Preface to the Chinese Edition of A History of Macroeconomics from Keynes to Lucas and Beyond

M. De Vroey

Discussion Paper 2019-6

Institut de Recherches Économiques et Sociales
de l'Université catholique de Louvain

Preface to the Chinese Edition of *A History of Macroeconomics from Keynes to Lucas and Beyond*

Michel De Vroey [*]
Université catholique de Louvain
January 2019

Abstract

In this preface, I reflect on the evolution of macroeconomics in the wake of the 2018 recession. I give a sketch of the developments that have taken place and assess the validity of some of the main criticisms that have been addressed to DSGE macroeconomics

Keywords: History of macroeconomics, 2008 recession, DSGE macroeconomics, RBC model, New Keynesian model

JEL code: B22, E12, E 13, E30

---

[*] michel.devroey@uclouvain.be I thank Pierrick Clerc for his remarks on a previous version of this Preface.

I am much honored to have my book on the history of macroeconomics translated into Chinese. I hope that its Chinese edition will have the same success as its English one. As stated in the Preface to the English text, my aim in writing this book was to provide graduate students and young researchers a complement to their technical education. I am aware that macroeconomics is a thriving field in China. If the need for the 'big picture' is present in Western countries, it may be surmised that this is also true for China.

Two main threads span my analysis. First, I was inspired by Leijonhufvud's view that the development of economic theory can be regarded as a decision tree, the branches of which originate from choices made about basic methodological nodes. Taking one bifurcation rather than another puts theory on different tracks, with possible far-reaching consequences. To begin with, there are elementary or basic choice nodes to be addressed. Next, once a given branch has grown, choices become more specific as second-, third-level, etc., nodes enter the picture. Even thriving research lines face hurdles. As a result, changes in bifurcations, the emergence of new nodes, and the abandonment of old ones are usual features in the unfolding of theory. It may also happen that, at some point, a successful research track loses its momentum: conundrums are brought to light, objections are leveled, and doubts about the validity of the route taken set in. Such an occurrence may result in what Leijonhufvud calls 'backtracking' – that is, returning to a previous node and taking a previously neglected bifurcation. Using such a grid of analysis allows us to regard the history of macroeconomics as a succession of turns: a turn from the 'Economics of Keynes' to Keynesian macroeconomics; a turn from the latter to the dynamic stochastic general equilibrium (DSGE) program, the first stage of which, initiated by Lucas and Sargent, was new classical macroeconomics; a turn from the latter to RBC macroeconomics; and, finally, a turn from the latter to DSGE *modeling* (to date, the last stage of the DSGE *program*). The term 'scientific revolution' has been overused, yet I am of the view that, if there is one episode in the history of macroeconomics which deserves to be called thusly, it is the Lucasian transformation, which led to the dethroning of Keynesian macroeconomics. The other turns consisted of endogenous transformations rather than of a radical breach.

The second guiding thread of my book is the Marshall-Walras divide. I have long thought that the three distinct branches which co-exist within neoclassical economics – the Marshallian, the Walrasian, and the Austrian ones – are alternatives rather than complements. For the purpose of studying macroeconomics, only the Marshallian and Walrasian variants matter. A central claim of my book is that the Marshall-Walras divide provides a powerful tool for shedding light on the unfolding of macroeconomics. Keynes's *General Theory* and Keynesian macroeconomics were fully Marshallian, their specific aim being to generalize Marshallian partial equilibrium analysis. As for Lucas, he regarded the transformation of the field he stewarded as a move from Marshall to Walras without, however, fully breaking away from Marshall. RBC modeling is a different blend. It studies identical autarkical agents. As trade is absent, the RBC baseline model is neither Marshallian nor Walrasian. As far as DSGE modeling is concerned, it combines a Marshallian trade technology with Walrasian principles.

Although published in 2016, the story told in my book stops at the eve of the 2008 recession. The reason is that, in Hegel's words, the owl of Minerva spreads its wings only with the falling of the dusk. In plain language, historians must wait until the dust has settled to start their investigations. Ten years later, things have become clearer. It is thus worth the while to make an assessment of the developments that have taken place over the last decade. This is the task I have set for myself in the following pages.

