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MACROECONOMICS AFTER TWO DECADES OF RATIONAL EXPECTATIONS

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ABSTRACT

This expository paper describes major developments during the second decade of rational-expectations macroeconomics, roughly 1982-1991. Topics attracting the most attention from researchers differed from those of 1972-1981, with considerable emphasis being devoted to technical matters. Here the discussion focuses on four prominent areas: real business cycle analysis, growth theory and its empirical application, issues involving unit roots in macroeconomic time series, and sticky-price models of aggregate supply. The paper concludes by arguing that the current state of knowledge in macroeconomics is not as bad as is often suggested.

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I. Introduction

This paper has been designed for a non-technical, expository talk on recent developments in macroeconomics. The title refers to the fact that it is just about 20 years since the publication of three papers by Robert Lucas (1972a, 1972b, 1973) that began the series of developments that has frequently been called the "rational expectations revolution" in macroeconomics. The title is also a one-decade update of one that I used for a survey paper about ten years ago (McCallum, 1983). By that time it was the case that the first round of controversies was almost over and rational expectations (RE) had become the dominant hypothesis concerning expectational behavior. It had also become widely recognized that adoption of RE does not necessarily imply ineffectiveness of monetary policy--indeed, John Taylor (1979) had already conducted stabilization policy simulation experiments with a small RE model that was a forerunner of the multi-country model that he has been using recently (Taylor, 1989).

During the first decade of RE macroeconomics, the main issues concerned the specification of aggregate supply or wage-price behavior. Details of this specification pertaining to lags, overlapping contracts, the presence or absence of dynamic money illusion, and so on were recognized to be crucial for the ineffectiveness issue and would be crucial for the design of stabilization policy if it were to be successful. And such details are also critical for an extremely basic issue, namely, whether existing cyclical fluctuations have major or minor effects on social welfare. But the issues emphasized by researchers have changed drastically during the past decade, moving away from the specification of aggregate supply behavior and toward a basically new set of topics. To a considerable extent these new topics have been concerned with technical rather than substantive issues, but that demarcation is not a precise or tidy one, so it would be wrong to be highly critical on that basis--even if one feels, as I do, that a bit more emphasis should be given to substance. And in one area of study, a formerly arid domain of purely theoretical concern has actually been converted into a vibrant field of great substantive importance. Here I refer, of course, to the area of growth analysis.

In this talk I would like to have a few things to say about this rejuvenation of growth analysis. In addition, I will discuss some issues relating to two extremely active areas of research over the past decade, namely, real business cycle (RBC) theory and unit-root econometrics. Then at the end I will briefly return to the aggregate supply issues that continue, whatever the profession's fads, to be important.

Before starting, however, I should perhaps mention briefly that there continues to be work and some controversy relating to the RE hypothesis itself. A large number of studies have indicated that either RE, or a certain class of representative-agent models of saving, labor supply, and asset choice behavior, is inconsistent with the data. Most researchers would view the class of models as the more likely culprit, however. More troublesome to some researchers are the results of studies based on survey data regarding expectations, a number of which have suggested that errors are systematically related to available information in one fashion or another. One prominent example involving exchange rates is provided by the work of Ken Froot and Jeff Frankel (1989).

Some analysis has moved the other way, however. A recent AER study by Michael Keane and David Runkle (1990) devoted an unusual amount of attention to details such as data availability and revision, potential aggregation bias, and the incentives of sampled forecasters. And their study yielded results in which survey data on GNP deflator forecasts are consistent with rationality. Theoretical work continues, moreover, on the feasibility of rational expectations in environments with many individuals who are differentially informed. But rational expectations continues to be the dominant hypothesis for the same reason as 10 years ago--the unattractiveness of the available alternatives. Thus it is probably correct to say that the main controversy in this general area concerns the empirical relevance of "bubble" or "sunspot" solutions in RE models. This possibility has received additional attention in recent years as a consequence of the development of models involving complex nonlinear dynamics of the type that is termed "chaotic." I am myself quite dubious of the empirical relevance of this work, but I will not have time to develop that argument in this hour.

II. Real-Business Cycle Analysis

Instead, let me begin now with the subject of RBC analysis, which must certainly be regarded as one of the most active areas of macroeconomic research over the past decade.[1] I'm sure that everyone here is familiar with the general nature of this line of work, which views cyclical fluctuations in aggregate quantity variables as resulting primarily as the consequence of exogenous shocks to the production function that is common to all producers. Product and factor prices are fully flexible, or the economy works as if they were, so monetary policy actions have no significant impact on real magnitudes. Those are the main substantive features of the RBC approach, but it also has two notable methodological aspects. These are, first, that macroeconomic analysis should be conducted in the context of a competitive general equilibrium model in which agents solve explicit dynamic optimization problems and, second, that this model should be implemented quantitatively with parameter values that are empirically justified by means of "calibration" or some more formal estimation method.

