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Editorial, NBER Macroeconomics Annual 1993

This—the eighth—edition of the *NBER Macroeconomics Annual* contains six papers, with a mix reflecting the *Annual*'s twin goals—first of presenting, extending, and applying frontier work in macroeconomics, and second, of encouraging and stimulating work by macroeconomists on current policy issues.

Among the papers that present and extend frontier research are those by Ricardo Caballero and Adam Jaffe, who develop a growth model in which research and innovation are the driving forces of growth; by Gilles Saint-Paul, who develops a political economy model that seeks to explain why European governments can be reelected even when unemployment in their countries rises to double-digit levels; by Anton Braun and Ellen McGrattan, who use a real business cycle model to explain the behavior of real output, consumption, and wages in the United States and United Kingdom during the two world wars; and by Robert Pindyck and Andrés Solimano, who develop and seek empirical confirmation of a well-known model pioneered by Pindyck and others in which uncertainty delays investment.

Two of the papers directly address current policy issues. Alan Gelb, Gary Jefferson, and Inderjit Singh examine the extraordinary economic growth achieved in China during the last 15 years. The Chinese success story is often held out as an example of how a formerly socialist economy *should* reform itself, and the authors examine the contrast between it and East European countries. Finally, the paper by John Boyd and Mark Gertler describes the dimensions and causes of the ongoing banking crisis in the United States.

We believe that these papers offer a good sample of the current issues and exciting research directions in macroeconomics. We limit ourselves in this introduction to brief descriptions of the papers; an important contribution of the conference, however, is in the formal and informal comments that follow each paper.

Growth comes in large part from productivity growth. And productivity growth comes, in turn, largely from R&D. The nature of R&D and its contribution to growth are the subject of the paper by Ricardo Caballero and Adam Jaffe.

There are two aspects to the dynamics of R&D. One has to do with ideas. New ideas build on previous ones and in the process make them partly obsolete. The other has to do with new products. New inventions lead to new products and in the process make old products less attractive. Recent models of growth and R&D have captured these twin processes, but empirical work has yet to quantify them. This is the task taken up by the authors.

They start by constructing a model built on both processes. Inventions are a function of research time and of the stock of existing knowledge. Old ideas lose usefulness in proportion not to time but to the number of new ideas that have since been developed. In the product market, inventions lead to better products. Those new products in turn steadily displace older and, thus, less attractive products. The two processes obviously interact. Higher values for patents lead to a faster rate of invention and in turn to faster product cycles. Faster product cycles lead to a smaller present value of monopoly rents, smaller values for patents—thus, a smaller rate of inventions. In the rest of the paper, Caballero and Jaffe estimate the crucial parameters of both parts of their model.

Using a random sample of patents granted in the United States from 1975 to 1992, which includes not only the patents but also the old patents cited by the new patents, they construct and estimate a "citation function." The citation function is a way of thinking about and estimating how useful existing patents are in the production of new patents and how patents become obsolete as new patents are developed. One tantalizing finding, for it offers a potential clue to the slowdown of productivity growth, is that recent patents appear to have smaller spillovers for other patents than was the case before, thus leading to a smaller rate of productivity growth for a given level of research.

To estimate the rate at which new products displace old ones, the speed of "creative destruction," the authors use the NBER R&D data set, which includes both Compustat information and U.S. patent data. From this they conclude that the rate of creative destruction was in the range of 2–7% a year in the 1970s, with the rate being as high as 25% in some sectors.

Having obtained estimates, they finally return to their model, and show the quantitative implications of their findings; these results provide only a hint of what this line of work can yield. We think that their paper is an important contribution. Not only does it provide a framework to integrate the available evidence on the R&D process in a macroeconomic context, but it already makes much progress in this process of integration.

China's economic performance since it started reforming in 1978 has been spectacular. Per capita income has risen more than 6% per year over that period, which means that average income has more than doubled since 1978. Alan Gelb, Gary Jefferson, and Inderjit Singh describe the policies and responses that underlie that growth, and they ask what else China needs to do to maintain its growth momentum.

