

KEYNES AFTER LUCAS

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"The extraordinary achievement of the classical theory was to overcome the beliefs of the 'natural man' and, at the same time, to be wrong." —John Maynard Keynes

I come here neither to praise Keynes nor to bury him, but to ruminate about where Keynesian economics is going in view of what it has been through. The last 10-15 years have been a period of intellectual ferment in macroeconomics. Old beliefs have been questioned, old models discarded. Many creative and imaginative—some might say fanciful—new ideas have come to the fore. Many clever people have toiled long hours in the macro vineyards seeking to develop new, more critique-resistant strains of theory and econometrics. Many new techniques have been added to our toolkit. To be the very model of a modern macroeconomist today, your technical baggage must be much heavier than was true a decade or two ago. Yet do we know much more today than we did then about how the macroeconomy really works? I am not convinced.

Marx said that thesis and antithesis lead to synthesis. But, as we know, Marx was not always right. There is today a strong consensus that there is no macroeconomic consensus. So, if you came to hear the new post-Lucas consensus, you will be disappointed. Instead, I want to talk about resource allocation within our subdiscipline. I want to ask whether or not the prodigious amounts of labor and capital devoted to macroeconomic research since 1972 have been allocated correctly. Or has there instead been a colossal market failure? There are those, I know, who will assert that macroeconomic research must be efficient since the market is competitive, there are no government regulations, no distorting taxes, and macroeconomists rationally pursue their own self-interest. I remind them that both externalities and rational speculative bubbles can exist, and proceed.

I. The Keynesian Consensus as of 1972

This section summarizes the Keynesian consensus as it existed in 1972, before Bob Lucas and company mounted their spirited attack:¹ an IS/LM model plus an expectations-augmented Phillips curve, with expectations modeled as adaptive. Since the model is so familiar, I will be brief.

IS and LM: Private aggregate demand came from a consumption function based on the work of M. Friedman [30] or Ando and Modigliani [6] and an investment function based on the work of Jorgenson [45]. While expectations of future variables play important roles in the

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¹Actually, one of Lucas' important papers on rational expectations [53] was written before 1972. But that paper was neither well known nor well understood.

theories underlying both functions, simple-minded empirical proxies for these expectations were accepted without much question in applied work.² The financial sector was summarized in an LM curve of the sort estimated by Goldfeld [34] which, in turn, was loosely based (such looseness was tolerated in those days!) on the Baumol-Tobin model of money demand.

Predetermined Price Level: Thinking that the ghost of Pigou had been laid to rest, few macroeconomists thought it necessary to defend the notion that labor and goods markets do not clear each "period"—even if the period was as long as a year. The macroeconomic servo-mechanism was thought to work much more slowly than this. Hence, it was natural to think of prices and wages as predetermined, which left IS and LM together to determine output in the short run.

The Phillips Curve: The price level evolved according to an expectations-augmented Phillips curve:

$$\dot{p} = E(\dot{p}) + f(y - y^*), \quad (1)$$

where \dot{p} is the inflation rate, y is (the log of) real output and y^* is the natural rate of output. I choose 1972 as my starting point because by then the vertical-in-the-long-run view of the Phillips curve, originally promoted by Phelps [63] and M. Friedman [31], had won the day.³ Causation in equation (1) was supposed to flow from GNP gaps to inflation. Most economists gave (1) a disequilibrium, price-adjustment interpretation, following Lipsey [52], although the Phelps-Friedman model could be interpreted as a market-clearing model subject to an informational lag. It was also taken as axiomatic that recessions were economic maladies; the idea that Okun gaps might be Pareto-optimal would have been thought eccentric.

Modeling Expectations: The expected rate of inflation needed in (1)—which also entered the model as the difference between the IS curve's real interest rate and the LM curve's nominal rate—was typically modeled as adaptive, or in some other distributed lag form. The idea that expectations lagged behind reality was rarely questioned and was, of course, central to the Phelps-Friedman Phillips curve mechanism.