As you know, this approach got its first major impetus from the 1982 Econometrica paper by Kydland and Prescott, which was followed up by Prescott's 1986 paper in the C-R conference series. By now several survey papers have been written; naturally I favor my own, which appears in Barro's volume called Modern Business Cycle Theory (McCallum, 1989b). In that survey I tried to bring out both the strengths and weaknesses of the RBC approach, with its strengths being largely methodological and its weaknesses empirical. In the latter category I emphasized that existing RBC models, driven entirely by technology shocks, fail to match the second-moment data that RBC researchers have focused on in two important respects. First, average labor productivity is much more highly correlated with output and employment in the models than it is in the U.S. data and, second, the dynamic pattern of output-productivity correlations is also the source of a mismatch. Recently, Christiano and Eichenbaum (1992) have elaborated on the former point and showed that inclusion of a second source of fluctuations--shocks in government fiscal policy variables--would be helpful but would not eliminate the problem.

Another difficulty with RBC analysis is the absence of any convincing description or identification of the unobserved quarterly "technology shocks" that it posits as the primary source of cyclical fluctuations. If this random component is interpreted literally as pertaining to shifts in the state-of-knowledge physical frontier relationship between inputs and outputs, then it would seem implausible that there could be enough variability at the aggregate level; specific technological innovations will impact on the production functions of only a few of the economy's many products. These sectoral shocks will then tend to average out, yielding a small variance in the aggregate. In this regard, there is some evidence that the so-called "Solow residuals" are indeed not pure exogenous technology shocks--specifically, there is a recent JME paper by Charles Evans (1992) that carefully documents the fact that Prescott's measure of the Solow residuals is Granger-caused by several monetary variables.

The second possibility is that these "technology shocks" refer largely to variations in fiscal policy, import prices, or other omitted but observable variables. This line of approach is currently being taken by a number of researchers, the cited paper by Christiano and Eichenbaum being only one of several recent examples. Now in this case the substantive hypothesis under consideration is rather different. So it might be asked whether models in which cyclical fluctuations are driven largely by changes in fiscal policy or import prices can still properly be considered RBC models. My answer would be that they can, when they utilize the methodological approach of the RBC literature and retain its presumption that prices adjust promptly, thereby ruling out monetary effects of the Phillips-curve type. One might even say that a model that includes monetary variables continues to be a RBC model if full price flexibility is maintained. This is the case in the inflation tax analysis of Cooley and

Hansen (1989), which introduces money via a cash-in-advance constraint and then studies the welfare cost of alternative inflation rates. There are other papers of this type, whose models represent a useful extension of RBC analysis. But it should be kept in mind that adding on a monetary sector in this way does not recognize the type of phenomena that is considered important by those who believe that cycles have a significant monetary component. That would require the inclusion of sticky prices. And it would not seem to be appropriate to apply the RBC label to such a model--i.e., one with sticky prices and/or wages--even if the analysis of cyclical characteristics is conducted in the fashion that typifies the RBC literature. That is not to suggest that such analysis is undesirable, however. In fact, my own belief is just the opposite--that research applying the disciplined quantitative analysis of cyclical behavior to various hypotheses regarding sluggish price adjustment could be extremely valuable. A good start has been provided in a working paper by Robert King (1991).

An interesting development in recent RBC work has been in the area of models with an international dimension.[2] Here one object is to see if the basic neoclassical mechanisms involving intertemporal choices and work effort serve to explain empirical regularities involving variability and correlation measures for variables such as net exports, saving, domestic investment, domestic and foreign consumption, etc. One of the most positive findings is that the RBC models, which incorporate high mobility of goods and capital, tend to reproduce the empirical fact that national saving and domestic investment ratios (relative to output) are positively correlated. This indicates that the high savings and investment correlations that are observed in the data do not imply a low degree of capital mobility, as was suggested in the literature engendered by Feldstein and Horioka (1980). At the negative end of the spectrum of findings, the RBC-style models tend to imply that cross-country correlations are much higher for consumption aggregates than for output, an implication that is sharply inconsistent with the facts. Much work on international aspects of RBC analysis is currently under way.

III. Growth Theory

One of the most desirable by-products of the RBC literature has been a greatly renewed interest in the analysis of economic growth. Indeed, it might not be too much of an exaggeration to say that recent work in this area has made growth economics a viable substantive area--as opposed to a fascinating but empirically empty theoretical domain--for the first time. Over the past few years serious analysts have actually been running regressions with data pertaining to growth rates and their determinants! In any event, the ties between the recent growth analysis, and the RBC work that preceded it, have been strong both intellectually and in terms of the individual scholars involved--see, for example, two papers by King, Plosser, and Rebelo (1988a), (1988b).

