Response to a Skeptic

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New findings in science are always subject to skepticism and challenge. This is an important part of the scientific process. Only if new results successfully withstand the attacks do they become part of accepted scientific wisdom. Summers (in this issue) is within this tradition when he attacks the finding I describe (in this issue) that business cycles are precisely what economic theory predicts given the best measures of people's willingness and ability to substitute consumption and leisure, both between and within time periods. I welcome this opportunity to respond to Summers' challenges to the parameter values and the business cycle facts that I and other real business cycle analysts have used. In challenging the existing quality of measurement and not providing measurement inconsistent with existing theory, Summers has conceded the point that theory is ahead of business cycle measurement.

Miscellaneous Misfires

Before responding to Summers' challenges to the measurements used in real business cycle analyses, I will respond briefly to his other attacks and, in the process, try to clarify some methodological issues in business cycle theory as well as in aggregate economic theory more generally.

Prices

Summers asks, Where are the prices? This question is puzzling. The mechanism real business cycle analysts use is the one he and other leading people in the field of aggregate public finance use: competitive equilibrium. Competitive equilibria have relative prices. As stated in the introduction of "Theory Ahead of Business Cycle Measurement" (in this issue), the business cycle puzzle is, Why are there large movements in the time allocated to market activities and little associated movements in the real wage, the price of people's time? Along with that price, Kydland and I (1982, 1984) examine the rental price of capital. An infinity of other relative prices can be studied, but these are the ones needed to construct national income and product accounts. The behavior of these prices in our models conforms with that observed.

In competitive theory, an economic environment is needed. For that, real business cycle analysts have used the neoclassical growth model. It is the preeminent model in aggregate economics. It was developed to account for the growth facts and has been widely used for predicting the aggregate effects of alternative tax schemes as well. With the labor/leisure decision endogenized, it is the appropriate model to study the aggregate implications of technology change uncertainty. Indeed, in 1977 Lucas, the person responsible for making business cycles again a central focus in economics, defined them (p. 23) as deviations from the neoclassical growth model—that is, fluctuations in hours allocated to market activity that are too large to be accounted for by changing marginal productivities of labor as reflected in real wages. Lucas, like me and

virtually everyone else, assumed that, once characterized, the competitive equilibrium of the calibrated neoclassical growth economy would display much smaller fluctuations than do the actual U.S. data. Exploiting advances in theory and computational methods, Kydland and I (1982, 1984) and Hansen (1985) computed and studied the competitive equilibrium process for this model economy. We were surprised to find the predicted fluctuations roughly as large as those experienced by the U.S. economy since the Korean War.

Some economists have been reluctant to use the competitive equilibrium mechanism to study business cycle fluctuations because they think it is contradicted by a real-world observation: some individuals who are not employed would gladly switch places with similarly skilled individuals who are. Solow (1986, p. S34), for example, predicted that "any interesting and useful solution to that riddle will almost certainly involve an equilibrium concept broader, or at least different from, price-mediated market-clearing." Rogerson (1984) proved him wrong. If the world had no nonconvexities or moral hazard problems, Solow would be correct. But the mapping between time allocated to market activities and units of labor service produced does have nonconvexities. Time spent commuting is not producing labor services, yet it is time allocated to market activity. With nonconvexities, competitive equilibrium theory implies that the commodities traded or priced are complicated contracted arrangements which can include employment lotteries with both winners and losers. As shown by Hansen (1985), competitive theory accounts well for the observation that the principal margin of adjustment in aggregate hours is the number of people employed rather than the number of hours worked per person—as well as for the observation of so-called involuntary unemployment.

Technology Shocks

Another Summers question is, Where are the technology shocks? Apparently, he wants some identifiable shock to account for each of the half dozen postwar recessions. But our finding is not that infrequent large shocks produce fluctuations; it is, rather, that small shocks do, every period. At least since Slutzky (1927), some stable low-order linear stochastic difference equations have been known to generate cycles. They do not have a few large shocks; they have small shocks, one every period. The equilibrium allocation for the calibrated neoclassical growth model with persistent shocks to technology turns out to be just such a process.

