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LUCAS' EXPECTATIONAL
EQUILIBRIUM, PRICE RIGIDITY,
AND DESCRIPTIVE REALISM

MAURO BOIANOVSKY

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Lucas' expectational equilibrium, price rigidity, and descriptive realism

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Abstract. Robert Lucas' 1972 article on the neutrality of money represented the first effective challenge to Samuelson's neoclassical synthesis methodological separation between static microeconomic optimization and macroeconomic dynamics. Lucas rejected disequilibrium price dynamics, as expressed by the Walrasian tâtonnement and auctioneer mechanisms. Lucas' new treatment of equilibrium as an expectational concept, determined by the rational behaviour of information processing agents, was not restricted to market clearing competitive economies. Lucas' effort to compare alternative rational expectations models of price stickiness (including his 1972 original formulation) led him to stress the notion of "descriptive realism" of the models' main assumptions, which played an important role in his original discussion of model robustness.

Keywords. Robert Lucas, economic dynamics, expectational equilibrium, price rigidity, descriptive realism

JEL codes. B22, B41, E32

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The issue of whether to treat observed prices and quantities as market clearing arouses more controversy than it deserves. I prefer thinking of markets as cleared partly because of logical difficulties with the leading alternative view ... and partly because it leads the theory into the crucial questions of intertemporal substitution and expectations and away from the mechanical ‘auctioneer’ of the standard dynamics. (Lucas [1972a] 1981a: 101, n. 20)

[T]o paraphrase Tolstoy’s observation about happy and unhappy families, complete market economies are all alike, but each incomplete market economy is incomplete in its own individual way. (Lucas [1989] 2013a: 272)

1. Lucas’ methodological wisdom

In February 1980 the American Enterprise Institute sponsored a Seminar on Rational Expectations held in Washington DC, published that same year in the *Journal of Money, Credit and Banking*. Robert Lucas ([1980] 1981a) contributed one of the principal papers, whereas leading Keynesian economist James Tobin (1980a) produced one of the discussion papers with reactions to the seminar. By that time, rational expectations equilibrium macroeconomics, launched by Lucas’s ([1972b] 1981a) *Journal of Economic Theory* paper, had become dominant, if still controversial in several aspects. 1980 was also the time when Tobin published his Yrjö Jahnsson Lectures, delivered two years before, which included a critical assessment of rational expectations macroeconomics and its implications for

stabilization policy as elaborated by Lucas, Thomas Sargent, Neil Wallace, Robert Barro and other “New Classical” macroeconomists (Tobin 1980b, chapter 2).

Unlike the strong critical tone of his Yrjö Jahnsson second lecture, Tobin praised in his *JMCB* comments what he described as Lucas’ ([1980] 1981a) “historical sweep, methodological wisdom and far-reaching vision” (Tobin 1980a: 795). Indeed, Lucas’s 1980 article has been regarded as his main contribution to economic methodology. In particular, it spelled out the methodological breakthrough represented by Lucas’ ([1972b] 1981a) successful challenge of Paul Samuelson’s (1947) separation between static optimization-based economic problems and macroeconomic dynamics. That division was a hallmark of the “neoclassical synthesis”, a term famously coined by Samuelson soon after.

That “synthesis” dominated macroeconomics from the post-war period until Lucas disputed such theoretical tradition that left the dynamics of adjustment accounted for by factors other than the choice-theoretic framework. As Lucas often pointed out, that was associated to the debatable Walrasian mechanisms of “automatic” or “fictional” auctioneer and tâtonnement of “disequilibrium price dynamics”, which he repeatedly rejected (Lucas [1980] 1981a, 1986, 1987, 1996, 2003a, 2004; see section 2 below).

Despite Tobin’s (1980a) endorsement of Lucas’ ([1980] 1981a) historical and methodological insights (reaffirmed in Tobin 1983), he would not accept Lucas’ hypothesis – put forward in 1972b and further developed in other papers throughout the 1970s – that business cycles are explained by misperceptions caused by rational economic agents’ incomplete information concerning the state of the economy. Tobin (1980a) advanced a distinction between Lucas’ ([1972b] 1981a) *substantive* and *methodological* contributions that would become prominent as assessments of Lucas’

theoretical achievements came out around the time he was awarded the 1995 Nobel Prize in economics (Fischer 1996; Hall 1996; Svensson 1996; Sargent 1996; Chari 1998). Those authors distinguished between what they perceived as the “temporary” substantive or theoretical impact of Lucas’ 1972b article – as expressed by Lucas’ aggregate “surprise” supply function, often criticized since the 1980s after initial theoretical and empirical success – and its “permanent” methodological effect encapsulated by the notions of rational expectation stochastic equilibrium and of prices as functions of the sets of states of the world – with its offspring embodied by the celebrated “Lucas Critique” that economic models should in principle be based on optimization analysis of agents’ behaviour and therefore invariant to economic policy changes (Lucas [1976] 1981a).

As recalled by Lucas (2013b: xxiv), his 1972 *JET* paper was his first attempt to understand how real effects of monetary shocks – that is, non-neutrality of money – come about. The paper’s contribution was “to propose one way that prices might be ‘sticky’, or ‘rigid’, or non-neutral, something other than an across-the-board units change” (ibid). Lucas ([1972b] 1981a) set out a complex mathematical model to show how disparate information among individuals may cause sluggish price or wage adjustment to emerge as reactions to monetary shocks in an otherwise perfectly flexible prices economy. Unexpected price changes associated to incomplete information may cause producers to mistake an increase in aggregate demand for a relative demand rise, accompanied by larger output. Under complete information, higher aggregate demand would be followed by proportional increases in all prices (the price level) and therefore no output effects – and vice-versa for a less- than-proportional reduction of prices provoked by an unanticipated contraction of money

supply. This is Lucas' famous "signal extraction" problem (see e.g. Romer 2012, section 6.9).

Under the assumption of perfect price flexibility, monetary non-neutrality (apart from inflation tax effects) can only arise from imperfect information, an insight Lucas got from Edmund Phelps (1969; and introduction to Phelps et al 1970). The price flexibility market clearing set up seemed to Lucas in the early 1970s as the only one compatible with optimizing behaviour. However, as discussed further in section 3 below, Lucas' 1972 new concept of rational expectations stochastic equilibrium was not limited to competitive equilibrium. Hence, when Tobin (1980b: 44-48, section on "Rational expectations without market clearing") criticized New Classical economists for assuming market clearing, Lucas (1981b: 564-65) reacted by referring positively to rational expectations models with price rigidities caused by long-term economic contracts in non-cleared markets, originally put forward by Stanley Fischer (1977), Phelps and Taylor (1977), and John Taylor (1979).

