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Technology and Growth: An Overview

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During the 1990s, the Federal Reserve has pursued its twin goals of price stability and steady employment growth with considerable success. But despite—or perhaps because of—this success, concerns about the pace of economic and productivity growth have attracted renewed attention. Many observers ruefully note that the average pace of GDP growth has remained below rates achieved in the 1960s and that a period of rapid investment in computers and other capital equipment has had disappointingly little impact on the productivity numbers. Others see faster growth as softening the impact of widening income inequality or the stagnant real wages earned by many citizens.

Most of the industrial world has experienced a similar decline in trend and productivity growth, an increase in income inequality, and even slower job creation than we have seen here in the United States. While some (particularly Asian) developing countries are rapidly joining the ranks of the industrialized, most remain mired in poverty. According to the World Bank's recent report on poverty, over 20 percent of the world's population lives on less than one dollar a day. This situation wastes human talent and contributes to political instability.

While raising trend growth rates would not directly address distributional issues, increasing growth rates by even a fraction of 1 percent would, with compounding, have profound implications. As Robert Lucas has pointed out, "the consequences for human welfare are simply staggering. Once one starts thinking about them, it is hard to think of anything else." Unfortunately, economists and policymakers do not know how to engineer such an outcome. While the determinants of growth are widely agreed to be capital, labor, and a composite including managerial skills and organizational culture that Robert Solow abbreviated as "technology," the interrelationships among these variables are not clearly understood. In the developed economies, at least, recent large capital investments have shown surprisingly little positive impact

on productivity or potential growth. Accordingly, attention has increasingly turned to the role of such intangibles as human capital, social organization, and technology.

Because these puzzles are so compelling, the last few years have seen a resurgence in research on the economics of growth. This groundswell reflects the availability of new data bases and an improved ability to model imperfectly competitive conditions. Primarily, however, this enthusiasm indicates that many members of the economics profession concur with *The Economist* (June 1, 1996) that "understanding growth is surely the most urgent task in economics." For these reasons, the Federal Reserve Bank of Boston devoted its fortieth economic conference, held in June 1996, to *Technology and Growth*. We hoped to explore what we know and clarify what we do not know about these issues.

While research has not clearly shown that the technology embodied in widely available capital equipment has much impact on productivity, participants generally agreed that technology defined to include management, social organization, and culture is likely to be important.

A number of themes emerged from the discussions. For the most part these themes took the shape of questions repeated in various contexts. For example, one fundamental question asked throughout the meeting was just how important is technology—to growth, to productivity, to convergence? The answer, it was generally agreed, depends on one's definition of technology, with the majority favoring an inclusive approach. Most participants were sympathetic with the need to decompose technology into its constituent parts—innovation, development, and diffusion—and to include intangibles like organizational structure, management skills, and culture in the package labeled technology. Another theme that arose early on and reappeared throughout the conference was the unpredictable nature of technological change and the consequences of our uncertainty (or lack of imagination) concerning its ultimate path.

A third motif involved the role of innovation and the importance of knowledge-based spillovers within the growth process. While early work based on Robert Solow's model attributed most growth to exogenous technological change, more recent neoclassical research, exemplified by Dale Jorgenson's work, has greatly reduced technology's role by broadening our definition and improving our measures of capital. Indeed, Jorgenson concludes that human and physical capital accumulation, properly measured, explains almost all growth with little scope for innovation or knowledge-based spillovers.

But not everyone is fully persuaded that capital accumulation, however defined, can by itself account for the great bulk of welfare improvements experienced in recent decades. Noting a major inconsistency between the rate of convergence to steady state growth rates predicted by the neoclassical approach and the slower rate observed in fact, the new growth theorists give technological change, rather than capital, a bigger role in the growth process. They argue that technological change requires human effort and is, therefore, not exogenous, that the returns to R&D and other knowledge-based investments are not fully appropriable, and that spillovers from innovation have contributed importantly to growth. Naturally, thus, the new growth theorists stress the need to model the innovative process and the role played by these spillovers. While participants of both camps generally favored developing fully endogenous models, they disagreed about our current or potential ability to meet this challenge and, more basically, about its actual importance. In this regard, most, but not all, of the participants believe that spillovers are pervasive and significant.

A further theme was the need to be realistic in at least two areas. First, we need to acknowledge that potential growth may not return to its pace in the 1960s and that we may have to be satisfied with raising the level of output rather than the rate of growth. Economists also need to admit how little we understand about the growth process and how small are the likely consequences of the policy measures we advocate.

The conferees did agree on several points. Since the previous heyday of growth economics in the late 1950s, economists have greatly improved their ability to model the growth process by broadening their definitions and measures of physical and human cap-

ital. This development has reduced the role of exogenous technological change and narrowed the differences between the neoclassical and new growth theorists. Remaining areas for dispute and research include the need for modeling the various components of technology and the interactions between the determinants of growth and the growth process itself. Moreover, although research has not clearly demonstrated that the technology embodied in widely available capital equipment has much impact on productivity, participants generally concurred that technology defined to include management, social organization, and culture is likely to be important.

As for policy recommendations, conference participants largely agreed that the path of technical development and diffusion is highly unpredictable. Given this uncertainty and the gap between the social and private returns to R&D, most participants favored modest and balanced public support of basic research and other pro-competitive policies. They were less convinced about the benefits of the patent system.

On the macro side, participants universally endorsed the need to reduce fiscal deficits in order to promote saving and investment and the desirability of maintaining open trading systems in order to spur innovation. Several attendees advocated greater use of consumption-based tax systems. Many also saw an ongoing need for government investment in education and training, in limited amounts of R&D, and in improved statistical capabilities. Monetary policy's contribution was generally seen to be limited to maintaining price stability, but Bob Solow reminded us that balancing relatively tight fiscal policy with relatively accommodative monetary policy tends to favor growth. He also noted that below-potential growth discourages investment and innovation. Finally, if increasing productivity growth remains out of reach, some participants saw a need for more generous redistributive policies.

Keynote Address: The Networked Bank

In his keynote address, **Robert M. Howe** provided an intriguing view of how one industry—financial services—has responded to rapid technological change, and a vision of how that industry will be transformed with the introduction of technologies already in the development pipeline. Howe's vision is that of the networked economy: "the integration of people and institutions obtaining information, transacting business, entertaining and educating themselves in a connected world with electronic networks as the underlying backbone." In addition to detailing the modifications required of banks to survive in this networked environment, Howe shows where consumers fit into this system.

The networked bank has three components. The first component includes the access channels that link the consumer to the bank—ATMs, telephones, PCs, and bank tellers. Control over these channels rests in the hands of consumers and of third-party providers, such as on-line services. The second component is the "customer information and relationship management system," the bank's data base tracking customer activities to glean information about customer preferences. Howe suggests that effective use of this information—to tailor products to individual consumers or to determine the bank's most profitable market segments—will become the bank's "most valued asset and source for competitive advantage." The third component of the networked bank is the "core back-office system," which coordinates the operational systems, retail and commercial banking functions, and alliances with other service providers—for example, insurance firms or travel agents—that offer their services through the bank.

According to Howe, the bank's most valuable asset will shift from its branches, the current interface with its customers, to its customer data base and its expertise in extracting useful information from that data base.

Howe forecasts the emergence and widespread distribution of a suite of new technologies that will support the networked bank. These include improved communications interfaces, such as speech and handwriting recognition; three-dimensional, high-resolution graphics; and touch screens. Network infrastructure will improve rapidly in speed and price, and user-screening and encryption will enhance security. In addition, the continued miniaturization of processor and storage technology will allow smart cards with PC capabilities for financial transactions, inven-

tory control, or transmission of medical patient information. "Intelligent agent" software will respond to a consumer's complex queries; for example, "Go find me the lowest-priced Brand X automobile with the following features." Finally, networked banks will make greater use of new tools for data management, to analyze customers and transactions for targeted marketing campaigns.

These changes in the competitive environment pose new challenges to banks. Because the provision of a service will often involve a number of players, banks must establish "electronic value chains" that link the bank, the customer, perhaps a vendor, and a network infrastructure provider. Howe foresees a notable shift of power from banks to consumers and providers of access channels. With easy access to many options, a consumer may have little loyalty to a particular financial institution. A bank will need to differentiate its product from its easily accessible competitors, even when its product may appear only as a menu item on a screen. The bank's most valuable asset will shift from its branches, the current interface with its customers, to its customer data base and its expertise in extracting useful information from that data base.

How can the networked bank respond to these challenges? Howe proposes three possible strategies. The first, the "customer-centric" strategy, uses the bank's customer data base and data base analysis to serve each customer with unique, customized services. A second response is the "life-event" strategy: The bank becomes the provider of a cluster of services required by the consumer at key life events, such as buying a house or planning for retirement. A third option is the commodity strategy, in which the bank competes by providing standardized services through a wide range of access channels at the lowest cost.

Finally, Howe points out that these technological advances pose difficult questions for financial regulators. For example, does a global electronic financial system imply greater systemic risk to the payments system? How are standards of security and reliability established for new products? Will new clearinghouse organizations be required for new products? How are consumers to be protected if non-regulated industries can offer bank-like services? Who guards the consumer's right to privacy?

Technology in Growth Theory

Dale Jorgenson's paper traces the economics profession's understanding of technology and economic growth from the seminal works of Harrod (1939), Domar (1946), Solow (1956), and Kuznets (1971) to the more recent "endogenous growth models" of Grossman and Helpman (1994). In Jorgenson's view, the profession formed a rare and temporary consensus in the 1970s around the neoclassical growth model of Solow and the empirical work of Kuznets. Solow's simple theoretical framework, which decomposed contributions to output according to a constant-returns-to-scale production function with capital and labor as inputs, "provided conceptual clarity and sophistication." Kuznets' complementary work linking measures of capital and labor inputs to final output provided "persuasive empirical support" for the neoclassical growth model by documenting the correlation among inputs and outputs for the United States and 13 other developed countries over a long historical span. What stands out most for Jorgenson about these twin pillars of early growth theory, theoretical and empirical, is the lack of integration between them.

In early implementations of the Solow growth model, growth arose primarily as a result of increases in productivity. Because the reasons behind productivity increases were not understood, most economic growth was attributed to exogenous causes that largely reflected, as Abramovitz (1956) phrased it, a "measure of our ignorance." The contribution of investment in physical and human capital was assumed to be relatively minor.

Work by Jorgenson and others in the 1980s has attempted to diminish our ignorance by using carefully constructed measures of the inputs to production in an econometric model. The product of this research strategy is a model that fully characterizes the accumulation of human and physical capital and attributes almost all of economic growth to increases in the rate of capital accumulation, once properly measured. A truly satisfactory model of endogenous investment in new technology has eluded the profession thus far, however, in large part because of the difficulties inherent in measuring the output of the research and development sector (a problem first identified by Griliches in 1973).

Interest in growth theory waned in the 1970s, in the aftermath of the oil price shocks and a renewed attention to the determinants of business cycle fluctuations, but the debate over "convergence" in the 1980s and early 1990s revived interest, even as it challenged the validity of the Solow framework. Because the convergence debate focused on the long-run growth experience of nations, it brought to light a key ques-

tion that had not previously been addressed: Could private investment, whose returns accrue only to the investor, account for the leaps and bounds in output that some countries have observed over centuries? Or do we need "spillovers" in "knowledge capital," which may result from individuals' investment but which benefit all, to explain growth over long spans of time?

Jorgenson describes the essence of the convergence debate as follows: If Solow's model is approximately correct, then over a long enough period of time, a country will converge to its "steady state" or long-run rate of per capita income growth, which is determined by its saving and population growth rates. The Solow model predicts that the rate of convergence to the steady state will depend upon the share of capital in GDP, the rate of population growth, the rate of productivity growth, and the rate of depreciation of capital equipment. Using plausible estimates of these determinants for many countries implies a rate of convergence of about 4 percent per year. While empirical studies have found evidence of convergence, the estimated rate of convergence—about 2 percent per year—is too slow to be consistent with the Solow model.

An influential paper by Paul Romer (1986) highlights the inconsistency between the simple Solow model and the evidence on rates of convergence. Romer deduced that, for the slow observed rates of convergence to be consistent with the Solow model, the share of national income devoted to capital accumulation must be about twice as large as normally assumed. The reasoning is as follows: The larger is the share of national income devoted to capital accumulation, the more investment is required to increase output; the more investment is required, the slower will be the convergence to the steady state for a given investment rate.

Because doubling the share of income going to investment is just a "crazy explanation" of the slow-convergence puzzle, Romer and others suggest what they consider to be more plausible alterations to the standard growth model, such as increasing returns to scale in the aggregate production function, and spillovers of the returns to private investment to the rest of the economy. In their view, only these alterations can reconcile the standard growth model with the convergence data.

Mankiw, David Romer, and Weil (1992) find, however, that Paul Romer's crazy explanation is unnecessary and the Solow model can be resurrected once one controls for differences in human capital across countries. Allowing for these differences again reconciles the basic Solow model with the share of capital in the value of output and with the slow rates of convergence observed over time across countries.

A recent paper by Islam (1995) extends this work, allowing for different levels of productivity across countries. Islam's work shows that once one accounts for differences in the level of productivity, the Solow model captures well the endogenous accumulation of physical capital, without any need to account for the accumulation of human capital. Islam suggests human capital's contribution to changes in growth may not be as evident because it changes so slowly: While physical capital may completely adjust to changes in tax policy in a matter of decades, human capital may require a century to respond to changes in educational policy!

The complete econometric model developed over many years by Jorgenson and his colleagues attributes fully 83 percent of growth to the endogenous changes in capital and labor inputs.

Despite this evidence, Jorgenson continues, the proposition that private investment in physical and human capital is a more important source of growth than productivity remains as controversial today as it was in the early 1970s. Jorgenson believes that he has largely resolved this issue, however, with a perfectly competitive, constant-returns-to-scale neoclassical model that employs constant-quality indexes of both labor and capital input and investment goods output. The complete econometric model developed over many years by Jorgenson and his colleagues attributes fully 83 percent of growth to the endogenous changes in capital and labor inputs, with the remaining 17 percent accounted for by technological change and fertility rates. This finding essentially reverses the attribution of growth from that of Solow who found that only 12.5 percent of growth in per capita output could be attributed to capital accumulation (he did not consider human capital).

Discussant **Susanto Basu** assesses the success of the Jorgenson (and coauthors) research program according to its ability to explain three "fundamental questions of growth theory": (1) Why does per capita income increase over time? (2) Why are some countries rich and others poor? (3) Why has economic growth slowed down in developed countries?

With regard to the first question, Basu points out that Jorgenson treats technology as knowledge, which is a form of capital and behaves just like any other capital. The New Growth theory, by contrast, believes that the knowledge that propels technological advance differs from other capital in one crucial aspect: "Investors cannot fully internalize the benefits from accumulating knowledge." The presence of strong spillovers from private investment in knowledge can imply significant differences in the answers that Jorgensonian and New Growth theories give to the first question. The Jorgensonian rendering implies that in the very long run, no growth in per capita income can occur, since growth arises only from capital accumulation, and the marginal product of capital must diminish as capital accumulates. By contrast, the New Growth theory implies that the long-run growth rate of the economy will depend on the rate of accumulation of "knowledge" capital. Jones (1995) provides compelling evidence against the latter hypothesis for the United States and other advanced economies. Taking the inherent plausibility of knowledge spillovers together with Jones's evidence, Basu favors an intermediate position with modest spillovers, consistent with the Jones evidence and with the Jorgenson position.

Basu presents estimates showing that only a small portion of the slowdown in productivity can be attributed to a reduction in the growth rate of technology. He suggests that changes in the allocation of inputs across sectors may account for the bulk of it.

The work of Islam (1995) highlights a deficiency in Jorgenson's approach with respect to the second question, namely that differences across countries in income per worker cannot be explained by differences in capital per worker, as required by the Jorgenson model. That is, countries' production functions cannot be the same. To explain income differences, we require another factor of production that varies across locations, perhaps a factor that involves differences in the diffusion of technology or the degree of infrastructure in place, and thus drives a wedge between technological change and productivity.

Could this wedge also explain the observed slowdown (since the early 1970s) in productivity in advanced countries? Basu suggests that it may. Using the methods of Basu and Fernald (1995), he presents estimates showing that only a small portion of the slowdown in productivity growth can be attributed to a reduction in the growth rate of technology. Basu suggests that changes in the allocation of inputs across sectors may account for the bulk of the productivity slowdown. He concludes by agreeing that Jorgenson's paper documents the explanatory power of the neoclassical model augmented by careful measurement. He believes, however, that the model will need to be amended to allow for some spillover effects.

Discussant **Gene Grossman** focuses on four key questions about the role of technology in growth theory. First, "Is technological progress needed to sustain growth?" Grossman notes that, technically, our economy could grow indefinitely without technological enhancements if we continue to invest in physical and human capital *and* if the returns to doing so always remain above a minimum level. However, he suggests that long-run growth with such static technology is implausible. In the presence of factors in fixed supply, such as land and fuels, capital must eventually experience severely diminishing returns. Would the world economy have evolved as it has over the past 200 years in the absence of all the innovations introduced in that period—without steam engines, electricity, or semiconductors? Adding more and more shovels and horses would not have allowed us to reach today's level of output. A role for technology in long-run growth seems mandatory.

A second question is whether innovation represents the product of intentional activity and is thus "endogenous" to the economy, or not. Grossman suggests that innovation is endogenous; the firms that spend in excess of $100 billion on R&D must be doing so for a reason. He also cites the evidence in Baumol that innovations vary across history in response to variation in incentives facing innovators.

Third, Grossman asks whether "formal" R&D is

responsible for the bulk of technological progress. The evidence presented by Jones (1995) suggests not: The long-run surge in R&D activity in the postwar period has not been accompanied by equal surges in the growth of per capita output, and the decline in productivity since 1973 does not seem to be explained by declining R&D (Griliches 1988). Perhaps this mismatch of R&D and output growth reflects a focus on the use of "formal" R&D, which may not measure efforts to improve manufacturing processes or organizational structures, or, more generally, to innovate at the margin.

Finally, Grossman asks whether the market-determined level of R&D investment is socially optimal. The answer to this question depends upon the existence of knowledge "spillovers": Knowledge gained from one firm's investment makes research more productive for other firms, while the other firms need not compensate the originating firm for this knowledge. When spillovers exist, the *social* returns to investing, which include the returns to those who did not pay for the investment, exceed the *private* returns. Jorgenson is skeptical of the existence of such spillovers, but Grossman reads the bulk of the empirical evidence as pointing to social returns to R&D investment that are more than twice as large as private returns. Does the presence of excess social returns suggest an investment tax credit or subsidy to foster innovative activity? Not necessarily; as Mansfield (1986) points out, R&D tax credits often encourage firms to relabel existing activities as investment, rather than to undertake new research.

Grossman notes that an economy could grow indefinitely without technological enhancements, but suggests that long-run growth with static technology is implausible. Would the world economy have evolved as it has over the past 200 years without steam engines, electricity, or semiconductors?

Grossman acknowledges the important contributions of the neoclassical framework, favored by Jorgenson, to growth theory. However, he points out limitations of the model that make it "not well suited for studying innovation": The neoclassical model assumes constant returns to scale and perfect competition. Investment in knowledge, on the other hand, requires large up-front fixed costs that imply *increasing* returns to scale, and pricing in excess of marginal costs to recover high fixed costs, in violation of the assumptions of perfect competition. Thus, Grossman feels, one must study innovation in a setting that allows for imperfect competition, even when this makes policy prescriptions more difficult.