My Claims

Summers has perhaps misread some of my review of real business cycle research (in this issue). There I do not argue that the Great American Depression was the equilibrium response to technology shocks as predicted by the neoclassical growth model. I do not argue that disruptions in the payment and credit system would not disrupt the economy. That theory predicts one factor has a particular nature and magnitude does not imply that theory predicts all other factors are zero. I only claim that technology shocks account for more than half the fluctuations in the postwar period, with a best point estimate near 75 percent. This does not imply that public finance disturbances, random changes in the terms of trade, and shocks to the technology of exchange had no effect in that period.

Neither do I claim that theory is ahead of macroeconomic measurement in all respects. As Summers points out, Mehra and I (1985) have used the representative agent construct to predict the magnitude of the average risk premium of an equity claim over a real bill. Our predicted quantity is small compared to the historically observed average difference between the yields of the stock market and U.S. Treasury bills. But this is not a failure of the representative agent construct; it is a success. We used theory to predict the magnitude of the average risk premium. That the representative agent model is poorly designed to predict differences in borrowing and lending rates—to explain, for example, why the government can borrow at a rate at least a few percentage points less than the one at which most of us can borrow—does not imply that this model is not well designed for other purposes—for predicting the consequences of technology shocks for fluctuations at the business cycle frequencies, for example.

Measurement Issues

Summers challenges the values real business cycle analysts have selected for three model parameters. By arguing that historically the real U.S. interest rate is closer to 1 percent than to the model economy's approximately 4 percent, he is questioning the value selected for the subjective time discount factor. He explicitly questions our value for the leisure share parameter. And Summers' challenge to the observation that labor productivity is procyclical is implicitly a challenge to my measure of the technology shock variance parameter.

Real Interest Rate

Summers points out that the real return on U.S. Treasury bills over the last 30 years has been about 1

percent, which is far from the average real interest rate of the economies that Kydland and I have studied. But for the neoclassical growth model, the relevant return is not the return on T-bills. It is the return on tangible capital, such things as houses, factories, machines, inventories, automobiles, and roads. The return on capital in the U.S. business sector is easily calculated from the U.S. National Income and Product Accounts, so we use it as a proxy for the return on U.S. capital more generally. This number is obtained by dividing the income of capital net of the adjusted capital consumption allowance by the capital stock in the business sector. For the postwar years, the result is approximately 4 percent, about the average real return for the model economies.

Preferences

Summers also questions the value of the leisure share parameter and argues that it is not well tied down by micro observation at the household level, as we claim. This is a potentially important parameter. If it is large, the response of labor supply to temporary changes in the real wage is large. Only if that response is large will large movements in employment be associated with small co-movements in the real wage.

Kydland and I conclude that the leisure share parameter is not large based on findings reported by Ghez and Becker (1975). They report (p. 95) that the annual productive time endowment of U.S. males is 5,096 hours. They also say (p. 95) that U.S. females allocate about 75 hours per week to personal care, leaving 93 hours of production time per week. This multiplied by 52 is 4,836 hours, the annual productive time endowment of females. Ghez and Becker also report the average annual hours of employment for noninstitutionalized, working-age males as about 2,000 hours (pp. 85–91). If females allocate half as many hours to market employment as do males, the average fraction of time the U.S. working-age population spends in employment is about 0.30. Adding to this the time spent commuting yields a number close to those for our models. (They are all between 0.30 and 0.31 in Kydland and Prescott 1982 and 1984.)

Initially Kydland and I used time additive preferences, and the predictions of theory for productivity movements were as large in percentage terms as aggregate hour movements. This is inconsistent with observations, so I did not take seriously the prediction of theory that a little over half the aggregate output fluctuations in the postwar period were responses to technology shocks. At that time, measurement was still ahead of theory. Then, the prediction of theory would have been consistent with the relative movement of productivity and aggregate hours, and technology shocks would have accounted for the business cycle phenomena, if the leisure share parameter were five-sixths. With the discipline we used, however, this share parameter had to be consistent with observations on household time allocation. That we are now debating about a theory of aggregate phenomena by focusing on household time allocation is evidence that economic theory has advanced. Now, like physical scientists, when economists model aggregate phenomena, the parameters used can be measured independently of those phenomena.