Contract models, as Lucas (1981b) observed, were originally designed as arguments in support of Keynesian "activist" monetary stabilization policy. Nevertheless, it gradually became clear to Lucas – especially after the development of the real business cycle approach by Kydland and Prescott (1982), which built on Lucas' equilibrium concept but assumed complete markets in which equilibrium and optimal allocations are the same (see Lucas 2007: 8) – that contract models and his own 1972 *JET* article shared a broad common perspective. Both assumed incomplete markets in a rational expectations setting, and sought explanations for price rigidity as part of monetary accounts of economic fluctuations. Market clearing has not been a fundamental tenet of Lucasian macroeconomics, despite widespread opinion to the contrary. As aptly put by Kevin Hoover (2012: 56, n. 26), while commenting on

Lucas' ([1980] 1981a: 293, n. 11) remark about non-competitive rational expectation equilibrium, "Lucas is not a nihilist who would hold economics hostage to an ideal" – in this case, the ideal of market clearing.

Lucas (2013b: xxiv) remarked how "I have kept my eyes open for other resolutions to the price rigidity problem" in addition to his own 1972 misperceptions model but sharing the same broad rational expectations hypothesis that "people use their limited information as best they can" (Lucas 1981b: 562). In 1989 he circulated a paper – which remained unpublished for a long time – presenting a model in which producers fixed nominal prices in advance and consumers decided how much to purchase at these pre set prices (Lucas [1989] 2013a). That was followed by a similar but more complex model of price rigidity, joint with Michael Woodford, with the weaker assumption that sellers obtain no direct information on demand during a period (Lucas and Woodford 1993). Years later, Lucas worked with Mikhail Golosov on an influential general equilibrium calibrated price-rigidity model of menu costs and state-dependent (instead of time-dependent, as in Calvo 1983) pricing (Golosov and Lucas 2007).

In his Nobel lecture, Lucas (1996: 677-78) put his 1972 *JET* article in the company of contracts and sticky price models in general (including Svensson 1986 and Lucas-Woodford 1993) as sharing the same fundamental implication that anticipated money changes will not affect output, but some unanticipated changes might. Lucas (see Snowdon and Vane 1998: 127) regarded that distinction as the "key idea in post-war macro" and the solution to what he called the "David Hume paradox" of long-run monetary neutrality and short-run trade-off. A relevant question was whether it mattered "which of those rationales [for price rigidity and monetary non-

neutrality] is appealed to?” (Lucas 1996: 678). That posed an important methodological issue, dealt with in more detail by Lucas ([1989] 2013a).

In his relatively unknown 1989 piece, Lucas argued that the substantive predictions that emerged from alternative price-stickiness formulations of that class of models (Lucas [1972b] 1981a, Fischer 1977, Phelps and Taylor 1977, Taylor 1979, and Lucas 1989) was robust, irrespective of the specific assumptions of each model. Lucas’ 1989 discussion of robustness anticipated some points that would be addressed by economic philosophers much later (see e.g. Kuorikoski, Lehtinen and Marchionni 2010). Another, related issue addressed by Lucas ([1989] 2013a) was whether such models’ parameters were “structural”, that is, invariant to policy changes.

All those models, according to Lucas ([1989] 2013a), shared the “important virtue” of “descriptive realism” of their respective fundamental assumptions about sources of price rigidity: people “really” signed nominal contracts, people “really” had incomplete information about the economy, and people “really” fixed nominal prices in advance for long periods of time. As discussed in section 4 below, that was the first time Lucas explicitly deployed the notion of “descriptive realism” as a desirable feature of economic hypotheses.

. Unlike his first reactions to the economic contracts literature – when he criticized the absence of a microfounded account of why individuals should choose to sign restrictive contracts, and rejected the treatment of contract lengths as “free parameters” (Lucas [1980] 1981a; 1981b) – by 1989 Lucas came to regard the existence of contracts as a fact whose existence did not require justification by a fully fledged optimization model. The same applied to Lucas’ ([1972b] 1981a) assumption of imperfect information about the current money supply and price level, which had been under fire throughout the 1980s as contra-factual and ad hoc.

Lucas' ([1989] 2013a) support of descriptive realism as an ingredient of economic theories adds a new dimension to discussions of the role (or lack thereof) of realism in his economic methodology, usually organized around his 1977 essay on the foundations of business cycle theory, and, especially, his notion of economic models as “analogue systems” advanced in his 1980 “Methods and problems” article (see Vercelli 1991, chapter 8; Hoover 1995: 33-36; Boumans 1997: 74-77; Backhouse and Boianovsky 2013: 97-98; De Vroey 2016, chapter 10; Ingrao 2018: 119-22). More recently, Galbács (2020, chapter 4) has argued for the relevance of “causal realism” in Lucas’ macroeconomics, which is not incompatible with Lucas’ 1989 endorsement of descriptive realism. Lucas’ sense of descriptive realism built on his previous ([1980] 1981a) notion of economic models as artificial systems that should be able to mimic reality in its key aspects. From that perspective, Lucas’ later attention to descriptive realism may be regarded a new twist that broadens what Tobin called Lucas’ “methodological wisdom”.

2. Rejecting the “automatic auctioneer”

Lucas’ methodological breakthrough performed in his 1972 “Expectations and the neutrality of money”, as it is generally recognized, consisted in the introduction of a new concept of equilibrium, announced at the outset of the paper.

The substantive results developed below are based on a concept of equilibrium which is, I believe, new ... and which may be of independent interest. In this paper, equilibrium prices and quantities will be characterized mathematically as *functions* defined on the space of possible states of the economy, which are

in turn characterized as finite dimensional vectors. This characterization permits a treatment of the relation of information to expectations which is in some ways much more satisfactory than it is possible with conventional adaptive expectations hypotheses. (Lucas [1972b] 1981: 67)

Lucas ([1972b] 1981: 72-79) formally characterized equilibrium as a fixed point in the mapping from beliefs to laws of motion of the state variables. The expectations function described how prices depend on exogenous disturbances or shocks, whereas the pricing function depicted how actual prices are affected by those shocks. The solution to the functional equation – formed by the mapping of the expectation function into the space of pricing functions – is a fixed point where the expectation function and the price function accord, which has been labelled “rational expectations equilibrium”, in Muth’s (1961) sense (see e.g. Svensson 1996: 5; Hall 1996: 36).¹

Lucas’ new concept of equilibrium prices as functions of the set of states of the world in stochastic equilibrium opened the possibility to investigate economic systems that were in equilibrium and yet featured business cycles driven by random shocks, such as changes in money supply. Moreover, it did not necessarily mean “competitive equilibrium” (see Backhouse and Boianovsky 2013: 96-97). Lucas made that clear in another, less formal definition of his equilibrium concept.

[E]quilibrium in this (now entirely standard) context obviously does not refer to a system ‘at rest’, nor does it necessarily mean ‘competitive’ equilibrium in the sense of price taking agents, nor does it have any connection with social

¹ Lucas (1983) published a correction to his 1972b article, acknowledging that, on the basis of correspondence with Jean-Michel Grandmont, the uniqueness of rational equilibrium was not established in that overlapping generations model, unless very strong assumptions were made. Multiple equilibria in rational expectations OLG models would become a main feature of sunspots business cycles formulated by Grandmont (1985) and others (see Cherrier and Saïdi 2018).

optimality properties of any kind. All it *does* mean is that, in the model, the objectives of each agent and the situation he faces are made explicit, that each agent is doing the best he can in light of the actions taken by others, and that these actions taken together are technologically feasible. (Lucas 1987: 15-16)

Lucas' criticism of the notion of economic equilibrium as a state of "rest" was addressed to the equilibrium concept he ascribed to static general equilibrium theory as sorted by Samuelson (1947, part I), borrowed from physics (Lucas [1980] 1981a: 278). That differed from the expectational concept of equilibrium as an outcome that conforms to the expectations of economic agents. Surely, economists had investigated equilibrium as an expectational concept long before Lucas, but with no formal analysis of equilibrium in terms of optimal forecasts conditional upon particular information sets (see Phelps 1987).

