SHOULD THE HISTORY OF MACROECONOMICS STEER CEAR OF THE FRAY OR BE PARTISAN? A CRITICAL ESSAY ON BANKS AND FINANCE IN MODERN MACROECONOMICS BY B. INGRAO AND C. SARDONI

Michel De Vroey

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Should the History of Macroeconomics Steer Cear of the Fray or be Partisan? A Critical Essay on *Banks and Finance in Modern Macroeconomics* by B. Ingrao and C. Sardoni

Michel De Vroey [1]

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Abstract

My review of Ingrao and Sardoni's book paper focuses on its Part II, entitled "From the Neoclassical Synthesis to New Keynesian Economics." My criticisms amount to three. First, I disagree with Ingrao and Sardoni's account of the twists and turns that have occurred in modern macroeconomics. Often, where they see continuity, I see cleavage; where they see cleavage, I see continuity. Second, I put forward that the result of the 2008 recession is that DSGE economists were led to zero in on the hitherto neglected issue of the workings of the financial sector and its integration in their models. Hence, Ingrao and Sardoni's conclusion of failure must be revised. Third, I want to bring out that the internal history of economics can be written in two ways: the approach can be partisan or steer clear of the fray. As I am in favor of the latter, I regret that Ingrao and Sardoni have adopted the former.

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I am glad to have been invited to participate in this symposium on Bruna Ingrao and Claudio Sardoni's book, *Banks and Finance in Modern Macroeconomics*, because I hold both of them in high esteem. The aim they pursue in this book is ambitious, assessing the interplay of the financial and the real sides of the economy from Wicksell and Fisher to present-day macroeconomics. Their overall conclusion is one of frustration. This interplay, they claim, has been almost completely ignored or mishandled. The underlying reason is a wrong methodological turn, the drift of macroeconomics toward mathematical modeling, especially of the neo-Walrasian type, at the expense of adopting a broader perspective that focuses on processes and institutions. To overcome this impasse, Ingrao and Sardoni call for a radically alternative approach borrowing from Karl Marx, Joseph A. Schumpeter, and Fridriech Hayek (Ingrao and Sardoni 2018: 246), as well as from post-Keynesian macroeconomics.

Due to the tardy delivery of my paper, I have been able to read Robert Dimand's and Goulven Rubin's contributions to this symposium. Their detailed account of the content of the book allows me to go straight to my assessment, which is twofold. On the one hand, I find that Ingrao and Sardoni's analysis of the 1930-1980 period is first-rate scholarship. On the other, their analysis of the 1980s to the present day seems feebler to me. In this review, as unfair as it may be, I will focus on the second aspect. The underlying motivation is that we learn more from our dissension than from our accord.

My critiques of Part II of the book, "From the Neoclassical Synthesis to New Keynesian Economics," amount to three. First, I disagree with Ingrao and Sardoni's account of the twists and turns that have occurred in modern macroeconomics. Often, where they see continuity, I see cleavage; where they see cleavage, I see continuity. Second, I put forward that the result of the 2008 recession is that DSGE economists were led to zero in on the hitherto neglected issue of the workings of the financial sector and its integration in their models. Hence, Ingrao and Sardoni's conclusion of failure must be revised. Third, I want to bring out that the internal history of economics can be written in two ways: the approach can be partisan or steer clear of the fray. As I am in favor of the latter, I regret that Ingrao and Sardoni have adopted the former.

Too vague a depiction of the history of macroeconomics

We expect books like Ingrao and Sardoni's to be based on a well-delineated account of the history of macroeconomics from its inception to the present. This is not the case. Their narrative is imprecise and incorrect in several respects. It runs as follows.

(a) It is unfortunate that the rich message of John Maynard Keynes's book came to be reduced to the IS-LM model, which they regard as the first step in a wider process of 'Walrasization' of macroeconomics.

(b) They depict Milton Friedman's work as marking a radical breach from Keynesian macroeconomics. They associate the loss of dominance of Keynesian macroeconomics with the success of the 'monetarist counter-revolution,' Harry Johnson's expression.
(c) Monetarism was succeeded by 'new classical macroeconomics' (NCM), of which Robert Lucas was the steward. They implicitly tread James Tobin's footsteps when declaring that NCM is 'monetarism mark II,' which suggests a relation of continuity.
(d) The real business cycle (RBC) model, triggered by Fynn Kydland and Edward Prescott's 1982 article, is the next episode. This is an evolution that they strongly deprecate.
(e) They characterize the late 1980s as a period of conflict between NCM, RBC, and the emerging New Keynesian Economics models.
(f) This conflict ended in the mid-1990s with the rise of the 'new neoclassical synthesis.'
(g) The last decade was witness to some progress in the integration of finance in macroeconomics. However, in their view, the current state of affairs remains basically wanting.

