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Stagflation and the crossroad in macroeconomics: the struggle between structural and New Classical macroeconometrics

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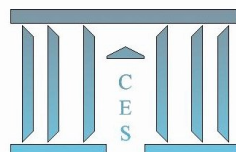
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**Stagflation and the crossroad in macroeconomics:
the struggle between structural and New Classical
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Aurélien GOUTSMEDT

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Stagflation and the crossroad in macroeconomics: the struggle between structural and New Classical macroeconometrics

Aurélien GOUTSMEDT *

October 10, 2017

Abstract

The article studies the 1978 macroeconomics conference titled “After the Phillips Curve”, where Lucas and Sargent presented their fierce attack against structural macroeconomic models, “After Keynesian Macroeconomics”. The article aims at enlarging the comprehension of changes in macroeconomics in the 1970s. It shows : 1) that Lucas and Sargent did not tackle directly the issue of the explanation of stagflation; 2) but that the struggle between different methodological stances in the conference cannot be separated from the way macroeconomists interpreted stagflation; 3) that it was not an opposition between being in favor or against microfounded models, but rather on the way we build microfoundations; 4) finally that the study of the 1978 conference opens the doors for scrutinizing the evolution of institutional macroeconomic models of the 1970s which were not totally overthrown by Lucas and Sargent’s arguments.

Keywords: History of macroeconomics, Keynesian economics, Microfoundations, Structural Macroeconometric Models.

JEL Code: B22, B41, E60.

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Introduction

The issue of how to account for and explain the changes in macroeconomics in the 1970s continue to question historians of macroeconomics as macroeconomists themselves. A debate took place on some macroeconomists' blogs in the summer 2014, on the way to interpret the “New Classical Revolution” (Thoma, 2014; Krugman, 2014; Smith, 2014; Wren-Lewis, 2014b,a). The contest between Paul Krugman and Simon Wren-Lewis dominated the debate. The latter thinks that the “New Classical revolution” was methodological above all and came from the fact that economists “were unhappy with the gulf between the methodology used in much of microeconomics, and the methodology of macroeconomics at the time” (Wren-Lewis, 2014b). Besides, “Keynesians” were quickly able to explain the stagflation with an “accelerationist Phillips curve” plus the idea that policymakers underestimated the natural rate of unemployment (*ibid.*). On the opposite side, Krugman argues that the stagflation did play a decisive role in favoring the success of the New Classical analysis. But Wren-Lewis regards the fact that the Keynesians promptly adapted their models to describe the stagflation as an indirect proof that the economic situation was not that important and so that the “fatal flaw” of the Keynesian theory in the 1970s was “theoretical rather than empirical” (*ibid.*). In a second blog post titled “Re-reading Lucas and Sargent 1979”, Wren-Lewis (2014a) focuses on the “After Keynesian Macroeconomics” paper of Robert Lucas and Thomas Sargent (1978). Regarding it as the “manifesto” of the new classical school, he claims that “it deserves to be cited as a classic, both for quality of ideas and the persuasiveness of the writing”. He argues that such a re-reading shows that the methodological part lies at the heart of the paper, and that Lucas and Sargent did not really link their criticism of “Keynesian” macroeconomic models to stagflation. Consequently, “it was this methodological critique, rather than any superior empirical ability, that led to the success of this manifesto” (Wren-Lewis, 2014a).

The issue of concern for us bears on the place of the historian of macroeconomics in the kind of debate we are confronted with here. Considering the complexity of the issue of New classical economists' success, which implies a causality dimension—this is a *why* question as we want to understand why a particular branch of macroeconomics apparently succeeded in imposing its research agenda—, this is not about declaring who is the winner. A good understanding of the reasons for some ideas to become mainstream requires a multi-dimensional analysis and, in any case, the emergence of a clear causality is a pipe dream. The historian is condemned to raise confluences, correlations and coincidences, and to suspect only the existence of a certain

kind of causality.

However, it is clear that on this issue, the historian cannot limit himself to the distanced textual exegesis, because it constitutes the shortest way to the retrospective bias, that is imposing the current views in macroeconomics on the reading of past contributions. The necessity is then to find a mean to move closer to the debates of the time. A good way to do it here is to look directly at the proceedings of the conference where Lucas and Sargent presented their fierce attack against structural macroeconometric models. The Federal Reserve Bank (FRB) of Boston organized the conference, called “After the Phillips Curve: the Persistence of High Inflation and High Unemployment”, in June 1978.¹ I will show that analyzing the context of the pronouncement of this “classic” of macroeconomics is illuminating because it enables to question the place of stagflation in the debates and to see that it could not be clearly truncated from the methodological issue. The study of the conference also shows that Lucas and Sargent’s argument was far from being a “success” at the time and that the issue of the relation between macroeconomics and microeconomic theory —and so of the dismissal of the Keynesian analysis for its “fatal flow”—was more complicated than what the “standard narrative” (Duarte, 2012; Hoover, 2012) usually claims.

The meeting represents a critical moment in the controversies between new classical economists and the supporters of large-scale structural macroeconometric models, stemming from the Cowles commission program and the Keynesian consensus.² The conference bears on the central “stylized fact” of the period: the simultaneous and persistent rise in inflation and unemployment at rather high levels, labeled as stagflation. It is obvious that the article of Lucas and Sargent was methodological above all. It was a scale attack against traditional macroeconometric models of the time (the Brookings, MPS or Wharton models) and the authors did not propose any direct explanation of stagflation.³ Benjamin Friedman, who had in charge to discuss their article, attacked the methodological point. However, at the

¹ Interestingly, Wren-Lewis talked about “Lucas and Sargent 1979”. But 1979 actually represents the year of the article reprint in the *Quarterly Review of the Federal Reserve Bank of Minneapolis*, and not the year of the original publication.

² By Keynesian consensus, I mean the adherence to theoretical principles close to the IS-LM model, a clear partition between short-term and long-term with price and wage rigidity or sluggishness in the short-term, and a belief in the efficacy of stabilization policies. I use the expression as a mere convention here, without engaging in the debate to know whether it was faithful to Keynes’ writings. I think it is consistent with the way many macroeconomists of the 1970s described the dominant consensus of the 1960s.

³ In an interview with Snowdon and Vane (1999, p.155), Lucas explained that they targeted “The Wharton model, the Michigan model, the MPS model, models which existed and were in some sense Keynesian”.

same time, it is clear that Lucas and Sargent's approach was also interpreted in a positivist way by their opponents. The explanation of stagflation was central during the meeting, and a great number of participants positioned themselves in front of Monetarist and New Classical economists. A distinct opposition appeared between an explanation relying on bad economic policies and change in agents' behavior as the fundamental causes of stagflation—the New Classical implicit stance—and an interpretation giving more weights to external factors as the oil shock—the explanation of the partisans of the Keynesian consensus and the structural macroeconometric models.

Four propositions are deduced in the article from the study of the Boston FRB conference. First, even if one could distinguish two distinct explanations to stagflation in the different contributions of the meeting, it is clear that Lucas and Sargent neither endorsed any explicit explanation, nor linked precisely and with empirical details the failure of the Keynesian consensus with the contemporaneous economic situation.

Second, it would be too shallow to consider the difference between the Keynesian consensus and the New Classical framework as merely relying on the apparition of microfoundations in macroeconomic models and on the insistence on expectations. It would be more a question of the type of microfoundations that we want to put in the models, and so of the priority of standard microeconomic theory in the building of macroeconometric models.⁴ For instance, Fair (1978) proposed a structural macroeconometric model with clear microfoundations while opposing the New Classical approach and the rational expectations.

Third, the article highlighted the existence at the end of the 1970s of a dynamic research program around the structural macroeconometric models built since one or two decades. Following Kevin Hoover (2012), I named it the “aggregation program”.⁵ Historians of macroeconomics generally insist on the place of disequilibrium theory in the 1970s as an alternative to the standard narrative in history of macroeconomics (Backhouse and Boianovski, 2013). But an important pattern of the period was the persistence of the large structural macroeconometric models, which focused a great deal of

⁴ The distinction is far from being clear-cut today. As Fair (2012) exemplified, Shimear (2009), for instance, explained that what distinguishes “modern macroeconomics” is that “models build upon two foundations”: “First, households maximize expected utility subject to a budget constraint. Second, firms maximize expected profits.” (Shimear, 2009, p.280). However, these two foundations could also be found in traditional macroeconomics of the 1970s, and in Fair's model among others.

