeling (Assous 2015). His approach was quite close to Malinvaud's line of attack. Indeed, Solow took for granted that the short run was made of various disequilibria, which he rationalized with imperfect competition. Accordingly, he instead focused on portraying how the economy evolved across subsequent periods, thanks to variations in capital accumulation, thus developing "medium-run macroeconomics" as well. Similar observations can be drawn on the dynamic approach developed by Modigliani in the 1970s (Rancan 2020).

Third, many Old Keynesians opposed the Lucas Critique along the same lines as Malinvaud did. Indeed, they recognized that Lucas's point was correct in principle and that it rightly pointed out the loose treatment of expectations in large-scale models at the time. However, none of them saw any valuable reason to discard those models for studying the effects of alternative economic policies. To put it differently, they never took for granted the empirical relevance of the Lucas Critique and asked for further evidence (Goutsmedt et al. 2015). In this respect, the manner in which the younger generation of Keynesians reacted to that critique was not fundamentally different, except for the fact that they embarked on testing it and cast doubts on its empirical relevance (Goutsmedt et al. 2019). For instance, Robert Gordon argued that the rejection of large-scale models was an extreme and unnecessary conclusion while he discussed Lucas's paper at the Carnegie-Rochester Conference Series on Public Policy in 1976, and in the series of letters he exchanged with Lucas afterward (Da Silva 2017, pp. 7–8).

Fourth, the Old Keynesians and the younger generation stood up for the Phillips curve on similar terms. Like Malinvaud, they considered the Phillips curve as being primarily an empirical relationship, in no way stable in the long run. For this reason, they could hardly believe it offered an inflation-unemployment trade-off for policy makers. Accordingly, the reaction of Old Keynesians in face of the instability of the Phillips curve in the 1970s was not one of great surprise. This is particularly the case of Lipsey, who recently wished to document that empirical attitude that prevailed throughout the 1960s (Lipsey 2001, 2016). Following this line, Klein and a few Keynesians of the younger generation (Blinder and Gordon) embarked on identifying the shifts of the Phillips curve by the end of the 1970s. They showed that one could retrieve such an empirical regularity in the short run by taking into account additional variables such as oil or import prices. As a result, the Phillips curve could no longer be depicted in two traditional dimensions, as it turned into a multivariate relationship (Goutsmedt et al. 2019; Goutsmedt 2019; Goutsmedt and Rubin 2018).

VI. CONCLUDING REMARKS

Malinvaud's opposition to the New Classical Economics was nothing but radical, i.e., both multi-dimensional and systematic. He criticized each dimension of this approach and did it systematically, opposing not only its postulates but also derived developments. In so doing, Malinvaud unveiled his alternative conception of macroeconomics. Strikingly, none of his criticism was really inspired by the disequilibrium theory, including his opposition to market clearing and his defense of involuntary unemployment. By contrast, Malinvaud usually brings to the forefront the inductive-validation method and the assessment of theoretical proposals applied in macroeconomic modeling.

Thanks to the dual characteristic of Malinvaud's criticisms, they help shed a light on a more general point: the nature and rationale of the Old Keynesians' opposition to New Classical Economics. The same applies to a few Keynesians of the younger generation, though to a lesser extent. Based on the overlapping of Malinvaud's and other Old Keynesians' criticisms, it can be argued that the latter's opposition was radical as well and somewhat connected to the methodology and the practice of macroeconomic modeling. Surely, this result is everything but a definite one. Accordingly, it shall be taken as a call for further research, since some of the figures covered in this paper have not been yet systematically researched (e.g., Tobin, and most Keynesians of the younger generation). Furthermore, other figures not tackled here would deserve similar attention (e.g., Albert Ando or Michael Rothschild).

If valid, however, this proposal about the Old Keynesians (and the younger generation of Keynesians) could shed a different light on the history of macroeconomics. In the first place, it would put the reliability of the large-scale macroeconomic models at the center of debates in macroeconomics throughout the 1970s and 1980s. As these models provided the Old Keynesians with a methodological framework to think in terms of "applied economics" and dealing with economic policy issues, they turned into the actual target of New Classical Economics in their crusade against mainstream macroeconomics. Such a confrontation took place at the conference "After the Phillips Curve" (1978), where both camps crystallized their oppositions in regard to the large-scale macroeconomic models (Goutsmedt 2019).

