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ABSTRACT
Half a century has passed since Robert Lucas got his paper on expectations and the neutrality of money published in the *Journal of Economic Theory*. That article is widely considered as pathbreaking, starting a movement that changed the professional standards of doing macroeconomics. It arguably also affected the perception, if not the conduct of monetary policy and other stabilisation policies. This roundtable discussion collects comments from a panel of experts in a combination of prominent macroeconomists who have gathered ample experience of work in central banks and other authorities, and historians of economic thought.

KEYWORDS
Robert E. Lucas; methodology; neutrality of money; monetary policy

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1. Introduction

Trautwein: Welcome to the roundtable session on monetary non-neutrality and stabilisation policy 50 years after Lucas's expectations paper! It is a great pleasure to have this roundtable, even if only in a hybrid format today, video-linking the conference site at the UNWE in Sofia with Paris, London (Ontario), the Boston area and elsewhere.

In 2022 half a century will have passed since Robert Lucas got his paper on expectations and the neutrality of money published in the *Journal of Economic Theory* (Lucas 1972a). That article is widely considered as pathbreaking, starting a movement that changed the professional standards of doing macroeconomics. It arguably also affected the perception, if not the conduct of monetary policy and other stabilisation policies. This conference provides a good opportunity to get the views from a panel of experts in a great combination of prominent macroeconomists who have gathered ample

CONTACT Hans-Michael Trautwein michael.trautwein@uol.de FK II – VWL, Carl von Ossietzky Universität Oldenburg, Oldenburg 26111, Germany; Olivier J. Blanchard ojblanchard@gmail.com Department of Economics, MIT, Morris and Sophie Chang Building, 50 Memorial Drive, E52-506, Cambridge, MA 02139, USA; Beatrice Cherrier beatrice.cherrier@gmail.com Bureau 2012 CREST, ENSAE, 5 avenue Henry Le Chatelier, 91764 Palaiseau cedex, France; Pierrick Clerc pierrick.clerc@uliege.be HEC Liège, UER d'Économie, Liège, Belgium; David Laidler laidler@uwo.ca Department of Economics, University of Western Ontario, London, Canada; Athanasios Orphanides athanasios.orphanides@mit.edu MIT Sloan School of Management, Cambridge, MA, USA
The roundtable was organized by Pierrick Clerc, Muriel Dal Pont Legrand and Hans-Michael Trautwein. It took place during the Annual ESHET conference at the University of National and World Economy (UNWE) in Sofia, 8 October 2021.

experience of work in central banks and other authorities, and historians of economic thought.

Let me briefly introduce the panellists in alphabetical order. *Olivier Blanchard* is the first, and he hardly needs to be introduced since he is well known as a leading macroeconomist in multiple functions, as researcher, supervisor, textbook author and policy adviser, if not more. For many years he was chief economist at the International Monetary Fund. Olivier is considered to be one of the most prominent critics of Lucasian economics, and he himself is most likely on the way to becoming an object of studies in the history of our discipline. So he is heartily invited to consider the roundtable as a chance to boost his future standing in the history of macroeconomics.

The next panellist is *Beatrice Cherrier*. Her blog name is "The Undercover Historian." She is the queen of tweets on the history of economics, letting the community closely follow her research on Twitter, sharing her findings in archives and other contexts, and thereby greatly contributing to the common good. She has worked extensively on the development of dynamic theory and economics since the Second World War, including reactions to the Lucas 1972 paper in terms of sunspot modelling.

The third panellist is *David Laidler*, the professed Monetarist whose concern with Lucas is that he put the profession on a track on which it is not taking money as seriously as it should. David is strongly qualified for this roundtable, since he has many overlapping skills of theoretical and empirical work, of policy advice (for a long time at the Bank of Canada) and of work on the history of economic thought, with a focus on the evolution of the quantity theory of money.

The fourth panellist is *Athanasios Orphanides*, who sports the impressive job title of "Professor of the Practice, Global Economics and Management" according to his website at the MIT (Massachusetts Institute of Technology; https://mitsloan.mit.edu/faculty/directory/athanasios-orphanides). Athanasios, too, is a wanderer between the worlds of academia and central banking. Moreover, he is also a wanderer within the world of central banking as he has moved back and forth between the two most important central banks, namely the Fed and the ECB. He has served as governor of the Central Bank of Cyprus, helping to introduce the euro there in 2008, and he was a member of the Governing Council of the European Central Bank. So he is a true insider in the field under discussion at this roundtable.

The fifth person then is *Pierrick Clerc*, yet another wanderer between the worlds of academia and central banking with a background in the Banque de France and the Swiss National Bank. He is now at the University of Liège in Belgium. Together with Muriel DalPont Legrand, Pierrick took the initiative to organising this roundtable and he will chair the discussion, ask the pertinent questions and open up towards the end for questions from the audience.

Clerc: Before starting, I would like to thank all four panellists for giving us some of their precious time. We will consider two sets of questions regarding the contribution of Lucas article. The first set will essentially deal with the methodological aspects of this article and, more specifically, with the importance of those aspects for modern macroeconomics. The second set of questions will focus on the substantive issues addressed by Lucas paper. We will notably discuss Lucas's explanation of the non-neutrality of money, the posterity of that explanation and, more generally, the role played by imperfect information and signal extraction problems in the dynamics of macroeconomic variables. We will also discuss the influence of Lucas's paper on the practice of monetary policy, past and present, and in particular on its counter-cyclical dimension.

2. What was the contribution?

Clerc: Let us start with the basic methodological aspects and with a very basic question. What is, according to you, Beatrice, the most important contribution of the paper to modern macroeconomics?

Cherrier: The way you handle all these questions in history of thought is, for instance, to do some bibliometrics. So I did a bit of that. Alexandre Andrada has a whole paper on the bibliometrics of Lucas's papers, comparing the citation patterns of the Lucas 1972 paper, the Lucas critique and others (Andrada 2017). It is super difficult to reach a conclusion, first because of technical issues, but also because you are comparing papers like the 1972 one which remains cited as a paper – with the Lucas critique which becomes an object of its own and even becomes a weapon. Lucas has joked that at some point it becomes a holy cross that you can put in at a seminar.

Andrada's paper shows how the reception of this paper differs depending on whether you are looking at Google Scholar, the Web of Science or another database. One interesting conclusion, though, is the following: If you look only at the citations of the stream of paper of Lucas's papers from the seventies – between 1970 and, say, 1982, the first decade or 15 years – the most cited paper is neither the Lucas critique nor the neutrality of money paper. It is the asset pricing paper that was published by the end of the decade, in 1978. In spite of that, this wasn't the most influential one, which means that perhaps the early reception of Lucas 1972 was slow-moving.

Clerc: Many thanks, Beatrice, for this comparison. David, what is the most important contribution of Lucas's 1972 paper to modern macro?

Laidler: Remember, I was there in 1972. So my comments are not history; they are memoirs. Around 1970, macroeconomics was in ferment – a mess. The microfoundations debate – Clower, Leijonhufvud, et cetera – was going on. So was the monetarist controversy, both within academic macroeconomics, but also in policy circles because inflation was bubbling up: the monetary demand pull versus cost-push debate was going on. Also, people by then knew that inflation expectations were important, but modelling them was a problem. Most people used error learning because that is all that was there, but a small-scale industry was trying to find better ways of doing it. The crucial point is that the literature was fragmented. Nobody understood how all these issues (and more) hung together.