Samuelson's (1947) distinction between microeconomic optimization and macroeconomic dynamic problems was a main feature of the neoclassical synthesis which prevailed until Lucas' ([1972b] 1981a) article (see Boianovsky 2020). It contrasted with Lucas' dynamic optimization methodology based on his new concept of stochastic contingent-claim equilibrium. Samuelson's *Foundations* was divided into Part I on optimization-based problems, and Part II on dynamics, with a distinct mathematical apparatus in each case. Differential and difference equations were the mathematical tools deployed for stability and dynamic analyses. Samuelson (1947: 5) claimed that meaningful operational propositions in economics originated from two classes of hypotheses. The first assumed that the conditions of equilibrium for economic agents are equivalent to maximization. "When we leave single economic units", however, the determination of "unknowns is found to be unrelated to an

extreme position”. The “dynamical properties of the system are specified, and the hypothesis is made that the system is in ‘stable’ equilibrium or motion” (ibid).

In Samuelson’s dynamic realm, equilibrium was related to stability instead of optimum (second-order) conditions. That was the function of Samuelson’s well-known “Correspondence Principle” between comparative statics and dynamics, which restricted the values of the parameters of a system by assuming dynamic stability. Samuelson’s models of the “Keynesian system” of income determination and of Walrasian general equilibrium alike dealt with the “interaction between individuals”, not with optimizing action “within an economic unit” (Samuelson 1941, p. 98; 1947, pp. 138, 258, 351). Aggregate behaviour could be neither understood as the result of maximization or “extremum” problems nor “converted into this form” (1947, p. 138). That methodological stance became central to macroeconomics during the long period of Samuelson’s “neoclassical synthesis” dominance.

Critical reactions to aspects of Samuelson’s research program of dynamic stability analysis started to appear even before the publication of the *Foundations*, especially by Don Patinkin and John Hicks. But it was only in the 1970s that criticism led by Lucas reached the core of that program. As put by Chari (1998: 174), the pre-Lucasian generations deployed macroeconomic models rationalized as positions of static general equilibrium together with “tacked on dynamics representing slow wage and price adjustment to shocks”, in a way that “the parameters describing the speed of adjustment were not derived from maximizing behaviour.” That was illustrated by Samuelson’s (1947: 263) differential equation of price adjustment described by excess demand, regarded, together with his stability analysis, as a first attempted formalization of Walrasian tâtonnement analysis (see Patinkin 1965: 540).

Donald Gordon and Allan Hynes (1970: 371-73) would sharply criticize Samuelson's disequilibrium price dynamics in their contribution to the influential Phelps volume, as Lucas ([1972b] 1981a: 292, n. 4) noticed. Whereas the properties of static demand and supply functions are derived from maximization, Samuelson's dynamic properties are not deduced as the "maximizing response of economic units to changing data", Gordon and Hynes pointed out. Samuelson left unexplained who is the economic unit whose behaviour is described by the equation of excess demand, unless he assumed the *deus ex machina* auctioneer with its ad hoc dynamics (Gordon and Hynes 1970: 371-72).

Gordon and Hynes' final, decisive remark referred to the role of information and anticipations in price dynamics. The hypothesis of stability of a function like Samuelson's price adjustment equation "makes little sense in a private market inhabited by maximizing traders", since such a market implies that given the initial price and excess demand level "the course of future prices is predictable" (Gordon and Hynes 1970: 372). But, in such situation, traders would exploit profit opportunities and "destroy the stability of the hypothetical differential equation", just like in the argument that stock market prices may be understood as a random walk (*ibid*). Interestingly enough, as Lucas ([1972b] 1981: 293, n. 4) observed, that point could be found in Samuelson (1965) already, although unrelated to price dynamics in general.

From Lucas' ([1980] 1981a: 278) perspective, Samuelson's synthesis consisted of the addition of "free parameters" – that is, parameters describing economic behaviour that are not derived from optimization – to a static general equilibrium neoclassical system, which allowed for a variety of Keynesian approaches to business cycles. Samuelson attempted to solve the "disparity" between parts 1 and

2 of the *Foundations* through his model of price dynamics, which “introduced sufficient additional ... free parameters to the equilibrium system.” But such parameters reflected past behaviour and would not remain stable when the system was exposed to shocks. Tobin (1983: 194) agreed that, without the constraints of maximization assumptions, the neoclassical-Keynesian synthesis expressed by Samuelson’s dynamic stability analysis contained an “embarrassing abundance of free parameters on whose values the model-builder has few clues”.

Lucas’ rejection of Walrasian tâtonnement and auctioneer mechanisms – as used in Samuelsonian type disequilibrium price dynamics – reflected in part the influence of Phelps’ (1969, 1970) incomplete information paradigm, as illustrated by Phelps’ famous islands metaphor borrowed by Lucas ([1972b] 1981a).² Lucas followed on Phelps’ (1969: 148) removal of the “Walrasian postulate of complete information”, crucially adding rational expectations to the model. Patinkin’s classic *Money, Interest, and Prices*, extensively read by Lucas in the early 1960s as graduate student, embodied the main attempt to deploy Samuelson’s tâtonnement formulation in monetary macroeconomics. Lucas (2004: 15) felt disappointed that “all the dynamics [in Patinkin 1965] are the mechanical auctioneer dynamics that Samuelson introduced, where *anything* can happen”. At the same time, Lucas (16) suggested that in the verbal discussion Patinkin (1965) tried to take into account expectations and intertemporal substitution problems. “What are people thinking? What are they expecting? He’s too good an economist to take the Samuelsonian dynamics literally.”

² Walrasian tâtonnement was also rejected at the time by a set of Keynesian authors (A. Leijonhufvud, R. Clower; R. Barro and H. Grossman, E. Malinvaud, J.-P. Benassy, among others) who established, together with D. Patinkin, “disequilibrium macroeconomics” as an alternative a research strategy. That was quite distinct from Lucas’ equilibrium macroeconomics (see Backhouse and Boianovsky 2013). Lucas (1987: 51-52) referred critically (if imprecisely) to that literature as “fix-price” models obtained by “dropping the assumption of market clearing while retaining all other aspects of the Walras auction scenario.”

A similar comment on Patinkin and the tâtonnement disequilibrium mechanism may be found in the concluding section of Lucas' Presidential Address to the American Economic Association, as Lucas (2003a) pointed to what he perceived as theoretical and methodological progress in macroeconomic dynamics since the 1960s. Patinkin (1965) tended to associate fluctuations in production and employment, caused by monetary shocks, to "disequilibrium adjustments unrelated to anyone's purposeful behaviour modelled with a vast number of free parameters." Lucas (2003a: 12) contrasted that with modern dynamic models subject to unpredictable shocks, featuring agents able to process information and to make intertemporal choices.

