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Diagnosing Unemployment: The Dual Project of the ENSAE's Band[1]

Abstract

Throughout the 1980s and 1990s, there was a continuing effort to specify and estimate general equilibrium models with rationing (GEMR). However, this applied work has never been studied in a historical perspective. My article contributes to fill this gap. The focus is on the research led by French statisticians/econometricians including Patrick Artus, Sanvi Avouyi-Dovi, Christian Gourieroux, Jean-Jacques Laffont, Guy Laroque, and Alain Monfort. I show that there were two different motivations behind their work on GEMR: to inform economic policy and to advance data analytics. I explain the conditions under which each project emerged, account for their development, and discuss their scope. It follows a fresh perspective on the history of the microfoundations of macroeconomics.

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

1. The "ENSAE's Band"

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

> A number of European econometricians has 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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[2] GEMR had different labels including "fixed-price equilibrium models," "disequilibrium models," and "non-Walrasian models." Since many economists (e.g., Bénassy and Malinvaud) elaborated models where prices moved from one market period to another (Plassard et al., 2021), it seemed inappropriate to use the label "fixed-price equilibrium models." Then, despite the existence of dynamic models with rationing, I also chose not to use the label "disequilibrium models." The reason is that all the models considered in the 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 seems not specific enough. For instance, sunspot models are equally "non-Walrasian" and, yet, very different from GEMR.

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

In 1984, only four GEMR had been estimated with actual data, and all rested on highly simplified specifications – e.g., two aggregate markets, one composite good, one category of labor, and money. However, the situation changed. Guy Laroque and Bernard Salanié (1995) described a second wave of applications involving disaggregated and dynamic GEMR (e.g., Drèze and Sneessens, 1986; Lambert, 1988; Salanié, 1991; Artus et al, 1993). Throughout the 1980s and 1990s, there was therefore a continuing effort to specify and estimate GEMR. Our article aims to analyze this applied work in a historical perspective.

In existing surveys (Gourieroux et al, 1984; Laffont, 1985; Uctum, 1991; Andreassen, 1993; Laroque and Salanié, 1995), the relationships between GEMR and empirical data are often put in the background, and there is no account of 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?). Moreover, the surveys provide few information on how to analyze data with GEMR, on the results of empirical research, or on the motivations behind data analyses. They mainly focus on the econometric challenges posed by the estimation of GEMR.[4]

Historians, on their sides, focused on a more abstract form of application, i.e., the move from general theory to the analysis of specific economic problems (Backhouse and Boianovsky, 2013; De Vroey, 2016; Béraud, 2020; Plassard et al, 2021; Renault, 2022).[5] 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 (2022). 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 explain the various choice of specifications and estimation methods. Moreover, they barely addressed the analyzes of empirical data. Therefore, a wide range of applied activities has remained unexplored.

[3] The 1984 article was in French. All translations of French articles are mine.

[4] 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.

[5] Applying economics covers numerous activities (Backhouse and Biddle, 2000). "Application" can also involve participating in the design of economic policy.

To fill this gap, we focus on what Patrick Artus called the "ENSAE's Band."[6] Most of its members have already been mentioned –i.e., Artus, Avouyi-Dovi, Gourieroux, Laffont, Malinvaud, Monfort, Laroque, and Salanié.[7] This group of French statisticians and econometricians shared a common training, at the National School of Statistics and Economic Administration (ENSAE).[8] Then, they all taught at the ENSAE. Salanié notably recalled that while he was a student at ENSAE (1984-1986), he had Gourieroux and Monfort in statistics, Malinvaud in growth economics, and Laroque in microeconomics (05/18/2021, PC). 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).

While working on GEMR, the ENSAE's Band had two goals. The first goal, shared to varying degrees by all its members, was to advance data analytics. The second goal, driving essentially Artus' and Avouyi-Dovi's research, was to inform economic policy. It follows two distinct but complementary projects. I explain the conditions under which each project emerged, account for their development, and discuss their scope.

