

Discussion Questions, 5/18

History of Macroeconomics

After reading all three articles, I was very interested in the progression from what is known as classical macroeconomic theory towards what is today called neoclassical macroeconomic theory, or macroeconomic theory with micro-foundations. It would be a welcome start to the discussion if we could obtain a brief history of the transition (beyond what was covered in the articles) as well as the instrumental roles these authors played in that transition.

Lucas says that the major achievement in macroeconomics in the 1970s was the realization that unexpected monetary shocks affect the economy differently than anticipated shocks. What was/were the major accomplishment/s of the 1980s? Are there others besides Prescott's "Revolution?" How about the 1990s?

In reading Solow's speech, one gets the sense that while he had made significant contributions, more work needed to be done. Specifically, Solow had made huge strides in growth theory, but by 1987, there really wasn't much of a theory to explain business cycles in a growth context, or why economies fluctuate around a trend. However, after reading Prescott and Kydland, I don't get that same sense of a need for progress. They have their models and seem to be content in using them to explain real-world phenomena. I'm sure macroeconomics isn't finished (esp. after reading Lucas), but are we pretty content with DSG, representative agent macro models? Whither now?

Prescott describes a revolution in macroeconomics and traces some of the history of macroeconomic methodologies. How have these changing systems and assumptions affected how the Fed does its job? Has the way that the Fed conducts business and draws policy conclusions adjusted historically to reflect the prevailing economic views? I think that it would be very interesting if the behavior of our workplace could be tied into the macroeconomic group discussions.

Representative Agents in Macro Models

Are there any conceptual problems with representative agent macro models? Some of the main questions Kirman (1992)¹ raises are:

- Why bother to construct representative individuals if macroeconomists are only interested in problems of aggregate behavior?
- Does it make sense to make homogenous assumptions about behavior across individuals?
- What about the behavior *between* individuals (arbitrage, trading, profits, etc.) which lead the economy back to equilibrium?
- What exactly are the properties (utility maximization, reactions to change, preferences, etc.) that could conceivably carry over from the individual to the aggregate demand curve, and which ones shouldn't?

Other questions raised on this topic:

¹ Kirman, Alan P. 1992. "Whom or What Does the Representative Individual Represent?" *JEP*, vol. 6, no.2 (Spring).

-What evidence is there to suggest that micro parameters aggregate nicely, i.e. that their aggregation is a good estimate of the parameters that govern macro behavior? That is, what do we know that suggests that the whole is a sum of the parts?

-How confident are we that current estimates reflect the true parameter values for the *representative* American agent? Do we think that these values may vary across age, socioeconomic status, gender, state of mind, day of the week, etc?

-For most purposes, $pV=nRT$ is an accurate description of the relationship between the pressure, volume, and temperature of gases. Obviously the actual behavior of a gas is determined by the interaction of individual atoms. Yet for most purposes we understand gases in terms of aggregate properties, properties that individual atoms do not possess. A single atom has neither pressure nor temperature. Is there a flaw in extending this analogy to macroeconomics? Prescott and Kydland focus on the utility and production functions of individual consumers and producers. For most purposes, however, would anything be lost by simply studying the relationship of economic aggregates?²

Prescott Lecture

Prescott asserts that neoclassical macro is scientific. However, a key feature of science is that hypotheses and models can be rejected by some objective criterion. Can macroeconomic theory be falsified when "theory derives its concepts from measurement, and in turn theory dictates new measurement (pg. 392)" as Prescott asserts? In other words, can a theory be scientific when you both assess its validity by checking its predictions against empirical data when the theory predicts well, but when the theory does not work well you instead assert that there is a gap in measurement rather than faulty theory?

Prescott emphasizes that "having good macroeconomic policy requires having an educated citizenry that can evaluate macroeconomic policy" (page 371) and later that "economists should educate the people so that they can evaluate macroeconomic policy rules and...pick the policy rule" (page 393). Is this a realistic goal? If so, is there a good way for economists to go about doing this in practice? What other advantages, if any, are there to having an "educated citizenry," in terms of the way they can form expectations about future employment and inflation for example?

How close is the real business cycle model to models in the natural sciences? Is this a goal of macroeconomists? The goal of "naturalizing" macroeconomics seems to be a clear theme of Prescott's prose.

What are the possible problems of calibrating an RBC model with historical data that may have been generated under a variety of different policy rules, in order to forecast under a single, time-consistent policy rule?

Lawrence Summers suggests that Prescott may have just devised a model that fits well with observed behavior, and just because it fits well does not necessarily make the model correct theory. Does Prescott's idealistic vision of the nature of macroeconomics get in the way of his research, ie lead him to use comfortable assumptions?