While Gelb, Jefferson, and Singh are moderately sceptical about Chinese data, they do not doubt that growth has been extremely rapid, and that per capita real income on a purchasing power basis is closer to $2000 than the $370 (based on an exchange rate conversion method) recorded in World Bank publications. This would make the Chinese economy either the second or third largest in the world. They warn that the potential understatement of GDP may not apply equally to all its components, and they suggest that the recorded Chinese investment ratios of nearly 40% may be overstated—but that if they are, then Chinese productivity performance has been even better than the 3+% per annum increase in total factor productivity that Gelb, Jefferson, and Singh calculate for the 1980s. They also lay some stress on the remarkable demographic transition in China, which gives it one of the world's lowest dependency ratios.

The Chinese reform program began in the rural areas with the breaking up of the communes. By allowing farmers to work for themselves on leased land, and by freeing up agricultural prices at the margin, the Chinese government produced a rapid supply response to the agricultural reform. The improvements in agriculture were the backbone of the original reform program, providing an anchor that helped ensure stability as the reforms spread to the rest of the economy.

Although no precise reform strategy for China had been worked out before the rural reforms went into effect, Gelb, Jefferson, and Singh suggest that the reform program can be divided into four phases and seven areas of change. By now, elements of reform have affected all sectors of the economy. Still, there remain major difficulties that will have to be dealt with if the pace of Chinese reform and growth is to be maintained.

First, property rights in most Chinese firms, and for land, remain ambiguous. State-owned enterprises have not been reformed or sold, and many lose money. Interesting firm-level evidence presented by Gelb, Jefferson, and Singh suggests that productivity performance has been better the more exposed firms have been to market forces. Still, one of the surprises of their paper is that productivity rose quite fast in state-owned as well as town and village enterprises. Second, the credit mechanism has not been sufficiently reformed, and there is once again the possibility of a major credit expansion and inflation, the sequence of events that led to the repression and stabilization of 1989. Indeed, both discussants emphasized the potential for macroeconomic instability in the Chinese economy.

Gelb, Jefferson, and Singh end their paper by discussing the lessons of the Chinese reforms, which most readers will take to apply to Eastern Europe. They argue that partial, bottom-up, reforms, can be successful, and that a big bang is not necessary unless required by macroeconomic imbalances. They also argue that the Chinese reforms will have to be deepened for growth to continue at the present rates: The discussants concurred with this view. In the floor discussion, the issue was raised of why China had grown so fast when conditions thought to be necessary for growth, such as clear property rights, were absent. The discussion wrestled with the issue but did not settle it. Nonetheless, Gelb, Jefferson, and Singh certainly substantially advance our knowledge of the Chinese reform process.

Unemployment has been high in Europe for more than a decade. Many economists have suggested more active policies, either from the demand side, or the supply side. But little has been done. Is it because governments feel that nothing can be done, or because there is little political concern about unemployment? This is the general question taken up by Gilles Saint-Paul. The specific question he asks is the following: Suppose that high firing costs are indeed one of the causes of high unemployment. Will a government that cares about its electoral popularity try to reduce those costs, and, if so, how should it do it?

Saint-Paul first sets up a simple model in which firing costs decrease the rate of separation. But because workers are costly to fire, the shadow cost of workers to firms is higher, so that firing costs also decrease the hiring rate. Thus, a reduction in firing costs increases both separations and hirings. Now consider a government that wants to reduce firing costs. As this increases hiring, the unemployed will support such a move. But, as it increases firing, the employed will typically oppose it. If, as is obviously the case, the employed are more numerous than the unemployed, the reform will lack political support.

Can the government nevertheless do something? Saint-Paul shows that progressive reform may work. A two-tier system in which the employed remain protected by high firing costs, and the unemployed work under lower firing costs, will typically have the support of both groups. Those currently employed will be protected in their job by high firing costs, and they know that, if they were to become unemployed, higher hiring would make it easier to get a job. And the unemployed will also see improved hiring prospects. As time passes, more and more workers will be subject to low firing costs. At some point, those workers will have the majority and will want to implement lower firing costs for all. Then, the full reform will pass.

That this is not the end of the argument will be clear to readers conversant with issues of time consistency. As employed workers at the beginning of the process realize that they will eventually be in the minority and the full reform will be passed, they may oppose the initial steps. Saint-Paul shows that this will limit the size of the reduction in firing costs that the government can achieve. If the reduction is too large, employed workers will oppose it from the start. Saint-Paul finally shows that the government has one more tool in its arsenal. By introducing conversion clauses, e.g., a positive probability that workers with low firing costs can become high firing cost workers, it will slow down reform, but in doing so, will make it more palatable to employed workers and will, thus, get their approval.