⁵ The “aggregation program” is one of the three microfoundational programs distinguished by Hoover. I consider that the description he gave of it constitutes a good representation of the practices and beliefs of a good part of the macroeconomists attending the conference, as I will illustrate in the Section 3.

attention. For economists gravitating around this research program, it was still a progressive and promising approach.

Fourth, the conference symbolized the rupture that was growing at the time between academic macroeconomics, and the practice of building macroeconomic models for institutions and policymakers—models like, for instance, the MPS model used by the Federal Reserve in the 1970s. Macroeconomics was at a crossroad at the time and had to choose between a new approach which was appealing for its apparent theoretical consistency—the approach advocated by Lucas and Sargent—or pursuing a pragmatic but damaged approach to continue to advise policymakers in front of the stagflation. Even if the research agenda of academic macroeconomics was greatly changing at the time, structural macroeconomic models survived to New Classical economists’ attacks. Such a conclusion opens the way for further research in the history of macroeconomic models.

In the article, I give first some elements of context for the Boston conference (Section 1), before underlining what was the different stances (explicit or implicit) on stagflation causes (Section 2). I then move to the debate between the two research programs, and I illustrate it by studying how each camp interpreted the issue of expectations (Section 3). In the last section, I illustrate how the conference symbolized the separation between the practice of macroeconomics in the academic field and the practice of expertise with the help of macroeconomic models (Section 4).

1 The 1978 Conference

The conference, “After the Phillips Curve: Persistence of Inflation and High Unemployment”, was held the 1st and 2nd of June, 1978, in Edgartown, an island 150 km southward of Boston. After having exposed the general context of the conference, I will turn on its organization and its content.

1.1 The context of the 1978 meeting

The Phillips curve —the negative correlation between inflation and unemployment rates— represented the main point of contention in the academic context of the 1970s and a major issue in the macroeconomic outlook.⁶

⁶ Of course, the original statistical relationship displayed by Phillips linked wage inflation and unemployment. The works of Samuelson and Solow (1960) and Lipsey (1960), and their followers, moved the focus to prices. But the debates during the conference show that the use was not totally stabilized and the relation between both price and wage inflation was an issue of scrutiny.

Analytical debates on the Phillips curve are well documented, even if subject to many historiographical issues.⁷ But according to the standard narrative, one decade before the meeting, the works of Friedman (1968) and Phelps (1967, 1968) contributed to put into question the existence of a long-run trade-off between inflation and unemployment—a central issue during the conference.⁸ In other words, Friedman and Phelps attacked the idea (supposedly widespread in the 1960s) that policymakers could maintain permanently a low rate of unemployment in exchange of some additional points of inflation, defending the accelerationist trend for inflation of such a low rate. At the time of the conference, the idea of a natural rate of unemployment, raised by Friedman and Phelps, was already introduced in many macroeconomic models as, for instance, in the work of Robert Gordon (see chapter 3), or even as in a large-scale model like the MPS model, but it did not dismiss the existence of a short-term trade-off between inflation and unemployment, and so the defense of stabilization policies.

But the work of Friedman and Phelps had also an inspiring influence on Lucas and Sargent. The latter undermined the mere existence of a short-run trade-off (which would actually represent a statistical illusion due to monetary surprises), building their models on two fundamental assumptions: the fact that people pursue their own interest (what implied according to Lucas and Sargent the use of the rational expectations hypothesis) and that markets clear. Lucas (1972, 1973) argued that the Phillips curve was a statistical illusion, stemming from imperfect information and a signal extraction problem in a context of uncertainty : economic agents shall determine which part of their price increase is a relative price increase and which is mere inflation. In a stochastic environment, they make error because of insufficient information and they can rise their production whereas the increase in the price of the good they produce is simply due to a general price level increase. However, because of rational expectations, it is impossible to fool them systematically and, consequently, only unforecasted monetary creation (leading to inflation) would cause output to move, and no systematic monetary policy could be implemented to stabilize output. In other words, only “monetary surprises” could influence real variables. The misinformation business cycle model developed by Lucas shows that the more volatile the monetary policy is, the

⁷ See for instance Hoover (1988), Snowdon and Vane (2005) or De Vroey (2015). Forder (2014) dedicates a whole book to the history of the Phillips curve and the problems of its construction.

⁸ Forder (2014) shows that Phillips’ article never had in the 1960s the influence one gives it today. Furthermore, the belief in a stable trade-off between inflation and unemployment in the long run was not so deeply established at the time, and was rather built a posteriori in the 1970s, mainly to criticize economic policies of the 1960s.

higher the inflation rate will stay—because the volatility of money creation is integrated in the formation of expectations. Starting from the same two fundamental assumptions, Sargent and Wallace (1975, 1976) built a standard IS-LM model concluding to the inefficacy of monetary policy to stabilize the output. The proposition played the role of catalyst for the struggle between Keynesian and New Classical economists in the 1970s.

According to Gordon (1989, footnote 1), after a rapid ascent, the New Classical economics knew its “apogee” between 1976 and 1978.⁹ One of the strengths of their work relies on the timing as Lucas acknowledged (see Klammer 1984, p.56-57). Their models seemed to explain indirectly the major “stylized fact” of the period: the disappearance of the standard Phillips relationship between inflation and unemployment, as Stephen McNees documented in the conference (see Figure 1 here). The first rise in inflation appeared at the end of the 1960s, but the major increase in inflation followed closely the first oil shock of October 1973. More broadly, the perturbations encountered by the U.S. economy were numerous and diverse. McNees underlined: the implementation and relaxation of wage and price controls, or the switch from fixed to flexible exchange rates after the end of Bretton Woods, to name just a few of these significant transformations (McNees, 1978, p.45).

The standard macroeconomic models at that time (like the MPS, the Brookings or the DRI models) were challenged by such a disruption. They regularly underforecasted the inflation rate or missed the recession to come, and they naturally came under the fire of the government or the media, as of some skeptical macroeconomists, for misleading policymakers.¹⁰

⁹ Ironically, Gordon dated very precisely the moment he considered the New Classical construction has collapsed:

The high-water mark can be placed fairly precisely at 8:59 A.M. EDT on Friday, October 13, 1978, at Bald Peak, New Hampshire, just before Robert Barro and Mark Rush [Barro and Rush (1980)] began their presentation of an empirical test of the policy-ineffectiveness proposition on quarterly U.S. post-war data that was not only severely criticized by three discussants, but also contained dubious results that seemed questionable even to the authors. Never again after that occasion did any prominent proponent of the central proposition of new-classical macroeconomics even attempt to present empirical evidence in its support, and soon thereafter strong evidence against the proposition was presented by Mishkin (1982) and Gordon (1982).

Gordon (1989, footnote 3)

¹⁰ Such a judgment is widespread in macroeconomics, but to my knowledge, there are not plenty of systematic analysis of the forecast errors of the structural models and

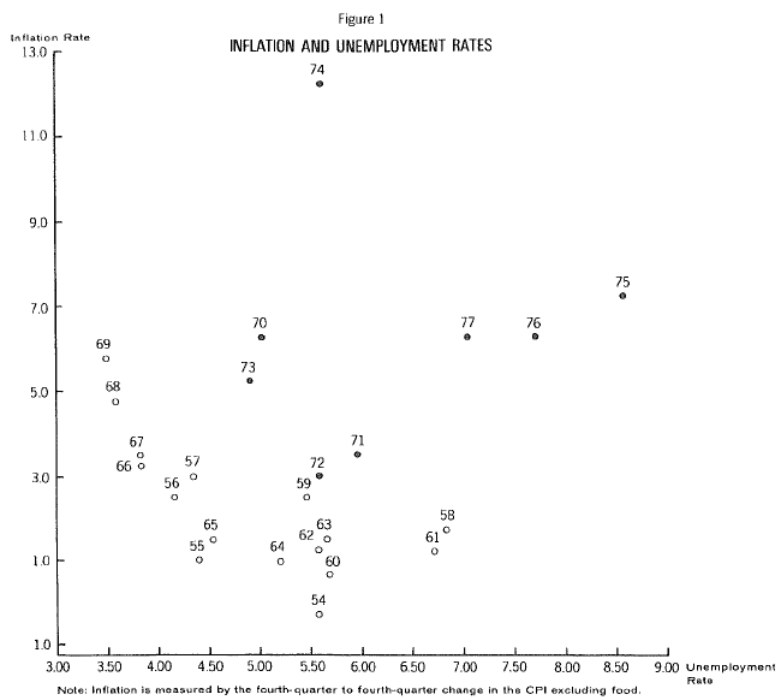


Figure 1: McNees (1978, p.30).

