Keynes, Lucas, and Scientific Progress

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In one of those marvelous coincidences of intellectual history, Robert Lucas was born the year after the publication of Keynes' *General Theory*. For the first thirty-five years of their mutual lives, the two apparently coexisted in harmony. But their relationship has been tumultuous ever since. Lucas has frequently criticized Keynesian economics as poor science; and it is precisely in that spirit that I want to address the debate today.

We all know the old joke about the professor who uses the same exam questions year after year, but changes the answers. That joke encapsulates all too well what has happened to macroeconomics these last fifteen years and seems to reflect poorly on economics as a science. Or does it? On second thought, the best answers to scientific questions *do* change as new observations are made, as new experiments are run, and as better theories are developed. The issue is whether the answers to important questions in macroeconomics have changed for good scientific reasons or for other reasons.

The joke provides the framework for my talk. I will pose eight exam questions; and for each one I will summarize the answers given by Keynes, by Lucas and his followers, and by modern Keynesians. I pick only questions that are answered differently by Keynesians and Lucasians and that are central to contemporary macroeconomic debates. The focus is on whether the Keynesian or new classical answers have greater claim to being "scientific." Each student must answer every question.

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I. Are Expectations Rational?

Keynes, though no stranger to probability theory, was nonetheless unequivocal in his denial that expectations are what we now call rational:

> ...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.... [1936, pp. 161–62]

That attitude left a big loose end in *The General Theory*. Business investment is supposedly driven by "the state of long-term expectations," but expectations are not pinned down by the theory, leaving substantial room for gyrations in macroeconomic activity driven by autonomous changes in animal spirits. That hardly constitutes a tight scientific theory; but Keynes was probably happy to leave the loose end loose. Modern "sunspot theorists" have tightened up the argument considerably, in ways that Keynes might have found congenial.

Lucas, of course, changes the answer to yes. Was this change motivated by empirical evidence that subjective expectations match the conditional expectations generated by models—or even that actual expectations are unbiased and efficient? No. Indeed, Edward Prescott has boldly asserted that "surveys cannot be used to test the rational expectations hypothesis" (1977, p. 3). Rather, economists are supposed to convert to rational expectations (RE) because of the unloveliness of the *ad hoc* expectational mechanisms that preceded it and because RE is more consistent with their (unverified) worldview that people always optimize at all margins. As Thomas Sargent put it: "Research in rational expectations...has a momentum of its own...that...stems from the logical structure of rational expectations as a modeling strategy" (1982, p. 382). The momentum, you will note, does not stem from empirics. I leave it to you to decide whether these criteria are more like those that led physicists to dump Newton in favor of Einstein, or those that led artists to abandon Manet to follow Picasso.

Modern Keynesians are split on this question. To some, the theoretical appeal of RE and the general idea that expectations should respond to policy changes are sufficient reason to conclude that "rational expectations is the right initial hypothesis." Others harbor doubts. I think the weight of the evidence—both from directly observed expectations and from indirect statistical tests of rationality (usually in conjunction with some other hypothesis)—is overwhelmingly against the RE hypothesis. Furthermore, RE is not without theoretical difficulties. We all know that RE models often have multiple equilibria. More fundamentally, RE is theoretically coherent only in the context of a single agreed-upon model. In an economy in which different people hold different views of the world, the very notion lacks clarity. For example, if Paul Volcker announces today that on New Year's Day he will raise $M1$ by 20 percent, I imagine Lucas and I will make different revisions in our expectations for, say, real GNP in 1987. Whose expectations are "rational?" Heterogeneous beliefs pose serious theoretical problems for RE. As scientists, then, I think we should be hesitant to embrace RE.

II. Is there Involuntary Unemployment?

Keynes said, nay screamed, yes. Lucas not only says no, but questions whether the phrase has meaning. In his words, "To explain why people allocate time to...unemployment we need to known why they prefer it to all other activities" (1986, p.38). Notice the words *allocate* and *prefer*. In his view, the unemployed are engaged in intelligent search or purposeful intertemporal substitution. He scoffs at the Keynesian tradition which, "by dogmatically insisting that unemployment be classified as 'involuntary'...simply cut itself off from serious thinking about the actual options unemployed people are faced with" (1986, p. 47).

