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Towards a broader history of modern macroeconomics

Introduction to the special issue of the *European Economic Review* on “Histories of Macroeconomics since 1970.”

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1. Introduction

Anyone who has studied macroeconomics will be aware that the field changed dramatically in the 1970s. These changes are usually associated with the introduction of models based on rational expectations and the assumption of continuous market clearing. The use of a representative agent meant that models could be given more rigorous microfoundations than had been available in the period when Keynesian economics had dominated. The Keynesian IS-LM model, though it survived in elementary and intermediate textbooks, was displaced by models comprising a supply function often based on an expectations-augmented Phillips curve, an IS relation in which consumption was typically related to the rate of interest via a Ramsey rule, and a policy reaction function such as a Taylor rule (see McCallum 2000, 124). Keynesianism may have returned through taking account of market imperfections, but this was very different from the Keynesianism of previous generations.

This is the version of the history of macroeconomics that is typically told in macro textbooks. It means that the story of what happened to macroeconomics the 1970s can be told in terms of a very limited cast of US-based characters, including names such as Milton Friedman, Edmund Phelps, Robert Lucas, Robert Barro, Finn Kydland, Edward Prescott, Joseph Stiglitz, John Taylor and Michael Woodford.¹ The work of these economists was undoubtedly extremely influential, but it constitutes only a small fraction of literature on macroeconomics during this period. Goutsmedt and Truc (2022) have recently built a digital platform that allows to navigate the whole corpus of EconLit articles published with at least a macroeconomic JEL code since 1969. The corpus totals 85,000 references. Even the more limited JSTOR archive contains nearly 20,000 articles, chapters and books containing the word ‘macroeconomics’ for the period 1970-1999. This raises the question of what happens when we look at wider range of the literature: does it merely confirm the textbook story, or does it lead to an account that is more complex, possibly showing that a whole range of research programs, practices and contributors existed beyond these well-known figures? Before turning to that question, however, it is useful to consider how the textbook history arose.

An economist looking back at the history of the discipline typically (almost by definition) views it through the lens of contemporary economics. Because it tells us which ideas have survived, today’s economics provides guidance on which are the significant ideas in the past and hence which are the most important historical figures on which to focus. Awards such as the John Bates Clark medal awarded by the American Economic Association and the Prize in Economic Sciences in Memory of Alfred Nobel are based on fundamentally similar criteria, though they are based on more thorough investigations and advice from specialists in the subfields concerned (Cherrier and Svorencik, 2020; Offer and Soderberg, 2016). Such awards generally reflect the views of leading economists as well as helping to determine who is considered to be in that category.

An econometric analogy explains one of the dangers with such an approach. Approaching data with a specific theory in mind means the researcher will select data and the model to be tested

¹ An example of a macroeconomics textbook that containing such a history is Blanchard 2017. A much more comprehensive account, centred on a similar narrative, is De Vroey 2016.

in the light of this theory, which may rule out other possibilities. It is therefore perfectly possible that the data will confirm the researcher's theory whether or not there is another theory that is a better explanation. Such reasoning is the basis for 'general to specific' methodology. For obvious reasons, such an analogy cannot be pushed too far but it serves as a useful warning.

A good illustration of this is the history of the Phillips curve. There is a well-known story centred on the alleged introduction of expected inflation into a Phillips curve that was assumed to present a stable trade-off between inflation and unemployment. Instead of trusting the conventional account, James Forder (2014) tried to read every paper he could identify on the Phillips curve (which involves reading a lot of papers), not just the canonical ones that feature in the textbook story, but also many that he and most readers of this journal would consider bad papers, justifiably forgotten. What he found is open to different interpretations but even if one does not accept his claim that it completely overturns the conventional story, it modifies it drastically. For example, he establishes conclusively that the idea that wages depend on expected inflation was far from being a novelty when Friedman raised the issue in his Presidential Address to the AEA (Friedman, 1968). The language economists had used was often different (e.g. terms such as "cost of living adjustments" instead of "expected inflation") and the context was frequently different (a world in which collective bargaining was important). Even those most closely associated with the Keynesian Phillips curve like Paul Samuelson and Robert Solow (1960) were far from being naïve in the way the textbook story claims: their widely cited paper expressed doubts about whether a stable curve existed in the United States.