Then along came Lucas. In 19 pages he told you that you didn't need non-Walrasian microfoundations for macroeconomics. Walrasian microfoundations were OK. He then told you that you should model expectations as the prediction of the economic system, or the model of the economic system, that you are actually talking about. And, finally, he also came down, (apparently – things were actually more complicated as we shall see), very hard on the side of Milton Friedman in the monetarist controversy. Instead of consisting of hundreds of articles coming from here, there and everywhere, contemporary macroeconomic thought seemed to have found order in 19 pages.

Now Lucas's accomplishment was not without precedent. Let me read you a little passage that you are all going to recognise: "*A monetary economy is essentially one in which changing views about the future are capable of influencing the quantity of unemployment and not merely its direction.*" That's the *General Theory* (Keynes 1936, xxii). Though his view of how this worked was very different, Lucas nevertheless brought macroeconomics back to the same theme as Keynes, and like Keynes too, he brought a new orderliness to the sub-discipline in so doing. But instead of 410 pages, he did it in 19. That was an extraordinary achievement, and he really did change the trajectory of the sub-discipline forever after.

Blanchard: Let me start with some personal history. This is the history I know best, and I shall leave serious history to Beatrice. I was a student of Advanced Macro in 1974 in a course taught by Stan Fischer, and we spent half a semester on Barro/Grossman in a preliminary version (Barro and Grossman 1976), and the other half semester on the [Lucas] 1972 paper. So I have a very vivid memory of the effect. I think what he did in that paper was to define the rules of what a macro model had to be: It had to be dynamic; it had to have general equilibrium; it had to have optimising agents; then it had to have expectations, in that case rational expectations, but that was not essential. Basically, he defined the rules and people started playing by those rules, and that was an immense challenge.

If you go back to where we were – David talked about content, but now let me talk about methodology – the widely accepted notion was that macro was different from micro because of aggregation, because of complex behaviour, because of all that. You were inspired by theory, but you surely didn't feel like you actually had to derive things from first principles. And I was always struck by my revered colleague Paul Samuelson, who, when he did micro, did it absolutely rigorously. And when he did macro he just wrote equations down which sounded right, and that was thought to be the only way to go. It is not crazy – and I think part of what has happened since is the realisation that it is very difficult to follow the Lucas agenda and go back to reality because of the complexity of aggregation behaviour and so on. But there is no question that from then on, if you didn't have all these ingredients, you were considered outside the set of people who one could talk to. So I think that that was really the fundamental contribution of the article.

Orphanides: I came after Olivier in my courses, and by then Olivier and Stan Fisher actually had managed to digest everything and teach it to us. But I went directly to central banking, and I want to give you the central banking implications of the methodological centrality of the Lucas paper in 1972, and also link it to the years before. As David mentioned earlier on, this was an environment in which there was quite a bit of confusion about how to understand the macroeconomy. I will actually place the starting point of the 1972 paper at the 1970 conference that was hosted at the Federal Reserve on the econometrics of price determination. Because from the policy side, what you want to do is understand the economics of price determination, the economics of business cycles in order to see how to best use policy tools to stabilise the economy and enhance welfare. And there we were in an environment where the modelling up to the late sixties, for the most part, did not take expectations seriously. They knew that expectations were important. But in the macro modelling used for policy analysis, they did not try to be rigorous about how expectations were formed. And in 1970, at that conference, Robert Lucas pointed out that all of this work that was being done inside central banks and policy institutions was actually questionable because you could not draw policy conclusions from the modelling that was taking place. We have the Lucas critique in 1976 (Lucas 1976), but it was already there in the 1970 paper (Lucas 1972b).

The importance of the 1972 paper, in my view, was in providing an example of how to start doing the analysis in a more complete way more rigorously, as Olivier pointed out. Which is what was necessary to start trying to incorporate the effect of expectations on policy and to build better policy going forward. I have to tell you that we have not completed that agenda, as Olivier pointed out. It is very, very difficult to do. I don't think we have good ways of incorporating expectations and dynamics of the economy yet. But Lucas set the rules and created an example of how we should go about it in that very simple paper from which many strands of the literature have continued.

Clerc: Thank you, Athanasios, you allow me to make a very nice transition because I would like to know if you see Lucas's 1972 paper as more or less important than Lucas's 1976 paper and the associated Lucas critique.

Orphanides: I look at these papers together and actually start from the 1970 paper. I also look at related papers that were written by others, for example, Tom Sargent's papers and the work that Sargent and Lucas did together. From the policy perspective, I look at all of that body of work. This was pushing policy modellers towards being more rigorous and serious. So I would not say this one is more important than the other one, because the body of this work is what led to an attempt, an effort to improve the modelling. I say "an attempt" and "an effort" because I'm not happy with what we have right now, but at least we have the methodology and we can push the younger modellers to keep going at it until we find better answers.

Blanchard: It seems to me that there are two different contributions. I mean, you are right that the philosophy is common to all of Lucas's papers. After Lucas/Rapping (Lucas and Rapping 1969), he had changed his mind, and the 1972 paper really defined how he thought you should do macro theory. And the 1976 paper said: But if you're going to do empirical work, be careful because it is much more complex than you thought, so it had a lot of effects on the mapping from theory to empirical work. I think in a way it was oversold. I think the first one just changed the profession for the better or for the worse. The second – Beatrice was talking about the holy cross – the Lucas critique became too much because in many cases, the relations we are looking at are fairly stable. The Lucas critique is not of the essence, but it became very big and you had to confront it or at least pay lip service to the Lucas critique. So I think they are both extremely important in two different directions under the same general theme.

Laidler: I agree entirely with Olivier about that. The Lucas critique was indeed overdone. And I would add that there is this widely believed story that the inflationary experience of the 60 s and 70 s, where policy was allegedly based on the Phillips curve, shows how relevant the Lucas critique was at the time. Well, that's a myth. That didn't happen. The Phillips trade-off had nothing to do with the conduct of policy in the 60s and 70s.

But if I may bring the discussion back to the History of Economic Thought, and one of my own hobby horses: What the Lucas critique really forces you to think about is the economic ideas that at any time underlie the conduct of policy, the reaction of agents to policy, and the role of all this in driving the economy's behaviour and in generating the resulting data. For example, it is only after 1972 that the "true" model of the economy could become the Lucas model; but the Barro 1978 paper on applying new classical macro to the U.S. used data generated when everybody was fully paid up IS-LM Keynesian (Barro 1978). So IS-LM should have been the model used to generate expectations in Barro's empirical work. The general and lastingly important implication of the Lucas critique is that it says to economists: Study the history of economic thought because you need to understand what people were thinking when the data you are trying to explain were generated, and not just what people now think in the light of current knowledge.

