Revista de Análisis Económico, Vol. 6, Nº 2, pp. 3-19 (Noviembre 1991)

REAL BUSINESS CYCLE THEORY: WHAT HAVE WE LEARNED?*

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Abstract:

Real business-cycle theory is the application of general equilibrium theory to the quantitative analysis of business-cycle fluctuations. The theory is real in the sense that there really is something there. In this review article I document that in applying established theory to quantitatively address business-cycle questions, a lot has been learned. We learned that business cycles are not deviations from this established theory, but rather are just what this theory predicts. In particular, we learned that business cycles are induced by highly persistent changes in those factors that determine the steady-state level of the deterministic growth model. Non persistent shocks do not induce fluctuations of the business-cycle variety. In this article I also discuss some methodological issues concerning judging and testing business cycle models.

Real business cycle theory is the application of general equilibrium theory to the quantitative analysis of business cycle fluctuations.[1] This theory is real in two senses. First, and most importantly, the theory is real in the English usage of the term. The dictionary definition of real is "serious" and "not imaginary, fictional, or pretended".[2] Second, the theory is real in the sense that the real values of commodities and securities are determined. If there is fiat money in the model economy, with the general equilibrium approach, the *real* value of this fiat money is determined. The only distinction between real and nominal prices in such worlds is that in one case the numeraire is fiat money and in the other it is some bundle of goods and services. With general equilibrium theory, the choice of the numeraire does not matter.

* This is the Revista de Análisis Económico Special Invited Lecture presented at the Tenth Latin American Meeting of the Econometric Society, Punta del Este, Uruguay, August 27-30, 1991.

** I thank Fernando Alvarez for helpful criticisms and for his assistance in preparing the reference list.

1. Real Aggregate Theory and the Return of Business Cycles

Equilibrium theory, even if we restrict attention to competitive equilibrium theory, is virtually vacuous. The important Sonnenschein (1973)–Mantel (1974)–Debreu (1974) finding that any system of continuous excess demand functions is consistent with general equilibrium theory establishes this result. What is needed is some strong or real theory that is appropirate for studying business cycle fluctuations. This we now have. When the quantitative implications of models based on this strong theory are derived, we sometimes are surprised by the answers. The nature of the surprise may be a quantitatively important devition from the strong theory, or it may be that what we thought was a deviation from the strong theory is not a deviation at all, but is just what theory predicted. That the results of real business cycle theory computational experiments suprise us and change our views is evidence that there really is some theory.

What is the strong theory to which I refer? It uses the Solow's neoclassical aggregate production function with capital and labor as the inputs. In particular CES production functions are used. These functions permit us to organize our empirical knowledgre of technology around a limited number of parameters–namely the substitution and the share parameters of the production function. This strong theory also uses a stand-in agent or set of representative agents with preferences for consumption over time to represent people's willingness to substitute. This simple structure accounts in a parsimonious way for the growth facts once technology change is introduced. This strong theory underlies much of modern quantitative public finance. This strong theory is the basis for modern quantitative business cycle theory as well.

Business cycle theory was a central topic in economics in the first half of this century. Frisch, the founder of the Econometric Society and first editor of *Econometrica,* lists it as one of the four main fields of interest to econometricians in his editorial statement to that journal's first issue in 1933. Business cycles is the only one of these fields that is substantive in nature. In the 1930s and the 1940s a proliferation of business cycle models were spawned by Frisch's (1933) seminal Cassel volume paper. By varying the parameters of the equations that constituted these models, these models could generate time series with almost any statistical properties. I think this is the reason interest in business cycles virtually disappeared in the 1950s and 1960s. The topic of business cycles again became a central topic in economics in the 1970s and the 1980s. Robert Lucas is the person who directed and lead the development of modern business cycle.[3] He developed ways of applying neoclassical reasoning to the study of dynamic stochastic phenomena–which is what business cycles are. Lucas brought back the question of why do market economies display recurrent fluctuations, and he showed that this question could be addressed within the fully neoclassical paradigm.