The analysis of the paper makes us aware that there are better and worse ways of implementing reform. Some build constituencies, some do not. The results in the paper are far from obvious, but once presented, they strike one as very relevant. Indeed, in the last section, Saint-Paul shows how the analysis can shed light on the process of labor market reform in Spain. Reform design is clearly an issue that economists, in their role as advisers, should pay more attention to.

When thinking about war time economies, most economists are quickly led to think of rationing, forced saving, patriotism, and other nonmarket mechanisms. How else can one explain how higher military spending displaces private consumption and investment spending while real interest rates typically become negative? How else can one explain higher labor force participation and higher average hours at roughly the same real wages? How else can one explain how higher capacity utilization and higher employment are typically associated with higher measured productivity?

In their paper, R. Anton Braun and Ellen McGrattan take on the challenge. They examine the behavior of the U.S. and the U.K. economies during World War I and World War II. The basic characteristics of those economies, they argue, are largely consistent with those that emerge from a competitive market clearing model. Their goal is not to argue that rationing, price and wage controls, and so on, were not present; they clearly were. It is to argue that these are not essential to an understanding of what happened. Their paper is important, not only for the specific issue it takes on, but as a contribution to the larger debate about the nature of economic fluctuations in general.

Their paper starts with a careful review of facts for the United Kingdom and the United States during the two world wars. For once, actual facts turn out to accord quite well with traditional perceptions. Output increases, while consumption and private investment usually decrease. Civilian employment tends to increase, except when the level of conscription becomes very high. Labor productivity goes up. And real wages do not show a regular pattern.

From the point of view of models of perfect competition, those facts present two challenges. The first has to do with the joint behavior of quantities. In the absence of shifts in technology, wartime economies should be moving along a given labor demand. Thus, labor productivity and employment should move in opposite directions. But, in fact, wars appear to be often associated with higher employment and higher labor productivity. The second has to do with the joint behavior of prices and quantities. For the same reason as labor productivity should decrease with employment, real wages should also decrease; they often do not. Also, the large increase in public spending should lead to a large increase in real interest rates; typically interest rates decrease instead.

Braun and McGrattan do not take up the second challenge, the joint behavior of prices and quantities. Implicitly, they assume, various forms of controls and rationing may lead to deviations between observed and shadow prices. Looking at observed prices would then not be very useful. But they focus on the first, the behavior of labor productivity and employment. They suggest that the puzzle has a simple solution, the accumulation of government-financed capital, not counted as private investment, but used by the private sector. They show that, in all four episodes, a large part of public spending was indeed on capital used by private firms. And they argue, the steady accumulation of this capital is what shifts the labor demand curve over time, explaining the positive co-movements in employment and labor productivity.

They proceed to calibrate a real business cycle model in which they allow for two deviations from standard assumptions, the existence of conscription and the existence of government provided capital. Their goal is to see whether they can replicate not only the sign but also the magnitude of the co-movement in labor productivity and employment. They come close.

Thus, they conclude, the allocation of resources in wartime economies is quite consistent with what one would expect from real business cycle theory. Their paper is not only instructive but also a good example of the methodology underlying real business cycle research.

There has been a great deal of work recently on the implications of the irreversibility of investment—the fact that once an investment project has been undertaken, it is very costly to undo. The popular version of the outcome of that work is that in the presence of irreversibility, investment can be very sensitive to uncertainty about returns. The investor contemplating a project always has the option of waiting for better conditions; the moment he or she commits to the project, the option of waiting is lost. Calculations reported by Robert Pindyck and Andrés Solimano in Figure 1 of their paper show both that the hurdle rate for investing is both very sensitive to uncertainty, and that reasonable levels of uncertainty can easily double or triple the hurdle rate.

This work, applying option theory to physical investment, has been widely although informally cited to argue that low levels of investment following stabilizations are accounted for by the uncertainty about the success of the reforms. One of the contributions made by Pindyck and Solimano is to point out that great care has to be taken in using the option theory approach, because it does not necessarily show that average investment over long periods would be lower in countries with more instability than in more stable countries.

They first outline the partial equilibrium theory on which the informal arguments linking low investment to uncertainty and instability are based, and then go on to present a more fully worked-out equilibrium model in which asset prices and interest rates can adjust when uncertainty rises. In these models, greater uncertainty may lead to lower interest rates rather than lower investment.