Second, this proposal about the Old Keynesians could help revise the meaning of the Neoclassical Synthesis from the 1970s onwards. In place of the theoretical project to reconcile Walrasian and Keynesian analyses or the series of elements comprised of the "Keynesian consensus" patchwork, this term would primarily refer to the methodology and the practice of macroeconomic modeling. Arguably, if the Neoclassical Synthesis meant something at the time, it would have been incorporated into the large-scale models—as shown in the case of Malinvaud. Noteworthy, this redefinition is in line with what Michel De Vroey and Pedro Duarte (2013, pp. 21–23) call the "second understanding of the Neoclassical Synthesis," which rather relied on a "methodological principle" from the 1970s onward. This methodological principle could well be nothing but the methodology and the practice of macroeconomic modeling.

REFERENCES

- Armatte, Michel, Annie Cot, Jacques Mairesse, and Matthieu Renault. 2017. "Edmond Malinvaud and the Problem of Statistical Induction." *Annals of Economics and Statistics* 125–126: 79–111.
- Assous, Michaël. 2015. "Solow's Struggle with Medium-Run Macroeconomics, 1956–95." *History of Political Economy* 47 (3): 395–417.
- . 2017. "Review of Michel de Vroey's A History of Macroeconomics from Keynes to Lucas and Beyond." *Erasmus Journal for Philosophy and Economics* 10 (1): 112–119.
- Backhouse, Roger E., and Mauro Boianovsky. 2013. *Transforming Modern Macroeconomics: Exploring Disequilibrium Microfoundations, 1956–2003*. Cambridge: Cambridge University Press.
- Backhouse, Roger E., and Beatrice Cherrier. 2019. "The Ordinary Business of Macroeconometric Modeling: Working on the Fed-MIT-Penn Model, 1964–74." *History of Political Economy* 51 (3): 425–447.
- Ballandonne, Matthieu, and Goulven Rubin. 2020. "Robert Solow's Non-Walrasian Conception of Economics." *History of Political Economy* 52 (5): 827–861.
- Blanchard, Olivier. 2000. "What Do We Know about Macroeconomics that Fisher and Wicksell Did Not?" *Quarterly Journal of Economics* 115 (4): 1375–1409.
- Blinder, Alan. 1986. "Keynes after Lucas." *Eastern Economic Journal* 12 (3): 209–216.
- . 1987. "Keynes, Lucas, and Scientific Progress." *American Economic Review* 77 (2): 130–136.
- . 1988. "The Fall and Rise of Keynesian Economics." *Economic Record* 64 (4): 278–294.
- . 2001. "Keeping the Keynesian Faith." Edited by Brian Snowdon. *World Economics* 2 (2): 105–140.
- Buiter, Willem. 1980. "The Macroeconomics of Dr. Pangloss: A Critical Survey of the New Classical Macroeconomics." *The Economic Journal* 90 (357): 34–50.
- Da Silva, Danilo. 2017. "The First Keynesian Reactions to Lucas's Macroeconomics of Equilibrium." Paper presented at the the Center for the History of Political Economy, Duke University. Available online: <https://hope.econ.duke.edu/node/1342>.
- De Vroey, Michel. 2016. *A History of Macroeconomics from Keynes to Lucas and Beyond*. Cambridge: Cambridge University Press.
- De Vroey, Michel, and Pedro Duarte. 2013. "In Search of Lost Time: The Neoclassical Synthesis." *The B.E. Journal of Macroeconomics* 13 (1): 965–995.
- Dechaux, Pierrick. 2017. "L'économie face aux enquêtes psychologiques 1944-1960 : unité de la science économique, diversité des pratiques." PhD, diss. Paris 1 Université Panthéon-Sorbonne.
- Drèze, Jacques, and Edmond Malinvaud. 1994. "Growth and Employment: The Scope of a European Initiative." *European Economic Review* 38 (3): 489–504.
- Duarte, Pedro. 2012. "Not Going Away? Microfoundations in the Making of a New Consensus in Macroeconomics." In Pedro Duarte and Gilberto Lima, eds., *Microfoundations Reconsidered*. Cheltenham, UK: Edward Elgar Publishing, pp. 190–237.
- Fair, Ray. 1992. "The Cowles Commission Approach, Real Business Cycle Theories, and New-Keynesian Economics." In Michael Belongia and Michelle Garfinkel, eds., *The Business Cycle: Theories and Evidence*. Dordrecht: Springer, pp. 133–157.
- Giraud, Yann. 2014. "Negotiating the 'Middle-of-the-Road' Position: Paul Samuelson, MIT, and the Politics of Textbook Writing, 1945–55." *History of Political Economy* 46 (suppl. 1): 134–152.
- Goutsmedt, Aurélien. 2019. "Macroeconomics at the Crossroads: Stagflation and the Struggle between 'Keynesian' and New Classical Macroeconometric Programs." OSF Preprints. doi:10.31219/osf.io/y364t.
- Goutsmedt, Aurélien, Erich Pinzón-Fuchs, Matthieu Renault, and Francesco Sergi. 2015. "Criticizing the Lucas Critique: Macroeconometricians' Response to Robert Lucas." SSRN Working Paper 2837766. Rochester, NY.
- . 2019. "Reacting to the Lucas Critique: The Keynesians' Pragmatic Replies." *History of Political Economy* 51 (3): 535–556.