Uncertainty and Technological Change

Nathan Rosenberg examines the relationship between uncertainty, technological change, and economic growth. Rosenberg's approach to the topic is, he admits, anecdotal; but he discusses many of the most important innovations of this century, demonstrating the influence of uncertainty for technologies that have had tremendous economic impact.

Many of Rosenberg's primary conclusions are exemplified in his study of the laser. The laser currently has dozens of applications, from producing CDs to enabling delicate eye surgery, from an essential instrument in chemical research to the rapid carrier of data, voice, and optical information across telecommunications lines. And yet the initial developers of the laser at Bell Labs not only could not foresee these applications, but did not think the invention worthy of a patent application, since "such an invention had no possible relevance to the telephone industry." This lack of foresight was not a malady unique to the telecommunications industry or to potential users of lasers; the same inability to predict the general usefulness of an invention, let alone its particular uses, extends to the developers of the telephone, the computer, the transistor, the jet engine, and the radio.

What categories of uncertainty make it so difficult to foresee the usefulness of innovations? Rosenberg catalogues several. First, new technologies arrive on the scene with characteristics that do not immediately or obviously lend themselves to application. For example, new techniques for visualization in medicine, such as CAT scanners and magnetic resonance imagers (MRIs), were developed before it was known how to interpret their output in a clinically useful fashion. Significant additional research was required to render

the innovation not only technically feasible but also usable by doctors and technicians in making diagnoses.

A second class of uncertainty arises when the success of invention A depends on improvements in complementary invention B, which may not exist at the time invention A is introduced. Take, for example, the use of lasers in communications. Only upon the development of fiber optics, and upon understanding how laser light could be transmitted through fiber optic cable, did lasers become a viable communications medium. When the success of the innovation depends upon a system of complementary innovations, as may be the case with computer technology, the length of the gestation period from inception to a full menu of uses may be decades.

Rosenberg suggests that the increased emphasis on the "relevance" of research to social and economic needs is misplaced; we cannot know which research or development will turn out to be relevant, or relevant to what!

A third class of uncertainty arises because many inventions were designed to solve very specific problems. For example, British engineers invented the steam engine in the eighteenth century to pump out flooded mines. The possibility that such an engine could be used in entirely different industries, for transportation or power generation for manufacturing, became evident only after many decades, during which time a sequence of improvements were made to the initial invention.

Finally, Rosenberg identifies uncertainty about the marketability of an invention. As he puts it, inventions need "to pass an economic test, not just a technological one." When Marconi invented the radio, he did not possess David Sarnoff's vision of a new medium "to transmit news, music, and other forms of entertainment and information into every household in the country." Without someone to anticipate and champion the commercial possibilities of the technology, the radio might have gone the way of the buggy whip.

In concluding, Rosenberg draws out the policy implications of the almost overwhelming uncertainty involved in technological innovation. First, he suggests that the increased emphasis on the "relevance" of research to social and economic needs is misplaced; we cannot know which research or development will turn out to be relevant, or relevant to what! For the same reasons, the government should not attempt to support a single technological approach to a problem, or one narrow area of research. These caveats do not necessarily apply to the private sector, however. In the face of uncertainty, Rosenberg asserts, the market will of its own accord encourage individual firms to pursue a wide array of research strategies, which, given uncertainty, is more likely to produce a useful innovation.

Joel Mokyr is largely sympathetic to Rosenberg's characterization of the uncertainty (or perhaps ignorance) facing decision-makers, but he suggests a modest reinterpretation. First, Mokyr posits two levels of uncertainty in technological change, the firm's *micro*-uncertainty, and the economy's *macro*-uncertainty. The former comprises a host of firm-level questions: Can this particular technical problem be solved? Can this firm solve it? Will we arrive at the answer first? Will it sell, or sell profitably? At the macro level, uncertainty involves which technological regime will dominate: nuclear or fossil fuels? Both levels of uncertainty figure prominently in the decisions of potential innovators.

Mokyr poses an analogy between evolutionary biology and technological innovation. The analogy holds in two regards. First, innovations, like mutations, occur at least somewhat randomly, and thus we do not know in advance what the future supply of innovations will look like. The degree of randomness likely differs between biology and technology, as the latter presumably attempts to respond to economic need. However, Mokyr and Rosenberg agree that while the correlation between need and mutation "may not be zero, it is not very high either."

Second, we do not well understand the "laws" that determine whether a particular mutation will be *selected* or not, in the biological case by natural selection, and in the case of technology by the market. Success in many instances depends on luck; Mokyr points out that 70 percent of all new products that make it to the distribution stage disappear again within 12 months. This high mortality rate underscores the poverty of knowledge, even among the innovators themselves, about the laws that determine which innovations will be successful.

Mokyr adds a third "evolutionary" process that is germane to understanding the uncertainty in innovation: the evolution of economic institutions. As Douglass North (1990) has emphasized, institutions evolve in a way that is no more predictable than the evolution of science and technology.

But the situation is even more complex, as the sources not only evolve but *co*evolve. Many institutions—free labor markets, enforced property rights—are good for technological development, whereas others—uncertain property rights, totalitarian government—clearly are not. Modern innovators need to know how the institutional climate will be when they bring their product to market. Will the FDA approve it? Will I get sued? Will it pass environmental restrictions? Only as institutions friendly to innovation evolve with technology will technology succeed.

If the private benefit to changing technologies does not cover the social costs of not changing, Mokyr suggests, another role for the government may be to spur such transitions.

Mokyr concludes with reference to a final biological/technological debate, between "adaptationists" and "anti-adaptationists." Do technology and living species adapt so that we see only efficient technological and biological outcomes, or do important examples exist of innovations (mutations) that are clearly suboptimal and persistent? Is the dominance of the Qwerty keyboard a result of inefficient lock-in and path-dependence, or do we not properly understand its inherent efficiency? Mokyr declines to take a firm stance on this issue, but notes a difference between the biological and technology versions of the debate. The biological adaptation debate involves a more constrained evolutionary process: A species can adapt or become extinct. Technology is somewhat less constrained; societies can, at least in principle, adopt a completely different technology very rapidly, albeit at significant private and social cost. Does the private benefit to changing technologies cover the social costs of not changing? If not, another role for the government may be to spur such changes when private benefits fall short of total social benefits.

Luc Soete cautions against drawing broad conclusions from the anecdotal evidence presented by Rosenberg. The innovations chosen by Rosenberg may have sparked the interest of historians precisely because they had such unanticipated success; if so, they may not be truly representative. Soete also suggests that sectors vary greatly in the type of uncertainty facing their research efforts. A drug firm that pursues hundreds of leads on a trial and error basis faces a different kind and magnitude of uncertainty from a chip manufacturer that is developing the next generation that will double processing speed.

Soete questions whether omnipresent uncertainty could explain the productivity slowdown. Do the productivity gains that we expect from, for example, information processing technologies, seem to lag their invention because of the difficulties in identifying their most efficient uses? "You ain't seen nothin' yet" is the optimistic buzz-phrase of this explanation.

Soete proposes two other equally plausible explanations of the "missing productivity." The first is the difficulty inherent in measuring the output of information goods and services. As suggested by Nakamura (1995), the failure to properly capture the consumer surplus generated by the vast array of new electronic and communications products recently made available will likely underestimate output growth, perhaps by enough to account for the missing productivity. The second explanation centers on the possibility that the short-term disinflationary monetary policies of the 1980s, which significantly increased real long-term interest rates, may have turned businesses' research focus to short-term R&D with immediate payoffs, at the expense of longer-term, more uncertain research.

Soete questions whether omnipresent uncertainty could explain the productivity slowdown, and he proposes two other explanations.

Cross-Country Variations in National Economic Growth Rates: The Role of "Technology"

J. Bradford De Long's paper attempts to explain two striking observations about the cross-country distributions of living standards and growth. The first is that the cross-country disparity of per capita real incomes has increased markedly over the past two centuries. The second is that the growth rates of real income in individual countries seem to be converging to the pace that is consistent with their rates of investment and population growth (as documented in the work of Ball, Mankiw, and Romer 1988).

Broadly construed, De Long's explanation works as follows. He notes that the countries that were relatively poor 200 years ago are relatively poor today, and those that were relatively rich 200 years ago are relatively rich today, and that the gap between the rich and the poor is increasing. According to the neoclassical model, if each country had started with somewhat different endowments of labor, capital, and materials but had access to the same technology, then over long spans of time, all countries would approach the same level of real per capita income. The long-run divergence of incomes argues against this simple case. If, however, the rich countries enjoy *amplified* effects of technology improvements on standards of living, while poor countries do not, then we will not observe even a gradual convergence of living standards.

De Long concludes that technology, broadly defined as differences in productivity, explains much of the disparity in standards of living across countries, while technology, narrowly defined as the possession of the most modern machinery and manufacturing processes, explains relatively little.

De Long's paper identifies two novel sources of income divergence, each of which rests on a magnified long-run effect of productivity on real per capita income for richer countries. The first source is the strong endogeneity of population growth with respect to productivity and income. Countries with high productivity and thus high real incomes tend to have lower population growth rates. De Long shows that, for the United States, each tripling in real per capita GDP is associated with a 1 percentage point fall in the annual rate of population growth. De Long suggests several explanations for this pattern. More prosperous countries are often more educated countries, and better-educated women demand better birth control; in poor countries, the average number of years of schooling is low, and children are more valuable to production there because they can be put to work at an earlier age. In other words, children in poor countries are "investment goods" rather than "consumption goods," as they are in rich countries. Other things equal, then, a country that experiences rapid growth through increasing productivity will experience lower population growth that will, in turn, raise income per capita.

The second magnification effect arises from the endogeneity of the relative price of capital. Prosperous countries tend to benefit from a low relative price for investment goods. Most wealthy countries have achieved their prosperity largely through attaining high levels of manufacturing productivity. This achievement implies a relatively low price for manufactured goods, including the investment equipment that firms use to produce more goods. In support of the negative correlation between prosperity and the price of capital, De Long notes that the real purchasing power of domestic currency in foreign markets can be as much as eight times higher in rich countries than in poor countries. The disparity in real purchasing power directly reflects the difference between the relative price of easily traded goods, such as physical capital, in richer and poorer countries. This negative correlation between prosperity and the price of capital also magnifies the effects on real incomes of changes in productivity: As productivity and real incomes rise, investment goods become cheaper, and the economy can afford more investment goods for a given pool of savings, thus affording further increases in productivity.

De Long shows that the combined effect of these productivity magnifiers is substantial. Including them implies that the estimated effect of a productivity increase on the steady-state level of output is orders of magnitude larger than simple growth accounting would suggest. These important endogeneities be-

tween income, population growth, and physical investment could go a long way toward explaining the extreme divergence in national incomes that we have observed over the past two centuries.

Thus, De Long concludes that technology, broadly defined as differences in productivity, explains much of the disparity in standards of living across countries. He notes, however, that technology, narrowly defined as the possession of the most modern machinery and manufacturing processes by a particular country, explains relatively little of the differences in per capita incomes across countries. He cites work by Clark (1987) that shows remarkable differences in output per hour in cotton textiles across countries in the early twentieth century, even though many of these countries used exactly the same textile machinery. The McKinsey Global Institute's study (1993) of cross-country productivity differences reveals similar puzzles: Japan appears to be 47 percent more productive than the United States in steel manufacture, but 67 percent less productive in food processing. It seems unlikely that Japan is adept at using and refining the best manufacturing procedures for steel manufacture, yet is completely inept at "learning and developing technologies for making frozen fish."

Reacting to De Long's observation concerning the link between income and population growth, **Jeffrey Frankel** points out that "a prime motive in poor countries for having many children is that they provide the only form of insurance against destitution in old age." As a country develops, its financial institutions develop with it, and the increased accessibility of savings instruments can substitute for a high ratio of children to working-age population as a savings plan.

Frankel also observes that De Long's hypothesis about the endogeneity of both population growth and the price of investment goods suggests a timing test: Under De Long's interpretation, one ought to see significant decreases in population growth or increases in investment rates *following* surges in real growth. Frankel finds little evidence in the data for East Asian countries that declines in population growth are more likely to follow peak growth rates than to precede them. Investment rates follow peak growth rates in some cases, perhaps confirming De Long's hypothesis. However, the data also show large increases in investment that predate the peak in growth rates and could, thus, be considered the proximate cause of subsequent growth, contrary to De Long's interpretation.

Frankel ends by noting De Long's omission of a critical determinant of differences in growth across countries: openness to trade and investment. A large body of empirical work finds openness to be an important contributor to growth, even accounting for differences in factor accumulation. The economies that have converged are those that are open, whether

Frankel notes that openness to trade and investment is "how countries absorb the best technology," whether we construe technology narrowly, as in the most up-to-date equipment, or more broadly, to include managerial and organizational techniques.

across the OECD, across Europe, or within the United States. The reason, according to Frankel, is that "openness is how countries absorb the best technology from the leaders," whether we construe technology narrowly, as in the most up-to-date machinery and equipment, or more broadly, to include managerial and organizational techniques. In addition, openness to trade is part of a self-reinforcing pattern of growth: Countries that open their boundaries to trade grow more, but countries that have grown also tend to lower tariffs and promote trade.

Adam Jaffe presents cross-country evidence supporting the effect of income on population growth. Real per capita income and population growth exhibit a strong negative correlation, with an increase in per capita income from $1,000 to $10,000 associated with a decline in population growth from 2.5 percent per year to 1.5 percent. Of course, the link between income and population growth is partly mechanical: As population grows, holding income constant, per capita income must fall. But Jaffe shows that the strength of the correlation could not arise exclusively from this mechanical relationship. Suppose two countries begin with the same per capita income, but the population of one grows at 1.5 percent while the other grows at 2.5 percent. The low-population-growth country will reach an income 10 times the rapid-population-growth country only after 156 years! It is plausible, therefore,

that much of the cross-section variation in income and population growth rates arises because high income causes low population growth, and not vice versa.

Jaffe suggests that the negative relationship between real income and population growth is not continuous. The correlation falls substantially for incomes above the median, and vanishes for countries with per capita incomes above $10,000. Thus, the returns (measured in lower population growth) to higher income appear to cease above this threshold income level. This observation alters De Long's story somewhat. Once the one-time demographic threshold is crossed, no further population growth effect would occur for the rich country.

Jaffe also clarifies the explanation for the observed correlation between income and the price of investment goods. Productivity improvements must (by definition) make goods and services cheaper. Because most of the productivity enhancements of the past century have been concentrated in manufactured goods, the real price of manufactured goods has fallen faster than the real price of services. As investment is likely to draw more heavily on manufactured goods than on services, the relative price of investment goods will also fall as productivity rises. The importance of this observation is that the apparent feedback between income and the price of investment goods can arise from productivity increases in an autarkic country, and thus does not depend upon foreign trade. The correlation between the real purchasing power of domestic currency and growth simply reflects underlying differences in productivity improvements across countries.

Jaffe suggests that the negative relationship between real income and population growth is not continuous. It vanishes for countries with per capita incomes above a threshold level.

Finally, Jaffe questions the usefulness of a debate over which inputs to production should be labeled "technology." Echoing comments made by a number of participants during the conference, Jaffe finds it more useful to expand the list of potential explanations of differences in growth across regions and sectors. He suggests that a deeper understanding of the importance of hardware, software, human capital, ideas, and institutional and market factors in production may help us better explain differences in productivity and growth.

Luncheon Address: Job Insecurity and Technology

Alan Greenspan's address focuses on human reactions to the structural changes caused by modern computer and telecommunications technologies. Pointing to the paradoxical pervasiveness of insecurity and malaise in a period of extended economic growth, restrained inflation, and a comparatively low layoff rate, he examines the origins of this anxiety and suggests ways of alleviating it.

He sees modern societies as having evolved from a time when the creation of economic value depended on physical brawn and physical product to the present when ideas are the critical input. This accelerating trend has had two important consequences: It has played a major role in changing the distribution of income in this country, and it has created a sense of foreboding in a large part of the work force.

Expanding on the first outcome, Greenspan explains that as ideas have become critical to the creation of economic value, education and intellectual skill have become increasingly important determinants of earned income. Although the supply of college graduates rose with demand in the 1960s and 1970s, by the 1980s the demand for skilled workers was apparently outstripping supply. The seeming result was a rise in the compensation of college graduates relative to that of less-educated individuals. Because the growth in real incomes slowed markedly in the mid 1970s—reflecting a similar (and not fully explicable) slowdown in productivity growth—widening income disparity has meant that parts of the work force have experienced stagnant or falling real incomes and understandably feel rooted to a treadmill.

Greenspan suspects that an even larger share of the work force is suffering from the job insecurity caused by rapid technological change. This group, composed of relatively skilled, experienced, and well-paid individuals who interact closely with our high-tech capital stock, are acutely aware of the speed at which this stock is being radically transformed. As a

consequence, they fear that their own job skills may suddenly become obsolete. Greenspan suggests that these fears have led to an extraordinary period of labor peace, with a preference for job security over wage hikes, lengthening labor contracts, and unusually subdued strike activity.

Given widespread recognition of the growing income disparity, labor's acquiescence is somewhat surprising. Still, the relative economic welfare of low-income workers may not have deteriorated as much as the rising disparities in the distribution of income and wealth suggest. For example, recent work by Johnson and Shipp (1996) finds that the rise in consumption inequality since 1981 is only three-quarters as large as the rise in income inequality. Since purchases of consumer durables provide services throughout their useful lifetimes and are more akin to investments, the distribution of consumer durables deserves special attention.

Greenspan focuses on human reactions to the structural changes caused by modern computer and telecommunications technologies. He suggests that the solution to the malaise created by rapid technological change involves finding ways to enhance skills.

Since 1982, household ownership of consumer durables has grown at an annual average rate of 3.3 percent a year, a slightly faster rate than in the 1960s and 1970s. Moreover, according to data provided by Stephanie Shipp and her colleagues at the Bureau of Labor Statistics, while ownership of consumer durables clearly rises with income, the distribution of ownership rates across income groups for cars and many appliances actually became more equal between 1980 and 1994. By exception, the disparity in ownership rates for personal computers remains large—unfortunately, given that knowledge of computers is linked to economic success.

Stressing that economic security depends on much more than owning selected consumer durables, Greenspan argues that the solution to the malaise created by rapid technological change involves finding ways to enhance skills. Since education has clearly become a lifetime activity, it is fortunate that many companies are beginning to see that human capital development is crucially important to improving profitability and shareholder value. He hopes that this approach will also help to reduce income disparities.

While the twenty-first century is likely to remain just as fast-paced as the recent past, Greenspan concludes, individuals currently entering the work force are used to rapid change and many six-year-olds are computer literate. Thus, as in previous periods of great structural change, the current frictions and uncertainties will diminish as people learn to adapt.

Microeconomic Policy and Technological Change

Reviewing the impact of public policies towards R&D spending, patents, and competition on innovation, **Edwin Mansfield** argues that government has a major influence on the rate of technological change in major industries. He points out that the federal government finances about 35 percent of all U.S. R&D investment and 60 percent of the R&D performed by colleges and universities. He provides two rationales for these expenditures. First, where government is the primary purchaser of public goods, like national defense or space exploration, the government clearly bears primary responsibility for promoting related technological change. In addition, much federal R&D is directed towards basic research because market failures or spillovers could cause private sector investment to fall short of socially optimal levels.

However, it is not self-evident that R&D spending is actually suboptimal. In many oligopolistic markets, product improvement is a major form of competition. As a result, R&D spending might actually exceed socially desirable levels in such industries. In addition, the government currently subsidizes R&D activities through the R&D tax credit and various grant programs. Thus, the government may already have offset any tendency for the economy to underinvest in R&D.