This is a tough question to adjudicate on scientific grounds since the issue is largely definitional and, as Lewis Carroll pointed out, everyone is entitled to his own definitions. In Lucas's view, a person laid off from a job can, presumably, shine shoes in a railroad station or sell apples on a street corner. If he is not doing any of these things, he must be *choosing* not to do so. Both statements like this and reactions to them tend to be polemical. I guess dogmatism is in the ear of the beholder.

However, a few pertinent facts should leaven the ideological debate. First, when the unemployment rate rises, it is layoffs, not quits, that are rising while consumption falls rather than rises—all of which are bad news for search theory. Second, real wage movements are close to a random walk—which is bad news for the intertemporal substitution approach. Third, unemployment is heavily concentrated among the long-term unemployed; in 1985, for example, people who were jobless for 27 weeks or more constituted 54 percent of all unemployment and the expected duration of a complete spell of unemployment was 31 weeks. Can that be intertemporal substitution? Fourth, unemployed workers normally accept their first job offer, and those who are looking for work spend an average of only 4 hours per week on search activity. That hardly suggests a predominant role for search in explaining unemployment.

III. Do Wage Movements Quickly Clear the Labor Market?

Keynes certainly thought not, for such reasons as trade union aggressiveness, custom and inertia, and outright stubbornness. Lucas answers yes—though perhaps only in a broad sense. He has, for example, cited approvingly the competitive contract equilibrium approach in which workers have 100 percent unemployment insurance and, because of indivisibilities, are chosen randomly to work either, say, 40 hours a week or zero—meaning, of course, that unemployed workers have higher utility than employed ones. In Lucas's opinion, there is "no reason to believe" that competitive models of labor markets that treat unemployment like leisure commit "a serious strategic error."

No reason? I think the preponderance of the evidence says otherwise. Unemployment insurance replaces only about 40 percent of lost earnings. Lately, only about one-third of the unemployed collect it. Where is the evidence that the unemployed are happier than the employed? Most economists think Lucas's distinguished predecessor at the University of Chicago had it right when he wrote, "Under any conceivable institutional arrangements, and certainly those that now prevail in the United States, there is only a limited amount of flexibility in prices and wages." And it is hard, for me at least, to look at what has gone on in this country—not to mention in Europe—since 1974 and see clearing labor markets. That the market-clearing approach caught on in this environment is testimony to Lucas's keen intellect and profound influence, not to economists' respect for facts.

More than just casual empiricism supports this view; numerous formal econometric studies reject the market-clearing hypothesis against some sort of disequilibrium alternative. Unfortunately, it is usually spot-market clearing that is rejected. Equilibrium contracting models in which the wage plays little or no short-run allocative role are difficult to formulate econometrically, much less to reject. Indeed, it is hard to know what observations could contradict such models; theory just leaves too many open possibilities.

Nonetheless, certain observations are worth making. For one, several authors have pointed to interindustry wage differentials that are persistent across both time and space—differentials which are not easily squared with market clearing. Theoretically, we know that the wage rate may not be able to clear the labor market in a world of imperfect information—not even in the long run. Of course, that efficiency wage models can be built does not imply that they describe reality. But it does mean that market-clearing models have no particular claim to the theoretical high ground.

In sum, the scientific basis for modeling labor markets—or goods markets for that matter—as continuously clearing escapes me.

IV. Is the Natural Rate of Unemployment a Strong Attractor for the Actual Rate of Unemployment?

Keynes thought not. Indeed, in his revolutionary zeal, Keynes spoke loosely (loose talk was a problem for Keynes) of an "unemployment equilibrium"—which would seem to deny the natural rate any attractive force at all. Lucas answers in the affirmative.

Modern Keynesians have long had trouble with the master's notion that the economy could equilibrate below full employment; they prefer to think of unemployment as a long-lasting disequilibrium. In the United States at least, the validity of the natural rate hypothesis has not been at issue for a long time. The argument, instead, is over whether the speed of convergence to the natural rate is rapid or glacial.