My discontent with such an account lies in its missing the most important transformation that took place in modern macroeconomics, the dethroning of Keynesian macroeconomics, centered on the IS-LM model, and its replacement by the DSGE (dynamic stochastic general equilibrium) *program* initiated by Lucas.[2] An aspect of this transformation relevant for this paper is a remark made by Thomas Sargent stating that the Lucasian revolution was 'impartial in the rough treatment it handed out to participants on both sides of the monetarist-Keynesian controversies" (Sargent 1996: 5). Once the revolution was over, it turned out that the erstwhile foes were reasoning within the same methodological framework. In short, the radical transition that took place is between (a)-(b) jointly and (c)…(f) jointly, rather than between (a) and (b)…(f).

Lucas's 1975 model based on monetary shocks and imperfect information leading to a signal-extracting problem started the DSGE program. However, it was short-lived. Having managed to construct an equilibrium model of the business cycle was considered a feat. The next step, however, was unclear. What to do with his model? How could it be transformed into a springboard for a new research program? It makes nonetheless sense to regard Lucas as the founding figure of the DSGE program, because he succeeded in spelling out new methodological standards for the practice of macroeconomics and proved able to convince the majority of the profession to adopt them. The technical impetus for the DSGE program is to be credited to Kydland and Prescott, whose 1982 article inaugurated RBC modeling, alongside John Long and Charles Plosser's 1983 article.

[2] For a time, it was called 'new classical macroeconomics,' but the usage of this name was short-lived. The DSGE label is better from a substantive viewpoint. It also fits aptly what Lucas had in mind. Yet, there is a semantic trick. It came into usage later as a designation for the third stage of the program! To solve this semantic ambiguity, I make a distinction between the DSGE *program* and DSGE *models*.

My requalification of the history of macroeconomics differs from Ingrao and Sardoni's account in its methodological grounded Let me illustrate through Axel Leijonhufvud's decision-tree device. It is based on the view that the development of economic theory can be compared to a decision tree, the branches of which originate in choices made about basic methodological nodes. Progressively, nodes become more specific as second-, third-level, etc. nodes need to be solved. Choosing one initial bifurcation or fork in the road rather than another puts theoretical constructs on different tracks with far-reaching consequences.

Table 1 below uses the decision-tree device to compare the sets of basic methodological choices underpinning Keynesian macroeconomics, Friedman's expectations-augmented Phillips curve model (Friedman 1968), and Lucas's equilibrium model of the business cycle (Lucas 1975).[3]

Table 1 indicates that the Keynesian and the monetarist approaches see eye to eye for most benchmarks – despite their policy antagonism – while they differ from the DSGE program on most of them. The 'monetarist counter-revolution' terminology turns out to be a bluff. The table also shows that Ingrao and Sardoni's association of the IS-LM model and the Walrasian approach lacks foundation.[4] Finally, it displays Lucas's Walrasian filiation, but also its limits (benchmark 4). The single yet crucial thing that Lucas wanted to retain from Keynesian macroeconomics was its applied character, the requirement of an empirical validation of theoretical models – that is, adhering to Friedman's positivistic standpoint. The consistency of the predictions of the model with the data is the overarching criterion for assessing their validity. All DSGE economists have followed suit. Finally, Table 1 is also useful to understand the motivation of Lucas's willingness to transform the standards for a 'good' macroeconomics practice. In the late 1960s, jointly with Leonard Rapping, he came to the view that Keynesian macroeconomics was deeply flawed – its basic concepts being badly defined (largely due to the absence of explicit microfoundations), the state of rest equilibrium was unfit for dynamic analysis and econometrics models were more data- than theory-constrained. These were times when neoclassical theory was on the defensive. Yet, Lucas and a few others held the strong conviction that the future of macroeconomics lay in making it more rather than less neoclassical. More specifically, he wanted it to be grounded in the methodological standards governing microeconomics (Lucas, 1987: 107). This disposition translates into the quasi-general shift in the methodological benchmarks displayed in Table 1.