1.2 The organization and the audience

At that time, among the twelve federal reserve banks, the FRB of Boston, which organized the conference, represented one of the most interventionist for stabilizing the U.S. economy, as one of the most reluctant to accept disinflation policies.¹¹ Lucas himself was well aware of this state of affairs and, coming back some years later on this precise moment, he explained to Snowden and Vane what his intention with Sargent was:

We were invited to a conference sponsored by the Boston Fed. In a way it was like being in the enemy camp and we were trying to make a statement that we weren't going to be assimilated.

(Snowdon and Vane, 1998, p.128)¹²

The participants list (Figure 2) gives us some details and some intuitions on what kind of persons we could encounter in Edgartown. It appears clearly like an eclectic meeting. In addition to the professors and associate professors working in the academic sphere (like Martin Baily, Lawrence Klein, Lucas, Franco Modigliani, William Poole, Sargent or Robert Solow), we find many economists working for some federal reserve banks (like McNees) or for the Department of the Treasury, some journalists (for the *Boston Globe*, the *Wall Street Journal*, *Business Week* and the *Washington Post*), and more particular profiles: Barry Bosworth, the director of the Council on Wage and Price Stability, Napoleon B. Johnson II, the director of the Economic Development Department of the National Urban League (a civil right organization based in New-York), Barbara Becnel, an economist of the AFL-CIO (the largest federation of unions in the United States) or Nicholas Perna, a representative of General Electric, just to name a few. One can guess that the interests were many, what stimulated the richness of the conference. But no doubt that many were searching for applied issues and political implications, and it explains why the discussion around practical models used (or to be used) in institutions lied at the heart of the debates.

of the consequences for policymaking of such bad forecasts during the 1970s. In other words, it became such a commonplace that macroeconomists did not felt the necessity to scrutinize the link.

¹¹ See the record of Meltzer (2010, chapter 7) on the positions in the FOMC during the early 1970s, or Chappell et al. (2005).

¹² No doubt the contract had been fulfilled when we see how Friedman, Modigliani or Solow (and even Poole) were taken aback by the vocabulary and the tone used by the new classical economists.

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Figure 2: Conference Participants (Boston Federal Reserve Bank, 1978, p.4-5)

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Figure 3: Contents of the conference (Boston Federal Reserve Bank, 1978, p.6-7)

1.3 The conference content

Introducing the conference, Frank E. Morris, the President of the Federal Reserve Bank of Boston, exposed the concerns of the conference. He acknowledged that economic policy was now conducted in a very uncertain environment because it was “being made in at least a partial vacuum of economic theory” (Morris, 1978, p.7) and relying on econometric models that seemed not to perform as well as before. Morris claimed that:

Unlike earlier periods, no one body of theory seems to have a very broad acceptance. If Keynesianism is not bankrupt as Messrs. Lucas and Sargent suggest, it is at least in disarray. Certainly, the confidence that I felt as a member of the Kennedy Treasury in our ability to use the Keynesian system to generate outcomes for the economy which were highly predictable has been shaken, and I believe a great many other people have also lost that confidence. I look back with nostalgia on those years in the early sixties when we used, with remarkable success, small econometric models to make fairly exact estimates of what we needed to produce a given result in the economy. Now we have much more elaborate econometric models that are coming up with estimates in which we have much less confidence.

(Ibid.)

Even if “Monetarists” had challenged the “Keynesian system” before, he considered that the relevance of their propositions was already declining. The new challenge was of course the “Rational Expectations school”. They played a significant role in underlining the “market feedback” encounters after formulating a policy but the President of Boston FRB worried about the practical implication for modeling purpose, as Lucas and Sargent seemed not able to bring quickly “a complete system ready for policy-makers” (*ibid.*). So Morris hoped that the conference would be a first step in the building of a “new synthesis in economic theory” to help the conduct of policy. No doubt, the concern was clearly asserted at the beginning of the day and the conference dealt with the models that should be used to understand stagflation and to cure it.

The first part of the meeting aimed at “documenting the problem” (see the program of the conference, Figure 3). In the second presentation,¹³ McNeese tackled more directly the issue of competing schools and new theories

¹³ The first presentation by Geoffrey Moore, from the NBER, aimed at measuring the lags between variables in business cycles.

raised by Morris.¹⁴ McNees' goal was to "look at the empirical success of these "new theories". The second part of the meeting dealt more directly with these "New Explanations of the Persistence of Inflation and Unemployment". After Lucas and Sargent exhibited their destructive stance, Klein presented the LINK Model, a global multi-country model which constituted an international linkage of several national economic models.¹⁵ The fourth presentation, by Michael and Susan Wachter tackled the institutional factors, by looking at the "obligational market contracting"—that is to say "the tendency of firms and labor unions to engage in contracting" (Wachter and Wachter, 1978, p.125)—and testing its role in the link between the output gap and inflation.¹⁶ The last speaker, Ray C. Fair, Associate Professor at Yale and research fellow in the Cowles Commission, presented the model he had been building in the last several years (Fair, 1974, 1976) in the tradition of the Cowles, and so clearly elaborated in the spirit of the aggregation program. The general conclusion of the meeting was delivered by both Solow and Poole.

The first point which is worth stressing is that the Edgartown's meeting clearly exemplified the different beliefs at stake among macroeconomists on the stagflation issue.

2 Tell me how you explain stagflation...

Two types of explanation for stagflation dominated the debates in the conference. A first one insists on the role played by economic agents' adaptation when economic environment is changing. Consequently, when the government or the Central Bank implement a policy, agents react by modifying their behavior, what could lead the policy to fail if such a modification is not taking into account. In other words, the policies of the 1960s were too expansive and created a more inflationary environment. The opposite camp

¹⁴ At that time, McNees was the Assistant Vice President of the FRB of Boston.

¹⁵ The third contribution, which is of less interest for my purpose, was perhaps the less "academical" one, as the speaker, Barry Bosworth, was the director of the Council on Wage and Price Stability. He focused on the role played by institutions in the U.S. inflation, and the fact that the U.S. economy "is not that competitive anymore" (Bosworth, 1978, p.118). A major cause of inflation for him followed that it was given "groups in society more and more discretion over their ability to set wages and prices" and that "labor contracts are so structured today that they build in inflation" (*ibid.*p.119). He also underlined the role played by the government on inflationary pressures, because of its tendency to enact "legislative actions which are responsive to special interest groups" (*ibid.* p.120).

¹⁶ The Wachters were Professor of Economics and Associate Professor of Finance at the Wharton School in the University of Pennsylvania.

underlined the role played by external factors, like the end of Bretton Woods, the bad harvests and the 1973 oil shock. The two last ones constitute supply shocks that lead the price higher and can reduce output.

2.1 The new classical (implicit) explanation

We could obviously grant a point to Wren-Lewis when he stressed that the purpose of Lucas and Sargent was methodological before anything else. The issue of stagflation was clearly a minor point of interest for them, but a careful reading nevertheless gives some clues on the type of explanation favored by Lucas and Sargent. They designed their target from the very first line, assimilating, but with some vagueness, the failure of economic policies since the end of the 1960s and the bad predictions involved by the “Keynesian doctrine”:

We dwell on these halcyon days of Keynesian economics because, without conscious effort, they are difficult to recall today. In the present decade, the U.S. economy has undergone its first major depression since the 1930s, to the accompaniment of inflation rates in excess of 10 percent per annum. These events have been transmitted (by consent of the governments involved) to other advanced countries and in many cases have been amplified.

(Lucas and Sargent, 1978, p.49)

They continued by highlighting that this economic situation was following expansionary policies, defended by the Keynesian framework:

These events did not arise from a reactionary reversion to outmoded, "classical" principles of tight money and balance budgets. On the contrary, they were accompanied by massive governmental budget deficits and high rates of monetary expansion: policies which, although bearing an admitted risk of inflation, promised according to modern Keynesian doctrine rapid real growth and low rates of unemployment. That these predictions were wildly incorrect, and that the doctrine on which they were based is fundamentally flawed, are now simple matters of fact, involving no novelties in economic theory.

(Ibid.)