Such perspective on the history of monetary macroeconomics was also deployed in Lucas' Nobel lecture, while discussing responses to the "Hume paradox" of temporary real effects of monetary changes. Although economists (he mentioned Hume, Wicksell, Hayek and Patinkin) had resorted to disequilibrium dynamics for lack of better "analytical equipment", in their verbal accounts "the adjustment to a new equilibrium is not seen as a purely mechanical tâtonnement process, the character of which is determined by forces apart from the producers and consumers in the system", but as the outcome of the "actions people take as rational responses to their situations" (Lucas 1996: 669). Concerning his own 1972b imperfect information model, Lucas (1996: 676) pointed out that the output effects of monetary shocks could only take place by assuming away a "Walrasian centralized market" in the exchange of money for goods, as he did in the extension of Samuelson's (1958) overlapping generations model to a monetary economy.³

³ As pointed out by Wade Hands (2012: 110-111), Samuelson's neoclassical synthesis distinguished between the "behavior" of the Walrasian agent and the "behaviour" of the Walrasian auctioneer, with ensuing disequilibrium dynamics. Lucas, on the other

Lucas discussed the decision rules of the Walrasian “fictional” auctioneer (Lucas 1987: 16) in more detail in his 1986 article on convergence to rational expectations equilibrium through adaptive behaviour. Walras’ theoretical description of convergence to market equilibrium was centred on a fictional auctioneer who initiates the tâtonnement process by announcing a trial price, which will be modified until excess demand is zero and transactions take place at equilibrium prices. Under some conditions, this “adaptive process (though it is only the auctioneer who does any adapting) converges to the market clearing price“ (Lucas 1986: S413).

That differed, according to Lucas (1986), from modern experimental and theoretical models of adaptive convergence to market equilibrium (he referred to papers by Vernon Smith and Margaret Bray), where the task of adaptation is shifted from the auctioneer to “the same agents whose preferences determine equilibrium” (ibid). As pointed out by Lucas (2013a: xxiv), his 1972 *JET* article formalized the idea that, because individuals must choose production and consumption on the basis of imperfect information, this “could lead them to decisions that to an omniscient observer [that is, a Walrasian auctioneer] would appear pathological.” As Tobin (1980a: 796) observed, the auctioneer abstraction was good enough for the older generation of theorists, but not for a generation that is “erasing the distinction from equilibrium to disequilibrium and building an analogue economy with markets continuously clearing ... operated by human agents ... [using] scarce resources.”⁴

hand, modeled the equilibrium of the Walrasian market in the same way as the equilibrium of the Walrasian consumer, with permanent equilibrium of the price system and no tâtonnement adjustment. Hence, equilibrium in agents and markets meant essentially the same thing in Lucas’ economics.

⁴ Despite the textual evidence concerning Lucas’ misgivings about the Walrasian tâtonnement and auctioneer “automatic” or “mechanical” devices (see also the opening quotation at the outset of this paper), some commentators have argued that those concepts are central to Lucas’ framework (see e.g. De Vroey 2016: 157-58; 184; 352).

Lucas' ([1972b] 1981a) new approach to price stickiness gave up the hitherto dominant Samuelsonian differential equation of price adjustment – performed by a fictitious agent – as a reaction to excess demand. He replaced it by a model featuring information limitations faced by producers. Incomplete price reactions to monetary shocks (accompanied, therefore, by output changes) were explained by choices made by optimizing agents, which had implications on how to design policy formulation. As clarified by Bennett McCallum (1987), Lucas ([1972b] 1981a) put forward that strategy

[N]ot for aesthetic reasons, but in order to produce a model that would be well designed for the Keynesian objective of guiding macroeconomic policy. Such would not be possible with an algebraic adjustment equation, for the latter would give the analyst no basis for knowing whether the relation would itself shift if policy were substantially altered ... It is necessary, according to [Lucas'] view, to understand the *nature* of price sluggishness to know if its quantitative manifestation will remain intact in the face of altered conditions. (McCallum 1987: 127)

Such theoretical and methodological innovations would also affect other approaches to price stickiness and monetary non-neutrality, as discussed next.

3. Macroeconomics and the “technology” available to economists

Despite Lucas’ criticism of the Samuelsonian neoclassical synthesis, he was significantly influenced by Samuelson’s approach to economics as mathematical science. Lucas (2009) recalled how his reading of the first, microeconomic part of Samuelson’s *Foundations* in his graduate student days left a deep impression on him.

I loved the *Foundations*. Like so many others in my cohort, I internalized the view that if I couldn’t formulate a problem in economic theory mathematically, I didn’t know what I was doing. I came to the position that mathematical analysis is not one of many ways of doing economic theory: it is the only way. Economic theory *is* mathematical analysis. Everything else is just pictures and talk. (Lucas 2009: 279)

Samuelson’s (1947) mathematical formulation of general equilibrium theory in part I of the *Foundations* laid out the first way to construct “artificial systems” as analogues that provided imitations of the real economy, although focused on static equilibrium as a state of rest (Lucas [1980] 1981a: 277-78).

The economists’ ability to construct models of analogue economies is limited by the “available technology” formed by mathematical methods and computational capacity (Lucas [1980] 1981a: 272, 286). From Lucas’ perspective, Axel Leijonhufvud’s (1968) influential criticism – that “Keynesian economics” represented by IS-LM models à la Hicks and Modigliani failed to capture the essence of the “economics of Keynes” – missed the point that progress in economics means getting “better and better abstract, analogue economic models, not better verbal observations about the world” (Lucas [1980] 1981a: 276). Interestingly enough, Samuelson would

express the same criticism of what he perceived as Leijonhufvud's (1968) failure to "grasp the essence of scientific methodology". Keynes' insights and intuitions only became part of economic science after model-builders (such as Hicks, Tobin, Modigliani and Samuelson himself) succeeded in "extracting from their moon glow a prosaic, tractable, describable, testable and understandable model or paradigm" (Samuelson [1982] 1986: 859; see also Samuelson [1983] 1986: 277). Samuelson's ([1982] 1986: 859) claim that "science has to grapple with the tractable" is compatible with the overall role of tractability in Lucas' methodology.⁵

Samuelson's (1958) overlapping generation (OLG) model is a good example of improvement in the "technology" available to economists, as Lucas realized. Lucas ([1972b] 1981a) used that framework to implement Phelps' (1969, 1970) islands metaphor of imperfect information, in a formal rational expectations economy with money as the only asset (see Chari 1998: 176-77). In his careful restatement of the OLG foundations of the 1972 *JET* model in his Nobel lecture, Lucas (1996: 672-76) explained how he had chosen the OLG general equilibrium set up over Arrow-Debreu general equilibrium because it was better suited to investigate monetary issues.