As I see it--I have not worked in the area myself--the main catalyst for the new growth analysis was some work by Paul Romer (1986) (1987) and shortly thereafter Bob Lucas (1988), both of whom argued that the prevailing neoclassical growth framework with exogenous technical progress fails to explain even the most basic facts of actual growth behavior. Both Romer and Lucas went on, moreover, to propose new theoretical structures that would endogenize the growth process. Their arguments have not gone unchallenged, but they have--however the controversy turns out--provided much of the stimulus for recent developments, both theoretical and empirical.[3]

Basically the story goes something like this. The neoclassical growth model consists of a setup in which infinite-lived households make consumption vs. capital accumulation decisions so as to maximize a time-separable utility function that has current and future consumption of the household as its arguments. (It doesn't change things much to include the leisure/labor choice also.) These choices are constrained by a production function pertaining to the household's inputs and outputs, and its current stock of capital. Labor-augmenting technical progress and population growth rates are exogenously given constants. Then in a competitive economy with many such households, capital accumulation will proceed along a path that approaches a steady state in which the value of capital per <u>effective</u> unit of labor--i.e., taking account of technical progress--is a constant. So capital per person grows at the rate of technical progress in the steady state and the same is true for consumption and output per person. Thus the steady state growth rate of output--either total or per person--is independent of preference and technology parameters, indeed, everything except the exogenous rate of technical progress. Inefficiencies induced by distorting taxes will, like the state of tastes and technology, affect <u>levels</u> of output and consumption but not their steady state growth rates.

The trouble with this neoclassical model is that it explains so little about observed growth behavior. We know that different economies have different per capita growth rates that are sustained over long spans of time and that these rates are higher in economies that devote large shares of their output to investment. But such differences cannot be explained by the <u>steady state implications</u> of the neoclassical model since it takes technology growth to be exogenous. The model suggests either the same rate for all economies or different rates about which it has nothing to say.

Of course the neoclassical model does imply that <u>transitional</u> growth rates will differ, being faster in economies with capital-to-effective-labor-ratios far below their steady state values. But these transitional differences cannot quantitatively explain the magnitude of long-lasting growth differences under the standard neoclassical presumption that the production function is close to the Cobb-Douglas form with a capital elasticity of about 1/3. One way to describe the problem is to consider a comparison in which one economy's output per person increases by a factor of 2.4 relative to another's, over a period of 30 years, which is the amount of change that would take place if the first economy grew on average at a rate that is higher by 3% per year. That's a pretty big difference but not nearly as big as the difference between actual Japanese and U.S. growth rates over the years 1950-1980. Then with a capital elasticity of 1/3, the capital stock per person would have to increase by a factor of 14, relative to the second economy, if their rates of technical progress were the same. And the real rate of interest--the marginal product of capital--in the first economy would fall by a relative factor of 6. So if the two economies had similar real interest rates at the end of the 30 year period, the first economy's would have been 6 times as high as the second's at the start of the period. Well, we do not observe in the data changes in relative capital/labor ratios or interest rates anything like those magnitudes even though we do observe output growth differentials of 3%. And this calculation understates the difficulty for the neoclassical model, because transition dynamics is such that the absolute rate of approach to the steady state path becomes very slow as that path is approached. A calculation by King and Rebelo (1993) suggests that for Japan's growth relative to the U.S. over 1950-1980 to be explained by the transitional capital accumulation mechanism, with a capital elasticity of 1/3, Japan's real marginal product of capital would have been about 500% per year in 1950!

Besides this clear inconsistency with the data, the neoclassical model's implication that an economy's steady state growth rate is independent of preferences for saving versus consumption seems at odds with empirical observation. And the same might be said with respect to tax rates, although the empirical situation is not so clear.

Consequently, Romer, Lucas, King and Rebelo, and others have developed models in which steady growth can be determined endogenously--that is, it may occur even without exogenous technical progress--at rates that will depend on saving behavior and tax policy. The key to these models is the role of human capital, which is assumed to affect labor productivity. In other words, the efficiency term that multiplies labor inputs in a household's production function is proportional to the amount of human capital that it has accumulated. This makes technical progress endogenous and responsive to the incentives of individual agents. Now, if the technology for the production of human capital features constant returns to scale in capital and efficiency units of labor, then the model will generate steady state growth of human capital, physical capital, output, and consumption per person.[4] This happens because with constant returns to the two accumulated factors, physical and human capital, growth is not halted by diminishing returns. The result is somewhat as if output depended only on capital alone with constant returns to this one factor. And the growth rate of per capita values that obtains in the steady state will depend on many parameters of the model, including distorting tax rates. There are versions of the endogenous growth model that posit the existence of externalities to human capital accumulation, but the version I have just sketched does not rely on any such assumption.