Lucas (1977, p. 9) defines business cycles as being fluctuations of GNP about trend, and he viewed these fluctuations as deviations from existing economic theory as it is reflected in the neoclassical growth model wiht the labor-leisure decision endogenized. As is well known from the work of Solow (1957), variations in the per capita labor input accounts for little of the growth in per capita output. What accounts for growth in per capita output are changes in the capital stock per worker and changes in the production function. What accounts for most business cycle fluctuations, however, are changes in hours worked per capita. Another fact is that cyclically employment and the real wage are roughly orthogonal. These two facts lead people to the conclusion that technology factors that cyclically altered the marginal product of labor were not the major factors that were giving rise to business cycle fluctuations. This is why business cycles were viewed as a deviation from then established theory that equated peoples' ability with their willingness to substitute.

2. Some Methodological Issues

The methodology used in modern quantitative business cycle theory is that advocated by Lucas (1980) in his paper *Methods and Problems in Business Cycle Theory.*[4] In it he advocates the construction of artificial economies, which I prefer to call model economies, and the use of these model economies to carry out policy experiments. Any such model economy is an abstraction, and therefore "unreal". He states that for the model economy to be useful in addressing some question, the equilibrium behavior of the model economy must mimic the behavior of the actual economy on some dimensions if the results of these experiments are to influence what we think the answer to the question is for the actual economy. He goes on to point out that we have more confidence in the answer if the model economy used mimics the behavior of the actual economy in more dimensions. Here I will illustrate the use of these very general principles in judging business cycle models.[5]

Judging Business Cycle Models

Sometimes when a model economy fails to mimic the data in particular respect, we are suspect of the answer provided by the model. In such situations, I say that there is the need for stronger theory before we can quantitatively answer the question. Here I will review findings concerning the effects of monetary disturbances. The model economies used in these computational experiments fail to mimic, in what I think is a crucial dimension, the behavior of the actual economy. For this reason, I am hesitant in drawing inference about implications of monetary and credit policy for the behavior of the actual economy based on these computational experiments.

At other times, a model economy fails to mimic the data in a certain respect, yet I still have great confidence in the intuition based on the results of the experiments. I think business cycle models are well suited for estimating the quantitative consequences of a given variations in the technology process, even though the model economy fails to mimic the behavior of labor's income share. The difference is that model economies labor's share is constant, while for the actual economy it is counter cyclical. One could modify the model economy by having less than unit elasticity of substitution between capital and labor. For the modified economy labor's share of income would move counter cyclically, but then the secular pattern of labor share would be at variance with the data. A principle of this methodology is that there can not be one substitution elasticity to account for the growth observations and another for business cycle fluctuations. One reason that I am not that bothered by this deviation is that ex ante and ex post substitution opportunities between capital and labor are the same in the model economy, while in the actual economy this is not the case. Consequently, we would expect some deviation between theory and observation along this dimension. Deviations from strong theory define good questions to be addressed. In the case of the counter cyclical behavior of labor share deviation, it hast lead Gomme and Greenwood (1990) to explore the implications of Arrows-Debreu risk allocation that results in compensations being different than income. They find with recursive preferences that labor compensation is counter

cyclical as it is in the data. Before these alternative structures become part of established theory, these structures must prove useful for interpreting not only business cycle data but also other observations, including those concerning growth, asset price behavior, and micro labor market facts. Only if this alternative preference structure gains the status of established theory is the Gomme and Greenwood model a resolution of the counter cyclical labor income share puzzle.

Any real business cycle model is necessarlly an abstraction and as such, there will be differences in the behavior of the model economy and the actual economy. What determines whether or not these differences are such that we have little or no confidence in the answer that the model provides to the question we want answered? I cannot provide a set of rules to make this determination. But in practice agreement sometimes emerges that some theory is strong relative to the given class of questions. In such cases there is no need to defend the theory. The theory is merely used until someone develops a better theory.

Mimicking is not Always Good

When using strong theory to answer a question, the choice of models should not be dictated by the nature of the answer provided. This is perverted scientific practice. To illustrate this important point, consider the question that Kydland and I have focused on in our business cycle research. This question is how important are technology shocks as a source of business cycle fluctuations? For this purpose, we use a model economy with technology shocks being the only source of fluctuations. Our finding could have been that these shocks are a quantitatively unimportant source, an all important source, or something in between. To parameterize the model and search for that set of parameters which maximizes the fraction of fluctuations induced by technology shocks is silly. To estimate the model is to implicitly assume that technology shocks are the only significant source of fluctuations. That is not a hypothesis we were willing to maintain. If we knew this were a fact, there would be no reason to use the strong theory to answer the question posed. The answer would alreday be known.