In Prescott's paper, does he imply that evaluating a policy based on a systems-of-equations approach is never appropriate and that it is going to become outdated? That is basically what I

² This question is taken from Kevin Hoover (2001), *The Methodology of Empirical Macroeconomics*.

interpreted from what I read and that seems to be a very dramatic statement. How can modelling behavior using classical statistical methods be wrong in all circumstances?

Prescott argues that his dynamic inconsistency story is important for understanding inflation. Prescott also believes that "business cycle fluctuations are the optimal response to real shocks" (p. 389). In Prescott's story, however, central bank policy is dynamically inconsistent because printing money reduces unemployment in the short term. What do you think Prescott believes? That the money supply has large effects on output and thus central banks should be bound by rules or reputation? Or that fluctuations about trend are the result of real shocks, in which case central banks are irrelevant for output determination?

In both 2004 Nobel lectures, the authors reveal that their interest in economic theory is largely motivated by its ability to evaluate economic policy, and make policy recommendations through these evaluations. Why have they chosen this as their interest? How else could they hope that their work might affect economies?

What exactly is meant by an operational definition? Prescott states that "the concept is defined by the procedure used to determine the value of the concept" (p. 376). Can this be elaborated on?

Prescott discusses how consumer durables were first treated as other consumer goods, but that more robust models treat them more like producer durable investment. Were Prescott and Kydland the first people to do this? What was the rationale for the earlier categorization? Is there any lingering debate about this, or is it generally accepted that consumer durable expenditures should be considered in the later way?

What is the advantage of different methods of calibration (using parameter estimates from other studies vs. using impulse response functions to fit models)?

The Lucas critique

In the section "Macroeconomic Models after the Transformation", Prescott argues that the neoclassical macro avoids the Lucas critique by explicitly modeling individual and firm maximization. This approach avoids the Lucas Critique only if "preferences and technology are policy invariant". Is this assumption reasonable? Is it really true that differences between US and France economic policy have had no effect on French and US preferences?

Prescott writes (p. 373), "*Preferences and technology are policy invariant.*" (emphasis Prescott's). Suppose I believe this. It follows that:

- a. Seventy years of Communism in the Soviet Union had no influence on the hopes, desires, or knowledge of the Russian population.
- b. Wars begun by a country's government have no influence on *any* person's preferences or technology.

Yet, if I do not accept this statement, how is it that Real Business Cycle style models avoid the Lucas critique? i.e. If policy affects preferences and technology, how can I use calibration today to forecast the effects of a new policy tomorrow?

If "preferences and technology are policy invariant" (page 373), according to Prescott, then what exactly accounts for the differences in preferences and technology across nations? I have a hard time believing that these differences have nothing to do with cultural history, which is largely shaped by policy and political structures (at least in my mind).

Labor Supply Elasticity

The central finding of Kydland and Prescott (1982, "Time to Build and Aggregate Fluctuations") is that given labor supply elasticity (LSE) close to 3 and persistent TFP shocks of the right magnitude, business cycles are as the neoclassical growth model predicts. Prescott (1986) found that indeed, the shocks do have the right characteristics, and so given $LSE \approx 3$ TFP shocks are the major contributor to business cycle fluctuations. Is there an intuitive explanation why the value of the aggregate labor supply elasticity being close to 3 is so critical to Prescott's model? Are TFP shocks no longer so important for economies with other values of labor supply elasticity? Methodologically, is it problematic to build a model that requires the elasticity of substitution of labor supply be close to 3 in order to describe the world accurately, and then go searching for evidence supporting this value of the parameter?

Prescott states that only e (not h) affects the labor supply except in cases where $e=1$ (page 386). Can e really $=1$, or does he just mean when unemployment is at its natural rate? Further, might immigrants willing to work more hours for a lower wage account for any variation in h at the industry level?

Is Prescott right that the difference in marginal tax rates entirely accounts for the difference in hours worked? Alesina, Glaeser, and Sacerdote (2005) think that taxes are important but not the whole story; they ultimately attribute most of the divergence to Europe's stronger and more prevalent labor unions, and also distinguish between genders in their analysis (the labor supply of women is more elastic than that of men). How much do labor markets vary across nations? Do individuals' preferences for time utilization, etc, vary across nations?

Is the market clearing behavior of prices (including the price of labor) a correct assumption in the short run? In other words, are prices never sticky? Can people choose between labor and leisure as easy as implied?

TFP Shocks

Much emphasis is placed on the robustness and importance of the finding that total factor productivity shocks are crucial to business cycle fluctuations, perhaps comprising 70% of the variation from trend. But what does this really tell us about the economy given that the range of phenomena placed under the exogenous TFP umbrella is so broad? Does it not only push our ignorance back one level, to the nature of these stochastic shocks? What is a TFP shock in reality? What isn't? Can we identify specific technology shocks and empirically estimate their impact on economic fluctuations, where "technology" here means what we commonly think of as technology? Why did Prescott and Kydland put in two different TFP shocks to the model? What is the real phenomenon this is trying to capture?

