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Editorial

Like its predecessors, this third NBER *Macroeconomics Annual* presents two types of papers. Papers of the first type are relevant to current concerns about the economy, those of the second type seek both to bring recent developments in macroeconomics to a wider audience, and to demonstrate the empirical relevance of these new developments. David Romer's paper on the economic cost of excessive deficits, Kazuo Ueda's paper on the causes of the Japanese current account surplus, and the three shorter papers on the significance of the October 1987 stock market crash, are in the first category. Alberto Alesina's paper on recent developments and tests in game theoretic approaches to economic policy making, Matthew Shapiro and Mark Watson's paper, which seeks to identify the sources of disturbances to the economy, and John Kennan's work on the very old and very basic question of whether the labor market acts as if it is always in equilibrium, fall into the second category.

Traditionally, economic policy making has been studied normatively: a social planner with the best interests of society at heart optimizes the relevant utility function. The outcome is a description of optimal policies. This tradition is essential in enabling economists to say which policies should be adopted; its contributions are impressive, particularly in the area of fiscal policy. More recently though, interest has shifted towards positive models that attempt to explain actual, as opposed to ideal, policy choices.

The new models are game theoretic, with policy makers interacting with a public whose interests may or may not be identical with those of the policymakers, and recognizing in more sophisticated models the imperatives of reelection and the possibility that the next government will have different interests. The proliferation of game theoretic models of policymaking in the last five years has not only bewildered the reader who is attempting to keep up with the literature, but has also appeared to lack empirical power, since there is at least one model to justify any outcome observed in practice.

Alberto Alesina's well-executed intent is to bring order to the literature and to demonstrate its relevance to the economy. He focuses on two sets of empirical implications of the literature: one for the political business cycle, and one for central bank independence. In each case he shows that the data have some power to discriminate among theories, and that the game theoretic approach does well in accounting for the facts. Alesina concludes by discussing a variety of other areas in which the game theoretic approach may help understand macroeconomic events. Both the discussants, Kenneth Rogoff and Kenneth Shepsle, approve of the basic approach, but suggest extensions; there is no doubt that this field is still, despite its size, at an early stage.

The U.S. budget deficit that developed during the 1980's has been at the center of domestic and international economic policy controversy. It has been held responsible for a major decline in domestic saving, for high real interest rates, and thus, indirectly, for the international debt problem, and for the deficit in the current account of the balance of payments. However, an important subset of the economics profession believes that the deficit does not produce any such effects, because individuals recognizing that a deficit represents merely deferred taxation increase their own saving when the deficit rises—this is the so-called *Ricardian equivalence* view of deficits, which is associated particularly with Robert Barro.

Papers in the two previous issues of this *Annual* bear on the role of the deficit. In 1986, Martin Feldstein showed that the trade deficit is significantly affected by the budget deficit. In 1987, Douglas Bernheim reviewed the arguments and evidence for and against Ricardian equivalence, concluding against. However the evidence in this area is surprisingly weak, and the controversy continues.

David Romer's paper is an innovative attempt to move beyond the question of whether or not the budget deficit matters, and to ask instead to what extent it might matter. This can only be done in a model in which Ricardian equivalence fails. Romer works with three different channels of real effects of deficits: first, taxes that are cut today generate government debt that will be paid off later by other people, thus current deficits help the current generation at the cost of future generations; second, individuals may not be able to borrow freely at current interest rates ("liquidity constraints"), so that an increase in the deficit and increase in disposable income increases consumption; and third, because there are inefficiencies in the collection of taxes, cutting taxes now, only to raise them above their initial level in the future, may generate welfare costs. As Romer notes, there are many other potential sources of real effects and costs of deficits, but these three are generally regarded as the most significant. He confines

himself to a closed economy, and thus does not address the international impact of large U.S. fiscal deficits.

Romer's primary conclusion is that the intergenerational redistribution of welfare associated with temporary tax cuts that produce a deficit may be significant. One of his more striking calculations is that under plausible assumptions a temporary tax cut that raises the deficit may have a cost that is as large as the tax cut. Also interesting is his result that the costs that arise from the inefficiency of allowing tax rates to vary over time are small.