[3] By Keynesian macroeconomics, I mean the IS-LM tradition as initiated by Hicks and Modigliani (De Vroey 2000). The two exceptions to the Marshallian affiliation of first-generation Keynesian economists are Oskar Lange and Don Patinkin. On this, see Rubin (2002, 2004, and 2016).

[4] For a criticism of this association, see De Vroey (2016: 350).

Table 1. Basic methodological forks[5]

		Keynesian macro	Friedman	Lucas
1. Aim	– explaining involuntary unemployment or underemployment – providing a non-Keynesian explanation of money non-neutrality – constructing an equilibrium model of business fluctuations	✓	✓	✓
2. Model/theory relation	– theory ≠ model (**M**) – theory = model **(W)**	✓	✓	✓
3. Eclecticism/ monism	– as many models as issues (**M**) – single baseline model (**W**)	✓	✓	✓
4. Pure theory/theory & measurement	– pure theory (**W)** – theory & measurement (**M**)	✓	✓	✓
5. Equilibrium concept	– state of rest (**M)** – intertemporal equilibrium (**W**)	✓	✓	✓
6. Adjustment	– sluggish (**M**) – instantaneous (**W**)	✓	✓	✓
7. Microfoundations	– implicit (**M**) – explicit (**W)**	✓	✓	✓
8. Expectations	– adaptive (**M**) – rational (**W**)	✓	✓	✓

Unlike Lucas's, model, Kydland and Prescott's was not a one-shot contribution. In a timespan of half a decade, it came to be transformed into the baseline RBC model (King, Plosser, and Rebelo, 1988). The latter is on Lucas's side for all the benchmarks in Table 1. Yet, RBC modeling departs from Lucas's model on second-level methodological bifurcations, which are absent from Table 1. First, in Lucas's model, agents are heterogeneous. By contrast, in RBC models all are identical self-producers. Second, the RBC model is a Ramsey planning model rather than a Walrasian model. Third, in RBC modeling, shocks are real. Fourth, perfect information is assumed in Kydland and Prescott's model. Fifth, the validation of the model occurs though calibration rather than econometric testing. Seven, there is the change that leaves

[5] **M** refers to A. Marshall and **W** to L. Walras. The following nodes require a description. *Node 2*: Marshall regarded a theory and a model as separate entities. In this line, models are subservient to theory, serving the purpose of checking the logical validity of theoretical propositions. Theoretical propositions are deemed to pertain to the real world. To Walras, a theory and a model were the same thing, necessarily a mathematical model. In this view, theoretical propositions pertain to the fictitious model economies created by economists rather than to the real world. *Node 4*: both Marshall's *Principles* and Walras's *Elements* are pure works of theory. However, Marshall was inclined to engage in the search of empirical evidence. Walras, for his part, held the view that pure theory requires no confirmation from reality. *Node 5*: Marshall adopted the state of rest equilibrium concept (the rocking chair analogy). Equilibrium and disequilibrium are then organically linked. "The ordinary economic situation is one of disequilibrium moving in the direction of equilibrium rather than of realized equilibrium" (Viner 1 [1932] 1953: 206). Here, **W** must be understood as neo-Walrasian theory. Equilibrium is defined as generalized individual equilibrium *paths* implying intertemporal substitution. Equilibrium is regarded as characterizing the way in which neoclassical economists decide to look at reality rather than a characteristic of reality. Put differently, the existence of equilibrium and market clearing relate only to the model economy and their existence is postulated. *Node 7*: while Marshall was keen to derive firms' input demand functions and output supply functions from maximizing behavior, he did not bother to do so for households. As far as the latter were concerned, he found it acceptable to skip the formal derivation of aggregate demand and supply functions from their individual decisional process. This is the 'implicit microfoundations' bifurcation. For his part, Walras strongly believed that all aggregate functions needed to be explicitly grounded in individual agents' optimizing decision-making ('explicit microfoundations'). For a more detailed account of all the nodes, see De Vroey (2018).