What is the mechanism that translates TFP shocks into changes in labor input? We know the story for monetary shocks: fixed labor contracts mean that real wages change with the money supply. What's the TFP story?

I'm skeptical of the notion that TFP shocks are more important than monetary shocks for the business cycle. I would think productivity evolves slowly over time with technology and business practices and thus would have only long run growth effects. Don't monetary shocks explain events like the "Volker" recession, the Great Depression, East Asian currency crisis, Argentine crash, &c? How do nominal shocks affect an economy in this real business cycle model economy? How are they incorporated into the model?

Lucas Lecture

The way that money is treated in the model presented by Lucas seems kind of unnecessarily strange. The model relies on dividing up the population into two age cohorts, the old and the young, and money is introduced so that old people can buy goods from the young, who can't store them and save them when they get older. This model seems incredibly unrealistic, and there must be simpler ways of dealing with money (though, perhaps not). What other ways have people explicitly introduced money into macro models? I'm sure people have thought of this, but it seems more natural to model money as a separate good, whose price in terms of other goods fluctuates.

Are there cases in which money plays a role that is not neutral? Is it possible to add a bit of the theory of money to this or future discussions?

Lucas writes that "Unanticipated monetary expansions...can stimulate production." In this day and age of information and increasing amounts of government disclosure and even blogging, is it ever possible to have an unanticipated monetary expansion? If the economy is going through a downturn for whatever reason, what role does the Central Bank and other economic policymakers play in stimulating recovery apart from ensuring price stability?

Inflation was low or non-existent in the United States between 1870 and 1970. Yet during this time there was no formal understanding of dynamic inconsistency or even of the distinction between the long-run and short-run Phillips curve. And in the 1950's and 1960's, Keynesians dominated economic decision making. Is this absence of inflation a paradox? Does it tell us anything about the real world importance of dynamic inconsistency?

On the subject of the real effects of money, couldn't short-term price fluctuations affect long-term income inequality through the mechanism of capital ownership, as Prescott mentions on page 384? What (if any) is the relationship between short-term and long-term changes?

Lucas writes that "...every major depression in the United States over the period 1867-1960 was associated with a large contraction in the money supply, and that every large contraction was associated with a depression." Do these findings hold with most international data as well? What were the specific reforms that Europe put in place after WWI to reduce the monetary growth rates without having any such negative effects?

Lucas says earlier theorists didn't have the right "analytical equipment" to solve the models and so had to resort to disequilibrium ideas. Lucas seems to say that this missing equipment is modern mathematical economics. If the broad results of a theory can't be thought through in words and require resorting to mathematical procedures not just for speed, elegance, etc but rather for the results themselves, how much should we trust the theory?

How would expected changes in monetary policy relate to asset bubbles in the Lucas discussion of the quantity theory of money. For instance, what if the relative price of assets under a loose monetary policy begins to pull away from the general price level (essentially inflating one type of good so the holders of that good can sell it to acquire more real resources than they would otherwise be able to). Is this predicted by Lucas?

Solow Lecture

I was very interested by Solow's reference and analysis of the efforts made by economists to model and explain paths of economic growth and their relative stability or instability. This seems to be a common subject to all three articles. In particular, Solow states that one of the achievements of growth theory has been to relate equilibrium growth to asset pricing under tranquil conditions but it has not been able to provide a "really good" theory of asset valuation under turbulent conditions (page 310). Could we discuss what conditions can be classified under each of these states? In addition, has there been significant advancement in the ability of growth theory to further explain equilibrium paths in turbulent times?

In Solow's address, he states some fundamental problems that he has with Prescott's approach to macro modeling, specifically that "any kind of market failure is ruled out from the beginning, by assumption. There are no strategic complementarities, no coordination failures, no prisoners' dilemmas." Solow is troubled that Prescott's model economy is "disturbed only by stationary stochastic shocks to tastes and technology" and that the model assumes that all observed paths are equilibrium paths (as Prescott himself puts it, "business cycle fluctuations are the optimal response to real shocks"). How valid do you find Solow's criticisms of Prescott's approach?

What is the "Cambridge Controversy?" (Solow, page 309).

Solow is quite critical of the direction of modern macroeconomics. He says that sometimes, "there is no good alternative to the specification and estimation of a model. To leave it at that, however, to believe as many American economists do that empirical economics begins and ends with time-series analysis, is to ignore a lot of valuable information that cannot be put into so convenient a form. I include the sort of information that is encapsulated in the qualitative inferences made by expert observers, as well as direct knowledge of the functioning of economic institutions" (Solow, p311). To what extent has the progress of modern macroeconomics been stunted by our recent lack of interaction with and respect for political scientists, sociologists, psychologists, and philosophers?