Another way to view the textbook history is to view it as the field's collective memory. Again, the metaphor cannot be pushed too far, but it is arguable that collective memories are subject to the same problems as the memories of individuals. Memories are a long way from being photographic images that record past events, albeit from a particular perspective. They are shaped by psychological processes and factors too numerous to discuss here, with the result that the reliability of any memory needs to be tested: it is even possible for completely false memories to be created (see for example Shaw, 2017). Keith Tribe (1997) and Roy Weintraub (2005) draw on research in psychology to warn historians about the types of bias and false memories likely to arise in the autobiographies and oral histories on which they draw (for instance, it is common to remember certain periods of one's life more clearly than others).

Historians clearly cannot avoid these issues completely, but they have the advantage that they are generally aware of them and have methods for trying to get round them. The traditional method is to turn to documentary evidence. In the history of economics that means reading not only publications but also unpublished sources that shed light on what lies behind published material. This includes correspondence, in which economists may express reasons, doubts and even forthright criticisms of other economists that never made it into print; unpublished drafts, revealing how the final version was reached, or projects that were abandoned; reading lists, syllabi and lecture notes that show what was actually taught; students' notes that tell us what students believed their teachers were telling them; and administrative records that show what economists were doing. In addition, modern information technology has opened up methods that were unavailable to previous generations of historians. Few researchers would have been able even to access the thousands of publications on macroeconomics let alone read them all, but modern computers make it possible to use algorithms that analyze them systematically. In the last decade, as developments in IT have opened up possibilities for large-scale data analysis not available to those doing such work in previous decades, bibliometric analysis, network analysis, prosopography, topic modeling and other quantitative techniques have been increasingly used to study the development and dissemination of ideas and practices in the history of economics, including the history of macroeconomics (Claveau and Gingras, 2016; Goutsmedt et al., 2021; Edwards et al., 2018). Finally, historians of economics resort to oral history projects to supplement the lack of archives about the development of economics in the

past 50 years.²

These various methods can of course be combined, complementing each other as is the case both within papers in this symposium as well as across them. The five papers aim at reinvestigating the decades usually associated with the new classical revolution, the rise of real business cycle models with representative agent and market clearing, and the resulting “new neoclassical synthesis” centered on DSGE models. Taken together, they rely on published sources, archives, oral histories and quantitative research to paint a picture of macroeconomics between in the 1970s to 1990s with a greater variety of geographical and epistemic diversity than is found in the conventional textbook account. Though far from exhaustive, our collective narrative emphasizes the popularity of now largely forgotten approaches models of equilibrium with rationing (often called disequilibrium macroeconomics) in Europe, the gradual spread of heterogeneous agent models across several US-based and European communities before they became as fashionable as they are now. It also highlights how macroeconomists’ understanding of unemployment in the 1960s to the 1990s was shaped by their economic and institutional contexts: this resulted in different kinds of model being developed in the US, in the UK and in continental Europe, and in new questions being asked at the European Commission on how to build multi-country macroeconomic models of the European economy.

2. Quantification

Aurelien Goutsmedt and Alexandre Truc first propose to excavate general patterns in the evolution of European versus US-based macroeconomics through the use of quantitative methods. They start with the *European Economic Review*, explaining its history and position in European economics, before using an analysis of the articles in the journal and the economists associated with it, either on the editorial board or authors of articles, to demonstrate the way it has become more international. This makes it possible to assess how far the goals of those who wanted to strengthen European economics in the early years of the EER have been achieved.

This is the prelude to what is arguably the central part of their paper, namely the identification of areas within economics that can be considered European specialties: areas in which European authors dominate relative to American. They do this by comparing the contents of the *EER* with that of the “top 5” journals, either American (*American Economic Review*, *Journal of Political Economy* and *Quarterly Journal of Economics*) or thoroughly international but now dominated by American authors (*Econometrica*, *Review of Economic Studies*). Using declared JEL codes, they pick out macroeconomics articles but then they need to find a way to classify these into topics, a purpose for which JEL codes are inadequate. They do this in two ways, bibliographic coupling (dividing articles into groups that are linked by common citations) and topic modelling (searching for combinations of words). With economists tagged by their affiliations, they can then measure the extent to which topics are dominated by Europeans or Americans (authors from the rest of the world are relatively few in number).

This approach is analogous to general-to-specific econometric methodology in that, *within the field of macroeconomics*, they start with no preconceptions about how sub-fields are to be defined. This has the obvious and important attraction that the data does the talking, substantially eliminating confirmation bias. A downside, recognized by those who use such methods, is that some of the categories produced by the algorithms may be hard to interpret. However, what matters in this case is that some of the possible sub-fields they identify can be used as the basis for further inquiries.

²The development of these methods in the history of economics are discussed extensively in Düppe and Weintraub (2018).