Cherrier: I think the early reception of Lucas 1972 needs to be disaggregated. I mean, for some economists – just like Athanasios said – what matters is the stream of papers, not one single paper. You will find Friedman talking about "Lucas's rational expectations" or "Lucas's big ideas" or "Lucas's decision-based microfoundations." A few others single out the 1972 paper as having a key influence on them. We also need to disentangle methodological influence from substantive influence. For instance, there were economists who endorsed both the methodology and the police ineffectiveness conclusion. To give you one example, this is what happened at the Federal Bank of Minneapolis at that time in 1972. They had a whole team of economists, John Kareken, Neil Wallace, Arthur Rolnick, Thomas Muench, Thomas Sargent and others trying to get some optimal police rules out of large-scale macroeconomic model that was maintained by the Fed, the FMP model.[1] Long-term Minneapolis Fed economist Rolnick remembers that they ran stability tests, in which this large-scale model failed. He added: "And *then Bob Lucas sends his neutrality paper, showing that there was something seriously flawed with macroeconomics… Wallace gets the paper, he chews on it for a week, then gets out of his office, and he says: 'Macroeconomics is dead. We need to abandon that programme. We need to go to microfoundations.' Wallace would later say: 'It's also the paper that got me thinking about money. I mean, I just changed my topic.'*"[2]

So this is one example. You can contrast it, for instance, with the reception by David Cass and Karl Shell, because they also reflected on Lucas's paper. And that's interesting because in this case, they endorsed the methodology but they rejected the policy conclusion, and that led them to create other models. So Shell, for instance, would say something like: "Yeah, I agree with the rational expectations [hypothesis]. But I had worked on overlapping generations models, and I couldn't believe that OLG

[1] FMP stands for Fed-MIT-Penn. It was developed in the 1960s by a team of academic and Fed economists led by Franco Modigliani and Albert Ando.

[2] Interview with the author, June 2018. Rolnick entered the Fed of Minneapolis, stayed there for his entire career and became its long-standing Director of Research between 1985 and 2010.

modelling would lead to some policy ineffectiveness conclusion." And Cass would say something like: "This was a new definition of equilibrium, the state space, and I wasn't sure what the state space was. I couldn't understand how precisely Lucas defined his new concept of equilibrium." And again and again, this led them to post alternative models to handle that (for exact quotes see Cherrier and Saïdi, 2018; Spear and Wright 1998; Spear and Wright 2001).

Blanchard: Just a footnote. It is clear that the effect of the 1972 paper, or maybe the 1970 and 1972 papers, was quite dramatic. I mean there is a marvellous example that David will remember, which is the macroeconomic textbook written by Tom Sargent, and Tom basically had written the first part, which was very much an IS-LM, Patinkin type, but IS-LM in essence. And then suddenly there was a road to Damascus moment, and the second half is entirely about optimisation, dynamic optimisation under uncertainty. I don't know what the role of Lucas was in that, but it is clear that a number of people just changed their research agenda, indeed from the MPS (the MIT-Penn.-Social Sciences Research Council model), to go back to Beatrice's example, to completely different ways. So it was really a fairly dramatic change in the mind of many people – I think more on the methodological side than on the neutrality of money – but people who believed in the neutrality of money were happy to see the result. People who didn't believe it believed that the model was incomplete. But on methodology it was quite dramatic.

3. The microfoundations requirement

Clerc: Thank you very much for these answers and reactions. Just to follow up on the methodological questions: Do you think that all of what you mention – rational expectations, general equilibrium perspective, the microfoundations requirement … Do you think that, making a counterfactual exercise, all those aspects would have been at the core of mainstream macro had Lucas's paper never been written? The microfoundations requirement, for example, was already on the way, as David recalled. I have notably in mind the Phelps volume for price and wages stickiness, which was published two years earlier (Phelps et al. 1970), but also many other examples for the consumption and investment functions.

Laidler: It follows from what I said earlier that I think that the Lucas 1972 paper changed the way in which the subject has worked ever after. Yes, people were concerned with microfoundations before 1972. Yes, they were worried about how to deal with expectations and so on and so forth. But those 19 pages synthesized all those issues and brought order to where there had been intellectual chaos and gave the whole profession a new jumping-off point. It really was a bit like the process by which Keynes' *General Theory* changed macroeconomics in the 1930s. You know, economic ideas have biographies, as do economists. Economic ideas and economists continuously meet and interact. Usually these encounters influence the subsequent biographies of the economists, but sometimes an encounter changes the subsequent biography of the ideas. That is what I think happened in 1972 with the Lucas paper.

I should add, perhaps, that personally, I wasn't convinced by the paper, when I first encountered it. But it still had a big effect on the rest of my career. For example, I found that I could no longer work with some of my colleagues at Manchester, where I was at the time, because they really did buy its arguments. Off they went into the new-Classical camp, and I remained skeptical for good or ill. And of course the paper's pervasive influence helped define the intellectual context of all my subsequent work

Blanchard: My impression is that Lucas had intellectual charisma to a degree that very few of us have. So I suspect something like this could have happened. I mean, if you look at the Patinkin book, you know, it is microfounded, it has expectations, it has all these things, but Patinkin is not the same writer as Lucas. Only God knows about counterfactuals, but my sense is that Lucas played a role by just being incredibly convincing about his methodology and the way to think about things. You know, Sargent is a brilliant man. Would Sargent have done the same thing if Lucas had not been there, I don't know. But clearly it was, as I remember it, very tough to fight it intellectually, to argue with Lucas. He was just incredibly good, incredibly convincing, incredibly talented. So God knows – maybe.

Orphanides: I would say that the microfoundations aspect probably was the most important aspect of that specific paper. The rational expectations hypothesis was already coming in single equation aggregate models that had been written down before. General equilibrium, well, all macro models at some level were already in general equilibrium. The question is what do you mean by general equilibrium? But deriving a specific example of what are the aggregate implications for price determination from microfoundations? Providing a specific example in showing that that could overturn what policy models – I always go back to that – would have been using to understand business cycle dynamics and prices was very different. Sure, microfoundations was already there. Arthur Burns was talking about the Phillips curve in the sense of being determined by regional markets and so forth already in the 1950s, but without models. That was all words. But this, the Lucas paper, provided a very concrete and specific example that showed how you could go from micro to macro. Now, I should add that I don't think that the policy implications were convincing. I don't think that this is really what most people interested on the policy side picked up. The neutrality results, for example, were driven from the simplistic assumptions that were there. But the methodology of how to go from micro to the aggregate and how – and here I am going back to the earlier discussion – you should be careful not to be misled by the aggregate equations for policy implications. This was a wonderful example that did have a big influence.

Cherrier: I also want to insist that all the blocks were already there – the microfoundations, the general equilibrium, the rational expectations – and of course the prize goes to whoever combines the blocks. Lucas did it and was convincing with the way he did it. If someone else had done it, it would have probably been done differently. I can't help thinking of what was happening in other fields at that time, in particular in public finance with James Mirrlees also bringing an equilibrium framework to optimal taxation theory. So there was something in the air and everyone was combining the blocks. What matters is how much of Lucas's combination was being picked up by other economists. And much of it was because, as you just said, Lucas was extremely convincing, but that is not the whole of it: He gave up fairly quickly on the information aspect of the paper.

4. Lucas, monetarism and money

Clerc: Just to quickly come back to what David said about the impact of Lucas's paper on his own career. I think it had an impact on other monetarist authors, too. There is a paradoxical situation because in some sense Lucas has always defined himself in interviews as monetarist. Yet, clearly the direction that economics took after his paper was very different from what the monetarists defended. I have in mind this sentence by Sargent in his 1996 paper, just after Lucas got the Nobel Prize, when Sargent argued that "[f]rom today's standpoint, it is evident that the rational expectations revolution was impartial in the rough treatment it turned on participants on both sides of the Monetarist-Keynesian controversy raging in the 1960s" (Sargent 1996). So, David, what is your feeling about the relationship between the Lucas papers of 1972 and 1976 and monetarism?

