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Romain Plassard

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# LEDa WORKING PAPERS

## The Applied General-Equilibrium Program of the ENSAE's Band

Romain PLASSARD

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# The Applied General-Equilibrium Program of the ENSAE's Band<sup>1</sup>

## Abstract

What was driving the estimation of general-equilibrium models with rationing (GEMR)? Our article explores this issue by focusing on the research led by a group of French econometricians aka the “ENSAE's Band” (named in reference to the National School of Statistics and Economic Administration). We show that the estimation of GEMR aimed to empirically discriminate between equilibrium and disequilibrium macroeconomics and provide a policy analysis tool. Since both projects required addressing the microfoundations of macroeconomics, our article also shows that there was an applied side to what Kevin D. Hoover called the “general-equilibrium program” (2012).

**JEL Codes** : B21, B22, B23, E13, E65.

## 1. The two sides of the same coin

In 1984, the French Institute of Statistics and Economic Studies (INSEE) Journal published a special issue on general-equilibrium models with rationing (GEMR).<sup>2</sup> In the introductory article, Christian Gourieroux, Jean-Jacques Laffont, and Alain Monfort claimed:

Several European econometricians have begun to work on the estimation of the three-goods models developed by Barro and Grossman (1971), Bénassy (1973), and Malinvaud (1977). We review below some of the problems with the specifications used in this first wave of macroeconometric applications –

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<sup>1</sup> Correspondence may be addressed to Romain Plassard, Université Paris-Dauphine, Université PSL, Laboratoire d'Économie de Dauphine, CNRS, IRD, 75016 Paris, France. E-mail : [romain.plassard@dauphine.psl.eu](mailto:romain.plassard@dauphine.psl.eu).

We extend our heartfelt gratitude to Patrick Artus, Sanvi Avouyi-Dovi, Pierre-Yves Hénin, Jean-Pierre Laffargue, Alain Monfort, Quang Vuong, and Bernard Salanié for generously sharing their recollections and responding to our inquiries. Their invaluable input was instrumental in shaping the content of this article. We also wish to express our appreciation to the two anonymous referees, Avouyi-Dovi, Laffargue, and Eric Pinzón-Fuchs, for their constructive feedback, which greatly enhanced the quality of earlier versions of this work. Lastly, our thanks go to Matthieu Renault and the 2023 Conference in the Latin American Society for the History of Economic Thought participants for their fruitful discussions, which have enriched our perspective on the history of macroeconomics.

<sup>2</sup> GEMR had different labels, including “fixed-price equilibrium models,” “disequilibrium models,” and “non-Walrasian models.” Since many economists elaborated models where prices moved from one market period to another (Plassard et al., 2021), it seems inappropriate to use the label “fixed-price equilibrium models.” Then, despite the existence of dynamic models with rationing, the label “disequilibrium models” is not relevant either. The reason is that all the models considered in our article rest on equilibrium. Although markets do not clear, economic agents are rational, and their optimizing plans are coordinated. Finally, the label “non-Walrasian” model is not specific enough. For instance, sunspot models are equally “non-Walrasian” and very different from GEMR.

Kooiman and Kloek, 1980; Sneessens, 1981; Vilares, 1981; Artus et al., 1984 (1984: p. 21).<sup>3</sup>

Only four GEMR had been estimated in 1984, and all rested on highly simplified specifications – e.g., two aggregate markets, one composite good, one labor category, and money. However, the situation changed from the second half of the 1980s. Guy Laroque and Bernard Salanié (1995) described a second wave of estimations involving disaggregated and dynamic GEMR (e.g., Drèze and Sneessens, 1986; Lambert, 1988; Salanié, 1991; Artus et al., 1993).<sup>4</sup> Therefore, throughout the 1980s and 1990s, there was a continuing effort to specify and estimate GEMR. Our article shows how and why this applied work contributed to research on the microfoundations of macroeconomics.<sup>5</sup>

GEMR were the culminating point of what Kevin D. Hoover (2012) called the “general-equilibrium program” of microfoundations.<sup>6</sup> Hoover explained that “taking macroeconomics to describe (theoretically or econometrically) robust features of the economy, [this program] asked whether a fully disaggregated, general-equilibrium model could generate those features as a characteristic of the normal operation of the system” (2012: p. 21).<sup>7</sup> Then, Hoover argued that changes in general-equilibrium models occurred whenever economists encountered problems with the deduction of macroeconomic phenomena. For instance, GEMR emerged because Don Patinkin (1956) and Robert Clower (1965) showed the incompatibility between market-clearing assumptions and involuntary unemployment.<sup>8</sup> Last but not least, Hoover observed that there were few concerns with actual data in the general-equilibrium program (2012: p. 36; p. 39). Hoover thus depicted a “theoretical” program of microfoundations

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<sup>3</sup> The 1984 article was in French. All translations of French articles are ours.

<sup>4</sup> Laroque and Salanié’s (1995) review focuses on research in Western Europe. However, GEMR were also tested against Eastern economies’ data. GEMR particularly served to determine whether and to what extent Czechoslovakia (Dlouhy, 1984), Hungary (Hulyák, 1985; 1989), and Poland (Charemza and Gronicki, 1985; 1988) experienced excess demands on markets. For more information about the estimation of GEMR in Eastern Europe, see Plassard and Renault (2023).

<sup>5</sup> Applying economics covers numerous activities (Backhouse and Biddle, 2000). Besides elaborating and estimating econometric models, “application” can also involve participating or at least trying to participate in the design of economic policy. All these applied activities are considered in the present article.

<sup>6</sup> According to Hoover (2012), the general-equilibrium program traced back to John R. Hicks’ *Value and Capital* (1939). E. Roy Weintraub documented its history in *Microfoundations: The Compatibility of Microeconomics and Macroeconomics* (1979).

<sup>7</sup> Hoover acknowledged that the general-equilibrium program also involved the development of aggregate models (2012: p. 37). For more information on the issue of aggregation, see Hoover (2012: pp. 36-37). See also section 3 of our article.

<sup>8</sup> For more information on Clower’s and Patinkin’s approaches to the microfoundations of macroeconomics, see Plassard (2017, 2018) and Rubin (2012).

(2012: p. 21). Was there an applied side to the general-equilibrium program? If yes, what were its differences *vis-à-vis* the theoretical side?

Reviewing research on the estimation of GEMR, Laffont (1985) raised the issue of the microfoundations of macroeconomics. For instance, he explained how Patrick Artus, Guy Laroque, and Gilles Michel's (1984) GEMR allowed to empirically distinguish between voluntary and involuntary unemployment (1985: p. 338). However, it is an open question whether Artus et al. (1984) sought to provide microfoundations to macroeconomics and whether and how data analysis affected their search. More generally, reviews like Laffont's (1985) focused on the econometric challenges posed by the estimation of GEMR, not on the microfoundations of macroeconomics (Gourieroux et al., 1984; Uctum, 1991; Andreassen, 1993; Laroque and Salanié, 1995).<sup>9</sup>

Historians, on their side, focused on the theoretical side of the general-equilibrium program (Backhouse and Boianovsky, 2013; De Vroey, 2016; Béraud, 2020; Plassard et al., 2021; Renault, 2022). They particularly studied how Edmond Malinvaud (1977) specified Jean-Pascal Bénassy's (1975), Jacques Drèze's (1975), and Yves Younès' (1975) GEMR to explain unemployment.

The exception was Romain Plassard and Matthieu Renault (2023). Both historians discussed the conditions under which the estimation of GEMR took place in Europe and some policy implications of associated research. However, they did not address the relationship between the estimation of GEMR and the search for microfoundations. Therefore, it is an open question whether econometricians used the estimation of GEMR to advance the microfoundations of macroeconomics and, if yes, how and for what purpose.

Moreover, little is known about the estimation of GEMR. The above surveys addressed the different methods for estimating GEMR (e.g., the maximum or pseudo maximum likelihood). However, they provided little information on how to analyze data with GEMR, on the results of empirical research, and did not elaborate on the actual implementation of estimation methods (e.g., was there a ready-made software for estimating GEMR? How long did it take to estimate GEMR? How tricky was the estimation work?). Furthermore, Plassard

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<sup>9</sup> Gangadharrao S. Maddala (1980) and Richard E. Quandt (1982) reported on existing attempts to estimate GEMR. The focus was also on the econometric difficulties posed by the estimation of GEMR.

and Renault (2023) neither elaborated on data analysis nor rationalized the various choice of econometric specifications and estimation methods.

Our article contributes to fill these gaps by focusing on what Artus called the “ENSAE’s Band.”<sup>10</sup> Most of its members have already been mentioned: Artus, Avouyi-Dovi, Gourieroux, Laffont, Malinvaud, Michel, Monfort, Laroque, and Salanié.<sup>11</sup> The ENSAE’s Band was a group of French econometricians named in reference to the National School of Statistics and Economic Administration (ENSAE).<sup>12</sup> All its members studied at ENSAE. Then, all its members taught at ENSAE. For instance, while at ENSAE (1984-1986), Salanié had Gourieroux and Monfort in statistics, Malinvaud in growth economics, and Laroque in microeconomics. Last but not least, the ENSAE’s Band shared a common workspace when initiating research on GEMR. All the Band worked in the INSEE’s building in Malakoff (a city bordering Paris), where ENSAE was.<sup>13</sup>

The ENSAE’s Band involved theoretical econometricians like Gourieroux and Monfort. Why did they design methods for estimating GEMR? Were they concerned with the microfoundations of macroeconomics? The answer seems more evident in Laroque’s and Malinvaud’s cases. With Jean-Michel Grandmont, his colleague from the Centre Pour la Recherche EconoMique et ses APlications (CEPREMAP), Laroque (1976) developed an imperfect competition GEMR to provide microfoundations to macroeconomics (Backhouse and Boianovsky, 2013; Béraud, 2020). The same applied to Malinvaud and Younès (1977), with a GEMR based on game theory. However, with Grandmont and Younès, Laroque and Malinvaud were interested in providing general theories about how markets worked (Backhouse and Boianovsky, 2013; Béraud, 2020). Why, then, did they turn GEMR into macroeconomic models and how did this change their search for microfoundations? Finally, there is the case of Artus and Avouyi-Dovi. Both econometricians contributed to the development of large-scale models. According to Hoover (2012), such models were part of a microfoundational program

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<sup>10</sup> Artus used this expression in an interview we had on 30 September 2021.

<sup>11</sup> Bernard Migus and Alain Trognon were also part of the ENSAE’s Band.

<sup>12</sup> The ENSAE is the INSEE’s school. See Alain Desrosières (2013, Chapter 11) for details on its history,

<sup>13</sup> When Laffont started to work on GEMR, he had a position at École Polytechnique, not at ENSAE. However, he used to come to INSEE to attend seminars or do research (Bos and Simon, 2005). ENSAE (and École Polytechnique) eventually moved to Saclay and became part of Paris-Saclay University. See <https://www.ensae.fr/lecole/presentation-de-lensae-paris/histoire-et-identite> (consulted on 10 July 2023).

distinct from Hicks's (1939).<sup>14</sup> What was the relationship between their research on GEMR and large-scale models?