The OLG framework was attractive because the demand for money as an asset resulted from optimizing rational behaviour. However, Lucas (1984) eventually – to the dismay of some (see Sargent 1996: 544) – became attracted to and formalized cash-in-advance-constraint monetary models, of the kind that had been introduced by Robert Clower in the 1960s.⁶ Although the finance constraint was often seen as ad hoc, Lucas (2013a: xviii) recalled how he "became dissatisfied with the abstractness

⁵ Samuelson had been since the late 1960s impressed by Peter Medawar's (1969) definition of science as the "art of the soluble", which influenced his treatment of tractability issues in economics (see Boianovsky 2022, section 2).

⁶ The Brazilian economist Mario H. Simonsen anticipated some main features of Clower's cash-in-advance model in the early 1960s. See Boianovsky 2002 and Walsh 2010: 100.

of the [OLG] framework and sought a view in which money is an asset that we hold to pay bills with, as a factor in a *payment system*.”

Lucas’ quest for less abstract models – able to capture essential features of the working of the economy – throughout the 1980s was not restricted to the cash-in-advance approach. Indeed, he would gradually become more open to non-competitive equilibrium rational expectation models. Non-competitive equilibrium was deemed empirically relevant but theoretically difficult to handle. As put by Lucas and Sargent ([1978] 1997: 284), “forms of [non-competitive] equilibrium can easily be imagined (if not so easily analysed).” The then new literature on economic contracts in goods and, especially, labour markets was a case in point, motivated by the “indisputable fact that there exist long-term labour contracts with horizons of two or three years” (ibid).⁷ The difficulty of constructing business cycle models based on non-competitive optimization was acknowledged.

The case for the use competitive theory in modelling business cycles would, if I were to develop it here, be based entirely on convenience, or on the limits imposed on us by available technology for working out the implications of other equilibrium definitions. (Lucas [1980] 1981a: 293, n. 11)

Hence, the available “technology” of mathematical economic modelling imposed limits not only on the old neoclassical synthesis, but on the new rational expectations equilibrium approach as well.⁸ Lucas then perceived non-competitive equilibrium models as not fully tractable (see also Hoover 2012: 50-51). Contract

⁷ As Lucas recollected, “the writing [of the 1978 paper with Sargent] was mine” (Hoover and Young 2011: 28).

⁸ As argued by Mäki and Marchionni (2012: 195), theoretical progress in economics comes from the interplay between beliefs about matters of causal fact and the “available research technology.” Hence, its is driven and constrained by both “ontological convictions and conventions about tractability.”

models could only be treated as equilibrium formulations if contract length emerged from a decision problem solved by economic agents, claimed Lucas ([1980] 1981a: 294, n. 14). Fischer (1977: 194, 202) acknowledged that his model did not provide a microeconomic foundation for the existence of long-term nominal contracts (apart from a passing reference to transaction costs), but claimed that in practice agents showed a “revealed preference” for such contracts.

However, Lucas’ initial qualified scepticism gave way to an emphasis on the similarities between his 1972 *JET* formulation and late 1970s contract models, which all shared a distinction between anticipated and non-anticipated money within a rational expectations framework. Fischer’s (1977) and others’ “Keynesian version of Lucas’ model” – as put by Collard and Dellas 2010: 483 – looked increasingly appealing to him. Lucas explained to Snowdon and Vane (1998: 127) how he had arrived at the anticipated/unanticipated difference through his 1972b information-processing model, but that others had reached the “same distinction by thinking about contracts.” In the 1970s he thought that his way of looking at that distinction was “just a lot better than other people’s ways of looking at it. Now [in 1998] they all seem pretty similar to me” (ibid).⁹

Lucas’ shift of attitude about the contract literature and rigid prices models in general reflected improvements in the “technology” available to economists – especially menu-costs (Caplin and Spulber 1987) and imperfect competition (Blanchard and Kiyotaki 1987) models, both positively cited by Lucas 2003b. It also resulted from changes in aspects of Lucas’ methodology (discussed further in section

⁹ In a letter of March 20 1990 to Lars Svensson (held in the Lucas Papers, Duke University; quoted from Duarte 2012: 226, n. 39), Lucas recalled how, by the early 1980s, he was still very “hostile to the idea of pre-set prices.”

4 below), and from his reaction to the dismissal – by real business cycle theorists – of Lucas ([1972b] 1981a) and other monetary approaches to economic fluctuations.

Lucas' 1985 Yrjö Janhsson lectures (Lucas 1987) may be regarded as the turning point in his treatment of contract models (with reference to Taylor 1979) as broadly belonging to the same research program (or “class of models”) as his monetary misperceptions model, with no mention of shortcomings regarding the determination of contract lengths or their existence – unlike Lucas' ([1980] 1981a; 1981) previous brief discussions. Lucas (1987) was mainly critical of the ability of real business cycle theory (as led by Kydland and Prescott 1982) to account for business cycle purely as optimal reactions to unpredicted real, non-monetary shocks (see especially Lucas 1987: 70-71). That was followed by other critical assessments of that approach. For instance,

Since Kydland and Prescott's surprising (1982) demonstration that productivity shocks with realistic statistical properties can account for *all* real output variability in the post World War II U.S. economy, the need for an adequate theory of monetary sources of instability has come to seem much less pressing. This important finding ... is an “ R^2 ” finding that does not bear directly on the size of the money multiplier. Nothing in the recent volume of real business cycle shows, or even suggests, that a sudden monetary contraction would have negligible output and employment effects, and that monetary policy is therefore of little real importance. (Lucas [1989] 2013a: 296-97)

Years later, Lucas (2007: 9) reasserted his “substantial dissatisfaction with the results so far achieved” by Kydland and Prescott's real business cycle research program.

Instead of establishing that monetary shocks were not relevant to explain economic fluctuations in the American economy, real business cycle econometric results (which Lucas did not dispute) should be seen as evidence that monetary policy had been efficient in achieving relative economic stability in the post-war period (up to the 2008 crisis) (see Lucas 1994: 13; 2003a: 11; 2004: 23). Hence, econometric tests of alternative business cycle theories (via calibration or other methods) should be interpreted with care. Empirical performance, from Lucas' standpoint, was not the main criterion to select among alternative models (see Galbács 2020, chapter 4).¹⁰

As it is clear from Lucas' letter to Prescott, by 1990 he treated the monetary misperceptions and the contract approaches as substantially equivalent in their implications. Lucas' first "encounter" with contract models took place in October 1978, when he presented his "After Keynesian macroeconomics" joint-paper at a conference sponsored by the Federal Reserve Bank of Boston. Lucas (2009: 295) recalled the success of his presentation, and how, in the question period that followed it, people called for him to "denounce work by John Taylor and Stan Fischer that, like some of my own work, attempted to account for real effects of monetary instability." Lucas felt then that his preference for his own approach "was not really defensible empirically". On the basis of the available evidence, "I did not see how it was possible to distinguish between my views and Taylor's."

¹⁰ De Vroey's (2016: 262) assertion that Lucas "came to endorse Kydland and Prescott's" real business cycle model is not accurate. In a letter of October 22 1990 to Prescott, Lucas stated that "I don't agree with your remark that 'persistence' is a difficulty with monetary-shocks business cycle theories. Monetary shocks must work because people react to them as if they were taste/technology shocks, because they can't tell the difference or because they are locked in to certain decisions. If so, then *any theory of the persistence of the consequences of actual tastes/technology shocks (like yours and Finn's) should be adaptable without change to monetary shocks*" (quoted from Duarte 2012: 201; original in the Lucas Papers, Box 26, David M. Rubenstein Rare Book and Manuscripts Library, Duke University).

