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Editorial, NBER Macroeconomics Annual 1991

This, the sixth, edition of the *NBER Macroeconomics Annual* contains six papers, with a mix reflecting the *Annual*'s goal of both presenting frontier work in macroeconomics and applying economic analysis to current problems.

Among the frontier papers, those by Robert Hall and by Julio Rotemberg and Michael Woodford, deal with the perennial macroeconomic issue of the origins and mechanisms of business cycles. Both reflect current research efforts to identify the role of imperfect competition and increasing returns in business cycles. The paper by John Campbell and Pierre Perron summarizes and extends work on unit roots in macroeconomic time series; it brings nonspecialists up to date on the implications of the possibility that macroeconomic variables, such as GNP, M1 or interest rates, do not return to a nonstochastic trend.

Three of the papers address current policy issues. Jean Tirole analyzes problems of privatization in Eastern Europe, examining the economic rationale and implications of different structures of ownership and control. Kenneth Froot and Kenneth Rogoff study the implications of the transition to a monetary union in Europe on the behavior of central banks and markets in the transition. Stanley Fischer examines whether and how macroeconomic policies affect growth.

We believe that, once again, these papers offer a good sample of the current directions of research in macroeconomics. They show the broadening of our theories of business cycles, as well as the broadening of interest to include the macroeconomic implications of alternative institutional arrangements, and the mechanisms behind sustained growth. We limit ourselves in this introduction to brief descriptions of the papers; an important contribution of the conference, however, lies in the formal and informal comments that follow each paper.

Much of the recent discussion of the sources of business cycles has been organized around labor supply and demand, their slopes and their shifts. In the real business cycle approach, for example, the emphasis has been on technological shocks shifting labor demand, and on reasons why labor supply may be quite flat, so that shifts in demand generate large movements in employment without much movement in the real wage. Both the papers by Hall and by Rotemberg and Woodford explore the implications of deviations from the standard real business cycle model. The paper by Hall focuses on increasing returns in both the goods and the labor market. The paper by Rotemberg and Woodford focuses on imperfect competition in the goods market.

Hall first gets a semantic issue out of the way. Under perfect competition, one can speak of labor supply and labor demand without ambiguity. Under imperfect competition, this may become trickier. For example, it is well known that a monopolist does not, strictly speaking, have an output supply or a labor demand function. But it is easy to extend the simple notions. One may still refer to the locus of points traced by the real wage and employment in response to shifts in labor supply as the labor demand locus. Equivalently, one may refer to labor demand as the locus traced by the real wage and employment in response to shifts in goods demand. Others have suggested the use of "pseudo," or "surrogate" (Phelps) to emphasize the nature of those loci; this is only a matter of semantics.

With terminological issues out of the way, Hall argues that one should think of labor demand and labor supply as both being very flat, so that small shifts in either one lead to large movements in employment and small movements in wages.

Hall first looks at *labor demand,* making the distinction between the labor demand of an individual firm and aggregate labor demand. Individual firms' demands are downward sloping, but if the marginal cost of one firm depends negatively on the level of activity of the other firms in the industry or economy, the industry or economy labor demand may be much flatter, even upward sloping. Thus, Hall emphasizes the potential importance of external increasing returns.

He first estimates industry labor demand curves. In contrast to most of the abundant empirical research on labor demand, he adopts the philosophy that one should be very picky in the choice of instruments, that, for example, traditionally used lagged values of the variables are unacceptable when one knows so little about the properties of the disturbance term. In this instance, he restricts the set of instruments to the "Hall–Ramey" set, which is composed of the price of oil, a dummy for the political party in power, and changes in military spending. He argues that all three variables are uncorrelated with shifts in technology. (Were he to adopt the Rotemberg–Woodford approach that the markup varies with the interest rate, his instruments, which plausibly affect real interest rates, would become unacceptable.) Under those assumptions, he finds that labor demands are very flat, sometimes even upward sloping. He then marshals additional evidence in favor of increasing returns, from work by him and others on productivity, and from work by Ramey on the cyclical behavior of inventories. Overall, the evidence in favor of increasing returns as one of the keys to understanding the aggregate production–sales–pricing behavior of firms is fairly compelling. The question of where increasing returns come from, in particular when they are external, is left largely unanswered. Hall suggests the importance of agglomeration externalities, the general idea that when activity is high, production is more efficient. While the evidence on agglomeration externalities in space is compelling, the evidence for such externalities in time is, at this stage, much less so.