Much fuss has been made about the fact that DSGE modeling was unable to foresee the 2008 recession. But this is hardly a devastating critique because great recessions always come as a surprise. More important is the fact that before the recession, for good and bad reasons, DSGE economists paid scant attention to financial intermediation, which inevitably meant that they were unable to come to grips with a recession like the one that occurred. The Smets-Wouters model, the emblematic DSGE model, assumed that financial intermediation worked in a frictionless way. To compound the matter, when later financial frictions were introduced into this model, this model proved unable to account for their protracted character. Such shortcomings are definitely serious. Those economists who from the onset were skeptical about the DSGE program found their earlier judgment about it confirmed. By contrast, DSGE macroeconomics regarded it as a challenge they needed to address. The result was a burst of papers on finance and banking exploring the incidence of agency and asymmetric information problems on the workings of the economy.

Groundbreaking developments also occurred in other topics. Here, I will limit myself to evoking three of them. First, the study of monetary policy went through a significant renewal, the result of the need to address the effects of a zero lower bound on nominal interest rates. As standards tools were no longer operational, unconventional policies based on quantitative easing and forward guidance needed to be devised. Such policies had already been studied before the recession by Eggertsson and Woodford (2003). Gertler and Karadi (2011) revisited the issue in the context of the Great Recession. Heterogeneity is a second topic which underwent significant changes. Krusell and Smith (1998) had blazed the trail in the 1990s. Early works on heterogeneity turned out to generate responses to total factor productivity shocks that were hardly different from those generated by the representative agent model. New developments allowed going beyond this result. HANK (heterogeneous agent new Keynesian) models are one of them. In a series of papers, Kaplan, Violante, and co-authors reintroduced the forgotten Keynesian notion of marginal propensity to consume – a nice example of Leijonhufvud's backtracking idea. In these models, heterogeneity takes the form of differences in wealth and portfolio holding in an incomplete market context. In such a surrounding, aggregate demand shocks have a more significant output impact than in the representative agent model. Now, at last, heterogeneity makes a difference. Models such as the HANK model also testify to a reversal in the focus of research. In the first decade of this century the trend in DSGE modeling was to try to improve the fit of models by piling up new shocks even if their microfoundations credentials were thin. By contrast, with these models and in view of the tremendous increase in the size of databases, a return to the study of microeconomic behavior and the transmission mechanism has taken place. A third challenge that had to be faced was the "missing deflation puzzle." Despite the huge increase in unemployment that occurred in the US over the 2009-2012 period, no disinflation occurred. As shown by Hall (2011) and Ball and Mazumder (2011), the standard DSGE model based on the new Keynesian Phillips curve has been unable to replicate the observed weak decline in inflation. Del Negro, Giannoni, and Schorfheide's (2015) and Christiano, Eichenbaum, and Trabandt's (2015) papers tackle this issue. The former shows that an increase in nominal price rigidity can explain the anomaly, the second that a new Keynesian model with credible wage bargaining can account for a weak drop in inflation if neutral technology shocks and a risky working capital effect are assumed.

A preface is not the place to present a more detailed account of these developments. The interested reader can turn to the "Macroeconomics a Decade after the Great Recession" Symposium, published in the 2018 Summer issue of the *Journal of Economic Perspectives.* It includes papers on developments in financial intermediation by M. Gertler and S. Gilchrist, on the household demand channel by A. Mian and A. Sufi, on identification by E. Nikamura and J. Steinsson, papers providing a general picture of post-recession developments by (a) J. Gali, (b) L. Christiano, M. Eichenbaum, and M. Trabant, and (c) P. Kehoe, V. Midrigan, and E. Pastorini, and, finally, an article on the HANK model by G. Kaplan and G. Violante.