- Goutsmedt, Aurélien, and Goulven Rubin. 2018. "Robert J. Gordon and the Introduction of the Natural Rate Hypothesis in the Keynesian Framework." *History of Economic Ideas* 26 (3): 157–187.
- Hansen, Peter, and James Heckman. 1996. "The Empirical Foundations of Calibration." *Journal of Economic Perspectives* 10 (1): 87–104.
- Hoover, Kevin. 2012. "Microfoundational Programs." In Pedro Duarte and Gilberto Lima, eds., *Microfoundations Reconsidered*. Cheltenham, UK: Edward Elgar Publishing, pp. 19–61.
- Klein, Lawrence. 1992. "Some Tendencies in Modern Econometrics." *Indian Economic Review* 27: 5–14.
- Kydland, Finn, and Edward Prescott. 1977. "Rules Rather than Discretion: The Inconsistency of Optimal Plans." *Journal of Political Economy* 85 (3): 473–492.
- . 1982. "Time to Build and Aggregate Fluctuations." *Econometrica* 50 (6): 1345–1370.
- Layard, Richard, Stephen Nickell, and Richard Jackman. 1991. *Underemployment: Macroeconomic Performance and the Labour Market*. Oxford: Oxford University Press.
- Lipsey, Richard. 2000. "IS-LM, Keynesianism, and the New Classicism." In Roger Backhouse and Andrea Salanti, eds., *Macroeconomics and the Real World*. Volume 2. Oxford: Oxford University Press, pp. 57–82.
- . 2001. "Successes and Failures in the Transformation of Economics." *Journal of Economic Methodology* 8 (2): 169–201.
- . 2016. "The Phillips Curve and an Assumed Unique Macroeconomic Equilibrium in Historical Context." *Journal of the History of Economic Thought* 38 (4): 415–429.
- Lucas, Robert. 1972. "Expectations and the Neutrality of Money." *Journal of Economic Theory* 4 (2): 103–124.
- . 1976. "Econometric Policy Evaluation: A Critique." *Carnegie-Rochester Conference Series on Public Policy* 1 (1): 19–46.
- Lucas, Robert, and Thomas Sargent. 1979. "After Keynesian Macroeconomics." In *After the Phillips Curve: Persistence of High Inflation and High Unemployment*. Boston: Federal Reserve Bank of Boston, pp. 29–46.
- Malinvaud, Edmond. 1966. *Statistical Methods of Econometrics*. Chicago: Rand McNally.
- . 1977. *The Theory of Unemployment Reconsidered*. Oxford: Basil-Blackwell.
- . 1980. *Profitability and Unemployment*. Cambridge: Cambridge University Press.
- . 1981. "Econometrics Faced with the Needs of Macroeconomic Policy." *Econometrica* 49 (6): 1363–1375.
- . 1982. "Où en est la théorie macroéconomique?" *Revue Européenne Des Sciences Sociales* 20 (62): 5–22.
- . 1984. *Mass Unemployment*. Oxford: Wiley-Blackwell.
- . 1985a. "Le marché du travail en déséquilibre." In Alain Barrère, ed., *Keynes aujourd'hui: théories et politiques*. Paris: Economica, pp. 462–477.
- . 1985b. "Sur l'analyse macroéconomique du chômage." *L'Actualité Économique* 61 (2): 147–170.
- . 1985c. "Unemployment Insurance." *The Geneva Papers on Risk and Insurance* 10 (34): 6–22.
- . 1987a. "The Challenge of Macroeconomic Understanding." *PSL Quarterly Review* 40 (162): 219–238.
- . 1987b. "La courbe de Beveridge." In AFSE, ed., *Flexibilité, Mobilité et Stimulants économiques*. Paris: Nathan, pp. 59–77.
- . 1989a. "Lessons to Be Learned from the European Unemployment of the 80s." *The Pakistan Development Review* 28 (4): 311–333.
- . 1989b. "Observation in Macroeconomic Theory Building." *European Economic Review* 33 (2–3): 205–223.
- . 1990. "Microéconomie et macroéconomie relèvent-elles des mêmes principes méthodologiques?" In AFSE, ed., *La Méthodologie de l'économie théorique et appliquée aujourd'hui*. Paris: Nathan, pp. 3–13.
- . 1991a. "Incomplete Market Clearing." In Lionel McKenzie and Stefano Zamagni, eds., *Value and Capital: Fifty Years Later*. London: Macmillan, pp. 179–196.