Laidler: Well, monetarism was really a fairly diverse body of doctrine, and pragmatically policy-oriented too. Its core policy claim was that the quantity of money is a more important macroeconomic variable than then-standard Keynesian economics, which focussed on fiscal policy, taught; and it tried to provide evidence to back up this claim. Monetarism was very down to earth, and even when it wasn't directly discussing policy never strayed too far away from the data. If you had to find a methodological ancestor for Milton Friedman, it would be Alfred Marshall, right? It wouldn't be Leon Walras. For Milton the test of an economic theory was how it stood up to empirical testing, not how well rooted it was in economic "fundamentals."

So, what Bob Lucas did was put Leon Walras at the centre of macroeconomics, and that ultimately left no room for traditional monetarism because it prioritised deductive theory over empirical evidence. The first thing that stuck in my throat about new Classical economics, once I began to understand it, involved an empirical question. In the Lucas (1972) Walrasian model, you interpret the Phillips curve as an aggregate supply curve. Causation – if there is causation, because everything happens simultaneously – runs from prices to quantities. And yet, it is an empirical commonplace that monetary policy works with a long and variable lag, first affecting output and employment and only after about 18 months beginning to affect the price level. Lucas's analysis was totally incompatible with this most basic stylised fact, that everybody was familiar with at the time. And yet people adopted his Walrasian approach, apparently prioritising theoretical rigour over empirical content. This was a huge change in the way people did macroeconomics – for good or ill.

Blanchard: May I ask a question to David because he might know the answer, which is what was Milton's reaction to the 1972 paper, because indeed, it was incredibly different from Milton's approach. I mean, the conclusion was the same as Milton Friedman's presidential address. Money, if you try to use it, will not work. But in terms of methodology, David has said Milton was the pragmatic guy with a very simple model, and he probably didn't love the particular story that Lucas was pushing in that 1972 model. So I'm just wondering whether that conversation ever took place and whether you actually know what Milton thought?

Laidler: Well, it's difficult to speculate, because I wasn't there. I was in Manchester, and Milton was in Chicago. But in 1975, Milton did publish a lecture on inflation and unemployment given in the UK in which his expectations-augmented Phillips curve was Lucas's (Friedman 1975). It was an aggregate supply curve. I didn't react immediately to this feature – my assigned task was to discuss the lecture's implications for UK policy, with inflation then heading towards 25 per cent per annum. But, sometime later (I don't remember precisely when) I did have a little correspondence with him about the compatibility between the theory he was now propounding and the evidence. I got a kind of noncommittal answer that this was a technical matter, and that he was sure things will work out. I don't like putting words into deceased economists' mouths. But (*pace* Ed Nelson 2020a, esp. 271. et ff) I still think Milton was faced with a choice between competitive markets, which he loved, and empirical evidence as the primary determinant of what you believed about the economy, to which he was also devoted. And finally, with that Phillips Curve paper of 1975, he chose competitive markets over the empirical evidence, and thereafter he was pretty quiet. I regretted that, but I don't think anybody took much notice of this at the time.

Clerc: A strange thing is that Lucas is very often credited for having impacted with this paper the turn of macroeconomics. But on the other hand, the cashless economy is a feature of modern macroeconomics. And Woodford's cashless economy (Michael Woodford 2003, ch. 3) is very, very different from what Lucas intended at the beginning of the 1980s when he tried in many papers to make the quantity theory of money consistent with the Arrow-Debreu model. So clearly, this feature of the absence of money in the current vintage of macroeconomic models is not something that Lucas expected.

Laidler: Lucas co-authored a paper on the demand for money function as late as 2016, so I don't think that he himself ever gave up on money (Benati et al. 2016). Around 1989/1990, he also wrote a working paper in which he introduced price stickiness quite arbitrarily, which of course is the key to getting money to do anything to real variables in the old-fashioned monetarist approach (Lucas 1990). So he was never quite Lucasian when it came down to it. But, he also insisted on his methodology, which, for example, he did very dramatically at that 2003 HOPE conference, when he talked about his Keynesian education (Lucas 2004) and said it was great to get rid of the Samuelsonian sort of dynamics that patched up the spaces between static equilibria.

So, as long as you insist on assuming that markets clear all the time when you build a model, you run right back to the Arrow-Hahn critiques of the 1960s that this kind of analysis leaves no room for a theory that has anything to do with money's essential role as a means of exchange and unit of account. New classical attempts to put money into the system – by overlapping generations or just by imposing a cash advance constraint – never went anywhere; and the theoretical purists in that camp didn't much like these approaches anyway, particularly the latter. I think that this is the theory-based reason why money has faded from policy discussions. There were other reasons as well: the monetary contractions around 1980 were not exactly pleasant and showed that there was something wrong with the monetarist theories said to underlie their design. Also, there were increasing problems of instability in empirical demand for money functions as the pace of institutional change quickened up. So there were also empirical reasons for money to become marginalised.

But the theoretical reason embedded in the Lucas approach prevented any kind of new theory of the role of money in economic activity that might have proved empirically useful actually putting down microeconomic roots. And you know, if you saw that this couldn't be done, and you were a graduate student, you weren't going to work on money, because you wanted to get your Ph.D. published and you wanted to get a job and you wanted to get tenure. So I do think that the insistence on clearing markets weakened the role-of-money research agenda and helped sent it off onto the margins. I think that was a shame. Just look at what has happened to money growth over the last 18 months and look at what's happening to inflation. And then look at how many people were surprised by the latter. I really do think that this state of affairs is ultimately, an indirect legacy of Lucas 1972.

Orphanides: Let me add something to this from the central banking perspective. I started my career at the Federal Reserve, estimating money demand functions. I was never a monetarist, but I want to ask: What is the meaning of monetarism in this discussion? Because one meaning of monetarism refers to people who focus on the quantity of money, M1, M2 and high-powered money. But the other meaning, which I believe is more important and compatible with the research programme that Lucas started in the 1970s, including the 1972 paper, was the emphasis on stable rules as opposed to discretionary decisions for fine-tuning the economy. And in my view, one of the characteristics of what monetarism always was is a framework for identifying stable policy rules instead of focussing on fine-tuning.

I think that approach and that aspect of monetarism was actually supported by Lucas's work and has survived. Of course, it has nothing to do with whether you have money in specific models, for the reasons that David pointed out. It is very difficult to have money in specific models, and add to that the fact that, with financial innovation and deregulation from the late 1970s onwards, we really don't have any hope of having stable relationships between money and credit aggregates the way we used to until the 1970s. So the other definition of monetarism is to try to infer empirical relationships between some money aggregates in the economy and use those for policy.

Laidler: People have called inflation targeting monetarism without money, and there is indeed something to that. But I don't think Milton Friedman and the Chicago School were the only people arguing that macroeconomic policy should follow systematic feedback rules rather than evolve as just a series of random events. This is taking us rather too far away from Lucas (1972), but let me be clear that, though I'd love to resurrect monetarism, when I was writing about it, I was stressing the existence of a stable aggregate demand for money function. And so do Bob Lucas and his co-authors in 2016 – that is very comforting to me.

Blanchard: Basically I agree with both Athanasios and David. There are two aspects, and maybe I am not understanding all of the dimensions, but it seems to me that we have shifted from "*M*" to "*r.*" the interest rate. That is basically based on the notion that the demand for money is incredibly difficult to pin down. It shifts all the time. So in this case, you want to go directly to what you care about, which is the interest rate. This doesn't mean that we don't believe that money is important. We just believe that there are so many shifts in the demand for money, so it's better to just look at "*r.*" But I think the influence of saying expectations matter very much for current behaviour, which is the theme of Lucas and others, really implies that then you should have predictable monetary policy. You should be clear as to what your target is. Then you can be flexible with the instruments, but if people believe that you are serious about the target, they will not care too much about which instrument you use. As soon as you say expectations matter very much, then this implies that policy has to be predictable. It's not very useful to randomise it in some crazy way. Whether you go all the way to stricter rules or not, that can be discussed. But I see again, the emphasis on expectations is implying that, you know, some kind of predictable behaviour of the Monetary Authority is essential. But these are two different things.