Gyven that our model economy has only technology shocks, there is an important test of the theory. In particular, the theory puts restrictions on the estimated fraction of fluctuations accounted for by the technology shocks and the actual correlation between labor productivity and the labor input. The restriction is not that this correlation for the model economy is close to the value in the actual data.

McCallum (1989) has questioned modern quantitative business cycle theory because the correlation between the labor input and its productivity is near one for the model economy while it is near zero for the U.S. economy in the postwar period.[6] If our finding were that technology shocks were all important, this would be a legitimate criticism. But this is not our finding. Our estimate is that the variance of U.S. postwar business cycle fluctuations would have been about 70 percent as large if technology shocks were the only source of fluctuations.[7] Given this estimate, an implication of theory is that the actual correlation should be near zero, which it is. I now establish the nature of this restriction.

For our model economy (Kydland and Prescott, 1991a), which has technology shocks only, the equilibrium relation between logarithms of the labor input, h_1, and output, y_1, is approximately

$$y_1 = 1.5\, h_1.$$

The empirical elasticity of output with respect to the labor input is considerably in excess of the production function's labor share parameter, which is about two-thirds. The reason the empirical elasticity is so large is a strong positive correlation between the technology shock and the labor input. Cyclically, fluctuations in the capital input are small enough to be ignored in this discussion. If other shocks were introduced into the model economy, for example public finance shocks, the relation between fluctuations in output, y_2, and the labor input, h_2, induced by these shocks would be dictated by the production function. Consequently, given the labor share parameters is 0.67

$$y_2 = 0.67\, h_2.$$

If both technology and other shocks are present, fluctuations in output y and the labor input h are the sums of these two components. Assuming orthogonality of the technology shocks and the other shocks inducing fluctuations, the fraction of variance induced by technology shocks is

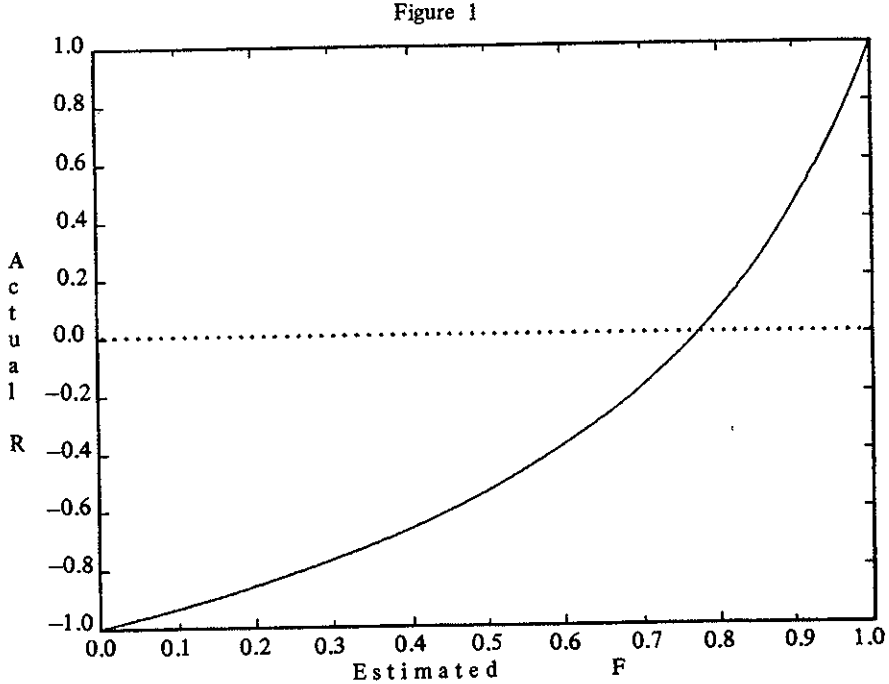
$$F = \mathrm{Var}(y_1)/\mathrm{Var}(y),$$

and the correlation between the labor input and ists productivity is

$$R = \mathrm{Corr}(y - h, h)$$

The figure depicts the locus of points that actual R and estimated F must satisfy. That estimated F and the actual R satisfy this

Figure 1



restriction is corroborating evidence for our 70 percent estimate of how volatile the U.S. postwar economy would have been if technology shocks were the only shocks. If the actual correlation R were near 1.0, as it is for the model economy, I would have little confidence in our estimate of the importance of technology shocks' contribution to cycle fluctuations. This corroborating evidence is importante because the process on the technology shock is not that well tied down and the answer to the question that we pose is sensitive to the specification of this process.