In what is surely the most controversial part of the paper, Hall turns to *labor supply*. He argues that it is also flat. He starts from the very valid observation that the margin faced by workers is not only between work and leisure, but between work and looking for another job. Thus, if the return to search for a job does not decrease with unemployment, the wage that workers will require to work will not decrease with unemployment either, and thus labor supply will be flat. For the return to search not to decrease as unemployment increases, it must be the case that firms create new vacancies in response to the increase in unemployment. This in turn may happen if there are agglomeration externalities, if the presence of many workers searching leads firms to create new jobs, or in Hall's terminology to reorganize. To explore this idea, Hall relies on evidence from both Blanchard and Diamond, and from Davis and Haltiwanger. Blanchard and Diamond show that the probability of exiting from unemployment to employment does not decrease much in recessions; this supports Hall's thesis. But they also show that the probability of exiting from nonemployment—unemployment plus out of the labor force—to employment does decrease a lot, and that vacancies go down very much in recessions. Davis–Haltiwanger show that their index of reallocation, equal to the sum of job creation and job destruction, indeed goes up in recessions. But this comes from a large increase in job destruction and a small decrease in job creation. Other pieces of evidence that do not quite fit are given by the discussants.

As the discussants point out, the paper by Hall is unlikely to be the definitive truth on the topic of business cycles. But by taking external increasing returns out of the theoretical closet, and showing how they can explain a number of important aspects of cyclical behavior, from labor demand to inventories to reorganization and the reallocation of labor, it represents substantial progress and is likely to generate further empirical research.

Under the conventional view of labor demand as derived from profit maximization by competitive firms, and absent shifts in technology, one should observe a negative relation between employment and real wages. The cyclical behavior of the real wage is one of the longest-running subjects in macroeconomics, and the subject of several papers in earlier issues of this *Annual.* The evidence suggests that the real wage is, if anything, procyclical. Thus, one must either argue in favor of technological shocks that shift labor demand or relax the assumption of perfect competition. Rotemberg and Woodford argue that the solution to the problem lies with models of imperfect competition. They draw out the implications of three such models.

In the first model, firms are monopolistic competitors whose elasticity of demand depends positively on the level of sales. Thus, as sales and employment increase, marginal cost may increase but the markup of price over marginal cost will decrease. Thus, the markup of price over the wage may decrease; put another way, the real wage, which is the inverse of the markup, may increase with employment. This theory can explain an upward sloping "labor demand" curve.

In the second model, current prices affect both current and future sales, for example, because a firm tends to retain customers so long as its price is not raised above levels at which customers think it is worth searching for a better price. In these models, the firm sets its price comparing current revenues from a higher price with future losses caused by the price rise. Thus when current revenue is low relative to expected future revenue, the firm will set a low price to have a larger share of a larger market in the future. In this model, an increase in the value of future profits compared to current profits leads to a reduction in the markup, or equivalently to an increase in the real wage at given employment; it shifts the labor demand curve out.

In the third model, oligopolies implicitly collude, with the collusion being maintained by the threat that a firm that reduces its price will face a price war, implying lower future profits. In this model, an increase in expected future profits compared to current profits reduces the incentive to cut prices (because the loss of future profits in the case of a price war will be larger). Thus, it leads to an increase in the markup given employment; it shifts the labor demand curve in.