Laidler: Let me just cut in with a quotation that I found when I was preparing myself for this panel. Listen to this: "*The rule of expectations is … much greater than is normally assumed in academic and journalistic comment. Changes in the climate of expectations brought about by events … or by the timing and manner of announcement and implementation of policy measures can often act to negate or greatly reinforce the tactics of the authorities.*" Right now, that sounds good. But it is the Bank of England at the Hove conference 1969! (Bank of England 1970) OK?

So I think the history of this topic is probably a little more complicated than we are making out right now. There is a bit of work that needs to be done before we come to conclusions about its origins.

5. Methodology and labels

Clerc: Let me just ask a last question on methodology. You mentioned the Walrasian foundations that Lucas imported. Would it be so difficult to develop Marshallian microfoundations? It seems that Leijonhufvud, for example, tried to do this at the beginning of the 1970s. So, are microfoundations necessarily Walrasian?

Laidler: Part of Milton's approach, which I said was Marshallian, was to treat economic theory as an engine of analysis and pick and choose which bits to use for particular problems. So, for example, sometimes you'd find him treating the behaviour of the money supply as exogenous, and sometimes you'd find him emphasising the role of feedbacks from the economy to the quantity of money. Which depended on the context, and on the specific problem. When it came to Walrasian microfoundations as deployed in new Classical economics, if you could not derive your approach to money from first principles, Neil Wallace, for example, would not even listen to what you had to say. He is on the printed record as saying that (Wallace 1990). So I do think that there was a big difference between the new Classical approach to microfoundations and what preceded it. And I do think that its widespread adoption hampered further work on money, even in a non-Walrasian tradition. Such work might have turned out to be a waste of time. Who knows? But the incompatibility between clearing markets and an essential role for money is one of the reasons why work on money went out of fashion.

Orphanides: If I could answer this question differently to bring in a different perspective, Pierrick, what you outline is a wonderful wish that I don't think is actually achievable. In my recollection of studying macroeconomics – and I wanted to be a macroeconomist at MIT – one of the first classes I had was with Franklin Fisher. He came to class and said: "You know, macroeconomics does not exist. I have proven this in a paper." He did have a nice paper in which he simply noted that the aggregation conditions that are required to be able for macroeconomists to work with macroeconomic concepts really are violated in most contexts. So we need to cut a lot of corners and make a lot of simplifying assumptions to go from microfoundations to macro aggregates. And those assumptions aren't really valid. So we will always have the conflict that was there between microeconomics and macroeconomics. And from the policy side, the way to resolve this conflict is to find out what approximation is most sensible for the specific question you want to ask. But you can never resolve that conflict. If you want to be true to microfoundations, you will never be able to have an aggregate economy and talk about an aggregate model really precisely.

Cherrier: I just want to make sense of this. According to David's definition of monetarism very few economists qualify as monetarists. According to Athanasios' definition, many or even most economists do.

Clerc: It was Modigliani who said that we are all monetarists now.

Laidler: That was a long time ago.

Clerc: Yeah, that was in 1977 at the San Francisco Fed debate with Friedman.

Orphanides: I think, Beatrice, you are exactly right that there are different perspectives, and I find fascinating the description in Ed Nelson's book on Milton Friedman (Nelson 2020b) that goes precisely through these points in great detail. There are different aspects from the central banking perspective. I am actually very much in agreement with Ed Nelson on the principles that have influenced central banking. And in that sense, many more of us are monetarists now than would have been the case 50 years ago. But yes, that is not David's definition; that's the other definition.

Blanchard: My sense is that labels, you know, fit for some period of time, and then they don't. If I go back – and that is long before my time and, again, I'm not a historian, but I think this is where David probably comes from initially – the Keynesians believed in fiscal policy, and they just didn't give a damn about monetary policy. It was not the thing; they used fiscal policy. Then the monetarists came and said: Well, money is actually really important. And then they created the Monetarist-Keynesian distinction. By now we all believe that monetary policy is important, that fiscal policy is important. So I am not sure that using the words today and saying "I am a monetarist" or not … Maybe David feels he still is, but my sense is: No, we have gone beyond that. We are talking about different issues.

Laidler: I don't want to get into a quasi-religious dispute about which sect we belong to. But I have been astonished over the last 18 months by the lack of attention that has been paid to the behaviour of the monetary aggregates – I really have. Almost everybody is surprised that the inflation rate is over five percent in the United States (Michael Bordo and Tim Congdon whom I saw quoted somewhere last October [2020] saying they thought that some people were going to get a nasty surprise in 2021, are rare exceptions). There has been widespread neglect of real information being conveyed by important data, that I think we are beginning to pay for. So it's not just a matter of whether I am in the right sect or not. It's that I do actually still look at some of these old variables and treat them as possible leading indicators. And if central banks had actually paid a little more attention to them, they wouldn't have committed themselves so firmly to rock-bottom targets for interest rates up to 2023, which I think they are going to find very embarrassing for their credibility.

Blanchard: I feel I have to come in. This now has nothing to do with Lucas, but I want to reassure David that he can sleep better because he has not taken into account in what he said – I am sure he knows about it – that most of money now pays interest. Really, when we look at the balance sheets of the central bank, when we worry about inflation, we worry about the non-interest-bearing part of money. This is, in our theory, I think what matters. The rest is really intermediation, buying bonds and then issuing central bank reserves which pay interest. The balance sheets have increased enormously, but the notion that this is going to lead to very high inflation in the future, I think, does not follow. But that is completely unrelated to Lucas – and therefore I will shut up from here on that particular topic.

6. Non-neutrality of money

Clerc: What you say, Olivier, makes me think that in the first edition of their textbook, Rüdiger Dornbusch and Stanley Fischer claim that, some years earlier, this latter would have been considered a monetarist treatise (Dornbusch and Fischer 1978, esp. 520–521). They argued that we now all agree that money is important and monetary policy is critical – even though we also have to take into account fiscal policy and investment shifting in order to fully understand short-run fluctuations. It is also interesting that Athanasios mentioned Nelson's book, in which we can see that Friedman – as David recalled from 1975 – seemed to embrace Lucas's explanation of monetary non-neutrality based on price surprises. On the other hand, in other contexts Friedman also emphasised the existence of nominal contracts for wages and prices and thus put forward more New Keynesian explanations of the non-neutrality of money. I think this is a nice illustration of Friedman's pragmatism.

So maybe we can now shift to the non-neutrality side of the Lucas paper. Here is my first question: What is, in your opinion, the most important reason behind the dismissal of Lucas's islands model? Is it its inability to generate persistent real effects of monetary shocks? The model is based on the assumption that once these shocks become public information, money is fully neutral. In the modern world – but even at the time of Lucas – monetary shocks pass into public information very quickly, whereas their real effects last for several quarters. Or is it rather its implication that only unanticipated monetary policy would have real effects? Or is it something else?