2. The move towards data analysis

Early research on GEMR focused on the microfoundations of macroeconomics and problems specific to general-equilibrium theory – e.g., the centralization of economic activity, the integration of money and value theory, and the stability of Walrasian equilibrium (Backhouse and Boianovsky, 2013, De Vroey, 2016; Béraud, 2020, Plassard et al, 2021). However, economists were not only concerned with the development of theory. Early on, there was interest in the estimation of GEMR.

2.1 Early interest

The first sign of interest came from Donald Tucker, in the United States. In a study on the speed of monetary policy, Tucker (1968) argued that spill-over effects had implications for empirical research. This was clear from John Kareken and Robert Solow's (1963) distributed lags model (1968: p. 83). While assuming lags in investment response and interest rate sluggishness, Kareken and Solow did not consider that investment could be constrained by available bank

[6] Excerpt from Artus' interview, conducted remotely on 30 September 2021. Interviews with Sanvi Avouyi-Dovi and Salanié will also be used in my study. Any except of interviews shall be indicated by the acronym PC (for Personal Conversation) in the article.
[7] Gilles Michel and Alain Trognon were also part of the ENSAE's Band.
[8] The ENSAE is the INSEE's school. For details on its history, see Alain Desrosières (2013, Chapter 11).

loans (1968: p. 83). Their model did not include the possibility of credit rationing and the associated spill-over effect on investment decisions (1968: p. 84). Tucker concluded that Kareken and Solow's investment demand function was not well specified, which questioned their results on lags of monetary policy (1968: p. 84).

Then, Tucker (1971) argued that switching regimes had implications for empirical research (pp. 73-74; p. 81). A comparison between a GEMR and the "Federal Reserve-MIT" model (De Leeuw and Gramlich, 1969) aimed to prove it.[9] Due to market price stickiness, both frameworks allowed disequilibrium transactions (1971: p. 81). However, the FMP model did not consider that an economy could switch from one regime to another. Its estimation was performed assuming that aggregate demand constrained aggregate supply. It followed an inability to test whether an economy had experienced switching regimes (1971: p. 81). It also followed misspecified behavior, which undermined the FMP's empirical results (1971: p. 81). Tucker concluded (1971) that the estimation of GEMR should be on Central Banks' research agenda (1971: p. 59; p. 81).

Second, there was interest in estimating GEMR with centrally planned economies (CPEs)' data. Early interest came from David Howard and Richard Portes. On the one hand, both economists sought to improve knowledge of CPEs. Applying GEMR would have showed that neoclassical theory worked behind the Iron Curtail (Portes, 1974; Howard, 1975). More importantly, it would have helped to determine whether CPEs experienced chronic excess demands on markets (Portes, 1974; Howard, 1975).

On the other hand, Howard and Portes sought to reflect on anti-inflation policy. In a letter sent to Grossman on 4 March 1974, Portes claimed that his interest in GEMR came from "the work [he] was currently doing on repressed inflation in Soviet-type economies and its implications for price and wage control policies in mixed-economies."[10] Howard, on his side, claimed that "the Barro-Grossman disequilibrium model [provided] a framework for analyzing the effects of repressing inflation by means of price controls" (1976: p. 871). According to Howard, GEMR allowed to investigate what would happen if authorities set price and wage below their equilibrium value (1976: p. 871). The question was whether Barro and Grossman

[9] Besides the Massachusetts Institute of Technology (MIT) and the Federal Reserve, the University of Pennsylvania contributed to the elaboration of what was later called the "FMP" model. For details on the history of the FMP model, see Acosta and Rubin (2019), Backhouse and Cherrier (2019), and Rancan (2019).
[10] Grossman's papers, Box 2 OF-IUF-G5, John Hay Library Special Collections.

(1971; 1974) were right in predicting an increase in the effective saving demand, a decrease in the effective labor supply, and a decrease in effective output (1976: p. 871).

Third and lastly, there is the case of Malinvaud (1977). Unlike Portes (1974) or Howard (1975; 1976), Malinvaud did not work on the estimation of GEMR in *The Theory of Unemployment Reconsidered* (1977). However, Malinvaud (1977) had interest in estimating GEMR. His methodology allows to understand why (Renault, 2022). Malinvaud used theory to find new specifications for large-scale macroeconometric models. He also considered that the new specifications and their implications had to be tested against data. Why would GEMR be an exception? This seems even less likely when knowing that in 1980, Malinvaud welcomed John Muellbauer and David Winter's (1980) estimation of a GEMR of the British manufacturing sector.