The discussants, Paul Evans and James Tobin, both consider the welfare costs calculated by Romer to be too large. Evans argues that the evidence is simply inconsistent with the view that individuals do not internalize future taxes; he argues that the models used by Romer should have parameter values showing implications that are similar to those of Ricardian equivalence. Tobin raises the interesting question of why the theory gives so much attention to intergenerational distributional concerns when intragenerational distributional issues are not even considered.

Shapiro and Watson address the question of the sources of business cycle fluctuations. Until the 1970's, macroeconomists—both Keynesian and monetarist—argued that demand shocks, caused by fiscal and/or monetary disturbances, were the prime causes of output fluctuations. The oil price shocks of the last fifteen years shifted analytic attention to the role of supply shocks. Equilibrium business cycle theory has increasingly emphasized supply shocks, in part because it is difficult in equilibrium models to find a good reason for demand disturbances to affect output.

Using a technique developed by Blanchard and Quah, Shapiro and Watson are able to identify supply and demand shocks and describe their statistical characteristics. The assumption that enables them to identify supply from demand disturbances is emphasized in the introduction to their paper—that only supply disturbances have permanent effects on the level of output. This is a strong assumption to which one can imagine exceptions: for instance, a change in individuals' desire to save, which is a demand shock, would eventually affect the equilibrium capital stock and thus output. But it is reasonable to assume that some of the most common demand shocks, such as an increase in demand caused by an increase in the money stock, indeed have only temporary effects on output. The identifying assumption they make is a good starting point.

Shapiro and Watson assume in their first model that output can be affected by shocks to technology, the price of oil, aggregate demand, and labor supply. Over shorter periods, perhaps looking two years ahead, they find that shocks to aggregate demand account for about 30 percent of the variability of output, and a much larger share of the variability of prices and interest rates. Technological shocks account for a smaller share of output

variability at the two year horizon. However, Shapiro and Watson do find that nearly half of output variability, at all horizons, is caused by shocks to labor supply.

At the conference, Robert Hall proposed eliminating what he calls the Herbert Hoover assumption—that recessions are caused by spontaneous attacks of laziness. Results obtained in this case are presented in Table 3, and show a much larger impact of aggregate demand, even at a five year horizon. This leaves the paper with a major unresolved issue—capsulized by the difficulty the reader has in knowing whether Table 2 or Table 3 gives a more accurate view of the sources of economic fluctuations. Shapiro and Watson indicate their preference for Table 2, arguing that they see no basis for attributing to aggregate demand the shocks that appear to be related to permanent labor supply fluctuations. They also argue that, for econometric reasons, the Table 3 estimates are only a loose upper bound on the contribution of aggregate demand to output fluctuations.

In Figures 3 and 6 Shapiro and Watson present time series estimates of the different shocks to the economy, with the two charts corresponding to the assumptions of Tables 2 and 3, respectively. While Figure 3 attributes a major part of the blame for the recent recessions to labor supply shocks, Figure 6 attributes most recessions to demand shocks. Thus, the paper leaves for future resolution the tough question of the underlying source of shocks, but does attempt to answer the key puzzle of output variation by attributing much of it to permanent changes in labor input.

Interpreting joint fluctuations in employment and wages has been high on the agenda of macroeconomics, at least since Keynes' *General Theory*. Keynes' assumption was that the level of labor input is determined by the demand for labor, with labor being supplied elastically at a constant nominal wage. Given that the quantity of labor demanded is negatively related to the real wage, the *General Theory* assumption implied that the real wage should behave counter-cyclically; high in recessions and low in booms. It has long been known that the real wage does not show pronounced counter cyclical behavior, a fact that John Kennan amply documents with data for six countries.

The question then is what does account for the joint behavior of wages and employment? A second Keynesian tradition, associated recently with John Taylor, attributes no significance to movements in the real wage. Rather, it sees the price level being determined as a possible constant markup on wages, implying a constant real wage over the cycle, with employment then being determined by the aggregate demand for goods.

Kennan examines the alternative approach, in which the labor market is interpreted as always being in equilibrium. He tests such models with monthly data from six countries. The multi-country data have the virtue of

showing that some U.S. stylized facts do not hold in other countries. For instance, employment is more variable than real wages in Canada and the U.S., but not in the other countries in his sample.