Cherrier: I have a theory, and I am happy to be proven wrong because it is really just a theory. I know that other panellists are going to emphasise other substantial aspects. So I want to point to the accidental historical elements and their dynamics. My theory is that the informational aspect in Lucas's paper was partly forgotten because, with regard to money, neutrality and policy effectiveness, people tended to work on translations of Lucas's model rather than on Lucas's model itself. It was a model difficult to understand, widely, and there was a mathematical mistake that was only seen like ten years afterwards by Jean-Michel Grandmont. When it was corrected, Grandmont thought, it opened the door to a multiplicity of equilibria.[3] The paper was rejected by the *American Economic Review*, and it is super symptomatic why it was rejected. The referee said – let me quote because it is really symptomatic: "*I find the paper exceedingly formal and I am not sure I fully understand the economics of the*

[3] This story is narrated in Cherrier and Saïdi (2018; Lucas 1983).

theorems Lucas presents… I have been following fairly closely the format of the articles published in the AER, and in comparison, Lucas's exposition is pitched at what I think is a distressingly arid level" (cited after Gans and Shepherd 1994, 172)

For instance, when you look at Stanley Fischer's paper, he responds to Sargent and Wallace. When you look at the paper by Edmund Phelps and John Taylor, they also respond to Sargent and Wallace and they even say that they are going to address Barro, who is "building on models by Lucas." James Tobin also responds to Barro (Fischer 1977; Phelps and Taylor 1977; Snowdon and Vane 2006, 148–162). There is a process whereby the paper becomes very influential methodologically, but once you have taken whatever you wanted in terms of general equilibrium, definitions of equilibrium and rational expectations, it seems that the conversation is taking place with reference to other contributions. That is just an hypothesis.

Laidler: I agree with that. Some people just looked at the Lucas model and said: "Look, prices are sticky, let's get on with it" – Fischer and Phelps and Taylor, for example; and that approach took off among "New Keynesians" (Fischer 1977; Phelps and Taylor 1977). But there is an old saying which I heard from Milton: It takes a theory to beat another theory. The islands model still hung around as well, however, until you got real business cycle theory that observed all the methodological rules, but could also produce persistence. If you had not had Kydland and Prescott (1982), I don't think the islands model would have disappeared quite as quickly.

Blanchard: Sure. Let me have a go at it. I think it didn't have a major effect because it was wrong, not wrong in terms of equations – although there was a mistake, and I must say that, as a student, I did not find it. It was wrong in what David said, which is that basically we know from the data that the way money works is by affecting quantities first and then prices adjust over time. And this was just the reverse. It affected prices and then quantities reacted. So it was fundamentally wrong in terms of description of reality. And there was an alternative which was created by Fischer and Taylor and others which built on the 1970 volume that Athanasios talked about, which was basically: Prices and wages are sticky; for this reason, money first affects quantity, affects demand, and then eventually there is overheating and it goes into inflation.

What was really fascinating in 1974 when I took the course is that the two theories had the same equation for the Phillips curve. It was basically a relation between "y" – or deviations from output or unemployment – and "$p - p^e$," price minus expected price. But in the Lucas way of thinking about it – Lucas, Barro – basically – it was a relation in which "y" depended on "$p - p^e$." If "p" moved, then people made a mistake and they produced too much. In the Fischer-Taylor way you just rewrite "$p = p^e$" plus something which depends on "y." And then the causality went from output or unemployment, given expectations of prices, to actual prices. It was fascinating how these two completely different ways of thinking about things led to the same equation. But over time, it became clear that stickiness of prices and wages, which we still don't fully understand but which is clearly a feature of reality, was much more important than information issues. Signal extraction plays a central role in many aspects of the economy, and it is terribly important. But for that particular mechanism, I think that signal extraction is not central.

Orphanides: I agree with that. You ask and think about it inside a policy institution. You want a model that will at least fit the basic dynamic facts of the economy with some assumptions. This is what was being done and the expectations assumptions were all wrong. What Lucas showed in this series of papers was that that was a problem with a very simple example that didn't really fit the facts. So it was a wonderful example to show what was wrong with the framework used to build models earlier, but it wasn't really giving you an alternative model that could be used for policy purposes. There is nothing wrong with that. Olivier mentioned the two equations. I remember in the 1980s this was an exam question for years at MIT: Write down two one equation models of the economy and discuss their properties and their differences. And one of them prominently was the Lucas supply function that was at the centre of the discussion about how we understand the economy, but not for how we build models for policy purposes.

So I would not say that the Lucas islands model was dismissed. I would say it was used there to provide insights, but it was too simple to be used for policy models that could be used for actual policy decisions in complex situations.

Blanchard: I think Athanasios is a bit too nice. I think it was seen as irrelevant by policymakers. You know, beyond the general message, and we all agreed – and that is true also of Fischer and Taylor – that, basically, fully expected money should have very little effect. But the particular mechanism, I think, was dismissed as not relevant.

Clerc: Perhaps we have to disentangle Lucas's islands model on the one hand, and the importance of signal extraction on the other. Since the early 2000s, and especially with the rational inattention literature initiated by Sims and further developed by Maćkowiak and Wiederholt (Sims 2003; Maćkowiak and Wiederholt 2009), it seems that signal extraction problems can trigger very, very persistent monetary non-neutralities. Do you think that imperfect information in general, and signal extraction problems in particular, are important for the real effects of monetary shocks? For business cycle fluctuations?

Blanchard: I think signal extraction is an extremely important insight. There are many situations in which we are trying to see whether something is transitory or permanent. It happens in our own lives. It happens for firms which have to take investment decisions. So it is absolutely fundamental to understanding the world. But with respect to money non-neutrality it seems to me to be not terribly important. Now, I think we have to be precise about the semantics. I am not saying information issues are unimportant. Information issues are of the essence, including for monetary non-neutrality. But the examples you have given – Sims' approach, the K-loop stuff – that is not signal extraction. That is basically how we process information. We process information in complicated ways. We simplify and there are interactions between what others think and what we think. All this is very relevant, but if I use signal extraction as a precise term, I don't think that is central to monetary non-neutrality.

Orphanides: I don't agree with that. I agree with Olivier that signal extraction is only one of the imperfections in information processes that are important. But I think it is one of the important elements that create non-neutralities and make monetary policy matter at the end of the day. But I would go beyond that.

Blanchard: Explain how signal extraction is relevant in creating money non-neutrality. What is it about the Fed that you don't know?

Orphanides: There are a lot of things about the Fed that I don't know. And what I wanted to add more specifically is that it is not the pure form of signal extraction in which you know the model, you know all of the other parameters of the model, you observe a shock and then you are supposed to split that shock into two components, which is one of the elements that you have in the Lucas model. That's just too simple. That is one component of a more general problem in which you are not sure what the model is. You are not sure what the parameters are. And these things interact with each other. So, Pierrick, you mentioned some families of models that emphasise one approach. There are different families of models, and at the end of the day, the problem is that all models used for practical purposes are approximating models. One of the difficulties in modelling is how to understand the implications for policy of the fact that the model is an approximating model. And this is where it gets complicated.

I would actually say that one of the reasons that in the 1970s some of the profession ended up not helping policy institutions in setting policy is because they did not provide answers. Now you tell us that the way we were doing modelling in the 1960s was wrong, and Lucas has these wonderful papers that show exactly how we are wrong. But we don't know how to improve on this. How do we build aggregate models that we can use in practice, taking into account the learning that needs to take place when the economy is uncertain, the information processing, the signal extraction? Here I will recall all the papers you wrote, Olivier, that I thought were very important, about the different stages of production and the fact that you have so many additional ways in which you can bring lags into the aggregate economy (e.g., Blanchard 1983).