We had to explore uncharted territory to address these questions. We went through all the ENSAE's band contributions about GEMR, which required analyzing articles in theoretical econometrics (e.g., Gouriéroux et al., 1980a) and applied econometrics (e.g., Artus et al., 1984; Boissou et al., 1986a; 1986b). We also built our own dataset by carrying out semi-directed interviews and exchanging e-mails with members of the ENSAE's band and some of their co-authors (e.g., Quang Vuong).<sup>15</sup>

The research carried out by the ENSAE's band unveils the applied side of the general-equilibrium program. At ENSAE, the estimation of GEMR aimed to empirically discriminate between equilibrium and disequilibrium macroeconomics and provide a policy analysis tool. We show that both projects entailed an exploration of the microfoundations of macroeconomics, elucidate how data analysis influenced the specifications chosen by the ENSAE's band, and compare the microeconomic underpinnings of GEMR on both the theoretical and applied sides of the general-equilibrium program.

## 2. Motivations

In France, GEMR became macroeconometric models under the influence of Gouriéroux, Laffont, and Monfort (1980a), Green and Laffont (1981), and Malinvaud (1982). What motivated their research?

### 2.1 Discriminating between equilibrium and disequilibrium macroeconomics

Monfort and Gouriéroux explained the conditions under which they started working on GEMR (Ghysels and Renault, 2011: pp. 6-7). Monfort summed up:

When [Laffont] came back from Montreal, in 1975, I was just coming back from the L.S.E. [London School of Economics] and I had been appointed as Professor of Statistics at ENSAE. Jean-Jacques [Laffont] had obtained a position as researcher at CNRS [Centre National de la Recherche Scientifique] and he

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<sup>14</sup> According to Hoover (2012), large-scale models were part of Lawrence Klein's "aggregation program" (p. 21). For information on its history, see Acosta and Rubin (2019), Backhouse and Cherrier (2019), and Rancan (2019).

<sup>15</sup> The list of interviewees includes Artus, Avouyi-Dovi, Laffargue, Salanié, and Vuong. Any references to the interviews shall be indicated by the acronym PC (for Personal Conversation) in the article. Moreover, we will indicate in footnotes when using information obtained via e-mails.

became a member of the “Laboratoire d’Économie de l’École Polytechnique,” headed by Claude Henry [...] Jean-Jacques was interested in fixed-price equilibrium, or disequilibrium. In fact, many economists in France were working on this subject either at INSEE [e.g., Laroque and Malinvaud] or at CEPREMAP [e.g., Bénassy, Grandmont, and Younès], but the econometric side was not developed at all. Jean-Jacques, who has always been interested in both the economic and the statistical aspects of the problems he met, thought that it was important to try to fill this gap. And we started working on this theme (2011: p. 6).

Before visiting the L.S.E., Monfort had worked as an economist in the Direction de la Prévision (1971-1974), an institution designed to inform French economic policy. However, Monfort acknowledged that he had little interest in economics before working with Laffont: “I was mainly interested in rather abstract statistical problems, like coordinate-free approaches to linear models or identifiability. Jean-Jacques’ economic culture clearly played a decisive role in the orientation of my research themes” (2011: p. 6).<sup>16</sup> The same applied to Gourieroux who, after graduating from ENSAE (1972), had turned to mathematics and sampling theory.<sup>17</sup> Therefore, the question is why Laffont sought to turn GEMR into macroeconometric models.

Like Gourieroux and Monfort, Laffont might have considered that allowing for the estimation of GEMR was his “job” (Ghysels and Renault, 2011: p. 7).<sup>18</sup> However, Laffont did not only specify and provide the tools for estimating GEMR.<sup>19</sup> He also tested GEMR against data (Ducos et al., 1982; Ducos and Laffont, 1984; Boissou et al., 1984; 1986a; 1986b). His ambition thus went beyond merely doing the job of a theoretical econometrician.

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<sup>16</sup> Before teaming up with Gourieroux and Monfort, Laffont had already done some research on externalities (Laffont, 1971; Laroque and Laffont, 1972), nonlinear estimation (Jorgenson and Laffont, 1974), asymmetric information (Helpman and Laffont, 1975; Laffont, 1975), and public policy (Green and Laffont, 1976).

<sup>17</sup> Gourieroux obtained the position of Professor of Statistics at ENSAE in 1976.

<sup>18</sup> Gourieroux explained: “We had a lot of discussions with institutions such as INSEE and CEPREMAP, with several other young people like Jean-Pascal Bénassy or Guy Laroque [...] Our job was to adapt economic theory to make it applicable to economic practice, that is, to write the econometric models for applications, discuss the identification issues, [...] and to propose the appropriate estimation methods” (Ghysels and Renault, 2011: p. 7).

<sup>19</sup> See section 3 for details on the challenges posed by the estimation of GEMR and the solutions offered by Gourieroux et al. (1980a).



Laffont aimed to contribute to the debates over the microfoundations of macroeconomics. In an article written during his stay in Canada (Montreal University), Laffont claimed:<sup>20</sup>

The interest of applied econometric work within disequilibrium frameworks seems to us particularly important in view of recent developments in disequilibrium micro-economics. We believe that an important task of econometrics will be to define empirically the relevant ranges of Walrasian and disequilibrium (Keynesian?) economics, respectively (1977: p. 1187).

According to Laffont, empirical research should determine which markets were in equilibrium, which markets were in disequilibrium, to what extent, and whether imbalances were due to a lack of supply or demand.

The goal was to discriminate between equilibrium and disequilibrium macroeconomics. Green and Laffont (1981) explained:

The microeconomic foundation of macroeconomics has two fairly well-articulated paradigms. The neo-classical paradigm maintains that ‘markets are working’: competitive behavior achieves a Pareto optimal outcome under the guidance of the price system. Authorities should interfere as little as possible with this allocation mechanism as long as competitive behavior is maintained [...] The Keynesian paradigm on the contrary maintains that ‘markets are not working’. Price rigidities, even with competitive behavior, lead to a misallocation of resources which can be partially remedied by government interventions [...] We will analyze how [...] the spillover effects in a fixed-price equilibrium produce certain testable characteristics in macro time series data. We will argue that these can be used to discriminate between a model of the type we study and the analogous flexible-price system (1981: p. 200).

Having spent time not only in Canada but also in the U.S. (Harvard University, 1972-1974), Laffont knew that there were two competitive approaches to the microfoundations of

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<sup>20</sup> Laffont wrote this article with René Garcia, an economist based in Canada. In the acknowledgments, Garcia and Laffont claimed: “We are very grateful to Professor Marcel G. Dagenais and to the Département de l’Éducation du Québec for financially supporting this work. In addition, Professor Dagenais provided many encouragements and suggestions all along the study. We benefited also from discussions with Mrs. Guthrie, Mr. J.A. Galbraith, R. Theoret, and with several members of the Department of Economics of the Université de Montréal, including especially Professor P. Fortin” (1977: p. 1187).

macroeconomics. On one side, macroeconomists like Edmund Phelps (1970a) or Robert Lucas (1972; 1975) developed market-clearing models.<sup>21</sup> On the other side, macroeconomists including “Barro and Grossman (1971), Bénassy (1975), Drèze (1975), [and] Malinvaud (1977)” (1981: p. 199) developed models in which market prices varied too slowly to ensure continuous equilibria on markets. Laffont’s question was which framework was the most appropriate for macroeconomics and economic policy.

According to Laffont, the answer depended on whether economies experienced disequilibria or equilibria in markets.<sup>22</sup> If data showed that markets were balanced or close to balance, then macroeconomists should develop equilibrium models. However, if data showed the existence of disequilibria in markets, then GEMR offered the relevant approach to macroeconomics and economic policy.

This empirical contest raised the issue of the hypothesis testing framework (Garcia and Laffont, 1977: p. 1187; Gouriéroux et al., 1980b: p. 245; Green and Laffont, 1981: p. 205). A proper test requires two competitive hypotheses: a null hypothesis (e.g., equilibrium) and an alternative hypothesis (e.g., disequilibrium). To test whether observed production and employment were generated by supply (due to an excess demand), demand (due to an excess supply), or by supply and demand (equilibrium), the model, therefore, had to allow for every configuration to occur.<sup>23</sup>

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<sup>21</sup> Neither in 1977 nor in 1981 did Laffont mention Lucas or Phelps. However, it is hard to believe that he did not think about Lucas’s macroeconomics when describing models where prices adjusted instantaneously, markets cleared, and individuals formed “rational expectations” (Green and Laffont, 1981: p. 200). Moreover, it is also hard to believe that Laffont did not know about *Microeconomic Foundations of Employment and Inflation Theory* (Phelps et al., 1970b). Phelps claimed to develop a “disequilibrium” approach to macroeconomics (Phelps, 1970b: p. vii). However, it was rapidly acknowledged that Phelps et al. (1970b) developed market-clearing models. For instance, see Grossman (1973: p. 1362) and Barro and Grossman (1976, Chapter 7).

<sup>22</sup> The Lucas’s (1976) “Critique” did not factor into Laffont’s discrimination between equilibrium and disequilibrium macroeconomics. Laffont may have considered it an inadequate discrimination criterion. On the one hand, several economists known to Laffont (e.g., Malinvaud and Franco Modigliani) questioned the empirical validity of the Critique (Goutsmedt et al, 2019). Hence, immunity to the Lucas’s critique could not demonstrate the empirical superiority of equilibrium over disequilibrium macroeconomics. On the other hand, by associating disequilibrium macroeconomics with GEMR, Laffont could not apply Lucas’s distinction between models where parameters changed or not in response to economic policy implementation. In GEMR, parameters changed whenever economic regimes changed.

<sup>23</sup> While researching centrally planned economies (CPEs), Richard Portes and David Winter (1977, 1978, 1980) also made this case. To test whether CPEs experienced chronic excess demands on markets, both economists claimed that “the appropriate procedure [was] to allow disequilibrium in the maintained hypothesis. We may then be able to test the observed macroeconomic data are generated by equilibrium configurations” (1978: p. 9). Portes and Winter also used this argument to criticize David Howard’s (1976) research on CPEs: “[Howard] seeks to test the quantity-constrained model by *assuming* constrained behavior, using the constraint variable as a regressor, and inspecting the coefficient estimates [...] This approach cannot tell us that the consumption goods market was in excess demand, and that [his] specification [of the labor supply] is therefore correct. For suppose that households were in fact unconstrained, so the true model generating the observed  $c$  and  $l$  [implies market-clearing]. Then

By construction, market-clearing models could not guide such empirical investigations. However, GEMR could. Within GEMR, individuals could formulate notional or constrained demand and supply depending on market prices. Therefore, not only did GEMR reduce the risk of specification errors (e.g., assuming constrained behavior while markets cleared, or assuming notional behavior while there were disequilibria on markets). They also offered the proper framework for testing the existence of rationing. Hence Laffont's motivation to turn GEMR into macroeconomic models.

## 2.2 A tool for policy analysis

Malinvaud also compared empirically GEMR and Lucas's macroeconomics (Renault, 2020: pp. 566-569). However, unlike Laffont, Malinvaud did not require testing the specifications of each framework simultaneously (Renault, 2022: p. 227).<sup>24</sup> Therefore, he had other reasons to turn GEMR into macroeconomic models.

According to Renault (2022), Malinvaud (1977; 1980; 1982) developed GEMR i) to rationalize some practices in the estimation of large-scale macroeconomic models (e.g., the use of indicators of tension); ii) to find specifications capable of improving the fit of large-scale macroeconomic models (e.g., firms' investment behavior); and iii) to improve the measure of the indicators of tensions (e.g., job vacancies or capacity utilization) used in large-scale macroeconomic models.