To address this issue, Mansfield reviews empirical estimates of the social rate of return from innovation, a body of work to which he has made major contributions. He starts by showing that the social benefits from an innovation equal the sum of the gains to consumers from the resulting decline in prices and society's resource saving (alternatively, the innova-

tor's profit). Arguing that a high social rate of return signals a productive investment, Mansfield reports that empirical studies consistently find the median social rate of return from innovation to be substantial (the lowest median cited was 56 percent), even when private returns were low or negative.

The gap between the social and private rates of return from innovation provides an important rationale for government support of civilian technology. But, while a remarkable number of independent studies find the gap between marginal social and private rates of return to be sizable, many economists suspect that federal intervention could do more harm than good. Accordingly, Mansfield offers guidelines for public R&D support programs. First, given the huge uncertainty surrounding R&D outcomes, government incentives should remain modest, encourage parallel approaches, and provide information for appraising the desirability of further support. Such programs should not aid declining industries or late-stage development work. Recommending a pluralistic, decentralized approach, Mansfield also suggests that potential users of new technologies play a role in project selection so that public R&D efforts reflect market realities.

Despite widespread skepticism about the value of the patent system, Mansfield acknowledges that few economists would recommend abolishing it, given our limited understanding of its impact.

Mansfield's paper then reviews the pros and cons of another important instrument of national technology policy, the patent system. Some supporters argue that patent protection provides necessary incentives for innovation and development activities by slowing the introduction of relatively low-risk, low-cost copycat products. Other proponents assert that the patent filing process actually speeds the disclosure and dissemination of new technologies. Critics complain that the patent system creates usually weak but sometimes self-sustaining monopolies that slow the spread of new information. Still others conclude that patents have minimal importance, especially for large corporations; firms keep secret what inventions they can, they say, and patent those they cannot.

Turning again to empirical results, Mansfield reports that while patent protection does not make entry impossible or even unlikely, it does raise the cost of imitation. According to one study by Mansfield, Schwartz, and Wagner (1981), patenting raised the median imitation cost by 11 percent—30 percent in ethical drugs and 7 percent in electronics. Despite widespread skepticism about the value of the patent system, Mansfield acknowledges that few economists would recommend abolishing it, given our limited understanding of its impact.

Mansfield's paper ends with a discussion of the effects of market structure and antitrust policy. He concludes that while market entrants often play an important role in promoting technological change, some R&D activities exhibit economies of scale. Since a complementary mix of firm sizes appears to benefit technological change, public policy should aim to eliminate unnecessary barriers to entry and discourage industrial concentration.

The theme of **Samuel Kortum**'s comments is that the effectiveness of government technology policy depends crucially on the responsiveness of technological change to research effort, and that the evidence about the actual impact of research activity on innovation is weak. Although a vast literature has uncovered a systematic relationship between growth of total factor productivity and research effort (R&D/sales), Kortum points out that these studies provide no evidence concerning the direction of causality in this relationship.

Kortum raises the provocative possibility that technological change may be largely impervious to government incentives—if, for instance, innovation is an exogenous process more dependent on the chance arrival of technological opportunities than on incentives to exploit them—and sets out to show that this idea is not so easy to disprove. To do so, he develops a model in which R&D spending is the means by which firms compete for patent rights to innovations that arise within the economy regardless of the level of research activity. The larger a firm's share of industry spending on R&D, the greater is the probability that it will win patent rights valued at the industry's cost savings from the innovation. If the above model describes the real world, a cross-industry estimation of the impact of R&D effort on total factor productivity will reflect the fact that R&D effort depends on the value of exogenous innovation.

In Kortum's model with exogenous technical change, the private rate of return to R&D is the interest rate, but the social rate of return is −100 percent since the marginal expenditure has no benefit for society. Even careful economists, like Mansfield, who sum all research costs for losing as well as winning firms in calculating the social rate of return on R&D, are likely to find huge social payoffs—erroneously if innovation is actually exogenous. Although Mansfield and his coauthors state that social benefits should be measured only between the date when the innovation occurred and the date when it would have appeared if the innovator had done nothing, Kortum questions the validity of survey work based on hypothetical questions about the timing of competitors' innovations.

To provide additional evidence as to whether innovation is endogenous or exogenous, Kortum recommends careful analysis of the impact of a specific policy change, like the increased patent protection stemming from the 1983 creation of a single appellate court for patent cases. If technological change is actually exogenous, then such a policy shift should have no impact on productivity. By contrast, evidence that the policy action raised productivity would be highly suggestive of endogenous technological change.

Kortum raises the provocative possibility that technological change may be largely impervious to government incentives.

Joshua Lerner focuses his comments on Mansfield's policy prescriptions. In particular, he asks whether technology policy should recognize that small firms generate a disproportionately large share of major innovations, since, as Mansfield and others have pointed out, many studies find that start-ups play a big role in applying radical technologies. Although key innovations are usually developed with federal funds at universities or research labs, small firms are often the first to act upon the commercial possibilities. As important examples of this phenomenon, Lerner cites the development of biotechnologies and the Internet. Given the uncertain path of technical developments and the critical role often played by previously unknown firms, Lerner is skeptical of Mansfield's stress on a "proper coupling between technology and the market" and his prescription that federal R&D be directed with the advice of potential users.

Lerner next addresses issues raised by the patent system, particularly the impact of the single court of appeals for patent cases established in 1983. Lerner argues that the new court has produced more pro-patent rulings than the previous system—with the result that large and small firms are putting more effort into seeking new and defending old patent protection. Viewed broadly, Lerner contends, the consequent growth in patent litigation has created a substantial "innovation tax" that falls particularly hard on small firms. In a recent research effort Lerner (1995) has found that patent litigation begun in 1991 will lead to total legal expenditures amounting to more than one-quarter of the private dollars spent on basic research; the indirect costs of this litigation are also substantial. Survey results suggest that these costs are a more important deterrent to development efforts for small firms than for large firms. Accordingly, reforms intended to protect and spur innovation have actually discouraged entry. Lerner is concerned that efforts to make federal research commercially relevant could have the same effect.

Lerner asks whether technology policy should recognize that small firms generate a disproportionately large share of major innovations.

Technology in U.S. Manufacturing: The Geographic Dimension

Continuing with a micro perspective, **Jane Sneddon Little** and **Robert K. Triest** explore the process by which advanced technology enters general use. Using relatively new data from the U.S. Census Bureau's Surveys of Manufacturing Technology (SMTs) for 1988 and 1993 (U.S. Bureau of the Census 1989 and 1994), their paper examines variations in the adoption of 17 advanced technologies across the nation and

within individual states. The authors consider a variety of plant and locational characteristics that might raise the probability of technology use, but they are particularly interested in whether proximity to firms already using advanced technologies fosters adoption. Proximity to early users might affect adoption decisions by reducing the perceived risk and actual cost of investing in this new equipment.

Little and Triest estimate a set of econometric models that control for the effects of plant, firm, and locational characteristics. As measures of technology diffusion, the authors examine the change in the number of advanced technologies used by SMT establishments between 1988 and 1993, the number of technologies used in 1993, and the probability of adopting a particular technology by specified dates covered by the SMT survey. In each case, the authors first control only for proximity to other users of advanced technologies. They then add in a set of plant and firm characteristics, such as size and industry. As a final step, they include a set of locational characteristics, like educational attainment of the work force, in the group of explanatory variables. In all three estimations, proximity to early users almost always has an economically and statistically significant positive effect on technology adoption, not only when proximity is the only explanatory variable but also when plant characteristics are taken into account. While introducing locational characteristics always reduces the coefficient on proximity, these coefficients still remain positive and statistically significant in the equations for the number of technologies used in 1993 and for the change in number of technologies used. By contrast, for the models estimating the probability of adopting specific technologies over a span of years, the proximity variable generally loses its significance when the geographic variables are added.

Little and Triest conclude that proximity to other users of advanced technologies is associated with higher rates of adoption.

Little and Triest conclude that proximity to other users of advanced technologies is associated with higher rates of adoption, even when industry and other plant characteristics are controlled. They find this result noteworthy since, with its well-developed communications networks and national markets for capital goods and skilled workers, the United States might be expected to approach the limiting case of immediate, costless diffusion of technology. Human capital appears to be an important part of the proximity effect, they speculate, because, among the locational variables, access to a work force with a high school education or some technical training is associated with a higher rate of technology adoption. Some of the remaining proximity effect may reflect the impact of social interactions in spreading technical information.

Although the authors were not able to separate proximity/spillover effects from the impact of educational attainment/university R&D to their satisfaction, they believe that the evidence of uneven technology diffusion warrants further research. Because technology adoption is extremely expensive for individual firms and the nation, gaining a better understanding of this process remains an important goal.

John Haltiwanger's comments on the Little-Triest paper center on his concerns about data and measurement issues and about the appropriate interpretation of their results. Citing recent research by Dunne and Troske (1995), Haltiwanger points out that the answers to the retrospective questions in the 1993 SMT on the timing of technology adoption appear subject to substantial recall bias. Respondents systematically date adoption more recently than was actually the case. As a result, Haltiwanger suggests, the Little-Triest variable measuring the change in the number of technologies used may actually be a better measure of the number of technologies in use in 1993. Thus, although Little and Triest find some evidence of clustering, the timing problems raise questions about the direction of causality and the underlying source of this clustering.[1]

Dunne and Troske's work raises another important issue, Haltiwanger contends. Their 1995 study finds evidence of significant rates of de-adoption for

[1] In response to Haltiwanger's comments concerning their use of retrospective data, Little and Triest reran their regressions using the subsample of firms responding to both the 1988 and 1993 SMTs. Relying on current rather than retrospective data on technology use did not change the flavor of their results. If anything, the change strengthens the impression that proximity affects technology adoption. For details, see Little, Jane Sneddon and Robert K. Triest, "Technology Diffusion in U.S. Manufacturing: The Geographic Dimension," footnote 43, in *Technology and Growth*, Federal Reserve Bank of Boston Conference Series No. 40, 1996.

specific technologies. For example, for the matched sample of plants responding to both the 1988 and 1993 SMTs, 39 percent of the establishments using local area networks in 1988 were not using them in 1993. This finding suggests additional measurement problems or the intriguing possibility that firms experiment with new technologies that they eventually decide not to use. If so, a region that is relatively slow to de-adopt should not be labeled "advanced," Haltiwanger suggests.

Haltiwanger notes that the growth process is noisy and complex, and he suggests caution in interpreting empirical results.

Haltiwanger then takes up a line of argument similar to that raised by Samuel Kortum: "Does the adoption of advanced technologies actually affect outcomes we really care about—the growth of income or employment or productivity?" While one might presume such a connection, work by Doms, Dunne, and Troske (1995) suggests that differences in technology use are not particularly meaningful. Although Doms, Dunne, and Troske find that advanced technology use has a significant positive effect on plant-level labor productivity, differences in technology adoption account for only 1 percent of the total variation in labor productivity across plants. Moreover, these authors find no statistically significant link between technology adoption and the growth in plant-level labor productivity. (Perhaps the failure of micro studies to find much connection between the adoption of new technologies and productivity levels or growth should not be so surprising, given our similar inability to find any productivity payoff to vast investments in new technologies at the macro level.)

Alluding to research stressing the dominance of idiosyncratic factors and the importance of the reallocation process steering resources from less to more productive plants, Haltiwanger suggests caution in interpreting empirical results concerning technology diffusion. Seemingly, the growth process is noisy and complex, and the required resource reallocation is time-consuming.

In commenting on the Little-Triest paper, **George Hatsopoulos** provides the perspective of his many years of experience in managing high-technology companies. He interprets proximity as representing local management culture or standard technological practice within a given area. In this context he finds that the authors' conclusions correspond with his own observations.

Hatsopoulos starts by emphasizing the relative importance of diffusion—compared with innovation—in determining a country's technological sophistication. Like John Haltiwanger, he also finds that intangibles like managerial and organizational skills, and labor-management relations, exert an extremely important influence on micro and macro productivity levels.

Turning to Little and Triest's empirical results concerning the impact of proximity on the probability of technology adoption, Hatsopoulos reports that this finding matches his observation that decisions about the use of specific technologies are determined by middle managers and foremen who, in turn, are heavily influenced by prevailing practice at neighboring plants. To illustrate this point, he cites the example of two plants, one in Manchester, England and one in Auburn, Massachusetts. Although the two were making identical products for the paper industry, labor productivity in Manchester was about half that in Auburn. The problem, it turned out, was that managers and workers in Manchester were extremely reluctant to import manufacturing and organizational technologies that headquarters had found useful in the United States but that were uncommon in Britain. Because these workers were very heavily influenced by local practice, Thermo-Electron had a very hard time trying to change their behavior.

Hatsopoulos finds that intangibles like managerial and organizational skills, and labor-management relations, exert an extremely important influence on micro and macro productivity levels.

Reacting to Little and Triest's finding that plant size has a significant positive impact on technology adoption while firm size does not, Hatsopoulos indicates that these relationships again appear intuitively

plausible to him since plant scale must be considered in making technology decisions while access to capital, a firm-level characteristic, has only an indirect impact on local technology choices. Similarly, Hatsopoulos reports that he is not particularly puzzled by the result that access to a work force with a high school education has a greater impact on the probability of technology adoption than does access to workers with a college education. Because he finds the importance of foremen and other middle managers to be of overriding importance in the technology decision, Hatsopoulos finds this result matches his expectations.

Macro Policy, Innovation, and Long-Term Growth: A Panel Discussion

Martin Baily begins by dissecting potential GDP growth, estimated to be 2.3 percent per year, into its major components: labor inputs, which have been rising about 1.1 percent annually; and labor productivity, which has shown trend growth of 1.1 percent per year since 1973. As Baily points out, while trend labor productivity has fallen from its 2.9 percent average in the 1960–73 period, the explicable part of productivity growth (the part due to capital intensity, education and experience, and R&D) has been remarkably constant at 1.1 percent since 1960. By contrast, the unexplained residual, the productivity "bonus" enjoyed between 1960 and 1973, has entirely disappeared; "We did not know where it came from, and now we do not know where it has gone." In a related puzzle, the growing gap between the annual earnings of college and of high school graduates is widely attributed to a rising demand for technically skilled workers, but we see no signs of major technological breakthroughs in the productivity numbers. More formally, we see evidence of technological bias in the increased return to education but no evidence of technological change in measured productivity growth.

Turning to policy prescriptions, Baily concludes that current growth rates reflect supply rather than demand constraints and, thus, that the potential role for monetary policy in spurring growth is limited. By contrast, fiscal policy is important: During the 1980s the federal budget deficit was a primary cause of our low rates of saving and investment, which in turn contributed to the deceleration in capital intensity and productivity growth. Thus, reducing the federal deficit remains an important policy goal. A second area for policy action relates to education and training. Although the contribution to productivity growth made by education and experience has risen recently, that increase merely reflects the growing experience of the aging baby boom generation, and the rising return to education suggests that the demand for highly skilled workers continues to outstrip supply. Because Alan Krueger's work (1993) shows that computer skills in particular are linked to higher wages, and presumably, thus, to higher productivity, federal seed money for computer literacy programs might prove especially helpful. Finally, since studies by Edwin Mansfield and others suggest that the social return to private R&D is substantial, Baily concludes that tax incentives for R&D could play a positive role. Moreover, since private R&D appears correlated with prior federal R&D spending, Baily is concerned about congressional proposals to curtail the rate of public nondefense research.

As Baily points out, we see evidence of technological bias in the increased return to education but no evidence of technological change in measured productivity growth.

Baily ends by speculating about the unexplained growth bonus enjoyed between 1960 and 1973. Much of that spurt in productivity growth may have resulted from a burst of innovation and a shift from craft to mass production that cannot be repeated. If so, we may simply have to adjust to a world with lower productivity growth and slower growth in average real wages—a world split into winners and losers. Such a world would require an enhanced safety net and a progressive tax system, Baily submits. On the other hand, because measuring output and productivity is extremely difficult, particularly in areas like health care, or in retailing and financial services where convenience is important, output and productivity growth may actually be better than we think. Accordingly, Baily advocates investing in, not starving, our statistical agencies in order to get better data and better policies. Finally, maintaining open economies and deregulating domestic markets provide important incentives to adopting better technologies.

Ralph Gomory addressed his remarks to the impact of economic development in technically backward countries on welfare in the industrialized nations, a topic of great concern to many policymakers. As underdeveloped countries improve their technical capabilities, they become significant contributors to world output, but they also become more effective competitors to established industries in developed nations. What is the net impact on the national welfare of the technically advanced nations? To analyze this issue, Gomory offers a classical Ricardian model of international trade in which the relative efficiencies determining comparative advantage are allowed to vary, as in Gomory and Baumol (1995).

Gomory sketches a two-country model—or rather a family of two-country models—that assumes single-input linear production functions, Cobb-Douglas utilities, and fixed labor supplies and demand parameters, as well as a fixed number of industries. In equilibrium, both countries actively participate in a given industry *only* if their unit labor costs in that industry are equal. The exercise then calculates, for all possible values of average labor productivity, the equilibrium outcome in terms of national utility and share of world income for each country.

Gomory considers the impact of economic development in technically backward countries on welfare in the industrialized nations, a topic of great concern to many policymakers.

The results suggest opportunities for inherent conflict between the two countries because, even though *world* output is greatest when both countries have similar productivities and split world production 50–50, the best outcome for each one *singly* occurs when it has a large share of world output and income; this point always represents a poor result for the other. As Gomory carefully points out, improvements in productivity in one country (which always increase that country's share of world income) sometimes enhance welfare in both countries; however, in other cases, unilateral improvements in productivity decrease the welfare of the other.

What conditions determine the outcome? Assuming, as in Gomory and Baumol (1995), that efficiency rises in an active industry and decays in a less active industry, the model suggests a natural tendency for national shares of world output to remain close to their original values, while incomes expand as a result of improving efficiencies. However, if one country (generally the lower-wage country) succeeds by policy measures in "capturing" a growing share of world output in a given industry, its welfare improves. Whether or not welfare improves in the second (advanced) country depends on whether the depressing effect of the capture is or is not outweighed by improved efficiencies (via learning-by-doing, for instance) in all other industries. This result contrasts with Ricardo's original insight that trade based on comparative advantage determined by a specific pair of production functions always enhances well-being in both countries.

Abel Mateus's experiences with the Banco de Portugal and the World Bank permit him to examine the impact of macro policies on growth from the perspective of developing as well as developed economies. He suggests that technological progress is a primary determinant of growth in developed countries, whereas in developing countries most growth is due to the accumulation of physical and human capital that incorporates ideas transferred from advanced nations; thus, in these developing countries, outward orientation is complementary to the capital accumulation process.

Mateus points out that in small open economies miraculous growth is linked with rapid accumulation of human capital and use of that knowledge to operate physical capital to produce goods near the country's technological frontier. Shifting labor and capital to ever more advanced activities allows learning by doing and augments the accumulation of human capital. Export orientation is essential to such a growth strategy because this approach creates a gap between the mix of goods consumed domestically and the mix of goods produced and by necessity exported to larger, more demanding foreign markets. By contrast, Eastern Europe provides counterexamples of countries where the technology gap is sufficiently huge that trade promotes so much Schumpeterian (creative?) destruction that short-term welfare actually declines. Nevertheless, Mateus argues that these "industrialized" transitional nations must pursue the painful path of institutional change, industrial restructuring, and integration into the world trading system. Moreover, most developing countries, with smaller initial

manufacturing sectors, do not face such conflicts; for them, the benefits of trade based on comparative advantage apply even in the short run. The policy implications stemming from Mateus's observations of small open economies include an emphasis on formal education, protection of property rights, and an export-oriented trade stance to promote competition and technology transfer.