On this, the American evidence is unequivocal and the European evidence is overwhelming. The U.S. civilian unemployment rate peaked at 8.9 percent in May 1975 and then took almost three years to get back down to 6 percent. It then peaked again at 10.7 percent in November-December 1982; now, four years later, it has yet to fall below 6.7 percent for even a single month. Some will argue that 7 percent is now the natural rate, without worrying much about how it grew so high. My view is that a theory that allows the natural rate to trundle along after the actual rate is not a natural rate theory at all.

In Europe, the evidence is far more compelling. Unemployment rates rose more or less steadily from 1974 to 1985—from 3 to over 13 percent in Britain, from 2.8 to 10.5 percent in France, and from 1.6 to 8 percent in Germany. Some young men in these countries have *never* held a job and may never be productive workers. Facts like these have prompted several authors to seek models which explicitly reject the natural rate hypothesis in favor of hysteresis. And recent econometric work suggests hysteresis in postwar U.S. real GNP as well. It may well be that Keynesians caved in too readily to the natural rate hypothesis.

V. Is there a Reliable Short-Run Philips Curve?

Keynes, of course, did not answer this question; the Phillips curve came later. I include it on the exam because Lucas and Sargent made it central to their attack on Keynesian economics. The alleged failure of the Phillips curve was their main piece of evidence that empirical Keynesian models "were wildly incorrect, and that the doctrine on which they were based is fundamentally flawed." (Please notice the adverbs.)

This charge was repeated so often and with such certitude that it became part of the conventional wisdom. Unfortunately, it is, to coin a phrase, wildly incorrect. The fact is that, the Lucas critique notwithstanding, the Phillips curve, once modified to allow for supply shocks (any one of several variables will do), has been one of the best-behaved empirical regularities in macroeconomics—much better behaved, in fact, than we had any right to expect. A long list of studies supports this conclusion. Nonetheless, Lucas continues to speak of the Phillips curve as an econometric basket case.

Let me anticipate the obvious objection that saving the Phillips curve after the fact by adding a supply variable does not absolve it of its *ex ante* forecasting errors. It is true that, while Robert Gordon's latest Phillips curves fit the data well, his pre-1972 equations do less well. And they did not predict the rise and fall of OPEC. But there is no sense in which new classical models either anticipated the error or pointed to the solution; like Keynesian models, they were designed to analyze demand shocks. Events in the 1970's and 1980's demonstrated to Keynesian and new Classical economists alike that Marshall's celebrated scissors also comes in a giant economy size. It is a debater's tactic, and a poor one at that, to claim that supply shocks are outside the purview of Keynesian economics.

VI. Does a Change in the Money Supply have Real Effects?

Keynes and the Keynesians answered yes, without bothering to distinguish between anticipated and unanticipated changes. Lucas and the Lucasians answer that money has real effects only if it is misperceived. In their view, a properly perceived injection of money is like a currency reform.

Here, again, the weight of the econometric evidence (though certainly not all of it) suggests that Keynes had the right answer after all. Robert Barro's alleged empirical demonstration that only unanticipated money has real effects did not hold up. Perceived changes in money are not neutral.

VII. Does Social Welfare Rise when Business Cycles are Limited?

Keynes tacitly, but unequivocally, answered yes. If asked for proof, he probably would have chuckled with the condescension of the British upper crust—which is hardly a scientific attitude.

Lucas is carefully agnostic, but clearly leans toward the answer no. He has long been sympathetic to the idea that successful stabilization policies that smooth business cycles may actually decrease welfare. Prescott is less circumspect. Without bothering to draw any distinction between modeling a conclusion and proving it, he asserts that "costly efforts at stabilization are likely to be counterproductive" because "economic fluctuations are optimal responses to uncertainty in the rate of technological change." Clearly, Harberger triangles look bigger and Okun gaps smaller near lakes than near oceans. Is Prescott's attitude more scientific than Keynes'?