Calibrating the Growth Model

Part of the methodology of real business cycle theory is to calibrate the growth model economy to the growth facts displayed in the national income and product accounts. These key growth facts are that investment and consumption shares of product, the capital-output ratio, and the return on capital have remained more or less constant. Standard national income and product account data, however, must first be modified so that they conform with the growth model economy before using this data to calibrate the model. The key modification is that consumer durable expenditures must be treated as investment and, as for owner occupied housing, imputed services from these consumer durables must be included in consumption. Treating consumer durable expenditures as part of consumption as it is done in the accounts may be reasonable practice for some steady state analyses. It is not reasonable practice for business cycle analyses or for non steady state analyses.

These real business cycle models typically abstract from growth and hence these economies fluctuate about some level. In fact the U.S. economy on average grows. In order for the average capital-output ratio, product shares, and income shares of the model economy to match the corresponding observations for the actual economy, the subjetive time discount rate of the stand-in consumer and the depreciation rate of capital must be higher for the business cycle model without growth than they are for the growth model calibrated to the steady state growth observations. Hansen (1988) has formally established that relative to the business cycle phenomena, economies with different growth rates, provided that these average product shares, income shares, and the capital-output ratio are the same, are equivalent.[8] The principle here is that if a business cycle model abstracts from the fact that there is positive growth, then some of the parameters of the model should be adjusted in order that average product and income shares continue to match the actual economy.

3. Findings of Real Business Theory

In 1980, when Finn Kydland and I began our quantitative exploration of the implications of the neoclassical growth model for business cylces, we thought that technology shocks were unimportant and that business cycles were the response to temporary shocks. Our candidate for propagation mechanism was a time-to-build technology as in Frisch's (1933) business cycle model. To study business cycles using the neoclassical growth model it was necessary to first extend it by introducing the labor-leisure decision into the model. This structure, following Solow (1970), was calibrated to steady state observations. The intertemporal elasticity of substitution of leisure for our model economy, being 2, was quite high relative to what micro labor economists then thoght reasonable. It was not high relative to the number that macroeconomists Lucas and Rapping (1969) estimated.

Persistent Shocks Important

Our model economy (Kydland and Prescott, 1982) has both highly persistent and transitory shocks. Our expectation, as stated previously, was that the transitory shocks would be the important ones. We were surprused to find that it was the permanent shocks that gave rise to fluctuations of the business cycle variety. This forced us to revise our views of business cycles and to adopt the view that they are the sum of random causes rather than being responses to damped oscillatory systems. We found that if quarterly technology shocks had standard deviations of 0.9 percent and if the intertemporal elasticity of labor substitution was somewhat larger than 2, the model economy displayed business cycles of the same magnitude and nature as those that the U.S. economy displayed in the postwar period. At this point in time, theory was not strong in accounting for business cycle fluctuations. It was strong, however, in documenting deviations from theory.

Labor Indivisibilities Important

A major theoretic breakthrough occurred in 1984. Rogerson (1984, 1988) considered a static world in which there was a labor indivisibility. When this feature is present, the difference in the utility function of the stand-in household and that of the households whose behavior is being aggregated is dramatic. Once employment lotteries are introduced, when preferences are additively separable in consumption and leisure, the stand-in households utility function becomes linear in leisure if there is a labor indivisibility. Hansen (1985) introduced labor indivisibilities into the neoclassical growth model. He calibrated it to the U.S. steady state growth observations. Given the linearity of the utility function in leisure, the intertemporal elsticity of substitution of the stand-in household is infinite and independent of the value of this parameter for the individual whose behavior is being aggregated. With this feature, and the Prescott (1986) estimate of the variance of the technology shock, the Hansen model economy displays fluctuations slightly bigger than the U.S. economy experiences in the postwar period[9].