View of Policy: Fiscal and monetary policies were typically viewed as exogenous (a word we thought meaningful in those days) and episodic, not the sort of thing that was suitable for forecasting by time series techniques developed for stationary stochastic processes. Despite this, there was some empirical literature on policy reaction functions, especially for monetary policy, and a well-developed approach to optimal policy in the Tinbergen-Theil framework.⁴ No one paid much attention to the ideas that the economy might react differently to anticipated versus unanticipated policy changes or that changes in the nature of policy might change the "structural" equations of the model.

Econometric Models: Finally, all of the above elements plus a hideous number of other details (some of them quite important) were embodied in highly complex, large-scale macroeconomic models which were estimated and identified by what I will call, as a shorthand, Cowles Commission techniques. No one will accuse me of understatement if I say that the theoretical structures of these models were not very tight.

II. The Consensus Crumbles: Can Anybody Spare a Paradigm?

The insurgents that sacked the Keynesian temple in the 1970s raised objections to every item on this list. But I think it fair to say that IS/LM was spared the heavy artillery, which

²However, Eisner's [22] insightful piece on the 1968 surcharge was an example of what later became known as the Lucas critique.

³See, for example, Gordon [37].

⁴Goldfeld and Blinder [36] provides a contemporary perspective on all of these matters, plus many references.

was aimed at the other items. In the New Classical view, prices moved quickly to clear markets; expectations about inflation, policy "rules," and everything else were rational; the Phillips curve offered no exploitable tradeoff; and existing econometric models were useless, or worse.

The Phillips Curve: Lucas' [56] original paper, of course, made the Phillips curve one of the central examples of his critique of econometric policy evaluation. And Lucas and Sargent [57] used the alleged collapse of the Phillips curve, which they characterized as "econometric failure on a grand scale" [57, 57], as the major piece of empirical evidence in their premature obituary for Keynesian economics. The Lucas supply function turned the Phillips curve on its head. When Lucas [54] changed equation (1) to:

$$y - y^* = g(\dot{p} - E(\dot{p})), \quad (2)$$

he gave it a market-clearing interpretation similar to Phelps-Friedman, and he meant the causation to run from expectational errors to GNP gaps. On the surface, the difference between (1) and (2) hardly looks revolutionary. Lucas's more important point was that neither the $g(\cdot)$ function nor the distributed lag proxies typically used to model $E(\dot{p})$ were likely to be stable; they would, instead, change whenever the time-series behavior of inflation changed. His suggested answer was to model expectations as "rational," that is, as consistent with the structure of the model.

Rational Expectations: It took a while, and some help from Fischer [23] and Phelps and Taylor [64], for the profession to get clear that rational expectations (RE) is an assumption about behavior which may be right or wrong but which is logically disconnected from the hypothesis that prices move instantly to clear markets. It is more from the latter than from the former that the New Classical Economics (NCE) derives its distinctive implications.

Separating those two ideas helped spread the RE gospel, since formal econometric tests of the joint hypothesis of RE and market clearing almost always rejected it.⁵ Most economists had a strong suspicion that the market-clearing hypothesis was the weak link in the partnership. Many, especially in the U.S., eagerly embraced rational expectations as the natural accompaniment to utility maximization and profit maximization, without bothering to ask for evidence. Fischer [26, 13] probably speaks for many when he states unequivocally that "rational expectations is the right initial hypothesis." This attitude, it seems to me, would not have appealed to Keynes, who wrote [46, 161-162]:

... a large proportion of our positive activities depend on spontaneous optimism rather than on a mathematical expectation ... Only a little more than an expedition to the South Pole, is it based on an exact calculation of benefits to come. Thus if animal spirits are dimmed and the spontaneous optimism falters, leaving us to depend on nothing but a mathematical expectation, enterprise will fade and die ...