This last motivation shows that Malinvaud's "Econometric Model" (1982) was part of the search for microfoundations opened in *The Theory of Unemployment Reconsidered* (1977) and furthered in *Profitability and Unemployment* (1980). More generally, Renault's (2022) study shows that Malinvaud's search for microfoundations was "oriented toward the needs of large-scale models" (p. 220).

In line with Renault's (2022) interpretation, it is possible to identify another reason why Malinvaud developed GEMR (1977, 1980, 1982). Malinvaud's goal was to provide a tool for policy analysis that would complement the expertise offered by large-scale models.

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taking  $c$  as exogenous (for households) and estimating [Howard's labor supply] would be a specification error" (p. 143).

<sup>24</sup> Moreover, Renault (2020) shows that Malinvaud (e.g., 1984, 1989, 1991, 1992) relied on stylized facts (e.g., the weak sensitivity of demand to the prices of manufactured goods) and on business survey data (e.g., the entrepreneurs' statements to suffer from involuntary accumulation of stocks) to compare GEMR with the New Classical Macroeconomics. For more information on Malinvaud's method, see Mazodier (2017) and Renault (2022).

### 2.2.1 Real-time macroeconomic diagnoses

According to Malinvaud (1977), governments' policy should depend on the "regime" in which the economy was. In a "Classical regime," unemployment resulted from supply deficiency. The return to full employment thus required reducing firms' costs and encouraging capital accumulation. However, in a "Keynesian regime," unemployment resulted from demand deficiency. Therefore, governments should increase public spending to reduce unemployment.

This policy message is well known (Backhouse and Boianovsky, 2013; De Vroey, 2016; Béraud, 2020; Plassard et al., 2021). However, historians of macroeconomics did not address whether and how Malinvaud (1977) intended to use GEMR in policymaking. At INSEE, Malinvaud aimed to use GEMR to diagnose in real-time in which regime(s) the various sectors of the French economy were.

This use of GEMR is implied by the section on "The classical nature of present unemployment" (1977: pp. 107-110). Malinvaud claimed:

There is no doubt that the main features of the 1975 unemployment are again Keynesian [...] But there are signs that [classical unemployment] would soon again emerge. Indeed, profitability in some sectors of production is now – and may for long remain – too low for firms to wish to develop production in them. Firms may refrain from investing or recruiting labor even when they anticipate a strong new rise in demand. The stage will then be set for the reappearance of classical unemployment, even before Keynesian unemployment has disappeared (1977: p. 109).

Remember that Malinvaud wrote *The Theory of Unemployment Reconsidered* in 1975 for a Yrjö Jahnsson Lecture to be delivered in January 1976.<sup>25</sup> It means that Malinvaud not only used his GEMR to explain the origins of stagflation (1977: p. 108). Malinvaud also used his GEMR to diagnose the unemployment that he was observing in France and, more generally, in the "Western world" (p. 108).

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<sup>25</sup> In 1954, Hilma Jahnsson created a Foundation to honor her husband, the Finnish economist Yrjö Jahnsson. Aiming "to promote Finnish research on economics and medicine" (<https://www.yjs.fi/en/>, consulted on 22 June 2023), the Yrjö Jahnsson Foundation invites every two years a worldwide recognized economist to deliver a lecture in its headquarters, at Helsinki. For a list of economists invited since 1963, see <https://www.yjs.fi/en/seminars-and-awards/#yrjo-jahnsson-lectures> (consulted on 22 June 2023).

According to Malinvaud, stagflation showed a rise in Classical unemployment. Since the end of the 1960s, “anticipations or social tensions [had led] to an abnormal increase in real wages” (1977: p. 107). Profits had thus started to decline, a tendency reinforced by the 1973 oil shock (1977: p. 108). Due to the lack of firms’ profitability, entrepreneurs would have decided to reduce production, postpone investments, and fire workers. That would have explained why classical unemployment “was an important part of the picture during the 1975 sharp depression” (1977: p.108).

Moreover, Malinvaud was convinced that the 1973 oil shock created the conditions for an increase in the occurrence and in the extent of classical unemployment in the future (1977: p. 108). According to Malinvaud, this raised a difficulty for policy experts. “The trouble with classical unemployment [was] that it [was] difficult to diagnose in time: the process of building new equipment and scrapping old may develop unfavorably during periods of Keynesian unemployment or repressed inflation, without any apparent signal of the risk of classical unemployment” (1977: p. 109).

Since “this [was] not a purely short-run phenomenon,” proper macroeconomic diagnoses needed a model explaining disequilibrium dynamics (1977: p. 109-110).<sup>26</sup> Moreover, proper diagnoses needed to turn GEMR into macroeconometric models. While reflecting on empirical research led with GEMR, Malinvaud (2006) recalled that his *Theory of Unemployment Reconsidered*:

Aimed at showing the potentials of [GEMR] for macroeconomic diagnosis. But that could only be a first step, at least in two respects. The fix-price hypothesis was too strong for a diagnosis beyond the very short-term. Analysts had also to wonder how prices, wage rates, demands, and supplies would evolve subsequently, depending in particular on the configuration of market disequilibria. Such was the theme I attempted to explore in [*Profitability and Unemployment* (1980)]. At the time, the habit was, moreover, well anchored to often test a macroeconomic diagnosis by references to a structural econometric model. The question was then natural to wonder what form of econometric model would correctly embody the diversity of regimes which the fix-price theory was exhibiting [...] I chose to explore the issue in the meeting conveyed

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<sup>26</sup> For details on how Malinvaud modeled the dynamics of non-clearing-markets, see Plassard et al. (2021).

for the 25<sup>th</sup> anniversary of the Econometric Institute [Malinvaud (1982)] (2006: p. 172).

Malinvaud (1982) acknowledged that his “model [was] much too simple-minded to be directly used in applied macroeconomics” (p. 240). However, it was allegedly a first step in this direction (1982: p. 240). Therefore, by adapting GEMR to data analysis, Malinvaud’s (1982) goal was to have a policy analysis tool.

### 2.2.2 A distinct but complementary expertise

Malinvaud (2006: p. 172) recalled that when developing GEMR (1977, 1980, 1982), the practice was to use large-scale models to perform macroeconomic diagnoses. How did he intend to change this practice?

Since Malinvaud did not offer explanations, it is necessary to imagine possible changes. The first change would be to elaborate a new large-scale model. For instance, METRIC (Modèle Économétrique Trimestriel de la Conjoncture) would be merged with a GEMR.<sup>27</sup> The goal would be to provide macroeconomic diagnoses with a model involving not only a detailed description of behavior in the various sectors of the economy (e.g., the manufacturing, the agricultural, and the energy industries), but also the diversity of the regimes which characterized GEMR.

The second change would be to feed large-scale models with the estimation results of GEMR. The indicators of tension likely served to provide macroeconomic diagnoses. Depending on the values of production capacities, cashflow difficulties, and job vacancies in METRIC (Artus et al., 1978 p. 68), it was possible to explore whether unemployment was due to a lack of supply or demand. This practice would continue to be implemented. However, instead of being inferred from business survey data (Artus et al., 1978 p. 68), the indicators of tension would be estimated by GEMR.

The third and last change would be no longer using large-scale models to provide macroeconomic diagnoses. Large-scale models would continue to be used for policy simulation and forecasting. However, GEMR would perform macroeconomic diagnoses. There would therefore be two distinct but complementary policy tools. For instance, if a GEMR detected

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<sup>27</sup> METRIC was a large-scale model (approximately 900 equations) whose construction was a collective endeavor involving the Direction de la Prévision (e.g., Pierre Morin) and INSEE (e.g., Artus and Henri Sterdyniak). METRIC started to be used in policy analysis (e.g., simulation and forecasting) in 1977 and was still operational when Malinvaud elaborated his “Econometric Model for Macro-Disequilibrium Analysis” (1982).

situations of classical unemployment in various sectors of the economy, the team developing METRIC would have to simulate the effects of supply-side policies – e.g., a decrease in the employers’ social contribution or the corporate tax.

This last change is likely to be the one that Malinvaud considered. When addressing how to improve large-scale models, Malinvaud did not raise the issue of macroeconomic diagnoses. *Profitability and Unemployment* (1980) and “Econometrics Faced with the Needs of Macroeconomic Policy” (1981) are cases in point.<sup>28</sup> Malinvaud (1980: p. 5; 1981: p. 2) acknowledged that large-scale models did not offer a satisfactory guide to economic policy during stagflation. However, he did not mention their difficulty in providing macroeconomic diagnoses. Problems involved their ability to assess the effects of policies in the medium run and to provide accurate forecasts (1980: p. 5; 1981: pp. 3-4). Therefore, Malinvaud did not propose solutions for improving their ability to provide macroeconomic diagnoses. Instead, he focused on how to find new specifications, strengthen the relationships with theory, and address endogeneity and identification issues.

Then, Malinvaud might have considered that large-scale and GEMR could not be merged. In 1982, Malinvaud considered that every sector of the economy had the same size, assumed the same production function for every firm, and ruled out the possibility that profitability could have different effects depending on the regime in which a sector was (1982: p. 243). Moreover, he did not consider econometric relationships involving lags or, more generally, did not specify the dynamics of non-clearing markets. His GEMR was, therefore, very simple compared to large-scale models like METRIC or DMS (Dynamique Multisectoriel).<sup>29</sup>

However, Malinvaud anticipated difficulties in estimating it: “the transformation defined by (4) [the production in the economy] and (6) [the proportion of sectors in a Keynesian regime] is not easy from a computational point of view. Different systems of equations apply to different regions of the  $(e; \varepsilon)$  space [the partition of regimes]; moreover, these systems have no easy analytical solution” (p. 246). Therefore, Malinvaud might have considered that it was

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<sup>28</sup> The 1981 article is the Ragnar Frisch Lecture Malinvaud gave at the Fourth World Congress of the Econometric Society (Aix-en-Provence, 1980). The publication of his lecture coincides with the 25<sup>th</sup> anniversary of the Econometric Institute, namely when Malinvaud designed his “Econometric Model for Macro-Disequilibrium Analysis” (1982).

<sup>29</sup> DMS is another large-scale model built at INSEE (between 1974 and 1976). It had five sectors, including a productive sector in which different goods were produced in thirteen branches (industry, energy, housing...). It also had almost 1900 equations aiming to account for the dynamics of the French economy (Service des Programmes de l’INSEE, 1980: pp. 934-935).

impossible to estimate a large-scale model in which the specifications changed depending on the sectors' regimes.

Last but not least, despite considering indicators of tension, large-scale models were not appropriate for providing macroeconomic diagnoses. Macroeconomists involved in the elaboration of large-scale models acknowledged it.<sup>30</sup> For instance, Pierre-Alain Muet explained:

Large-scale models consider the role of supply and demand in determining macroeconomic variables. However, by describing an intermediate situation between Classical and Keynesian unemployment, they cannot identify clearly which factor hinders production (1987: p. 212).

Likewise, while commenting on Laroque's (1989) estimation of a GEMR, Jean Waelbroeck claimed:

For the purpose at hand – investigation of the nature of unemployment – Laroque's model is the right tool: a standard model could not assess whether unemployment has been 'Keynesian', i.e., due to deficient demand, or 'Classical', i.e., caused by an excessive real wage (1989: p. 994).