Moreover, Malinvaud (1977) used his GEMR to explain stagflation. According to Malinvaud, real wage increases were "partly responsible" for the rise of unemployment during the first half of the 1970s (1977: p. 108). Malinvaud also argued that despite the reappearance of "Keynesian" unemployment in 1975, "there [were] signs that [Classical unemployment] would soon again remerge" (1977: p. 109). Lastly, Malinvaud claimed that "profitability may for long remain too low for firms to wish to develop production" (1977: p. 109).

Was Malinvaud right in predicting that during stagflation, "Classical" unemployment would occur more frequently than "Keynesian" unemployment? What were the periods during which unemployment was due to a lack of profitability and the periods during which it was due to demand deficiency? To what extent the lack of profitability and demand deficiency explained stagflation? Only the estimation of GEMR could tell.

1.2 Disequilibrium econometrics

The estimation of GEMR required changes in econometric methods. Tucker (1971) explained why.

On the one hand, the "usual approach" to estimation was not relevant in a GEMR (1971: p. 74). What would happen if econometricians ran regressions by considering that every observed point belonged to supply and demand functions? Tucker argued that the estimated parameters were likely to be inconsistent. He made this point by taking the example of effective demand estimation. The inconsistency of parameters would come from a problem of endogeneity. "The true dependent variable [was] measured with error whenever that demand

[was] not satisfied, and this error [was] correlated with predetermined variables [e.g., market prices]" (1971: p. 75). Moreover, Tucker argued that econometricians would not be able to test whether some markets experienced rationing in the economy (1971: p. 81). Since disequilibrium was not allowed in the maintained hypothesis, the estimation procedure could not give information about the existence and extent of market disequilibria.[11]

On the other hand, Tucker raised the issue of how to address endogeneity. According to Tucker, a standard solution was to use a two-stage least squares method – i.e., to substitute "values predicted by an estimated reduced-form relationship" to explanatory variables (1971: p. 74). The problem was that in a GEMR, "some or all of the reduced-form relationships [changed] whenever any market [switched] from excess demand to excess supply" (1971: p. 74). Tucker concluded that the standard two-stage least squares method could not be applied to GEMR. Which method(s) could then be appropriate for estimating GEMR?

Tucker identified several avenues, including disequilibrium econometrics (1971: p. 76). This econometric method, originally designed by Ray Fair and Dwight Jaffee (1972), eventually served to estimate GEMR. Three reasons might explain why.

First, Fair and Jaffee 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 either be determined by demand or by supply. Fair and Jaffee had therefore found a way to estimate a model involving regimes switching. Second, Fair and Jaffee expanded their model to include a price adjustment rule. The rule served to gather information about the sign and extent of the excess demand on a given market. If there was a strong increase in price, for instance, Fair and Jaffee concluded that the market was in a situation of excess demand and that the rationing was important. Therefore, they could address the lack of information on effective supply and effective demand.[12] Third, thanks to the maximization

[11] Howard's (1976) empirical strategy can be questioned on similar grounds. Since Howard did not allow equilibrium in the maintained hypothesis, he could not test the existence of excess demands on markets. Portes and Winter (1980) explained: "[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 taking *c* as exogenous (for households) and estimating [Howard's labor supply] would be a specification error" (p. 143). A proper testing framework therefore required to allow equilibrium and disequilibrium in the maintained hypothesis (1980: p. 138).

[12] According to Tucker, "the most serious estimation difficulties arise from the absence of complete data when, as is usually the case, the data available measure only the actual quantity exchanged in each market and not unrealized demands" (1971: p. 74).

of a likelihood function, Fair and Jaffee could calculate the probability for each observed point to be generated by an excess supply or by an excess demand. Disequilibrium econometrics therefore allowed to address the kind of empirical problems posed by Malinvaud or Portes.