Thus the three models have different empirical implications with respect to the behavior of the markup with respect to the ratio of deviations of output to the expected present value of output. As the present discounted value of future output is not observable, Rotemberg and Woodford therefore construct and work with several proxies for that variable. They also construct several estimates of markups. They conclude that the bulk of their aggregate evidence is more consistent with the implicit collusion model than with the other models.

They then examine industry level relationships. In particular, if collusion is an important part of the explanation of markups, then more concentrated industries—in which collusion is easier—should see more countercyclical markups. The results here too are, on the whole, more consistent with the implicit collusion model than with the other two models. In addition, they use data from two industries in which collusion is known to have taken place—baby foods and electrical equipment—and find that markups tended to be countercyclical.

The paper therefore concludes that collusion in the goods market is a central component of business cycles. It argues that changes in the composition of aggregate demand that increase real interest rates therefore lead firms to decrease markups at given employment, leading in turn to an expansion of employment. We strongly suspect that the paper overplays the role of collusion in the business cycle. But one must, however, be impressed by the weight of empirical evidence brought to bear on the issue.

Ten years ago, an econometrician estimating, say, a money demand equation would not have given much thought to the time series properties of the individual variables in the regressions or to those of the residual, except perhaps to do a standard serial correlation correction, if faced with a low Durbin Watson. After 10 years of research on unit roots and cointegration, how differently should she proceed today? Should she test for unit roots in each variable, and if so using which of the many available tests? Should she look for cointegration between the variables, and if so how? Should she use instruments to correct for potential simultaneity, or just run OLS? Questions such as these motivate the survey by Campbell and Perron. There is no point in summarizing it, except to say that the paper, which deals with both univariate and multivariate issues, is both rigorous and designed to be read by nonspecialists. And, at the end, they indeed show how a researcher should, in the light of the survey, estimate a money demand function, and how to do Granger causality tests, and how to test the expectation hypothesis of the term structure, today. Yes, the econometrician should check the order of integration of the variables. Yes, she should test for cointegration of real money balances, output, and nominal interest rates. And, yes, if these variables are indeed cointegrated, she may not need to use instruments to estimate money demand.

The problems of East European transition have captured the interest of much of the economics profession as well as the entire world. Central among those problems is that of privatization of state owned assets, and particularly industrial firms. Privatization is proceeding in different ways in different countries: very small firms have been successfully privatized in several countries, but the privatization of the four or five hundred largest industrial firms, which typically account for well over half the industrial output in the East European countries, has proceeded very slowly. Voucher schemes are in the process of implementation in Poland and Czechoslovakia, while in Hungary firms are being sold rather than given away.

There is already a large and intensely practical literature on privatization. Perhaps because of the generally recognized urgency for action, this literature has drawn relatively little on the existing analyses of related issues in industrial organization and finance. Jean Tirole's paper handsomely repairs that omission. In the body of his paper he sets out general principles of the role of stock markets, methods of providing managerial incentives, and the significance of market structure, without particular reference to the problems of Eastern Europe. He introduces the notion of the power of a regulatory scheme, which is a measure of the extent to which a firm captures the returns to reductions in its costs or increases in its profits. A high-powered regulatory scheme provides incentives for efficient operation of firms, but also implies potentially large rents for firms and demands detailed knowledge of the regulators. Low-powered schemes, such as cost-plus contracts, do not encourage efficiency, but they do prevent the collection of large rents by the firm's owners.

Tirole then draws on the principles he has laid out in analyzing the privatization problem in Eastern Europe. There will be very large uncertainties at the start of the transition process—economic and political—and these are not circumstances under which stock markets work well. Nor will it be possible to use high-powered regulatory or incentive schemes during the noisy phase. Accordingly, Tirole argues there should be little reliance on stock markets or on individual entrepreneurship during the early "noisy" phase of transition; rather the stock market should be introduced during the "mature" phase of privatization.

Tirole lays considerable stress on the need for a competition-oriented restructuring of industry, arguing, counter to the conventional view, that trade liberalization will perform only a limited role in this context. Nor is he optimistic that firms can be restructured after they are privatized, pointing to the difficulties of regulators in the west in breaking up firms. Thus he sees the need for a great deal of restructuring in advance of privatization.