Ingrao and Sardoni flabbergasted: the disappearance of money.[6] A seventh and final difference is that Lucas's 1972 and 1975 papers were purely theoretical contributions, unlike Kydland and Prescott's. Their overarching aim was to address an empirical issue: business fluctuations in the US over the 1950-75 period through the examination of a few second-moment time series (output, consumption versus output, investment versus output, hours worked versus output, and correlation between hours worked and output per capita). The success of their undertaking, they claimed, was to be gauged on whether the model-generated time series fitted the actual time series statistics.[7]

The initial reception of Kydland and Prescott's 1982 article was lukewarm not only because it was difficult to understand but also – and mainly – because its claim that business fluctuations in the US from 1950 to 1975 could be studied using a Robinson Crusoe-like model seemed preposterous. Yet, their model had one important factor going for it. Though resting on just one shock and six parameters, it fitted the data on the chosen second moments better and more parsimoniously than existing models containing dozens of equations and many more free parameters. A small group of economists, centered around the Economics Department of the University of Minneapolis, decided to pursue Kydland and Prescott's approach, and it did not take long for the majority of the macroeconomics profession to make the leap.

Before continuing with commenting on the passage from the RBC to the DSGE model phase, let me bring out another insufficiency of Ingrao and Sardoni's narrative. It concerns the 'New Keynesian' category. By regarding it as generic, they miss the point that a distinction must be made between first- and second-generation new Keynesian economists. The first comprised such eminent economists as Gregory Mankiw, Joseph Stiglitz, or George Akerlof. They accepted Lucas's 'equilibrium discipline,' yet fought hard to salvage the involuntary unemployment and non-market-clearing notions. More generally, they were strongly opposed to the RBC framework. By contrast, second-generation new Keynesians, such as Jordi Gali or Mark Gertler, had no such qualms. Realizing the powerfulness of the RBC approach, they decided to adopt it as their workhorse, while trying to gear it towards different policy conclusions. Thus, the methodological conflict, which Ingrao and Sardoni refer to in their

[6] To them, this 'erasing' (their expression) is a conundrum. Not for Lucas, however: "Though I did not see it at the time, the Bald Peak conference [organized by the Federal Reserve Bank of Boston in 1978, where K&P presented a first version of their paper] also marked the beginning of the end for my attempts to account for the business cycle in terms of monetary shocks. … Later on, as they gained more experience through numerical simulations of their Bad Peak model, Kydland and Prescott found that the monetary shocks were just not pulling their weight: by removing all monetary aspects of the theory they obtained a far simpler and more comprehensible structure that fit post-war U.S. time series data just as well as the original version" (Lucas 2001: 28).

[7] In a later paper in which Kydland and Prescott returned to their project, they summarized the question addressed as: how volatile would the US postwar economy have been if technology shocks had been the only contributor to business-cycle fluctuations? Their answer runs as follows: "We found that the model economy displays business cycle fluctuations 70 percent as large did the US economy. The number is our answer to the posed question" (Kydland and Prescott 1996: 74).

narrative, was between the first generation of new Keynesians and RBC economists. Yet, there was none between second-generation new Keynesian and RBC economists.

The transition from RBC to DSGE modeling occurred in a discrete way. It cannot be equated with the end of RBC modeling. Rather, it was an offshoot of it.[8] Its gist consisted of introducing Marshallian elements into the RBC baseline model. The main transformations amounted to four. The first is the replacement of the initial perfect competition with monopolistic competition. It was motivated by the desire to bring the stickiness theme back to the forefront, initially as affecting price formation and, in a further step, as also bearing on wage formation. In this respect, Calvo pricing, in spite of its dubious microfoundations credentials, came to be omnipresent. The second change is the return of money (be it in a cashless way) and the return of the monetary policy theme to center stage under the impulsion of John Taylor (1993), and Julio Rotemberg and Michael Woodford (1997). Whereas earlier on the functioning of central banks had been a black box, now they received a micro-founded objective function. They were also assigned a more active role in price stabilization policy due to the need to counterweight the effects of sticky prices. These changes came along with a replacement of the quantity of money by the nominal interest rate as central banks' policy instrument. The third change is the introduction of other shocks than TFP shocks – monetary policy shocks, shocks in government consumption, price- and wage-formation shocks, and habit persistence and price indexation shocks. Finally, tremendous changes occurred on the measurement front. While calibration was still used (in the context of a huge increase in databases), structural VAR analysis and Bayesian econometrics became front and center.

