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Capital account liberalization, it is fair to say, remains one of the most controversial and least understood policies of our day. One reason is that different theoretical perspectives have very different implications for the desirability of liberalizing capital flows. Another is that empirical analysis has failed to yield conclusive results.

I. Theoretical Perspectives

Models of perfect markets suggest that international capital movements benefit both borrowers and lenders. Because international investment is intertemporal trade, trade between periods and trade between countries have precisely analogous welfare effects. The case for free capital mobility is thus the same as the case for free trade but for the subscripts of the model. To put the point another way, the case for international financial liberalization is the same as the case for domestic financial liberalization. If domestic financial markets can be counted on to deliver an efficient allocation of resources, why can’t international financial markets?

The answer, another influential strand of thought contends, is that this efficient-markets paradigm is fundamentally misleading when applied to capital flows. Limits on capital movements are a distortion. It is an implication of the theory of the second best that removing one distortion need not be welfare enhancing when other distortions are present.

There are any number of constellations of distortions, especially in developing economies, for which this is plausibly the case. If the capital account is liberalized while import-competing industries are still protected, capital may flow to

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1. The intertemporal approach to capital mobility owes its origins to Fisher (1930). Influential modern treatments that resuscitated this approach and summarized its implications include Sachs (1981) and Frenkel and Razin (1996).

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sectors in which the country has a comparative disadvantage, with immiserizing effects (Brecher and Diaz-Alejandro 1977). If a downwardly inflexible real wage causes too many resources to be devoted to capital-intensive activities, a capital inflow may aggravate this misallocation, again reducing the incomes and welfare of domestic residents (Brecher 1983). If information asymmetries are endemic to financial markets and transactions, then there is no reason to assume that financial liberalization, domestic or international, will be welfare improving (Stiglitz 2000). Even if information asymmetries in domestic markets are judged insufficiently severe to undermine the case for domestic financial liberalization, the same may not be true of international financial liberalization to the extent that international financial transactions take place among agents separated by greater physical and cultural distance. Insofar as these problems are most severe when the transactions in question involve developing countries, where the capacity to assemble and process information relevant to financial transactions is least advanced, there can be no presumption that capital will flow to uses for which its marginal product exceeds its opportunity cost.

But are restrictions on capital movements any better? Capital controls shelter financial intermediaries from foreign competition. They weaken the market discipline on policymakers. They vest additional power with bureaucrats who may be even less capable than markets at delivering an efficient allocation of resources and open the door to rent seeking and resource dissipation by interest groups seeking privileged access to foreign capital.

Although there is theoretical support for both positions, the unfortunate fact is that the evidence on them does not speak clearly. It is not simply quarrels among theorists that have rendered capital account liberalization controversial, in other words, but the failure of attempts to move beyond anecdote and assertion to systematic empirical analysis to yield conclusive results.

The question is why. Have the questions been formulated poorly? Are the methods flawed? Or are the data not up to the task? A critical review of the literature is the obvious first step toward answering these questions. The challenge is that the literature is large and varied. Some studies approach the phenomenon from a macroeconomic point of view, others from a microeconomic perspective. Some focus on the effects of capital account liberalization, others on the causes—that is to say, on the political economy of the decision to liberalize. Any survey of this extensive and varied terrain requires a focus. Here the focus is on cross-country studies of the causes and effects of capital account liberalization, because this is where the big questions are asked and attempts are made to reach conclusions of general applicability to developing countries.

2. This focus on cross-country ("large n") studies not only dictates what is reviewed and what is left aside. It also differentiates this survey from other reviews of the literature on capital controls and capital account liberalization (such as Dooley 1996; Williamson and Mahar 1998; Cooper 1999; Edwards 1999; Neely 1999). At the opposite end of the empirical spectrum lie case studies of particular episodes. While this "small n" approach allows a particular episode to be considered in great detail, it
II. Measuring Capital Account Liberalization

A first reason why studies of capital account liberalization do not speak clearly is the difficulty of measuring the policy. This section considers three approaches to the problem: measures based on statute, on actual flows, and on asset prices.