- . 1991b. “Macroeconomics and Market Failures.” In *Information, Strategy and Public Policy*. Oxford: Wiley-Blackwell, pp. 104–123.
- . 1991c. *Voies de la recherche macroéconomique*. Paris: Odile Jacob.
- . 1994a. *Diagnosing Unemployment*. Cambridge: Cambridge University Press.
- . 1994b. “Statistiques, analyse économique et économétrie pour la connaissance de l’emploi.” *Swiss Journal of Economics and Statistics* 130 (3): 251–265.
- . 1995. “Sur l’hypothèse de rationalité en théorie macro-économique.” *Revue Économique* 46 (3): 523–536.
- . 1997a. “The Proper Role of Theory.” In Peter Van Bergeijk, ed., *Economic Science and Practice: The Roles of Academic Economists and Policy-Makers*. Cheltenham, UK: Edward Elgar Publishing, pp. 149–165.
- . 1997b. “L’économétrie dans l’élaboration théorique et l’étude des politiques.” *L’Actualité Économique* 73 (1–2–3): 11–25.
- . 1998a. “La modélisation en macroéconomie appliquée: quarante ans après.” *Brussels Economic Review* 160 (4): 329–342.
- . 1998b. *Macroeconomic Theory: A Textbook on Macroeconomic Knowledge and Analysis*. Three volumes. Amsterdam: North-Holland.
- . 2000a. “The Role of Business Survey Data in Macroeconomic Research and Practice.” In Günter Poser and Daniel Bloesch, eds., *Economic Surveys and Data Analysis*. Paris: OECD, pp. 11–18.
- . 2000b. “The Role of Structural Policies Meant to Promote Employment.” *Rivista Di Politica Economica* 90 (12): 9–32.
- . 2001. “Some Ethical and Methodological Convictions.” *The American Economist* 45 (1): 3–16.
- . 2003. “Réformes structurelles du marché du travail et politiques macroéconomiques.” *Revue de l’OFCE* 3 (86): 7–30.
- . 2004. “The Difficult Dialogue between the Development of Macroeconomic Theory and Macroeconomic Policy Concerns.” In Vela Velupillai, ed., *Macroeconomic Theory and Economic Policy: Essays in Honour of Jean-Paul Fitoussi*. London: Routledge, pp. 128–144.
- . 2007a. “About the Role, in Older Days, of Econometrics in Quantitative Economics.” *European Journal of the History of Economic Thought* 14 (3): 423–448.
- . 2007b. “Quelle place donner maintenant à la macroéconomie dans l’enseignement de l’économétrie?” *Revue d’économie Politique* 117 (3): 411–430.
- . 2009. “La réévaluation de la stratégie de l’OCDE pour l’emploi: deux pistes d’amélioration.” *Travail et Emploi* 118: 29–35.
- Mankiw, Gregory. 1990. “A Quick Refresher Course in Macroeconomics.” *Journal of Economic Literature* 28 (4): 1645–1660.
- Modigliani, Franco. 1977. “The Monetarist Controversy or, Should We Forsake Stabilization Policies?” *American Economic Review* 67 (2): 1–19.
- Neary, Peter, and Joseph Stiglitz. 1983. “Toward a Reconstruction of Keynesian Economics: Expectations and Constrained Equilibria.” *Quarterly Journal of Economics* 98 (suppl.): 199–228.
- OECD. 1994. *The OECD Jobs Study: Facts, Analysis, Strategies*. Paris: OECD.
- Plassard, Romain, Matthieu Renault, and Goulven Rubin. Forthcoming. “Modeling Market Dynamics: Jean-Pascal Bénassy, Edmond Malinvaud, and the Development of Disequilibrium Macroeconomics.” *History of Economic Ideas*.
- Rancan, Antonella. 2020. “From Dynamics to Stabilisation: Albert Ando and Franco Modigliani’s Contributions to the Theory of Economic Growth and Fluctuations (1959–1970).” *European Journal of the History of Economic Thought* 27 (4): 635–660.
- Renault, Matthieu. 2016. “Edmond Malinvaud, entre science et action. Chronique d’une macroéconomie en transition.” PhD diss., Paris 1 Université Panthéon-Sorbonne.
- . 2019. “Theory to the Rescue of the Macroeconometric Models: Edmond Malinvaud’s Alternative View on Microfoundations.” *SSRN Working Paper 3434008*. Rochester, NY.