3. Making sense of unemployment

One of the most distinctive features of European macroeconomics was what came to be known as ‘disequilibrium macroeconomics’ which, Goutsmedt and Truc show, gave way in the 1980s to analysis of inflation and unemployment. The following two papers take up these two topics, and in doing so, modify the textbook story in different ways. In the first, Romain Plassard and Matthieu Renault provide a history of general equilibrium models with rationing, a term that, they argue, is preferable to the more commonly used term disequilibrium macroeconomics. The topic does not appear in the textbook history (see, for example, Blanchard, 2017) because, as Plassard and Renault document, these models became unfashionable in the 1980s; they were hard to reconcile with the assumption that people are rational, taking advantage of all opportunities open to them. Hence, as a line of inquiry that died out, it is not seen as relevant to modern macroeconomics. There is already a literature on such models (e.g Backhouse and Boianovsky 2013) but it has focused primarily on the early history of the topic.

The main novelties of Plassard and Renault’s paper are that it provides a more comprehensive account of the later history of general equilibrium models with rationing and of the associated empirical work than has been provided so far. The paper illustrates the importance of looking beyond the conventional canon. It is well known that disequilibrium macroeconomics was, in the 1970s, dominated by francophone economists, perhaps one reason why it was not taken more seriously by a predominantly American profession, but the sheer range of economists working on the subject has not previously been established. They also make the point that, in addition to their belief that markets must be seen as in equilibrium (with supply equal to demand), American economists, with a few notable exceptions, shunned disequilibrium macroeconomics because they did not see it as being relevant for policy. Where Americans saw inflexible prices as rendering disequilibrium theory unsuitable for the analysis of contemporary problems, Europeans saw the turmoil of stagflation as inconsistent with perfectly competitive markets that were in continuous equilibrium.

Two further aspects of Plassard and Renault’s analysis are worth mentioning here. Empirical work based on disequilibrium theory took account of the econometric problems presented by minimum conditions (e.g. realized sales are the minimum of quantities supplied and demanded), regime switching, and corner solutions. The authors review a vast range of such work, along with other attempts to test the hypothesis of non-market clearing, that goes far beyond what is discussed in the existing literature. They powerfully make the case that the analysis of models with rationing was far more significant than it is commonly taken to be. ‘Disequilibrium econometrics’ also provides evidence of the extent to which this approach to macroeconomics was rooted in empirical concerns. The second is that they relate the history of such models to the institutions that supported economic research in Europe, some of them the result of the attempts to raise the quality of European research to which Richard Portes drew attention. It is widely known that CEPREMAP, in Paris, and CORE, in Louvain, were important, but Plassard and Renault show that the range of institutions that promoted disequilibrium theory and disequilibrium econometrics was far wider. They tie the popularity of the subject in Europe, especially in Eastern Europe, to the prevalence of planning.

From the early 1980s, the main policy problem in most of Europe was the apparently rising trend in unemployment. It was difficult to explain this as the result of demand-side factors, and economists inevitably turned to the supply side. One of the ways this was approached was through providing explanations of what determined the unemployment rate consistent with a stable rate of inflation. This unemployment rate was predominantly labelled either the natural rate of unemployment or the NAIRU (Non-Accelerating Inflation Rate of Unemployment). These two terms, both of which emerged in the United States, were two labels for the same phenomenon but, as Roger Backhouse, James Forder and Christina Laskaridis show, the choice of terminology was not completely random. There was a tendency for economists to use natural rate when their focus was on the trade-off between inflation and unemployment, and to use

NAIRU when they were concerned with the level of unemployment consistent with stable inflation. A clear difference emerged in the 1980s when, in response to the European unemployment situation, European economists began to construct formal econometric models of how supply-side factors determined the rate at which inflation would be constant. The NAIRU was the name by which they chose to describe the unemployment rate determined by these models. In contrast, Americans tended to be more concerned with the trade-off and its slope, the different empirical focus of European and American economists in the 1980s being reflected in different choices about the terminology to use to describe what was, at one level, essentially the same phenomenon. Through looking at a wide literature, including many European contributions, rather than simply the papers generally believed to have been seminal, they are able to probe more deeply into the way apparently familiar terms, such as the natural rate of unemployment were understood. That phrase, as did the NAIRU acronym, meant different things at different times and in different places. In contrast, the textbook narrative treats them as a single concept with an unchanging meaning.