Further down in the article, when discussing the question of parameters stability (the point of the famous Lucas critique), they explained how Keynesian models predicted that the economic policies of the early 1970s would reduce unemployment to a very low level. As it is well known, unemployment increased, what seemed to constitute for Lucas and Sargent a proof that the relation between inflation and unemployment had changed due to the policies implemented. Nevertheless, they never clearly stated this point in the article.¹⁷ In their answer to Benjamin Friedman’s discussion, Lucas and Sargent pointed out that their empirical attack concerned actually a “specific and well-documented event”, the year 1970. Econometric models predicted a relatively low rate of inflation for a 4 percent rate of unemployment, and the authors of these models placed the result “at the center of a policy recommendation to the effect that such an expansionary policy be deliberately pursued” (Lucas and Sargent, 1978, p.82).

Nevertheless, the point was in fact more explicit in Poole’s conclusion—even if not detailed. He questioned ironically the title of the conference, claiming that we should have replaced “After” by “Because of” the Phillips curve, for the reason that “belief in a stable tradeoff between inflation and unemployment has had much to do with the persistence of excessively expansionary policies since 1965” (Poole, 1978, p.210). By this way, Poole defended what will be later called the “idea hypothesis” by Christina Romer. Considering what happened in the 1970s, this stance defends the view that “economic ideas were the key source of the Great Inflation, and indeed most of the policy failures and successes of the postwar era” (Romer, 2005, p.177). Here, the bad economic idea would be the belief in a long run trade-off between inflation and unemployment. Thus, stagflation was the result of bad economic policies, led by policymakers who thought they could permanently lower unemployment by stimulating aggregate demand, and which pushed inflation lastingly higher because of individuals’ expectations adjustment.¹⁸

¹⁷ We find many more indications in Sargent’s work, who dealt in the early 1980s with the Thatcher’s and Reagan’s policies and the good way to reduce inflation (Sargent, 2013; Goutsmedt, 2017).

¹⁸ The “idea hypothesis” seemed to be now the dominant explanation, as we could see in the conference held by the NBER in September 2008, on the “Great Inflation” of the 1970s. Several contributions explaining the situation of the 1970s by policy mistakes, and misguided belief in a stable Phillips curve (DiCecio and Nelson, 2013; Goodfriend and King, 2013; Levin and Taylor, 2013).

2.2 The external factors explanation

On the other side, other economists considered that the rise in inflation and unemployment had to be explained primarily by the role played by external factors, as for instance the end of Bretton Woods, the bad crops or the OPEC shock. For instance, Klein underlined that the major feature of the macroeconomic situation was the number of disturbances hitting the U.S. and the global economy.¹⁹ Klein's goal with the LINK project was to endogenize export volumes and import prices in order to study the international transmission mechanisms and the spillover effects of some economic policies.²⁰ One of the purpose was to understand better how inflation transmitted between countries, and so to observe the role of trade, exchange rates and national policies on stagflation in the 1970s.

However, a major part of Klein's contribution in his paper was to study the effect of an increase in basic commodity prices. He ran an indirect test of this effect by showing what would have prevailed if the embargo and the oil price shock had not taken place. Through such an analysis, he exposed "how important energy is in the pricing decision" (*ibid.* p.95) but also the impact on GDP. He explained that the results of the LINK model were consistent with the ones of Berner et al. (1974). The latter, who were members of the research team of the Federal Reserve Board, used the Federal Reserve model to show that for the period 1971-1974, 15 percent of the rise in the consumer price index (CPI) was explained by the dollar's depreciation and 25 percent by the price disturbance. According to Klein, such inflationary impulses coming from external sources generated stagflation, that is to say rising prices with rising unemployment (*ibid.* p.99). He added that, because of the place of petroleum in the process of production, an embargo can damage output, because of bottlenecks and slow production substitutions (*ibid.* p.102), and so increase unemployment.

John Helliwell, Professor of Economics at the University of British Columbia, discussed Klein's article. He proposed numerous suggestions for enabling the model to capture more precisely some decisive features like capital flows. But he claimed that these extensions wouldn't change the basic conclusion—and would actually rather strengthened it—that "the oil price

¹⁹ Klein's list is large, going from the "Nixon shocks", the end of Bretton Woods, the Soviet purchases impact, the oil embargo, to the increase both in protectionism and capital transfers, the wage offensive by unions, and also the international debt defaults and the speculation on currencies and commodities (Klein, 1978a, p.84).

²⁰ For more information on the development of the LINK model, see Hickman (1991). The project was launched in 1968 by the Committee on Economic Stability and Growth of the Social Science Research Council, which wanted to study more carefully international transmission mechanisms.

increase have been an important source of the mid-1970s stagflation in the industrial economies” (*ibid.* p.115). However, according to Helliwell, the debates still lacked of an assessment of “the relative importance of monetary and nonmonetary causes of the world inflation of the 1970s, or about the origins of the increases in the prices of oil and other major commodities” (*ibid.*). In other words, the LINK model emphasized the role of oil price increase in inflation but did not permit yet to decide between monetary policy or external factors as the fundamental impulse and major explanans of stagflation. That is to say, it did not allow to exclude the Monetarist and New Classical explanation.

In his presentation, Fair proposed the same kind of analysis than Klein, but with a different model. Fair called his theoretical model a “disequilibrium model”. It was a microfounded model with clearly identified optimization problems (consumers maximizing their utility and firms and banks maximizing their profits), but with external constraints on the process of optimization, like loan or hours worked constraints.²¹ The analysis of the 1970s with the help of the model led him to conclude that demand pressure had only a low impact on prices and wages inflation. However, a major source of inflation in the model was the price of imports—a rise of 1 percent leads to a direct increase of 0,07 point on inflation and a long term effect of 0,3 (Fair, 1978, p.182).²² He then simulated the model with a slightly lower federal fund rate for the period 1971I-1975I and concluded that “the unemployment rate by the end of the period would have been 1,9 percentage points lower than it actually was (...). Inflation, on the other hand, would have been little changed” (*ibid.* p.184).

Concluding his discussion of Fair’s paper, Franco Modigliani claimed that “with no oil problem the picture would have been a great deal different” (*ibid.* p.199). It was the major point of contention with Monetarists and New Classical economists. Even if a clear positioning from the latter on this issue is pretty scarce, Lucas did not hesitate to declare that “the direct effect of the OPEC shock was minor” in his interview with Snowdon and Vane (1999, p.152). In the paper with Sargent, there was no mention of oil shocks

²¹ Fair was one of the pioneers in developing econometric tools for disequilibrium models (Fair and Jaffee, 1972). Thus, in the 1970s, he was one of the first to mix the structural macroeconomic tradition, with the disequilibrium analysis. Renault (2016) shows that, as for Fair, one of the goals of Malinvaud was to make disequilibrium more quantitative, because he believed in the structural macroeconomic models to help policymaking.

²² Between 1969I-1972IV, the price of imports grew at an annual average rate of 6,17 percent, whereas it rose at a 34,37 percent annual average rate in the 1972I-1974IV period.

or even supply shocks. For the New Classical economists, what implicitly dominated the stagflation phenomenon was the change in structure of the economy. In other words, the change in individuals behavior changed the way economic variables used to behave. But for the other side, the failure of macroeconomic models came from the absence of important variables that were not at play before the 1970s. These economists thus proposed a plural explanation of the stagflation, with many intervening factors. Klein more recently summed up the opposition:

I believe the economy didn't change in structure; instead exogenous inputs changed a great deal within a largely unchanged structure.

(Klein in Errouaki and Nell 2013, preface)

In this opposition for explaining stagflation, the Wachters occupied an intermediary position. They considered that the “coincidental upward movement in unemployment and prices” depended upon three events: (1) the demographic changes in the population, and the corresponding increase in the “sustainable rate of unemployment” (or “nonaccelerating-inflation rate of unemployment”) that was not seen by policymakers ; (2) the food and fuels shortages that hit the U.S. economy in the early 1970s ; (3) the long lags of inflation in response to tight labor markets broke the traditional link in movements of both inflation and unemployment (Wachter and Wachter, 1978, p.124-25). But in the same time, they tried to show that the relatively high rate of inflation in the U.S. was changing the institutional patterns of contracting mechanisms, by shortening the periods of contracts. In other words, the structure of the economy was changing because firms, workers and labor unions were adapting to the new inflationary context, what reduced the lags between labor market tightness and wage inflation (and so strengthened the accelerationist tendency of the U.S. economy). Even if such an insistence on change in behavior brought them closer to Lucas and Sargent, they argued that these institutional changes are very low for being set up, and so that their analysis was compatible with rational expectations models in the long run, but contradicted them in the short run.²³

²³ They rejected short-term market-clearing and the use of rational expectations for short periods:

The bounded rationality of the economic agents weakens the sharp distinction drawn in the rational expectations literature between preannounced policy changes and policy surprises. In part, the cognitive skills of the micro parties, ignoring the skills of the policy-makers themselves,

The first observation to draw at the end of this section is that, in a conference dealing with the explanations of the supposedly disappearance of the Phillips curve, Lucas and Sargent refused to tackle directly the issue, and so refused to propose any direct and detailed analysis of stagflation. Whereas the standard narrative generally consider that the “fall” of the “Keynesian” analysis was due to its incapacity to explain correctly the events of the 1970s, it is rather obvious that their adversaries could not be supposed to fill the allegedly gap. However, it seems unreasonable, in a historical perspective, to detach the methodological stance of Lucas and Sargent from the stagflation issue. Indeed, as I will show in the next section, these different explanations are consistent with different ways to amend the current macroeconomic models—and, of course, the New Classical recommendations exceeded mere amendments.