Lucas (1987) eventually came to the position that the central issue for a theory of nominal price rigidity was not the nature of the game (competitive or not) agents are engaged in, but rather “the *information* agents are assumed to have about the state of the system at each date” when observing prices or signing contracts contingent on the relevant public information (Lucas 1987: 94). Economic agents in his 1972 *JET* model, he claimed, behaved “as if they were subject to ‘money illusion’” à la Modigliani (1944), or “as if they had signed contracts agreeing to behave as though they had money illusion” as in Taylor (1979) (Lucas 1987: 99). Lucas’ deployment of the “as if” clause should not be read in Milton Friedman’s (1953) sense of unrealistic behavioural assumption in order to make accurate predictions, but as an argument for the generality of Lucas’ own approach vis-à-vis alternative ways to account for price stickiness – even though Lucas (1987: 90) found Modigliani’s (1944) treatment of wage rigidity wanting.

Lucas (1996: 677) would repeat in his Nobel lecture the view that his imperfect information model, and others that assumed “non-competitive trading games” (such as the contract literature represented by Fischer 1977, Phelps-Taylor 1977 and Taylor 1979), reached the same general conclusions – despite controversies surrounding the so-called monetary policy ineffectiveness proposition – regarding the real effects of unanticipated monetary shocks. Contract models had been since the late 1970s popular, not just among New Keynesian economists, but also among some leading monetarists such as Milton Friedman, who rejected the New Classical market clearing assumption and whose natural rate of unemployment hypothesis had a strong influence on Lucas ([1972b] 1981a: 67) (see Nelson 2020: 312-16).

Dynamic stochastic general equilibrium (DSGE) models, of the kind introduced by Lucas ([1972b] 1981a) and further elaborated by Kydland and Prescott

(1982), were based on the double notion that business cycles, as described by the behaviour of co-movements among time series, “*are all alike*”, with a “*recurrent*” pattern (Lucas [1977] 1981a: 218, 224). This warranted the application to business cycle analysis of Muth’s (1961) identification of individuals’ subjective probabilities with the observed frequencies (“true” probabilities), named “rational expectations.” The latter concept is not applicable, as Lucas ([1977] 1981a: 223) pointed out, to situations when it is not possible to guess which observable frequency is relevant – that is, situations of Knightian *uncertainty* (Knight 1921). It was only when Knightian *risk* prevailed that the hypothesis of rational behaviour, as explainable by economic theory, made sense, as Lucas clarified. In particular, agents may then be assumed to react to cyclical fluctuations as “risk”, with rational expectations formed in a stable environment of information gathering and processing.¹¹

From that perspective, the “technology” available to economists posed a hurdle to business cycle modelling in general and to the investigation of the real effects of monetary instabilities in particular, especially under situations of serious discontinuities associated to uncertainty and its unexplained “residue”.

The problem is that the new theories, the theories embedded in general equilibrium dynamics of the sort that we know how to use pretty well now – there is a residue of things they don’t let us think about. They don’t let us think about the U.S. experience in the 1930s or about financial crises and their real consequences in Asia and Latin America. They don’t let us think, I don’t think, very well about Japan in the 1990s. We may be disillusioned with the

¹¹ As suggested by Hoover (1988: 227), Lucas’ endorsement of Knight’s distinction led to the view that, if the model was fully specified, uncertainty reflected a “residual” which cannot be economically analyzed. However, such a residual may be economically relevant, as discussed below. Keynes too, of course, distinguished between uncertainty and risk; see Gerrard 1994 on Lucas and Keynes on uncertainty.

Keynesian apparatus for thinking about these things, but it doesn't mean that this replacement apparatus can do it either. (Lucas 2004: 23)

Lucas would often refer to the Great Depression as outside the methodological boundaries of the dynamic stochastic equilibrium models that evolved from his 1972 *JET* article, while largely subscribing to the historical account by Friedman and Schwartz (1963) of that massive event as the result of a large monetary shock (see e.g. Lucas [1980] 1981a: 273, 277, 282, 284; 1994).¹² Nevertheless, there remained a substantial body of economic research demanding methodological clarification within the realm of rational expectations models and on how to design monetary policy to stabilize the business cycle.

4. Robustness and classes of models

While introducing his new model of prices set in advance, Lucas ([1989] 2013a) felt necessary to explain why yet another model – further to his 1972 *JET* article and to the 1970s contracts literature – of price stickiness with similar monetary non-neutrality results was warranted. Models of monetary economies depended on “assumed conventions” about how economies function in the absence of complete

¹² See also Laidler (2010). Likewise, the 2008 financial crisis was outside the scope of Lucasian macroeconomics, as he acknowledged while mentioning how those events “swept away” some of the premises of his approach to business cycles (Lucas 2013a: xvii). Lucas eventually found Cass and Shell's (1983) “sunspots” approach (self-fulfilling prophecies and multiple equilibrium) helpful in explaining bank panics, after showing skepticism about that literature in the 1980s and 1990s (Lucas and Stokey 2011; see Cherrier and Saïd 2018).

markets. Since such conventions are highly specific in relation to the actual trade practices,

[M]onetary theories can give the impression of basing important conclusions on slender, arbitrary reeds. I think this impression is exactly wrong, that the main implications of theories that attribute real effects to monetary causes by means of some form of price rigidity are largely independent of the way the rigidity is modelled or motivated. The present paper provides additional support for this opinion by offering an example of a rigid-price economy that is very different from earlier ones but very similar in its implication. (Lucas [1989] 2013a: 272-73)

The key question, therefore, was:

Do the various rigid price models have enough in common to have useful empirical or policy implications, or does everything hinges on the accuracy of the assumptions used in constructing each specific example? (Lucas [1989] 2013a: 296)

Although Lucas did not actually deploy the term, he was clearly making a new, general point about the *robustness* of economic models, as Martin Eichenbaum (1991: 625) and Sargent (2015: 59) observed. That was related to the concern about qualitative economic conclusions that emerge only under very specific assumptions, which raised the need to undertake “*theoretical* fragility analysis” (Eichenbaum, *ibid*). Lucas ([1989] 2013a) claimed that the class of rigid-price models considered in his paper produced substantive implications that were quite robust, in the sense of offering *general* principles to guide quantitative policy formulation despite specific,

particular assumptions for each model. As remarked by Sargent (2015: 59) – who found Lucas’ claim of robustness of predictions across classes of models a “tall order” – such a “tension” is visible throughout Lucas’ (2013a) collection.

Lucas deployed in 1989 a variety of robustness analysis later dubbed “derivational robustness” by James Woodward (2006), in the sense of the robustness of a theoretical conclusion with respect to distinct modelling assumptions. The stability of a theoretical result, under different determinations, provides epistemic support for the theory, since a “result is more likely to be real or reliable if a number of different and mutually independent routes lead to the same conclusion” (Kuorikoski, Lehtinen and Marchionni 2010: 544). The recognition of diverse kinds of assumptions plays an important role in this process.