Mateus then addresses the impact of free trade in goods and technologies on the developed countries, where these developments have been associated in the 1980s and 1990s with high unemployment rates and stagnant or declining real wages for unskilled workers. After a reminder that present levels of global integration are not unprecedented, he points to the drop in transportation costs and the increase in communication speeds as the truly new elements. Although he sees some evidence supporting Paul Krugman's (1981) hypothesis that these developments will improve the lot of peripheral regions at the expense of the core and Jagdish Bhagwati's finding that comparative advantage has become "kaleidoscopic," moving almost at random across developed countries, he finally concurs with Obstfeld (1994) that financial integration, with investment shifting from lower-return to higher-return projects, can yield substantial welfare gains throughout the world via its effect on output and consumption growth.

Mateus suggests that technological progress is a primary determinant of growth in developed countries, whereas in developing countries most growth is due to the accumulation of physical and human capital that incorporates ideas transferred from advanced nations.

Because the profitability of innovation and diffusion depends in part on the macro environment, Mateus then turns to fiscal policy and suggests that a high and rising debt ratio is likely to lower the long-term rate of growth. He cites World Bank findings that a 1 percentage point increase in the government surplus as a percent of GDP raises per capita growth by 0.37 percent and the investment ratio by 0.24 percent. Other research suggests that debt ratios and budget deficits are positively associated with increases in long-term risk premia. Mateus concludes, thus, that the near doubling in gross public debt as a share of GDP between the 1970s and the mid 1990s has had a significant negative impact on European growth rates. Accordingly, Mateus recommends wider use of consumption-based tax systems and a significant cut in the size of the public sector, to be accomplished, in part, through better project and activity evaluations. In addition, Mateus warns, social security systems in most countries are unsustainable.

Mateus ends by reprising his major policy recommendations. First, the emphasis on economic stability, trade liberalization, market-oriented policies, and human capital accumulation long advocated by international organizations appears to be appropriate. Second, the potential for improving world welfare by technology diffusion and portfolio diversification is enormous. Finally, within the developed world, blaming globalization and "social dumping" for current labor market problems is misguided. In Europe, reducing high rates of unemployment requires improving labor market flexibility, while in North America dealing with the plight of unskilled workers awaits more adequate redistributive policies.

Robert Solow expressed relief that the panel was discussing whether macro (not monetary) policy can promote long-term growth; as phrased, the question implies that fiscal policy is available for the task—luckily, since monetary policy cannot possibly address the many goals often assigned to it. Solow then begins his policy recommendations by urging advocates—academics as well as politicians—to stop making inflated claims for their favorite policy tools. The flat tax, a cut in the capital gains tax, and various labor market reforms may or may not be good ideas, but their impact on growth is likely to range from negligible to small—at most. In particular, Solow chides, too many theorists have taken to fabricating powerful policy options by leaping from empirically established links between levels to assumed links between levels and growth rates. For example, while most would agree that the level of human capital affects the level of output, too many go on to assume that a high *level* of schooling will increase the *growth* of human capital, or that a high *level* of R&D will speed the *pace* of innovation. With these assumptions, tax policy can readily be shown to affect the permanent rate of

economic growth since it is quite easy to design incentives for schooling or R&D. "But do we really know that an increase in schooling or R&D will generate more than a one-time shift in the level of output?" the self-described spoilsport asks.

Solow notes that in general growth is efficiently served by mixing relatively tight fiscal policy, to promote national saving, with relatively easy monetary policy, to spur domestic investment.

This plea for circumspection limits the list of growth-promoting policies severely, Solow admits. Still, he considers certain commonplaces worth repeating. Given how little we know about the links between stocks and growth rates, any policy that raises potential output permanently should be described as contributing to growth—even if the long-term rate of growth remains unchanged. Just shifting the steady-state growth path upward, parallel to itself, is a major feat, he contends.

After warning that the trade-offs between growth and current living standards must be weighed, he emphasizes that anyone choosing growth must favor investment over consumption. Since a pro-saving policy need not be pro-investment (because additional saving may reduce a current account deficit rather than raise investment, say), Solow proposes combining improved incentives to save with policies that shift the composition of demand in favor of investment. Any fiscal stance, he reminds us, can be weighted in favor of investment, with tax-and-subsidy policy an obvious instrument. Similarly, while a given macro posture can be achieved with many combinations of monetary and fiscal ease and tightness, in general growth is efficiently served by mixing relatively tight fiscal policy, to promote national saving, with relatively easy monetary policy, to spur domestic investment.

Solow also endorses a macro strategy that guides total demand toward potential whenever a gap between the two appears—for many reasons, but not least because this policy is growth promoting. He notes in this context that actual demand tends to fall below potential somewhat more often than it exceeds it, and that prospects for weak and fluctuating demand discourage investment. While the impact of modest overheating (particularly on investment volumes) is less clear, he cites consensus views that price stability encourages the most productive allocation of capital. Solow ends by asking, tentatively, if the Fed could usefully conduct open market operations at all maturities, not just at the short end, in order to affect long rates which, presumably, are the most relevant for investment decisions.

Moderator **Richard Cooper** initiated the general discussion by remarking that over the last 50 years, the process of innovation has, for the first time, become institutionalized and by asking the panelists and conferees to consider the price of future growth in terms of current income. Would it have been moral to ask our grandparents to save more in order that we could be even better off, compared with them, than we already are? In response, Robert Solow replied that he would be less concerned about growth if we were better at income redistribution, but, since we find redistribution hard, increasing today's growth is one way to help today's poor children and today's poor countries. Baily and Mateus added that public and private myopia about looming retirement needs require current policy action to spur saving. Other comments addressed the differential impact of environmental spending on measured productivity and the quality of life, and the need to explore the impact of the transitional costs of technological change on the growth process.

Cooper asked the conferees to consider the price of future growth in terms of current income. Would it have been moral to ask our grandparents to save more in order that we could be even better off, compared with them, than we already are?

Conclusion

After two decades of research focusing on the source and stabilization of short-run economic fluctuations, the profession has recently returned to considering the determinants of long-run growth. This resurgence in interest arises for several reasons. Many developed economies have seen their average growth rate halved since the mid 1970s, and as yet we have no compelling explanation. Differences across countries in standards of living and in growth rates are large and not obviously shrinking, even as modern technology has disseminated more widely and educational standards have risen. The welfare implications of these cross-time and cross-country income differences dwarf those that arise from business-cycle fluctuations.

One fruitful vein of research has striven to understand growth from within the neoclassical framework, attributing continued increases in income primarily to investments in physical and human capital. Dale Jorgenson's research constitutes probably the most carefully measured and estimated set of econometric models in the neoclassical tradition. His conclusion is that investment can account for the preponderance of growth. This assessment is important, as it provides a benchmark for the contribution of standard inputs to growth. And yet, as Susanto Basu points out, the neoclassical model ultimately cannot plausibly explain all of the differences in growth that we observe over time and across countries. It seems extremely unlikely that we could have achieved most of our high standard of living today simply by using more and more of the investment goods that were prevalent in the nineteenth century. We could not have arrived at our sophisticated communications-linked, information-processed, efficiently manufactured state simply by using more and more shovels, adding machines, and steam engines. And yet the available data do not reveal a clear relationship between the invention, development, or adoption of new technology and subsequent improvements in productivity or income. Where does this observation leave us? Participants in this conference generally agreed on a few tentative conclusions.

First, it may be helpful to understand the input to production that is neither human nor physical capital not simply as "technology," but as an aggregate of the state of technology, organizational and managerial ability, and "economic culture." These concepts are not easily measured, but given the inability of relatively well-measured constructs to explain the variation in productivity in disaggregated data, we must try to model and measure these intangibles better if we are to understand significant differences in growth and productivity over time and across countries.

Second, most conference participants agree that it is probably beyond our grasp to design policies that we can be confident will spur specific innovations, or even spur innovation generally. The difficulty arises largely from the tremendous amount of uncertainty that surrounds the process of innovation. Given the difficulty in knowing which innovations will succeed, when they will arise, and what complementary innovations they will require to become "useful," policymakers do not possess the foresight to tailor policies to foster specific innovations. Still, most participants agreed that the social returns to innovation exceed the private returns. Although the extent to which private returns spill over into non-appropriable social returns is not clear, most would say such spillovers are likely to be sizable. Thus, the government should play a limited role in promoting R&D.

Finally, two clear insights from our panelists merit special attention as pointers to future research. The first, highlighted by Richard Cooper, is that we assume, as a matter of default, that a higher long-run growth rate is better. In doing so, we are implicitly choosing the multiple by which our descendants' welfare will exceed our own. At a 1 percent rate of annual productivity growth, our grandchildren will on average have 65 percent higher real incomes than we do; at a 2 percent rate they will have nearly triple our real incomes. But in order to attain these increases for our descendants, we must forgo some current consumption. Cooper poses the question: How much better off should our grandchildren be than we are, and at what cost? Robert Solow points out, in response to Cooper's question, that productivity-generated increases in the size of the economic pie may benefit the poor children of today *and* tomorrow. This question lies at the root of the discussions about productivity slowdowns and hoped-for improvements.

The second insight, articulated by Robert Solow, is a reminder that not all improvements in welfare must be measured as changes in the growth rate of the economy. One-time permanent improvements in the *level* of potential output are also valuable and probably much more attainable. The profession may do well to focus more of its attention on policies that could more reliably achieve these less spectacular improvements.

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Technology and Growth

The Federal Reserve Bank of Boston devoted its fortieth economic conference, held in June 1996, to a critical but imperfectly understood aspect of the growth process: the link between technological progress and economic growth. Economists, businessmen, and policymakers met to consider the pace of economic and productivity growth, trends in income inequality, the widening income gap between developed and developing countries, and policy options related to innovation and growth. The conference agenda is outlined below.

Keynote Address: The Networked Bank
Robert M. Howe, International Business Machines Corporation

Technology in Growth Theory
Dale W. Jorgenson, Harvard University
Discussants: Susanto Basu, University of Michigan
Gene M. Grossman, Princeton University

Uncertainty and Technological Change
Nathan Rosenberg, Stanford University
Discussants: Joel Mokyr, Northwestern University
Luc L. G. Soete, University of Limburg

Cross-Country Variations in National Economic Growth Rates: The Role of "Technology"
J. Bradford De Long, University of California at Berkeley
Discussants: Jeffrey A. Frankel, University of California at Berkeley
Adam B. Jaffe, Brandeis University

Luncheon Address: Job Insecurity and Technology
Alan Greenspan, Board of Governors of the Federal Reserve System

Microeconomic Policy and Technological Change
Edwin Mansfield, University of Pennsylvania
Discussants: Samuel S. Kortum, Boston University
Joshua Lerner, Harvard University

Technology in U.S. Manufacturing: The Geographic Dimension
Jane Sneddon Little and Robert K. Triest, Federal Reserve Bank of Boston
Discussants: John C. Haltiwanger, University of Maryland
George N. Hatsopoulos, Thermo Electron Corporation

Macro Policy, Innovation, and Long-Term Growth: A Panel Discussion
Moderator: Richard N. Cooper, National Intelligence Council and Harvard University
Panelists: Martin Neil Baily, Council of Economic Advisers
Ralph E. Gomory, Alfred P. Sloan Foundation
Abel M. Mateus, Banco de Portugal
Robert M. Solow, Massachusetts Institute of Technology

The proceedings, Conference Series No. 40, will be published at the end of this year. Information about ordering this volume will be included in a later issue of this *Review*.

Unilateral International Transfers: Unrequited and Generally Unheeded

Norman S. Fieleke

Vice President and Economist, Federal Reserve Bank of Boston. Wei Sun provided valuable research assistance.

Among the major categories of international transactions, some, such as trade in goods and services, often garner headlines in the financial press. Others receive less attention. Perhaps none is usually farther from the limelight than unilateral, a.k.a. unrequited, transfers. To be sure, extraordinarily large or controversial transfers, such as those generated by the 1990–91 Persian Gulf conflict, do take their place on center stage, but these are exceptions.

This obscurity is somewhat puzzling, because countries' net receipts or payments of unrequited transfers often exceed their international balances on both trade and current account and sometimes amount to sizable fractions of their national incomes. Moreover, in many countries the bulk of unrequited transfers, unlike most other international transactions, is paid or received by governments, which could directly reduce or enlarge those transfers as part of any effort to correct troublesome overall imbalances in international payments. Finally, the "transfer problem"—maintaining equilibrium in international payments in the face of sizable transfers—remains a challenging issue, both for policymakers and for analysts.

This article discusses the singular nature of unrequited transfers, recalls an historic, and still relevant, controversy over their economic impact, and recounts an effort by the United States to neutralize their balance-of-payments consequences. The size of these transfers in recent years, and some plausible explanations for them, are then evaluated, with most attention given to those of the United States.

I. The Nature of Unrequited Transfers

Unrequited transfers are unique among international transactions in that, by definition, they entail no quid pro quo. Generally they are gifts,

as their name implies. Examples are grants of cash or food from one country to another.

By contrast, the great preponderance of international transactions do entail a quid pro quo, and because they do, they conform well to the double-entry bookkeeping system used in balance-of-payments accounting. For example, the quid pro quo for a country's commercial export of items priced at, say, $25 million, might be a promise from the foreign recipient to make payment within 90 days. In that case, the country's trade account would be credited for $25 million, while its capital account would be debited for $25 million to reflect the increase in the country's outstanding claims on foreign residents. Thus, the equality of total debits and total credits required by the double-entry system would be preserved.[1]

The "transfer problem"—maintaining equilibrium in international payments in the face of sizable unilateral transfers—remains a challenging issue, both for policymakers and for analysts.

Unrequited transfers pose a problem for this bookkeeping system, a problem that well illustrates their nature. If the exports discussed in the preceding paragraph were a gift instead of a sale, the donor country's trade account would still be credited for $25 million, but no debit could be recorded to reflect a quid pro quo of material value from the recipient country. In order to balance the books in such cases, the convention followed has been to debit an account called unrequited (or unilateral) transfers. (For some transfers, such as some grants of foreign aid, this debit might represent not merely a balancing item but, at least in part, the acquisition of good will implying some future reciprocal action by the foreign recipient.) The accounting treatment in the recipient country would be symmetrical: a debit to the trade account to reflect the imports, and a credit to unilateral transfers to recognize the complimentary nature of the imports.

An unrequited transfer may, of course, be made or received by the private or the public sector. Among those between private parties, the major categories include transfers by migrants of their possessions from their former to their new residences, remittances by migrant workers from their new residences to relatives or others in the countries the migrants have departed, and gifts, contributions, and pension payments between individuals and private organizations residing in different countries. Among the transfers in which official organizations participate, either with each other or with private parties, the major categories include financial grants or grants of material or technical assistance, cancellation of debt, and payment of pensions, taxes, and fees.

II. An Historic Controversy

Unrequited transfers in the form of foreign aid have often sparked controversy in the United States, but by far the most renowned intellectual debate on the subject focused not on foreign aid but on reparations. Following wars, the defeated have often been obligated to make reparations payments for damages or expenditures sustained by others because of the hostilities. Such payments were demanded of France after the Franco-Prussian War of 1870–71, of Germany after World War I, and, in recent times, of Iraq after the Persian Gulf conflict of 1990–91.

It was the reparations sought from Germany that provoked the famous debate, a debate that yielded a framework for analyzing transfers that remains applicable today. In the Versailles Treaty of 1919, Germany acknowledged war guilt and promised to compensate the victims of her aggression.[2] By 1921 a Reparation Commission had determined Germany's obligation to be about $31.5 billion, to be paid in yearly installments, with more than half the money going to France.

The Allies found it much easier to demand than to collect these payments, and in 1929 the eminent economist John Maynard Keynes argued that the reparations sought from Germany were too onerous. In a brief article entitled, "The German Transfer Problem," Keynes maintained that the true burden on Germany was greater than the nominal reparations payments. He noted that Germany had to make the payments not in its own currency but in foreign currency and that it had, therefore, to generate a surplus of exports over imports sufficient to earn the

[1] For an exposition of balance-of-payments accounting, see Fieleke (1996).

[2] The historical detail in this section is taken chiefly from Yeager (1976), especially pp. 313–17.

required foreign currency. Germany could do so, he believed, only by substantially lowering the real wages of its work force, and thus the cost and price of its exports, so as to induce enough added foreign purchases.

The required reduction in export prices would have to be substantial, Keynes argued, partly because purchasers in other nations were not very responsive to price reductions—their demand was not very price-elastic. In addition, competing producers in other nations would seek higher trade barriers in order to resist increased German penetration of their markets. As a consequence, the necessary decrease in German wages would be so great that German workers were unlikely to accept it until millions had been thrown out of work.

In a rebuttal to Keynes, Bertil Ohlin, another distinguished economist, maintained that Keynes had underestimated the favorable impact on Germany's trade balance of the reparations payments themselves (Ohlin 1929). The payments would reduce German buying power and German spending, while raising buying power and spending in the recipient countries. According to Ohlin, these changes in spending would prompt several adjustments, which would operate more powerfully than Keynes had recognized to improve Germany's trade balance without reductions in German export prices.

Specifically, some of the reduction in German spending resulting from the reparations payments would fall on imports, as Keynes had acknowledged; some would also fall on German merchandise that, going unpurchased by Germans, could then be exported, a point that Keynes gave little emphasis; and some would fall on German goods that were not internationally traded, allowing resources to shift from producing such goods to producing goods that could be exported or substituted for imports, a phenomenon that Keynes seemed to overlook. The reverse of this process would occur in the recipient countries. Some of the increased spending there would go for imports from Germany, again as Keynes had noted; but some would also go for goods that would otherwise have been exported to Germany; and some would go for nontraded goods, drawing resources into their production and away from the production of goods that could be exported to Germany or substituted for imports from that country. Because of all these spending adjustments, Germany's trade balance might improve substantially without the need for much decline in German export prices relative to the prices of goods that Germany imported.

Ohlin also argued that this process of adjustment had not yet been put to the test, because Germany had been borrowing from abroad twice as much as it had paid in reparations, thereby augmenting rather than reducing its buying power. He did, however, agree with Keynes that Germany would find it more difficult to generate the required export surplus if other countries raised their barriers against German goods.

Among other things, the debate served to highlight the difficulty of determining the impact of unrequited transfers—and of capital movements as well—on the relative prices of internationally traded goods. Theorizing alone, however elegant, will not yield a definitive answer. It provides only a launching pad for tough case-by-case empirical research.

As for the case of German reparations, the process of adjustment, which Ohlin declared untested as of 1929, was to remain largely untested in the years to follow. The Allies lowered their demands, and German payments essentially ended in the early 1930s. According to the Reparation Commission, Germany may have paid a total of only about $5 billion, while receiving foreign loans and transfers of $8 billion or $9 billion.

III. Easing the Adjustments Required by Unrequited Transfers

Concern over the adjustments that unrequited transfers might require has sometimes prompted policies tailored to ease the process. One noteworthy case is that of the United States during the 1960s, a period when the then-fixed exchange rate of the dollar was coming under increasing downward pressure because of large and persistent deficits, as variously measured, in the nation's overall balance of payments. To avoid devaluing the dollar, the government undertook, among other actions, to mitigate any downward pressure on the exchange rate arising from governmental foreign aid and related spending abroad.