However, the road to estimate GEMR was still long in 1972. On one side, in the wake of Fair and Jaffee (1972), research focused on the analysis of isolated markets. This is clear from Laffont and Monfort's (1976) review of disequilibrium econometrics (e.g., Maddala and Nelson, 1974; Amemiya, 1975; Goldfeld and Quandt, 1975). Laffont and Monfort indicated that in existing models, economic agents never incorporated quantitative constraints from other markets (1976: p. 5).

On the other side, the estimation of models incorporating spill-over effects was challenging. According to Quandt (1976), a general equilibrium may not exist when a rationing in one market affects demand and supply in another. Moreover, non-linearities within regimes could lead to multiple solutions. Quandt (1976) concluded that the existence of a reduced form was not guaranteed, which questioned the very possibility to estimate GEMR.

Besides this "coherency issue," Monfort recalled that in 1976, when starting to work with Gourieroux and Laffont, the procedure to maximize a likelihood function with spill-over effects was not identified.[13] 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 parameters represented global maxima. As a result, there was a gap between disequilibrium econometrics and GEMR.

Gourieroux, Laffont, and Monfort (1980) contributed to close this gap. They showed that a reduced form existed in a GEMR *à la* Barro and Grossman (1971). Moreover, thanks to an uphill maximization algorithm, they computed the global maxima of a likelihood function involving four economic regimes (1980: p. 87; p. 93). Therefore, Gourieroux, Laffont, and Monfort (1980) proved that a GEMR could be estimated.

The same is true for Ito (1980) and Henri Sneessens (1980). Like Gourieroux et al (1980), Ito's and Sneessens' estimation methods worked for GEMR involving only a

[13] Monfort's e-mail, sent on 21 May 2021. In line with Monfort's recollection, Takatoshi 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).

consumption good, labor, and money. However, they adopted different specifications for spill-over effects and for the stochastic structure of GEMR. For instance, instead of assuming stochastic supply and demand, Sneessens added an error term to the minimum condition (1980: p. 9). Moreover, Ito and Sneessens offered different estimation techniques. On one side, Ito showed that besides the maximum likelihood method, the two-stage least squares method could also be used in GEMR (1980: pp. 116-119). On the other side, Sneessens showed that a GEMR could be estimated by mixing disequilibrium econometrics with the method of ordinary least squares (1980: p. 9). Therefore, starting from 1980, there was a variety of methods to estimate GEMR. It remained to estimate GEMR with actual data.

1.3 Diagnosing unemployment

Throughout the 1980s, GEMR served to analyze Western and Eastern economies' data (Plassard and Renault, 2022). On both sides of the Berlin Wall, research focused on the identification of economic regimes and the quantification of disequilibria on markets.[14] Our goal is to elaborate on this empirical work. The focus is on "Estimation of a Quarterly Macroeconomic Model with Quantity Rationing" (Artus et al, 1984).

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 parameters values (1984, p. 1392-1393). However, since the French economy could either be in a Classical, a Keynesian, a Underconsumption, or a Repressed Inflation regime, their likelihood function really featured a sum of the densities associated with each regime. Its maximization therefore allowed to determine the probability of observing the four different regimes at each point of time. It was a "by-product" of the estimation in the sense that the probability associated with each regime simply 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

[14] The case studies include Belgium (e.g., Sneessens, 1981; Lambert, 1984), Czechoslovakia (e.g., Dlouhy, 1984), France (e.g., Artus et al, 1984), Hungary (e.g., Hulyák, 1985), Poland (e.g., Charemza and Gronicki, 1985; 1988), Netherlands (e.g., Kooiman and Kloeck, 1985), and Switzerland (Stalder, 1989). In Eastern Europe, research focused on the existence and extent of excess demands on markets (Plassard and Renault, 2022).

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).

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 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 simply 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).

Two main findings followed. 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 the dominant regime 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.