I think it is worth taking a moment to explain why Lucas believes that the potential gains from stabilization policy are so small. The postwar standard deviation of log quarterly consumption around trend is about .013. Lucas asks an infinitely lived consumer living under perfect capital markets how much he would be willing to give up to reduce this small standard deviation to zero. Unsurprisingly, the answer comes back: not much. So Lucas concludes that "the post-war business cycle is just not a very important problem in terms of individual welfare." That is a stunning assertion, especially when juxtaposed against the conventional wisdom that governments rise and fall on the vicissitudes of the business cycle.

Lucas's conclusion, it seems to me, ignores a few pertinent facts. First, the cycle is not mainly in consumer expenditures, much less in consumption. Indeed, there is virtually no cycle at all in spending on nondurables and services. Are large swings in consumer durables, in inventories, and in fixed investment all socially costless? Don't these ups and downs impose serious adjustment costs and dislocations on society?

Second, Lucas's calculation assumes that cyclical fluctuations take place around an unchanged trend, with booms as likely as recessions. But what if recessions leave permanent scars on either labor or capital or productivity? What if there is hysteresis, so the natural rate hypothesis fails? What if there is a systematic tendency for output to be too low on average? Then the Keynesian goal of filling in troughs without shaving off peaks starts to make sense.

Third, Lucas ignores a variety of psychological, sociological, and physiological costs which many people feel are important. Against Lucas's benign view of the cycle compare the opinion of Martin Luther King, who wrote that "In our society, it is murder, psychologically, to deprive a man of a job or an income. You are in substance saying to that man that he has no right to exist." The truth, I think, lies somewhere between Lucas and King.

Finally, it is important to remember that cyclical losses are not distributed uniformly, as Lucas assumes; instead, most people lose little while a minority suffers much. Let me illustrate with some simple calculations. Suppose everyone has log utility and consumes \$3000 per quarter. Let a severe recession reduce consumption 4 percent. Utility falls 4.1 percent, which is no big deal, especially since every down is matched by a subsequent up. This is Lucas's world.

Now change utility to the Stone-Geary form: $U = \log(C - \$1500)$. Here a 4 percent drop in consumption reduces utility by 8.3 percent. That seems a bigger deal. Finally, let the cycle instead reduce the consumption of 10 percent of the population by 40 percent while the other 90 percent loses nothing. (Note that I am allowing very generous unemployment insurance here.) With the Stone-Geary utility function, mean utility declines 16.1 percent. Now we're talking real utils.

Lucas will, of course, counter that any such problem is best dealt with by better unemployment insurance, not by stabilization policies that interfere with free-market allocations. The same logic says that fire and theft insurance—where moral hazard problems are certainly less severe—obviate the need for fire and police departments. Isn't prevention better than insurance?

However, Lucas's challenge to the Keynesian presumption that smaller cycles are better cycles needs to be addressed scientifically. And, since we can't observe cyclical fluctuations in utility, that requires the use of theory. The relevant theory is, I think, just beginning to be developed in the burgeoning literature on monopolistic competition and aggregate demand externalities. It would be foolish to say that a definitive answer is in hand; but some good answers may be on the horizon.

VIII. Must Macroeconomics be Built Up from Neoclassical First Principles?

Keynes answered no. A practical man living in a complex world, he would not close his eyes to apparent deviations from narrow-minded concepts of optimizing behavior—nor even to gross deviations from rationality. He believed in modeling behavior as it was. Witness his defense of money illusion in labor supply:

> Now ordinary experience tells us... that a situation where labor stipulates...for a money-wage rather than a real wage, so far from being a mere possibility, is

> the normal case.... It is sometimes said that it would be illogical for labour to resist a reduction of money-wages but not to resist a reduction of real wages.... But, whether logical or illogical, experience shows that this is how labour in fact behaves. [p. 9]

Lucas and other new classicists take a different view. They emphasize the importance of building up macroeconomic relationships from sound microfoundations, by which they mean the solutions to dynamic, stochastic games. Lapses from what Lucas called "the only 'engine for the discovery of truth'" are one of the chief grounds on which Keynesianism is branded unscientific.