So, does this type of model make more sense than the neoclassical construct that it was designed to replace? Well, it seems attractive and basically plausible but there is one weak link in the argument. That is that steady, never-ending growth requires <u>precisely</u> constant returns in the production functions; if the sum of the coefficients on capital and human capital is 0.99 rather than 1.00 in either production function, then the economy will approach a steady state with no growth in the per capita magnitudes. So its dramatically different properties as compared with the neoclassical model depend upon very special parameter values, ones that obtain only on a subset of the parameter space that is of measure zero. That hurts! But the analysis has been productive, nevertheless, because with returns to scale <u>close to 1.0</u>, the model will have very slow transition dynamics, which implies that observed growth differences may be sustained over very long time periods. Of course the same would be true in the neoclassical model if returns to capital were close to 1.0, but we know that for physical capital alone the magnitude is closer to 1/3. So one major contribution of the new growth analysis is the idea that human capital accumulation may be very important to the growth process. And with its importance recognized, analytical growth models can become relevant for actual growth processes. The models will still fail to explain never-ending steady state growth, except with an implausible assumption, but they will plausibly rationalize transitional growth differences that are quite long-lasting. And from a practical perspective that is what is needed for analysis.

IV. Unit Roots

The issue of parameters summing precisely to 1.0, a zero-measure subset of the parameter space, arises again in this section because its topic concerns the possible existence of "unit roots" in macroeconomic time series. This too has been an extremely active area of research so I can discuss only a small fraction of the issues that have been studied. In fact, I will briefly touch upon just two of these issues, the first of which concerns the hypothesis that the time series process for real GNP in the U.S. is one that has a unit autoregressive (AR) root. Equivalently, it is a difference stationary (DS) or integrated series, one that must be first-differenced to be rendered stationary. (Actually, I am talking about the log of real GNP, but will not say so explicitly.)

The first studies of this proposition, brought to the profession's attention by Nelson & Plosser (1982), proceeded by testing a null hypothesis that corresponds to a unit AR root. Thus the strategy was to represent the GNP series as a trend stationary ARMA process and test the special-case, zero-measure hypothesis that its largest AR root is precisely equal to 1.0. Clearly, that testing strategy is unsatisfying when the null hypothesis is accepted, since it is almost bound to be accepted falsely if the true value is close to but different from 1.0.

Consequently, Campbell and Mankiw (1987) adopted a different strategy. It was to represent GNP as a difference-stationary ARMA process, one stationary in the first difference of real GNP, and then test the special zero-measure hypothesis that there is a unit root in the moving average (MA) polynomial. (This would cancel out the difference operator and make the GNP series trend stationary). What did they find? Well, the answer cannot be given in a few words because it depends on how many AR and MA terms are included in the estimated model for Δy_t. I would summarize the Campbell and Mankiw (C&M) results as in Tables 1 and 2. They actually estimated 16 models but I've reported only those which include at least one AR and one MA parameter. The first of these tables gives statistics for a test of the hypothesis that the process is trend stationary (TS). Clearly you get different results for different ARMA specifications. But the matter is even messier than that, because the test statistic actually does not have a chi square distribution under the null hypothesis. Consequently, the usual test (that I have reported) will tend to reject a true hypothesis more infrequently than the nominal significance level implies. So the C&M results are actually more unfavorable for the trend stationarity hypothesis than it would appear in Table 1.

Partly because of this difficulty, Campbell & Mankiw's discussion emphasized the ARMA (2,2) case, which was chosen by some plausible selection criteria, and the A(1) statistic reported in Table 2. The latter statistic is the sum of coefficients in the A(L) polynomial, when Δy_t is expressed in the MA form $\Delta y_t = A(L)\varepsilon_t$, so it represents the ultimate, long run response of y_t to a unit shock--a 1.0 realization for ε_t. As can readily be verified, the A(1) measure is 0 for <u>any</u> trend stationary process and is 1.0 for a random walk. So many of the ARMA specifications indicate not only that the process is DS, but also that it is even farther from TS than is a pure random walk. So, although their language was guarded, C&M suggested that there are

Table 1

Test Statistics from Campbell and Mankiw (1987, Table 1)

Number of AR Parameters	Number of MA Parameters 1	2	3
1	22.96*	11.73*	0.00
2	2.06	4.02*	0.00
3	0.95	1.31	0.00

Notes: Tabulated entries are values of 2 log (SSE^{o}/SSE), where SSE denotes sum of squared residuals and SSE^{o} indicates imposition of the constraint that $A(1) = 0$. The ARMA models are estimated for Δy_t where y_t is the log of U.S. real GNP, seasonally adjusted, quarterly for 1947.1 - 1985.4. Asterisks indicate values significantly different from zero at the 0.05 significance level according to the usual chi-square test, which is (however) incorrect.

