NEW CLASSICALS AND KEYNESIANS, OR THE GOOD GUYS AND THE BAD GUYS

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ABSTRACT

Old-style Keynesian models relied on sticky prices or wages to explain

unemployment and to argue for demand-side macroeconomic policies. This

approach relied increasingly on a Phillips-curve view of the world, and

therefore lost considerable prestige with the events of the 1970s. The new

classical macroeconomics began at about that time, and focused initially on

the apparent real effects of monetary disturbances. Despite initial

successes, this analysis ultimately was unsatisfactory as an explanation for

an important role of money in business fluctuations. Nevertheless, the

approach achieved important methodological advances, such as rational

expectations and new methods of policy evaluation. Subsequent research by

new classicals has deemphasized monetary shocks, and focused instead on real

business cycle models and theories of endogenous economic growth. These

areas appear promising at this time. Another development is the so-called

new Keynesian economics, which includes long-term contracts, menu costs,

efficiency wages and insider-outsider theories, and macroeconomic models with

imperfect competition. Although some of these ideas may prove helpful as

elements in real business cycle models, my main conclusion is that the new

Keynesian economics has not been successful in rehabilitating the Keynesian

approach.

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Keynesian Models

When I was a graduate student at Harvard in the late 1960s, the Keynesian model was the only game in town as far as macroeconomics was concerned. Therefore, while I had doubts about the underpinnings of this analysis, it seemed worthwhile to work within the established framework to develop a model that was logically more consistent and hopefully empirically more useful. Collaborating with Herschel Grossman (Barro and Grossman, 1971), we made some progress in clarifying and extending the Keynesian model. But that research also made obvious the dependence of the central results on fragile underlying assumptions. The model stressed the failure of private enterprise economies to ensure full employment and production, and the consequent role for active macro policies as instruments to improve outcomes. Shocks to aggregate demand--but not aggregate supply--were the key to business fluctuations, and mere changes in optimism or pessimism turned out to be self fulfilling. These properties, which seem odd to economists who think in terms of price theory and well-functioning private markets, suggest coordination problems on a grand scale. But this perspective hardly accords with the basic source of market failure that characterizes the standard Keynesian model. It is the mere stickiness of prices or wages, primarily in the downward direction, that accounts for the principal results. Of course, many macroeconomists think of price stickiness as an "as if" device--a problem that is not to be viewed literally, but instead as a proxy for serious matters, such as incomplete information, adjustment costs, and other problems of coordination among economic agents. But this viewpoint has not been borne out by subsequent research. For example, the incorporation of these serious matters does not

support the Keynesian stress on aggregate demand, and also does not provide a normative basis for activist government policies of the usual Keynesian type.

One important function of a macroeconomic model is to isolate the sources of disturbances that cause aggregate business fluctuations. Keynesian analyses focus on shocks to aggregate demand, and typically attribute these shocks either to governmental actions (disruptive or corrective fiscal and monetary policies), or to shifts in private preferences that influence consumption or investment demand. Keynes's own discussion (Keynes, 1935, chapter 12) referred to the "animal spirits" of businessmen, and the consequent volatility of investment demand due to shifting moods of optimism or pessimism. Thus, aside from governmental actions, the Keynesian model is not strong at pinpointing observable, objective events that cause recessions or booms.

One reason that Keynes may not have been troubled by this "deficiency" is that he viewed the private economy as inherently unstable. It did not take large (and presumably objectively observable) shocks to trigger a recession, because even a small shock--when interacting with the multiplier (and, in some models, also the investment accelerator)--could generate a significant and sustained drop in output and employment. Curiously, however, later Keynesian developments deemphasized the multiplier. For example, in the well-known IS/LM model (in which interest rates adjust and matter for aggregate demand) or in Keynesian analyses that incorporate some version of the permanent-income hypothesis, multipliers need not exist. These extensions do improve the model's fit with some facts about business cycles, such as the apparent absence of a multiplicative response of output to changes in government purchases and the relative stability of consumption

over the business cycle. But the elimination of the multiplier means also that large responses of output, as in a substantial recession, require large impulses; hence, it again becomes important to identify the kinds of shocks that typically matter for aggregate fluctuations.