In 1986, Laroque reflected on this picture of stagflation. He acknowledged that it did not leave much room to Malinvaud's (1977) diagnosis (1986: p. 350). However, Laroque did

not conclude that Malinvaud was wrong. He instead wondered whether "alternative specifications […] could confirm Malinvaud's intuition" (1986: p. 541), namely that France and other Western economies experienced a rise in Classical unemployment during the 1970s. This raises the issue of how the confrontation between GEMR and empirical data affected the search for microfoundations. It is the topic of the next section.

3. An applied general-equilibrium program

Together with Avouyi-Dovi and Laffargue, Artus changed the specifications adopted in 1984.[15] This resulted in the estimation of two other GEMR of the French economy, in 1987 and 1993.[16] Constants and differences between the 1984, 1987, and 1993 GEMR show that data analysis influenced the search for microfoundations without breaking with the "general-equilibrium program" (Hoover, 2012).

3.1 The Hicksian logic

To feature the general-equilibrium program, Hoover (2012: pp. 35-36) focused on *Value and Capital* (Hicks, 1939). According to Hoover (2012), Hicks's program of microfoundations began with the analysis of optimization behavior. Hicks situated individuals in a Walrasian general equilibrium and determined optimal levels of production and consumption. Then, Hicks introduced time in Walrasian theory and analyzed market dynamics in a temporary equilibrium framework. According to Hicks, this move to dynamics was the precondition for macroeconomics. The last step was to design an aggregate model, to move from micro to macroeconomics. Hicks's solution was to rule out heterogenous behavior by applying the "composite-commodity theorem." The resulting model therefore involved representative agents and composite commodities.

A similar logic can be identified in 1984, 1987, and 1993. First, each GEMR involved individual optimization behavior. The specificity *vis-à-vis* Hicks (1939) was that optimization programs could be adapted to Walrasian and non-Walrasian scenarios. For instance, the supply of goods resulted from firms' maximization of profits and could either be "notional" or

[15] Laroque worked on the estimation of GEMR until the 1990s. However, he barely changed the 1984 specifications. See section 4 for details on Laroque and Salanié's research. Michel, on his side, stopped doing research on GEMR. He left the academia to work at Saint Gobain.

[16] Artus, Avouyi-Dovi, and Laroque (1985) also estimated a GEMR. The main change with respect to the estimation performed in 1984 was the period under scrutiny. The period of estimation was no longer [63-2; 78.4], but [63-2; 81-4]. Except a marginal change in the production function, Artus et al (1985) did not modify the specifications adopted in 1984.

"constrained" (1984: p. 1389). The same applied in 1987 and 1993. What changed was that based on anticipations of future rationing, firms also decided on their level of investment (1987) and prices (1993).

Second, like Hicks (1939), Artus et al (1984, 1987, 1993) linked macroeconomics with dynamics. To diagnose the nature of unemployment, they developed a temporary equilibrium framework. In 1984, they explained that:

> The estimation of the model has been made under the basic assumption that prices and wages were predetermined and, consequently, that the observed evolution of the economy was best described as a sequence of fix price equilibria with quantity rationing (1984: p. 1409).

What changed between 1984 and 1993 was the determinants of markets dynamics. In 1984, income and employment levels differed from one quarter to another because of exogenous changes in prices and wages. However, starting from 1987, the dynamics of the economy also became dependent on investment decisions and the resulting variations in the stock of capital. Finally, in 1993, the sequence of temporary equilibria depended on the amount of investment and the level of prices chosen by firms.

Third and lastly, Artus et al developed models with representative agents and composite commodities. In 1984, the GEMR involved one representative firm, one representative consumer, and two aggregate markets: the commodity market and the labor market on which commodity and labor were exchanged against money.

The originality *vis-à-vis* Hicks (1939) was that Artus et al ended up disaggregating the commodity market. In 1987 and 1993, they distinguished between a commodity sold abroad and a commodity sold domestically. It followed a "traded" and a "non-traded good" sector in which there was room for heterogeneity between firms. For instance, Artus et al (1987) showed that firms' investment decisions mainly depended on anticipated demand in the non-traded good sector while they depended on profitability in the traded-good sector (1987: p. 218).