One measure was the "tying" of foreign aid, that is, requiring that monetary aid provided directly to other countries be spent in some sense on U.S., rather than foreign, goods and services. (A U.S. good was defined as one in which the "foreign components" did not exceed 10 percent of the price of the good.) So thoroughgoing was this policy that the Treasury Department could report, in 1968, that "The only significant elements in the A.I.D. program not specifically tied to U.S. goods and services are salaries and payments to A.I.D. overseas personnel and contractors

(only part of which is spent abroad) and limited offshore procurement for A.I.D. administrative purposes" (U.S. Department of the Treasury 1968, Tab C, p. 1).

Despite this and other measures, the balance-of-payments deficits persisted; and government outlays abroad, including unrequited transfers, continued to arouse concern. For example, in January 1969, Frank E. Morris, then President of the Federal Reserve Bank of Boston, argued that "... the U.S. economy, as such, has not been a deficit economy in its international accounts, it is simply that the private U.S. economy has not been able to generate sufficiently large surpluses since 1949 to finance the foreign exchange costs of the enormous military and aid programs of the United States Government around the world" (Morris 1969, p. 42). Unlike in the years immediately following World War II, foreign producers, Morris reasoned, had become capable of supplying the great bulk of foreign demand; no longer was it true that "any dollar cast adrift in Europe or Asia will come home in the form of a demand for U.S. goods" (p. 46). That, of course, was one way of rephrasing the traditional "transfer problem" addressed by Keynes and Ohlin.

However one analyzes the source of the U.S. balance-of-payments deficits in those years, the policy measures taken proved inadequate to defend the dollar, and on August 15, 1971, the Nixon Administration formally suspended its willingness to convert foreign official dollar balances into gold or other reserve assets. Such outcomes help to demonstrate that manipulations of particular balance-of-payments transactions, such as the tying of foreign aid, seldom succeed in resolving overall balance-of-payments disequilibria. Much more efficacious are comprehensive macroeconomic policy measures, perhaps including exchange-rate changes, that reduce a country's overall absorption of goods and services, including imports, relative to the country's output.

Nonetheless, "tying" policies persist. And as one measure of their efficacy, official presentations of U.S. balance-of-payments statistics continue to divide "U.S. Government grants and transactions increasing Government assets" into "Estimated transactions involving no direct dollar outflow from the United States" ($9.9 billion in 1995), on the one hand, and "Estimated dollar payments to foreign countries and international financial institutions" ($5.6 billion in 1995), on the other hand.[3]

Table 1
Leading Recipients of Net Unrequited Transfers, 1959 to 1993

Period and Country	Annual Averages in Millions of Dollars (Ranks in Parentheses)					
	Total		Private		Official	
1989–93:						
Israel	6,262	(1)	2,367	(7)	3,896	(1)
Portugal	6,113	(2)	4,293	(2)	1,820	(5)
Egypt	5,818	(3)	4,680	(1)	1,139	(6)
Greece	5,572	(4)	2,036	(8)	3,536	(2)
Spain	5,060	(5)	2,548	(5)	2,512	(4)
1984–88:						
Israel	4,686	(1)	1,152	(11)	3,534	(1)
Egypt	4,155	(2)	3,417	(1)	738	(5)
Portugal	3,085	(3)	2,782	(2)	303	(9)
Pakistan	3,023	(4)	2,574	(3)	449	(8)
Greece	2,470	(5)	1,154	(10)	1,315	(2)
1979–83:						
Egypt	2,855	(1)	2,692	(1)	163	(11)
Israel	2,848	(2)	1,034	(8)	1,815	(1)
Pakistan	2,654	(3)	2,380	(3)	274	(7)
Portugal	2,645	(4)	2,634	(2)	11	(62)
Syrian Arab Republic	2,121	(5)	582	(13)	1,539	(2)
1974–78:						
Israel	2,110	(1)	781	(6)	1,330	(1)
Spain	1,300	(2)	1,314	(1)	−13	n.a.
Turkey	1,251	(3)	1,224	(2)	26	(33)
Portugal	1,177	(4)	1,183	(3)	−6	n.a.
Egypt	884	(5)	884	(4)	0	n.a.
1969–73:						
Israel	1,028	(1)	635	(2)	393	(1)
Spain	847	(2)	855	(1)	−8	n.a.
Turkey	625	(3)	597	(3)	28	(22)
Greece	480	(4)	478	(5)	2	(50)
Italy	242	(5)	536	(4)	−294	n.a.
1964–68:						
Spain	401	(1)	398	(2)	2	(38.5)
Israel	384	(2)	277	(3)	107	(2)
Italy	319	(3)	486	(1)	−168	n.a.
Greece	226	(4)	219	(4)	7	(26.5)
Korea	213	(5)	83	(5)	130	(1)
1959–63:						
Israel	324	(1)	193	(2)	132	(2)
Italy	287	(2)	339	(1)	−52	n.a.
Korea	250	(3)	30	(10)	220	(1)
Spain	156	(4)	132	(4)	24	(9)
Phillippines	107	(5)	76	(5)	31	(8)

n.a.: Not applicable.

Note: A minus sign indicates a net payment rather than a receipt. Some countries had to be omitted from the analysis for lack of data, and no country for which 1993 data were lacking was included. For each 5-year period, differing numbers of countries were identified as recipients and included in the ranking, ranging from 37 in 1959–63 to 91 in 1989–93.

Source: IMF data base on DRI/McGraw-Hill.

[3] See *Survey of Current Business*, vol. 76 (July 1996), p. 86.

IV. The Magnitude of Recent Transfers

Despite the balance-of-payments problems that unrequited transfers may present, they occur in significant volume between many countries. For example, they have often exceeded the aggregate current-account deficit or surplus of the industrial countries as a group. In 1991, 1992, and 1993, the aggregate balance of the industrial countries in current-account transactions with the rest of the world amounted to −$31 billion, −$40 billion, and $19 billion, respectively, while their corresponding net unrequited transfers amounted to −$48 billion, −$84 billion, and −$79 billion.[4]

Which countries are the major beneficiaries of these transfers, and which are the major donors?[5] As reported in Table 1, Israel has ranked first or second among the recipients ever since 1959. Egypt and Portugal have also been in the front ranks since the late 1970s.

Even greater consistency prevails in the ranking of the leading donors. Germany, the United States, Saudi Arabia, France, and the United Kingdom have composed the top five donor countries since the mid 1970s, and Germany and the United States have alternated in first and second places ever since 1959 (Table 2). However, the differences between the amounts contributed by the leading donors are much greater than the differences separating the leading recipients.

By no means do all unrequited transfers take the form of governmental

Table 2
Leading Donors of Net Unrequited Transfers, 1959 to 1993

Period and Country	Annual Averages in Millions of Dollars (Ranks in Parentheses)					
	Total		Private		Official	
1989–93:						
Germany[a]	−29,358	(1)	−7,368	(3)	−21,990	(1)
United States	−23,442	(2)	−13,312	(1)	−10,130	(2)
Saudi Arabia	−15,634	(3)	−12,528	(2)	−3,106	(8)
France	−8,925	(4)	−2,649	(4)	−6,276	(4)
United Kingdom	−7,123	(5)	−489	(10)	−6,634	(3)
1984–88:						
United States	−23,148	(1)	−10,388	(1)	−12,760	(1)
Germany	−13,464	(2)	−4,964	(3)	−8,500	(2)
Saudi Arabia	−8,476	(3)	−5,346	(2)	−3,129	(4)
France	−4,440	(4)	−1,760	(4)	−2,680	(5)
United Kingdom	−4,280	(5)	65	n.a.	−4,345	(3)
1979–83:						
United States	−12,174	(1)	−4,848	(2)	−7,326	(1)
Germany	−11,128	(2)	−5,034	(1)	−6,094	(2)
Saudi Arabia	−9,458	(3)	−4,758	(3)	−4,700	(3)
France	−4,172	(4)	−2,144	(4)	−2,028	(5)
United Kingdom	−3,556	(5)	−36	(27)	−3,520	(4)
1974–78:						
Germany	−7,352	(1)	−3,942	(1)	−3,410	(2)
United States	−5,640	(2)	−904	(4)	−4,736	(1)
Saudi Arabia	−4,335	(3)	−1,282	(3)	−3,053	(3)
France	−2,690	(4)	−1,463	(2)	−1,228	(5)
United Kingdom	−1,777	(5)	−188	(10)	−1,588	(4)
1969–73:						
United States	−3,720	(1)	−1,102	(2)	−2,618	(1)
Germany	−3,358	(2)	−2,080	(1)	−1,278	(2)
France	−1,315	(3)	−848	(3)	−466	(4)
United Kingdom	−636	(4)	−95	(6)	−540	(3)
Switzerland	−439	(5)	−380	(4)	−59	(11)
1964–68:						
United States	−3,052	(1)	−732	(2)	−2,320	(1)
Germany	−1,582	(2)	−734	(1)	−848	(2)
United Kingdom	−742	(3)	−80	(4)	−661	(3)
Switzerland	−262	(4)	−246	(3)	−16	(10)
Japan	−128	(5)	−6	(14)	−122	(5)
1959–63:						
United States	−2,608	(1)	−498	(1)	−2,110	(1)
Germany	−1,064	(2)	−260	(2)	−804	(2)
United Kingdom	−308	(3)	−3	(11)	−305	(3)
Canada	−105	(4)	−121	(3)	17	n.a.
Venezuela	−83	(5)	−83	(4)	0	n.a.

n.a.: Not applicable.

Note: A minus sign indicates a net payment and a positive sign a receipt. Some countries had to be omitted from the analysis for lack of data, and no country for which 1993 data were lacking was included. For each 5-year period, differing numbers of countries were identified as donors and included in the ranking, ranging from 14 in 1959–63 to 46 in 1984–88.

[a]Four-year averages including 1989, 1991, 1992, and 1993 because appropriate data for 1990 are not available.

Source: IMF data base on DRI/McGraw-Hill.

[4] International Monetary Fund, *World Economic Outlook*, October 1994, p. 153. A caveat: It is well known that balance-of-payments statistics are imprecise. For example, for the 144 countries for which data are reported for 1987 by the IMF, the grand total of all net unrequited transfer payments is $75.3 billion, while the grand total of all net unrequited transfer receipts is only $53.2 billion.

[5] Countries paying unrequited transfers are donors in the sense that they receive nothing of material value in return at the time of payment. They may have received something of value in the past, as in the case of pensions they are paying to persons who previously resided and worked within their borders but who then resettled abroad.

Table 3
Leading 20 Recipients of Net Unrequited Transfers Relative to GNP in 1992–93

Country	Transfers as Percent of GNP (Ranks in Parentheses)					
	Total		Private		Official	
Lesotho	12.9	(1)	.1	(42.5)	12.8	(1)
Israel	8.8	(2)	3.6	(4)	5.2	(5.5)
Mali	7.4	(3.5)	1.7	(10)	5.7	(4)
El Salvador	7.4	(3.5)	6.1	(1)	1.3	(22)
Greece	7.4	(3.5)	2.7	(7)	4.6	(7)
Portugal	7.1	(6)	4.2	(3)	2.9	(12.5)
Burkina Faso	6.6	(7)	1.4	(12.5)	5.2	(5.5)
Ireland	6.4	(8)	−.1	n.a.	6.5	(2)
Jordan	6.3	(9)	4.5	(2)	1.9	(18)
Guinea-Bissau	6.1	(10)	−.1	n.a.	6.2	(3)
Tanzania	4.9	(11)	1.6	(11)	3.3	(10.5)
Ethiopia[a]	4.7	(12)	1.8	(9)	2.9	(12.5)
Jamaica	4.4	(13)	3.4	(5)	1.0	(25.5)
Burundi	4.1	(14)	.3	(31.5)	3.8	(9)
Chad	3.8	(15)	−.6	n.a.	4.5	(8)
Mauritania	3.4	(16.5)	.1	(42.5)	3.3	(10.5)
Egypt	3.4	(16.5)	2.8	(6)	.6	(30.5)
Benin	3.0	(18.5)	1.2	(17.5)	1.8	(19)
Nicaragua	3.0	(18.5)	.2	(37.5)	2.8	(14)
Morocco	2.9	(20)	2.6	(8)	.3	(38.5)

n.a.: Not applicable.

Note: A minus sign indicates a net payment rather than a receipt. Some countries had to be omitted from the analysis for lack of data. Underlying GNP data are purchasing power parity estimates.

[a]Data for Ethiopia include 1992 only.

Source: World Bank, *World Development Report 1995*, pp. 220–21; *World Development Report 1994*, pp. 162, 220, and 221; and IMF data base on DRI/McGraw-Hill.

foreign aid or other official transactions. To be sure, for most of the leading donors, official transfer payments have exceeded private, except for the period 1969–73. However, for most of the leading beneficiaries, private transfer receipts have exceeded receipts of official transfers.

For some countries, unrequited transfers have amounted to noteworthy fractions—as much as one-eighth—of their total income and output, as indicated in Tables 3 and 4. Although most of the leading recipients by this measure have relatively less developed economies, some are comparatively well-to-do. Specifically, Israel and Ireland are classified as "high-income" economies by the World Bank, and Greece and Portugal as "upper-middle-income." The major donors of transfers relative to GNP have enjoyed comparatively high incomes per capita.

Although the United States ranks low in net transfer payments relative to GNP, the nation has been consistent in its donor role. Its unrequited transfer payments have exceeded its receipts for decades, except in 1991 (Figure 1). In that year the country received cash contributions of $42.5 billion from its coalition partners in Operation Desert Storm, carried out during the conflict with Iraq following Iraq's invasion of Kuwait.

These "contributions" nicely illustrate the occasionally misleading nature of the term, "unrequited." It would be hard to make the case that the U.S. coalition partners received little or nothing of material value, not to mention political value, in return for their contributions; had they not valued the role played by the United States in subduing Iraq, they would hardly

Table 4
Donors of Net Unrequited Transfers, in 1992-93, Ranked by Size of Transfers Relative to GNP

Country	Transfers as Percent of GNP (Ranks in Parentheses)					
	Total		Private		Official	
Saudi Arabia[a]	−7.9	(1)	−7.1	(1)	−.8	(6.5)
Oman	−6.9	(2)	−6.9	(2)	[c]	n.a.
Kuwait[b]	−5.1	(3)	−3.9	(3)	−1.2	(4.5)
Germany[b]	−2.3	(4)	−.6	(9.5)	−1.7	(1)
Norway	−2.0	(5)	−.5	(13.5)	−1.5	(2)
Netherlands	−1.9	(6)	−.6	(9.5)	−1.3	(3)
Switzerland	−1.8	(7)	−1.5	(5)	−.4	(12.5)
Sweden	−1.4	(8)	−.2	(16.5)	−1.2	(4.5)
Singapore	−1.3	(9)	−.9	(8)	−.4	(12.5)
Côte d'Ivoire	−1.2	(10)	−2.3	(4)	1.1	n.a.
United Kingdom	−.9	(11.5)	[c]	(27)	−.8	(6.5)
Finland	−.9	(11.5)	−.2	(16.5)	−.6	(8.5)
Denmark	−.7	(13.5)	−.1	(22.5)	−.6	(8.5)
France	−.7	(13.5)	−.2	(16.5)	−.5	(10.5)
Congo	−.7	(13.5)	−1.2	(6)	.6	n.a.
Austria	−.6	(16)	−.5	(13.5)	−.1	(15.5)
Italy	−.5	(17.5)	[c]	(28)	−.5	(10.5)
United States	−.5	(17.5)	−.2	(16.5)	−.3	(14)
Japan	−.2	(19.5)	−.1	(22.5)	−.1	(15.5)
Venezuela	−.2	(19.5)	−.2	(16.5)	[c]	(22)
Trinidad and Tobago	−.1	(21)	−.1	(22.5)	[c]	(24)

n.a.: Not applicable.

Note: A minus sign indicates a net payment and a positive sign a receipt. Some countries had to be omitted from the analysis for lack of data. Underlying GNP data are purchasing power parity estimates.

[a]Data for Saudi Arabia include 1992 only.

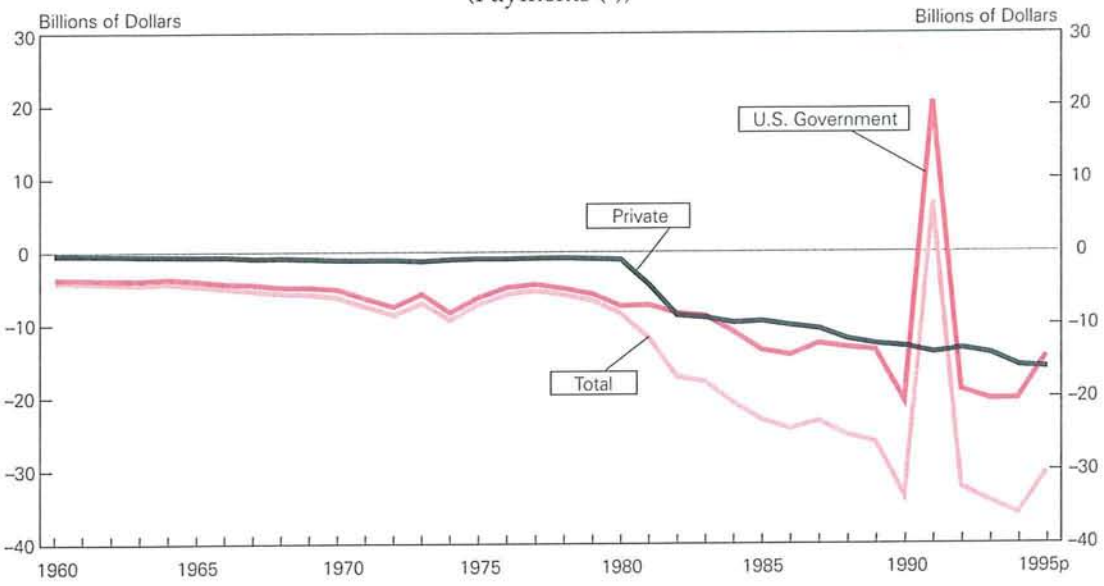
[b]Data for Kuwait and Germany Include 1993 only.

[c]Less than 0.05 in absolute value.

Source: World Bank, *World Development Report 1995*, pp. 220–21; *World Development Report 1994*, pp. 162, 220, and 221; and IMF data base on DRI/McGraw-Hill.

Figure 1

U.S. Net Unrequited Transfers, 1960-95
(Payments (-))



Note: Because of differences in classification, these unrequited transfers data, taken from *Survey of Current Business*, differ somewhat from those elsewhere in this article that were taken from the International Monetary Fund.
p: Preliminary.
Source: *Survey of Current Business*, vol. 76, no. 4, April 1996, p. 69; vol. 75, no. 6, June 1995, pp. 84-85; vol. 74, no. 6, June 1994, p. 94; and vol. 73, no. 6, June 1993, p. 70.

have contributed so substantially. However, because they received no goods, services, or financial assets in exchange for their contributions, those contributions, by balance-of-payments accounting rules, had to be classified as unrequited.

Although U.S. net transfer payments have increased substantially over the years, they rose less rapidly than GNP between 1960 and 1980, as indicated in Figure 2, then rose relative to GNP through the mid 1980s, and subsequently more or less stabilized at only about 0.5 percent of GNP. Their composition has also varied, as was illustrated in Figure 1. During the 1960s and 1970s, U.S. government net transfer payments consistently exceeded net private payments, as then recorded, by a wide margin. But during the early 1980s, private payments soared, as changes in reporting were instituted; net taxes paid by U.S. residents to foreign governments came to be included in the category, and estimates of personal remittances by the foreign-born population in the United States were improved, and thus increased.[6] Thereafter, net private payments have fairly consistently accounted for more than two-fifths of all U.S. net transfers, compared to only one-tenth in the early 1960s. The corresponding decline in the government share has occurred in grants to foreigners, which recently have comprised roughly two-fifths of all net transfer payments, only half the share of the early 1960s.[7] Of these grants, roughly one-third have been designated for military assistance in recent years.

