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## *Editorial, NBER Macroeconomics Annual 1992*

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In previous years the *NBER Macroeconomics Annual* has included two types of papers: those that introduce and develop recent frontier contributions, and those directed at policy or applied problems. It is one mark of the welcome shift of emphasis in macroeconomics in recent years toward dealing with real-world problems that most of this year's papers fit into both categories.

The opening paper, "A Tale of Two Cities: Factor Accumulation and Technical Change in Hong Kong and Singapore" is by Alwyn Young. The phenomenal growth performance of these two economies is of direct interest and can also be used to examine how well the growth models developed in an explosion of output in the last 5 years fit the facts.

At first glance, the economic stories of Hong Kong and Singapore appear very similar. Both have been growing at high rates, for nearly four decades in Hong Kong and for close to three in Singapore. Both have gone through a similar sequence of products, moving from textiles early on to electronics, and more recently to banking.

However, these similarities are less impressive than one extraordinary difference. A decomposition of growth into the contributions caused by labor, capital, and technological progress reveals two fundamentally different processes of accumulation and growth. Growth in Hong Kong has come largely from technological progress: The rate of investment has been stable at a moderate 20% of gross domestic product (GDP), and technological progress accounts for 59% of the growth of output per worker over the last 20 years. In sharp contrast, growth in Singapore appears to have come entirely from capital accumulation and capital deepening. Investment rates have been high and increasing, standing

in 1985 at 40% of GDP. And Young's estimates imply that all of growth in output per worker has been due to capital accumulation. Put another way, he concludes that there has been no technological progress at the aggregate level in Singapore over the last 20 years.

Having documented these facts, Young seeks to explain them. The explanation, he suggests, is that Singapore has grown too fast for its own good, moving into new products before having the expertise, moving out of old products before having fully exploited learning by doing. He paints a picture of Hong Kong as an economy with a hands-off government, where growth has been driven by domestic entrepreneurs employing a skilled labor force and moving to the next product only when the time was right. In contrast, he argues, growth in Singapore has come from a heavy-handed industrial policy, in which the government has moved too fast from product to product, and has relied too much on foreign investment and entrepreneurs, and too little on domestic talent.

The portrait he paints of Singapore is of an economy with foreign firms coming in to take advantage of generous tax breaks, later going out and taking with them their capital and their expertise. Thus, he argues, growth has not triggered the development of domestic learning by doing and has not led to the development of a class of domestic entrepreneurs. Rates of return on capital are low, and Singapore cannot expect to grow at high rates for much longer.

By relating the theoretical discussions of the role of increasing returns, externalities and learning in the growth literature to the real-world experience in these two countries, the paper makes an important contribution. As Young and his discussants both emphasize, the facts go strongly against a popular class of models of endogenous growth, those with increasing returns to capital accumulation. If those models were true, Singapore would show high, not low, technological progress. The facts point instead to the key roles of learning and of entrepreneurship in growth. The analysis obviously leaves a number of questions open. For example, should one conclude from the Singaporean example that industrial policy is a terrible idea, or only that industrialization should have proceeded at a slower pace? Should one also conclude from the Singaporean example that foreign investment is to be avoided, or instead that it should be coupled with domestic content requirement, with requirements on the employment of domestic managers? Should one conclude that the hands-off policy of the Hong Kong government was the secret of growth in Hong Kong, or that more infrastructure would have led to an even more impressive performance? These are old questions in development; as this paper shows, these are the questions on which new growth theory must now focus.

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Growth experience in Latin America and Africa in the 1980s was dominated by the international debt crisis. After the shift from the new-financing Baker strategy to the debt-reduction Brady Plan in 1989, and with East European news crowding the debt out of the headlines, the question is whether the debt crisis is on its way to a solution, or rather has simply become less fashionable. For some countries, most notably Chile and Mexico, the debt crisis is clearly on its way to solution. That is not the case for most African countries.

In his paper, "The Debt Crisis: A Postmortem," Daniel Cohen addresses three main questions. First, how did the *creditors* fare during the debt crisis of the 1980s? Cohen answers that they have done reasonably well. He calculates the present value of aggregate payments by the debtors, including the liquidation value of the remaining debt, and finds in several cases that the countries have successfully paid off at a rate higher than LIBOR. Even the 20 or so severely indebted countries as a group would have repaid 77% of their 1982 indebtedness if the creditors had sold the remaining outstanding debt at its 1989 market value.

These high servicing rates during a period of real economic difficulties lead Cohen to ask why the debtors serviced their debt to the extent they did. His answer is that they feared the cutoff of trade credit. He develops a simple model of the consequences of being cut off from trade credit and shows that this financial autarky imposes high costs on the borrowing country.

The second major question is whether the Brady Plan is, as many have suggested, a scheme for bailing the banks out at the expense of the international financial institutions, and ultimately at the expense of governments and taxpayers. Cohen starts by showing that for most countries, a debt consolidation carried out at the *marginal* rather than average price of debt would have brought relatively little change in the market value of the debt while allowing a significant reduction in its face value. This means that debt reduction is unlikely to have cost the commercial banks a great deal. Evaluating the Mexican deal, he concludes, as have others, that the deal was mainly a transfer from the IFIs to the country, with the banks gaining very little.