In our 1982 paper, Kydland and I did claim that fluctuations of the magnitude observed could plausibly be accounted for by the randomness in the technological change process. There we explored the implications of a distributed lag of leisure being an argument of the period utility function rather than just the current level of leisure. Like increasing the leisure share parameter, this broadening results in larger fluctuations in hours in response to technology shocks. Kydland (1983) then showed that an unmeasured household-specific capital stock could rationalize this distributed lag. In addition, the lag was not inconsistent with good micro measurement, and these parameters could be measured independently of the business cycle phenomena. The distributed lag was a long shot, though, so we did not claim that theory had caught up to measurement.

Since then, however, two panel studies found evidence for a distributed lag of the type we considered (Hotz, Kydland, and Sedlacek 1985; Eckstein and Wolpin 1986). With this development, theory and measurement of the business cycle became roughly equal.

Subsequently, an important advance in aggregate theory has made moot the issue of whether Kydland's and my assumed preferences for leisure are supported by micro measurement. Given an important nonconvexity in the mapping between time allocated to market activities and units of labor service produced, Rogerson (1984) showed that the aggregate elasticity of labor supply to temporary changes in the real wage is large independent of individuals' willingness to intertemporally substitute leisure. This nicely rationalized the disparate micro and macro labor findings for this elasticity—the microeconomists' that it is small (for example, Ashenfelter 1984) and the macroeconomists' that it is large (for example, Eichenbaum, Hansen, and

Singleton 1984). Hansen (1985) introduced this nonconvexity into the neoclassical growth model. He found that with this feature theory predicts that the economy will display the business cycle phenomena even if individuals' elasticity of labor supply to temporary changes in the real wage is small. Further, with this feature he found theory correctly predicts that most of the variation in aggregate hours of employment is accounted for by variation in the number of people employed rather than in the number of hours worked per person.

Technology

□ *Uncertainty*

In our 1982 paper, Kydland and I searched over processes for the technological change process. We did sensitivity analysis with the other parameters, but found the conclusions relatively insensitive to their assumed values (except for the distributed lag of leisure parameters just discussed). The parameters of the technological change process did affect our predictions of the aggregate implications of uncertainty in the technology parameter. In fact, Lucas (1985, p. 48) criticized us for searching for the best fit. In "Theory Ahead of Business Cycle Measurement," I directly examined the statistical properties of the technology coefficient process. I found that the process is an approximate random walk with standard deviation of change in the logs approximately 0.00763 per quarter. When this number is used in the Hansen model, fluctuations predicted are even larger than those observed. In Kydland's and my model (1984), they are essentially equal to those observed.

Some, on the basis of theory, think that the factors producing technological change are small, many, and roughly uncorrelated. If so, by the law of large numbers, these factors should average out and the technological change process should be very smooth. I found (in this issue) empirical evidence to the contrary. Others have too. Summers and Heston (1984) report the annual gross national products for virtually every country in the postwar period. They show huge variation across countries in the rate of growth of per capita income over periods sufficiently long that business cycle variations are a minor consideration. Even individual countries have large variation in the decade growth rates of per capita output. Given Solow's (1957) finding that more than 75 percent of the changes in per capita output are accounted for by changes in the technology parameter, the evidence for variation in the rate of technological advance is strong.

Obviously, economists do not have a good theory of the determinants of technological change. In this regard, measurement is ahead of theory. The determinants of the rate of technological change must depend greatly on the institutions and arrangements that societies adopt. Why else should technology advance more rapidly in one country than in another or, within a country, more rapidly in one period than in another? But a theory of technological change is not needed to predict responses to technological change.