Each of the models of price rigidity and monetary unpredictability examined in Lucas’ 1989 paper focused on a specific source of the “crucial rigidity”: nominal contracting (Fischer 1977; Phelps & Taylor 1977; Taylor 1979), incomplete information about the current state of the economy (Lucas [1972b] 1981a), and a game that compels sellers of goods to commit in advance to nominal prices (Lucas [1989] 2013a). As Lucas pointed out, his new model of price rigidity was similar to 1970s contracting models. Significantly, he acknowledged that “in common with these authors, I offer no explanation beyond an appeal to *descriptive realism* for assumption that prices are pre-set ... I will however spell out in detail the maximum problems” faced by producers in their price setting decision, and the nature of the game played (Lucas [1989] 2013a: 273, italics added). Gone was the requirement that such assumptions should be deduced from first principles. Lucas’ resort to “descriptive realism” introduced a new element in his methodological framework. All the assumed sources of price rigidity had

[T]he important virtue of descriptive realism: people really do sign contracts, people do have seriously incomplete information about the state of the economy in general and the quantity of money (and where it is located) in particular, people really do put dollar prices on the goods they sell and live with these pricing decisions for non-negligible time periods. (Lucas [1908] 2013a: 296)

Lucas would again call attention to “descriptive realism” as a desirable feature of economic models in general in Lucas & Moll (2014: 6). The expression may be found as well in his Nobel lecture, while observing that one cannot choose among alternative models of price rigidity “on the basis of descriptive realism” (Lucas 1996: 678). All those models put forward alternative versions of a function $\psi(x)$ relating labour supply and output decisions to the monetary transfer x , with the same conclusion regarding the real effects of unanticipated money transfers. However, “none of these models deduces the function ψ from assumptions on technology and preferences alone” (Lucas 1996: 678). That function depended on those factors, but also on specific assumptions about the players’ strategies, the way information is revealed, etc. Moreover, “these specifics are all, for the sake of tractability, highly unrealistic and stylized” (ibid).

Lucas ([1980] 1981a: 271-72) had advanced, in a couple of often-cited passages, the notion that models are “artificial economic systems” that “mimic” or “imitate” reality. Such “analogue systems” differ sharply from the real world. They serve as laboratories in the sense that they are tested by comparing the way they react to shocks with how real-world economies react to the same shocks. More “realism” in a model is clearly preferable to less” (ibid), interpreted as the ability of the model

to account for the factors determining the investigated variables (see also Hoover 1995: 34). Unlike Friedman (1953), the main methodological requirement was not the ex-ante predictive ability of the hypotheses (often built on unrealistic assumptions), but the analogue system's power of mimicking or reproducing the ex-post trends of the historical time series when subject to shocks (see also Ingraio 2018: 119-21; Galbács 2020, chap. 4).

Lucas' methodological stance, as revealed in his 1980, 1989 and 1996 papers, is better appreciated if one distinguishes between substantial assumptions, Galilean assumptions and tractability assumptions, along the lines suggested by Kuorikoski, Lehtinen and Marchionni (2010: 547). Substantial assumptions identify the causal mechanism investigated by the model-builder; such mechanism is supposed to work in reality. Galilean assumptions (a term borrowed from Nancy Cartwright) serve to isolate the core causal mechanism by "idealising away the influence of the confounding factors"; they are typically unrealistic. Finally, (mathematical) tractability assumptions are also generally unrealistic, but their falsehood is hoped to be irrelevant to the conclusions of the model. It is the failure of robustness in respect with tractability assumptions that is "epistemically problematic" as it indicates that the models' results originate from the specific array of tractability assumptions. Robustness analysis should protect against the unknown effects of unrealistic (tractability) assumptions, provided the substantial assumptions are empirically realistic.

Lucas' ([1989] 2013a) reference to descriptively realistic assumptions points to the notion of "substantial assumptions" seen above. This does not contradict his 1980 notion of models as artificial systems. Model building requires "unrealistic and stylized" tractability assumptions, as expressed by Lucas (1996: 678). He claimed that

rational expectations price-stickiness models, as a “class” of models, had useful implications that were robust in respect with specific assumptions made for each model. Lucas’ claim that his 1972 assumption – that agents face misinformation about current money supply and price level – is descriptively realistic has been controversial. Barro (1981: 74), like many others, argued that such an assumption of incomplete contemporaneous knowledge of monetary aggregates was questionable, since they would presumably “be observed cheaply and rapidly if such information were important.” Barro found a parallel in the use of adjustment costs to explain sticky wages and prices in “Keynesian” models, which were also vulnerable on a priori grounds. Lucas was aware of such criticism, but maintained that if idiosyncratic relative prices shocks were dominant for individual wealth, and if collecting information on aggregates involved any costs – no matter how small – individuals could rationally decide to ignore aggregate variables in their decision making (see Lucas [1977] 1981a: 230; 1987: 98).¹³

Much of the macroeconomics produced from the mid 1970s to late 1980s (and beyond), as Lucas ([1989] 2013a: 297) pointed out, stressed *differences* in the implications of various rational expectations models of price rigidity for monetary policy “activism” or “irrelevance”/“ineffectiveness”. Aggregate demand shifts that become anticipated after prices are set in the first period will affect output in models of predetermined prices. Lucas, however, found that emphasis “false”. From his standpoint, the main result, common to the whole class of models of price rigidity, was that any information about aggregate demand every agent had had a chance to

¹³ See also Galbács’ (2020, chap. 4) argument, that Lucas’ point about the costs of gathering information indicated the “realism” of his monetary misperception hypothesis and illustrated how Lucas’ abstract monetary models attempted to render the causal, “real” behaviour of agents. Interestingly enough, Blanchard & Fischer (1989: 367) and other New Keynesian authors found relevant affinities between Lucas’ notion of small information costs and the menu costs approach to price setting.

respond to – at it became known sufficiently far in advance – had no effect on output.¹⁴ Moreover, models in that class justified “activist policy” in the “trivial sense” that a central bank with information unavailable to private agents could affect positively resource allocation and welfare (ibid). However, the implications for the formulation of rules of monetary policy were not necessarily the same. The conclusion of Lucas ([1972b] 1981a), concerning perfectly smooth growth of money supply as the best policy, did not carry to contracting models if non-monetary shocks were taken into account (Lucas 1994: 11).