I think that the desire to find observable, aggregate shocks motivated many Keynesians--although not Keynes nor many of his immediate followers--to assign a substantial weight to monetary disturbances as a source of the business cycle. Within a framework where prices adjust slowly and output is determined by aggregate demand, it is easy to conclude that an increase in money raises output and also leads gradually to a higher price level. Moreover, the positive correlation between money and output--and perhaps between the price level and output--showed up in some data.

During the 1960s and early 1970s, Keynesian analysis became increasingly identified with this Phillips curve-view of the world. Thus, this analysis also lost considerable prestige when the Phillips curve disappeared in the mid 1970s; the rise in unemployment along with the increasing rate of inflation was difficult to explain in this kind of model. New Keynesians have, however, demonstrated their flexibility by arguing that the old Keynesian model merely need to be patched up to incorporate the supply side. But this argument does not work. In a single market, one can think of quantity as determined by demand with the excess supply rationed--as in the Keynesian model--so that changes in quantity depend only on shocks to demand. Then if this situation applies to the majority of markets, one can generate orthodox Keynesian prescriptions for demand-oriented governmental policies. Alternatively, quantity in a typical market could be determined by supply with the excess demand rationed--as in markets subject to effective price

controls--so that movements in quantity depend only on shocks to supply. If this situation holds for the majority of markets, one again gets prescriptions for the government's macro policies, but they are basically opposite to those from the Keynesian model. The serious alternative to either of these two polar cases is a framework where demand and supply are somehow balanced or *equilibrated* on the various markets. Although I regard this equilibrium approach as the logical way to think about macroeconomics, this approach--pursued by new classical macroeconomists--turns out to be inconsistent with basic Keynesian themes.

The New Classical Approach

The new classical macroeconomics, sometimes referred to as rational expectations macroeconomics or as the equilibrium approach to macroeconomics, began with Bob Lucas's research (Lucas, 1972, 1976) in the early 1970s. A guiding discipline of this work was that economic agents acted rationally in the context of their environment; notably that people assembled and used information in an efficient manner. Although the approach stressed fully worked out equilibrium theories, the analysis was directed at explaining real-world business fluctuations. The basic viewpoint implied that it would be unsatisfactory to "explain" these fluctuations by easily correctable market failures, such as those present in Keynesian models. Hence fluctuations had to reflect real or monetary disturbances, whose dynamic economic effects depended on costs of obtaining information, costs of adjustment, and so on.

The biggest challenge to the new classical approach was to explain why money was non-neutral, and, in particular, why monetary disturbances played a

major role in business cycles. This area was a significant challenge because first, it seemed to be empirically important, and second, the equilibrium framework with flexible prices tends to generate a close approximation to monetary neutrality.

Initially, the approach seemed to achieve notable successes. On a theoretical level, short-term real effects of monetary disturbances could arise from imperfect information about money and the general price level. Monetary shocks, which affected the general price level in the same direction, could be temporarily misperceived as shifts in relative prices, which led to adjustments in the supply of labor and other quantities. These real effects vanished in the long run, but could persist for awhile because of information lags and costs of adjusting the quantities of factor inputs. On the other hand, anticipated monetary changes--which include systematic monetary policies--would not matter because they did not lead to informational confusions (Sargent and Wallace, 1975).

On an empirical level, there was also evidence that appeared to support the approach. Monetary disturbances seemed to be important sources of business fluctuations, and there was some indication that it was mainly the unanticipated or surprise part of monetary movements that mattered for real variables (Barro, 1981). Some cross-country evidence supported the theoretical predictions concerning the relation between the volatility of money and the slopes of estimated Phillips curves (Kormendi and Meguire, 1984). The theory was also consistent with the observed absence of a substantial long-term relationship between real economic performance and the growth rates of money and prices; that is, with the absence of a long-run Phillips curve.