Efforts to establish the presence of capital account restrictions on the basis of statute typically build on the data published by the International Monetary Fund (IMF) in its *Exchange Arrangements and Exchange Restrictions* annual. Most studies focus on restrictions on payments for capital transactions. When capital account liberalization is related to a measure of economic performance like GDP growth over a period of years, the annual observations are transformed into a variable measuring the proportion of years when the country had restrictions in place. Some investigators supplement this information with the IMF's measure of restrictions on payments for current transactions and sometimes with its measures of surrender or repatriation requirements for export proceeds, separate exchange rates for capital transactions or invisibles, and bilateral payments arrangements with members and nonmembers.

is likely to run head long into an identification problem, because many things will have been changing in the country in question in the period under consideration. "Hybrid studies" attempt to strike a balance between these approaches by pooling detailed information on the capital account regime for several countries and years. An example is Reinhart and Smith (1998), who focus on five cases in which restrictions on capital account transactions were imposed or tightened—Brazil in 1994, Chile in 1991, Colombia in 1993, the Czech Republic in 1995, and Malaysia in 1994—and analyze a four-year window surrounding the event. Similarly, Edison and Reinhart (1999) use daily financial data to examine four capital control episodes: Brazil in 1999, Malaysia in 1998, Spain in 1992, and Thailand in 1997. Four countries offer more degrees of freedom than one, to be sure, but it is still hard to know how far one can generalize from a handful of cases.

3. Along with narrative accounts of the main changes in policies toward the exchange rate and current and capital account payments, starting in 1967 this report has included a table summarizing the exchange arrangements adopted by member countries, but without any detail on how the narrative accounts are converted to summary data. Prior to 1967, the publication provided exclusively qualitative descriptions of restrictions. Some investigators (for example, Quinn 1997) have built indices of capital account liberalization for the earlier period from this information. In the second half of the 1990s, the IMF began providing more detailed breakdowns of policy measures. Starting in 1996, the report disaggregated controls on export proceeds into "surrender requirements for export proceeds" (requiring exporters to surrender to the authorities foreign exchange earned from exporting) and "repatriation requirements for export proceeds" (requiring them to surrender even payments made to overseas accounts). Starting in 1997 it distinguished controls on capital inflows and outflows. These changes create problems for investigators seeking to create time series for capital account liberalization. Thus Glick and Hutchinson (2000) use surrender requirements for export proceeds, which are more restrictive than repatriation requirements for export proceeds, as equivalent to the pre-1996 export surrender measure, and code a country as having capital account restrictions in place in 1997 or 1998 when the report listed controls as in place for five or more of these capital account subcategories and "financial credit" was one of the categories restricted.

4. Restrictions on current account transactions affect the ability of the private sector to obtain foreign exchange for payments related to merchandise imports and to retain foreign exchange earned through exporting and the ability of foreign direct (and other) investors to repatriate interest earnings and other profits. The argument for using them is that current account transactions can be used to evade restric-
These data have limitations. Data on "restrictions on payments for capital transactions" available before 1996, for example, may not reflect restrictions on capital transfers by nonresidents. In addition, drawing a line between measures affecting the current account and those affecting the capital account is problematic. Thus data on separate exchange rate(s) for some or all capital transactions, for instance, include measures affecting some or all invisibles, which may include payments on current as well as capital account. Bilateral payments arrangements with members and nonmembers include not just separate exchange rates for capital transactions, which are directly relevant to a consideration of capital account liberalization, but also the use of one unitary rate for transactions with one country but a different unitary rate for transactions for another country, where the second kind of multiple rate is often used to discriminate among transactions on current as well as capital account.

Although the presence of current account restrictions, export-surrender requirements, bilateral payments arrangements, and separate exchange rates may convey information on the scope of efforts to deter the evasion of capital controls, deterrence is not their main purpose. Moreover, current account restrictions are likely to have other important effects that the unwary investigator may conflate with their impact on capital mobility. They influence merchandise trade. They limit opportunities for repatriating interest and principal. And insofar as they tend to be imposed by countries suffering from serious policy imbalances, their "effects" will reflect the influence of these deeper policy problems as much as those of the capital controls themselves.