This labor indivisibility matches well with many labor market observations. Empirically, most of the variation in the labor inputs is in the number of people working in a given week and not in the length of the workweek. Empirically, the variance in the number of hours worked in a given year is much higher for those who on average work half the weeks than it is for those who typically work all weeks[10]. The market premium associated with less variability on employment is small[11]. There are huge seasonally variations in the number of people working in retailing yet very little seasonal variation in the wage. All these observations fit well with the labor indivisibility story. A discipline of quantitative business cycle approach is that macro models are restricted by micro as well as macro observations.

Why is there this institutionally determined workweek? Hornstein and Prescott (1989) have what I consider a good answer. Hornstein and I assume that the output of a worker is

$$hk^{\theta}$$

where k is the capital used by the worker and h is the length of the workweek that the worker works. We found that in equilibrium, such worlds behave as if there were an

institutionally determined workweek. That is, as if there were a Rogerson-Hansen labor indivisibility.

Kydland and Prescott (1991a) introduce this feature into their hours and employment variation model of business cycles. The feature alone does not result in the workweek h varying. As for the Hansen economy, there are only variations in the number employed. Given that there is some variation in the length of the workweek, we thought that a model with variations along both margins would be a better one to estimate the importance of technology shocks. We introduced resource costs of moving people between the household and the business sectors. We calibrated the model so that the relative variation in the length of the workweek and the number employed matched those for the postwar U.S. economy. It turned out that resources used in moving people between sectors are on average for the model economy about 0.01 of a percent of tis GNP. This is not a large number. Introducing these small costs reduces our estimate of the contribution of technology shocks to business cycle fluctuations by 15 percent.

One feature or ours and Hansen's model world that differs from the real world is that in the actual economy not all idiosyncratic employment risk is insured. I do not find this bothersome for the theory. Green (1987) and Díaz-Giménez (1991) have found that borrowing and lending is a surprising good substitute for insurance, and the credit arrangement is less subject to moral hazard problems arising from private information than are insurance arrangements.

Labor Hoarding Not Important

A problem with theories that assign no importance to technology change is that cyclically measured labor productivity moves procyclically in violation of the law of diminishing returns. When there is a discrepancy between a theory and measurement, the problem may be with the measurement and not with the theory. One possible measurement problem is that when out-put is high, unmeasured investment is low. A candidate for unmeasured investment is maintenance. These maintenance expenditures increase future production possibilities and therefore are an investment. They, however, are not part of measured investment because maintenance expenditures are expensed rather than capitalized. A problem with this unmeasured investment story is that no reason is provided for unmeasured investment to behave any differently than measured investment. Cyclically all components of measured investment; whether for plant, equipment, or inventory change, are high when output is high and low when output is low. Temporary shocks other than technology shocks will give rise to comovements in output and investment in opposite directions. But the finding of quantitative business cycle theory is that business cycles are induced by relatively permanent shocks in factors which significantly affect the steady state of the deterministic growth model economy.

Another problem with the measurement error explanation is that there is reason to believe that, cyclically, better measures of the labor input would vary less than the now standard one. The reason is that the standard measure of the labor input is hours of employment. No adjustment is made for the fact that hours of time of different people are not all the same commodity. This aggregation procedure of treating everybody's hours equally is at variance with the procedure used to measure components of final product and the capital input. The procedure used in these cases is to use base year prices to aggregate. Items that cost twice as much receive twice the weight. When Kydland and Prescott (1988b) applied this same principle to construct a constant price measure of

the labor input for a large and somewhat representative group of Americans for the 1969-1982 period, we found that this measure of the labor input fluctuates significantly less than the standard measure. Insofar as this finding generalizes to the economy as a whole and to other periods, our finding indicates that labor productivity measured in a way consistent with the traditions of national income and product accounting is more strongly procyclical than the standard measure of the labor input. This measurement corrections works in the wrong direction for those who view technology shocks as being unimportant however.

Another suggested explanation of the procyclical movement of the productivity of labor is that labor is in part a fixed factor of production. It is in part a fixed factor because there are hiring and firing costs. The introduction of adjustment costs into the business cycle model results in significantly less fluctuations in employment and, therefore, less fluctuations in output as well. With this feature, for economies having no technology shocks, labor productivity is counter cyclical. Adjustment costs do not rationalize the procyclical movement of labor productivity in worlds where technology shocks are not an important contributor to fluctuations.