Post-recession DSGE macroeconomics

> For a large part of the 20th century, spanning from the late 1930s to the 1980s, mainstream macroeconomics put banks and the financial system to backstage, or even expelled them completely from its theoretical representations of the economy (Ingrao and Sardoni 2019:1).
>
> Over the years, and especially after the 2007-2008 financial crisis, there has been a growing number of writings concerned with credit and financial markets (Ingrao and Sardoni 2019: 10).

These two quotations are drawn from the introductory chapter of Ingrao and Sardoni's book. Does the second quotation mean that the flaws denounced in the first ended up being removed? Many passages in the book seem to point to the contrary.[9] Chapter 9 of Ingrao and Sardoni's book is entitled "Credit and Finance in Today's Mainstream." Its title suggests a systematic examination of recent theoretical developments. Unfortunately, this is hardly the case. The new way of envisaging monetary policy, the modeling strategy initiated in Lawrence

[8] Kimball and Gali characterize it as follows: "a model following the RBC paradigm as closely as possible except for adding what is logically necessary in order to graft in sticky prices" (Kimball 1995: 1241). "[Models having a core structure that corresponds to an RBC model on which a number of elements characteristic of Keynesian models are superimposed" (Gali, 2008: 2).

[9] For example, they refer to "the *final* erasing of money from business cycle theory associated with the rise of RBC models" (Ingrao and Sardoni 2018: 181; my emphasis).

Christiano, Martin Eichenbaum, and Charles Evans (2005), and further developed by Frank Smets and Raf Wouters (2003) as well as the post-2008 recession developments are absent from the chapter or merely alluded to.

In particular, the post-recession evolution of DSGE macroeconomics is crucial for Ingrao and Sardoni's query. At the very time when the new modeling strategy triggered by the two papers mentioned above was close to stabilization, its limitations came to light. Though these models comprised a financial sector, it was assumed to work in a frictionless way. Hence, it is small wonder that they were unable to come to grips with a recession like that of 2008. Those economists who from the onset were skeptical about the DSGE program found their earlier judgment confirmed. By contrast, DSGE macroeconomists regarded it as a challenge. 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.[10] Although Ingrao and Sardoni do not discuss these developments, they acknowledge their impact:

> Nowadays it is hard to find mainstream macroeconomic models that no not incorporate, in one way or another, some form of financial intermediation. Credit rationing, the accelerating effects of the financial sector on the economy's fluctuations, some attention to the complexity of the financial sector characterize most of the current t macroeconomics literature which mainly deals with such topics by using DSGE models, generally regarded as the most adequate available analytical tool (Ingrao and Sardoni 2019: 243).

The conclusion I draw from these new developments in mainstream macroeconomics is as follows. Whereas for a long time financial intermediation was absent from the DSGE program, a breakthrough occurred in the last decade. It was permitted, in a first step, by the transformation of RBC modeling into DSGE modeling, and in a second, by the post-2008 recession transformations of DSGE modeling. Thereby, RBC modeling, which initially could have been regarded as a dead end, ended up being justified since it served as a launching pad for DSGE modeling.

For their part, Ingrao and Sardoni are far from adhering to such a conclusion.

> These results, however, should not be contemplated with complacency. Important crucial issues are still open and in need for further reflection and developments to arrive at a more satisfactory theoretical and analytical capacity to deal with finance in market economies. We argue in Chapter 19 that the main difficulties encountered by the current mainstream essentially derive from it still strong connection to the previously dominant paradigm and the excessively simplistic approach to issues like the very notion of imperfections and the use of the exogenous shocks to explain phenomena that require deeper and richer analysis (Ingrao and Sardoni 2019: 243).

In the next section, I ponder upon what lies behind our conflicting assessments.

An 'out-of-the fray' versus a 'partisan' internal history of economics

When evoking the differences in practicing the history of economics, the main one that comes to mind is that between the internal and external history of economics. The former focuses on textual content and the rational reconstruction of theoretical reasoning. It implies an inner knowledge of the theoretical nucleus under study. The latter is mainly concerned with the

[10] For example, in the balance-sheet channel literature, Christiano, Motto, and Rostagno (2014); in the banking-lending channel one, Gertler, and Karadi (2011); and in the heterogeneity one, Boissay, Collard, and Smets (2016).

historical, biographical, and sociological aspects of the activity of economists. This line, which makes the history of economics part of the broader history of sciences discipline, requires less in-depth theoretical knowledge. Although a tendency exists within each of the two groups to dismiss each other's works, the two specializations are not antagonistic. Be that as it may, this difference is not my concern here. What I wish to bring out is different: the existence of a division within the internalist stream of the history of economics bearing on the attitudes taken by historians of economics about their object of study – the partisan and the steering-clear-of-the-fray attitudes.