Further investigations cast doubt on these successes. First, the informational lag in observing money and the general price level did not seem to be very important. If incomplete information about money and the general price level mattered a lot for economic decisions, people could expend relatively little effort to find out quickly about these variables. Second, the theory did not do so well in terms of its predictions about monetary effects on real interest rates, real wage rates, and consumption. Third, the predicted Phillips curve-type relation between price surprises and real economic activity basically disappeared after the early 1970s. Fourth, the positive relation between monetary shocks and output shows up most clearly with broad monetary aggregates. The relation with narrow aggregates, such as the monetary base, is much weaker.

The upshot of these arguments is that the new classical approach does not do very well in accounting for an important role of money in business fluctuations. However, this failing may not be so serious because the empirical evidence on the causal role of money for real variables seems also to have been overstated. In other words, the accounting for major short-run non-neutralities of money was a misplaced priority for the new classical approach. Some empirical evidence supports this conclusion; for example, the observation that the correlation of real economic activity with broad monetary aggregates is greater than that with the monetary base or the price level, or the finding that real effects from the quantity of money are weak once the behavior of nominal interest rates is held constant. These results suggest that endogenous responses of money--partly from the behavior of policymakers and partly from the workings of the financial system--may

account for most of the correlations between money and real economic activity.

This verdict does not invalidate some of the major successes of the new classical approach. In terms of methodology, these successes include the application of equilibrium modeling to macroeconomic analysis, the use of rational expectations as part of this modeling, and the revolution in approaches to policy evaluation. One specific application in which the equilibrium approach has achieved some success is in analyses of fiscal policy (see Barro, 1989b, for a survey). Some of this research revolves around the *Ricardian equivalence theorem*, which provides conditions under which substitutions of budget deficits for taxes are of no consequence.[1] But further developments have brought out the real effects from government purchases and public services, the composition and timing of distorting taxes, and so on.

Another interesting off-shoot from the new classical approach is the application of game theory to the interaction between government policymakers and the private sector. The results here involve the distinction between *rules and discretion*, and the related roles of commitment, credibility, and reputation (see, Rogoff, 1989, for a survey). Some of the early analyses in this area dealt with monetary models; specifically, with the Phillips curve and the tradeoff between unemployment and inflation. But subsequent applications, such as to tax and regulatory policies and to international debt, do not rely on an important role for money in business fluctuations.

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[1] Bartley (1989) claims more than I ever would by describing Ricardian equivalence as "an Exocet aimed at the heart of the Keynesian notion that deficits stimulate the economy."

Real Business Cycle Theory

With the deemphasis on monetary models of the business cycle, most proponents of the new classical approach have moved over the last five to ten years to analyses that rely on real disturbances as sources of business fluctuations (see Kydland and Prescott, 1982, and the survey by McCallum, 1989). These models stress technology shocks or other disturbances to the supply side as central driving forces, but allow an important role for the dynamic elements that influence the ways that shocks propagate. The models are equilibrium in style, featuring cleared, competitive markets; optimizing agents who are typically modeled as representative households with infinite horizons; and neoclassical production functions that are subject to stochastic disturbances. Although the models deemphasize monetary shocks, the analysis of propagation mechanisms would apply as much to monetary models as to real models. In the real business cycle (or RBC) framework, any positive correlation between output and money reflects the endogenous response of monetary aggregates (see King and Plosser, 1984).

A number of authors have simulated versions of RBC models on U.S. data, where the underlying parameters of preferences and technology are calibrated to be consistent with findings from cross-sectional studies. In many respects the results accord with observed characteristics of business cycles. For example, RBC models can get right the relative variances of consumption, investment, capital stocks, and worker hours; and also account for the procyclical behavior of these variables. However, the models tend to overstate the procyclical patterns of hours, productivity, real interest rates, and real wage rates. In addition, to explain the standard deviation of output growth, the models require a standard deviation for technological

disturbances that may be excessive. However, so far, such judgments are based solely on introspection.