However, data determined heterogeneity between firms, not theory. Firms had the same optimization program in each sector (1987: pp. 215-216; 1993: p. 3). Then, the composite-commodity theorem applied in the traded and in the non-traded sectors. Artus et al (1987; 1993) assumed that in both sectors, a representative firm produced one composite good. They also kept assuming an aggregate labor market in which a representative household sold labor against

money. Last but not least, Artus et al (1984: p. 1396; 1987: p. 228; 1993: p. 5) ruled out distribution effects. For instance, in 1987, households' disposable income resulted from "total income (the sum of added values in both sectors less exogenous transfers) which implied that the distribution of income between wages and profits had no effect on consumption" (p. 228). Therefore, Artus et al did not break with the general-equilibrium program.

3.2 The empirical driver

Empirical data 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 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 resulted in a decrease in the 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. Data therefore 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 an excess 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 lags necessary to have procyclical changes in productivity.

The second evidence of a causal relationship between data and microfoundations is the endogenization of investment. 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).[17] To fit a GEMR to the data, there was therefore no choice but to endogenize investment.

[17] There were also concerns with the "exogenous demand for inventories" (1984: p. 1388). Artus recalled that the non-consideration of inventory change was particularly problematic for Laroque (09/20/2021, PC). According to Artus, Laroque was convinced that inventory change was central to the short-run fluctuations of economic activity.

Moreover, Avouyi-Dovi recalled that when starting to work on the estimation of GEMR, existing models of investment could not explain data (09/27/2021, PC). The period 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. The accelerator and profit models were therefore 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. To explain their modeling choice, Artus et al (1987) started to contrast 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 adapted microfoundations to better fit GEMR to data.

3.3 The estimation constraint

The confrontation between GEMR and data 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.[18] 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 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 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 to substitute 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

[18] 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).

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 major 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/or 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 harder 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 maximalization of the likelihood function was (09/30/2021, PC). According to Artus, the 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 the increased complexity of their model. Their solution was to assume exogenous behavior in the labor market and to preclude the possibility to have 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.[19] Since the method was way much easier to implement, Sneessens could estimate GEMR with more complex specifications, including a putty-clay production function.

To conclude, while searching for microfoundations, Artus et al faced a trade-off between empirical relevance and econometric tractability. The disaggregation of the commodity market and the endogenization of investment show that they did their best to fit GEMR to the data.[20] However, the constraint posed by their estimation method was strong and prevented from improving the specifications of the labor market. This raises the issue of the scope of Artus et al's program of microfoundations. It is the topic of the next and last section.

4. Two goals, two fates

To determine the scope of Artus et al's program, it is necessary to distinguish between its policy and analytical goals.

4.1 Economic policy and data analytics

Artus et al's (1984; 1987; 1993) analyses were not only positive. They were also normative (1984: p. 1409; 1987: pp. 232-235; 1993: pp. 8-9). For instance, in 1993, Artus et al wondered "what types of fiscal and incomes policy should the French government have pursued in the 1980s?" Based on policy simulations, they claimed that "output, employment, and prices in both sectors, as well as the external balance, would have benefited from stricter wage restraint.

[19] 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).

[20] Artus et al were also concerned with theoretical consistency. Their decision to endogenize prices is a case in point. They explained: "[In 1987], we attributed exogenous movements to prices and wages rates. But how could we explain the prevalence of excess demand for traded goods in 1979-1984, when the traded producers could have eliminated the imbalance simply by raising their sales prices? The present paper seeks to meet this objection. While maintaining exogenous real wage rates, we introduce endogenous prices" (1993: p. 3).

[However] a rise in government consumption of manufactured goods would have had a negligible impact on their output while sharply worsening the trade deficit" (1993: p. 9).

This normative view provides information on Artus et al's (1984; 1987; 1993) motivations. When diagnosing the nature of unemployment, their goal was to design a tool to inform economic policy. However, GEMR did not intend to simulate the effects of economic policies (Artus, 09/30/2021, PC; Avouyi-Dovi, 09/27/2021, PC). GEMR intended to diagnose in real-time the nature of French unemployment and, based on the results, to determine broad guidelines for economic policy.[21]

Artus recalled that in most OECD countries, policymakers addressed stagflation by increasing public expenditure. It took some time before they realized that the economic crisis resulted from supply shocks and, in turn, required policies decreasing the cost of labor and stimulating the accumulation of capital. The Mauroy Government was a case in point. Right after the election of François Mitterrand (05/10/1981), it implemented policies adapted to an economy experiencing Keynesian, not Classical unemployment. Preventing such policy mistakes from happening again was the motivation behind Artus et al's program.