The third issue is the impact of the debt crisis on growth in the debtor countries. Cohen estimates that large debtor countries suffered an excess (relative to other countries) decline in growth of 1.9% per annum in the 1980s. He attributes 0.8% (per annum) to the worsening of their terms of trade, and 0.2% to a decline in investment. If one assumes these latter sources of decline are not caused by the debt crisis, the residual that Cohen attributes to indebtedness is a decline of 0.9% per annum. This is sizeable, even though it accounts for only about a third

of the excess decline and a fifth of the total decline in growth in the indebted countries in the 1980s.

Cohen concludes his paper by asking how large an impact foreign capital inflows should be expected to have on growth, a question given added interest by Eastern Europe's hopes of attracting foreign capital. He concludes that domestic saving and particularly domestic accumulation of human capital are far more important determinants of growth than foreign capital, but that foreign inflows are likely to be most productive in countries that are relatively rich in human capital and in countries integrating into a larger economy—conditions that apply reasonably well in Eastern Europe.

The paper by John Cochrane and Lars Hansen, "Asset Pricing Explorations for Macroeconomics," presents a unified description of work on asset pricing conducted within the framework of models of intertemporal optimization by households, and also by firms. They address two related but not identical empirical issues: the *equity premium puzzle*, of why the return on the stock market has on average exceeded the real return on Treasury bills by more than 500 basis points; and the *risk-free rate puzzle*, of why the real riskless rate is so low.

These are puzzles in the sense that the standard model of frictionless asset markets populated by intertemporal optimizing households is not consistent with the rates of return and variances of rates of return observed in U.S. capital markets. While both Hansen and Cochrane have played prominent roles in developing the econometrics and theory of the standard approach to asset pricing, the present paper is less technical, relying heavily on a powerful graphical technique, which can be understood from Figure 1.

Using this technique, Cochrane and Hansen show in Figure 1 that implausibly high degrees of risk aversion would be needed to account for the equity premium. And if risk aversion were at these levels, the riskless real rate of return would be extremely high, as much as 17% per quarter. In the remainder of the paper, Cochrane and Hansen examine the power of many different explanations that have been put forward to account for the puzzles.

Among the explanations they examine is whether lengthening the investment horizon can whittle down the puzzle. Because many individuals hold stocks over longer horizons than a quarter, perhaps periods longer than a quarter are relevant to determining their basic asset holding decisions and the implied pricing of assets. Regrettably, the lengthening of the investment horizon seems only to accentuate the puzzles. Nor do they get any further in accounting for the puzzle when they take account of the (limited) predictability of asset returns (Fig. 5).

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Models of habit persistence in consumption, nonexpected utility theories of intertemporal choice, and production-based models of intertemporal discount rates are discussed next. While habit persistence can help account for the risk-free rate puzzle, Cochrane and Hansen do not focus on this set of explanations.

Rather, they give more emphasis to the role of borrowing constraints, paying particular attention to asset pricing in markets where some individuals are credit constrained, and others satisfy the unconstrained conditions for intertemporal utility maximization. They also discuss alternative forms of constraint: one in which individuals are not permitted to hold portfolios whose initial values are negative, and another in which they are not allowed to take positions that allow their final portfolio values to be negative. They show that borrowing constraints can deal with the risk-free rate problem but do not—at least in the versions of the equilibrium pricing model they present—solve the equity premium puzzle.

By the end of the Cochrane–Hansen paper, the reader will be convinced of the power and elegance of the analytic and graphical approach used in the paper, and will probably be more frustrated than he or she was initially by the difficulty of accounting for the long-standing puzzle of the equity premium. Whether alternative approaches that do not derive so closely from models of individual optimization can do better remains to be seen. But whatever new approaches are developed, they will have to meet the same rigorous standards of explanation as Cochrane and Hansen demand of the models they present in their paper.

Economists have organized their thoughts about monetary policy around two basic models—and their abundant progeny—, the model developed by William Poole to think about the choice of instruments, and the model developed by Finn Kydland and Edward Prescott to think about the choice between rules and discretion. While policymakers and central bankers have listened politely, their policies look quite different from anything these models suggest. Central banks frequently set many targets, frequently miss target ranges, and not infrequently change targets altogether. Instead of giving yet more advice, the paper by Ben Bernanke and Frederic Mishkin looks at central bank behavior, on the reasonable idea that we, academic economists, may thereby learn something about what central bank concerns are and about why they do what they do.

Bernanke and Mishkin first present a detailed description of monetary policy since the breakup of the Bretton Woods system for six countries: the United States, the United Kingdom, Canada, Germany, Switzerland, and Japan. Their purpose is to show what the various monetary

“strategies” have been, and how and why those have changed through time. In reading their description, one is struck by the variety of strategies both through space and through time. One counts no less than six different targets, M0 to M3, nonborrowed reserves, “central bank money,” used at one time or another by one of those countries. And changes are frequent. The move by Germany from targeting M0 to targeting M3 nearly coincides with a move by the United Kingdom from targeting M3 to targeting M0, and so on.