Most studies "solve" the problem of measuring the intensity of controls by ignoring it. They settle for constructing a dummy variable for the presence or
absence of controls. In an attempt to go further, Quinn (1997) distinguishes seven categories of statutory measures for 56 countries for 1950–94 and for 8 more countries starting in 1954. Four are current account restrictions, two are capital account restrictions, and one captures international agreements constraining a country’s ability to restrict exchange and capital flows, such as membership in the Organisation for Economic Co-operation and Development (OECD). For each category, Quinn codes the intensity of controls on a 2-point scale (from 0, most intense, to 2, no restriction) to produce a 0–14 index of current and capital account restrictions and a 0–4 index of capital account restrictions. Not surprisingly, Quinn’s index has proven wildly popular and has been used by many subsequent investigators.

The difficulty of deriving measures of the policy regime from information on statutes and policies has led investigators to experiment with alternatives. Kraay (1998) and Swank (1998) use actual capital inflows and outflows as a percentage of GDP as a measure of the freedom of capital movements. The problem, as these investigators are aware, is that actual inflows and outflows will be affected by a range of policies and circumstances—monetary, fiscal, and exchange rate policies; the global economic and financial climate; and political circumstances, to name three—and not merely by restrictions on capital flows. Hence, this measure is unlikely to be an informative indicator of the capital account regime.

Bekaert (1995) and Aherane and others (2000) use one minus the ratio of the market capitalizations of the International Finance Corporation’s (IFC) Investable and Global Indices. The Investible Index consists of the stocks (or portions of stocks) in the Global Index deemed to be available to foreign investors. Thus, one minus the ratio of the two can be interpreted as a measure of the intensity of foreign ownership restrictions. The limitation of this measure, obviously, is that it captures only restrictions on equity inflows.

7. Such a high degree of differentiation necessarily relies on the judgment of the coder. Quinn addresses this problem by having each observation coded by two coders and then reconciling the differences.

8. A more detailed index has been constructed by Johnston and others (1999) for 41 industrial, developing, and transition economies, but only for 1996. This uses the detailed breakdown of 142 individual types of exchange and capital controls (aggregated into 16 categories) first published in Exchange Arrangements and Exchange Restrictions in 1997. The existence and intensity of controls are measured by normalizing the number of actual categories of controls (separately for controls on current and capital accounts) by the number of feasible measures. The number of countries for which they provide these estimates is limited, reflecting the limited coverage of the 1997 edition of Exchange Arrangements and Exchange Restrictions. In addition, the time dimension is lost due to the absence of comparable data for prior years.

9. It is likely to be useful only for distinguishing countries wholly closed to capital flows, where payments on capital account will be zero, from more open countries, the notion being that only countries with draconian controls that render them wholly closed to international financial markets will display neither inflows or outflows.

10. In addition, the measure captures more than statutory controls; for example, if a large firm that trades on, say, the Manila Stock Exchange is held mainly by one or two Filipino investors, their share would enter the Global Index but its weight in the IFC would be based on the portion of the shares available to foreigners.
Several researchers have used the correlation of stock market returns across countries as a measure of the international integration of securities markets. But the correlation of raw returns says little about the integration of markets, because returns will vary with the characteristics of the underlying assets, which depend on the characteristics and condition of the entities issuing the claims. Thus, in a study representative of the genre, Bekaert (1995) first regresses national returns in excess of the U.S. interest rate on five instrumental variables (lagged local and U.S. excess returns, local and U.S. dividend yields, and a transformation of the U.S. interest rate, variations in which might create reasons why the excess returns on different markets might differ) to derive expected returns, before computing the correlation of the these expected returns with expected returns in the United States as a measure of market integration. Clearly, the resulting measure is only as good as the model that generates the expected returns. These studies show some markets to be more integrated than would be expected from the statutory restrictions placed on foreign ownership of domestic securities. A limitation of the approach is that it is hard to know whether the contrast reflects the limited effectiveness of the statutes, which results in a misleading picture, or problems with one or more of the assumptions needed to derive expected returns.

Other researchers use onshore-offshore interest differentials and deviations from covered interest parity to measure capital mobility. Unlike stock market returns, which must be purged of premia and discounts associated with the characteristics of the entities issuing them before they can be used to gauge market integration, short-term interest rates can be analyzed without transforming them in model-contingent ways. However, interest differentials tend to be available only for a limited number of countries and years—specifically for countries important enough to have well-developed offshore markets and advanced enough financially to have well-developed forward currency markets. Because industrial and emerging markets with these characteristics are not representative of the

11. A disadvantage of this simple implementation is that no changes in the estimated degree of market integration are allowed to occur over time. Harvey (1995) and Bekaert and Harvey (1995) implement rolling- and switching-regression methods that, subject to further assumptions, permit the degree of market integration to vary over time.