3 ...and I will guess how you want to build your models

In a collective book on microfoundations, Hoover (2012) distinguished three types of microfoundational programs: (1) the “General Equilibrium program”, which was the Hicks-Patinkin program; (2) the new classical program, called the “representative-agent program”; (3) the “aggregation program” which was best represented by Klein. The new classical program is well known, as the article of Sargent and Lucas, and De Vroey (2015, chapter 12) offers a good summary both of their article and of B. Friedman’s discussion. Consequently, I will just remind what is needed for us here, before presenting the research agenda of the aggregation program in the conference.

make it difficult to translate short-run macro announcements into the proper course of action. This is not a minor point: the assumption that preannounced policies will lead to market-clearing behavior in the near term is likely to prove erroneous. This does not rest on the notion that the micro parties form expectations irrationally; rather it means that there are cognitive limitations for translating preannouncement policy changes into appropriate micro responses.

(*Ibid.* p.130)

In a way, B. Friedman defended the same reasoning in his discussion of Lucas and Sargent (1978, p.78), claiming that rational expectations models convoked an “asymptotic reasoning” for issues of a “shorter time frame”. In other words, the conclusion of Lucas and Sargent on economic policies, relying on agents’ change in behavior, was actually a long run conclusion (see Friedman, 1979, for a more developed version of the argument).

3.1 Lucas and Sargent's attack against structural macroeconomic models

As explained above, the purpose of the article was rather methodological. The scope was general: it came back on the Lucas critique argument (Lucas, 1976), while adding some other criticisms on standard macroeconomic practice. First, they claimed that the Keynesian revolution was methodological. Its success relied on “the development of explicit statistical descriptions of economic behavior”, and on “the introduction of the use of mathematical control theory to manage an economy” (Lucas and Sargent, 1978, p.50). In other words, the building of macroeconomic models was key to understand the domination of the Keynesian paradigm. While acknowledging the importance of the macroeconomic practice implemented by the Keynesians, they logically argued that to understand the current failure of the paradigm, one need to tackle this practice. Thus, the criticism they formulated was a methodological criticism on macroeconomic modeling.

Lucas and Sargent explained that three types of restrictions were involved in the standard practice of macroeconomics: (1) an a priori setting of which variables determine another variable among all the potentially relevant ones, (2) an a priori restrictions on the error terms and (3) an a priori categorization of which variable is endogenous and which one is exogenous (*ibid.* p.53). The true problem for Lucas and Sargent here is not the a priori stance, as it could be for Christopher Sims (1980), but rather the fact that these restrictions did not rely on the proper theory, that is the standard microeconomic theory with optimizing agents in the spirit of Arrow and Debreu's model (Arrow and Debreu, 1954). The problem of macroeconomics is the lack of “foundations in microeconomic and general equilibrium theory” (*ibid.* p.54).²⁴ Indeed, according to Lucas and Sargent, the deductions of microeconomic theory were generally in contradiction with the restrictions imposed on “Keynesian” macroeconomic models.

They illustrated their point by targeting the formulation of adaptive expectations in these models, that is to say price expectations were determined by a few lags on the price themselves. It means that economic agents use no other variables to predict the price behavior, what, according to Lucas and Sargent, contradicts the rationality of individuals. Besides, the restrictions used for equation identification are not theoretically justified by some form of microeconomic optimization.²⁵ The second type of restrictions was also

²⁴ They also talked about the “modern probabilistic microeconomic theory” and quoted later Debreu (1959) and Arrow (1964).

²⁵ Sargent (1971) rejected the modeling of price expectations as lagged rates of inflation with weights adding up to unity, because it had no foundation on the shape of inflation

inappropriate for the authors because no justification were brought for the fact that error terms imply no cross equation serial correlation. Thirdly, they argued that Sims had developed some tests to decide if a variable has to be exogenous, but such tests were rejected by Ando and Klein. Consequently, given the three arguments, there existed no reason for the behavioral parameters of the model equations to be truly structural, what seriously undermined the capacity of this model to evaluate economic policies.²⁶

So, the central question for Lucas and Sargent was to know if behavioral parameters in Keynesian macroeconomic models were stable. And they claimed their skepticism on this point invoking two reasons. The first relied on the work of Muench et al. (1974) who showed that the behavioral parameters of the FRB-MIT model were not stable for different samples. The second “test” was the macroeconomic situation of the 1970s.

The argumentation of Lucas and Sargent was cautious. They did not blame the Keynesian economists for the volatility of the fine-tuning in the 1970s, that they rather regarded as a consequence of the political competition. However, considering the monetary and fiscal policy implemented at that time, the macroeconomic models predicted, according to Lucas and Sargent, “the lowest average unemployment rates for any decade since the 1940” (Lucas and Sargent, 1978, p.56-57). As a result, “the highest unemployment since the 1930s” (*ibid.*) was a factual proof of the parameters instability, and so of their “theoretical” argument.

They concluded this set of criticism by claiming that, consequently, they considered that it was impossible to save the standard macroeconomic practice:

Some, of course, continue to believe (...) that these models can be adequately refined by changing a few structural equations, by adding or subtracting a few variables here and there, or perhaps by disaggregating various blocks of equations. We have couched our preceding criticisms in such general terms precisely to emphasize their generic character and hence the futility of pursuing minor variations within this general framework.

(*Ibid.* p.57)

time series.

²⁶ This is the Lucas Critique: if a parameter is not structural and has been estimated for a certain policy regime, then a shift in this policy regime will change the value of this parameter, which will modify the effect of the policy at stake. It is worth noting however that in the reasoning of Lucas and Sargent here, no mention was made of the rational expectations assumption. On the ambiguity of Lucas’ article, see Goutsmedt et al. (2015).

Follows a defense of their general equilibrium approach to business cycle which they considered as a way to answer to the fundamental problems raised by the 1960s consensus. It combined the postulates that markets clear and that agents pursue their own self-interest (that is “agents optimize”, *ibid.*, p.60), with the imperfect information hypothesis. The second postulate implied for Lucas and Sargent that agents form their expectation rationally, that is they use optimally the information they have. With the rational expectation hypothesis, one mathematically deduce some cross-equation restrictions which form a fourth class of restrictions to identify the econometric model (but a class which is assumed to rely on economic theory).

B. Friedman had the task to answer them. Friedman strongly disagreed with the strong divide that Lucas and Sargent introduced between the Keynesian macroeconometric approach and the general equilibrium business cycle approach. He first contradicted the distinction on the microfoundations line, claiming that optimizing agents exist in current macroeconometric model and that there was research on the microfoundations side. He took for example Fair’s model where agents explicitly maximize inter-temporally profit and utility functions. It is worth noting that Lucas and Sargent took into account Friedman’s remark by adding a footnote in revision of their paper (see p.54). They acknowledged that there exists a lot of work in the frame of “optimizing microeconomic theory” by economists “within the Keynesian tradition” (Lucas and Sargent, 1978, p.54). Nevertheless, they claimed that “it has become increasingly apparent that microeconomic theory has very damaging implications for the restrictions conventionally used to identify Keynesian macroeconometric models” (*ibid.*)—yet they brought no proof of their claim. But it manifested the strong priority granted to microeconomic theory against macroeconomics.²⁷ On the question of the arbitrary restriction, while acknowledging that it was perhaps a weakness in the models, Friedman went on to argue that he saw no clear distinction with Lucas and Sargent’s approach where such restrictions existed too (*ibid.* p.76-77).²⁸

After the conference, there was in fact a rebuttal from Lucas and Sargent, and from Friedman too, what clearly manifests the force of the opposition between the two sides. Lucas and Sargent argued that Friedman “makes no effort to explain either how [his]proposition is related to anything in [their] paper (it is not) or what possible bearing it might have on the questions of economic policy which [they] thought were under discussion” (*ibid.* p.81). For Friedman, they did not answer to the questions “do not the MPS and

²⁷ Sergi (2017) offers good examples of this priority, notably in his chapter 2.