Market Clearing: The separation of RE from market clearing probably doomed the latter because the hypothesis of instantaneous market clearing was wildly at variance with events that were unfolding as the RE revolution took hold. For example, since rational expectations implies that $\dot{p} - E(\dot{p})$ is white noise, equation (2) implies that output vibrates randomly around the natural rate. (More on this below.) Yet 1973-1975 witnessed the deepest recession since the 1930s, 1981-1982 eclipsed that standard, and, as this is written, unemployment has yet to return to the natural rate. How and why the market-clearing approach caught on in this environment will, I venture to guess, be a source of consternation and amazement to economists of the 21st century—much as modern physicians marvel at the 18th-century

⁵For a discussion of this, see Rotemberg [68].

belief in bleeding. It might have prompted Keynes, were he still alive, to add the prefix "neo" to the adjective "classical" and say:

The classical theorists resemble Euclidean geometers in a non-Euclidean world who, discovering that in experience straight lines apparently parallel often meet, rebuke the lines for not keeping straight—as the only remedy for the unfortunate collisions which are occurring. [46, 16]

Optimizing Behavior: Closely connected to the puzzling popularity of market-clearing models was a revival—I might almost say a fundamentalist revival—of the search for micro-foundations for macroeconomic models. That is always a worthy cause; but the NCE school pushed it to incredible extremes. It became *de rigueur* to derive everything from neoclassical first principles; all behavioral functions had to come from first-order conditions to simplistic optimization problems solved by representative agents. Realism was sacrificed to rigor, as internal consistency replaced consistency with observations as the principal criterion by which models were judged. Old ideas like corner solutions, heterogeneity, aggregation problems, and coordination failures were forgotten. New ideas that were spewing forth from the new microeconomics of imperfect information—which suggested many reasons why prices might not be able to clear markets—were ignored.

Econometric Models: The neoclassical revival, in concert with the Lucas critique, stripped large-scale econometric models of any academic respectability they ever had. These ugly, crassly empirical creatures, with their tenuous theoretical structures, looked pitifully old-fashioned at a time when styles were turning toward smaller, cuter, more tightly theoretical models. Lucas argued that the big models, which either treated expectations as adaptive or ignored them entirely, could not be used to analyze policy. That robbed them of their main purpose. Instead of tools for investigation, the models became butts of jokes. That was a shame, because only vector autoregressions (VARs) were offered in their place,⁶ and most economists (though not Sims [72]), thought VARs useless for policy analysis.

View of Policy: The Lucasian revolution also sought to change the standard view of government policy. Instead of seeing policy as exogenous and episodic, rational expectationists preferred to think of the government as following "policy rules" that were stable enough to be known (up to random error) by consumers and firms. And they took it as axiomatic that optimizing private agents would change their behavior whenever the policy rule changed. Some went even further to suggest that, since the government was already optimizing (Would that it were so!), it could not change its behavior at all.⁷

Several new issues became prominent. One was the difference between anticipated and unanticipated policy changes, especially for monetary policy. In one of the few empirical salvos fired by the New Classical counterrevolutionaries, Barro [8] claimed to have found support for the idea that only unanticipated money growth can move real output. Another new issue was the so-called time consistency problem: When would the government want to stick to a pre-announced rule and when would it want to renege?⁸

Persistence: While criticizing the Keynesian answers to every thing, the new macroeconomic paradigm created an intellectual puzzle where none existed before: why do economic fluctuations persist? Keynesians thought distributed-lag consumption, investment, and money demand functions; accelerator mechanisms in inventories, fixed investment, and consumer durables; gradual adjustment of prices and wages; and lags in expectations were

⁶I refer here to econometric methods for dealing with complete models. The "deep parameters" methodology, while useful for dealing with specific equations, never offered much hope for dealing with complete models.

⁷See Sargent [70] and my comment following.