All these articles evince high spirits, a feeling that the DSGE program is thriving, as if the challenges raised by its lack of prognosis of the 2008 recession had resulted in a forward surge. It suffices to look at the REPEC list of publications under the heading *New Economic Papers on Dynamic General Equilibrium* or at the content of the *Review of Economic Dynamics* to observe the breadth of issues that can be tackled using the DSGE conceptual and empirical apparatus. The following two quotations from Kehoe *et al*.'s paper in the *Journal of Economic Perspectives* set the tone:

> The Great Recession has had essentially no impact on macroeconomic methodology per se (Kehoe *et al*. 2018: 151).
>
> In this sense, there is no crisis in macroeconomics, no massive failure in methodology, no need for undisciplined frictions and shocks. Overall, modern macroeconomists live under a big tent that welcomes creative ideas laid out in a coherent language, specified at the level of primitives, and disciplined by external validation. Kehoe *et al*. 2018: 164).

This is the view held among business fluctuations scholars. But outside this community, the picture is different. In the media – even in a magazine like *The Economist* or a newspaper like *The Financial Times* – macroeconomics seems to have lost whatever luster it had before. With respect to the economic profession at large, present-day macroeconomics seems to stand on the sideline. With the rise of different types of experimental research lines, the economics profession has indeed drifted away from the high-brow theoretical style embraced by DSGE macroeconomics. The latter has also been the target of fierce attacks from eminent members of the profession like J. Stiglitz, P. Romer, and P. Krugman, all three Nobel Prize laureates. Let me briefly assess these attacks.

Stiglitz's general appraisal of DSGE macroeconomics in his paper, "Where Modern Macroeconomics Went Wrong," reprinted in the *Oxford Review of Economic Policy,* is as severe as can be.

> I believe that most of the core constituents of the DSGE model are flawed—sufficiently badly flawed that they do not provide even a good starting point for constructing a good macroeconomic model (2018: 76).

His list of flawed ingredients runs as follows: a) theory of consumption; b) theory of investment; c) representative agent; d) distribution matters; 5) aggregation; 6) source of shocks; and 7) adjustment to shocks. Prominent among these is the representative assumption. Its adoption impedes any consideration of what Stiglitz regards as one of the most important breakthroughs in economics (to which he was a main contributor), information economics. Obviously, asymmetry of information cannot enter the picture when agents are assumed to be identical. Stiglitz is right in arguing that no cogent explanation of financial failures can be made without integrating the informational dimension. But, there is a snag. While it is, to some extent, valid for the pre-recession state of DSGE macroeconomics, it is no longer so for its post-recession state. In the financial intermediation literature, agency problems between borrowers and lenders, vulnerability in balances sheets, and bank runs are now front and center. The same is true for heterogeneity, as touched on above. By voicing his critique in 2018, Stiglitz exposed himself to being debunked for a lack of knowledge of what is going on at the cutting-edge of the discipline – a rebuttal that Gali and Christiano et al. did not stop themselves from making.

Romer's 2016 paper "The Trouble with Macroeconomics" is another fierce attack on modern macroeconomics. It comprises a long list of indictments. Let me just focus on the two most important ones. The first is that the 'truth-value' of modern macroeconomics is nil. Hanging over this judgment is my old friend, the Marshall-Walras divide. Surely, searching for the truth fits the Marshallian approach more than the Walrasian one. The latter addresses reality only indirectly through fictitious model economies. RBC economists, who proudly proclaim their adoption of Walrasian methodological principles, nonetheless took a step toward Marshall by claiming that their models must be gauged with respect to their ability to replicate real-world time series. Still 'replication' is a far cry from 'explaining' in the strong sense of the term. Hence their truth-value is indeed weak. The bottom line is thus that Romer wants macroeconomics to be based on Marshallian rather than Walrasian principles. DSGE macroeconomics displays the opposite preference (yet less so in its last installment, new Keynesian DSGE models). Yet, expressing methodological preference for one research line rather than another cannot be regarded as a critique of it. Romer's second indictment is that DSGE models attribute fluctuations in aggregate variables to forces that are not caused by the actions of economic agents. His target is thus exogenous shocks, the impulse/propagation mechanism, which is indeed a cornerstone of the DSGE program. What is needed, he claims, is an endogenous explanation of business fluctuations. That is, business fluctuation studies must experience a move similar to that which occurred in growth theory (thanks to Romer. I find this point cogent. Unfortunately, declaring that a good theory of business fluctuations must have them generated endogenously is easier said than done. Romer gives no hint as to how to proceed. Hence DSGE economists could retort that as long as the ins and outs of such a radically new bifurcation have not been set out and implementation of the program is missing, it makes sense to pursue the existing line as its momentum remains strong.