Table 2

Estimates of A(1) from Campbell and Mankiw (1987, Table IV)

Number of AR Parameters	Number of MA Parameters 1	2	3
1	1.72	1.73	0.03
2	1.77	1.52	0.00
3	1.36	1.60	0.00

grounds for believing that GNP is DS and has a A(1) measure greater than 1.0.

That conclusion was contested by Christiano and Eichenbaum (1990), who suggested that the C&M results provide little evidence on A(1). Their argument is that one could easily get results like those in Table 2 even if A(1) = 0 in actuality. They provided many bits of evidence but the most convincing was as follows. They took C&M's estimated ARMA (3,3) model, which implies A(1) = 0, used it to generate simulated data, and then estimated ARMA (2,2) models from this data. And in each case they tested the hypothesis, true by construction, that A(1) = 0. But in 2000 replications that true hypothesis was rejected 74% of the time although the test's significance level was 0.05.

What is the appropriate conclusion? I would tend to agree with the Christiano-Eichenbaum suggestion that we are simply unable to accurately estimate long-run properties of the U.S. GNP data from the postwar quarterly time series. That same conclusion has also been suggested by analysis based on two other approaches. The first is the unobserved components approach used by Watson (1986) and Clark (1987), which views the y_t series as the sum of two components, one of which is TS and the other DS. This implies that the y_t series is itself DS but with a TS component that is possibly more important quantitatively than the DS component. Thus the object is to estimate the relative importance of the two components, neither of which is a zero-measure special case. Studies using this approach tend to estimate A(1) substantially below 1.0, closer to 0.5, but with a very large standard error. The other approach involves fractional differencing, in which case the parameter attached to the 1-L operator may be a non-integer value, rather than the 0 or 1 that would represent trend stationarity or difference stationarity, respectively. Estimates of this parameter tend to lie around 0.5, but again with very large standard errors--see Sowell (1992).

From all of this it seems hard not to conclude that U.S. real GNP is probably a DS process--why would there be <u>no</u> permanent component in the shocks driving it?--but quite possibly with a long run response statistic considerably smaller than the random walk value of 1.0. But we cannot be confident about the latter--there is just not enough information in the data to estimate long-run properties with much accuracy.[5]

The other aspect of unit root analysis that I would like to mention switches attention from trend estimation to trend removal; to "detrending" for the purpose of making macro time series suitable for econometric study of relationships among variables. For example, when conducting regression analysis with growing series, is it better to difference the series or to remove estimated deterministic trends?

Some notable arguments in favor of differencing have been put forth in the literature. The most famous of these was in Granger and Newbold's (1974) paper which showed that a regression of y_t on x_t would tend spuriously to find a relationship when the two variables are generated by <u>independent</u> random walk processes. Then Nelson and Kang (1984) showed that <u>trendless</u> random walk variables would be spuriously related to time trends in estimated regressions. Finally, Plosser and Schwert suggested in a 1978 <u>JME</u> article that under-differencing will usually have more serious consequences than over-differencing. Now, all three of these arguments are based on the presumption that the investigator is wrong about the appropriate degree of differencing <u>and</u> that he fails to correct for serial correlation of residuals in the estimated regression. But to me that seems like a straw man, because most actual investigators would not conclude their study with a regression that has strong serial correlation in the residuals. The actual case of interest, then, would seem to be estimation with under- or over-differenced series plus AR or MA corrections for the disturbance terms.

I have found a few papers reporting the results of Monte Carlo studies that bear on this case. One is a different paper by Plosser and Schwert (1977); it indicates that over-differencing is not seriously detrimental in inference regarding the regression slope parameter when a moving average disturbance is included. It does not consider the corresponding case with under-differencing accompanied by an autoregressive disturbance term, but a few relevant results have been reported by Andrew Harvey (1980) and (ironically) by Nelson and Kang. In both cases these results suggest that inferences about slope coefficients will not be strongly distorted. I won't take time to describe the studies in any more detail, partly because all of these Monte Carlo results are for setups with exogenous regressors.[6] Since that case would be the exception in macroeconomics, I have recently conducted a very small investigation in the context of an equation that I have used in some recent work. This is equation (24) of Table 3, an extremely simple one equation model of nominal GNP determination. Note that the predetermined variable Δb_{t-1}, measuring the one-quarter lagged growth rate of the monetary base, and the lagged dependent variable both enter with moderate sized positive coefficients when the relation is estimated in first differences. Now equation (25) estimates a levels version of this relation with no serial correlation correction; this yields very different slope estimates. But when an autoregressive disturbance term is included, the point estimates and standard errors in equation (26) return to pretty much the same as in (24).