⁸Kydland and Prescott [50].

more than enough to explain persistence. But all of these were banished by the new orthodoxy. Instead, we had equation (2), which insisted that GNP gaps had to be white noise if expectations were rational. Of course, Lucas [54] knew this to be untrue and so had appended the lagged GNP gap to (2) without much theoretical justification. Subsequently, explaining persistence became a minor growth industry. Sargent [69] appealed to adjustment costs, Lucas [55] to fixed capital, Blinder and Fischer [16] to inventories, Taylor [76] to staggered contracts, Kydland and Prescott [51] to technological lags. Each of these innovations, it seems to me, made the basic model look somewhat more Keynesian. And, once their implications were thought through, most provided a potential channel through which anticipated money might have real effects via changes in real interest rates.⁹

III. Elements of a Renaissance

In this fiftieth birthday year, Keynesian economics appears to be moving out of the dark ages into a renaissance. It has been victorious by default, I think, because the new classical economics failed miserably to meet the criterion for a Kuhnian paradigm change. Not only did it fail to explain any empirical phenomenon that baffled Keynesian analysts (and there were several), but it created anomalies (like persistence) where Keynesian economics offered coherent explanations.

Phillips Curve: As noted, the Phillips curve bore the brunt of the RE/NCE attack. But we now know that, once suitably modified to include "supply shocks," this empirical regularity stands up remarkably well.¹⁰ Indeed, its stability through the 1970s and 1980s is amazing in view of the troubles encountered by money demand, investment, and other functions.

Lucas Critique: Similarly, the Lucas critique, which at first was misinterpreted as a directive to throw the baby out with the bath water, has been put into perspective. While Lucas's conceptual point is valuable and indubitably correct, so are the well-known points that heteroskedastic or serially correlated disturbances lead to inefficient estimates and that simultaneity leads to inconsistent estimates. But we also understand that small amounts of serial correlation lead to small inefficiencies and that minor simultaneity leads to only minor inconsistencies; so suspected violations of the Gauss-Markov theorem do not stop applied econometrics in its tracks. In the same spirit, the realization is now dawning that the Lucas critique need not be a show stopper.¹¹ Indeed, evidence that it is typically important in applied work is lacking.

Rational Expectations: The hypothesis that expectations are rational, that is, consistent with the model, seems to have had more enduring success, and rightly so. Certainly, the previous unthinking loyalty to adaptive expectations is gone forever. Economists will, forevermore, worry about possible direct effects of policy on expectations. But, even here, there has been significant backsliding. Taylor [75], B. Friedman [29], and before them Muth [61] outlined circumstances under which adaptive expectations might be exactly or approximately rational. Frydman and Phelps [32], among others, have pointed to a fundamental difficulty with the assumption that expectations are model-consistent when people's beliefs may differ—a difficulty well appreciated by Keynes.¹² And it has become increasingly well-known that matching subjective probability distributions to objective ones is no trivial

⁹This is a major point of Blinder and Fischer [16], and is pointed out for the Lucas [55] model by Fischer [24]. Real interest rate effects are not emphasized in the other papers, but are implicitly present.

¹⁰Among many references that could be cited, see Gordon [38; 41], Ando and Kennickell [5], Perry [62], B. Friedman [28], Blanchard [12], and Fischer [25].

¹¹See, for example, Sims [72], Geweke [33], and Blinder [14].

¹²See the famous statement likening the stock market to a beauty contest in *The General Theory*, page 156.

matter when the objective distribution may not be stationary, nor even exist, because the events being forecast are not repetitive. For example, who would like to tell me the probability distribution of the real price of crude oil, or of the federal budget deficit, in the year 2000?

Market Clearing: The old-fashioned idea that goods and labor markets do not clear instantaneously is staging a strong comeback. How could it be otherwise, given the overwhelming evidence in its favor? Since economists are rarely moved by evidence that assaults the naked eye, it is important that rigorous econometric studies that test disequilibrium against equilibrium models on time series data almost always favor disequilibrium.¹³ It is also important that new developments in micro theory show how imperfect information gives rise to moral hazard and adverse selection problems that may keep prices, interest rates, and wages from performing their traditional role of clearing markets—even in the long run.