4. Microeconomics and macroeconomics

A key element in the textbook history, as in the writings of many critics of contemporary macroeconomics is macroeconomists' exclusive reliance on representative agent model since the 1970s. Beatrice Cherrier, Pedro Duarte and Aurélien Saïdi challenge this through tracing the history of heterogeneous-agent models. One of their main points is that heterogeneous-agent models have been developed by different communities of economists since the early 1980s, each starting from different assumptions, with the result that the field looks different depending on the perspective from which one views it. At one end of a spectrum of approaches there is Minnesota real business cycle theory, while at the other end are approaches less wedded to general equilibrium theory, spanning institutions from MIT to the UK and economists at a number of French and German universities. The important role of these different intellectual communities means that it is important to understand networks of economists dealing with these issues and, for that, published materials alone are inadequate. Since that archival evidence for such near contemporary evidence is not available, Cherrier, Duarte and Saïdi draw on extensive interviews with participants. Given the inescapable psychological biases in memories, these interviews are cross-checked with one another and with the documentary record, whether archived documents or publications.

Using such sources, the paper explores the way in which heterogeneous agents have been introduced within these different communities and the motivations for these different approaches. The Minnesota approach was driven primarily by the empirical anomalies or puzzles found with representative (homogenous) agent models; it is characterized by a commitment to a general equilibrium approach, which created substantial computational constraints. Other economists were more willing to adopt a partial equilibrium approach, and many of them had much greater interest in structural heterogeneity (in preferences, risk attitudes, skills, age, wealth, etc.) for its own sake. It is probably no coincidence that some of those focusing on borrowing constraints faced by poor households were at one time also contributors to the disequilibrium macroeconomics literature. Some of the most significant contributors, in particular in the UK, would not even describe themselves as macroeconomists, even if their work has clear implications about how to understand the behavior of aggregates, such as consumption or wealth. Some of these approaches were thoroughly empirical, and their proponents faced computational challenges, while others remained theoretical, centered on existence and uniqueness proofs. In both cases, proper aggregation strategies that accommodate heterogeneity have been a major faultline.

5. Policymaking

The textbook account focuses almost exclusively on macroeconomic theory the role of empirical work being little more than to document the basic facts that drove theoretical developments: the emergence of stagflation that showed the importance of inflation expectations, and perhaps the demonstration that the business cycle could not be explained by monetary shocks alone. However, a growing body of work has recently looked at the macroeconomic research undertaken in policymaking institutions, where the connection to policy is obviously even more direct.³ Juan Acosta, Antonella Rancan and Francesco Sergi extend our knowledge of the way macroeconomic knowledge was used by looking within the institutions of the European Community (as it was then known). The focus of their paper is the EUROLINK model developed within the European Commission's Directorate General for Economic and Financial Affairs (DG II). Given the project of European integration, in particular the goal of monetary integration, the Commission needed information on the European Community as a whole, including the relationships between the different economies, not then integrated within the single market. The EUROLINK model was a project that involved integrating, though the addition of trade equations, models of different European countries produced within their respective countries (SYSYFO, METRIC, etc). As its name suggests, the approach of integrating separate single-country models was inspired by project LINK, organized by the American, Lawrence Klein.

However, despite the pedigree of the approach, it was problematic. Integrating models from different sources created econometric and practical issues, which were particularly acute in an age when even econometric techniques and the necessary computer routines were not standardized, and it proved extremely costly in resources, time and personnel. Additionally, as Acosta, Rancan and Sergi explain, those responsible for policy did not consider that such decentralised models provided the guidance policy makers needed. The paper documents the process by which DG II abandoned EUROLINK, switching to a more centralized modeling approach through building multi-country models, such as COMPACT and QUEST. This change in direction could be seen as echoing wider international developments, in which there was increasing disillusion with large, heterogeneous models, perceived by their critics to be unmanageable (responding, for example, to the Lucas critique), and a turn to smaller, theoretically more consistent models, notably ones based on optimization and rational expectations. However, though there are clear parallels, the changes within DG II were distinct, and rooted in European institutional circumstances.

6. Challenging the conventional history

The papers brought together here make no claim to be providing a comprehensive history of macroeconomics during the last half century, even in outline.⁴ However, they do show that the commonly accepted history needs amending and that such changes have the potential to change our perspective on what was happening. We pick out a few examples.