Table 3

Estimates Reported in McCallum (1992)

(24) $$\underset{}{\Delta x_t} = \underset{(.002)}{0.0076} + \underset{(.075)}{0.2845}\Delta x_{t-1} + \underset{(.105)}{0.3831}\Delta b_{t-1}$$

$R^2 = 0.215$ $SE = 0.0096$ $DW = 2.07$ $Q(10) = 8.0$

(25) $$x_t = \underset{(.072)}{0.0273} + \underset{(.0002)}{0.00021}t + \underset{(.020)}{1.0160}x_{t-1} - \underset{(.014)}{0.0321}b_{t-1}$$

$R^2 = 0.9999$ $SE = 0.0104$ $DW = 1.40$ $Q(10) = 23.1$

(26) $$x_t = \underset{(34.1)}{5.857} - \underset{(.060)}{0.0067}t + \underset{(.081)}{0.2763}x_{t-1} + \underset{(.150)}{0.592}b_{t-1} + \underset{(.023)}{0.996}e_{t-1}$$

$R^2 = 0.9999$ $SE = 0.0095$ $DW = 2.14$ $Q(10) = 9.0$,

(27) $$\Delta\Delta x_t = \underset{(.0009)}{0.0002} - \underset{(.074)}{0.3993}\Delta\Delta x_{t-1} + \underset{(.158)}{0.363}\Delta\Delta b_{t-1}$$

$R^2 = 0.182$ $SE = 0.0110$ $DW = 2.12$ $Q(10) = 18.3$

(28) $$\Delta\Delta x_t = \underset{(.0008)}{0.00001} + \underset{(.065)}{0.1666}\Delta\Delta x_{t-1} + \underset{(.139)}{0.3571}\Delta\Delta b_{t-1} - \underset{(.032)}{0.946}e_{t-1}$$

$R^2 = 0.370$ $SE = 0.0097$ $DW = 1.89$ $Q(10) = 9.5$

And equations (27) and (28) show that something similar is true for a second differenced version of the relation when a moving average disturbance is included. It is too soon to draw general conclusions, but these results lead me to conjecture that the issue of differencing prior to regression analysis is not so crucial as has been suggested. If serial correlation corrections are utilized, the results may not be greatly affected by under- or over-differencing.

V. Aggregate Supply

Let us now return to the topic of aggregate supply, which might be thought of as the manner in which price behavior departs from fully flexible, instantaneous market-clearing (the type presumed in real business cycle and growth analysis). Given this characterization, it may seem surprising that I have downplayed the extent of research activity in the area, for there have been a substantial number of papers published on the interaction of monopolistic competition and "menu costs". Indeed, a two-volume collection of reprints of notable articles has been edited by Greg Mankiw and David Romer (1991) under the title of New Keynesian Economics. My main reason for de-emphasizing this body of work is that it seems thus far to have been rather unsuccessful.[7] To introduce my explanation of this conclusion, let me quote Blanchard's (1990, p. 813) concise statement of the main idea: "To summarize, imperfect competition implies that, in response to an increase in nominal money, the incentive to adjust relative prices may be weak. Small costs of changing prices will [in that case] prevent adjustment of relative prices, thus of nominal prices, leading to an increase in aggregate demand. Because price initially exceeds marginal cost, firms will willingly increase output even if they do not adjust prices. Output will go up and so will welfare."

This line of argument admittedly has some attractive features--but it also has several important weaknesses. One of the latter involves the welfare effect. Since monetary-induced contractions will occur with about the same frequency as expansions, the first-order welfare effects will cancel out over any substantial span of time.[8] Second, the emphasis on monopolistic aspects of imperfect competition seems overdone. Although the normal level of output will tend to be lower than under competitive conditions, it is unclear that this is important from the perspective of macroeconomic fluctuations. Blanchard and Kiyotaki (1987) refer to the suboptimality of output as involving an "aggregate demand externality" but this is misleading in the following respect: of the three equations of their aggregate model, only the "wage rule" and "price rule" are needed to determine output and the real wage. The "aggregate demand function" then serves only to determine the price level, and has no effect on the values of the model's real variables. Indeed, the assumption of monopolistic competition does not itself imply the existence of monetary policy effects on real variables. Such effects also require the existence of the above-mentioned menu costs, costs of changing prices that cause nominal prices to be unaffected by changes in the stock of money--which then alter real variables. But there is a problem with this part of the story, too, for it simply assumes that the menu costs pertain to changes in <u>nominal</u> prices. But rational sellers and buyers are concerned with quantities and relative prices, so it is unclear why prices are not temporarily fixed in "real" or "indexed" form. Why, in other words, are menus not printed for real rather than nominal prices? The menu cost literature has not addressed this issue but has instead simply assumed that menu costs pertain to nominal prices. But that amounts to assuming that monetary policy actions will have real effects, which is something that the theory is supposed to explain.