As far as Krugman is concerned, in my book I critically discussed his 2009 *New York Times Magazine* article, "How did economists get it so wrong?" – a title that aptly summarizes its author's contention. In his contribution to the *Oxford Review of Economic Policy* entitled "Good Enough for Government Work? Macroeconomics Since the Crisis" (2018), Krugman pursues a complementary strategy. Having contended (unconvincingly in my eyes) that DSGE macroeconomics has it all wrong, the task he sets for himself in this new paper is to show that Keynesian macroeconomics has it almost all right. Its robustness, Krugman claims, has been verified on three scores. First, the liquidity trap version of the IS-LM model put forward by Hicks has proven correct: in a zero lower bound context, large budget deficits have not driven interest rates higher. Second, events have confirmed the Keynesian contention that, during recessions, increases in the monetary basis have little price impact. Third, Keynesian theory predicts that changes in government spending and taxation generate a multiplier effect greater than one, and this is what occurred. Moreover, the post-recession years have testified to the potency of activist policy in dealing with short-run fluctuations. Hence Krugman's conclusion that the 'old guard' can be comforted. I agree with him that the Keynesian theoretical apparatus, combined with Keynesian econometric models, is "good enough" for policymaking in recession phases. But this conclusion cannot be regarded as a critique of DSGE macro. What it rather does is support the view that macroeconomics must be pluralistic in method, a standpoint defended by Blanchard (2018) and Wren-Lewis (2018).

Will these critiques exert an impact on the development of macroeconomics? To answer this question a distinction must be drawn between external and internal criticisms. External criticism follows from differences in methodological preferences. It amounts to criticizing an approach because it is based on premises different from those endorsed by the person voicing the critique. For example, Marxian economists dismiss neoclassical economics because it sweeps under the rug the existence of social classes holding opposite interests. Symmetrically, neoclassical economists will disparage Marxian economics because it disregards that economic agents are choice-makers. Internal criticism zeroes in on the logical or empirical shortcomings of an approach without questioning its premises. Böhm-Bawerk's critique of the transformation of values into prices in Marxian theory or Edgeworth's and Bertrand's criticism of Walras's theory of *tâtonnement* are examples of the past. Friedman's expectations-augmented Phillips Curve or Lucas's critique are more recent ones related to macroeconomics. The hallmark of internal criticisms is that they hurt. They compel a response, either debunking the criticism or taking stock of it and modifying the theoretical construct accordingly.

Against the background of this distinction, Stiglitz's is an internal critique, but it is largely anachronic. The other two are external. Romer criticizes DSGE macro for regarding business cycles as caused by exogenous shocks. Krugman criticizes it because he believes that Keynesian macroeconomics is more useful for policymaking. This leads me to think that, although the three papers will be music to the ears of all those who dislike DSGE macroeconomics, in no way will they have an impact on the practice of DSGE economists.

In an above quote, Kehoe et al. depict DSGE macroeconomics as a big tent, which suggests that everybody is welcome. This is false. The standards for 'good' theoretical practice as enunciated by Lucas and soon adopted by the profession act as a barrier to entry. They are as follows: general equilibrium analysis, the stability postulate, explicit microfoundations, the intertemporal equilibrium concept, rational expectations, the impulse/propagation approach, stochastic shocks, and empirical verifications. Taking these bifurcations is regarded as *a sine qua non* for good theoretical practice. Notice that perfect competition, perfect information, the representative agent assumption, monetary aspects, and any specific trade technology assumptions do not belong to this set of standards. The strength of the DSGE program then lies in its subtle combination of rigidity and flexibility. The above standards are constitutive of its identity. For the rest, internal criticism is the rule of the game, the fuel of the development of the program. The big tent analogy must be understood as meaning 'internal criticism is welcome.' No assumption is sacred as long as the standards are respected. Some are indeed outrageous – e.g. the representative agent assumption. Their justification is tractability and the view that theory construction must proceed step by step – even if removing outrageous assumptions can take decades. All this is underpinned by the fact that general equilibrium analysis faces constraints that are absent from partial equilibrium analysis.