12. If assets are priced according to a multifactor model rather than the one-factor model with constant risk exposures that Bekaert assumes, emerging markets might display cross-section differences in risk exposures and in the correlation of expected returns with the U.S. market, even if those markets are otherwise integrated internationally.


14. Researchers justify their disregard of the country risk premium by focusing on high-quality debt securities for which default risk is close to zero. They disregard currency risk by focusing on covered interest parity.
larger population of developing countries, drawing broad generalizations from these studies is likely to be problematic. Onshore-offshore interest differentials also have the inconvenient property of widening when there is an incentive for capital to move (when there is fear of a crisis, for example), while remaining narrower at other times. To put the point another way, differentials reflect not just the stringency of statutory controls but their interaction with ancillary policies and circumstances, making it difficult to separate the two influences.

This observation points to a limitation of virtually all studies of capital controls. Controls tend to be imposed and removed as part of a larger package of policy measures. Clearly, then, it is important to control for the other elements of the reform package when studying the connections between capital account restrictions and economic growth, investment, and financial depth. Alas, this is easier said than done. Trade openness, financial depth, institutional development, and the like may be no easier to measure in an economically meaningful way than the presence or absence of capital controls. Developing adequate measures of capital account restrictions is a particular problem for the literature on the causes and effects of capital controls, but the more general problem of adequately capturing the economic, financial, and political characteristics of economies, which impinges on all cross-country empirical work of this sort, should not be overlooked.

III. WHO USES CONTROLS, WHO LIBERALIZES, AND WHY?

A large literature addresses the circumstances under which capital accounts are opened and the circumstances under which restrictions are retained. Perhaps the single most robust regularity in this literature is the negative association between per capita income and controls. Per capita income is typically interpreted in this context as a measure of economic development: The more developed the country, the more likely that it will have removed restrictions on capital flows. The observation that all of today's high-income countries have removed their controls is consonant with the view that capital account liberalization is a corollary of economic development and maturation.

But why is this the case? Does the more advanced development of institutions and markets in the high-income countries mean that these countries can better
accommodate capital account liberalization—that well-developed markets and institutions shift the balance toward benefits and away from costs? Do these countries' well-developed political systems create avenues through which those who oppose restraints on their civil liberties—including their financial liberties—can make that opposition felt? Explaining why restrictions on international financial flows are more prevalent in some countries than others and why, in particular, they are less prevalent in the high-income countries is at the center of the literature on the political economy of controls.

A specific development-related rationale for controls—on capital outflows in particular—is that they can usefully channel domestic saving into domestic investment in countries where the underdevelopment of markets and institutions would otherwise result in a suboptimal supply of finance for investment. Thus Garrett and others (2000) find that there is a tendency to restrict capital account transactions in countries where domestic savings are scarce and that this effect is strongest for developing economies, where the premium on mobilizing savings for domestic investment is presumably the greatest.

Another strand of work pursues the association of controls with the exchange rate regime. Capital mobility increases the difficulty of operating a currency peg. Countries committed to pegging—China and Malaysia come to mind—may therefore support pegs with restrictions on capital flows. Contributors to the cross-country empirical literature generally find that countries with pegged exchange rates are less likely to have an open capital account (Leblang 1997, 1999; Milesi-Ferretti 1998; Bernhard and Leblang 1999; Garrett and others 2000).17

But it is not clear what should be regarded as endogenous and what as exogenous in this analysis. Does a willingness to adopt a more flexible exchange rate determine the readiness of some countries to remove controls? Or do increases in capital mobility, associated perhaps with the removal of capital controls, lead to the adoption of a more flexible exchange rate, either voluntarily or as a result of crisis? Causality may run both ways, making it difficult to interpret an ordinary least squares regression coefficient on the exchange rate. As will become apparent, this difficulty of pinning down the direction of causality is a chronic problem in the literature on capital account liberalization (and a theme of this survey).

Another line of thought portrays capital controls as an instrument of government revenue management. Controls limit the ability of residents to avoid the inflation tax on domestic money balances by shifting into foreign assets (Alesina and Tabellini 1989). They permit the authorities to raise reserve requirements on domestic financial institutions and thereby reduce their debt servicing costs.

