



FEDERAL RESERVE BANK  
OF MINNEAPOLIS

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## **A Toast to a Legend**

V. V. Chari

## **Hearing Robert E. Lucas, Jr.**

Thomas J. Sargent



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In September 2022, we organized, at the Federal Reserve Bank of Minneapolis, a conference to celebrate the 50th anniversary of the publication of “Expectations and the Neutrality of Money,” by Robert E. Lucas Jr. V.V. Chari prepared a speech, which he delivered during the conference dinner. Tom Sargent, in his Keynote lecture, presented “Hearing Robert E. Lucas Jr.” This article is published as it was submitted by Tom Sargent.

In our permanent effort to honor our infinite indebtedness to Bob for everything he gave us, and with the aim of sharing these wonderful writings to as many economists as possible, we decided to publish them in this issue.

# Hearing Robert E. Lucas, Jr.

Thomas J. Sargent  
Professor of Economics  
New York University  
and Hoover Institution, Stanford University

## 1 Introduction

Bob Lucas spoke and wrote so well that after reading one of his papers, I still can't try to write anything myself for days or weeks. So when asked to say something about Bob's work at the September 2022 Minneapolis Federal Reserve Conference celebrating the 50th anniversary of the publication of Lucas (1972) my approach was to say as little as possible myself and just to read some of Bob's words.

With a few additions, this paper is essentially a transcript of what I read at the September 2022 conference. Most of these are drawn from my review (Sargent 2015) of Bob's collected works on monetary economics (Lucas 2012). I isolate my comments by putting Bob's words mostly in quotes.

### Unemployed mathematicians

"Klamer:

So what do you call the many mathematicians who are now working as taxi drivers?

Lucas:

Taxi drivers."

—Interview with Robert E. Lucas, Jr. (Klamer 1983)

### Social Experiments

"Social experiments on the grand scale may be instructive and admirable, but they are best admired at a distance. The idea, if the marginal social product of economics is positive, must be to gain some confidence that the component parts of the program are in some sense reliable prior to running it at the expense of our neighbors." (Lucas 1981, p. 81)

## “The Death of Keynesian Economics”

“But what do we mean by ‘managing’ an economy? Prior to Keynes, ‘managing’ was taken to involve a good deal of governmental intervention at the individual market level—socialism in Russia, fascism in Italy and Germany, the confusion of early New Deal programs in the United States. It meant a fundamental shift away from market allocation and towards centralized direction. The central message of Keynes was that there existed a middle ground between these extremes of socialism and laissez faire capitalism. (Actually, there is some confusion as to what Keynes really said—largely Keynes’s own fault. Did you ever actually try to read the General Theory? I am giving you Keynes as interpreted by Alvin Hansen and Paul Samuelson.) It is true (Keynes argued) that an economy cannot be left to its own devices, but *all* we need to do to manage it is to manipulate the general level of fiscal and monetary policy. If this is done right, all that elegant 19th century economics will be valid and individual markets can be left to take care of themselves. . . . These were hard times, and this was too good a deal to pass up. We took it. So did society as a whole. (Conservatives were a little grumpy, but how bad off could we be in a country where Paul Samuelson is viewed as a leftist?)” (Lucas 2012, p. 502)

“This middle ground is dead. Not because people don’t *like* the middle ground any more but because its intellectual rationale has eroded to the point where it is no longer serviceable. . . . The problem in a nutshell was that the Keynes-Samuelson view involved *two* distinct, mutually inconsistent theoretical explanations of the determinants of employment.” (Lucas 2012, p. 502)

“My contributions to monetary theory have been in incorporating the quantity theory of money into modern, explicitly dynamic modeling . . . . When Don Patinkin gave his *Money, Interest, and Prices* the subtitle ‘An Integration of Monetary and Value Theory,’ value theory meant, to him, a purely static theory of general equilibrium. Fluctuations in production and employment, due to monetary disturbances or to shocks of any other kind, were viewed as inducing disequilibrium adjustments, unrelated to anyone’s purposeful behavior, modeled with vast numbers of free parameters. For us, today, value theory refers to models of dynamic economies subject to unpredictable shocks, populated by agents who are good at processing information and making choices over time. [Such] macroeconomic research . . . makes essential use of value theory in this modern sense: formulating explicit models, computing solutions, comparing their behavior quantitatively to observed time series and other data sets.” (Lucas 2012, p. xxvi, p. 465)

## A Phillips curve that is not exploitable

“[There is a] tension between two incompatible ideas—that changes in money are neutral units changes and that they induce movements in employment and production in the same direction— [that] has been at the center of monetary

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theory at least since Hume wrote. . . . The fact is this is just too difficult a problem for an economist equipped with only verbal methods, even someone of Hume’s remarkable powers. . . . The theoretical equipment we have for sharpening and addressing such questions has been vastly improved since Hume’s day.” (Lucas 2012, p. 378)

Lucas’s 1972 paper put rational expectations to work in a general equilibrium context for one of the first times, applying Muth’s idea of a rational expectations equilibrium not to a simple textbook cobweb cycle example but instead to one of the most pressing macro issues of the day, what James Tobin had called the “Cruel Choice” between inflation and unemployment. In the economy described in Lucas (1972), there is no cruel choice.