Kennan then constructs and estimates a labor market model in which both workers and firms choose employment according to dynamic labor supply and demand models. Costs of adjusting the labor input by firms, effects of past work on current utility, and intertemporal substitution of leisure (which works more when wages are higher than they are expected to be in future) are responsible for the dynamic elements in demand and supply respectively. He also estimates a model in which labor supply is controlled by a monopoly union. The model does reasonably well in accounting for the joint behavior of employment and real wages in the U.K., but not in the U.S.

Kennan views his results as sufficiently promising to continue exploration of equilibrium approaches. One of his discussants, Mark Bils, concurs, though he tends to favor alternative equilibrium models. John Taylor, the second discussant, argues that contracting models of the labor market do a superior job in accounting for the data. Here, too, is a discussion that has been sharpened, but where much is left for future work to develop.

The Japanese current account surplus for some time threatened to disrupt U.S.-Japan trading relations. The most popular explanation for the surplus was that Japan was engaging in unfair trade practices. However, that is not the main focus of Kazuo Ueda's paper on the topic. He does give some weight to another popular explanation; restrictive Japanese fiscal policy in the first half of the eighties accounts for the surplus.

Ueda is mainly concerned with two other factors. The first is the decline in the real price of oil since 1981; the second is the U.S. fiscal expansion. Looking back, Ueda also notes that the decline in the rate of investment in Japan in the 1970's, accompanied by a lower decline in saving, helps account for the underlying trend to surplus in the Japanese current account.

As the Japanese current account has finally begun to decline in dollar terms in 1987–88, the focus on the problem has declined. Ueda sees the recent decline in the surplus as resulting from rising investment in Japan, and raises the possibility that in the future as in the 1970's, the current account will be driven more by the behavior of domestic investment than by fiscal policy.

The final section of the *Annual* contains three papers on the implications for understanding of markets of the stock market crash of October 1987. Participants were explicitly requested not to discuss the implications of the crash for investment and consumption. Implicitly, they were being asked

to evaluate the efficient markets hypothesis in the light of the behavior of the market in October.

The efficient markets hypothesis asserts that market prices efficiently reflect all relevant information about firms' prospects. The efficient markets hypothesis certainly does not rule out abrupt, large price changes: prices should change rapidly in response to new information. However, the question is whether any new information arrived in the middle of October that should have caused investors to conclude that stock prices needed to be reduced by 20 percent.

Fischer Black attributes the crash to a sudden realization by investors that tastes in the market had been changing over the past several months. In essence, each (or at least many) realized that other investors were expecting the market to fall. In addition, Black is willing to entertain the notion that psychological factors affect participation in the market and also its level. He describes his as an equilibrium model, though not one in which expectations are rational. Presumably, the market does not process information optimally, and so is not efficient in that sense.

It is quite another matter, as Kenneth French argues, to know when the market is at an inappropriate level, and what to do when it is. He believes on the basis of the very low dividend yields before the crash that the market was in fact overvalued—but that this was not then known. He, like Black, gives some weight to the dynamics of prices, suggesting that "the big news that drove price down on October 19 may have been the market's large response to moderately bad news over the previous three trading days." He does not support the Brady Commission's view that limits should be placed on price changes, believing limits that prevent trading could make the situation worse.

The third participant, Robert Shiller, had the insight and energy to poll market participants on the day of the market crash and in the next few days. His direct evidence suggests there was very little news disturbing the market in the days just before the crash. People had *concerns*, for example about the deficit, but had not received and fresh news in the middle of October that caused price changes. Much of their reaction appears to have been to price changes themselves.

All the papers given some weight to the possibility that price changes themselves are regarded as important information, and thus create the possibility that the market reflects "bubbles" rather than facts. French does not dismiss this possibility; it appears that many analysts are now more willing than they were before October 1987 to give some role to extraneous forces in moving market prices. But, as many participants argued, knowing that the market may be inefficient does not mean that regulators could necessarily make it behave more efficiently.

The session on the stock market evoked lively discussion, which is reported after the papers.

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Stanley Fischer