17. Similarly, countries with macroeconomic problems that may threaten the stability of a peg (a weak current account, a large budget deficit, sudden increases in interest rates, for example) have a disproportionate tendency to maintain controls, outflow controls in particular (Johnston and Tamirisa 1996).
without eroding the inflation tax base (Drazen 1989). This perspective suggests that controls are likely to be used where the domestic financial system is tightly regulated and reserve requirements can be used to compel financial institutions to hold public sector liabilities. Consistent with this prediction, Leblang (1997) finds that governments that are less reliant on seigniorage are less likely to have capital controls. A further implication is that controls are less likely to be used where the inflation tax is not available because the central bank is independent and monetary policy is controlled by a conservative board. Epstein and Schor (1992), Alesina and others (1994), Quinn and Inclán (1997), Milesi-Ferretti (1998), and Bai and Wei (2000) all find that countries with more independent central banks are less likely to use controls.

But does this pattern reflect the implications of central bank independence and domestic financial liberalization for the availability of inflation tax revenues, as these authors argue, or a common omitted factor—laissez-faire ideology, for example—associated with financial liberalization, central bank independence, and capital decontrol alike? Some investigators have sought to distinguish between these alternatives by adding the political orientation of the government as a further determinant of the propensity to use controls. Once ideology is controlled for, they argue, any surviving correlation between central bank independence and domestic financial liberalization on the one hand and capital account liberalization on the other will reflect the implications of central bank independence and domestic financial liberalization for the seigniorage revenues promised by controls. Though findings on the effect of government ideology are mixed, the effect of central bank independence survives this extension, consistent with the implications of the seigniorage-centered approach.

A number of investigators pursuing this line have found democracy to be positively associated with capital account liberalization (see, for example, Quinn 2000 and Garrett and others 2000). Democracy may be a mechanism for resolving social conflicts that otherwise force resort to financial repression and the inflation tax (Garrett and others 2000). More generally, with democracy comes an increasing recognition of rights, including the international economic rights of

18. Moreover, by facilitating the use of rate ceilings and other administrative measures that cap interest rates, controls limit the cost of borrowing for those at the head of the financial queue, including the government and any private sector borrowers that it favors.

19. Epstein and Schor (1992) find that left-wing governments are more likely to maintain controls. While Garrett and others (2000) also conclude that left-wing governments are more likely to resort to controls, the effect is statistically insignificant at standard confidence levels. Only when high-income countries are removed from the sample is the association robust. While Quinn and Inclán (1997) also find some evidence that left-wing governments are more likely to retain controls, this effect is much more pronounced for the 1960s and 1970s than the 1980s. Alesina and others (1994) reach even more negative conclusions: They find little discernible effect of ideological orientation either before or during the 1980s after controlling for other characteristics of governments—coalition or majoritarian, cabinet durability and turnover—that plausibly reflect the time horizon of the government and therefore its propensity to put off tax increases to another day in favor of the inflation tax.
residents, and a greater ability to press for the removal of restrictions on their investment options (Dailami 2000).

Several recent studies (Simmons and Elkins 2000; Garrett and others 2000) suggest that “policy contagion” affects the decision to open the capital account. Countries are more likely to liberalize when members of their peer group have done so, holding constant other factors. The pattern can be interpreted as policy emulation (governments are influenced by the initiatives of their neighbors) or signaling (when competitors have liberalized portfolio flows, it becomes harder to retain controls and, at the same time, remain an attractive destination for foreign direct investment).

But are such interpretations justified? It is a common problem in the literature on contagion, financial and otherwise, that the simultaneity of policy initiatives in different countries may reflect not the direct influence of events in one country on another countries but a tendency for decisionmakers to respond similarly to economic and political events not adequately controlled for in the analysis. Simmons and Elkins (2000) address this possibility by defining a country’s economic neighbors as those that compete with it for foreign investment (in the case of capital account restrictions) and those that compete with it in export markets (in the case of current account restrictions). These more sophisticated proxies for policy contagion matter even when crude measures of common omitted factors (such as the share of countries in the same region that have liberalized their capital accounts) are also included in the specification.