²⁸ He gave two examples: a theoretical one, targeting the Lucas misinformation model (Lucas, 1972) and an empirical one, criticizing the work of Barro (1977) on the effect of unanticipated monetary policy.

other current “Keynesian models” include these optimizing features? Is the intended contrast against today’s models or against those of a generation ago? Why not say precisely which models are under criticism and then look carefully at their actual record of performance?” (*ibid.* p.83).

3.2 The aggregation program in the 1978 conference

What Hoover called the “aggregation program” relied on a dialogue between microeconomics and macroeconomics, and not just on an absorption of the latter by the former, as promoted in the New Classical vision. It was not a question of “logical implication”, because the “commitment to microeconomics is not merely theoretical (...) but empirical” (*ibid.* p.44).

A large part of this dialogue deals with the interaction between macroeconomic equations and microeconomic data. A good example is Klein’s defense of the use of survey data to understand how agents form their expectations in the real world, rather than assuming rational expectations (see Klein interview with Mariano, 1987, p.419-420).²⁹ The whole approach is well summed up by the following quotation reproduced in Hoover’s article:

In contrast with the parsimonious view of natural simplicity, I believe that economic life is enormously complicated and that the successful model will try to build in as much of the complicated interrelationships as possible. That is why I want to work with large econometric models and a great deal of computer power. Instead of the rule of parsimony, I prefer the following rule: the largest possible system that can be managed and that can explain the main economic magnitudes as well as the parsimonious system is the better system to develop and use.

(Klein, 1992, p.184)

It is illuminating of the top-down approach championed by Klein. As Pinzon-Fuchs (2017) explained, the aim is to represent the whole economic system in all its completeness, and so to deal with numerous phenomena. Such a goal goes through the building of separate parts of the economic system. Inside this project, the general macroeconometric model is seen as the unification of several building blocks, each one touching on a certain portion of the real economy. The natural extension of the aggregate program hinges upon the development of new building blocks and the disaggregation of existing equations. The disaggregation is the by-product of a dialogue between

²⁹ See also Goutsmedt et al. (2015) for Klein’s positioning against the rational expectations hypothesis.

microeconomic and macroeconomic theory, as well as between microeconomic and macroeconomic data.³⁰

The research agenda of the aggregation program was active in the 1970s. In the Boston conference, we see that the disaggregation and broadening goal of structural macroeconometric models constituted a common matrices for many participants. It was a way for them to correct the bad forecasts of the early 1970s and to explain stagflation by widening the microfoundations in the models, by extending them with new variables and new agents.

I have chosen two examples of such a work to illustrate the program: the development of a multi-countries model by Klein and the appeal to different groups of agents to explain the labor market.

The goal of the LINK project was to connect several national models together in order to run simulations on a multi-countries model. It enabled to study the interactions between countries in imports and exports, and to reveal some international transmission mechanisms. The model aimed at making endogenous import prices and export quantities, which were generally assumed to be exogenous in open-economy macroeconomic models. Indeed, when some models studied the link between the national economy and the rest of the world, they supposed that we were in a “small country”, that is to say with no influence on the output and the prices of the rest of the world. The LINK project got rid of this assumption by studying the interrelationships between output and prices in several countries.

It was considered of primary importance for the 1970s, in order to assess the international transmission of oil shock and its multi-dimensional consequences. For instance, the rise in oil prices displays a negative effect on general price level and output in oil-importer countries. But it also conveys a revenue transfer from these countries to the oil-exporter countries. And the gain of output in the latter could generate new imports which could offset the initial output loss in the first countries. So the LINK project enabled to check quantitatively these feedback mechanisms to appraise the net effect of the oil shock. It also permitted to test the efficiency of an expansionary fiscal policy in one country.

The LINK model offers a good example of Klein’s practice of macroeconomics. Because of the global dimension of the 1970s macroeconomic situation and the disturbances generated by the end of Bretton Woods, Klein considered that the relations between different countries should have been taken into account. Thus the LINK model constitutes a new block to add to the whole national macroeconomic model to better understand the role of exchange rates, inflation international transmission and spillover effects of

³⁰ In this perspective, new types of data shall be developed and integrated in the model.

economic policies. It dealt with enriching further the number of mechanisms that the model could account for.

John Helliwell followed himself the same approach when discussing Klein's presentation. He considered the lack of capital mobility modeling as the major weakness of the model. Adding such a feature by endogenizing capital flows and exchange rate should enable to produce a better picture of the monetary consequences of oil shocks (Klein, 1978a, p.113). Here again, the question was to find new extensions of the model, in order to enrich its ability to explain the numerous features of the real world and to reproduce by simulation the observed data.

The discussion around the presentation of the Wachters offers another example of this research agenda. They focused on the issue of price and wage dynamics, proposing and testing different aggregate equations to represent them.³¹ In the discussion, Baily defended the need to "disaggregate" the equations, in order to better understand the link between wage inflation and unemployment and to "track the data" (Wachter and Wachter, 1978, p.158). For instance, he proposed to "distinguish workers in unions or who work for large corporations from self-employed workers and employees of small companies". By separating the labor supply in different sub-groups, the aim was to track more closely the relation between inflation and unemployment and to explain its instability when we reason with an aggregate labor force.

By reading the contributions of the Keynesians during the conference, it seems that they considered the contemporaneous economic situation as an exciting moment—even if a true challenge—for attempts to improve the model, by adding new features and new detail, and by disaggregating further the behavioral equations. No doubt they believed that their research agenda should be regarded as the one of a progressive research program. And the description of the program enables to understand better where the opposition with Lucas and Sargent can be found, and so to bring a less naive story of the 1970s than the standard narrative. The issue of expectations offers a good way to understand the opposition.

3.3 The debates on expectations as representative of the struggle between the two programs

Qin underlined that the problem raised by Lucas in his Critique—and exposed again in his paper with Sargent—was far from being new. Actually,

³¹ They did not use rational expectations, and did not put forward explicit optimization problems. Yet, they advanced rational choice rationale for the contracting processes they exposed.

it was its interpretation that seemed to be changing:

It is interesting to note that the argument no longer associates time-varying parameter estimates with the omitted-variable problem. Instead, time-varying parameters are regarded as the structural representation of the changing behavior of agents as they adapt to changing economic reality, a position which bears close similarity to Lucas's (1976) critique.

(Qin, 2013, p.120)

One could conceive the variation of a certain behavioral parameter either as resulting from the omission of some factors (like expectations or supply shocks) or as being the consequence of individuals reacting to change in economic environment (like changes in economic policy).³² The second case implies larger change in the building of your model, because it requires that your behavioral parameters are actually necessarily changing and should be deduced from microeconomic decisions. If you favor the first case to explain the stagflation phenomenon, even if you acknowledge some veracity in the second case, you have some rationale for being reluctant against totally shifting your modeling strategy. The debates around the expectations issue during the conference offer a good representation of the opposition at stake, concerning modeling strategies.

What is fundamental for Lucas and Sargent is to build models on the basis that economic agents pursue their own self-interest. But they considered that the rational expectations assumption is a consequence of this fundamental principle. So one needs to build models where agents form their expectations rationally in the sense of Muth (1961) and work out the equilibrium for the model. Such a deduction gives some cross-equation restrictions and one can then estimate the model³³. For Lucas and Sargent, the point of departure is microeconomic theory in the spirit of Arrow-Debreu, and one needs to build models consistent with this framework, but in a stochastic environment. They claimed a consistent methodological stance, and the rational expectations, which are at the heart of this stance, imply a totally new way to build macroeconometric models.

According to the other camp, the issue of expectations is a question of misspecification. B. Friedman acknowledged that "the inadequate treatment

³² The omission could also come from aggregation and, as Baily advocated in the previous subsection, one can think that disaggregates the inflation-unemployment equation by looking at different types of worker could remove instability.