Developing a model which displays large systematic procyclical measurement errors in the labor input, that is displays labor hoarding, appears difficult. I know of only one such quantitative equilibrium model, namely the one of Eichenbaum (1991)[12]. His key assumption is that the intensity with which people work during a quarter of the year, which is the time period of his economy, is adjustable after the technology shock is seen. Prior to the observation, the number of people who work and the number of hours that they work is determined. The labor input is the number of people working times the hours worked per employed person times the intensity of their work effort. Disutility of work depends upon the product of hours worked and effort intensity.

Work intensity probably varies, but does it vary systematically with the cycle? People have searched intensely for a model with this feature. Eichenbaum is the only one who has enjoyed any success in this endeavor, and the micro observations backing finance shocks to business cycle theory have not been that successful. The theory is prov-employment and hours weekly. The do not commit employment and overtime decisions for thirteen weeks and stick with these decisions independent of what happens in the interim. If the Eichenbaum model is modified to be consistent with these micro observations and the commitment is for one rather than 13 weeks, cyclically, work intensity varies hardly at all. The labor hoarding story makes sense for seasonal fluctuations, which are responses to seasonal variation in preferences and technology. The labor hoarding story does not make sense for business cycles which quantitative theory has found to be responses to relatively persistent or permanent shocks.

To summarize, for fifty years labor hoarding was viewed as the explanation of the procyclical movement of labor productivity. However, once the full set of the quantitative implications of this story were worked out, the story made *no* sense. This illustrates the discipline of quantitative general equilibrium approach to the study of business cycles. Finding the model economy which justifies prior intuition often proves impossible, and one is forced either to give up being an economist or to change one's economic intuition.

Efficiency Wages Not Key

Another popular story that has been proposed to account for the procyclical real wage observations is some efficiency-wage consideration. The key element of the efficiency-

wage construct is a moral hazard problem on the part of the worker. This problem results in the equilibrium contracting outcome being such that those employed realize higher expected utility than similar people who are not employed. Danthine and Donaldson (1990) introduce these so called efficiency-wage consideration into a real business cycle model. They find that these consideration have important consequences for the steady state level about which their model economy fluctuates, but little consequences for business cycle fluctuations.

Increasing Returns Not Key

Another proposed explanation for productivity's procyclical movement that does not rely on technology shocks is that there are increasing returns to scale. Having increasing returns necessitates the abandonment of the neoclassical aggregate production function. One way that increasing returns have been introduced into the real cycle models is via Spence-Dixit-Sitglitz monopolistic competition[13]. It is difficult to reconcile this structure with the growth facts that lead Solow to develop the neoclassical aggregate production function. But, if the monopoly power is local and if the number of monopolists increases proportional to the number of people in a country, the growth facts could hod for a monopolstic competitive model economy.

Rotemberg and Woodford (1989) quantitatively explore the behavior of such an economy and find that labor productivity is procyclical absent technology shocks if monopoly rents are sufficiently large. That these rents must be so large in their world indicates to me, however, that the reason labor productivity moves procyclically is not increasing returns.

Hornstein (1990) also studies calibrated monopolistic competitive business cycle models. He finds that with this element present the standard methods for measuring the variance of the technology shocks over estimates their variance. This error increases with the importance of increasing returns and resulting market power. He also finds that the amount of business cycle fluctuations induced by shocks of a given size increase with the degree of increasing returns. When both these factors are taken into account, the implication of his analysis is that the estimate of the importance of technology shocks is not very sensitive to the degree of increasing returns. Even with increasing returns, technology shocks are needed for procyclical movement of labor productivity.

Agent Heterogeneity and Market Incompleteness Not Key

Most real business cycle models use the representative infinitely-lived family construct. Model worlds with this feature can not be used to assess the significance of market incompleteness for business cycle fluctuations. When there is only one type of risk averse agent, there are no gains form allocating risk among agents and the behavior of incomplete and complete market arrangements are the same. A question then is whether the absence of Arrow securities for the allocation of risk in heterogeneous agent worlds is important for business cycle fluctuations. Another related question is whether the infinitely-lived family abstraction is a reasonable in the study of business cycles or whether the overlapping generational structure should be used instead. The analyses of Ríos-Rull (1991b, 1991c) provide answers to these questions. He finds that relative to business cycle fluctuations, calibrated infinitely-lived family models are essentially the same as

calibrated life cycle models. He finds that this is true whether there are Arrow securities or whether there are just borrowing and lending markets.