### **Finance and monetary theory**

“If it is easier today than in 1960 to identify exactly in which respects the theory of finance fails as a monetary theory, this is largely due to rapid progress in the theory of finance. Theoretical research in finance is now conducted almost entirely within the contingent-claim framework introduced by Arrow (1964) and Debreu (1959). This is not an historical statement, for each of the three pillars of modern financial theory—portfolio theory, the Modigliani-Miller Theorem, and the theory of efficient markets—was discovered within different (and mutually distinct) theoretical frameworks, but all three have since been reformulated in contingent-claims terms, and it was this reformulation that revealed their essential unity and set the stage for many further theoretical advances.” (Lucas 2012, p. 161)

“Ultimately, however, financial and monetary theory have quite different objectives, and however desirable theoretical ‘unity’ may be, one can identify strong forces that will continue to pull apart these two bodies of theory. . . . The empirical failures of the simplest ‘representative consumer’ models indicate that increased generality is required to produce success in the sense of first-order conditions that can pass the modern descendants of the efficiency test of finance. Such generality is not difficult to obtain, and I expect much additional fruitful work in this direction. The objective of designing simulatable models, an objective central to monetary theory, necessarily pulls in the opposite direction. . . . If I am right that the relationship between financial and monetary economics is not, even ideally, one of ‘unity,’ it is nevertheless surely the case that there is much to be gained by close interaction. The power in applications of the contingent-claim point of view, so clearly evident in finance, will be as usefully applied to monetary theory.” (Lucas 2012, p. 189-190)

### **Monetary shocks versus real business cycle theory**

“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 a theory of monetary

sources of instability has come to seem much less pressing. This important finding has been buttressed by much subsequent research, but it 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 research 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 2012, p. 296-297)

“One may thus think of the [Kydland-Prescott real business cycle] model not as a positive theory suited to all historical time periods but as a normative benchmark providing a good approximation to events when monetary policy is conducted well and a bad approximation when it is not. Viewed in this way, the theory’s relative success in accounting for postwar experience can be interpreted as evidence that postwar monetary policy has resulted in near-efficient behavior, not as evidence that money doesn’t matter. Indeed, the discipline of real business cycle theory has made it more difficult to defend real alternatives to a monetary account of the 1930s than it was 30 years ago. It would be a term-paper size exercise, for example, to work out the possible effects of the 1930 Smoot-Hawley Tariff in a suitably adapted real business cycle model. By now, we have accumulated enough quantitative experience with such models to be sure that the aggregate effects of such a policy (in an economy with a 5% foreign trade sector before the Act and perhaps a percentage point less after) would be trivial.” (Lucas 2012, p. 371)

## Lucas on deep monetary theory

“Applied theory is always a mixture of rigor and compromise. . . . 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. . . . Successful applied science is done at many levels, sometimes close to its foundations, sometimes far away from them or without them altogether. . . . The analysis of sustained inflation illustrates this observation, I think: Though monetary theory notoriously lacks a generally accepted ‘microeconomic foundation,’ the quantity theory of money has attained considerable empirical success as a positive theory of inflation.” (Lucas 2012, p. xxvii, p. 191, p. 427-428)

### 1.1 *Regulating credit and money*

What features of the economic environment account for the cash-in-advance constraint? (Neil Wallace lurks in the shadows here.) Lucas says that legal regulations on intermediation can or maybe should give rise to cash-in-advance constraints.

“The question ‘What is Money?’ becomes, then, the question of what we want to *make into* money via government restrictions of various kinds on the

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operation of the private banking system.” He calls for analyzing the merits of such restrictions partly by comparing the “poor business cycle experience of those economies with relatively unregulated banking with . . . [economies] (such as ours) in which institutions providing transactions-effective services are sharply differentiated by legal restrictions [under the Glass Steagall Act] that necessarily oppose the competitive forces working to blur these restrictions.” (Lucas 2012, p. 84)

(Lucas wrote this long before U.S. financial deregulation in the 1990s.) Lucas also wrote

“The question we face now is not whether there is some ‘natural’ reason to treat *M1* as an interesting number but whether we want to enforce an ‘unnatural’ situation that will make it interesting.” (Lucas 2012, p. 84)

### Wealth distribution and portfolios

In his mid two country model with cash-in-advance constraints (Lucas 1982), Lucas emphasized the role of assumptions he makes about the initial distribution of wealth across people in the two countries:

“The fact that, in equilibrium, all traders in the world hold the identical market portfolio is a simplification that is absolutely crucial to the mode of analysis used above. It is also grossly at variance with what we know about the spatial distribution of portfolios; . . . Why is this? . . . A real answer must have something to do with the local nature of the information people have, but it is difficult to think of models that even make a beginning on understanding this issue. It is encouraging that the theory of finance has obtained theories of securities price behavior that do very well empirically based on this common portfolio assumption, even though their predictions on portfolio composition are as badly off as those of this paper.” (Lucas 2012, p. 114-115)

### Sticky prices and money multipliers

“I do not see how [the question of the appropriate conduct of monetary policy] can be resolved without better theories of price rigidity than we now have available to us.” (Lucas 2012, p. 368)

Lucas warned that this is going to be a grim and difficult task yielding outcomes of qualified applicability:

“Is a money multiplier a structural parameter? No, of course it isn’t. One purpose of models such as those in [chapter 12 (of Lucas 2012)] is to understand the ways in which changes in policy parameters 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 labor contracts—which must in fact react to sufficiently large changes in policy. . . .

A money multiplier is *never* going to be recognized by the American Kennel Club. 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 2012, p. 298)

### **Rational expectations and time consistency**

“... A discrepancy between the best future tax policies to announce today and the best policy actually to execute when the future arrives is precisely what is meant by time-inconsistency.” “[Time-inconsistency of government policy] arises, more generally, whenever the private sector must first commit itself to a current decision on the basis of its beliefs about a future action taken by government, and then, with this commitment made, the government is free to select this future action.” (Lucas 2012, p. 119-120, p. 205)

If left unchallenged, Kydland and Prescott’s conclusion snuffed out any practical significance to be attached to the Ramsey plans for optimal monetary and fiscal policy constructed in key papers by Lucas and Lucas and Stokey.

“Since the normative advice to a society to follow a specific ‘optimal’ policy is operational only if that policy might conceivably be carried out over time under the political institutions within which that society operates, the Kydland-Prescott paper calls into serious question the applicability of all dynamic adaptations of the Ramsey framework.” (Lucas 2012, p. 119)

One needs to appeal to *something* in order to restore practical interest to optimal plans calculated at time 0 in the face of the conclusions presented by Kydland and Prescott (1977). Lucas describes systems of beliefs that serve to weaken the temptations to deviate from a time-inconsistent optimal plan that arise under a sequential timing protocol:

“In common with written constitutions, each of these disciplines can be amended or evaded, an observation that has led to some skepticism about the usefulness of trying to bind economic policy at all. What is the ‘discipline’ of a monetary standard if the government always has the option to devalue? This is a difficult question, I think, because it is a poor response to conclude that since the effectiveness of such disciplines is hard to measure, they are unimportant forces. Certainly there are innumerable episodes in U.S. history where disciplines like these appear to have been, for better or worse, binding constraints on policy.” (Lucas 2012, p. 211)

### **Rational expectations econometrics**

“[brought] ... a degree of empirical stringency without precedent in economic research.” (Lucas 2012, p. 170)

“From the point of view of classical hypothesis testing, nothing is gained in restricting attention to models that have solutions or solutions that can be characterized or simulated. If a [particular] first-order condition ... is tested and



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rejected, one can view as rejected all models carrying this equality as an implication, without having to spell out each model or verify its internal consistency. Since there is no doubt that with rich enough data sets any such condition will be rejected, a research program based on purely negative application of first-order conditions has, in a sense, inexhaustible possibilities. Yet I think it is clear that pursuit of this line is at best a useful adjunct in the effort to obtain simulateable, necessarily 'false' models that have the potential for shedding light on the questions that lead us to be interested in monetary theory in the first place." (Lucas 2012, p. 171-172)

**Robustness of predictions across classes of models**

Despite the fact that outcomes in models with frictions depend sensitively on many details, Lucas nevertheless wanted general principles that can guide quantitative policy advice. My reading of a message from the menu-cost literature is that this is a tall order. It is better to listen to Bob Lucas struggle with the issue than it is to hear me second guess him:

"To 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. . . . Models of monetary economies necessarily depend on assumed conventions about the way business is conducted in the absence of complete markets, about who does what, when, and what information he has when he does it. Such conventions are necessarily highly specific, relative to the enormous variety of trading practices we observe, so monetary theories can give the impression of basing important conclusions on slender, arbitrary reeds. I think that 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 modeled or motivated. . . . [We now have a] list of theoretical examples that illustrate possible mechanisms through which monetary instability may induce inefficient fluctuations in economic activity. [In these examples] . . . it is only unanticipated movements in money that are predicted to result in inefficient levels of production and consumption. Each of these models that trace real pathologies to a combination of rigid prices and monetary unpredictability focuses on one specific source of the crucial rigidity: nominal contracting (Fischer (1977), Phelps and Taylor (1977)), incomplete information about the current state of the system (Lucas (1972)), a game that obliges sellers of goods to commit in advance to nominal prices (the present paper). All of these assumed sources of price rigidity have the important virtue of descriptive realism: people really do sign nominal contracts, people really 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. All of the models we have that incorporate any one of these facts have the common implication that unanticipated monetary shocks have non-neutral,

multiplier effects that are quite different in character from the real distortions that result from anticipated inflations.” (Lucas 2012, p. 272-273, p. 295-296)