³³ However, the last step was not an easy task for new classical economists (see Sergi (2017), chapter 2).

of expectations constitutes a major weakness in modern macroeconomics” and that the new classical economists “had already made significant progress on this point” (Lucas and Sargent, 1978, p.79). However, it did not suffice to claim for a “fundamental methodological departure from the corpus of Keynesian macroeconomics”. In other words, Friedman, as the other supporters of current macroeconometric practice, considered that expectations formation in the macroeconometric models had to be discussed and that the role of the rational expectations assumption must be debated.

We find the same point in Solow’s conclusion. He acknowledged the “valuable and important point” (Solow, 1978, p.205) of Lucas and Sargent on expectations. He understood their point as bearing on the issue of dealing with changes in economic structure, but he minimized their point by claiming that “what often looks casually like a change in structure is really the economic system reacting to its own past” (*ibid.*). In other words, what lacked to standard models in the 1970s was the formal integration of expectations and the way these expectations depend on past change in the economy. Here again, the problem is on the misspecification and omitted-variable side, rather than on the issue of continual change in agents’ behavior.

In his discussion of Fair’s presentation, Modigliani tended also to minimize the claim of Lucas and Sargent :

I trust that in the final version of their paper Lucas and Sargent will choose to stress that their analysis of rational expectations is not to be seen as a radical break with a hopelessly mistaken past but merely as a useful, or at least logically stimulating, contribution to an area which has long been recognized as deficient and open to the criticism of “ad hockery” –namely that of modeling expectations.

(Fair, 1978, p.194)

He went further by arguing against the rational expectations for not being realistic, and promoted the idea of “nonirrational expectations”, that is to say, expectations that “[take] into account the knowledge of the time and the cost and bother of refined forecasting” (*ibid.*). It is worth noting than in his interview with Klammer (1984), he came back to the non-realistic feature of the rational expectations and imagined that we could differentiate, for instance, expectation formation on financial market and on labor market. This would lead to elaborate different types of expectations depending on the type of economic agents we deal with, what would constitute a further step in the disaggregation process. Here again, expectation formation is regarded as

a separate building block that we have to detail progressively in order to improve the descriptive power of the large-scale model.

Obviously, the partisans of the 1960s consensus were not ready to make a clean break with their models by adopting rational expectations and by building models in the way advocated by Lucas and Sargent. Their concern was much more pragmatical. They wondered how to improve the equations on the expectations formation in a more realistic direction, in order to ameliorate the performance of their models. Hence, they did not understand the radicalism of Lucas and Sargent because they considered that their research program could perfectly progress on the expectations issue and could produce models able to understand the mechanisms of the whole economy, and more particularly the stagflation situation. According to them, their paradigm was still vivid and progressive.

What is clear from the current section is that the opposition between Lucas and Sargent, and the partisans of structural macroeconomic models was not on the necessity of microfoundations for aggregate equations, but rather on the good way to build these microfoundations. The second camp preferred to adopt an eclectic and pragmatic attitude, at the risk of appearing as less consistent. In the conference, it seems that very few economists were ready to adopt Lucas and Sargent point of view, whether they are “old Keynesians” like Solow, Klein or Mordigliani, or younger economists like the Wachters, Baily, or B. Friedman. Contrary to what Lucas and Sargent thought, the bad forecasts of the macroeconomic models were not a good rationale, for many economists in the conference, to adopt the new classical framework. A part of the explanation for this refusal relies on the fact that they interpreted differently the macroeconomic situation of the 1970s. Their interpretation rather encouraged them to pursue the development of the aggregation program, by developing new extensions for explaining new phenomena and by disaggregating progressively the building blocks to offer tractable models taking account of many features of the economic system. Thus, the paradox is that the 1978 conference gives us to see a research program that is believed to be still dynamic and full of promises by its defenders, while already being going downhill in the academic sphere.

4 Purity versus utility: which road to take for macroeconomic models?

The 1978 conference symbolizes a crossroad for institutional models. Macroeconomists in institutions like Central Banks had to decide between op-

erating incremental changes on existing models, which constituted a present aid for policymakers, or totally rebuilding macroeconometric modeling, which implied a high latency. It was generally acknowledged that the first type of models had failed in the early 1970s, but perhaps not entirely for the reasons invoked by the new classical economists, and some progress seemed still possible. I think that a rupture was operating at that time between academic macroeconomics, and macroeconomics for policy-making. The existing models were condemned to be useful in the present day and to progress to help fighting the stagflation. Institutional models thus pursued their own internal path, stepping aside from the contemporaneous academic developments. In a way, the history of this kind of models (that is, the models used for policy-making) is more complicated than the conventional history of macroeconomic analysis, and it would deserve its proper story.

4.1 1978, the beginning of a divorce?

As the president of the American Economic Association, Klein had given an address some months earlier, in December 1977, called soberly “the supply side” and in which he clearly exposed a path to follow for further research in macroeconomics (Klein, 1978b). After having acknowledged the part played by the macroeconomic models developed after the World War II, he claimed:

Yet the economic problems of today seem to be intractable when studied through the medium of simplified macro models. The new system should combine the Keynesian model of final demand and income determination with the Leontief model of interindustrial flows. This is the motivation for my focusing attention on the supply side of the economy.

(Ibid. p.1)³⁴

Klein’s article is clearly a plea for the disaggregation of the supply side of macroeconomic models, in order to explain the formation of good prices for

³⁴ Some lines below, Klein stated his own line of descent in economics:

In terms of the history of economic thought, the above approach means thinking in terms of the empirical implementation of the Walrasian system. Essentially, Tinbergen implemented the Keynesian system and Leontief implemented a part of the Walrasian system. By putting the two together, with due allowance to Kuznets for making the data bases of final demand and national income available, a complete synthesis of supply and demand in the economy as a whole can be put together. (*Ibid.*)

different sectors and of input prices for different types of production factors. He advocated the modeling of an energy sector to understand the role of supply shocks. It implies the building of what Klein called “satellite” systems, that relies on “partial system analysis giving more detailed and explicit treatment on the supply side” (*ibid.* p.6). The Boston conference clearly echoed the case of Klein.³⁵

Some months later, during the “After Phillips Curve” conference, the belief in the future of structural macroeconomic models remained. Even if the problems encountered in the first years of the decade were widely acknowledged, the structural macroeconomic models still appeared as the best tools at the disposition of the macroeconomist for helping policymakers. McNees (1978) underlined that these models were more able to describe movements in wage and price than time series models, and stressed their progress in comparison with the early 1970s. Staying cautious concerning their capacity to forecast future movements of these variables and economic policy results, he nevertheless argued that the models were more and more capable to explain the events of the 1970s (*ibid.* p.44-45).

In the conclusion of the meeting, Solow appeared a bit more optimistic and he declared that the “standard” models deserved “a B and some a B minus on occasion, especially for wage equations” and so he did not “see anything in that record that suggests suicide” (Solow, 1978, p.204). Even Poole, who was one of the most skeptic and the closest from Lucas and Sargent, acknowledged some progress in macroeconomic models (Poole, 1978, p.211).

Nevertheless, it seems clear that the structural models lost their reputation at the time and were less and less occupying the top place in the academic research agenda. A simple comparison gives another intuition of that point: four months later, another conference held in Bald Peak, organized by the NBER, and titled “Rational Expectations and Economic Pol-

³⁵ In a posterior interview, Klein underlined how the seventies were a period of intense stimulation for model building:

Then every econometrician, particularly within the United States, had to pay much more attention to energy modeling. The individual models said more about the distinctive influence of energy in the economy, and the LINK model showed how high energy prices affected the international trading system.

(Klein and Mariano, 1987)

He also defended the same case concerning the end of Bretton Woods and the necessity to endogenize exchange rates.

icy”.³⁶ Except from Solow’s article (1980), every participant seemed to give some interests to New Classical models, for instance by analyzing the impact of rational expectations on different mechanisms (Blanchard, 1980, for the monetary transmission, Shiller, 1980, for interest rates, or Stanley Fischer, 1980a, for active monetary policy). Barro and Rush (1980), Kydland and Prescott (1980), and Lucas (1980) himself were also presenting a paper. The latter testified in his “professional memoir” of the positive feeling he felt in the audience towards his research:

The influence my work has had was astonishing to me. I was very nervous about my presentation, which was extremely negative on what most of this group is up to, yet people were lining up in the question period to take their turn to say how right I am...