So you have to put in information processing. A shock happens, and I don't know why that occurred and where to attribute the different components. Is it lags in understanding what is happening? Is it some other lags that I am not modelling correctly? All of these things mix together and, in my view, overall the information imperfection is one of the main things that Lucas in that paper, and then Sargent and many others around them focussed on. That work highlighted how important it was to not take for granted some exogenously given or estimated expectations formation process, but actually trying very seriously to see how our expectations form and how we model them in a changing economy.

In my view, this is what led twenty, thirty years later to trying to integrate learning behaviour into macroeconomics. And that is not in Lucas, but it led to that. I link this whole methodology, I start with Lucas and Sargent (1978) in *After Keynesian Macroeconomics*, focussing on how important it is to understand expectations and the rational expectations revolution and then to go to Sargent (1993), *Bounded Rationality in Macroeconomics*, recognising: "You know what, we should not assume that we know the model and use model consistent expectations as the answer. That actually can also give us the wrong answers." Right now, I think we are we are trying to improve in that dimension. And I think we have made some progress, but we are still not there, integrating learning with microfoundations, with general equilibrium to understand the economy better.

Laidler: Can I just add one footnote to a discussion that I am really out of my depth in? Doesn't creating information upon which you can base price setting and production decisions cost resources? Surely information costs resources on the margin. And if you have alternative ways of insulating yourself against the consequences of mistakes that are cheap to employ, you are going to gather less information. If those alternatives are expensive, you are going to gather more information. I have always been struck that people who have lived through a really serious inflation, such as the Israeli graduate students that I used to meet, are a lot more sophisticated about the way in which monetary policy works than people like the current crop of Canadian graduate students, who have lived through thirty years of successful inflation targeting.

Orphanides: I agree with that statement, but that is what I see as an implication of the need to incorporate the learning that takes place and the approximating rules of thumb that people choose based on their recent experience. And this is where we have very nice work in recent years, for example, about lifetime experiences influencing how people form inflation expectations. Yeah, it does matter if you have lived through hyperinflation, or you haven't, for how you form expectations.

Blanchard: We are facing exactly this issue today. There is a discussion about inflation and whether it is going to get to a level which is salient enough that people will change the way they have been thinking for thirty years. So this is absolutely central. And it is interesting how we went from adaptive expectations – we rejected it, we said it's not rational enough – all the way to rational expectations, which was probably the way to do it. But we were really Bolsheviks about this. And then over time, people like Sargent, Woodford and others said: "No, no, no, we went too far. It is more complicated than that." And now we are basically struggling with issues like salience or bounded rationality. I don't know where we will go, but it is an interesting intellectual voyage.

Laidler: I can't resist a mischievous comment about progress in economics. Gunnar Myrdal's 1927 PhD thesis (see Lindahl 1929) was basically built on rational expectations, which he understood implied that the current state of the economy depended on its future states. This insight made him a nihilist about the relevance of maximising theory to the way economies actually worked. After that, the Swedish economists worked backwards from rational expectations through temporary equilibrium ideas etc. to *ex ante* – *ex post* modelling with something like adaptive expectations. But all this was technically very difficult to handle. And then, in 1936, you got to Keynes, who in effect said "Let's make expectations exogenous so that we can actually construct a model that our technique is up to." That was progress in the 1930s. And then we started moving in the opposite direction in the post-war world. Heaven knows where we are headed next. I don't know.

Clerc: Olivier, you offer a very nice transition with the concept of salience, because it is in the context of the flattening of the Phillips curve since the 1980s that you developed this, arguing that the flattening of the Phillips curve would strengthen the countercyclical role of monetary and fiscal policy.

Laidler: The flattening the Phillips curve is not just a matter of expectations changing and becoming anchored after you have experienced a long period of price stability. All kinds of habits change. When I was an undergraduate, the London Tube had a coin machines that issued only tickets of a single price., and on those machines they had engraved in glass the destinations to which sixpence or a shilling ticket, etc. could get you. You don't do something like that if you expect the inflation rate to become five or six percent and move towards twenty-five percent. Ticket machines configured in this way vanished in the early 1970s. And there are lots of examples like this. So it is not just expectations. It's all kinds of social conventions about setting prices and arranging transactions that get locked in during periods of price stability and it is costly to unwind these. So I am not too surprised that the Phillips curve has flattened over the last few years. We've had so little inflation by historical standards.

Blanchard: Just in a nerdy remark, again on semantics. There are two aspects to the Phillips curve. There is the coefficient on the unemployment gap, or whatever you want to call it. And there is the way in which expected inflation adjusts to actual inflation. I think we have very good stories for why expected inflation doesn't move much these days. That is basically a result of stable inflation over the last thirty or so years. That does not by itself explain why the coefficient in terms of the unemployment gap, given expectations, is smaller and that remains a bit of a mystery. "Phillips curve" is an ambiguous term in this case.

Laidler: Well, that was the point about my comment on arrangement like those for dispensing London subway tickets. You lock yourselves into ways of doing things whose persistence doesn't really depend upon expectations, but still prevents you from reacting to shocks that, if you didn't have these things in place, you would react to quite quickly. That's what I'm trying to get at.

Blanchard: Right.

7. Any impact of Lucas (1972) on monetary policy?

Clerc: … And just to make a connection with Lucas and the 1972 papers: Beatrice recalled that the paper was initially rejected at the AER, and Lucas wrote a letter to the editor to complain. He argued that the paper contained the first proof of the optimality of Friedman's constant money growth rule. So just in terms of monetary policy, what is for you the most important impact of Lucas on past and present practice of monetary policy?

Orphanides: I will not talk about just this paper again. I identify the 1972 paper as a wonderful, simple explanation of what Lucas had already pointed out in the 1970 conference, which was a whole discussion on the Phillips curve. Of course, intellectually Phelps and Friedman already had brought this in. The issue is this: Can you fine-tune the economy? Can you fine-tune business cycles? Is there an exploitable Phillips curve? And I think Lucas in his papers, including this one, showed that no, you can actually interpret the facts in a way that does not allow you to fine-tune the economy or exploit a trade-off in prices and unemployment. Frankly, I think this is what I would call part of my interpretation of the monetary principles that actually has survived. That is the sense in which we are all monetarists I believe. And this is what I find to be the most important contribution for policy purposes from that literature.

I would also add that this led to other things. David pointed out before that we can call inflation targeting a new incarnation of monetarism, and I actually agree with that. Back in the 1970s, people would have been talking about nominal income targeting along the same lines. Inflation targeting evolved from monetary and nominal income targeting. The principles are what stayed with us. The principles are: "Have a systematic policy that focuses on the long run." Why do we want to do that? Because we want to anchor inflation expectations? This is what Olivier was mentioning before. Right now, we have a very nice environment of inflation expectations remaining fairly well anchored. This is why most policymakers are not as concerned as they would have been thirty years ago about the spike we see in inflation. But this is the outcome of the intellectual process that started fifty years ago, thinking about how we model the economy differently, thinking about the limits of stabilisation, thinking about the importance of expectations in understanding business cycle dynamics and inflation dynamics.

Laidler: Two comments. First there is an extensive 19th century literature defending the gold standard that says a few things like that about the importance of long term stability. So this is not exactly a new topic. Second, there is a huge difference between Milton's policy rule and what we have now. His rule was for money growth, and he chose money growth self-consciously because he thought that trying to target the price level involved targeting something that was too far away along the chain of causation to be reliably hit. And I can also speak to the Canadian experience. We embarked on inflation targeting in 1991 with hearts in mouths, because we did not foresee that the inflation target would gain credibility in the way that it actually did. That it did so was not forecast from rational expectations. It was a learning-by-doing phenomenon that took hold when the Bank of Canada discovered that it could hit its inflation target more often than anyone expected it to be able to.