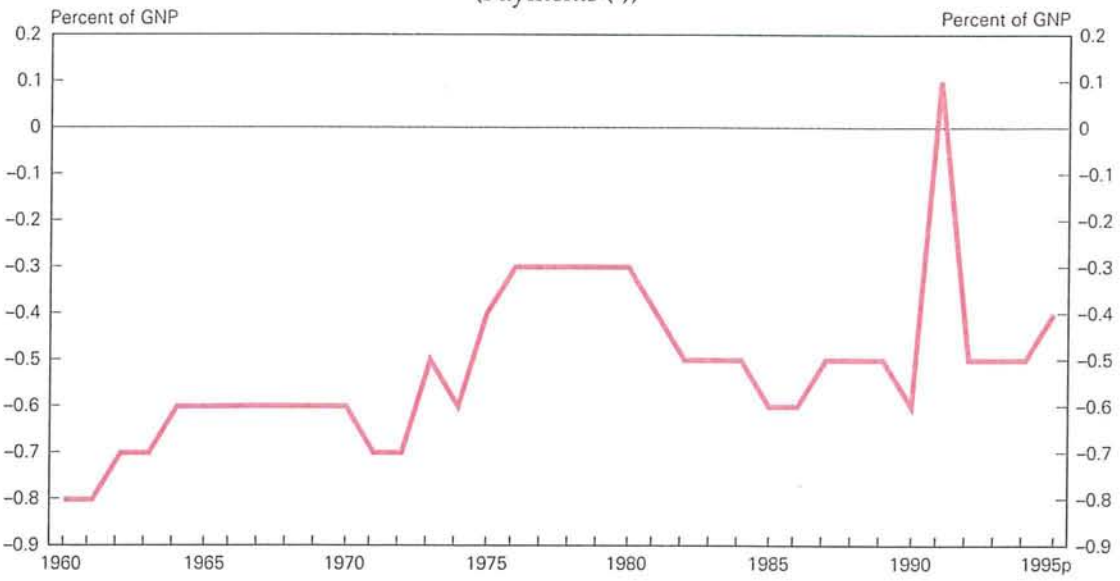
V. Some Explanations for Unrequited Transfers

To explain transactions, economists traditionally have appealed to somewhat selfish motivations. They have assumed, for example, that households spend on goods so as to maximize their welfare as consumers, and that firms channel their outlays so as to maximize their profits as producers. Unrequited transfers present severe difficulties for this mode of analy-

[6] See *Survey of Current Business*, vol. 72 (June 1992), pp. 68–69.
[7] For more detail on U.S. unrequited transfers, see section I in the appendix.

Figure 2

U.S. Net Unrequited Transfers as a Percent of GNP, 1960-95
(Payments (-))



Note: Because of differences in classification, these unrequited transfers data, taken from *Survey of Current Business*, differ somewhat from those elsewhere in this article that were taken from the International Monetary Fund.
p: Preliminary.
Source: *Survey of Current Business*, vol. 76, no. 4, April 1996, p. 69; vol. 75, no. 6, June 1995, pp. 84-85; vol. 74, no. 6, June 1994, p. 94; vol. 73, no. 6, June 1993, p. 70; IMF data base on DRI/McGraw-Hill; and Haver.

sis, for, by definition, a party making a transfer payment receives nothing of material value in return. To explain unrequited transfers, one must consider noneconomic motives, such as national defense—which, as Adam Smith observed, "is of much more importance than opulence"[8]—or even altruism. Such motives, especially national security, have inspired sizable unrequited transfers from the United States to Israel and Egypt, and largely account for the leading positions of those countries among the recipients of such transfers.

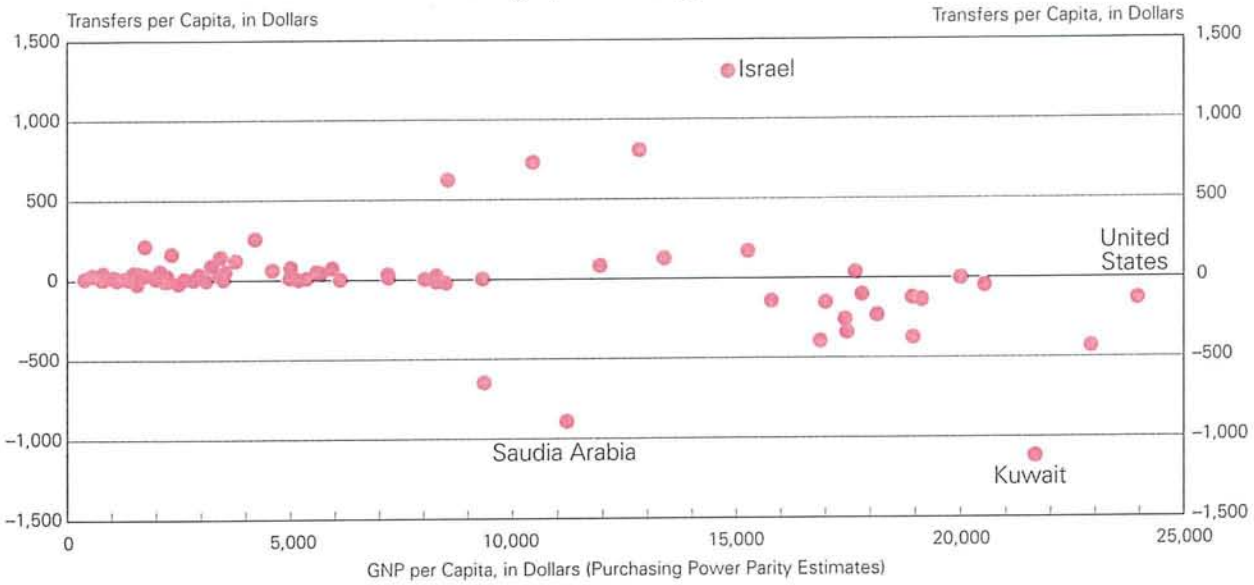
Even though noneconomic considerations may motivate some unrequited transfers, economics—or, more generally, political economy—may still yield some insights. For example, we might expect countries with high incomes per capita to be net payers of transfers, and those with low incomes per capita to be net recipients, other things equal. Indeed, some support for this hypothesis was found in our examination of the leading donors and recipients in Tables 3 and 4. As it turns out, however, a more comprehensive analysis—illustrated in Figure 3—suggests that this hypothesis explains very little of the variation in net transfer payments from country to country; the tendency for net transfer payments to increase with per capita incomes is extremely weak.[9]

For the United States, some further, albeit limited, analysis is possible. Two issues on which some data are available seem especially worthy of investigation. First, are the nation's net transfer payments to, or receipts from, another country generally larger, the larger the population born in that country but residing in the United States? Such an outcome would seem likely, since remittances, gifts, pensions, and the like between such populations and their former homelands probably tend to increase with the size of those populations. In addition, larger foreign-born populations may be more successful than smaller ones in influencing governmental policy on transfers. Second, are net U.S. transfer payments to another country typically larger, the greater the excess of U.S. per

[8] *The Wealth of Nations* [1776]. New York: The Modern Library, 1937, p. 431.
[9] The simple coefficient of correlation is −0.27 (where net payments carry a minus sign), significant at the 0.02 level.

Figure 3

Relationship between Net Unrequited Transfers per Capita and GNP per Capita, Annual Averages, 1992-93, for 82 Countries



Note: Data for Bulgaria, Ethiopia, Lao P.D. Rep., and Saudia Arabia include 1992 only; and data for Germany and Kuwait include 1993 only.
Source: World Bank, *World Development Report 1995*, Table 30, pp. 220-21; and *World Development Report 1994*, Table 30, pp. 162, 220, and 221; and IMF data base on DRI/McGraw-Hill.

capita income over per capita income in that country (and conversely for net U.S. transfer receipts)?

Standard statistical analysis yields results highly consistent with the first of these propositions, but not the second. The results, reported in section II in the Appendix, are only tentative, being based on very limited data (for the United States and only seven other countries) and, therefore, on a less than comprehensive analytic model. Nevertheless, the results merit attention, as the first proposition has strong statistical support, and the second—the finding that U.S. net transfer payments bear little relation to differences in per capita income—accords well with the pattern displayed in Figure 3.

VI. Summary and Conclusion

Unrequited transfers between nations usually receive relatively little attention, even though they often exceed trade and current-account balances and sometimes amount to sizable fractions of national incomes. Exceptional transfers, however, have attracted exceptional attention. Thus, between the world wars, extended negotiations, and intensive debate, took place over the amount of reparations that Germany should, and could, pay to the victorious allies. Again, as the dollar's exchange rate came under strong downward pressure during the 1960s, the U.S. government adopted intensive measures tailored to reduce such pressure from the government's unrequited transfer payments in the form of foreign aid and related spending abroad. Still more recently, in recognition of the resources it devoted to Operation Desert Storm, the United States obtained sizable unrequited transfer payments from its coalition partners in 1991.

Among the net recipients of such transfers, Israel has ranked at or near the top ever since 1959, with Egypt and Portugal also in the forefront since the late 1970s. Among the net payers, or donors, Germany, the United States, Saudi Arabia, France, and the United Kingdom have been in the vanguard since the mid 1970s. If countries are ranked instead by their net receipts or payments in relation to their national incomes, most, but by no means all, of the leading recipients have relatively less developed economies,

while the leading donors have comparatively high incomes per capita. Although the United States ranks low in net payments of unrequited transfers as a percent of national income, it has been, with the exception of 1991, a net payer for many years.

Evaluating the forces that motivate unrequited transfers is a daunting task, since the motivations may be primarily noneconomic, such as national defense or altruism, and highly imponderable. Thus, differences in per capita income between countries seem to account for little or none of the variation in net transfer payments from country to country. On the other hand, a substantial portion of that variation may perhaps be explained by the dimensions of the various foreign-born populations residing in the paying or receiving countries, if the case of the United States is typical.

Appendix

I. Appendix Table

U.S. Unrequited Transfers, Net, 1960 to 1995

(Payments (−); in millions of dollars unless otherwise indicated)

Year	Total	As Percent of GNP	U. S. Government			Private Remittances and Other Transfers
			Total	Grants	Pensions and Other Transfers	
1960	−4,062	−.8	−3,640	−3,367	−273	−423
1961	−4,127	−.8	−3,693	−3,320	−373	−434
1962	−4,277	−.7	−3,800	−3,453	−347	−477
1963	−4,392	−.7	−3,818	−3,479	−339	−575
1964	−4,240	−.6	−3,626	−3,227	−399	−614
1965	−4,583	−.6	−3,907	−3,444	−463	−677
1966	−4,955	−.6	−4,301	−3,802	−499	−655
1967	−5,294	−.6	−4,415	−3,844	−571	−879
1968	−5,629	−.6	−4,793	−4,256	−537	−836
1969	−5,735	−.6	−4,796	−4,259	−537	−939
1970	−6,156	−.6	−5,060	−4,449	−611	−1,096
1971	−7,402	−.7	−6,285	−5,589	−696	−1,117
1972	−8,544	−.7	−7,435	−6,665	−770	−1,109
1973	−6,913	−.5	−5,663	−4,748	−915	−1,250
1974	−9,249	−.6	−8,232	−7,293	−939	−1,017
1975	−7,075	−.4	−6,169	−5,101	−1,068	−906
1976	−5,686	−.3	−4,769	−3,519	−1,250	−917
1977	−5,226	−.3	−4,368	−2,990	−1,378	−859
1978	−5,788	−.3	−4,944	−3,412	−1,532	−844
1979	−6,593	−.3	−5,673	−4,015	−1,658	−920
1980	−8,349	−.3	−7,304	−5,486	−1,818	−1,044
1981	−11,702	−.4	−7,186	−5,145	−2,041	−4,516
1982	−17,075	−.5	−8,338	−6,087	−2,251	−8,738
1983	−17,718	−.5	−8,676	−6,469	−2,207	−9,043
1984	−20,598	−.5	−10,855	−8,696	−2,159	−9,742
1985	−22,954	−.6	−13,406	−11,268	−2,138	−9,549
1986	−24,189	−.6	−14,064	−11,867	−2,197	−10,126
1987	−23,107	−.5	−12,508	−10,287	−2,221	−10,599
1988	−25,023	−.5	−13,014	−10,513	−2,501	−12,009
1989	−26,016	−.5	−13,408	−10,892	−2,516	−12,698
1990	−33,393	−.6	−20,351	−17,417	−2,934	−13,042
1991	6,869	.1	20,733	24,194	−3,461	−13,864
1992	−32,184	−.5	−18,818	−15,083	−3,735	−13,330
1993	−34,084	−.5	−20,096	−16,311	−3,785	−13,988
1994	−35,761	−.5	−20,061	−15,814	−4,247	−15,700
1995p	−30,095	−.4	−14,141	−11,027	−3,114	−15,954

Note: Because of differences in classification, these unrequited transfers data, taken from *Survey of Current Business*, differ somewhat from those elsewhere in this article that were taken from the International Monetary Fund.

p: Preliminary.

Source: *Survey of Current Business*, vol. 76, no. 4, April 1996, p. 69; vol. 75, no. 6, June 1995, pp. 84–85; vol. 74, no. 6, June 1994, p. 94; and vol. 73, no. 6, June 1993, p. 70; IMF data base on DRI/McGraw-Hill; and Haver.

II. Following is the estimated regression equation discussed in the section entitled, "Some Explanations for Unrequited Transfers." T-statistics are in parentheses and, if starred, are significantly different from zero at the 0.01 level.

$$T_i = \underset{(0.78)}{307.90} + \underset{(0.41)}{0.02245}Y_i + \underset{(5.05)^*}{0.0008}F_i;\ \bar{R}^2 = 0.88; 7 \text{ observations;}$$

where

T_i = net unrequited transfers between the United States and country i, in millions of dollars;
Y_i = excess of U.S. GNP per capita over GNP per capita in country i, in dollars (purchasing power parity estimates);
F_i = number of persons born in country i living in the United States in 1990, with the same algebraic sign as T_i.

Data for T_i and Y_i are annual averages for the period 1988–93.

Sources of Data:

T_i: *Survey of Current Business*, various issues; and Statistical Office of the European Communities, EuroStat, *Geographical Breakdown of the Current Account*, EUR 12, 1984–93, pp. 139 and 181.
Y_i: OECD Statistics Directorate, *National Accounts, Main Aggregates 1960–1994*, 1996 edition, vol. I, p. 149.
F_i: U.S. Bureau of Census, *1990 Census of Population, The Foreign-Born Population in the United States*, 1990 CP-3-1, Table 1.

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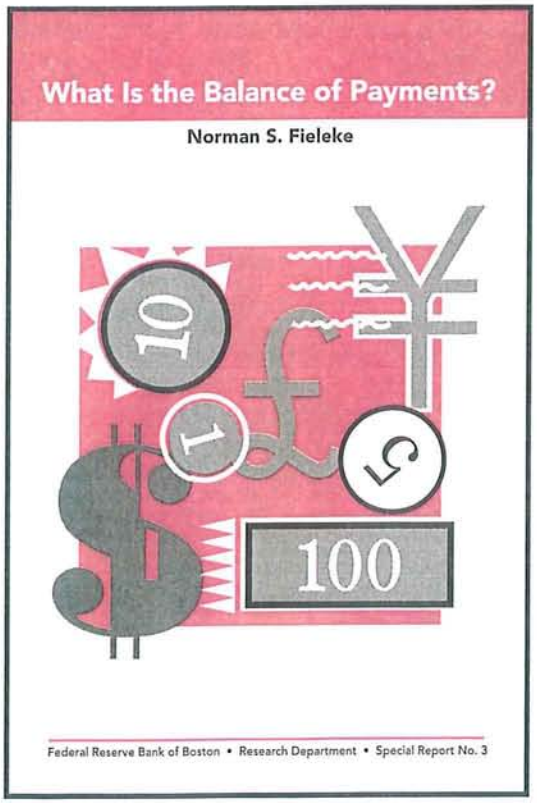
What Is the Balance of Payments?



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Regional Housing Supply and Credit Constraints

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Housing construction plays a critical role in the economy. Increases in housing starts raise construction employment, and recent home buyers often purchase other consumer durables, leading through the multiplier effect to increased employment. Construction is especially important for the business cycle, because changes in residential construction tend to lead recessions and recoveries. In addition, the elasticity of the supply of new housing is a determinant of movements in house prices, which affect both housing affordability and the wealth position of homeowners. Despite its importance, empirical research on housing supply is surprisingly rare. The dearth of work is particularly apparent when compared to the extensive literature on housing demand, a discrepancy noted in housing market overviews by Olsen (1987) and Smith, Rosen, and Fallis (1988), among others.

This article develops an empirical model of new housing supply based on the model of the conversion of raw land to urban use in Mayer and Somerville (1996). New single-family residences tend to be constructed at the fringe of the urban area, where raw land must first be converted into urban use as developed lots before construction can occur. Yet most empirical analyses of supply tend to ignore the relationship between land development and residential construction. Using this framework, the study shows that new housing starts are best modeled as a function of the change in the price of existing homes. In contrast, most previous work models starts as a function of the level of house prices.

This approach resolves several problems that can arise from treating housing starts as a function of the level of house prices alone. Urban theory suggests that house prices adjust to ensure an equilibrium between the stock of housing and the demand for it. Thus starts, a flow variable that equals the change in the stock of units (adjusted for depreciation and removals), should depend on changes in prices.[1]

We use this approach to examine regional differences in the supply of new housing. With the exception of Abraham and Shauman (1991),

studies of housing market dynamics tend to ignore regional differences. By comparing supply functions for the four U.S. Census regions, we uncover regional differences in the way builders respond to market signals. Following the literature, we estimate the price elasticity of starts, rather than a true supply elasticity, which would be the elasticity of the total stock of units. The results indicate notable differences in the supply elasticity of new construction across regions, from a low of 0.9 in the South to a high of 3.9 in the Northeast. One possible explanation for these differences in regional elasticities is the variation in the location of employment within urban areas across the regions. Finally, the response of starts to a given change in house prices is quicker in the South and Midwest than in the West and Northeast.

Housing construction plays a critical role in the economy. Construction is especially important for the business cycle, because changes in residential construction tend to lead recessions and recoveries.

A well-specified model that separately identifies supply and demand permits a search for other factors that might affect regional construction. We use this methodology to investigate credit availability. Various commentators have suggested that excessive bank regulation during regional downturns has caused banks to restrict lending below market equilibrium levels—a condition known as a "credit crunch." (See Browne and Rosengren 1992.) Because of the difficulty in measuring credit availability, researchers disagree as to whether credit restrictions are present and, if they exist, whether they have significant effects on aggregate output (Hubbard 1995). New construction should be highly susceptible to credit restrictions because the typical residential builder is dependent on banks for funds to finance land acquisition, up-front infrastructure expenses, and construction. We look for evidence of a credit crunch by examining the residuals from a supply equation during various periods that commentators have identified as potentially credit constrained. Credit constraints are consistent with prolonged periods of lower-than-predicted construction. The results show evidence of lower-than-predicted construction in the most recent downturn in the Northeast, but no evidence of reduced construction during other regional recessions, including the oil patch bust in 1986 and the general recession in 1982.