Tirole's analysis leads him to advocate a strategy of privatization that differs in several respects from those being put into place in Eastern Europe. He would first set up a group to examine which firms should be privatized early, and which will need extensive restructuring before privatization. Foreign experts and agencies can play a role here. He is willing to contemplate reasonable delays until firms are restructured. During the early phase of privatization, he advocates placing ownership of firms in holding companies, which would not, however, engage in stock market trading. Shares in the holding companies can be distributed, free, to the population, but trading would commence only when the mature phase begins. In the meantime the holding companies, on whose boards foreigners, including representatives of official agencies, could serve, help prevent the capture of the companies by interest groups.

Beyond the specific scheme however, it is clear that Tirole's paper makes an important conceptual contribution to the debate on privatization.

The collapse of the Bretton Woods exchange rate system in 1973 led to a period of floating exchange rates, with much wider swings in rates than had generally been expected before the changeover. Dissatisfied with exchange rate fluctuations, Europeans have gradually moved back to a system of almost-fixed rates in the European Monetary System, and intend by the end of the century to create a European Monetary Union (EMU), with irretrievably fixed rates.

Froot and Rogoff focus on the view, expressed in the Delors Report, that a 4- to 5-year period of convergence among the members of the EMU will permit a seamless transition into the new system. They present a detailed review of the evidence that has been presented in support of the belief that convergence is taking place: differences in inflation rates among EMS members have indeed declined; so have short-term interests; and surprisingly, so have primary (i.e., noninterest) budget deficits. This is impressive evidence, but Froot and Rogoff point out that even so, Italian inflation exceeds German inflation by as much as 4% per annum. Further, there remain large gaps between long-term interest rates, suggesting the markets believe exchange rates will change at some point.

Most significantly, Froot and Rogoff point to increasing *divergence* of relative price *levels* among countries. That is another way of saying that real exchange rates have changed significantly; for example, the lira has appreciated in real terms relative to the Deutschmark. Further, they find that the countries with appreciating exchange rates have been experiencing increasing current account deficits. And several of them, especially Italy, also have growing government debts.

What accounts for these real exchange rate, or relative price level, movements? Froot and Rogoff first disarm the reader by reminding her that it has proven very difficult to account for movements in real exchange rates, and then go on to show that a relative increase in government consumption spending tends to produce exchange rate appreciation. They also test the view that exchange rates are moved by differential rates of productivity growth, but find little evidence to support that view. They conclude that countries whose real exchange rates have appreciated will probably have to cut government consumption spending to undo the appreciations.

Then they move back to the transition to the EMU. The increasing overvaluation of some currencies, and the growing government debts in those countries, suggests these countries would want to devalue before parities are finally fixed. That would reduce the real value of the government debt. And, if prices are sticky (an issue that is raised though not pursued by Froot and Rogoff), a nominal devaluation would also improve a country's external competitiveness. They develop a model in which central banks that vanish at a certain point (the inception of EMU) care progressively less about their reputations as the end approaches, and are increasingly willing to devalue. Since there are several central banks involved, there will be a contest to devalue last. Thus they argue there is very unlikely to be a seamless transition into the EMU. Rather, as the end approaches, there is likely to be increasing instability.

However, this does not mean that a short transition would reduce the instability—because the incentives for competitive devaluations just before the creation of the EMU would remain. Froot and Rogoff argue that at least one more realignment is likely. Of course, the future members of the EMU can make that realignment at any time, by agreement, and commit themselves to no further realignments. So the final realignment need not come at the last minute, unless the last minute is defined to be the point at which exchange rates are fixed for the last time, rather than the time at which the EMU formally goes into effect.