View of Policy: Barro's [8] "demonstration" that only unanticipated money has real effects did not hold up to the intense scrutiny it received. It was shown (a) to depend on an unreasonable identifying assumption that fiscal policy affects monetary policy but does not directly affect real GNP, (b) to depend on truncating the lag structure [59], and (c) to be dominated by a "Keynesian" alternative model [39]. However, the general notion that anticipated and unanticipated policy changes may have different effects seems both useful and enduring.¹⁴ The importance of other aspects of the RE view of policy—such as time consistency and the idea of conceptualizing stabilization policy as a rule or "regime" rather than as something exogenous and episodic—remain controversial.

IV. The Forgotten Agenda

Despite some important new ideas, the NCE counterrevolution does not seem to me to mark a major step forward from the Keynesian tradition it supplanted. The attempted revival of market clearing was Quixotic, in the worst sense of that word. The attack on the Phillips curve was scurrilous and without empirical foundation. Although the basic idea that expectations are not purely mechanical is certainly important, the wholesale adoption of rational expectations was dubious, even though the hypothesis is doubtless of considerable use in some places (such as financial markets). The renewed search for microfoundations was welcome, but the insistence on neoclassical purity probably did macroeconomics little good. Finally, the Lucas critique, while conceptually correct, is of unproven empirical significance.

To my mind, that does not add up to a major improvement over the macroeconomics of 1972. Does that mean we had it all right in 1972? Hardly. It may mean, instead, that the RE/NCE revolution concentrated its fire in the wrong directions, that our macroeconomic research resources have been misallocated. I want to close this paper by suggesting that that was indeed the case—that the most serious flaws in 1972 Keynesianism were not prominent on the NCE hit list.

I begin with a brief mention of an obvious flaw. Vintage 1972 macroeconomics—whether it was Keynesian or monetarist—was all about demand fluctuations, a term then thought to be synonymous with economic fluctuations. The 1970s and 1980s destroyed this narrow-minded focus forever. We now know that Marshall's celebrated scissors also comes in a giant economy size. Economic fluctuations can, and sometimes do, emanate from the supply side—from oil shocks, food shocks, and the like. Much theoretical and empirical work has been done on supply shocks in the last dozen years; there was no misallocation here.¹⁵ This work

¹³See Ashenfelter and Altonji [4], Altonji [3], Brown [19], Rosen and Quandt [65; 67] and Romer [66], among others.

¹⁴See, among the many references that could be cited, Turnovsky and Miller [78].

¹⁵See Bruno and Sachs [20] and many of the references cited there.

will have a lasting and salutary effect on macroeconomics. But it is basically orthogonal to the debate between Keynesians and New Classicals.

My main point, however, is different. It is that macroeconomics might be in better shape today if, instead of arguing interminably about clearing markets, the rationality of expectations, the Phillips curve, and the Lucas critique, economists had devoted more time to improving the theoretical and empirical foundations of IS/LM.

The LM Curve: The Hicks-Hansen LM curve was barely touched by the insurgents; most NCE models accepted this equation without question. Indeed, they often whittled it down to the quantity theory of money. But, of course, the demand function for money was crumbling in our hands all the while. The first velocity debacle, which prompted Goldfeld's [35] well-known search for the "missing money," was dwarfed in size and reversed in sign by the second velocity debacle—which may still be in progress. It is the demand for money, not the Phillips curve, that evidenced "econometric failure on a grand scale." And the reason seems to lie in institutional change, not in the Lucas critique.¹⁶

Keynesian economics now needs to pick up the pieces. The apparent disappearance of the demand function for M1 does not, as is sometimes thought, strip the economy of its nominal anchor nor render central bank policy impotent. It means, instead, that the theoretical and empirical foundations of the LM curve may need to be reexamined and perhaps broadened beyond the narrow confines of "money." One possible route is to distinguish between the underlying demand for transactions services and the derived demand for any particular monetary aggregate, recognizing that the latter may change whenever the menu of available monetary assets changes, just as the demand for coal changed when oil was discovered.¹⁷ Another possibility is to remember the "bond market" that Keynes suppressed and to explore the notion that alter-native financial variables, such as credit, may be the main channels through which financial events impact on the real economy.¹⁸ And there may be yet better ideas.