As Lucas pointed out in his Nobel lecture, such class of models (encompassing his own and the contracting literature) did not deduce the real effects of monetary shocks from preferences and technology alone. “Consequently, *we have no reason to believe that the function ψ is invariant under changes in monetary policy* – it is just a kind of Phillips curve, after all” (Lucas 1996: 678; italics added). Free parameters were an ingredient of economic-model building, especially when alternative equilibrium conceptions (competitive and non-competitive) were involved. Selecting one equilibrium concept entailed the fixing of a free parameter. Anyway, models with a couple of free parameters represented significant progress over models with many of them (Lucas ([1980] 1981a: notes 11 and 14). The parameters of the function ψ are not structural, as Lucas remarked in 1989. As put by Sargent (2015: 52), Lucas’ endeavour to develop theories of price rigidity was a “grim and difficult task yielding outcomes of qualified applicability.” That was “not for the faint of

¹⁴ Interestingly enough, Fischer (1987: 648) claimed that the main implication of Lucas’ ([1972b] 1981a) supply function – that only unanticipated changes in the money stock had real effects – “is shared by sticky wage theories ... and turns out not to distinguish” Lucas’ macroeconomics “from other approaches.” But that was exactly Lucas’ point in his 1989 paper. Indeed, the aggregate supply function derived from contracts models is the same as Lucas’ supply function, even though the mechanisms are different (see Blanchard & Fischer 1989: 519; Mankiw 2010: 386).

heart”, who were likely to be scarred off by arguments not fully deduced from first principles, warned Sargent (2015: 50, 52). This is the passage that caught Sargent’s critical attention:

But is a money multiplier [the slope of the Phillips curve] a structural parameter? No, of course it isn’t. One purpose of models such as those in this paper is to understand the ways in which changes in policy affect this multiplier, but even to do this one needs to take as fixed other parameters – the length of the period over which prices are fixed, say, or the length of information lags or labour contracts – which must in fact react to sufficiently large changes in policy ... I think if we are to use economic theory to improve monetary policy and institutions, we are just going to have to get used to this. (Lucas [1989] 2013a: 298)

This – together with a similar passage from Lucas (1996: 678) quoted above – suggests a pragmatic approach to the Lucas Critique (Lucas [1976] 1981a) (cf. Hoover 2006: 147). New Keynesian DSGE models have been recently criticized on the grounds of parameter instability (of the kind mentioned by Lucas in 1989) caused by lack of optimizing microfoundations, which would render them vulnerable to the Lucas Critique (see the historical survey by Sergi 2018). However, instead of stressing individual optimization as a way to deduce structural parameters and avoid policy invariance, Lucas ([1989] 2013a; 1996) pointed to the role of descriptive realism and to the use of models as devices to identify the sources of non-invariance.

5. Epilogue

Lucas' 1972 *JET* article represented the first effective challenge to Samuelson's neoclassical synthesis methodological separation between static microeconomic optimization and macroeconomic dynamics. Samuelson's disequilibrium price dynamics – as expressed by the Walrasian tâtonnement and auctioneer mechanisms – was a main target of Lucas' research program. The new treatment of equilibrium as an expectational concept, determined by the rational behaviour of information processing agents, was not restricted to market clearing competitive economies.

From a general perspective, as put by McCallum (2002: 90), Lucas' ([1972b] 1981a) quite abstract article was not empty of empirical content, in the sense that “its theorizing was specifically designed to rationalize a set of broad facts that were (and are) of genuine, fundamental importance.” Such facts involved the real effects of monetary changes, due to price rigidity (or stickiness) explained by misinformation. It gradually became clear to Lucas that other models of price rigidity – such as contracting, pre-set prices and menu costs models – provided alternative rationalizations of monetary non-neutrality, with similar implications.

Business cycle modelling was restricted, like economics in general, by the “technology” available to economists. Rational expectations general equilibrium dynamic models significantly advanced that technology. However, the domain of application of economic theories should be specified as well – rational expectations models strictly applied, as Lucas pointed out, to situations of risk instead of uncertainty (in Knight's sense). The recurrent pattern of most business cycle fluctuations, Lucas claimed, warranted the relevance of his rational expectations models, as well as others that evolved from his 1972 original formulation. Lucas'

1996 Nobel lecture was largely a reflection about his 1972 paper from the double, intertwined perspectives of the history of economics and of economic methodology, with references to other contributions that shared similar conclusions about expectations and the non-neutrality of money. But he was never fully satisfied with the offered solutions to the price stickiness problem.¹⁵

Lucas eventually stressed descriptive realism as an important methodological feature of economic models. That came out as he provided further – to his 1972 misperceptions hypothesis – contributions to account for price stickiness. The substantial assumptions behind each model of monetary non-neutrality were regarded as descriptively realistic, even if the models featured unrealistic assumptions in part for tractability reasons. Lucas' sense of descriptive realistic assumptions played a role in his 1989 argument about the robustness of different rational expectations models of price stickiness – his main contribution to methodology after his 1980 *JMCB* article. It is also implicit in his formalization of the cash-in-advance approach to monetary economics, which has been criticized for lacking an axiomatic microfounded justification for the – supposedly realistic – assumption of a finance constraint. Lucas defended his work on cash-in-advance economies on the grounds that

¹⁵ Lucas (1996) must have been one of the very few Nobel lectures featuring passages with self-criticism. For instance, Lucas (1996: 676) asked: “*Why is it* that people cannot obtain that last bit of information that would enable them to diagnose price movements accurately? In reality, up-to-date information on the money supply does not seem at all that hard to come by.” Moreover, though the evidence indicated that monetary surprises had real effects, “they do not seem to be transmitted through prices surprises” as in Lucas ([1972b] 1981a) (Lucas 1996: 679). Lucas' frustration with the substantive results – as opposed to the methodological contributions – of his misperceptions model increased over the years. By 2011 he commented on how his 1972 article did not succeed in providing a positive theory about the observations called Phillips curve. “So we had what we call price stickiness, which seems to be central to the way the system works. I thought my [1972] model was going to explain price stickiness and it didn't. So we're still on working on it; somebody's working on it” (Hoover and Young 2011: 34).

Ultimately, the merits of a particular approach to the theory of money (as to the theory of anything else) will be judged less by its axioms than by whether it seems capable of giving reliable answers to the substantive questions that lead us to be interested in monetary theory in the first place. (Lucas 1984: 40-41)

The assumption of delays in the discontinuous adjustment of prices and wages has been often treated as an “institutional fact” in macroeconomists’ investigations of the effects of systematic monetary policy (see e.g. Woodford 2003a: 7), close to Lucas’ 1989 descriptively realistic assumptions. Moreover, as put by Woodford (*ibid*), echoing Lucas ([1980] 1981a), “rather than postulating that prices or wages respond mechanically to some measure of market disequilibrium, they are set optimally, that is, according to the information available at the time they are set.”

After a period of extended criticism – due in part to the persistence issue, that is, the fact that the incomplete adjustment of prices depends almost totally on the degree of informational incompleteness – misinformation theories of the kind suggested by Phelps (1969, 1970) and modelled by Lucas ([1972b] 1981a) made a comeback after the early 21st century. That is largely explained by technological progress in making models such as Townsend (1983) – which dealt with the problem of agents’ need to form forecasts about each others’ beliefs – computationally tractable, and in introducing new game-theoretic analyses of equilibrium under heterogeneous beliefs (see Hellwig 2010 for a brief survey). Models stressing information-processing constraints (e.g. Mankiw and Reis 2002; Woodford 2003b) have examined the effects on price setting of the limited ability of individuals to absorb and process information that is widely available. From this broad perspective,

the imperfect information paradigm formally advanced by Lucas ([1972b] 1981a) – already noticeable in some basic insights of the business cycle literature from mid 19th to early 20th centuries (see Boianovsky 2021) – has continued to influence macroeconomics at both theoretical and methodological levels.

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