Over the past decade, work on growth theory has grown from a trickle to a flood, with the development of models exploring the role in growth of—among other determinants—increasing returns, increased division of labor, increased diversity of products, R&D, human capital, and education. In doing so, the focus has shifted away from issues of fluctuations and short-term macroeconomic management. At the same time, the policy advice given to developing countries by development economists and organizations such as the World Bank has been increasingly to get their house in order, to focus on the adoption of sound fiscal and monetary policies as a precondition for growth. The goal of the paper by Fischer is to review and analyze the evidence on the role of macroeconomic management in growth.

Fischer defines macroeconomic policies as monetary, fiscal, and exchange rate policies that help determine the rate of inflation and the balance of payments. There are conceptually three ways in which such policies may affect growth over long periods of time. First, macro policies can lead to long lasting recessions or even depressions, taking output below its potential level for long periods of time, for example, through a sustained overvaluation of the currency. Second, macro policies can affect relative prices, for example real interest rates, and thus affect investment. Third, macro policies may create a more favorable climate for growth. The channels here are more vague, harder to identify with confidence, but not necessarily therefore irrelevant. Decreased uncertainty may well increase investment and boost confidence. Lower and less variable inflation may well allow for a better allocation of resources and higher output.

Some of these effects are conceptually level effects and others are growth effects. Long recessions are usually assumed not to affect the long-run growth rate. Even policies that affect the investment rate permanently do not, in standard growth models, affect the long-run rate of growth, although they do in a number of the more recent, endogenous growth models. These level/growth distinctions, Fischer argues, are largely irrelevant, and anyway impossible to test given the length of the time series we have for most countries. Thus, he focuses on the effects of macroeconomic policy variables on growth rates over the last 30 years in a large cross section of countries, and does not attempt to guess which ones will eventually disappear.

In going to the data, Fischer introduces a useful distinction between the effects of policies that work through physical investment and those that work through other channels, such as improved allocation, better education, and faster growth of human capital. He first reviews the recent and not so recent research examining the determinants of growth using cross-country evidence. This research has isolated a clear relation between the growth rate, the initial level of per capita output—which comes in negatively, suggesting that, other things being equal, poorer countries grow faster and thus that their income levels will eventually converge with those of the richer countries, the rate of population growth—which also comes in negatively, a variable reflecting the level of education—probably coming in as a proxy for investment in human capital, and the rate of physical investment. His first question is whether, given the presence of these variables, i.e., controlling for investment in physical and human capital, macroeconomic policies affect growth. He finds that (1) the effect of inflation is significant and negative, (2) the effect of budget deficits is significant and negative, and (3) the effect of foreign debt is negative. The results are reasonably robust. They hold when the time series dimension of the data is used and when panel data estimation is performed. And they hold under instrumental variables estimation.

His second question is then whether macroeconomic policies affect investment. He again reviews the empirical research on investment equations in developing countries, and adds macroeconomic policy proxies to a standard investment equation, one that includes growth and level of output effects. He finds (1) the effect of inflation to be significant and negative, (2) the effect of budget deficits to be positive (!) and insignificant, and (3) the effect of both variables to become insignificant when the black market foreign exchange premium is included in the regressions.

Thus, a reasonably clear picture arises from the regressions, one in which sound macropolicy indeed increases growth, directly and through investment. Fischer cautions about making too much of those results. The macro variables are only proxies for the unobservable policies. In interpreting the regressions, one cannot ignore the possibility of reverse causality from growth to the proxies, or from third factors such as exogenous changes in the terms of trade on both growth and proxies. In this respect, the instruments he uses may not be fully appropriate; it is hard, however, to think of better ones. Thus, Fischer turns to two case studies, which both show the complexities of the channels at work. The first is that of Côte d'Ivoire and the second that of Chile. In both, he concludes, macro mismanagement and macro policy mistakes in response to shocks bear a large share of the responsibility for the slow growth of the last 20 years.

The conference at which these papers were presented was smoothly organized and run by Kirsten Foss Davis and Ilana Hardesty. Joseph Beaulieu acted as editor of the papers and comments and as rapporteur for the general discussion. He has done a superb job.

Olivier Jean Blanchard and Stanley Fischer