To explain recessions within the RBC framework, one has to admit technological or other supply-side disturbances that are adverse as well as favorable. Some critics have argued that technological regress is impossible (although I noted recently the possibility that the gun turret on the U.S. battleship Iowa could not be repaired because the expertise in this area had been lost). Other events that amount to negative shocks to production conditions are cartelization of markets (as with OPEC and perhaps with European style labor unions), harvest failures, strikes, and--for a single country--unfavorable movements in the terms of trade. It is also likely that a collapse of the financial and credit system--as in the United States during the Great Depression--can be viewed as an adverse real shock (see Bernanke, 1983). However, it would be desirable to have a theoretical model that could explain financial collapses as an endogenous response to government policies and other developments.

Early versions of RBC models exhibit Pareto-optimal behavior, and thereby show that observed fluctuations in aggregate business activity are insufficient reason for advocating governmental intervention in the form of stabilization policies. Adverse shocks and recessions are unfortunate in these models; it is just that the government cannot improve matters. RBC models can be extended to include external effects, such as those implied by public goods and taxation. The models are then well suited to incorporate supply-side, incentive effects from taxation, regulation, transfer programs, and so on. In this setting the outcomes are generally not Pareto optimal, and--subject to the usual problems of public choice--there may be useful

roles for government policy. But the distortions that underlie these results are of the classical, excess-burden variety, rather than the Keynesian type--that is, they involve "triangles" instead of "gaps." Consequently, desirable policy in these models gets more from public finance theory than from traditional macroeconomics.

Overall, the real-business-cycle area has generated many new insights and techniques that assist in modeling the macroeconomy and in thinking about government policies. But it is not yet clear how much the models contribute toward understanding actual business cycles, or to the construction of policies that governments might wish to implement.

Endogenous Growth Models

Another recent development, which is consistent methodologically with real-business-cycle theory, concerns models of endogenous economic growth (see, Romer, 1989, for a survey). Unlike the predecessors of the Solow (1956)-Koopmans (1965)-Cass (1965) type, these new models generate long-run growth within the models, and therefore can relate long-term differences in growth rates to underlying parameters of technology, preferences, and government policy. There are two major strands of this literature. One strand features constant returns to a broad concept of reproducible capital, which includes human capital and perhaps even the number of persons (see Rebelo, 1987). With this type of constant returns, the long-term growth rate, which is intimately related to the saving rate, is determined by productivity and time preference. It is also possible to determine population growth along with growth per capita. Because there are no underlying externalities, the decentralized choices of growth and saving tend

to be Pareto optimal. However, as in the RBC context, some extended versions of these growth models allow for a role of government by introducing public services and taxation (see Barro, 1989a). Then the usual public-finance choices arise, and these choices interact with the determination of growth and saving. One major theme is that governmental provision of infrastructure services and the protection of property rights can be especially important in fostering private saving and economic growth.

The second strand of the endogenous growth literature, identified especially with Romer (1986), brings in spillover effects that involve the creation of knowledge. At the level of an individual firm, production may be subject to diminishing returns. However, because some advances in techniques and information also benefit other firms, returns at a social level may be constant or even increasing. Similar effects can arise with the accumulation of human capital if the value of one person's stock of capital is benefited by the accumulation of human capital by others. Two major implications from these models are first, long-term per capita growth is sustainable and can be explained by the underlying structural elements of the model, and second, because social returns to research and perhaps the accumulation of human capital exceed the private returns, the decentralized choices of growth and saving tend to be too low from a social perspective. Thus, the analysis has implications for positive analyses of differences in growth and saving rates across countries, and also for the design of government policies. The obvious policy implications relate to subsidies for research and development, although additional results apply to education spending, restrictions on free trade, immigration regulations, and so on.

After a lapse of serious interest for about 20 years, the field of economic growth is once again exciting. The initial impetus came from some theoretical breakthroughs, but the attention is now turning to empirical analyses of the determinants of growth and saving across countries and over time. In order for the interest to be sustained--as it was not in the earlier period--I believe that success at the empirical level will be crucial. Since differences in long-term growth rates have such a dramatic effect on levels of welfare, the success of this type of empirical work is obviously of more than academic interest.