The IS Curve: The Lucas critique cast a dark shadow over the Jorgensonian practice of extrapolating current variables (like relative prices, interest rates, and tax rules) into the future and set in motion a search for an alternative investment model that coped better with unobservable expectations. Since the stock market is supposed to discount future expectations in just the right way, this led to a revival of the *Q*-theory of investment, originally due to Tobin [77].¹⁹ And that, in turn, led to a dead end because of the absurd volatility of the stock market [71] and the failure of empirical *Q* models to explain the data.²⁰ I do not purport to know where the investment function is going, but it needs to go somewhere. I note in passing that the simple accelerator model that was known and loved in the early 1970s—perhaps beefed up by rational expectations—seems to be making a strong comeback.²¹ Here the wave of the future may be the wave of the past.

The application of RE, rather than the adaptive expectations assumed by M. Friedman [30], to the permanent income hypothesis (PIH) of consumption was a useful idea that remains controversial. We have learned several things since Hall [42] first suggested that only "surprises" in income cause consumption to change. First, current income is, by far, the

¹⁶See Gordon [40], among others. Of course, it can be argued that the institutional changes were induced by previous (inflationary) policies. In addition, Lucas-style reasoning may not be entirely irrelevant to the breakdown in the money demand function. See Walsh [79].

¹⁷See Spindt [73] or Baba, Hendry, and Starr [7] for examples. The empirical importance of this idea is still subject to dispute.

¹⁸See, among others, Bernanke [1], Blinder and Stiglitz [18], and Blinder [13].

¹⁹See Abel [1], Blanchard [11], and Summers [74].

²⁰That is the opinion, for example, of Abel and Blanchard [2], both of whom were early *Q* enthusiasts.

²¹See Clark [21] and Abel and Blanchard [2].

major determinant of current spending—just as Keynes had asserted. That is not necessarily inconsistent with RE/PIH notions because innovations in income are mostly permanent, but it does shift the emphasis away from long-run average income and back to current income. Second, liquidity constraints of one sort or another probably play some role in the consumption function, which is, of course, another Keynesian idea.²² Third, the “excess sensitivity” results of Flavin [27] and others notwithstanding, there do seem to be some gains to decomposing income and wealth into anticipated and unanticipated components [15].

However, at a more fundamental level, the RE revolution, in pointing consumption theorists toward ever tighter theoretical formulations of the PIH, may have pointed in the wrong direction. Specifically, accumulating evidence suggests that the life-cycle PIH model, which looks so good in time series, may be quite wrong. I am referring here both to the indirect evidence that life-cycle accumulation may be small relative to inherited wealth²³ and to a variety of studies on longitudinal data which fail to detect the patterns predicted by life cycle theory.²⁴ These issues have not been fully sorted out yet, but it may be that entirely new approaches to the consumption function are called for. If so, it seems unlikely to me that stricter adherence to narrow-minded conceptions of maximizing behavior will play major roles in the rehabilitation of the consumption function. But perhaps my expectations are not rational.

V. In Conclusion

By the early 1950s, the Keynesian revolution was consolidated. The next 20 years or so were a productive time in which Keynesian ideas were developed further, modified in places, and given empirical content. New features, like the Phillips curve, were grafted on; and the entire apparatus was built into giant “realistic” models of the economy. Much, but not all, of that development stopped in the 1970s as macroeconomics turned introspective and nihilistic.

Some of the fundamental questions raised were good ones (Why should expectations be adaptive?), others were not (Does the labor market really clear every period?). But they did stop a constructive research agenda dead in its tracks. Some will say that was necessary, for the 1972 consensus was leading us astray. I am less convinced. I cannot help thinking that macroeconomics would be better off today if Lucas’s valid questions about how expectations were handled in theoretical and empirical models had redirected the Keynesian research agenda rather than derailed it. It may now be time to get the train back on the tracks.

²²Hall and Mishkin [43], Hayashi [44].

²³Kotlikoff and Summers [47]. See also Modigliani’s [60] critique.

²⁴Blinder, Gordon, and Wise [17], Kurz [49], Mirer [58].