Nature of Technological Change Not Key

Still another question is whether the nature of the technological change is important for understanding why industrial market economies display business cycle fluctuations. With the single sector growth model, output can be used either for consumption or for investment. Technological change is necessarily with respect to these two components of output and, in equilibrium, their relative price is constant. In fact over time the relative price of durable goods, that is the investment goods, has declined relative to the price of nondurable goods and services (Greenwood, Hercowitz, and Krusell, 1991). This question is answered by the study of Greenwood, Hercowitz, and Huffman (1988). In their real business cycle model all technology change is in the durable good producing sector. They find that for this two sector model whether the technology change is neutral with respect to the sectors is not important for understanding business cycle fluctuations. I emphasize that this is not to say that the nature of technology change is not crucial for understanding other phenomena. It surely is crucial for modelling economic development, a phenomena whose importance dwarfs that of business cycle fluctuations.

Inventory Investment Not Excessively Volatile

Aggregate output is more volatile than are final sales. This result is inconsistent with inventories being used to smooth production over the cycle. The finding of business cycle theory is that investment in inventories should, as it does, behave like investment in other capital stocks. Once it is recognized that production, distribution and sales of final goods take time and that these intermediate goods are what constitute the stock of inventories, the huge volatility of inventory investment is not a puzzle[14]. It is merely what theory predicts.

Nominal Wage Contracting Not Key

Nominal wage contracting and price setting are features which characterize virtually all market economies, and the question is whether or not these features play a key role in business cycles by providing a mechanism for monetary policy actions to have significant real consequences. Cho and Cooley (1991) introduce nominal contracting into the neoclassical growth model and explore its quantitative implications for business cycle fluctuations. Their finding is that introducing these features does not provide a mechanism for monetary disturbances to induce business cycle type fluctuations. The nature of the fluctuations induced by monetary shocks in their model economy are not like those that the U.S. economy experienced in the postwar period. The serial correlation properties of output and the capital stocks do not match. Neither do the comovements match. They do find that when nominal contracting is introduced along with persistent technology shocks, the model economy better mimics the U.S. data than does a model which abstracts from nominal wage contracting. The improvement is in the higher frequency movements of the economic time series.

4. Implications of Real Business Cycle Theory for Other Fields

Theory can predict the consequence of public finance shocks processes for fluctuations in the economy. One problem with using public finance shocks to test the theory is that it is not clear what was the nature of the process on effective tax rates and on government expenditures. For this reason, efforts at estimating the contribution of public finance shocks to business cycle theory have not been that successful. The theory is proving useful, however, in evaluating the welfare consequences of alternative policies and in designing optimal tax policies. When better theory is used in public finance analysis, we have more confidence in intuition developed from the computational experiments. References to a number of quantitative public finance studies that use business cycle theory are listed in the bibliography section.

Another field upon which real business cycle theory has built and to which it has contributed is labor economics. The spectacular example of this is the development and use of the already discussed labor indivisibilities construct. A second contribution (Benhabib, Rogerson, and Wright, 1991 and Greenwood and Hercowitz, 1991) is the explicit modelling of household production. A third contribution (Cho and Rogerson, 1988) is the modelling of the two party household time allocation problem and its introduction into aggregate equilibrium models. A fourth contribution (Ríos-Rull, 1991a), which is also a contribution to public finance, is the quantitative determination of the importance of baby boom type variations in population growth rates for aggregate savings behavior and the return on capital. These studies strengthen labor economics by focusing on the aggregate implications of various elements of the households' allocation problem. Given that the allocation of time is so important in many policy evaluations, stronger labor economics strengthens public finance.

5. Need for Stronger Theory

An interesting question is how important a contributor to business cycle theory are monetary shocks? Colley and Hansen (1989, 1991) have addressed this issue using the Lucas and Stokey (1987) cash-credit good construct[15]. The beauty of this construct is that it permits the introduction of money into the neoclasdical growth model in a computational tractable way. Models of this type have been used to evaluate monetary policy. Unlike the case of fiscal policy evaluation, however, I have little confidence in these evaluations.