They then suggest a number of hypotheses to make sense of the apparent complexity. A first key to understanding the behavior of central banks, they argue, is that, while central banks have many ultimate goals, at any point in time they focus on one or two. In other words, they have a crisis mentality. A second key is that, when inflation becomes the main issue, as in the late 1970s in most of these countries, central banks give renewed emphasis to money growth targets. This signals to markets the central bank’s commitment to fight inflation. But even then, they feel compelled neither to meet target ranges nor to stick to the same targets.

Having offered a description of central bank behavior, Bernanke and Mishkin take some risks and offer tentative hypotheses as to what seems to work and not work. First, they argue, money targets work best when they are most transparent, i.e., constructed according to simple rules. They suggest that such transparency may be what has allowed the Swiss Central Bank to rebase its target ranges in response to permanent shifts in velocity without losing credibility. They point to the United Kingdom in the late 1970s as a bad case of the opposite. Second, maintaining credibility does not require short run adherence to money targets; they point again to Switzerland and Germany. Third, they suggest that the choice of instruments does not in the short run seem to affect the general performance of monetary policy. Switzerland is an example where the use of the monetary base as the instrument is associated with short-term, but not long-term, interest variability, and to Germany, where the use of an interbank rate as the instrument has been associated with very low money growth variability.

Bernanke and Mishkin insist that their paper is aimed at developing hypotheses rather than reaching firm conclusions. We think that both the positive and normative models of monetary policy they have sketched in their paper will indeed lead to more formal, quantitative, work, and move the debate on “rules versus discretion” closer to the concerns of central bankers.

Another ambitious and wide-ranging description of the economic landscape is given by Steven Davis, who in “Cross-Country Patterns of

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Change in Relative Wages" looks at the evolution of relative wages for nine advanced economies and four middle-income economies over the past 20 years.

The motivation for the paper comes from the dramatic increase in wage inequality in the United States since the early 1980s. For example, from 1979 to 1987, the wages of young male college graduates relative to young males with 12 years of schooling increased by over 30%. The facts are now well established, and research is moving to narrow the range of explanations. In this respect, it is clear that looking at many countries, advanced and developing, can be of great help.

Because of data constraints, Davis limits his examination for the most part to the wages of full-time male workers. He looks first at simple measures of wage inequality, such as standard deviations of log wages, or the differential between wages in the 90th and 10th percentiles of the wage distribution. He finds that the 1980s have indeed been times of increased wage inequality in all advanced countries. By contrast, wage differentials have declined in Brazil, Venezuela, Colombia, and South Korea, the four middle-income countries he includes in his study. The commonality within each of the two groups is difficult to reconcile with the dominance of labor supply shifts, which are largely country specific. The difference between the two groups on the other hand is suggestive of a reallocation of production across the two groups of countries. Davis clearly prefers the latter hypothesis, which he tests later in the paper.

Before doing so, Davis looks at wage differentials as a function of experience and education. He first shows that the returns to experience have increased in all nine advanced countries in the 1980s, and sometimes earlier. For the countries for which he can control both for education and age, his conclusion is that returns to experience have increased across education levels. Again, things are quite different in middle-income countries; there the returns to experience have remained either constant or decreased. The picture of the evolution of returns to education is only a bit more blurry. They have increased strongly in the United States, less so in other advanced countries. And they have decreased sharply in the four middle-income countries. Thus, when one looks at the evolution of wage differentials by experience and education, the picture is again largely one of commonality within each group, and differences between the two groups.

Davis then focuses on the role of international trade in the evolution of these wage differentials. Increased specialization of countries can easily explain why wages of unskilled workers have stagnated compared to those of skilled workers in advanced economies, and why the reverse has happened in developing countries. A first pass, in which



Davis looks at whether the structure of relative wages has converged across countries, is not encouraging: relative industry wage structures have diverged, not converged, since the early 1980s. Davis then sets up a more sophisticated test, in which he looks at the relation, across time and countries, between the evolution of openness, measured by exports plus imports and the deviation of the relative wage structure from the world average. He finds that openness is negatively related to the deviation of the wage structure from the average. His conclusion is, thus, that while other factors have been at work, the contribution of international trade has been to reduce the differences between national wage structures.

At the end of the paper, one cannot escape the conclusion that relative demand rather than relative supply shifts account for most of the increase in wage inequality in the 1980s in the United States and other advanced economies. How much of it comes from skill-biased technological progress versus reallocation of production because of increased trade remains unclear. Davis convincingly shows that trade has played some role. How much trade may widen income gaps within countries is a potentially explosive issue that remains largely for future research to establish.

But before you go out to do the research, you should read this interesting crop of papers. Bon appetit!

Olivier Jean Blanchard and Stanley Fischer