I am not claiming that DSGE macroeconomics is here forever. It will fall when (a) its standards have become an obstacle to further progress and (b) prospects for devising alternative bifurcations have seen the light of day. The relinquishment of the exogenous shock bifurcation and its replacement with endogenous shocks is a possible radical backtracking. If so, people might then return to Romer and hail him as their forebear. I also surmise that any such move will be engineered by people from within the paradigm – people who, after having been educated and worked in it, have become dissatisfied with it, and have found a way to blaze a new trail. Paradigms crumble from the inside rather than the outside. That said, I have the feeling that DSGE macroeconomics is still far from having run out of steam.

REFERENCES

Blanchard, O. 2018. "On the Future of Macroeconomic Models." *Oxford Review of Economic Policy*. **34**: 43–54.

Ball L, and Mazumder S. "Inflation Dynamics and the Great Recession." *NBER Working Papers*. National Bureau of Economic Research. No. 17044.

Christiano L, Eichenbaum M, Trabandt M. 2015. "Understanding the Great Recession." *American Economic Journal: Macroeconomics.* **7**: 110-167.

Christiano, L. M. Eichenbaum and M. Trabant. 2018."On DSGE Models." *Journal of Economic Perspectives.* **32**: 113–140.

Del Negro M, Giannoni M, Schorfheide F. 2015. "Inflation in the Great Recession and New Keynesian Models." *American Economic Journal: Macroeconomics.* **7**: 168-196.

Eggertsson, G. and M. Woodford. 2003. "Optimal Monetary Policy in a Liquidity Trap." *NBER Working Papers*. National Bureau of Economic Research. No. 9968.

Gali, J. 2018. "The State of New Keynesian Economics: A Partial Assessment." *Journal of Economic Perspectives* **32**: 87–112.

Gertler, M and P. Karadi. 2011. "A Model of Unconventional Monetary Policy." **58**: 17-34.

Gertler, M and S. Gilchrist. 2018. "What Happened: Financial Factors in the Great Recession. " *Journal of Economic Perspective.* **32**: 3–30.

Hall, R. 2011. "The Long Slump." *The American Economic Review.* **101**:431-469.

Kaplan G and G. Violante 2018. "Microeconomic Heterogeneity and Macroeconomic Shocks." *Journal of Economic Perspectives*. **32**: 167–194.

Kehoe, P. Midrigan, V. and E. Pastorini. 2018. "Evolution of Modern Business Cycle Models: Accounting for the Great Recession." *Journal of Economic Perspectives*. **32**: 141–166.

Krugman, P. 2018. "Good Enough for Government Work? Macroeconomics Since the Crisis." (*Oxford Review of Economic Policy*. **34**: 156–168.

Krusell, P. and A. Smith. 1998. "Income and Wealth Heterogeneity in the Macoreconomy." *Journal of Political Economy*. 105: 867-896.

Mian, A and A. Sufi. 2018. "Finance and Business Cycles: The Credit-Driven Household Demand Change." *Journal of Economic Perspectives.* **32**: 31–58.

Nikamura, E and J. Steinsson. 2018. "Identification in Macroeconomics." *Journal of Economic Perspectives*. **32**: 59–86.

Romer, P. 2016 "The Trouble with Macroeconomics." *The American Economist*. https://ccl.yale.edu/sites/default/files/files/The%20Trouble%20with%20Macroeconomics.pdf.

Stiglitz, J. 2018. "Where Modern Macroeconomics Went Wrong." *Oxford Review of Economic Policy*. **34**: 156–168.

Wren-Lewis, S. 2018. "Ending the Microfoundations Hegemony." *Oxford Review of Economic Policy*. **34**: 55-69

Institut de Recherches Économiques et Sociales
Université catholique de Louvain

Place Montesquieu, 3
1348 Louvain-la-Neuve, Belgique

ISSN 1379-244X D/2019/3082/6