Moreover, there was an ambition to advance data analytics. Artus and Avouyi-Dovi were fascinated by the estimation work on GEMR.[22] However, the resolution of the associated problems can hardly be separated from their ambition, with Laffargue, to inform economic policy. GEMR had to be estimated to become a tool for policy analysis.

Laroque's case is different. On the one hand, Avouyi-Dovi recalled that Laroque used to spend hours at the ENSAE's computing center to improve the Job Language Control. He was particularly interested in increasing the number and the complexity of tasks that computers could perform without the supervision of operators. His goal was to ease the estimation work and to increase its speed of execution (Avouyi-Dovi, 09/27/2021, PC).

[21] According to Artus, GEMR and large-scale macroeconometric models were complementary tools. GEMR would diagnose the nature of unemployment, and large-scale macroeconometric models would simulate the effects of policies in line with the regime(s) detected. For instance, if the GEMR had showed a high probability to be in a Classical regime, the economists developing large-scale macroeconometric models would have to simulate the effects of supply-side policies – e.g., a decrease in employer's social contribution or a decrease in the production tax (Artus, 09/30/2021, PC).

[22] Since there was no ready-made software for estimating GEMR, Artus et al (1984; 1987; 1993) had to design their own maximization modules. This component of the estimation work fascinated Artus and Avouyi-Dovi. While interviewed, they recalled their long and stimulating discussions on how to encode and maximize the likelihood functions. Avouyi-Dovi also claimed that learning about FORTRAN was one of the reasons why he began to work on the estimation of GEMR.

On the other hand, Laroque was interested in designing new methods for estimating GEMR (Salanié, 05/18/2021, PC). Early on, Laroque and Gourieroux worked on a method allowing to aggregate rationing estimated on different micro-markets. Then, Salanié recalled that Laroque had the idea to use Monte-Carlo simulations to estimate GEMR. According to Salanié, the goal was to simplify the estimation method used in 1984. Finally, Laroque and Salanié worked on the estimation of GEMR involving price and wage adjustment rules. The goal was to design a method for addressing the issue of lagged latent (unobserved) variables. Therefore, there were two sides to Artus et al's program. The question is whether the associated goals have been achieved.

4.2 An unsatisfactory tool for policy analysis

Artus et al's (1984; 1987; 1993) GEMR never served to inform 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, a large-scale macroeconometric model involving around 200 endogenous variables and 500 equations.[23]

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 highly non-linear and unbounded in certain regions of the parameter space, only simulations allowed to compute the maxima. Artus et al's (1984; 1987; 1993) approach was to initiate the estimation with specific parameters 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 a lot of optimization runs, just to make sure not to be trapped in local maxima. Therefore, their GEMR could not be used to provide real-time diagnosis on the nature of French unemployment.

Second, there were concerns with the reliability of the diagnoses. Between 1984 and 1993, there was a growing number of quarters for which GEMR detected a dominant Classical

[23] For details on the MIMOSA model, see Pisani-Ferry et al (1990).

regime. 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. Then, a lot of judgment was necessary to perform the estimation of GEMR. Short-circuiting the labor-leisure trade-off or introducing a constant term into the labor demand equation were cases in point.[24] Finally, the estimation of GEMR required a lot of craftmanship. For instance, Artus et al had to change the precision of the INSEE's computer to find the maximum of the 1984 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 (Avouyi-Dovi, 09/27/2021, PC). 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 is 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. In 1987, Artus et al stressed that "their results [differed] quite considerably from those obtained when […] the probability of regimes [was] inferred from [the INSEE's] business surveys (see the ongoing work of [Jean-Paul] Lambert and [Benoît] Mulkay [1987] or of [Frédéric] Gagey, J-P Lambert, and [Benoît] Ottenwaelter [1989]" (p. 235).[25] 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). There were therefore good reasons not to use their GEMR in policy analysis.