New Keynesian Models

According to a newspaper article that I read from Australia there is now a consensus among economists that a successful Keynesian revival is underway. (Unfortunately, the reporter neglected to mention that the consensus was acclaimed at a meeting of the Australian Economics Conference, where only Keynesians had been invited to attend.) No less than four new areas (the four horsemen of the new Keynesian economics?) are actively being pursued to provide Keynesian analysis with firm microeconomic underpinnings. Looked at this way, the mission of the new Keynesian economics (which I like to describe by the acronym NUKE) is peculiar. Instead of providing new theoretical results and hypotheses for empirical testing, the objective often seems to be to provide respectability for the basic viewpoint and policy prescriptions that characterize the old Keynesian models. It may well be more rewarding to look instead for new theoretical insights, empirical hypotheses, and policy implications.

The first NUKE area--implicit or explicit *long-term contracts* for labor or goods--is intended to rationalize sticky wages or prices. Although these models may explain why some wages or prices are sticky, the approach has been less successful in relating this stickiness to Keynesian style behavior of employment and output. Basically, the introduction of an ability to undertake long-term contracts tends to make private markets function more efficiently, rather than less efficiently as in the Keynesian model. If the basic problem in business fluctuations is an inability of agents to coordinate decisions, then it would indeed be surprising if this problem originated from an ability to make contracts.

As an example, in the context of a long-term labor agreement, it is possible to attain the appropriate variations over time in work effort without requiring day-to-day adjustments in pay. Workers agree at the outset--either formally or informally--that they will expend more effort when there is more work to do, with the understanding that they will also receive more leisure when there is less work (see Barro, 1977, and Hall and Lilien, 1979). As long as the variations in effort are not too great or long-lasting, it is unnecessary for wages to rise along with the extra work and vice versa. Thus, this analysis explains why the private economy can behave efficiently--as if markets cleared continuously--even if observed wages are sticky. The underlying shadow value of time is flexible, and the observed wages are merely installment payments that are part of a broader compensation package. Thus, in this view, it is also not surprising or disturbing if observed real wages do not correlate especially well with variations in labor supply. (There are some differences here in the predictions for movements in hours or effort from existing employees versus

changes in the number of workers, because new employees are likely not covered by previous labor contracts.)

Another point is that long-term contracting is an element of a real theory, and does not explain why monetary disturbances or the Phillips curve would be important. Moreover, it is just as likely that the real wages or relative prices determined in a long-term agreement would be "too low" as "too high." Thus, the implications for excess supply or demand are symmetric, and do not tend to support the Keynesian focus on aggregate demand.

The second area of the new Keynesian economics allows for *menu costs* in the adjustment of prices or wages (see Ball, Mankiw, and Romer, 1988, for a survey). Unlike long-term contracts, the idea here is that nominal prices are costly to change--thus, this theory does relate to monetary disturbances and to the interplay between nominal and real variables. In the absence of long-term contracts (as above), the "errors" in price formation could translate into inefficient choices of quantities. However, as with long-term contracts, this viewpoint does not point especially to the Keynesian case where nominal prices are too high rather than too low. That is, Keynesian excess supply would be no more likely than sustained excess demand.

As a theoretical matter, it has long been known that direct costs of adjustment could explain some stickiness in prices. However, the basic misgiving about menu costs is that the direct costs of adjusting prices are typically trivial relative to the losses from choosing inappropriate quantities. (The costs for changing prices tend also to be much less significant than those for changing quantities.) Thus, the main contribution of the new literature on menu costs was to show that--starting from a

position of market clearing--an error in price setting could involve costs
that are second order privately but first order socially. (Under imperfect
competition, the "market-clearing" price could also be allowed to deviate
from marginal cost.) Unfortunately, this result does not hold if output and
employment are already finite amounts away from their equilibrium values. In
this situation, the private cost from setting a price a little further from
its market-clearing value is also first order. Thus, if the costs of price
adjustment are minor, this approach still fails to explain significant
shortfalls in production and employment. New classical models with money
were often criticized for their reliance on faulty perceptions about the
general price level to explain major recessions. Since it was cheap to learn
about the general price level, the overall analysis was unconvincing. But it
is even more unconvincing to argue that major contractions of output and
employment arise because firms are unwilling to pay the small menu costs
required to change their prices.