I. Theoretical Issues in Modeling New Construction

The standard empirical model characterizes new construction as a function of the level of house prices—which is expected to fully capture demand for new units—and "exogenous" cost shifters, such as interest rates and labor and materials prices. This specification ignores the implications of spatial conditions such as current or future growth in city size. Furthermore, because house prices equilibrate the total stock of housing with the total demand for residential space, they may not be an accurate measure of the demand for new construction, which is a flow variable, approximating the change in total stock, less removals.

Mayer and Somerville (1996) propose an alternative model of new residential construction based on the land development process. By incorporating spatial issues, the authors present a treatment of housing supply well-suited for comparisons of new construction across regions. The result of this approach is a model that treats housing starts as a function of changes in housing prices instead of the current level of house prices.

A simple example demonstrates why the level of housing prices can be an inappropriate measure of demand for new construction. Imagine a city composed of a stable number of homogeneous households. The city is not growing, so the housing market is at its long-run equilibrium, with house prices constant. As long as the physical condition of existing units does not depreciate, housing starts are equal to zero. Suppose an unexpected, one-time influx of population takes place. When this influx occurs, demand for new residences increases, land and house prices rise, new construction is undertaken, and the city grows in size to accommodate the new residents. At

[1] Also, previous research (for example, Clayton 1994, Meese and Wallace 1994, and Rosenthal 1995) shows that the real price of existing housing may not be stationary in levels, possibly because of increases in population or real income over time, but it is stationary in differences—that is, house prices are I(1), not I(0). Housing starts, on the other hand, appear to be stationary in levels.

the new equilibrium, the city is physically larger, house prices are higher at any given location than they were before the population inflow, and starts are again zero. Starts occur only when the city makes the transition from one equilibrium to another, a period identified by the increase in the price level. Aggregate starts are uncorrelated with the house price level, but are positively correlated with the change in prices.

Following Mayer and Somerville, prices for developed sites are defined as the present discounted value of house rents. These prices capture the opportunity cost of the forgone agricultural rents, the capital cost of developing the land and building the structure, and the value of the location. The value is the savings in commuting costs at a given site when compared with more distant locations. To ensure a spatial equilibrium in the land market, this value declines as units are located closer to the urban fringe and farther from the employment subcenters. At the border, where landowners are indifferent between leaving land in agricultural use or developing it, equilibrium house prices must equal the agricultural value and the cost of the structure, so that location rents equal zero.[2] Beyond the boundary it is not economic to develop. At those points the implicit location value is negative, and while house prices there may exceed the agricultural values, they do so by less than the cost of developing the land and building the structure.

This model treats housing starts as a function of changes in housing prices instead of the current level of house prices.

New land is developed as the general level of house prices increases. Population growth triggers increases in the general price level of housing in the city as demand for housing rises at all locations. To accommodate the new residents, the area of the city must expand. And in a larger city, spatial equilibrium is achieved only if rents and prices at interior locations rise. At locations formerly beyond the area of urbanization, prices rise sufficiently to make it profitable to develop those sites; the raw land is now ripe for development because these formerly undeveloped sites yield greater returns in urban use as housing than as farm land.

In the aggregate, increases in the general citywide level of prices bring new land into development and allow new residential construction to occur. The process outlined above describes the mechanism by which a change in the price level leads to the development of raw land and housing starts. It reflects the equilibrium condition that house prices adjust to equate the total stock of housing with total demand for residential space. Changes in this stock, that is, starts, must then be a function of changes in price. For a given period, total housing starts equal the change in the stock, which is a function of the change in house prices over that period. The long-run equilibrium reflects the relationship between stock and price levels, where prices reflect demand.

Most existing empirical work on housing supply either combines supply and demand relationships together into a single equation or attempts to estimate starts as a function of the price level. In the former case, a single reduced form equation is used by authors such as Muth (1960), Follain (1979), Stover (1986), and Malpezzi and Maclennan (1994) to derive estimates of housing supply elasticities. They find no evidence of a statistically significant long-run relationship between price levels and demand measures, suggesting that the supply curve for new housing is perfectly elastic. Because the models do not separately identify supply and demand, the coefficient estimates may be unreliable, however.

Direct modeling of new housing supply curves by Poterba (1984, 1991), Topel and Rosen (1988), and DiPasquale and Wheaton (1994) yields a very different result, an upward-sloping supply curve. They directly compare housing starts with the general level of new home prices and prices for labor and material inputs, that is, output and input prices. The measures of the price elasticity of starts for these studies are around 3.0, dramatically lower than the levels found in the reduced form estimation.

The model in Topel and Rosen borrows heavily from the general investment literature. Investment models may be adequate to describe the supply of residential structures, but they are not appropriate for the role of land. This spatial aspect is a distinguishing

[2] This class of model describes the supply decision at the urban fringe, rather than redevelopment. However, since most new detached single-family construction occurs at the urban border, constraining ourselves to a model that describes dynamics at the border is not unreasonable. We are also ignoring the option value of undeveloped land, a topic developed in Titman (1985), Capozza and Schwann (1989), Capozza and Helsley (1990), and Williams (1991), among others.

feature of housing. For instance, new housing is not identical to existing housing because their locations differ. New construction tends to occur at the urban fringe, increasingly distant from existing units and the city center as an urban area grows. The price paths of new and existing housing units are also different: In a growing city, the price of existing units rises relative to the price of new housing, because with growth in city size the location premium for existing units increases. Yet, like other researchers, Topel and Rosen measure demand using the price series for new housing. Also, spatial conditions can cause cities with very different demands for new housing to have similar price levels. As DiPasquale and Wheaton note, house price levels in a large slow-growing city and a smaller fast-growing city may be identical, but their existing housing stocks and the amounts of new construction will differ dramatically.

House price levels in a large slow-growing city and a smaller fast-growing city may be identical, but their existing housing stocks and the amounts of new construction will differ dramatically.

DiPasquale and Wheaton use a stock adjustment framework to include land in the housing starts equation. The stock adjusts slowly, so that current starts are a function of the desired stock and the stock in the previous period. Lagged stock reflects the historic path of development and, with price, describes prevailing land market conditions. At a given price level, the larger the lagged stock, the lower are starts, because the city is closer to its long-run equilibrium in the housing market. A large, slow-growing city can have high land prices, and thus high house prices, and still have few starts because the existing stock is large relative to the price level. In contrast, a small, fast-growing city might have similarly high prices, which capitalize the future growth in housing demand, with a much smaller existing stock. Starts are higher, while the existing stock is small relative to the price level. The combination of price and the lagged stock allows DiPasquale and Wheaton to capture aspects of demand that are not revealed by the price level alone. Empirically, however, the stock of housing is notoriously difficult to measure in non-Census years, because physical depreciation is unobserved and not all starts are completed with the same lag. The authors are forced to assume constant decennial rates of removals or demolitions (which in fact vary significantly across time as well as across regions).[3]

Though the formal structure of the model we use differs from that of DiPasquale and Wheaton, our approach very much builds on their work. The model from Mayer and Somerville (1996) formalizes the interrelationship among movements in housing prices, land development, and the existing stock that is implicit in the DiPasquale and Wheaton treatment. But instead of including lagged stock in a starts equation, this relationship is captured by changes in house prices. This approach uses a more formal characterization of the land development process than they use, together with the same equilibrium relationship between the price of existing homes and the stock of housing.

II. The Empirical Model and Data

Based on the model described in the previous section, we construct an estimating equation for starts as a function of changes in house prices and input costs. Costs include the expected real interest rate, $E(r)$, and construction costs, c. In levels, construction costs determine price levels, so we use the change in construction costs to develop the following estimating equation:

$$starts_t = f(\Delta p_t, \Delta p_{t-1}, E(r_t), \Delta c_t). \qquad (1)$$

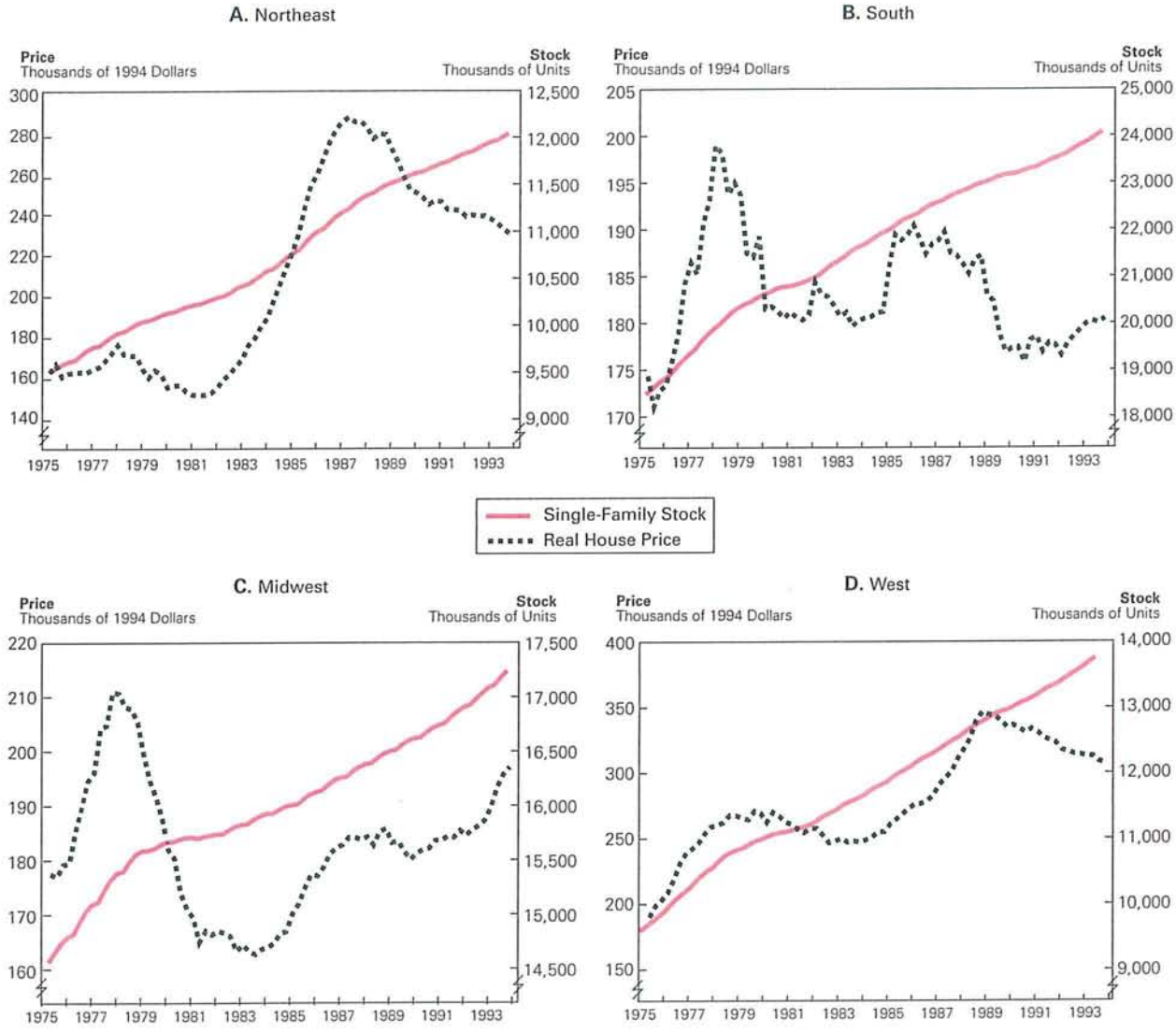
Builders indicate that their cost of funds depends on the prime rate rather than mortgage rates or rates on Treasury bills, so we construct an expected real interest rate using the current nominal prime rate and expected inflation. Because builders may not be able to respond immediately to changes in market conditions, possibly because of lags in the land assembly and permitting process, the estimating equation allows for one quarterly lag in changes in prices and in the prime rate.

Figures 1A to 1D graph the data for the four Census regions on housing prices and stock used

[3] The stock of housing in a given quarter is calculated by taking the stock last quarter, adding last quarter's starts, and subtracting the decennial average removal rate.

Figure 1

Stock of Single-Family Houses and Real House Prices



Source: Freddie Mac house price index and authors' calculations.

in this paper. The stock series is based on the 1970, 1980, and 1990 Census counts and the 1993 American Housing Survey estimate of the number of year-round single-family residences. Inter-decennial removal rates are linearly interpolated to ensure that for any inter-Census period, stock in the base year plus total starts, minus units estimated to have been removed, equals the end-year stock. The annual removal rates vary by region and decade, ranging from 0.12 to 0.91 percent of the stock.

We construct a series for real house prices that measures dollar changes in house prices in each region over time. The measure is developed by combining the quarterly Freddie Mac resale price index from 1975 to 1994 for the four Census regions, to determine the rate of change in prices, and the hedonic

Table 1
Descriptive Statistics, 1975 to 1994

	Northeast	South	Midwest	West
Single-Family Stock (000)				
Mean	10,769	21,570	15,962	11,644
Standard Deviation	798	1,645	700	1,213
Maximum	12,077	24,064	17,205	13,703
Minimum	9,479	18,321	14,438	9,409
Starts (000)				
Mean	33.72	116.33	51.99	61.80
Standard Deviation	13.81	24.18	23.24	17.47
Minimum	71	179	118	104
Minimum	9	62	9	24
Real Price (1994$)				
Mean	101,274	72,915	71,321	124,888
Standard Deviation	22,521	2,472	4,677	19,961
Maximum	140,022	79,354	82,642	158,117
Minimum	74,834	68,244	64,056	79,941
Change in Real Price (1994$)				
Mean	387.0	40.6	118.2	794.3
Standard Deviation	2,188	888	1,019	2,078
Maximum	6,191	2,312	3,146	6,276
Minimum	−3,950	−3,031	−2,447	−3,099
Expected Real Prime Rate (%)				
Mean	4.83	4.83	4.83	4.83
Standard Deviation	3.50	3.50	3.50	3.50
Maximum	13.93	13.93	13.93	13.93
Minimum	−1.10	−1.10	−1.10	−1.10

Source: U.S. Bureau of the Census, Freddie Mac, and the Board of Governors of the Federal Reserve System.

regional series developed in DiPasquale and Somerville (1995) that estimates constant-quality prices by region for identical units, to fix price levels across regions. The Freddie Mac index is a repeat sales index developed from observations, either sales or refinancings, of the same properties over time. It is consistent with the measure of house prices described in our model because it is based on houses at fixed locations. In contrast, the Census C-27 new house price series, used by most previous studies of new construction, measures houses in ever-changing locations.

Table 1 describes the data used in this study.[4] The national CPI-less-shelter series is used to measure inflation rates. The expected real prime rate series is the estimated value generated by regressing the current change in the real prime on two of its own lags. As is apparent from Figures 2A to 2D, housing starts vary significantly over the cycle and across all four regions. Annual starts as a percentage of the total stock range from a low of 0.3 in the Northeast in the first quarter of 1982 to a high of 4.2 in the West in the second quarter of 1977.

III. Empirical Results

Table 2 shows the results of estimating the empirical equation (1) separately for each region.[5] All regressions allow for first-order serial correlation (AR1) and use instrumental variables to control for the endogeneity of prices. Instruments include lagged changes in mortgage rates, changes in employment net of construction employment, changes in energy prices, and, following Buse (1989), lagged values of all other exogenous variables.

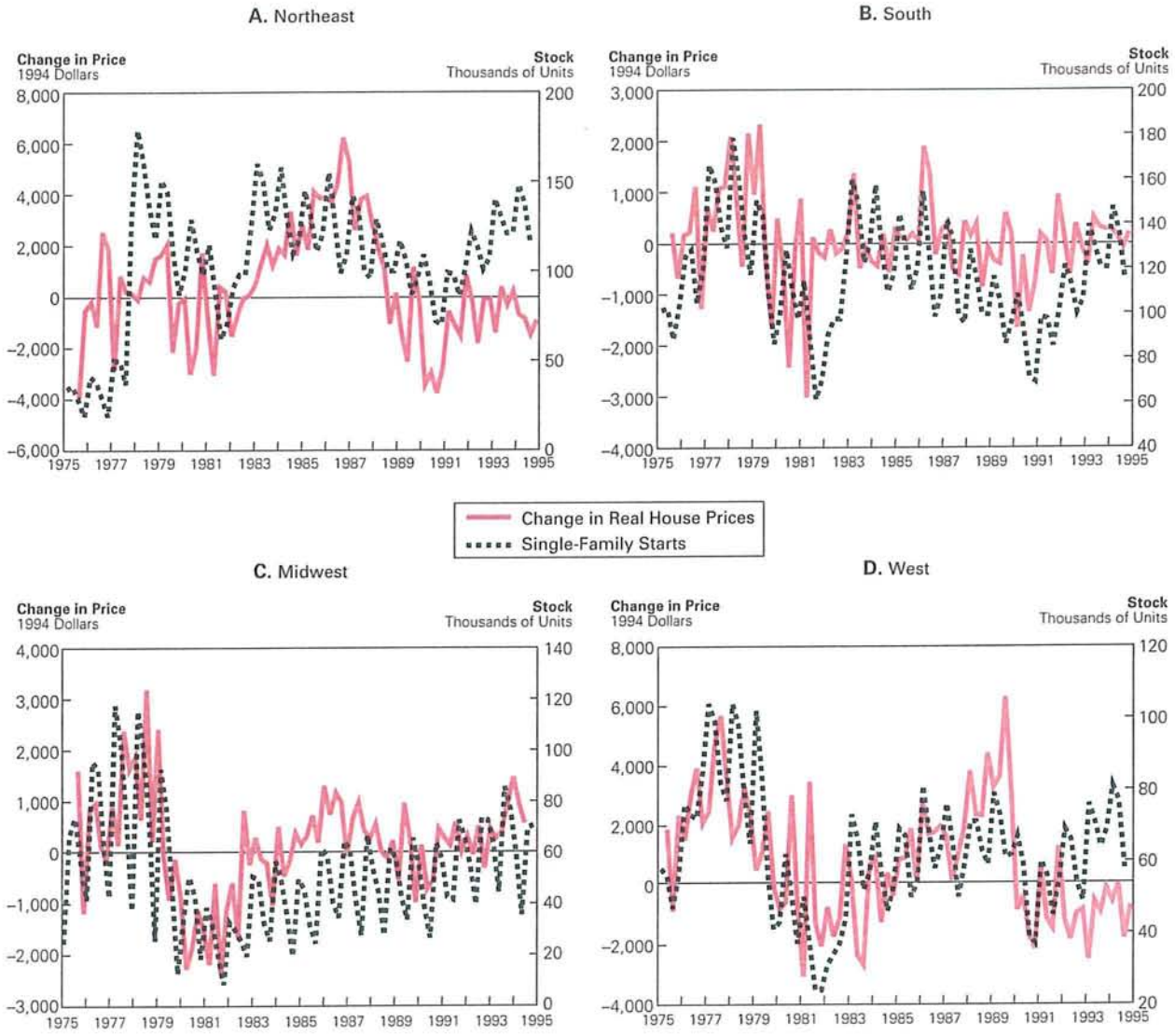
The coefficients on current changes in prices and current expected real interest rates are uniformly significantly different from zero across regions, while coefficients for lagged changes are mixed in significance. The effect of a $1,000 increase in real house prices is not uniform across regions. In the Northeast,

[4] Casual observation suggests that both the housing stock and prices are almost certainly non-stationary because the mean of these variables is increasing over time. This evaluation is not surprising given that both series depend on such non-stationary factors as population or real income. However, using non-stationary data as the dependent variable in an ordinary least squares equation poses problems because it violates the assumption of a finite variance. Instead, we focus on the relationship between two stationary variables, the change in the stock (starts) and the change in real prices. These series (shown in Figures 2A to 2D) appear to be strongly correlated. Previous research has shown that the real price of existing housing is stationary in differences. Housing starts are also likely to be stationary, but in levels. Augmented Dickey-Fuller (ADF) tests for stationarity give somewhat mixed results. Test statistics do not allow rejection at conventional levels (95 percent confidence interval) of the hypothesis that these series are non-stationary. The estimated Dickey-Fuller coefficients, where 1.0 is equal to the presence of a unit root—which indicates non-stationary data—run from 0.925 to 1.003 for the stock and from 0.604 to 0.861 for starts. For prices the range is 0.852 to 0.968, while for change in prices the range is 0.332 to 0.786. However, small sample size and high levels of noise mean that the power of these tests is low (Faust 1993). In estimating equation (1) we rely on the above-mentioned research that demonstrates the stationarity of house prices, as well as the theory presented in the earlier section. While stationarity is not always demonstrated in an ADF sense in these data, the data plots and the ADF tests are more consistent with our treatment of housing starts as a function of changes in prices than with the conventional estimation of starts as a function of the price level.