4.3 Methodological breakthrough

The situation was different on the analytical side of Artus et al's program. On the one hand, Laroque and Salanié (1989) designed a simulation-based method for estimating GEMR. They began to apply it "to the model of Artus et al (1985), with the same specifications, the same

[24] 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 significatively different from zero, but the authors could not dispense with it and find a local maximum of the likelihood function)" (1989: p. 846).

[25] For instance, Lambert et al (1989) found that in France, frictional unemployment increased marginally over the period 1964 to 1986.

data set, and on the same period" (1989: p. 842). The only difference was that Laroque and Salanié (1989) assumed several "micro markets," in different locations (pp. 835-836). Then, using the method designed with Laroque (1989), Salanié estimated a two-markets GEMR in which the effective excess demands in t affected price and wage levels in $t + 1$ (1991: p. 5). Laroque and Salanié (1989) had therefore found a solution to the problem of aggregation and lagged latent variables.[26]

On the other hand, their method was easier to use than Artus et al's (1989: p. 846). First, Laroque and Salanié no longer had to write down the GEMR's likelihood functions. Their method simply required to approximate the distribution of endogenous variables, conditional on exogenous variables and parameters values. It followed "pseudo maximum likelihood functions," obtained from Monte-Carlo simulations (1989: p. 835; 1991: p. 6). Second, Laroque and Salanié circumvented the problems with the maximization of Artus et al's (1984; 1987; 1993) likelihood 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). Laroque and Salanié therefore achieved the analytical goals of Artus et al's program.

5. New lights on the general-equilibrium program

Artus et al (1984; 1987;1993) estimated GEMR to diagnose the nature of French unemployment. Moreover, the confrontation between GEMR and data affected their search for microfoundations. Therefore, the general-equilibrium program was not only theoretical, but also applied.

Then, if Bénassy (1973; 1975), Grandmont (1976; 1977), or Younès (1975) considered that GEMR should only serve to explain how capitalist economies worked, the same is not true

[26] Despite progress, Laroque and Salanié (1995) acknowledged the need to further research on non-linear econometrics. For instance, there was still a long way to go before addressing properly the non-stationarity of macroeconomic variables. Laroque and Salanié observed that "most authors [had] chosen to pretend that the presence of nonstationary variables did not invalidate their estimators, or (an even less palatable assumption) their test statistics. [Laroque and Salanié] unfortunately [had] nothing better to propose at this stage" (1995: p. 349).

for Artus et al (1984; 1987; 1993). In their view, GEMR intended to inform economic policy. Moreover, Gourieroux's, Laffont's, Laroque's, Monfort's, and Salanié's cases show that GEMR also served to advance data analytics. There was therefore a great diversity in the roles and uses of GEMR.

Finally, GEMR left a mark in the history of (macro)economics. Their estimation led to the development of methods that are 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).

All this shows that there were blind spots in the history of the general-equilibrium program. Our article suggests that some still exist. The estimation of GEMR in Eastern Europe is the first blind spot. GEMR served to determine whether and to what extent Czechoslovakia (Dlouhy, 1984), Hungary (Hulyák, 1985; 1989), or Poland (Charemza and Gronicki, 1985; 1988) experienced excess demands on markets (Plassard and Renault, 2022). What were the points of convergence and divergence with the applied work performed in Western Europe? How empirical research on GEMR influenced the development of macroeconomics in Eastern Europe?

The second blind spot is the relationship between GEMR and large-scale macroeconometric models. According to Renault (2022), Malinvaud used GEMR to identify new specifications and to rationalize some practices in the estimation of large-scale macroeconometric models (e.g., the use of tension indicators). The same applied to Muet (1979) or Dominique Bureau, Didier Miqueu, and Michel Norotte (1984a; 1984b). However, Artus et al's (1984; 1987; 1993) search for microfoundations had different motivations. Was Artus et al's position representative in France? Abroad? Furthering research on the estimation of GEMR should shed new lights on the general-equilibrium program.

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