Finally, another weakness of the monopolistic competition-menu cost theory is that existing versions are basically non-dynamic and therefore non-operational. In some papers it is unclear whether the cost of a price change pertains to a change from one period to the next, or to a within-period change in relation to some preselected value; and in most of the other papers, it is the latter concept that prevails. But the former concept is, of course, the one that most people have in mind when referring to empirical evidence on sticky prices. In sum, it seems necessary to conclude that the menu cost literature has yielded no operational model of sticky price behavior that is satisfactory both theoretically and empirically.

What about other sources of such a relation? As it happens, the operational formulation that has recently come the closest to being a "consensus" model among empirically-oriented analysts is, I think it is safe to say, John Taylor's (1980) specification with overlapping nominal wage contracts. Since I have previously (1982) criticized this model for not conforming to the natural rate hypothesis, I would not bring up that point again--except that it seems worth mentioning that Jeffrey Fuhrer and George Moore (1992) have very recently implemented a modified version that is open to this particular criticism to a lesser extent. Thus this modification is rather attractive conceptually, and the Fuhrer-Moore study suggests that it may perform better empirically, as well. Let us then briefly examine the nature of the proposed modification.

A two-period version of Taylor's model posits that the aggregate price (or wage) index is an average of contract prices negotiated in the current and previous periods, x_t and x_{t-1}:[9]

$$(1)\quad p_t = \frac{1}{2}\,(x_t + x_{t-1}).$$

Contract prices are set by half the sellers in t to keep in step with prices pertaining to the other half of the sellers, with an adjustment added to reflect expected excess demand:

$$(2)\quad x_t = \frac{1}{2}\left[p_t + E_t\, p_{t+1}\right] + \frac{\gamma}{2}E_t\left[\tilde{y}_t + \tilde{y}_{t+1}\right] \qquad\qquad \gamma > 0$$

Together these imply that

$$(3)\quad x_t = \frac{1}{2}\left[x_{t-1} + E_t\, x_{t+1}\right] + \gamma E_t\left[\tilde{y}_t + \tilde{y}_{t+1}\right]$$

and thus that

$$(4)\quad 0 = \frac{1}{2}\left[E_t \Delta x_{t+1} - \Delta x_t\right] + \gamma E_t\left[\tilde{y}_t + \tilde{y}_{t+1}\right].$$

The Fuhrer-Moore assumption, by contrast, is that x_t is set to satisfy

$$(5)\quad x_t - p_t = \frac{1}{2}\left[v_t + E_{t-1}v_{t+1}\right] + \frac{\gamma}{2}E_{t-1}\left[\tilde{y}_t + \tilde{y}_{t+1}\right]$$

where

$$(6) \quad v_t = \frac{1}{2}\left[x_t - p_t + x_{t-1} - p_{t-1}\right]$$

is the aggregate index of real contract prices prevailing in period t. Substituting we derive

$$(7) \quad x_t - p_t = \frac{1}{2}\left[x_{t-1} - p_{t-1} + E_t(x_{t+1} - p_{t+1})\right] + \gamma\, E_t\left[\tilde{y}_t + \tilde{y}_{t+1}\right]$$

rather than (3). And since (1) implies that $x_t - p_t = \frac{1}{2}\Delta x_t$, we can substitute into (7) and obtain

$$(8) \quad 0 = \frac{1}{2}\left[(E_t\Delta x_{t+1} - \Delta x_t) - (\Delta x_t - \Delta x_{t-1})\right] + 2\gamma E_t\left[\tilde{y}_t + \tilde{y}_{t+1}\right],$$

rather than (4). Here an accelerating inflation that keeps Δx_{t+1} above Δx_t will not tend to keep $\tilde{y}_t + \tilde{y}_{t+1}$ above zero, expectationally, as it does in (4). Thus the model does not imply that an accelerating inflation will have a permanent affect on output--it does not, in other words, violate Lucas's version of the natural rate hypothesis to that extent.[10] To me that is a highly attractive feature of the Fuhrer-Moore modification of Taylor's formulation.

Their own principal claim however, is that the modification induces stickiness of inflation rates (as opposed to price levels) and so leads to a model that matches actual data better than Taylor's in several respects. They present evidence that this is the case in their working paper (1992). I should say that their empirical versions of both specifications uses four-period overlapping contracts and index weights that decline as you go back in time.

My enthusiasm for the Fuhrer-Moore modification, which was briefly mentioned in a 1981 paper by Buiter and Jewett, should not be understood as critical of Taylor's work. Over the last decade Taylor has done more than anyone else to keep alive the connections between careful theorizing and serious macroeconometric model building. He has done this in a series of research papers that culminates in a book to be published this spring. But I do think that the Fuhrer-Moore study is possibly the most promising development in aggregate supply analysis that I have seen for several years, so I wanted to call it to your attention.