Orphanides: I agree with that, but this in part reflects the fact that the stability of monetary aggregates and money demand functions had broken down, so they could no longer be used. I also agree with you, Milton Friedman – and Ed Nelson explains that in great detail in his two volumes – chose monetary aggregates because at the time they did have these stable relationships and they could assure the public that the central bank would not be engaging in discretionary fine-tuning that can be so counterproductive. But before the end of his life, he actually also accepted that the way inflation targeting had evolved was achieving what you would have wanted to do and became effectively a supporter of the inflation targeting approach.

Laidler: But for him that was going back to Henry Simons, with whom he had initially disagreed – right?

Orphanides: We want price stability, so should we target the price level? Indeed, of course, we have the Swedish experience early in the 20th century with targeting the price level and the issues of "Can you do this effectively? And what is the price level that you should be measuring? Can we measure prices effectively?" All of those questions were there. I think today we have converged to saying that it may not matter that much if you have the correct price index. What may matter more is that you commit to maintaining low and stable inflation with the price index you defined. I think that has worked quite well overall.

8. Final points

Clerc: So, now we are opening up to questions from the audience.

Lilia Costabile: I would like to go beyond Lucas and take this opportunity to ask the participants at the roundtable two questions. The first one is: He said that now central banks control the interest rate, as they do of course, because the money demand curve shifts a lot up and down. Do you think that we are back at what Kaldor used to say, that the central banks do not determine the quantity of money, but they determine the interest rate? And how is this connected with the question of the endogeneity of the money supply?

Second question: When central banks discount assets that mean financial intermediaries bring to them in order to demand money in repo operations, they can put assets in different classes, according to their liquidity risk and so on and so forth. So you have a hierarchy of assets in different classes of risk, and each class receives, or even each single bank can discount their assets with a different interest rate. Some assets received high haircuts because they are very risky, and some, like government bonds, are very safe, so there are different degrees of discounting. Do you think this is a good channel to achieve non-neutrality? Because there is this big debate about market neutrality and non-market neutrality, whether central banks should become agents of environmental change. So do you think these discounting operations by central banks can be an obvious channel of non-neutrality of money?

Laidler: I have a very quick answer to the first question: The stability in the demand for money function that Milton Friedman first published was in cycle average data, one business cycle, one observation (Milton Friedman 1959). Benati, Lucas, Nicolini, and Weber told us in 2016 that this kind of long-run stability is still there (Benati et al. 2016). So I don't think we should take for granted the disappearance of the demand for money function in the face of institutional change. I was one of the research assistants who constructed the money supply series for the *Monetary History of the United States* (Friedman and Schwartz 1963). Looking backwards, I was introducing new assets into those aggregate series all the time as they appeared on the historical scene. And then the definitions of money got frozen in 1963, and we went forward with that 1963 definition of money. Surprise, surprise, it eventually started wandering around all over the place, and we began to pay attention to institutional change again. My second point concerns endogenous money: The question is not really whether money is endogenous. Of course, it's endogenous. The question is whether its endogeneity is purely passive: does money just adjust to changes in the arguments of the demand function with nothing else happening? Or is its endogeneity active, in the sense that the factors that bring it into the economic system are not confined to the arguments of the demand for money function, so that once new money gets into the system, its introduction has further effects. This debate has been going on forever. We have perhaps lost sight of it for the moment, but I don't think it was ever properly settled. And finally on the question of discounting private securities and such things: Count me amongst the people who get very nervous when central banks start to get involved in allocative decisions That is how the British economy I grew up in during the 1940s and 1950s worked, and I really don't want to go back to that.

Orphanides: Let me add something to Lilia's second question. Effectively, what you are describing is the collateral framework of the central bank and how it should be used. What you are pointing out I would interpret as examples of fiscal and structural policy that the central bank can do with changes in what would appear to be the bowels of monetary policy operations that sometimes would go unnoticed.

As David pointed out, this is an area that can be very problematic for central banks and in my view, *is* problematic for central banks. Let me give you an example from Europe. One of the reasons why we have had this fragility in the euro area and government bond markets that we have seen in the last twelve years is because of a flaw in the collateral framework of the ECB and specifically a decision that was taken in 2005.[4] The ECB Governing Council said: "Well, look, you know, if the government debt rating of whatever country falls below some threshold, as determined by a credit rating agency, a *private* entity, then we no longer accept it as collateral. We have a 100 percent haircut." Of course, that creates self-fulfilling crises and creates a stream of subsidies to more highly rated countries and penalties for countries that have a lower rating. So that is an example of massive fiscal and financial stability implications that can be associated with the collateral framework. Of course, as you pointed out, recently people have said: "Oh, you can also do green finance through the collateral framework." This indeed is the case, but it is quite problematic, in my view, to have the central banks do fiscal or structural or distributional policies through their collateral framework. Unfortunately, in the case of the ECB, this is already being done. But that is unfortunate. That is something that I think we need to be looking more closely into, not encouraging.

Cherrier: I would like to ask a question to other panellists who know Lucas: To what extent did he intend to have a direct policy influence? Unlike Friedman, I rather see him as trying to influence the academic intellectual framework and leave policy advocacy work to other colleagues. And at what point did he realise the impact he had on the discipline? Did he really orient the use of his contribution, or did he rather build on the echoes that it provoked – some that he maybe didn't anticipate – into the policy sphere?

Laidler: Let me start, because Bob Lucas and I were in the same class at Chicago. We took the same Friedman micro course and we answered the same exam question about why the Phillips curve exists in 1961 (see Friedman 1962, 282–284). (I can't remember what I wrote). We wrote our Ph.D. theses in the same (public finance) workshop, and they are published in in the same volume (Harberger and Bailey 1969). In my view Bob was and is a scientist. I never saw any sign of an ambition to become a public intellectual along the lines of Friedman. But I do think that he is the kind of scientist who has a social conscience – remember that he began his career as a historian. In his work in economics he has tried to provide model economies, with which people who are going to conduct policy can experiment before they get around to

[4] Additional detail and references can be found here: https://voxeu.org/article/how-ecb-planted-seeds-euro-area-crisis.

experimenting on real people. So there is, in this sense, a very deep policy orientation to his work. But anything more direct? No.

Trautwein: Athanasios, did you ever meet Robert Lucas?

Orphanides: I met him, but I did not get to know him well enough to be able to answer this question. Based on my conversations with others and my reading of history I would subscribe to what David just said as being an accurate description.

Trautwein: Let me wrap up by adding that I met Lucas once – and David has already mentioned the occasion. It was the HOPE conference in 2003 during which Lucas left his papers to the Duke University Library after giving a talk about his Keynesian education (Lucas 2004). What impressed me most was how he, towards the end of the talk, described the limits of the general equilibrium dynamics that he had come to work on later in life. Lucas said that there is a residue of things that his approach doesn't let us think about, such as financial crises and their real consequences. This implies that you cannot say anything about the kind of policies that should be run in such situations. Lucas then praised Keynes for bringing economics to the insight that, in a depression, the government should take the responsibilities for stabilising overall spending flows. Lucas said this about five years before the global financial crisis forced us to think again about expectations and the non-neutrality of money.

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