“We have a wide variety of theories that reconcile long-run monetary neutrality with a short-run trade-off. They all . . . carry the implication that anticipated money changes will not stimulate production and that at least some unanticipated changes can do so. Does it matter which of these rationales is appealed to? The answer to this harder question must depend on what our purposes are. Any of these models leads to the distinction between anticipated and unanticipated changes in money, the distinction that seems to me the central lesson of the theoretical work of the 1970s. On the other hand, none of these models deduces the function  $\phi$  [relating production to the money growth rate] from assumptions on technology and preferences alone. Of course,  $\phi$  depends on such factors, but it also depends on the specific assumptions one makes about the strategies available to the players, the timing of moves, the way in which information is revealed, and so on. Moreover, these specifics are all, for the sake of tractability, highly unrealistic and stylized: we cannot choose among them on the basis of descriptive realism. Consequently, we have no reason to believe that the function  $\phi$  is invariant under changes in monetary policy—it is just a kind of Phillips curve, after all—and no reliable way to break it down into well-understood components.” (Lucas 2012, p. 393)

## Policy lessons

Lucas taught that “our interest in models . . . is whether their solutions can be constructed and characterized, given assumed behavior for the various shocks to the system,” an interest determined by the purpose and structure of modern macroeconomic models, from which “the main lessons, are first, the futility of trying to assess policy changes in terms other than changes in policy processes and, second, the impossibility of analyzing changes in monetary and fiscal processes independently of each other.” (Lucas 2012, p. 180, p. 183)

Lucas wanted microeconomic foundations for practical reasons. Prescott and Lucas’s calibration project aspired to use microeconomic studies to gather empirically credible values of key parameters governing preferences and technologies to import into a quantitative macroeconomic model: “This is the point of ‘microeconomic foundations’ of macroeconomic models: to discover parameterizations that have interpretations in terms of specific aspects of preferences or of technology, so that the broadest range of evidence can be brought to bear on their magnitudes and their stability under various possible conditions.” (Lucas 1987, p. 46).

Lucas stressed the discipline and coherence enforced by general equilibrium models:

“It would not be useful for me simply to run through the various writings of these and other economists, taking one principle here and another one there: Major differences in the analytical frameworks they used would make it impossible to see which principles are mutually consistent and which contradictory, and it would be impossible to tell, at the end, whether we had arrived at a complete

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characterization of an efficient monetary and fiscal policy or only a partial one.”  
(Lucas 2012, p. 193-194)

Lucas played by the rule that it takes a model to beat a model, and also by the rule that it takes an equilibrium model to pose a macroeconomic policy problem properly:

“It may be that some day we will have an operational theory of business cycles that suggests additional, useful principles besides those I have discussed [in Chapter 9, Principles of Fiscal and Monetary Policy]. In the meantime, it seems sensible to me to take policy guidance from models we can actually understand and work through, not from models we wish we had, or models other people think we have.” (Lucas 2012, p. 209-210)

Lucas eloquently explained how general equilibrium reasoning *matters*.

“The great disciplining virtue of applied welfare economics is that it forces one to take a position on all of the issues involved in constructing a quantitatively serious general equilibrium model of the entire economy. . . . *everything* must be faced. In a monetary application especially, this can be a humbling experience because it lays bare the many really basic issues on which we are far from a solidly-based understanding.” (Lucas 2012, p. 81)

### Improving macroeconomics

“I see . . . progressive element in economics as entirely technical: better mathematics, better data, better data-processing methods, better statistical methods, better computational methods. . . . learning how to do what Hume and Smith and Ricardo wanted to do, only better: more empirically founded, more powerful solution methods.” (Lucas 2012, p. 513)

“Macroeconomics was born as a distinct field in the 1940s, as a part of the intellectual response to the Great Depression. The term then referred to the body of knowledge and expertise that we hoped would prevent the recurrence of that economic disaster. . . . Macroeconomics in this original sense has succeeded: Its central problem of depression prevention has been solved, for all practical purposes, and has in fact been solved for many decades. There remain important gains in welfare from better fiscal policies, but I argue that these are gains from providing people with better incentives to work and to save, not from better fine-tuning of spending flows.” (Lucas 2012, p. 445)

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