Muet's and Waelbroeck's positions might be a good proxy of Malinvaud's. Like Malinvaud, Muet and Waelbroeck promoted using large-scale models in policy making.<sup>31</sup> Moreover, Muet and Waelbroeck were also responsible for informing economic policy. In 1987, Muet headed the econometric department of the Observatoire Français des Conjonctures Économiques (OFCE). Waelbroeck, on his side, was responsible for implementing Klein's "Project Link" in Belgium and, later, for the elaboration of a large-scale model of the European Economic Community.<sup>32</sup> Therefore, there are good reasons to believe that, like Muet and Waelbroeck, Malinvaud considered that GEMR were the relevant tool for providing macroeconomic diagnoses. Hence why he encouraged the estimation of GEMR and, in parallel, the improvement of large-scale models.

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<sup>30</sup> The same is true for Laffont. In the article written with Garcia, Laffont claimed that "econometricians have often included, without any special care, in their large-scale models [...] quantity constraints as explanatory variables which do not allow for testing the existence of rationing" (1977: p. 1187).

<sup>31</sup> Muet was part of the team which elaborated the DMS model at INSEE (Fouquet et al., 1978). Waelbroeck was also instrumental in the development of large-scale models. He was notably a founding member of Lawrence Klein's "Project Link" (Maes and Buys, 2005: p. 79), an attempt to build a large-scale model of the world economy.

<sup>32</sup> For more information on Waelbroeck's project at the European Commission, see Acosta et al. (2023).



To conclude, there were two motivations behind the early econometric specifications of GEMR. Laffont's was to discriminate between equilibrium and disequilibrium macroeconomics. Malinvaud's was to develop a tool for policy analysis. The following section will explore how GEMR became macroeconomic models.

### **3. Turning GEMR into macroeconomic models**

Early econometric models (Gourieroux et al., 1980a; Green and Laffont, 1981; Malinvaud, 1982) show differences with the specifications adopted on the theoretical side of the general-equilibrium program.

#### 3.1 The microfoundations of macroeconomics

Two classes of models existed on the theoretical side of the general-equilibrium program (Weintraub, 1979; Hoover, 2012). First, there were disaggregated general-equilibrium models. For instance, Kenneth Arrow and Frank Hahn (1971) explained how unemployment could emerge from the choice made by various economic agents operating on different micro-markets. Second, there were aggregated general-equilibrium models. For instance, Barro and Grossman (1971) explained the determination of income by considering the interactions between a representative firm and a representative household on aggregate labor and consumption goods markets.

Gourieroux et al. (1980a) specified a model analogous to Barro and Grossman's (1971).<sup>33</sup> However, they made this choice "for expository purposes" (p. 88). Gourieroux, Laffont, and Monfort "[expected] to find the most interesting applications in studies of interrelated microeconomic markets, rather than in highly aggregate markets" (1980a: p. 88). The reason was that actual economies exhibited mixed regimes (Laffont, 1985: pp. 349-350). For instance, some households may experience unemployment while some producers failed to fill vacancies. Laffont (1985) concluded that it was necessary to develop GEMR involving representative agents and micro-markets. Malinvaud (1982) agreed and contributed to the emergence of this new class of general-equilibrium models.

Malinvaud's (1982) GEMR shows a second difference between the theoretical and the applied sides of the general-equilibrium program. On the theoretical side, the minimum

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<sup>33</sup> "We consider an economy with three commodities, money, a consumption good labelled good 1, and labor labelled good 2, and two agents, a consumer, and a producer, i.e., essentially the Barro-Grossman model" (Gourieroux et al, 1980a: p. 76). Green and Laffont (1981) considered a similar economy.

condition served to determine the levels of income and employment in GEMR. For instance, Barro and Grossman (1971) assumed that aggregate supply (aggregate demand) determined the level of income when there was an excess demand (excess supply) on the markets for goods. According to Malinvaud (1982), this specification was not relevant since unemployment and unfilled vacancies could coexist. Instead of the minimum condition, Malinvaud (1982: p. 242) assumed that the level of income was a non-linear function of excess demands on markets.

This modeling choice allows to identify a third and last difference between the theoretical and applied sides of the general-equilibrium program. On the theoretical side, Hicks' (1939) "composite-commodity theorem" served to elaborate aggregated general-equilibrium models (Hoover, 2012: p. 36). For instance, Barro and Grossman (1971) moved from the individual to representative agents and aggregate markets by assuming that aggregates could be treated as individuals.<sup>34</sup>

However, Malinvaud (1982) built on John Muellbauer's (1978) aggregation method. While Hicks's (1939) removed the differences between markets, Muellbauer's (1978) intended to consider the variety of market situations. Muellbauer's method particularly allowed to compute the output of an economy considering the possibility that some labor markets were in excess supply (unemployment) while others were in excess demand (job vacancies).

Moreover, Malinvaud (1982) showed concerns with real data that economists using the composite-commodity theorem did not.<sup>35</sup> According to Hoover, the composite-commodity theorem "[said] nothing about how likely [its] conditions [were] to be found – even approximately – in real-world cases. Hicks [did] not address the applicability of the theorem to the real world" (2012: p. 36). However, Malinvaud addressed the empirical content of his aggregation method. For instance, Malinvaud knew it was unrealistic to assume that the different micro-markets had equal sizes (1982: p. 243). Malinvaud also raised the issue of

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<sup>34</sup> Barro and Grossman argued that: "when analyzing the behavior of firms, working households, and retired households, we consider the 'representative' unit; that is, a unit whose behavior, except 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). Regarding the move from micro to aggregate markets, Hicks explained that "the very important principle, used extensively in the text, [was] that if prices of a group of goods [changed] in the same proportion, [then] that group of goods [behaved] just as if it were a single commodity" ([1939] 1946: p. 312-313).

<sup>35</sup> Gourieroux, Laffont, and Monfort (1980a: p. 76) used the composite-commodity theorem. However, they acknowledged its lack of empirical content. They explained that their "two agents may be considered as aggregates of large numbers of competitive agents. However, this [left] aside delicate aggregation problems and a careful consideration of rationing schemes" (1980a: p. 76).

assuming a stable distribution of the shocks generating disequilibria in markets (1982: p. 243). Further research on aggregation theory was therefore necessary.<sup>36</sup>

### 3.2 Disequilibrium econometrics

Neither Gouieroux et al. (1980) nor Green and Laffont (1981) nor Malinvaud (1982) estimated their GEMR. However, they addressed the issue of what was the proper estimation method. New research questions thus emerged within the general-equilibrium program.

In “Econometrics of Equilibrium Models with Rationing” (1976), Laffont and Monfort claimed:

It is a question whether the estimation methods based on the assumption of instantaneous price adjustments are robust when considering market price sluggishness and the resulting quantity rationing [...] If all producers (consumers) fail to realize their notional plans, the exchanged quantity does not belong to the aggregate supply (demand). However, standard estimation methods assume that all observed points belong to supply and demand. Using these methods would thus imply specification errors if there were rationing on markets [...] The reconsideration of Walrasian economics thus seems to require essential changes in econometric methods (1976: p. 5).

When referring to “standard estimation methods,” Laffont and Monfort (1976) considered how econometricians ran regressions in large-scale models. Whether they used the ordinary least squares procedure or more complicated methods like the maximum-likelihood’s, they assumed market-clearing. Therefore, they misspecified behavior whenever markets failed to clear.

The solution to this misspecification problem could have been to assume constrained behavior. However, it would not have ruled out specification errors (Garcia and Laffont, 1977: p. 1187). If markets cleared on certain periods, economic agents would have behaved according to their notional plans, and the exchanged quantity would have been determined at the intersection between Walrasian supply and demand.<sup>37</sup>

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<sup>36</sup> Neither Gouieroux et al. (1980a) nor Green and Laffont (1981) raised the issue of the aggregation method. However, with Laroque, Gouieroux later designed a method for aggregating disequilibria over micro-markets. See “The Aggregation of Quantities in Quantity Rationing Models” (Gouieroux and Laroque, 1985).

<sup>37</sup> The American economist Donald Tucker already made this point in “Macroeconomic Models and the Demand for Money under Market Disequilibrium” (1971). Laffont and Monfort did not refer to Tucker (1971). Neither did the other members of the ENSAE’s band.

Laffont and Monfort's (1976) solution was to build on Ray Fair and Dwight Jaffee's (1972) disequilibrium econometrics. First, Fair and Jaffee had estimated a model involving one supply function, one demand function, and one minimum condition. The minimum condition implied that the quantity exchanged on the market could be determined by demand or supply. Therefore, Fair and Jaffee had found a way to estimate a model involving regimes switching.

Second, Fair and Jaffee had expanded their model to include price adjustment rules. The rules served to gather information about the sign and extent of the excess demand on a given market. If there was a strong price increase, for instance, Fair and Jaffee concluded that the market was in a situation of excess demand and that rationing was important. Therefore, they could address the need for more information on effective supply and effective demand.<sup>38</sup>

Third and lastly, thanks to maximizing a likelihood function, Fair and Jaffee (1972) could calculate the probability for each observed point to be generated by supply, demand, or supply and demand. Disequilibrium econometrics, therefore, allowed addressing the kind of empirical problems posed by Malinvaud and Laffont.

However, the road to estimate GEMR was still long in 1972. In the wake of Fair and Jaffee (1972), research focused on analyzing isolated markets. This is clear from Laffont and Monfort's (1976) review. Laffont and Monfort indicated that in existing models (e.g., Maddala and Nelson, 1974; Amemiya, 1975; Goldfeld and Quandt, 1975), economic agents never incorporated quantitative constraints from other markets (1976: p. 5). It was not until Gourieroux et al. (1980a) that GEMR could be estimated with disequilibrium econometrics.<sup>39</sup>

Then, there was the question of how to maximize a likelihood function with spill-over effects. Monfort recalled that in 1976, a procedure had yet to be identified.<sup>40</sup> The problem came from the non-linearity generated by switching regimes. Not only did it complicate the maximization of the likelihood function, but it raised doubts about whether estimated

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<sup>38</sup> Micro and macroeconomic time series usually give information about the actual quantity exchanged, not unrealized supply or demand. Unemployment data is one exception.

<sup>39</sup> Takatoshi Ito (1980) and Henri Sneessens (1980; 1981) were also instrumental in bridging the gap between GEMR and disequilibrium econometrics. A critical difficulty was to show that when latent variables (e.g., supply when there is an excess supply on a given market) and observed variables (e.g., exchanged quantities) are determined simultaneously, the reduced form of a GEMR existed (Gourieroux et al., p. 81; Ito, 1980: p. 117). For more information on the "consistency conditions" of a GEMR, see Gourieroux (1989: pp. 285-288).

<sup>40</sup> Excerpt from Monfort's e-mail, sent on 21 May 2021. In line with Monfort's recollection, Ito reported that: "Quandt (1976) [had] failed to realize spill-over effects and had an incorrect likelihood function. Amemiya (1977) [had] corrected the mistake in Quandt. In the Quandt-Amemiya model, the spill-over effect [was] not explicitly captured as an effect of the difference between actual and notional amounts of trade in the other market" (1980: p. 99).

parameters represented global maxima. Gourieroux et al.'s solution was to use an “uphill maximization algorithm” (1980a: p. 76). Thanks to it, they showed the possibility to compute the global maxima of a likelihood function involving four economic regimes (1980a: p. 87; p. 93).

Last but not least, there was the question of identifying regimes. Malinvaud distinguished between two identification strategies (1982: p. 245). On one side, there would be Gourieroux et al.'s (1980a).<sup>41</sup> To identify the Keynesian regime, for instance, Gourieroux et al. (1980a) would determine if, over a particular time frame (e.g., a quarter for quarterly time series), lack of demand was estimated to have a probability to constrain behavior higher than lack of equipment or lack of labor.