There are three related reasons for my lack of confidence. The first is that, unlike the actual economies, these model economies fail to display the sluggishness of the response of the inflation rate to changes in the growth rate of money[16]. The second is that households hold large quantities of liquid assets that earn low, and for extended periods, even negative returns. In the United States during the postwar period, household's holding of M2 was more than half annual GNP. The stock of these assets seems much larger than that needed for transaction purposes. The third reason is that the evaluation of monetary policy appears to be sentitive to the reason why people hold these liquid assets. Imrohoroglu (forthcoming) has constructed a model economy in which people vary their holdings of liquid assets as their income varies in order to smooth their consumptions[17]. She finds that if a transaction cost model is calibrated to data generated by her economy and the calibrated economy used to estimate the cost of inflation, this estimate is grossly at variance to the true cost of inflation for her model world. I found this result suprising

and bothersome. Typically, it does not matter how some feature is introduced as long as the aggregate substitution elasticities and quantities match.

Given that the answer to monetary policy questions depend upon whether money is held for transaction or for precautionary purposes, analytic tractability cannot dictate the way money is introduced. Besides matching better with the micro observation, model economies in which the principal reason people hold money is for precautionary reasons display considerable sluggishness in the inflation response to changes in the growth rate of the money supply. We currently do not have the tools for computing equilibrium of models with both the features of the neoclassical growth model and with idiosyncratic shocks that result in people holding money for precautionary reasons. That is why I say we need stronger theory when it comes to evaluationg non steady state monetary policy and determining the contribution of monetary policy shocks to business cycle fluctuations.

Another area in which rapid progress is being made, but in which theory has not yet progressed to the strong stage, is open economy business cycle theory. The basis for my assessment that this theory is not yet that strong is the open economy models fail to mimic the data in two important respects. The first is that for open economy business cycle models the variability of the terms of trade is much smaller and is more persistent that it is in fact[18]. The second important discrepancy between theory and observation is that in the model worlds the trade flows are much more volatile than in the real world. I will not even attempt to review the current stage of open economy business cycle theory given that most of the many open economy real business cycle papers are either just published, forth coming, or still in working paper form. (See the bibliography section). It is possible that these two important deviations are already resolved. I mention them becasuse this development illustrates a very appealing feature of this quantitative general equilibrium approach. When theory fails, it fails in particular ways and these failures define good problemas.

6. Concluding Comment

I have attempted here to clarify some methodological issues in quantitative business cycle theory. I argue that there is now some pretty strong theory and that business cycles are just what this theory predicts. These computations experiments have changed our views. This is evidence that the theory is real. I also argue that advances in business cycle theory have strengthened the theory and now this theory is proving useful in addressing issues in public finance as well as having a significant impact on labor economics. I argue that the theory is not so strong when it comes to evaluating and predicting the consequences of alternative monetary policy rules or when it comes to designing optimal monetary policy. Here, and in the case of international finance issues, theory has not yet reached the stage where we have confidence in the answers it provides.

Notes:

1 Long and Plosser (1983) introduced the expression real business cycles.
2 This is the definition found in the *American Heritage Dictionary*. Second College Edition (1985).
3 Lucas's papers on this topic are repinted in his 1980 book *Studies in Business Cycle Theory*.
4 Lucas develops further this methodologic view in his 1987 book *Models of Business Cycles*.
5 For a discussion of the econometrics see Kydland and Prescott (1991b).

6 See Christiano and Eichenbaum (forthcoming).
7 See Kydland and Prescott (1991a).
8 King, Plosser, and Rebelo (1988a, 1988b) develop this issue further as well as explore other issues concerning growth and fluctuations.
9 Hansen and Sargent (1988) introduce an overtime as well as a regular time work option.
10 See, for example, Kydland (1984).
11 See, for example, Abowd and Ashenfelter (1981).
12 Burnside, Eichenbaum, and Rebelo (1990) build on this model.
13 Baxter and King (forthcoming) introduce an externality.
14 Kydland and Prescott (1982, 1988a, 1991) and Christiano (1988) have examined this issue within the framework.
15 Kydland (1989) also introduces money into the business cycle. People hold real cash balances in his world because this economizes on their time.
16 Christiano and Eichenbaum (1991) make this point.
17 Imrohoroglu and Prescott (1991) introduce a banking technology to intermediate goverment debt.
18 See, for example, Quiroz (1991a, 1991b) or Backus, Kehoe, and Kydland (forthcoming) for documentation of this deviation.

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