The first point is to emphasize the role of empirical macroeconomics in relation to theory. This comes across in all the papers. Because of its generality, and the claim that theory provides the foundation on which empirical work rests, accounts of the history of economics have focused on theory. In contrast, much, if not most, economics has been empirical work, even prior to what has been called the "empirical turn" of the 2000 (Backhouse and Cherrier, 2017). These papers show that, at least in these areas, the evolution of theory cannot properly be understood independently of empirical work. It is perhaps not surprising that this is true of the theory of

³ See Acosta et al. 2023, Sergi 2020, Plassard 2021 and Rancan 2022 for histories of macroeconomic modeling at the Bank of England and the IMF, and the Federal Reserve Board.

⁴ For instance, though one of the papers tells the history of a research agenda that has since been abandoned, none of the papers tackle the history of approaches developed by self-identified heterodox communities from the 1970s onwards, such as post-Keynesians or agent-based modelers.

unemployment, but it is also true of the seemingly more theoretically driven agenda of heterogenous agent modeling. Without understanding the empirical work being undertaken and the epistemic commitments of those involved, it is impossible to understand the different approaches to problems ranging from how household heterogeneity is modeled to the way in which unemployment is understood.

A second point is that a broader approach internationalizes the story. It is completely obvious that the United States has been the dominant country in macroeconomic analysis since the Second World War. However, dominance does not mean that the field was exclusively American. Even where Europeans have applied ideas taken from the United States, these ideas have been applied in a different institutional setting, with a view to being applied to problems that are not the same as American problems. Significantly, in tackling European problems, European economists, and many Americans concerned with those problems have often modified the theory, creating distinctive approaches. Those differences may be fundamental, as with disequilibrium theory or theories of conflicts over the distribution of income, or more nuanced, as with varieties of heterogeneous agent model. Though economists from countries in other continents, notably Japan, India and Australasia, do show up in existing histories of macroeconomics, much more work is yet needed to investigate more systematically the existence of distinct regional perspectives.

A footnote to the theme of internationalization concerns the role of British economics, sometimes described as being in mid-Atlantic rather than as an integral part of European economics (see Backhouse 2000; see also Backhouse 1996). These papers suggest that, notwithstanding its very close connections to both European and American economics, British economics did retain a distinctive set of research questions, methods and agendas. British macroeconomics features prominently in the account of European approaches to unemployment. It also figures prominently in the account of heterogenous agent modeling; this shows, despite increasing internationalization, British macroeconomists retained distinctive theoretical and empirical approaches to aggregation. Another example, not covered in these papers, would be the approach to econometrics associated primarily with David Hendry. The quantitative analysis of Goutsmedt and Truc (this volume) seems to confirm both the dynamism and singularity of British macroeconomics, though it is not clear how far such an argument can be taken. Neither can we say anything about the driving forces, intellectual or institutional, that lie behind this.⁵

A third point is that it seems difficult to write the history of macroeconomics without explicitly discussing its changing and fuzzy boundaries with microeconomics. It is well known that even when macroeconomics emerged as a recognised field in economics, taught separately from microeconomics, macroeconomists never abandoned concerns with how macroeconomic models relate to models of individual behavior.⁶ This was inevitable given that it is hard to think about production, consumption, markets and prices without having actions of individuals, at least in the back of one's mind. The papers here cite many economists who would have identified themselves as microeconomists, as well as many who clearly contributed to both microeconomics and macroeconomics.⁷ It is possible to derive macroeconomic regularities

⁵ Fourcade (2006) has tried to identify different academic cultures; Tribe (2021) has explored some of the institutional factors behind the changing relative prominence of UK and US economics in the first half of the twentieth century. For an overview of economics in Western Europe, see Coats (2000).

⁶ Hoover (2012) makes the point that not only did economists in the 1950s and 1960s attach importance to microfoundations, most of them took it for granted that agents were heterogeneous.

⁷ This has always been the case. Consider the leading Keynesian macroeconomists of the 1960s: Samuelson, in 1942-3, directed a research project calculating the effect of changes in the inter-household distribution of income for postwar aggregate consumption (see Backhouse, 2017); Solow (1951) wrote his PhD thesis on the dynamics of the interpersonal distribution of incomes; and Tobin (1950) wrote an influential paper on consumers' demand.

purely from distributions across aggregates, ignoring how individuals respond to prices (as in the demand theory of Hildenbrand, 1994), but such reasoning has been used only to a limited extent in macroeconomic models.

In an age where macroeconomics is criticized for being abstract and irrelevant to the real world, complicating the story makes it harder to dismiss contemporary economics. This is not to take sides in the debate, but merely to make the point that any case for or against contemporary macroeconomics should take account of the subject as it exists and has existed, not attack a simplified version that fails to do justice to what actually takes place within the field.

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