On the other side was Malinvaud's (1982) identification strategy. Malinvaud elaborated a model allowing “the simultaneous determination of aggregate output  $y_t$  and the tension indicator  $P_{Kt}$ [i.e., the proportion of markets in a situation of excess supply]” (1982: p. 242). However, Malinvaud explained that “in practice, it was likely that  $P_{Kt}$  will be replaced by a proxy” (1982: p. 245).

This is where the originality of his identification strategy lay. In the discussion following the presentation of Malinvaud's paper, Jacques Drèze asked: “Do you think that the proportion of firms reporting to be in a Keynesian underemployment regime is the most informative coming from business survey data?” (p. 258). Malinvaud replied: “Yes. I took this quantity rather than the proportion of sectors that [were] in a classical state, which appears to be a more delicate question. The notion that someone would produce more if he had more demand is solid and may be a good proxy [of  $P_{Kt}$ ]” (p. 258). Therefore, unlike Gourieroux et al. (1980a), Malinvaud intended to use business survey data to identify economic regimes.<sup>42</sup>

To conclude, there were differences between the applied and the theoretical sides of the general-equilibrium program. On the applied side, there were different forms of general-

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<sup>41</sup> Malinvaud indicated that Ito used the same identification strategy (1982: p. 245). However, Green and Laffont (1981: p. 220) proposed exploring their model's stochastic properties to identify regimes. For more information on Green and Laffont's (1981) identification strategy, see section 4.

<sup>42</sup> At the Center for Operations Research and Econometrics (CORE), Jean-Paul Lambert explored a similar identification strategy in his Ph.D. thesis, “Disequilibrium Macroeconomics Models: Theory and Estimation of Rationing Models using Business Survey Data” (1984). It is unlikely that Malinvaud knew about Lambert's research when working on his “Econometric Model for Macro-Disequilibrium Analysis” (1982). Lambert started his Ph.D. under Drèze's supervision in 1981.

equilibrium models, different approaches to aggregation, and data analytics concerns.<sup>43</sup> The following section will also show that on the applied side data analysis was quantitative rather than qualitative. The focus will be on early empirical research led with GEMR.

## 4. Data analysis

Laffont continued his project to discriminate between disequilibrium and equilibrium macroeconomics. Two empirical strategies can be distinguished. The first strategy was to confront Green and Laffont's (1981) model with macroeconomic data (Ducos et al, 1982; Ducos and Laffont, 1984). It was implemented with Green and Gilbert Ducos, a young scholar from Toulouse University.<sup>44</sup> The second empirical strategy was to test Malinvaud's (1982) model against business survey data (Boissou et al, 1984; 1986a; 1986b). It was implemented with Michel-Benoît Boissou, Laffont's Ph.D. student, and Quang Vuong, a young econometrician Laffont had met at INSEE and to whom he had secured an Assistant Professor position at Toulouse University (Vuong, 09/21/2021, PC).

### 4.1 The empirical superiority of disequilibrium over equilibrium macroeconomics

Green and Laffont (1981) specified an aggregate model analogous to Barro and Grossman's (1971). Their model featured an economy where a representative firm and a representative household interacted through two markets. There was the labor market, where labor was exchanged against money, and the market for goods, where a composite commodity was exchanged against money.

However, unlike Barro and Grossman (1971), Green and Laffont considered a stochastic framework. They specified the economic shocks' distribution and added error terms to supply and demand functions (1981: pp. 204-205). Then, Green and Laffont introduced stock behavior into the firm's optimization program (1981: pp. 201-202). This implied that besides the price,

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<sup>43</sup> This raises the issue of the interactions between the two sides of the general-equilibrium program. Unfortunately, neither published articles/books nor interviews allow to elaborate on the reactions of theoreticians. We only know that Grandmont, Hénin, and Bénassy showed interest in the estimation of GEMR (Avouyi-Dovi, 09/27/2021, PC; Hénin, 07/07/2021; Laffargue, 03/08/2023, PC). Note that we did Hénin's interview with Matthieu Renault.

<sup>44</sup> During his career, Laffont had positions in Paris and Toulouse. In Paris, after being at the CNRS, he was Assistant Professor in Economics at École Polytechnique (1979-1987) and Head of Studies at the École des Hautes Études en Sciences Sociales (1980-2004). In Toulouse, he became an Associate Professor in Economics at the University of Social Sciences (1977), where he was promoted Professor in Economics in 1979. Laffont remained Professor in Economics at Toulouse University until his death (2004). For more information on Laffont's academic career, see his resume: [http://idei.fr/sites/default/files/medias/doc/cv/laffont\\_04e.pdf](http://idei.fr/sites/default/files/medias/doc/cv/laffont_04e.pdf) (consulted on 1 July 2023). For a short biography of Laffont, see <https://www.ensae.org/fr/variances/article/la-promotion-2005-autour-de-colette-laffont/520> (consulted on 1 July 2023).

the wage rate, and eventual market rationing, the supply of goods and the demand for labor considered the differences between the desired and actual level of inventories. Finally, Green and Laffont (1981: p. 205) offered a theory of price formation. They assumed that market prices were set at their Walrasian levels before the opening of markets and that random shocks occurred within each market period.<sup>45</sup>

If random shocks corresponded to their average levels, the model would feature the functioning of an economy under market-clearing conditions (Green and Laffont, 1981: p. 205). If not, quantities would adjust to ensure the coordination of economic activities under non-market-clearing conditions. Therefore, the same stochastic process implied the estimation of different behavior. Green and Laffont (1981: p. 217) particularly observed that in the equation describing the dynamics of inventories, the average value of the error term would either be zero (under market-clearing conditions) or different from zero (under non-market-clearing conditions).<sup>46</sup>

Due to this stochastic difference, Ducos et al. (1982) focused on stock behavior to discriminate between equilibrium and disequilibrium macroeconomics. Based on data from INSEE and OECD, they estimated the French and American inventory equations from 1970-1978 (1982: p. 217). They showed that, unlike the disequilibrium hypothesis, the equilibrium hypothesis “was strongly rejected” in both cases (Ducos et al., 1982: p. 218). Later, Ducos and Laffont (1984) reached the same conclusion by analyzing Canadian and Japanese data (p. 199). Therefore, four different time series showed the empirical superiority of disequilibrium over equilibrium macroeconomics.

What about business survey data? From June 1974 to June 1982, the INSEE surveyed 4081 firms operating on the main sectors of the French economy (i.e., agricultural and food industries, intermediate goods, professional equipment, transportation, and consumption

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<sup>45</sup> According to Green and Laffont, “this assumption should be contrasted with that previously used in the disequilibrium literature where measured excess demands were responsible for price changes in the following period. Anticipatory pricing has the advantage of being simpler, especially in the analysis of the stochastic evolution of the system. Furthermore, as the empirical evidence does not provide support for the hypothesis that excess demand is a principal determinant of price changes, we felt that it was necessary to explore an alternative. Our assumption is somewhat intermediate between the Walrasian (flexible prices) and Keynesian short-run (fixed-prices) models” (p. 200).

<sup>46</sup> This was because of the non-linearity introduced by economic regimes: “If [the inventory equation under market-clearing condition] were estimated,  $K_0^{eq} \bar{s}$  [the desired value of inventories] would be the value of the constant. Moreover, if the data were partitioned into various subsets, the same constant would be consistently estimated in each of them. This should be contrasted with the case of disequilibrium. If the data were partitioned according to the various regimes, a different constant, namely  $K_0^{eq} \bar{s} + E\theta^i(\varepsilon_t)$ , with the expectation conditioned on regime  $i$ , would be observed in each regime” (Green and Laffont, 1981: p. 217).

goods). Boissou et al.'s goal (1984; 1986a; 1986b) was to use the resulting information to test the disequilibrium hypothesis (Vuong, 09/21/2021, PC).

INSEE's surveys involved questions concerning the firm's size, the entrepreneur's perception of the business climate, and her expectations concerning the future of the French industry. Boissou et al. (1984; 1986a; 1986b) focused on the answers entrepreneurs gave to the questions concerning i) the present situation of their firm – e.g., whether they experienced bottlenecks, excess capacity, or whether their productive capacity matched current demand; ii) the state of the labor market – e.g., whether they experienced difficulties in hiring or whether the wage rate was too high; iii) and the output of their firm – e.g., whether they experienced an undesired accumulation of stocks, whether they experienced an increase in delivery times, whether their price varied since the last survey, and whether they anticipated an increase in demand.

To analyze entrepreneurs' answers, Boissou et al. (1984; 1986a; 1986b) started with a model analogous to Malinvaud's (1982). Like Malinvaud (1982), they assumed that each firm had its own good and labor markets. Moreover, Boissou et al. (1984; 1986a; 1986b) assumed that the output of a given firm was either determined by demand, by its productive capacity, or by its full-employment capacity. The only difference was that Boissou et al.'s (1984; 1986a; 1986b) demand variable referred to an anticipated constraint, not to the actual constraint on markets.

This model served to categorize entrepreneurs' answers. On the one hand, Boissou et al. (1984; 1986a; 1986b) designed qualitative variables to identify the states of the labor and goods markets. The "indicator  $IQ$  [was] obtained from the answer to the question: 'If you received more orders could you produce more with your actual means?' If the firm [answered] YES, [they] presumed that the firm [was] constrained on its good market ( $IQ = 1$ ), while if the firm [answered] NO, [they] presumed that the firm [was] not constrained on its good market ( $IQ = 2$ ). Similarly, the indicator  $IL$  [was] obtained from the answer to the question: 'Do you now have difficulties in recruiting?' If the firm [answered] YES, [they] presumed that it [was] constrained on its labor market ( $IL = 2$ ), while if the firm [answered] NO [they] presumed that it [was] not constrained on its labor market ( $IL = 1$ )" (Boissou et al., 1986a: p. 115).

On the other hand, Boissou et al. (1986b) designed a qualitative variable ( $IP$ ) to describe price variations. "This variable [was] trichotomous and [was] constructed from the answers to the question, 'would you indicate the variation of your sales price (net of tax) since the last



survey?’ The first category [ $IP = 1$ ] was constructed so that it [corresponded] to an increase in real terms; the second category [ $IP = 2$ ], to a stability; and the third category ( $IP = 3$ ), to a decrease” (1986b: p. 408).

Boissou et al.’s (1984; 1986a, 1986b) indicators served to discriminate between disequilibrium and equilibrium macroeconomics. The first step was descriptive (Vuong, 09/21/2021, PC). Boissou et al. (1984; 1986a) used the values of  $IQ$  and  $IL$  to determine whether firms were constrained or not. They showed that whatever the sectors and the surveys considered, firms rarely stated to be in an equilibrium situation. Moreover, Boissou et al. (1984; 1986a) observed that changes in regimes were consistent with French economic history. For instance, they observed that the share of firms in a Keynesian regime decreased after Jacques Chirac’s (1975-1976) and Pierre Mauroy’s (1981-1982) economic stimulus. Boissou et al. (1984; 1986a) could therefore infer the existence of rationing on markets.<sup>47</sup>

The second step implied statistical tests (1984; 1986a; 1986b). The Granger causality test was critical in assessing the empirical relevance of disequilibrium macroeconomics (Vuong, 09/21/2021, PC). This was because it could determine whether the fixed-price assumption was empirically valid (Boissou et al., 1986b: p. 396). Boissou et al. (1986b) focused on the goods markets (p. 407). The question was “whether the price variations  $IP$  [were] strictly exogenous to the disequilibria indicator  $ID$ ” (1986b: p. 409). According to Boissou et al. (1986b), this amounted to “test the null hypothesis that  $ID$  [did] not cause  $IP$ ” (p. 408). Boissou et al. (1986b) showed that while  $IP$  caused  $ID$ , the reverse was not true (p. 409). They concluded that GEMR offered the relevant framework for macroeconomics and economic policy.