The third NUKE area, *efficiency wages*, starts from the reasonable view
that the terms of a labor compensation package can affect workers' incentives
to provide effort. For example, the more attractive a job and the less
attractive is unemployment, the more willing someone is to work to avoid
being fired. (Marx also had this idea in his reserve-army model.) These
features can be incorporated as influences on labor supply in real business
cycle models (Danthine and Donaldson, 1988); an extension that is
straightforward because efficiency-wage theory applies to real variables
rather than to monetary forces. However, the incorporation of the
efficiency-wage idea tends to exacerbate one of the shortcomings of RBC
models. Namely (at least in the absence of long-term labor contracts), the

predicted pattern for real wages turns out, counterfactually, to be even more procyclical.

Instead of proceeding by introducing efficiency wages into an equilibrium model and then evaluating the empirical implications, most proponents of this approach have focused on the theoretical possibilities for generating Keynesian style "involuntary unemployment" (see, for example, Akerlof, 1984, and Shapiro and Stiglitz, 1984). Carmichael (1985) showed that this approach depended on some missing prices; that is, attractive jobs were effectively not sold up front. Even if job seekers have little access to credit markets or are worried about exploitation from employers, this process would work to clear the market; that is, to eliminate queues. Thus, to avoid this straightforward elimination of involuntary unemployment, one has to assume that the market for new jobs does not function or that the prices paid for jobs are exogenously sticky. In effect then, the efficiency-wage theory of unemployment is another example of old-style Keynesian theories in which some prices are arbitrarily treated as rigid. Other critics of efficiency-wage theories have noted that bonding and monitoring on the job can substitute for high wages as incentive mechanisms (see Katz, 1986). Also, if efficiency wages are important only on some jobs, there is another reason why the approach cannot account for involuntary unemployment (although it still may be significant in modifying the properties of a well specified equilibrium model).

Another area that is sometimes mentioned along with efficiency-wage theories is the *insider-outsider model* of the labor market (Lindbeck and Snower, 1988). This approach shows how insiders can effectively obtain a monopoly position over labor allocations. Thereby the determination of

employment and output can be Pareto inefficient. Moreover, the process sometimes leads to a high degree of persistence in unemployment, which is often discussed under the heading of *hysteresis* (Blanchard and Summers, 1987). In many ways this analysis is similar to the treatment of imperfect competition in the product market, which is the topic discussed next.

The last of the four main areas of the new Keynesian economics concerns models of business fluctuations that include *imperfect competition* with some elements of increasing returns (see, for example, Hall, 1988). As with efficiency wages, imperfect competition is a purely real theory that could be incorporated into real business cycle models. Aspects of imperfect competition are, in fact, central to some endogenous growth models. Although the results on fluctuations and growth under imperfect competition tend not to be Pareto efficient (as is also true in the presence of distorting taxation or public goods), there is no reason to think that the findings would support Keynesian arguments for aggregate demand policies. In any case, the important challenge is to show why the incorporation of aspects of imperfect competition leads to model characteristics that accord better with empirical evidence on business fluctuations. This demonstration has not yet been made, and one reason to be skeptical is that the approach does not identify any new elements as sources of fluctuations. (It also does not seem to lead to multipliers, which might lessen the need to identify sources of shocks.) Presumably, cyclical variations in the degree of monopoly are not the key to the business cycle.

Some of the ideas in the new Keynesian models, such as incentive mechanisms for labor effort and imperfect competition, may turn out to be useful for understanding the macroeconomy. But it is hard to see how these

ideas constitute a well-defined area of research that will actually
rehabilitate Keynesian analysis. At this point, I fear that the Australian
journalist's perception of an emerging consensus in macroeconomics is very
far from the truth. Macroeconomic research seems to be evolving into two
camps: could it be the good guys versus the bad guys?

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