- Sergi, Francesco. 2020. "The Standard Narrative about DSGE models in Central Banks' Technical Reports." *European Journal of the History of Economic Thought* 27 (2): 163–193.
- Sims, Christopher A. 1980. "Macroeconomics and Reality." *Econometrica* 48 (1): 1–48.
- Solow, Robert. 1956. "A Contribution to the Theory of Growth." *Quarterly Journal of Economics* 70 (1): 65–94.
- . 1978. "Summary and Evaluation." In *After the Phillips Curve: Persistence of High Inflation and High Unemployment*. Boston: Federal Reserve of Boston, pp. 203–209.
- . 1983. "Cowles and the Tradition of Macroeconomics." Paper presented at the Cowles Fiftieth Anniversary Celebration, June 3. <https://cowles.yale.edu/sites/default/files/files/conf/50th/50th-solow.pdf>. Accessed September 13, 2020.
- . 1984. "Interview with Robert Solow." In Arjo Klamer, ed., *Conversations with Economists*. Totowa, NJ: Rowman & Allenheld, pp. 127–148.
- . 1985. "Reflections on Macroeconomic Modeling: Confessions of a DRI Addict." *Eastern Economic Journal* 11 (1): 79–83.
- . 2008. "Comment." *Journal of Economic Perspectives* 22 (1): 243–249.
- Tobin, James. 1972. "Inflation and Unemployment." *American Economic Review* 62 (1): 1–18.
- . 1977. "How Dead Is Keynes?" *Economic Inquiry* 15 (4): 459–468.
- . 1980a. "Are New Classical Models Plausible Enough to Guide Policy?" *Journal of Money, Credit and Banking* 12 (4): 788–799.
- . 1980b. *Asset Accumulation and Economic Activity: Reflections on Contemporary Macroeconomic Theory*. Chicago: University of Chicago Press.
- . 1984. "Interview with James Tobin." In Arjo Klamer, ed., *Conversations with Economists*. Totowa, NJ: Rowman & Allenheld, pp. 97–113.
- . 1992. "An Old Keynesian Counterattacks." *Eastern Economic Journal* 18 (4): 387–400.
- . 1993. "Price Flexibility and Output Stability: An Old Keynesian View." *Journal of Economic Perspectives* 7 (1): 45–65.
- . 1995. "The Natural Rate as New Classical Macroeconomics." In Rod Cross, ed., *The Natural Rate of Unemployment: Reflections on 25 Years of the Hypothesis*. Cambridge: Cambridge University Press, pp. 32–42.
- Woodford, Michael. 1999. "Revolution and Evolution in Twentieth-Century Macroeconomics." In *Frontiers of the Mind in the Twenty-First Century*. Washington DC: U.S. Library of Congress.