VI. Concluding Remarks

I have just a few more comments to make before stopping. One is to admit that, frankly, I have been rather depressed at times about the emphasis on method as opposed to substance in recent macroeconomic research. But in writing this paper it has been impressed on me that there have been important substantive developments, too. In particular, the rejuvenation of growth analysis is a most encouraging development because any progress in that subject has the potential for suggesting policy changes that could have truly major welfare effects. In saying that, I am not, however, expressing agreement with the idea that cyclical fluctuations are highly unimportant from a welfare perspective. Lucas's (1987) argument to that effect presumes that cycles are generated by a process that keeps fluctuations around a reference path, and the level of that path, entirely separate. But that is not an innocuous assumption; its validity cannot be established without more progress in the area of aggregate supply theory.

Finally, I would like to conclude by arguing that it is wrong to claim, as many commentators have, that the present state of macroeconomic understanding is very bad. It is true that there are many wildly divergent modelling strategies appearing in current research papers and a wide variety of policy ideas being put forth for consideration.[11] But much of the apparent disagreement results, I believe, from the process of personal competition among the growing population of research economists. In the U.S. there are hundreds of capable and ambitious assistant professors who need, to win tenure, to publish about a dozen articles in which "originality" is supposed to be demonstrated. And their more senior colleagues are also rewarded in relation to their accomplishments of this type. In such circumstances the volume of intellectual product differentiation naturally becomes extreme. But mature and thoughtful members of the profession--even ones whose articles feature very different models--will nevertheless take quite similar positions on most of the truly fundamental issues. One would get basically consistent answers from Friedman, Tobin, Lucas, and Solow to questions such as: Is sustained inflation likely without monetary accommodation? Would sustained inflation lead to faster growth over extended periods of time? Will an increase in government purchases increase aggregate demand? Will an open-market purchase of bonds increase or decrease demand? Can a nation's monetary authority simultaneously pursue both output and exchange rate targets?

It might be objected that there is significantly less disagreement in the domain of microeconomics even though the same situation exists with respect to scholars' incentives. But I would emphasize that the type of <u>practical</u> problem traditionally addressed in micro is quite different and in a manner that does less to bring out potential disagreements. Microeconomists, to be specific, do not attempt to explain or analyze quarter-to-quarter or even year-to-year fluctuations in prices or quantities of micro variables. Thus their scientific and policy challenges are less ambitious than those in the macroeconomic sphere. When this difference is neutralized, as in the questions mentioned a minute ago, the extent of disagreement is, I believe, about the same in the two subdisciplines.

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Footnotes

[1] I do not mean to imply, by beginning with this topic, that the RBC viewpoint is the inevitable (or even natural) consequence of the RE hypothesis. As in my 1982 survey, I continue to be attracted by sticky-price models with rational expectations.

[2] Examples include Backus, Kehoe, and Kydland (1992), Baxter and Crucini (1993), and Mendoza (1991).

[3] Other notable contributions include Barro and Sala-i-Martin (1992), Mankiw, Romer, and Weil (1992), and Rebelo (1991).

[4] Here reference is to a setup such as Rebelo's (1991), in which features a Cobb-Douglas production function $A_k[(1-\phi_t)K_t]^{\beta}\ [H_tN_t]^{1-\beta} - \delta_k K_t$ where ϕ_t denotes the fraction of physical capital (K_t) devoted to the production of human capital (H_t) and N_t is the fraction of time allocated to the production of goods. Simultaneously, human capital is produced according to $H_{t+1} - H_t = A_H[\phi_t K_t]^{\alpha}\ [(1-L-N_t)H_t]^{1-\alpha} - \delta_H H_t$, where L denotes leisure.

[5] For other useful arguments, see Cochrane (1991) and Stock and Watson (1988).

[6] The interested reader is referred to the summary in McCallum (1992).

[7] Here, and in my concluding comments in Section VI, I draw on McCallum (1989a).

[8] This point was mentioned in the survey by Rotemberg (1987).

[9] Here the variables are expressed in logarithmic terms; p_t is the log of the price level, x_t is the log of the contract price, and $\tilde{y}_t$ is the log of the ratio of actual to capacity output.

[10]It does imply that a maintained increase in the acceleration rate $\Delta\Delta x_t$ will have a continuing effect on $\tilde{y}_t + \tilde{y}_{t+1}$, however, so it still violates the strict version of the natural rate hypothesis.

[11]It is also the case that poor performance in forecasting exercises is often cited. But this represents a misunderstanding of the function of economic analysis, which is to forecast the likely consequences of changes in institutions or policy stances; conditional rather than unconditional forecasting.