Now, neither side is hostile either to first principles or to factual accuracy. We all agree that the ideal macro theory would be built up logically from first principles and would explain the data well. But we also agree that such a theory is a long way off. The issue is how religiously we must adhere to frictionless neoclassical optimizing principles until that glorious day arrives. Here the devoutness of American economists distinguishes us from our colleagues in other lands. But which attitude leads to better science? Is it better to start deductively from axioms or inductively from facts? When the time comes to choose between internal consistency and consistency with observations, which side should we take? Must we be restricted to microfoundations that preclude the colossal market failures that created macroeconomics as a subdiscipline?

Here followers of Keynes and followers of Lucas often part company. Like Keynes, modern Keynesians are inclined to begin by "taking things as they are"; rigorous optimizing explanations for what they observe (such as nominal wage contracts) can come later. The important thing is to make sure our models are congruent with the facts. Lucasians, it seems to me, reverse the sequence. They want to begin with fully articulated, tractable models and worry later about realism and descriptive accuracy.

This is a judgment call; but I judge the Keynesian approach more scientific. First, good science need not always be built up from solid microfoundations. Thermodynamics and chemistry, for example, have done pretty well without much micro theory. Boyle's Law applies directly to aggregates, much like the marginal propensity to consume. And the microfoundations of medicine are often very poor; yet much of it works. Empirical regularities that are formulated and tested directly at the macro level *do* have a place in science.

Second, it is far from clear that the particular first principles selected by new classical economists deserve to come first. Why don't people know the money supply or the price level within very small margins of error? Who imposed a cash-in-advance constraint? Why should price move to equate supply and demand in markets with asymmetric information? Why, Keynes might ask, are these postulates more acceptable as first principles than nominal wage contracting?

Third, the model of man as a strongly rational maximizer is not the only option open to theorists. There are theories of "bounded rationality" and of "near rationality." Even within the strict optimizing framework, neoclassical tangencies are not the only, nor even the most likely, alternative. Pervasive lumpy transactions costs lead to "the optimality of usually doing nothing," meaning that it rarely pays to change your decision variable, even if it is not set at the frictionless "optimal" value. In a word, near rationality is full rationality. It is continuous optimization that would be irrational.

Direct empirical evidence on individual behavior is difficult—some would say impossible—to come by. But what little we know from experiments by psychologists like Daniel Kahneman and Amos Tversky and others does not suggest that *homo sapiens* behaves like *homo economicus*. (Perhaps that is why they have different names.) Inconsistent choices are common. People put too much weight on what has happened to them and to their friends and too little on statistical evidence. Framing of the question matters. The von Neumann-Morgenstern axioms are routinely violated. It is remarkable how little impact this evidence has had on modern economics. Is that scientific detachment or religious zealotry?

So I have come to the end of my exam with the conclusions you might have guessed at the outset: that when Lucas changed the answers given by Keynes, he was mostly turning better answers into worse ones; that modern Keynesian economics, though far from flawless, has a better claim to being "scientific" than does new classical economics.

REFERENCES

Friedman, Milton, "The Role of Monetary Policy," *American Economic Review*, March 1968, *78*, 1–17.

Keynes, J. M., *The General Theory of Employment, Interest, and Money*, London: Harcourt, Brace and World, 1936.

Lucas, Robert E., Jr., "Models of Business Cycles," paper prepared for the Yrjo Jahnsson Lectures, Helsinki, Finland, mimeo., March 1986.

Prescott, Edward, "Should Control Theory be Used for Economic Stabilization?," in Karl Brunner and Alan Meltzer, eds., *Optimal Policies, Control Theory, and Technological Exports*, Vol. 7, Carnegie-Rochester Conferences on Public Policy, *Journal of Monetary Economics*, Suppl. 1977, 13–38.

______, "Theory Ahead of Business Cycle Measurement," Federal Reserve Bank of Minneapolis Research Department Staff Report 102, February 1986.

Sargent, Thomas J., "Beyond Demand and Supply Curves in Macroeconomics," *American Economic Review Proceedings*, May 1982, *72*, 382–89.