#### 4.2 Macroeconomic diagnoses

Unlike Laffont, Malinvaud did not implement his project.<sup>48</sup> However, Artus did. When he arrived at ENSAE (in 1982), he teamed up with Laroque and Michel to estimate a GEMR.<sup>49</sup> At

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<sup>47</sup> There is another reason why Boissou et al. (1984; 1986a) were confident with their descriptive statistics. They claimed that “these results [could] be compared with the *ex-post* probabilities of the different regimes obtained by Artus, Laroque, and Michel (1984). One major feature of their results [was] obtained here: namely, the predominance of the Keynesian unemployment regime” (1986a: p. 116). See 4.2 for information on Artus et al.’s (1984) empirical research.

<sup>48</sup> The likely explanation is a lack of time. Malinvaud headed INSEE from 1974 to 1987. Such a responsibility left little time for engaging in applied activities. This explanation is all the more relevant since estimating GEMR could be very time-consuming. For more information on the time necessary to estimate GEMR, see section 6.

<sup>49</sup> Artus joined the OECD’s economic department (1980-1982) after working on METRIC (1975-1980) at INSEE. Artus left OECD to become the ENSAE’s Dean of Studies (1982-1985); The former Dean was Laroque (1979-1982). When Artus replaced him, Laroque became the head of the INSEE’s Quarterly National Accounts Division (1982-1985). Michel, on his side, was an Assistant Professor at ENSAE (1979-1983).

the time, Artus recalled that in most OECD countries, policymakers addressed stagflation by increasing public expenditure (09/20/2021, PC).<sup>50</sup> Artus argued that had policy experts correctly diagnosed the nature of unemployment, they might have realized that stagflation did not result from a lack of demand. Therefore, the implementation of demand-side policies could have been avoided. Artus worked on the estimation of GEMR to prevent such policy mistakes from happening again.<sup>51</sup>

Artus, Laroque, and Michel (1984) estimated a GEMR of the French economy by maximizing a likelihood function. Like every likelihood function, Artus et al.'s described the distribution of endogenous variables (income and employment) conditional on the predetermined variables (e.g., money wage and price) and the parameter values (1984, p. 1392-1393). However, since the French economy could either be in a Classical, a Keynesian, an Underconsumption, or a Repressed Inflation regime, their likelihood function featured a sum of the densities associated with each regime.<sup>52</sup> Therefore, its maximization allowed determining the probability of observing the four different regimes at each point in time. It was a “by-product” of the estimation in the sense that the probability associated with each regime corresponded to the weight of its density in the global likelihood at a particular point in time (1984: p. 1401).

Moreover, Artus et al. measured markets disequilibria thanks to “simulations” (1984: pp. 1404). The first simulation concerned the labor and goods markets. “Given the coefficients of the model and the observations, in each regime where there [was] excess demand (resp excess supply) [Artus et al. computed] the mathematical expectation of the difference between effective demand and the traded quantity (respectively the difference between the traded quantity and effective supply). On each market, the aggregate excess demand [was] the algebraic sum of these four numbers, weighted by the probability of the regimes” (1984: p. 1404).

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<sup>50</sup> The Mauroy Government was a case in point (Artus, 09/20/2021, PC). Right after the election of François Mitterrand (05/10/1981), it implemented an expansionary fiscal policy (around 10 billion francs).

<sup>51</sup> Like Malinvaud, Artus also considered that GEMR and large-scale models were complementary tools. Artus envisioned that GEMR would diagnose the nature of unemployment and that large-scale models would simulate the effects of policies in line with the regime(s) detected (Artus, 09/30/2021, PC). Whether Laroque and Michel were on the same page is an open question.

<sup>52</sup> In the Keynesian regime, households are constrained on the labor market, and producers are constrained on the market for goods; in the Classical regime, households are constrained on both markets; in the underconsumption regime, producers are constrained on both markets; and in the Repressed inflation regime, households are constrained on the market for goods, and producers are constrained on the labor market.

The second simulation focused on the labor market. The INSEE offered a quarterly measure of employed workers. Artus et al. (1984) concluded that for each observation, a measure of unemployment could be obtained by subtracting this number from the estimated labor supply. However, this did not allow to distinguish between Keynesian, Classical, and “frictional” unemployment (1984: p. 1407).

Simulations served to distinguish between Classical and Keynesian Unemployment. Artus et al. (1984) assumed an increase in public expenditure. The share of unemployment that decreased was labeled “Keynesian,” and the rest was considered to be “Classical” (1984; p. 1407). The measure of “frictional” unemployment resulted from the estimation. Since the labor supply was a linear function of the active population, the amount of “frictional” unemployment would correspond to the difference between the active population and the estimated labor supply for each observation (1984: p. 1407).

The first finding concerns the importance of Classical vs. Keynesian unemployment in France between 1963 and 1978. Artus et al. (1984, pp. 1400-1401) showed that Classical unemployment dominated in the French economy in 1963, between the first quarter of 1968 and the second quarter of 1969, and in the early 1970s (from 71-1 to 73-2). However, they did not find that classical unemployment became dominant after the first oil shock. The dominant regime was Keynesian, with a probability almost equal to 1, from the last quarter of 1974 to the last quarter of 1978.

The second finding concerned the evolution of frictional unemployment. Artus et al. (1984) showed that frictional unemployment had risen from 303 000 workers in the last quarter of 1969 to 583 000 workers in the last quarter of 1978 (1984: p. 1408). They concluded that the French economy experienced an increase in frictional and Keynesian unemployment during the economic crisis of the 1970s.

Providing such a diagnosis advanced Malinvaud’s project. However, Artus et al. (1984) did not implement Malinvaud’s (1982) empirical strategy. On the one hand, they did not estimate a model involving representative agents and disaggregate markets. Artus et al.’s (1984: pp. 1388-1389) GEMR was analogous to Barro and Grossman’s (1971). On the other hand, Artus et al. (1984) did not use business survey data to identify economic regimes. Following in Gourieroux et al.’s (1980a) footsteps, they maximized a likelihood function to determine the

probability, for each observed point, to feature a Classical regime, a Keynesian regime, a repressed inflation regime, or an underconsumption regime.<sup>53</sup>

Whatever the empirical strategies, early estimations of GEMR aimed to implement Laffont's and Malinvaud's projects. Either with Ducos and Green or Boissou and Vuong, Laffont reached his goal of discriminating between disequilibrium and equilibrium macroeconomics. However, there was still a long way to go before fulfilling Malinvaud's project. Even if Artus et al. (1984) managed to provide macroeconomic diagnoses, their GEMR could not inform economic policy (Artus, 09/20/2021, PC). It was too simple (e.g., investment was exogenous and economic agents did not form expectations), there were concerns with the robustness of its empirical results (e.g., did the macroeconomic diagnoses change when considering a longer period of estimation?), and its estimation was too time-consuming (INSEE's computer took around 15 hours to maximize the likelihood function and, very often, found aberrant values –e.g., a level of income below zero).<sup>54</sup> Further work was therefore necessary before having a tool for policy analysis (Artus, 09/20/2021, PC). The following section will discuss the changes in the specifications of Artus et al.'s (1984) GEMR.<sup>55</sup>

## 5. Operationalization

Together with Avouyi-Dovi, a former student at ENSAE, and Laffargue, a colleague from CEPREMAP, Artus changed the specifications adopted in 1984. This resulted in estimating two other GEMR of the French economy, in 1987 and 1993.<sup>56</sup> Constants and differences between the 1984, 1987, and 1993 GEMR show that data analysis drove the search for microfoundations.

### 5.1 The empirical driver

Data analysis influenced Artus et al.'s (1984; 1987; 1993) search for microfoundations. The first evidence is how they specified firms' behavior. Artus et al. (1984: p. 1389) identified an

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<sup>53</sup> In 1984, business surveys provided external data to validate estimation results. For instance, Artus et al. observed that “between 1963 and 1968, in 1971, and again after 1974, the excess demand for goods and the capacity utilization [were] highly correlated” (1984: p. 1405). According to Artus (09/20/2021, PC), Gourieroux et al.'s (1980a) identification strategy was more reliable than Malinvaud's (1982).

<sup>54</sup> Artus et al.'s (1984) explanation of stagflation was also puzzling. Laroque (1986) acknowledged that it was problematic to find that the Keynesian regime was dominant in France in the 1970s (p. 362). The reason was that prices were expected to fall in a Keynesian regime, not rise. According to Laroque, specifications errors could explain why their GEMR did not detect an increase in Classical unemployment. Laroque (1986) concluded that changing the specifications adapted in 1984 was necessary.

<sup>55</sup> Robustness tests and how to simplify the estimation of GEMR will be discussed in sections 5 and 6.

<sup>56</sup> Laroque worked on the estimation of GEMR until the 1990s. However, he barely changed the 1984 specifications. See section 6 for details on Laroque and Salanié's research. Michel, on his side, stopped doing research on GEMR. He left the academia to work at the World Bank (1983-1986) and, later, at Saint Gobain.

incompatibility between data and Barro and Grossman's (1971) model. Barro and Grossman (1971) showed that an excess supply in the market for goods immediately decreased labor demand. However, data showed that firms took time before firing. In the early stage of recessions, firms preferred having short time working than laying off their employees. Conversely, in the early stages of recoveries, firms would tend to pay overtime instead of hiring. Therefore, data revealed lags in the adjustment of employment and, in turn, procyclical movements in per capita productivity (1984: p. 1389).

To account for the "productivity cycle," Artus et al. designed a new profit maximization program (1984: p. 1389). Its specificity was to include a cost for adjusting the level of employment (1984: p. 1389; 1987: p. 215; 1993: p. 3). Due to quadratic adjustment costs, firms would not hire or fire whenever an excess demand or supply occurred in the market for goods. It was optimal to adjust the employment level only when a recession or a recovery lasted. Artus et al. could therefore replicate the adjustment lag necessary for procyclical productivity changes.

The endogenization of investment is the second evidence of a causal relationship between data and microfoundations. Since investment decisions were central to short-run fluctuations, Artus et al. acknowledged that it was "awkward" to assume an "exogenous demand for investment in a quarterly model" (1984: p. 1388). Therefore, there was no choice but to endogenize investment to fit a GEMR to the data.

Moreover, Avouyi-Dovi recalled that when starting to work on the estimation of GEMR, existing investment models could not explain data (09/27/2021, PC). 1977-1979 was a case in point (Avouyi-Dovi and Muet, 1987). Investment had stagnated for almost three years while aggregate demand and profits had increased. Therefore, the accelerator and profit models were unable to explain the dynamics of investment. Filling this gap was another motivation to endogenize investment in a GEMR (Avouyi-Dovi, 09/27/2021, PC).

Third and finally, the disaggregation of the commodity market was also data-driven. Artus et al. (1987) explained their modeling choice by contrasting empirical results. On one side, Artus et al. (1984) had showed that "since the first oil shock, the French economy [had] almost always been in a situation of Keynesian unemployment" (1987: p. 213). On the other side, "Artus (1983a; 1983b; 1986) [had showed] that since 1979 the exportable supply [of industrial goods was] systematically lower than the demand and determined French exportation" (1987: p. 213).