(Lucas, 2001, p.27)

In this second conference, with a more academic audience, New Classical ideas were rather well accepted and debated, in comparison to the clear opposition observed in the June 1978 conference. The distinction between the two conferences is representative of the state of macroeconomics at the end of the 1970s, which was characterized by a progressive separation between the new academic research agenda which was becoming dominant and the priorities of model building for policymaking.

It is why Morris, the Boston FRB president, was skeptic about the “rational expectations school”:

My only problem with the rational expectations school and the Lucas-Sargent paper is that they promise us a complete system ready for policy-makers in ten years. Obviously, ten years is a rather long time to wait; particularly for me, since ten years from now I will be on the verge of retirement.

(Morris, 1978, p.8)

Even if he acknowledged plainly the deficiencies of standard macroeconomic models, he put forward a major constraint for policymakers: taking daily decision of economic policy with or without “scientific tools”. And many economists chose to improve the already existing tools. This is why I think there exists a different story to tell about the structural macroeconomic models of the 1970s.

³⁶ The conference would be later published by the NBER and edited by Fischer (1980b).

4.2 Towards another story of macroeconomics: the evolution of institutional macroeconometric models

Even if they were submitted to severe criticisms, the macroeconometric models did not merely disappear in the 1970s. An history of institutional models should be able to explain, for instance, the particular path followed by modeling strategies in the FED. Until the 1990s, the change in the two major models of the FED (the MPS for United-States, and the MCM, for the world economy) “came about in response to economic events, changes in institutional and regulatory structure, and, to a lesser extent, developments on the academic front” (Brayton et al., 1997). Thereby, the priority in the 1970s was to incorporate the effect of world trade and the flexibility of exchanges rates. Thus, the MCM model was built, under the inspiration of the LINK project. As Klein underlined in the above cited interview (Klein and Mariano, 1987), energy became a major issue for large-scale models, whereas it remained in a large part absent of academic debates.

The MPS model used adaptive expectations and introduced the Natural Rate Hypothesis as soon as 1974 (Pierce and Enzler, 1974). But it was not until the 1990s that a new model was built (the FRB/US) to introduce the rational expectations and intertemporal optimization.³⁷ The introduction was done using the “extended-path” method developed by Fair and Taylor (1983). However, all the agents in the model do not necessarily have rational expectations (or “model-consistent” expectations), and could form them with an extrapolative scheme.³⁸

It is only in the last two decades that institutional models were moving closer to academic works, but we see that they conserved some particularities. Actually, the development of institutional models kept a relative autonomy from the academic transformations of the 1970s, partially for operationability and tractability rationale.³⁹ Coming back on his experience in the Federal Reserve Board during the 1990s and in building macroeconomic models for

³⁷ An important question for this history of institutional models would be why the process to change the FED’s model was so long, whereas the motivations for it, according to Brayton, were the forecast failures of the 1970s and the spreading of the rational expectation literature .

³⁸ Fair (1994) himself distinguished the way he used the rational expectations hypothesis from the lucasian program: he did not aim at measuring what the new classical considered as the “deep structural parameters”, that is the parameters of the utility function and the production function. Besides, he tended to reject the use of models with the rational expectations hypothesis for their inability to imitate the real world (see Fair 2004).

³⁹ On the question of operationality and descriptive capacity, Fair dismissed the models inspired by the new classical program, for their inability to offer a large description of the real world:

private companies, Laurence Meyer explained that what was called the New Classical revolution created a divide between academical economists, and economists using structural large-scale model:

We always thanked Robert Lucas for giving us a virtual monopoly. Because of Lucas and others, for two decades no graduate students are trained who were capable of competing with us by building econometric models that had a hope of explaining short-run output and price dynamics.

(Meyer in Cassidy 1996)

Not so far from Meyer's point, Mankiw (2006) explained that the New Classical approach was closer to the "scientist" approach than to the "engineer" approach which aims at solving problems. In other words, the new classical approach was far from enabling some progress in applied macroeconomics. For this reason, Mankiw argued about the institutional models like the FRB/US model that:

From the standpoint of intellectual history, these models are the direct descendants of the early modeling efforts of Klein, Modigliani, and Eckstein. Research by new classicals and new Keynesians has had minimal influence on the construction of these models.

(*Ibid.* p.18)

My point here is that if you endeavor to build a history of institutional models, you would be forced to move away from the standard history of macroeconomic analysis. And the detachment from this history began at the end of the 1970s. The 1978 conference is a crucial moment, because it was at that time that macroeconomics encountered a division between the direction taken by the analytical issues and the necessities of the large-scale models for current policymaking.

I have always thought it ironic that one of the consequences of the Lucas critique was to narrow the number of endogenous variables in a model from many (say a hundred or more) to generally no more than three or four. If one is worried about coefficients in structural equations changing, it seems unlikely that getting rid of the structural detail in large-scale models is going to get one closer to deep structural parameters.

(Fair, 1994, p.16)

Concluding Remarks

Coming back to our departure, what could be said on the debate between Krugman and Wren-Lewis? The first observation that could be stressed after the study of the conference was that the methodological debate appeared clearly entangled with different ways to understand stagflation. In a way, at that time, it seems that if one preferred a kind of explanation, one was more inclined to adopt a particular methodology for building models. The particularity of the history of macroeconomics in the 1970s is that we could not (and we should not consequently) easily cut off the stagflation issue and the methodological struggle. Nevertheless, no empirical explanation of forecast failures by structural models could be found in Lucas and Sargent's work.

The second point was that the struggle between Lucas and Sargent, on the one hand, and partisans of structural macroeconomic models on the other hand, was not an opposition between the advocacy of microfounded models versus non-microfounded models. Clearly, the models defended by Klein or Fair, for instance, relied on some microfoundations. The issue was actually on the *type* of microfoundations to be chosen.

According to Lucas and Sargent, the point of departure of macroeconomic model building had to be the Walrasian General Equilibrium Theory, because it represents the most robust edifice of economics. Consequently, macroeconomic equations should be logically and consistently derived from this point of departure. It should enable to avoid ad hoc assumptions, guided uniquely by the wish to describe a certain feature or a certain part of the real world. The risk would be to choose some assumptions in order to obtain a particular result. In a way, the New Classical economists proposed a "discipline" to build macroeconomic model (to take an expression frequently used in the macroeconomic literature).

For the participants of the aggregation program, microfoundations should result from an interaction between macroeconomic and microeconomic theories, as macroeconomic and microeconomic data. If the results obtained with the model are not consistent with the data observed in the real economies, it could be the microeconomic theory which is mistaken. This stance was in a way more pragmatic. In the 1970s, there exist several macroeconomic models, and it would be foolish to throw them away, even if they had encountered bad forecasts. It was considered as necessary to push further the disaggregation of the models, to use and to build new data. The macroeconomists involved in this research program clearly rejected the clean break advocated by Lucas and Sargent.

Thirdly, it appears that the New Classical ideas were slowly implemented

and still today, the models used by major economic institutions, like the FRB/US of the Fed, are not pure DSGE (Brayton et al., 2014; Fischer, 2017).⁴⁰ In a recent opinion column, Blanchard (2017) defends the necessity to distinguish five classes of models that he labels “foundational models”, “DSGE models”, “policy models”, “toy models” and “forecasting models”. The distinction between DSGE and policy models opens new doors for history of macroeconomics. I think that a careful scrutiny of the building of these different types of models is crucial to draw a realistic and relevant picture of the evolution of macroeconomics. Practical concerns, interactions between theoretical and instrumental developments and practical considerations, and the role of institutions in model building should be at the heart of the history we want to tell. My intuition is that a good part of the models currently used in institutions like central banks or national treasuries are not only a consequence—and perhaps sometimes not at all—of the synthesis between RBC models and New Keynesian economics (Goodfriend and King, 1997), but constitute equally the direct heirs of macroeconometric models built in the 1960s and 1970s. The structural tradition seems well alive in 1978, whereas the New Classical school was very far away to bring a credible alternative for policy-makers.

This conference reveals a divorce between academic research and the daily practices of macroeconometric model building in economic institutions dealing with economic policies. Studying the works of the defenders of structural macroeconometric models and of the Keynesian consensus of the 1960s is not only a way to give voices to the “academic losers” and to better understand the academic success of the New Classical economists, it also constitutes a mean for studying a major determinant of the large macroeconometric models currently used for forecasting and implementing policies.

⁴⁰ Recently, the European Central Bank decided to abandon its DSGE model and to build a new model in the spirit of the FRB/US (Constancio, 2017).

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