[5] We exclude construction costs from the regressions because, like other researchers, we find the coefficient on these costs to be statistically insignificant. Somerville (1996) finds that some construction cost measures are biased and endogenous, and that price coefficients estimated with and without conventional cost measures are similar. Lacking good national instruments for materials costs, we have chosen to leave construction costs out of the regressions.

Figure 2

Single-Family Starts and Change in Real House Prices



Source: U.S. Bureau of the Census and Freddie Mac.

this increase would raise starts by 5,150 units (3,390 units in the quarter of the price increase and 1,760 units in the next quarter), whereas starts would grow by 5,770 units in the South, by 10,930 units in the Midwest, and by 4,180 in the West. The coefficient on the current expected real prime rate is negative and significant at the 5 percent level in all four regions, but the lagged value is not. In all four regions the AR1 autocorrelation coefficient on the lagged error term is positive and significant.

Lagged price changes are significant in the Northeast and West, but insignificant in the South and Midwest, suggesting a faster adjustment process in the latter two regions. This result is not surprising given that environmental regulations and development constraints in the Northeast and in California, Oregon,

Table 2
Regression Results
AR1 Instrumental Variable Estimation[a]

	Northeast	South	Midwest	West
Current Change in Price	.00339 (.00066)	.00432 (.00197)	.00905 (.00153)	.00291 (.00083)
Lagged Change in Price	.00176 (.00050)	.00145 (.00157)	.00188 (.00125)	.00127 (.00053)
Current Expected Prime Rate	−.9187 (.400)	−2.203 (.804)	−2.453 (.605)	−2.132 (.475)
Lagged Expected Prime Rate	−.1525 (.401)	−1.164 (.898)	−.6296 (.580)	−.3919 (.507)
Time Trend	.1052 (.051)	−.2049 (.287)	−.0343 (.062)	.0783 (.127)
Second-Quarter Dummy	21.22 (1.83)	27.76 (2.54)	37.32 (2.71)	17.39 (1.76)
Third-Quarter Dummy	17.84 (2.02)	18.16 (2.97)	30.8 (2.85)	9.824 (2.05)
Fourth-Quarter Dummy	10.28 (1.88)	−2.891 (2.58)	15.84 (2.80)	−7.495 (1.82)
Constant	15.22 (4.48)	143.3 (26.87)	47.7 (5.62)	59.12 (11.79)
Number of Observations	75	75	75	75
AR1 Coefficient	.3093	.7946	.206	.6752
Adjusted-R sq	.765	.682	.841	.673
Log Likelihood	−238.6	−281.3	−265.7	−250.6

Standard errors in parentheses.
[a]Instruments for change in price include current and lagged values of change in non-construction employment, real energy prices, and nominal mortgage rates.

Table 3
Estimated Elasticities by Region

	Northeast	South	Midwest	West
Price Elasticity of Stock	.048	.020	.049	.045
Price Elasticity of Starts	3.87	.90	3.75	2.11
Percent of Total Increase in Starts in Initial Quarter of Price Increase	65.7	74.9	82.8	69.6

and Washington (a very large component of the West) may well delay the response of builders to changes in market conditions. Respectively, 74.9 and 82.8 percent of the eventual increase in starts occurs immediately in the South and the Midwest, as compared with 65.7 and 69.6 percent in the Northeast and West (Table 3). These results suggest that the development process is fastest in the South and Midwest.

Table 3 converts regression coefficients into estimated elasticities using the mean values of the relevant variables. Elasticities are computed for the total supply of housing (the stock) as well as for starts. The results are quite provocative. The total supply is almost completely price inelastic, with implied elasticities of less than 0.05 in all regions. This should not be too surprising, because starts are such a small fraction of the total stock, less than 2 percent on average for an entire year. For the true supply elasticity to equal unity, the price elasticity of starts would have to equal approximately 50, and be higher in areas such as the Midwest and the Northeast where annual starts comprise an even smaller percentage of the total stock. The estimates for the regional price elasticities of starts, a measure estimated in most housing supply models, range between 0.9 and 3.9, with an overall average of 2.7. Somewhat surprisingly, these results are similar to Topel and Rosen's estimate for national data in regressions of starts on price levels.

The low elasticity of starts in the South and the modest size of the elasticity in the West are striking, given the strong employment and population growth in those regions. One possible explanation for the lower than expected elasticities in the South and West is that the empirical model used in this article is based on a monocentric model of urban development, which may be less appropriate for these areas. The relationship between starts and price changes becomes less clear if employment and population are distributed more uniformly throughout a metropolitan area. In the extreme, if employment grows at the same locations as new housing (generally at the border of the metropolitan area), then the price of existing houses does not rise with employment growth. In this case, land values (for residential use) at the border of an urban area will rise sufficiently to support new development, but land values for existing units will remain unchanged. Edge cities appear to be more common in the South and the West, areas that have experienced much of their growth in the recent era of suburban employment.

IV. Testing for Evidence of a Credit Crunch

Claims of regulatory-induced reductions in credit availability—often referred to as a credit crunch—

have existed for many years. Early research on credit restrictions looked at the impact of interest rate regulation on the availability of credit. However, the passage of the Depository Institutions Deregulation and Monetary Control Act in 1980 phased out interest rate ceilings that were believed to have limited banks' ability to raise funds. During the mid 1980s, the national economy was strong and capital regulation was not as well developed as it would be in subsequent years. Beginning in 1986, however, the combination of falling oil prices and over-aggressive commercial real estate lending by banks and S&Ls in the oil patch states and the Southwest exacerbated interest rate losses that these institutions faced in the early 1980s (due to the mismatched term structure of assets and liabilities) and led to massive failures. (See Browne and Rosengren 1992 for a summary of this literature.)

A strong international banking agreement also led to a greater focus on bank capital regulation in the late 1980s. The Basle Accord, signed in 1987 and implemented between 1989 and 1992 by the major industrial countries, specified different risk weights for various categories of loans and effectively encouraged banks to increase their holdings of government securities and residential mortgages at the expense of other types of lending.[6]

As a reaction to the banking problems in the oil patch states in the late 1980s, some critics have argued that banks were scrutinized more carefully and in some cases forced to "mark to market" their distressed assets to a greater extent than in previous downturns. For example, in New England after 1989, regulators began to require banks to establish reserves for loans that were currently making payments but whose underlying collateral had fallen below the remaining loan balance, the so-called "performing nonperforming loans" (Litan 1992).

Later, the Federal Deposit Insurance Corporation Improvement Act of 1991 (FDICIA) required regulators to set explicit capital thresholds and take prompt corrective action to ensure that banks met these requirements. Under the new capital thresholds implemented after FDICIA, banks with a leverage ratio (total tier 1 capital, including equity, divided by total average assets) of as low as 4 percent were considered undercapitalized and were required to shrink asset growth (loans), cut dividends, or raise additional equity capital. For the first time, regulators had relatively little discretion to permit poorly capitalized banks to deviate from these new requirements. Regulators began enforcing the more stringent leverage ratios after 1991. These were often included in "formal actions," under which troubled institutions agreed to meet the new, higher leverage ratio threshold in as little as two years.

The Northeast, and in particular New England, was the first region to suffer a recession after the implementation of the stricter bank capital regulations and the signing of the Basle Accord.

The Northeast, and in particular New England, was the first region to suffer a recession after the implementation of the stricter capital regulations and the signing of the Basle Accord. The region also suffered a sharp decline in commercial real estate values that reduced bank capital significantly. Because raising new capital to meet regulatory requirements was extremely difficult during the economic downturn, banks under "formal action" often chose to shrink their assets. Real estate lending was particularly hard hit, possibly because regulators looked much more carefully at new real estate loans and because banks were unwilling to make new loans in a sector where they recently had suffered such severe losses (Peek and Rosengren 1996). This shrinkage in real estate and other types of small business lending led several researchers (Peek and Rosengren 1994, 1995a, 1995b, and 1996 and Litan 1992) to conclude that the region suffered from a credit crunch in the early 1990s.

More recently, Southern California suffered a significant downturn that some have argued is similar to the credit crunch in New England and the rest of the Northeast. Bank regulation may have been similar, but the structure of the construction industry differs between the two regions. New England, and to some

[6] Loans for commercial real estate and all types of construction, regardless of the terms of the loan, were deemed risky under the Basle Accord and banks were required to hold additional capital. Because these risk-weighted capital requirements were not binding for most institutions, however, the evidence is mixed as to whether banks adjusted their assets to be more consistent with the risk weights specified in the Basle Accord. (See Hall 1993 and Hancock and Wilcox 1994.)

extent the entire Northeast, are more mature areas with slower growth and fewer large tracts of land attractive to large national builders. As a result, most new residential housing is constructed by small local builders whose output is sensitive to reductions in lending or increased down payment requirements by local banks. Southern California, on the other hand, has many large builders, who might be expected to have direct access to national debt and equity markets. Also, national building firms, which have more diversified cash flows, have a much larger market share in California than in the Northeast. Finally, the loss in real estate values in California was much smaller than the decline in the Northeast. Thus, California was less likely to have suffered from credit-related reductions in the supply of new construction.

The results in this study suggest that more attention should be paid to housing supply in understanding regional real estate cycles.

While the previous discussion suggests that recent changes in banking regulation made a credit crunch more likely after 1991, particularly in the Northeast, other researchers (Bernanke and Lown 1991; Hancock and Wilcox 1995) argue that the loss of bank capital in previous periods was also associated with a credit-related reduction in output. Using data from 1984 to 1992, Hancock and Wilcox find a negative relationship between bank capital and a variety of construction measures. As the authors indicate, their results suggest that nonbank sources of capital do not necessarily fill the vacuum when bank capital declines. While their work clearly indicates the potential real effects of credit restrictions, their use of bank capital data may be problematic if declines in capital occur contemporaneously with slowdowns in general economic activity. In that case, researchers may not be able to distinguish between lower real estate activity that results from economic conditions that reduce demand for loans, and lower real estate activity that results from declines in the supply of bank lending (the credit crunch hypothesis).[7] Also, Hancock and Wilcox's econometric results suffer from the potential specification problems associated with using price levels instead of price changes in a housing starts equation, especially if capital reductions are correlated with real estate price declines.

The estimated housing starts functions shown in Table 2 provide an alternative approach to looking for real effects of credit shortfalls. If credit for residential real estate development and construction is constrained by overly strict banking regulation, then the level of new housing starts should be lower than the level that would be predicted by a well-specified housing supply equation. According to this view, a credit crunch is consistent with a prolonged period when the residuals from the housing supply estimation are negative. A measure of credit availability is not included directly in the regression equation because of possible endogeneity for which we have no readily available instruments.

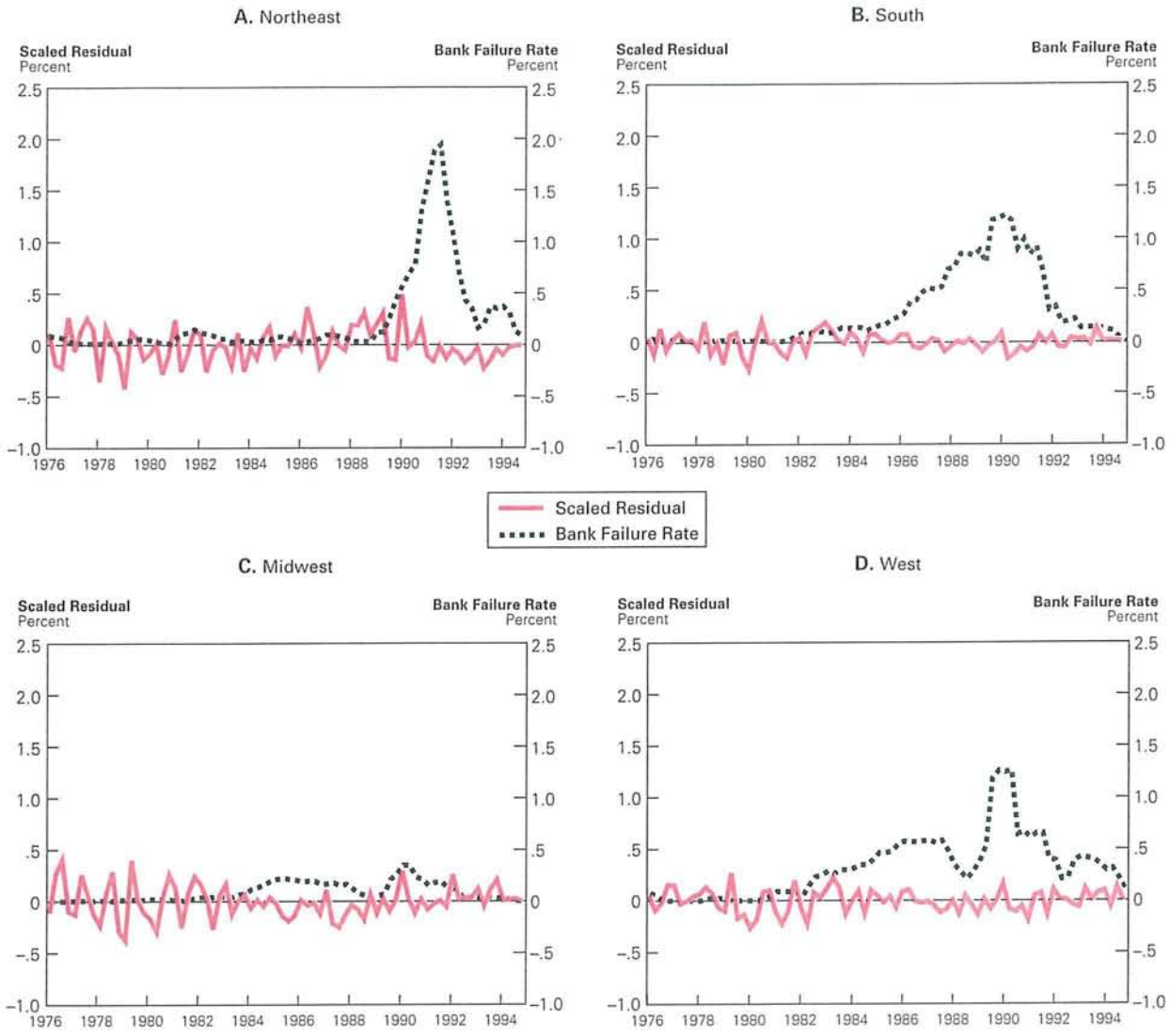
Figures 3A to 3D present graphs of the scaled residuals from the regressions in Table 2, along with a four-quarter moving average of the bank failure rate for each region. It is quite striking that for the Northeast, the residuals (the red line) are negative for an extended period in the early 1990s, a difference that is statistically significant from zero at the 10 percent level. During the same period, the region's bank failure rate was almost 2 percent per quarter, a much higher failure rate than that experienced by any other region since 1975. The high failure rate for banks was the result of a sharp decline in the region's real estate values and particularly commercial real estate, which comprised the largest part of most banks' real estate lending, possibly combined with the effects of stricter bank regulation.

Figure 3B shows similar data for the South. Somewhat surprising is the lack of evidence that housing construction was affected by credit-related problems during or shortly after the significant economic downturn in the oil patch states in 1985 and 1986. Although commercial real estate values fell even more in parts of the South than in the Northeast, regulators may have acted more slowly to close troubled banks in the South. The bank failure rate peaked in 1990, well after the trough in the region's economy. One potential problem in these findings is that this test has low power in testing for credit restrictions in areas that do

[7] The results in Peek and Rosengren (1995b) suggest that such endogeneity problems may be overstated. The authors show that declines in lending and the shrinkage in capital correspond to the timing of bank inspections and the imposition of regulatory enforcement actions rather than declines in demand.

Figure 3

AR1 Scaled Residuals and Bank Failure Rate



Note: The scaled residual is computed by dividing the actual residual by the average starts for each region.
The bank failure rate is a four-quarter moving average.
Source: Board of Governors of the Federal Reserve System and authors' calculations.

not correspond to the four Census regions used in this analysis: the "oil patch" is only a portion of the South region. Unfortunately, data on housing starts are not available for individual states.

Data from the Midwest and the West also show little evidence of credit-related constraints on new construction. Neither region suffered from as severe a decline in the number of banks as the Northeast or the South. Again, geographic aggregation may present problems in identifying credit-related problems in the West, where Southern California was suffering from a recession at the same time that the Mountain states were booming.

While by no means conclusive, these results suggest that if credit restrictions limited single-family housing construction, these effects occurred only in

the Northeast. Such a credit crunch likely resulted from the deep decline in asset values that led to poorly capitalized banks, and was exacerbated by changes in bank capital regulation. This analysis includes only one measure of banking problems, the failure rate. Other measures might be relevant, especially the aggregate loss of bank capital in a region. Unfortunately, a consistent measure of bank capital is not available over the entire period of this study. Also, the omission of other, non-credit-related factors from the analysis or simple misspecification could bias these results. This problem is difficult to address because measures such as bank capital are endogenous, and there are few good instruments for this variable.

V. Conclusion

This article presents an empirical model of new housing supply, based on models of the conversion of raw land to urban use. The empirical results show that housing starts respond to changes in existing house prices rather than to the level of house prices, the measure used in previous research. We apply this model to the four U.S. Census regions in order to estimate regional supply elasticities. While housing starts are somewhat price-elastic, with an estimated elasticity of between 0.9 and 3.9, the price elasticity of the housing stock, a fuller measure of supply elasticity, is quite small—less than 0.05 in all regions. The results also show a prolonged period of below-predicted construction in the Northeast during the early 1990s that does not appear during downturns in other periods or in other regions. These findings are consistent with the hypothesis that a severe negative shock to local asset values (and thus bank capital), possibly combined with changes in banking regulation in the late 1980s and early 1990s, led to a "credit crunch" that had real output effects, including reduced new housing construction.

Finally, as noted in the introduction, few papers have studied the determinants of the supply of residential real estate, despite the importance of supply in determining prices. The results in this study suggest that more attention should be paid to housing supply in understanding regional real estate cycles. To the extent that factors such as credit constraints, permitting restrictions, or regulatory-imposed delays limit the adjustment of housing supply in a recovery, prices may be more likely to overshoot their equilibrium level, leading to an exacerbated boom-bust cycle.

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