According to Artus et al., “this contradiction had to be addressed to determine whether the French economy experienced Classical or Keynesian unemployment” (1987: p. 213). Their solution was to disaggregate the commodity market:

A possible explanation to the abovementioned contradiction would be the predominance of Classical unemployment in the traded sector and of Keynesian unemployment in the rest of the economy: the aggregated estimations would thus show the dominance of Keynesian unemployment because the non-traded sector represented a much larger proportion of the French economy. We want to assess whether this divergence between sectors explain the results obtained (1987: p. 214).

It follows the third and last evidence that Artus et al. (1984; 1987; 1993) adapted microfoundations to fit GEMR to data better.

## 5.2 The estimation constraint

The estimation of GEMR also constrained the search for microfoundations. How Artus et al. (1984; 1987; 1993) specified behavior in the labor market is a case in point.

Artus et al. (1984) were not satisfied with the specifications of the labor market. On one side, while there was evidence of a “discouraged worker effect” (p. 1396), Artus et al. did not consider that the level of unemployment could influence the labor supply.<sup>57</sup> More generally, they short-circuited the labor-leisure trade-off and assumed an exogenous labor supply (1984: p. 1388). Therefore, Artus et al.’s GEMR could not explain the change in labor supply, which was particularly problematic given the detected increase in frictional unemployment (1984: p. 1408).

On the other side, Artus et al. acknowledged that their “labor demand function was not satisfactory” (1984: p. 1401). Laroque (1986) elaborated on the problem. With Artus and Michel, he had found that the real wage had almost no role in the determination of Classical unemployment. However, since the standard error of the labor demand was high, they could

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<sup>57</sup> According to the US Bureau of Labor Statistics, “discouraged workers are a subset of persons marginally attached to the labor force. The marginally attached are those persons not in the labor force who want and are available for work, and who have looked for a job sometime in the prior 12 months but were not counted as unemployed because they had not searched for work in the 4 weeks preceding the survey. Among the marginally attached, discouraged workers were not currently looking for work specifically because they believed no jobs were available for them or there were none for which they would qualify.” Evidence of a discouraged worker effect can be found in Perry (1977) or Clark and Summer (1981).

not rule out the possibility that the result came from a poor specification (1986: p. 360). Laroque concluded that it was necessary to find new specifications.

Artus, Laroque, and Michel (1984) identified several avenues. They considered introducing the level of unemployment into the labor supply function (1984: p. 1396). Regarding the demand for labor, they claimed that “a possible direction for research would involve taking into account the intertemporal character of the firm’s decision by introducing a measure of expected wages and prices in the definition of the profit function” (1984: p. 1401). Laroque (1986) also considered substituting a putty-clay or a clay-clay production function to the Cobb-Douglas used in 1984 (p. 361). This would have allowed the past accumulation of capital to affect the demand for labor and, in turn, to explain classical unemployment (1986: p. 361).

However, Artus et al. (1987; 1993) did not explore these options. What is puzzling is that the specifications existed and, in some cases, served in the estimation of GEMR. For instance, Sneessens (1983; 1984) had estimated a GEMR involving a putty-clay function. Peter Kooiman and Teun Kloek (1985), on their side, had estimated a GEMR involving a clay-clay production function. Finally, after introducing the probability of unemployment into a labor supply function, Jonathan Eaton and Quandt (1983) had studied the US labor market and showed the empirical significance of the discouraged labor effect. Why then did Artus et al (1987; 1993) not incorporate these specifications into their GEMR?

Their estimation method was a significant constraint. Since Artus et al.’s (1984; 1987; 1993) likelihood function described the distribution of endogenous variables in every possible regime and under the assumption of stochastic supply and demand, adding an endogenous variable and modeling a new market increased the number of integrals and error terms composing the likelihood function. Therefore, the more the model involved endogenous variables and markets, the more complex the maximization of the likelihood function was.

While there were only four regimes and two endogenous variables in the 1984 model, Artus recalled how hard the maximization of the likelihood function was (09/30/2021, PC). According to Artus, INSEE’s computer could hardly handle a higher degree of complexity (09/30/2021, PC). Therefore, when Artus et al. (1987; 1993) disaggregated the commodity market, endogenized investment, and considered endogenous prices, they had to compensate for the increased complexity of their model. Their solution was to assume exogenous behavior

in the labor market and to preclude the possibility of having an excess demand. In 1987, they explained:

Since we are distinguishing between a traded-good sector and a non-traded-good sector, a number of simplifying assumptions are necessary to have a reasonable number of regimes. First, we have assumed that over the period of estimation (1965-1984), firms had always satisfied their demand for labor, that is to say that the labor market had always been in a situation of excess supply. Introducing the possibility to have an excess demand for labor would have implied to distinguish between sixteen regimes (1987: p. 214).

In 1993, Artus et al added:

We did not model the labor market, as we implicitly assumed the existence of a non-employed workforce and some rigidity of output, reflecting the rigidity of unemployment. The chief reason for our choice was the complexity of the disequilibrium estimation, which made it advisable to limit the model size (p. 3).

The constraint of Artus et al.'s estimation method can also be identified in light of what Sneessens (1983; 1984) and Kooiman and Kloek (1985) did. Laroque (1986) acknowledged that it was more complicated to estimate a GEMR involving a putty-clay or a clay-clay production function (p. 361). However, Kooiman and Kloek (1985) could maximize their likelihood function because their model involved only four regimes. Moreover, unlike Artus et al. (1984; 1987; 1993) or Kooiman and Kloek (1985), Sneessens (1983; 1984) did not consider stochastic supply and demand. Assuming instead that error terms affected the minimum condition allowed him to use an estimation method different from Artus et al.'s.<sup>58</sup> Since his method was easier to implement, Sneessens could estimate GEMR with more complex specifications, including a putty-clay production function.

It follows two other puzzles. On the one hand, there was tension between Artus et al.'s (1987; 1993) choice of specifications and the purpose of GEMR. By identifying the regime(s) in which an economy was, GEMR served to identify the nature of unemployment. So why did

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<sup>58</sup> Sneessens drew inspiration from Ginsburgh, Tishler, and Zang (1980) to design the stochastic structure of his GEMR (1980; 1981; 1983; 1984). Sneessens claimed that "the Ginsburgh-Tishler-Zang specification [offered] numerical and statistical advantages" (1981: p. 8). Kooiman and Kloek (1985: p. 325) also acknowledged that by adopting a stochastic structure different from Artus et al.'s (1984), Sneessens (1983; 1984) "[circumvented] the technical problems [linked to] the maximization of [their] likelihood function." In particular, it avoided having "a maximum likelihood function unbonded in most cases, and [involving] multiple integrals" (1985: p. 324).



Artus et al. (1987; 1993) choose to disaggregate the commodity market or to endogenize price instead of improving the specifications of the labor market? On the other hand, there was tension between Artus et al.'s (1987; 1993) choice of specifications and their estimation method. Had they changed their estimation method, Artus et al. might have been able to estimate a GEMR involving a better specification of the labor market. So why did they stick to the method used in 1984?

Concerns with the operationalization of GEMR might explain the first puzzle. Artus et al. (1987; 1993) developed their GEMR at OFCE, where Avouyi-Dovi contributed to the elaboration of MIMOSA.<sup>59</sup> Avouyi-Dovi recalled that at OFCE, many economists considered using GEMR and MIMOSA in policy analysis (09/27/2021, PC). Muet was a case in point. Like Artus, Avouyi-Dovi, and Malinvaud, Muet considered that GEMR and large-scale models were complementary tools. He aimed to use GEMR to provide macroeconomic diagnoses and MIMOSA to simulate policies in line with the regime(s) detected.

However, beyond the reliability of macroeconomic diagnoses, there was a condition for using GEMR in policy analysis (Avouyi-Dovi, 09/27/2021, PC). The condition was to develop a GEMR with a structure analogous to MIMOSA.<sup>60</sup> This raised the issue of changing the specifications of Artus et al.'s (1984) GEMR.

MIMOSA did offer a complex representation of the labor market. The labor supply, for instance, categorized households according to their age and considered the discouraged worker effect (Pisani-Ferry et al., 1990: p. 148). However, there was no need to have an analogous representation of the labor market in a GEMR (Avouyi-Dovi, 09/27/2021, PC).

Since MIMOSA was a multi-country model involving different types of industries, what mattered was to have a GEMR capable of distinguishing between traded and non-traded sectors (Avouyi-Dovi, 09/27/2021, PC). Moreover, since MIMOSA served to simulate the medium and long-term effects of policies, what mattered was to have a GEMR considering the price

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<sup>59</sup> MIMOSA was a multi-country model (around 5000 equations) developed by CEPII (Centre d'Études Prospectives et d'Informations Internationales) and OFCE. Its goal was to forecast the world economy's evolution and simulate the medium and long-run effects of economic policies. MIMOSA started to be used to inform economic policy in May 1989. For more information about this large-scale model, see "MIMOSA: une modélisation de l'économie mondiale" (Pisani-Ferry et al, 1990).

<sup>60</sup> While aiming to provide a tool for policy analysis, Artus et al. (1987; 1993) did not address the Lucas's (1976) Critique. On one side, like Malinvaud, they might have had reservations about its empirical validity. On the other side, they might have considered that although not entirely immune to the Lucas's Critique, GEMR provided a degree of protection since parameters changed depending on the regimes in which the economy was (Avouyi-Dovi, PC).

dynamics. Hence why Artus et al. (1987; 1993) preferred to disaggregate the commodity market and to endogenize price.

Moreover, Artus et al.'s (1987; 1993) choice reflected the need to guide policy analysis better. On one side, the disaggregation of the commodity market allowed to narrow macroeconomic diagnoses. The goal was to encourage the implementation of sectoral economic policies:

According to our estimations [...] an expansionary fiscal policy increases the trade deficit while stimulating significantly output in the sheltered sector; a general decrease in wages (accompanied by a decrease in wealth) curtails significantly output and investment in the sheltered sector while doing the reverse in the exposed sector. It is therefore necessary to stop designing policies at the scale of the whole economy, as if it was either in one regime or another (p. 233).

On the other side, the endogenization of price allowed to understand better the dynamics of the French economy (1993: p. 8). Therefore, the transition from one regime to another could be better explained, which could help better identify which policy to implement and when.

The second puzzle concerns Artus et al.'s (1984; 1987; 1993) estimation method. Like Artus, Avouyi-Dovi considered that business survey data should not serve to identify economic regimes (09/27/2021, PC). Therefore, even if methods like Lambert's (1984; 1988) simplified the estimation of GEMR, it is not surprising that Artus et al. (1987; 1993) did not use it. Having a reliable identification strategy was more important than improving the specification of the labor market.