Having shown the ambiguity of the relationship between uncertainty and investment, Pindyck and Solimano go on to examine the empirical relationship. The key relationship is that between the marginal profitability of capital and the hurdle rate for investment. They calculate the former, for 29 countries, using Cobb-Douglas production functions, and they approximate the latter using extreme values of marginal productivity. Their estimate of uncertainty is the standard deviation of marginal profitability.

They also relate the variability of marginal profitability to various indicators of macroeconomic instability and find the tightest relationship between the inflation rate and uncertainty about profitability. This relationship is strongest for the developing countries in the group. The surprise is that the rate of inflation is more closely related to the variability of marginal profitability than the standard deviation of inflation and other measures of macroeconomic instability.

The evidence is mildly supportive of the view that there is a negative relationship between uncertainty and inflation, but Pindyck and Solimano clearly regard their results as cause for further research rather than definitive. In particular, in the discussion they focused on the possibility that more information about the power of the theory might be available using firm-level data; they also are considering extending the sample of countries. The bottom line is thus that the theory is very interesting and suggestive, but that a careful look at the data does not yet provide strong support for its empirical significance. That means the Pindyck-Solimano paper is sure to stimulate further research.

After the savings and loans crisis, many expected that the commercial banking system would suffer a similar though smaller scale crisis. With the assistance of the unusually steep term structure of the last few years, that crisis did not happen, even though the number of bank failures in the second half of the 1980s was well above earlier levels. Nonetheless, there has been great concern about the health of the banking system, and its supervision. One view that was widespread during 1991 and 1992 was that the banks' unwillingness to lend had reduced the effectiveness of expansionary Federal Reserve policy, and helped intensify a credit crunch. The reluctance to lend was in turn blamed on excessively cautious bank supervision and on the tight capital requirements imposed by the Basle rules.

In their paper, John Boyd and Mark Gertler document in a series of graphs the trends that have affected the banking system since World War II. Since 1975, banks have been providing a declining share of the credit granted by financial institutions. The share of loans in their portfolios increased, but since the early 1980s, the share of mortgages in bank loans has been rising as the share of commercial and industrial (C&I) loans has fallen. Most strikingly, an increasing proportion of C&I loans has been provided by foreign or offshore banks.

On the liabilities side, the share of checkable deposits has declined from 70% in 1952 to less than 20% today. Money market liabilities and long-term debt now each exceed checkable deposits, and small-time and savings deposits are the largest single liability category. Thus, the textbook picture of banks as deposit-taking institutions is not accurate; managed liabilities are far more important to their business than deposits.

Bank equity capital kept declining until 1974 and has since generally been rising. With the imposition of the Basle capital standards, equity will have to rise a bit further. Boyd and Gertler show that equity ratios decline with bank size, and that the ten largest banks had in 1991 an equity ratio of 4.7%, well below the Basle-required 8%.

Banks' rate of return on equity fluctuated between 11 and 15% until 1987, when the writeoffs associated with the international debt crisis drove the average return on equity below 2%. After a rebound in 1988, bank returns were below 10% from 1989 to 1991. Boyd and Gertler show that this poor performance is mainly attributable to the big banks. They ask whether this could be a result of the regional size distribution of banks, or of differences in their portfolio composition, but after statistical testing, conclude that it is bigness per se that is associated with low returns.

They argue that the "too-big-to-fail" doctrine was responsible for excessive risk taking by big banks and then discuss alternatives to this doctrine. They come out in favor of narrow banking—alternatively warehouse banking—in which certain banks hold only absolutely safe assets, such as Treasury bills, and so are never at risk. Depositors in other banks would be at risk, and would be clearly told so. Presumably this way those depositors who value safety could have it, and those who value higher returns could have that, at the cost of bearing the higher risks. Boyd and Gertler also come out in favor of the Basle capital requirements.

As with the other papers, the discussants' comments are well worth reading: Fischer Black presents a radical, finance-based view that banks and their failures do not matter, and Martin Feldstein criticizes the regulators and the Basle capital requirements.

The conference at which these papers were presented was smoothly organized and run by Kirsten Foss Davis and Rob Shannon. For the second time, Chad Jones of MIT (and Stanford) has done the detailed editing of the papers and comments, and acted as rapporteur for the general discussion. He has done a superb job.

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