The key parameter is the variance of the technology shock. This is where better measurement could alter the prediction of theory. Is measuring this variance with Solow's (1957) method (as I did) reasonable? I showed that measures of the technology shock variance are insensitive to cyclical variations in the capital utilization rate. Even if that rate varies proportionately to hours of employment and the proportionality constant is selected so as to minimize the measured standard deviation of the technology shock, that measured deviation is reduced only from 0.00763 to 0.00759. Further, when the capital utilization rate varies in this way for the model, the equilibrium responses are significantly larger. Variation in the capital utilization rate does not appear to greatly bias my estimate of the importance of technological change variance for aggregate fluctuations.

Perhaps better measurement will find that the technological change process varies less than I estimated. If so, a prediction of theory is that the amount of fluctuation accounted for by uncertainty in that process is smaller. If this were to happen, I would be surprised. I can think of no plausible source of measurement error that would produce a random walk–like process for technological change.

□ *Labor Hoarding*

Summers seems to argue that measured productivity is procyclical because measurement errors are cyclical. To support his argument, he cites a survey by Fay and Medoff (1985), which actually has little if anything to say about cyclical movements. Fay and Medoff surveyed more than 1,000 plant managers and received 168 usable responses. One of the questions asked was, How many extra blue-collar workers did you have in your recent downturn? They did not ask, How many extra workers did you have at the trough quarter and at the peak quarter of the most recent business cycle? Answers to those questions are needed to conclude how the number of extra blue-collar workers reported by managers varies over the cycle. Even if these questions had been asked, though, the response to them would not

be a good measure of the number of redundant workers. Such questions are simply too ambiguous for most respondents to interpret them the same way.

The argument that labor hoarding is cyclical is not supported by theory either. The fact that labor is a quasi-fixed factor of production in the sense of Oi (1962) does not imply that more workers will be hoarded in recessions than in expansions. In bad times a firm with low output may be less reluctant to lay off workers than in good times because the worker is less likely to be hired by another firm. This argument suggests that labor hoarding associated with firm-specific output variations should be procyclical. Leisure consumed on the job also may be less in bad times than in good because work discipline may be greater. That is, an entrepreneur might be less reluctant to fire a worker in bad times because the worker can more easily be replaced. One might reasonably think, therefore, that labor's quasi-fixed nature makes measured productivity less, not more, cyclically volatile than productivity really is.

There is another, better reason to think that. In the standard measures of aggregate hours of employment, the hours of an experienced MBA from one of the best business schools are treated the same as those of a high school dropout. Yet these hours do not on average command the same price in the market, which is evidence that they are not the same commodity. In the neoclassical growth model, the appropriate way to aggregate hours is in terms of effective units of labor. That is, if the MBA's productivity is five times that of the high school dropout, then each hour of the MBA's time is effectively equivalent to five hours of the high school dropout's time. The work of Kydland (1984) suggests this correction is an important one. The more educated and on average more highly paid have much less variability in annual hours of employment than do the less educated. Kydland (1984, p. 179) reports average hours and average wages as well as sensitivity of hours to the aggregate unemployment rate for adult males categorized by years of schooling. His figures imply that a 1 percentage point change in the aggregate unemployment rate for adult males is associated with a 1.24 percent change in equally weighted hours. When those hours are measured as effective units of labor, the latter change is only 0.65 percent. This is strong evidence that if the labor input were measured correctly, the measure of productivity would vary more.

To summarize, measurement of the labor input needs to be improved. By questioning the standard measures, Summers is agreeing that theory is ahead of business cycle measurement. More quantitative theoretic work is also needed, to determine whether abstracting from the fact that labor is a partially fixed factor affects any of the real business cycle models' findings. Of course, introducing this feature—or others—into these models may significantly alter their predictions of the aggregate implications of technology uncertainty. But respectable economic intuition must be based on models that have been rigorously analyzed.

To Conclude

Summers cannot be attacking the use of competitive theory and the neoclassical growth environment in general. He uses this standard model to predict the effects of alternative tax policies on aggregate economic behavior. He does not provide criteria for deciding when implications of this model should be taken seriously and when they should not be. My guess is that the reason for skepticism is not the methods used, but rather the unexpected nature of the findings. We agree that labor input is not that precisely measured, so neither is technological uncertainty. In other words, we agree that theory is ahead of business cycle measurement.

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