Partisan historians of economics claim a twofold identity; they consider themselves as economists *and* historians of economics. Although they usually produce no economic theory, they have a strong conviction about what 'good' economics is – in my terminology, what the methodological forks in the road that must be taken or not are. Often, partisan historians of economics are also on the side of heterodoxy – that is, opposed to neoclassical economics, which they derogatively label 'the mainstream.' Their main objection to it lies in its supposed ideological allegiance to neoliberalism and its methodological inadequacy; its premises are deemed ill-suited. As a result, whatever is built on them is judged to be flawed. Ingrao and Sardoni belong to this community. As an illustration, the following passage describes the way that must be taken in their eyes to appositely address the issue of integrating finance into macroeconomics.

> To accomplish such a task, we argue, requires to go beyond the analytical and methodological structures that still characterize the current mainstream. More specifically we need to reject the pretension that an exclusive recourse to models, as sophisticated as they may be, can provide a fully satisfactory understanding of banks and finance in their complex interrelation with the real economy. It is necessary to mobilize a wide range of available cognitive instruments, including historical knowledge, the critical exploration of economic ideas and their evolution over time, the consideration of the complexity of human behavior as well as the complexity of paths and trajectories that depend on crucial events, institutions and the social and political context. It is the lesson we draw from the great scholars whose ideas we examine in the book. 'Giants' of the past like Wicksell, Fisher, Schumpeter, Robertson, Keynes, Hicks and Tobin developed their theories, carried out their analyses and formulated their policy prescriptions in the context of wide cultural horizons, where historical explanations, the knowledge and understanding of the evolution of economic concepts played a crucial role together with the elaboration of new views and interpretations (Ingrao and Sardoni 2019: 19-20).

Their standpoint is thus one of external criticism. I have two observations to express about it. The first is that Ingrao and Sardoni may well praise the 'Giants' they evoke, but they also reckon that they failed to deliver on the matter of the integration of finance. By contrast, the theoretical strategy they dismiss on the grounds of its unpalatable basic methodological choices has achieved progress on this matter. There must be reasons for this discrepancy – it might be that, when it comes to cumulative development, simplistic formal modeling has the edge over wide-encompassing programs of the type hailed by Ingrao and Sardoni in the previous quotation. My second observation relates to the nature of external critiques. They amount to examining theories through the prism of one's preferences. To caricature, a Marxian economist may disparage neoclassical economics on the grounds that it is not Marxian, while a neoclassical

economist would level the inverse criticism at Marxian theory. The chances that such critiques will exert an impact are slim.

My viewpoint on the history of economics is different. I like to compare out-of-the-fray historians of economics to art historians. The latter do not create art (even though they may have tried to be artists at first). They comment on what artists do, classify them in different schools, compare the latter, and trace genealogies and mutations. The same can be said about the history of economics as practiced by its out-of-the-fray subgroup. The contributions of these historians of economics are meta-theoretical. Their role is not to set the agenda. When I study the history of mainstream modern macroeconomics, my standpoint is that I must not question its agenda (constructing equilibrium models of business fluctuations), the basic methodological choices underpinning it (as described in Table 1), and the criterion on which mainstream macroeconomists want their work to be judged (fitting the data). Once these objects discarded, a substantial set of tasks still remains to be tackled: (a) examining the internal consistency of models; (b) identifying second-level methodological bifurcations and bringing out puzzles and disputes that have arisen over the development of the approach under study; (c) assessing the cumulative development that has occurred over time; (d) checking whether the criteria for good theory that were decreed at the onset of the program have been respected; (e) checking whether mainstream macroeconomists are aware of the limits of their approach and whether their meta-theoretical discourse is congruent with their theoretical practice; (f) assessing the adequacy of their reactions to criticism; and (g) anticipating further developments or theoretical dilemmas. All in all, this amounts to engaging in critical work – the history of economics is a *via negativa* – yet the criticism in point is internal rather than external.

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ÉCONOMIQUES ET SOCIALES

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