However, Laroque and Salanié (1989) had designed a method simplifying the estimation of GEMR without changing Artus et al.'s (1984; 1987; 1993) identification strategy.<sup>61</sup> First, Laroque and Salanié no longer had to write down the GEMR's likelihood functions. Their method required to approximate the distribution of endogenous variables, conditional on exogenous variables and parameter values. It followed "pseudo maximum likelihood functions," obtained from Monte-Carlo simulations (1989: p. 835). Second, Laroque and Salanié circumvented the problems with the maximization of Artus et al.'s (1984) likelihood

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<sup>61</sup> Salanié started working with Laroque during his last year at ENSAE (1986). Salanié recalled that Laroque wanted to simplify the estimation method used in 1984. His idea was to use Monte-Carlo simulations to estimate GEMR. The resulting method was applied to a GEMR involving micro-markets in 1989.

functions (Salanié, 05/18/2021, PC). They could estimate the parameters of GEMR simply by minimizing the distance between the observed values, the mean, and the variance of simulations. Third and lastly, Laroque and Salanié explained that their estimation work was easier to monitor. The reason was that “an estimate of the residuals (i.e., the difference between the observed values and the mathematical expectation of the endogenous variables) [was] available at each stage as a by-product of the computation of the pseudo likelihood function” (1989: p. 846). So why did Artus et al. (1993) not use Laroque and Salanié’s method?

The problem was that it came too late (Artus, 09/30/2021, PC; Avouyi-Dovi, 09/27/2021, PC). Artus, Avouyi-Dovi, and Laffargue were closing their research on GEMR when Laroque and Salanié (1989) released their estimation method.<sup>62</sup> Given the investment necessary to get familiar with a new estimation method, they preferred to stick to the one used since 1984.

What was the scope of Artus et al.’s (1984; 1987; 1993) project? The following section addresses this question and, in the process, explores why the ENSAE’s band decided to stop doing research on GEMR.

## **6. Deadlock and new directions**

To determine the scope of Artus et al.’s (1984; 1987; 1993) project, the question is whether their GEMR eventually served to inform economic policy.

### **6.1 An unsatisfactory tool for economic policy**

Artus et al.’s (1984; 1987; 1993) GEMR did not serve to inform economic policy. Three reasons might explain why.

First, it took too much time to estimate their GEMR. In each case, the estimation work lasted approximately one year (Artus, 09/30/2021, PC; Avouyi-Dovi, 09/27/2021, PC). Setting aside the time necessary to build and clean datasets, Avouyi-Dovi indicated that it was three times longer than the estimation of MIMOSA.

The form of the likelihood functions caused the slowness of the estimation work (Artus, 09/30/2021, PC; Avouyi-Dovi, 09/27/2021, PC; Salanié, 05/18/2021, PC). Because they were

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<sup>62</sup> Artus et al (1990) had already estimated the GEMR involving a disaggregate commodity market, endogenous investment, and endogenous price.

highly non-linear and unbounded in certain regions of the parameter space, only simulations allowed computing their maxima. Artus et al.'s (1984; 1987; 1993) approach was to initiate the estimation with specific parameter values and, using the gradient technique, to change the values until finding a maximum. The problem was that each optimization run took a lot of time (around 15 hours!) and very often resulted in aberrant values or behavior – e.g., a level of income below zero or a positive relationship between the real wage and the labor demand. Moreover, given the non-linearity of the likelihood functions, Artus et al. (1984; 1987; 1993) had to perform many optimization runs to ensure not to be trapped in local maxima. Therefore, their GEMR could not be used to provide real-time diagnoses on the nature of French unemployment.

Second, there were concerns with the reliability of the macroeconomic diagnoses. Between 1984 and 1993, GEMR detected a dominant Classical regime for a growing number of quarters. However, the number of Keynesian regimes remained much higher (Artus, 09/30/2021, PC; Avouyi-Dovi, 09/27/2021, PC; Salanié, 05/18/2021, PC). It followed a lack of information to estimate parameters in the Classical regime. Moreover, the estimation of GEMR required much craftsmanship. Short-circuiting the labor-leisure trade-off or introducing a constant term into the labor demand equation were cases in point.<sup>63</sup> Artus et al. (1984) also had to change the precision of INSEE's computer to find the maximum of their likelihood function. It followed important reasons to distrust the diagnoses offered by their GEMR.

Third and finally, there were concerns with the robustness of empirical results. The differences between the results obtained in 1984 and 1985 are a case in point. To test the robustness of results, Artus et al. (1985) estimated again the 1984 model considering a longer period of estimation. This turned out to affect the results. For instance, the dominant regimes detected by the two GEMR differed in twelve quarters (63.2; 65.2-66.3; 68.1; 76.2-77.1). The difference of results over the period 1976.2-1977.1 was particularly problematic. Instead of finding a probability to be in a Keynesian regime close to 1, Artus et al. (1985) found that the French economy was in a Classical regime with a probability close to 1. Then, results were sensitive to Artus et al.'s (1984; 1987; 1993) estimation method. Artus et al. (1987) stressed that “their results [differed] quite considerably from those obtained when [...] the probability

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<sup>63</sup> In 1989, Laroque and Salanié explained that: “Artus et al [1984; 1985] had to depart from the original specification described in Table I by introducing an additional constant term in the equation defining  $L^d$  (the constant turned out to be barely significantly different from zero, but the authors could not dispense with it and find a local maximum of the likelihood function)” (1989: p. 846).

of regimes [was] inferred from [the INSEE's] business surveys (see the ongoing work of [Jean-Paul] Lambert and [Benoît] Mulkey [1987] or of [Frédéric] Gagey, J-P Lambert, and [Benoît] Ottenwaelter [1989])" (p. 235).<sup>64</sup> Finally, in the French version of the 1993 article, Artus et al. acknowledged that due to "the complexity of the estimation, results [were] sometimes inaccurate and fragile" (1990: p. 127). Therefore, there were good reasons not to use their GEMR in policy analysis. Does it explain why the ENSAE's band stopped doing research on GEMR?

## 6.2 Moving away from GEMR

When finalizing the estimation of their last GEMR, Artus and Avouyi-Dovi no longer worked at ENSAE and OFCE. They had been hired at the Caisse des Dépôts et Consignations (CDC), a public institution responsible for financial activities, including managing regulated savings, pensions plans, and long-term investments.

Their new job was why Artus and Avouyi-Dovi stopped doing research on GEMR (Artus, 09/30/2021, PC; Avouyi-Dovi, 09/27/2021). On the one hand, joining the CDC required to focus on financial economics. For instance, Artus started studying the effects of financial deregulation, particularly how the creation of a futures exchange would affect interest rates and investment. On the other hand, Artus and Avouyi-Dovi were responsible for creating an economics department at CDC. Therefore, besides the research constraints posed by CDC, they no longer had the time to develop GEMR.<sup>65</sup>

Then, there is the case of Gourieroux, Laffont, and Monfort. In the mid 1980s, they stopped doing research on GEMR because they had achieved their goals. Monfort recalled that for several years, "econometric problems prevented from estimating [GEMR]. There was the coherency problem, the difficulty to maximize a likelihood function involving spill-over

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<sup>64</sup> For instance, Lambert et al (1989) found that frictional unemployment increased marginally in France from 1964 to 1986.

<sup>65</sup> The same is likely valid for Michel when joining the World Bank. Laffargue, on his side, also stopped doing research on GEMR. The fact that Artus and Avouyi-Dovi had moved away from GEMR explained why he also did (Laffargue, 03/08/2023, PC). The other reason was that Laffargue developed models involving rational expectations. Laffargue was particularly interested in designing a computational method to solve such models. His research was instrumental in elaborating Dynare (Cherrier et al., 2023). Furthermore, the question arises as to why research on GEMR did not continue at INSEE and OFCE. While executive directors at both institutions recognized the distance yet to be covered before confidently using GEMR in policy analysis, there were also concerns regarding the costs associated with estimating GEMR (Avouyi-Dovi, PC). Extensive training would have been required for other staff members to estimate GEMR, and the workload involved in estimation was substantial. Consequently, the costs of advancing research on GEMR outweighed its potential benefits for policy analysis. This likely explains why neither INSEE nor OFCE continued to fund research on GEMR following the departure of Artus and Avouyi-Dovi.

effects, how to test the equilibrium hypothesis, how to forecast...”<sup>66</sup> Later, there was “the problem of how to estimate a GEMR involving micro-markets.” According to Monfort, “all these problems were resolved.”

Moreover, thanks to his work with Boissou, Ducos, Green, and Vuong, Laffont had managed to discriminate between equilibrium and disequilibrium macroeconomics. Further empirical research could have been done, but both business survey data and time series showed the empirical superiority of disequilibrium over equilibrium macroeconomics.<sup>67</sup> Therefore, neither Laffont nor Gourieroux nor Monfort had reasons to further research on GEMR.

At the same time, Monfort recalled that “other models, notably models involving rational expectations raised new econometric challenges.”<sup>68</sup> Monfort thus decided to change the orientation of his research. With Gourieroux and Laffont, he proposed methods to test New Classical Macroeconomics against data. Laffont, on his side, also continued to do research with Vuong. Together, they worked on the econometrics of game theory.

Last but not least, there are Laroque’s and Salanié’s cases. Individually or jointly, they used their pseudo maximum likelihood method to estimate GEMR involving lagged latent variables (Salanié, 1991; Laroque and Salanié, 1993; Laroque and Salanié, 1996). However, they eventually stopped doing research on GEMR in the mid-1990s. What triggered their decision was the feeling of being isolated in the research community (Salanié, 05/18/2021, PC). There was no reason to continue research on GEMR while almost nobody was any longer interested in their development.

## **7. New lights on the general-equilibrium program**

The ENSAE’s Band research shows an applied side to the general-equilibrium program. If Bénassy, Grandmont, or Younès considered that GEMR should offer general theories of how markets worked, the same is not true for Artus, Avouyi-Dovi, Laffont, Laroque or Malinvaud. According to Laffont, GEMR should discriminate empirically between equilibrium and disequilibrium macroeconomics. Moreover, Artus, Avouyi-Dovi, and Malinvaud considered

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<sup>66</sup> Excerpt from Monfort’s e-mail, sent on 21 May 2021.

<sup>67</sup> Commenting on Green and Laffont (1981), Portes (1981) recommended testing the equilibrium hypothesis considering another theory of price formation (p. 231).

<sup>68</sup> In his e-mail sent on 21 May 2021, Monfort referred to vector autoregressive (VAR) models. The econometrics of VAR models also became a research topic explored with Gourieroux and Laffont.

that GEMR should become a tool for policy analysis. Therefore, the roles and uses of GEMR were different on the theoretical and applied sides of the general-equilibrium program.

Second, the ENSAE's Band research shows that on the applied side, there were different forms of general-equilibrium models, different approaches to aggregation, and data analytics concerns. Its research also shows that econometric tests served to analyze data and that data analysis influenced the search for microfoundations. Therefore, the ENSAE's Band not only offered a different approach to the microfoundations of macroeconomics, but it also contributed to the emergence of a new class of general-equilibrium models.

Third, the ENSAE's band shows that research on GEMR did not stop due to the analytical shortcomings of GEMR (e.g., the lack of microfoundations for market price stickiness). On the one hand, Laffont showed the empirical superiority of disequilibrium over equilibrium macroeconomics. On the other hand, had they not joined CDC, Artus and Avouyi-Dovi might have furthered research on GEMR (Artus, 09/30/2021, PC; Avouyi-Dovi, 09/27/2021). They believed that the problems posed by the estimation of their GEMR (1984; 1987; 1993) were not insurmountable. More importantly, they are still convinced that GEMR could offer a relevant tool to inform economic policy.

Fourth and finally, applied research on GEMR left a mark on current (macro)economics. Their estimation led to the development of methods still being used today. Laroque and Salanié's (1989) is a case in point. Their simulation-based method is currently used to analyze data on financial markets (e.g., Roncalli, 2020), health (e.g., Hosseini et al., 2022), and on unemployment (e.g., Castex et al., 2022).

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