These findings go a good way toward explaining the recent trend toward capital account liberalization. Financial repression has given way to deregulation of domestic financial institutions and markets in a growing number of countries. Governments and central banks have abandoned currency pegs in favor of greater exchange rate flexibility. The 1980s and 1990s were decades of democratization in much of the developing world. As these developments led some countries to liberalize, the trend gathered momentum, as the literature on policy contagion suggests. Together these forces lent considerable impetus to the process of capital account liberalization.

Before researchers congratulate themselves for their success and close up shop, it is worth noting other explanations that have been denied the same systematic attention. For example, capital controls may have become less attractive because information and communications technologies have grown more sophisticated, rendering controls more porous and their effective application more distortionary (Eichengreen and others 1998). The technical progress in question is hard to measure. A time trend intended to capture secular improvements in information...
and communications technologies would be contaminated by a variety of other omitted factors that were also changing over time. As is the case all too often in empirical economics, there may have been a tendency to focus on factors that are readily measured and quantified to the neglect of those that are more difficult to capture.

IV. CAPITAL MOBILITY AND GROWTH

The most widely cited study of the correlation of capital account liberalization with growth is Rodrik (1998). Using data for roughly 100 industrial and developing countries for 1975–89, Rodrik regresses the growth of GDP per capita on the share of years when the capital account was free of restriction (as measured by the binary indicator constructed by the IMF), controlling for determinants suggested by the empirical growth literature (initial income per capita, secondary school enrollment, quality of government, and regional dummy variables for East Asia, Latin America, and Sub-Saharan Africa). He finds no association between capital account openness and growth and questions whether capital flows favor economic development.

Given the currency of this article among economists, it is striking that the leading study of the question in political science reaches the opposite conclusion. For 66 countries over the period 1960–89, Quinn (1997) reports a positive correlation between the change in his capital account openness indicator and growth. That correlation is robust and statistically significant at standard confidence levels.

What explains the contrast is not clear. One difference that may matter is that Quinn’s study starts earlier. Consequently, growth in his sample period is not dominated to the same extent by the “lost decade” of the 1980s (when there were virtually no capital flows to emerging markets to stimulate growth). With an earlier start, his sample may include more observations in which countries liberalized inflows of foreign direct investment, with positive effects on growth, and fewer in which they liberalized short-term portfolio flows, whose effects may have been more mixed. In addition, Quinn has more independent variables, and he looks at the change in capital account openness rather than the level. Edwards (2001) emphasizes that Quinn’s measure of capital account liberalization is more nuanced and presumably informative. For example, Quinn’s measure conveys information about whether capital account opening was partial or across the board, whereas the standard IMF measure does not.22

Quinn’s country sample is also different, in that he considers fewer low-income developing economies. There are reasons to think that the effects of capital account liberalization vary with financial and institutional development. Remov-

22. I return to the distinction between partial and comprehensive capital account openness and restrictions in section VI on crises.
ing capital controls may be welfare and efficiency enhancing only when there are no serious imperfections in the information and contracting environment, an implication of the theory of the second best, as noted at the beginning of this article. Portfolio capital inflows stimulate growth, this argument goes, only when markets have developed enough to allocate finance efficiently and when the contracting environment forces agents to live with the consequences of their investment decisions. The Asian crisis encouraged the belief that countries that open their economies to international financial transactions benefit only if they first strengthen their markets and institutions. Thus a positive impact on growth comes only if prudential supervision is first upgraded, the moral hazard created by too generous a financial safety net is limited, corporate governance and creditor rights are strengthened, and transparent auditing and accounting standards and equitable bankruptcy and insolvency procedures are adopted.

Although these institutional prerequisites are difficult to measure, there is a presumption that they are most advanced in high-income countries. Edwards (2001) supports this view: Using Quinn's measure of the intensity of capital account restrictions, he finds that liberalization boosts growth in high-income countries but slows it in low-income countries. He shows further that the significance of capital controls evaporates when the IMF index used by Rodrik is substituted for Quinn's more differentiated measure. Thus it is tempting to think that the absence of an effect in earlier studies is a statistical artifact. And there is some suggestion that capital account liberalization is more beneficial in more financially and institutionally developed economies.

But do these apparent differences between high- and low-income countries really reflect their different stages of financial and institutional development? Kraay (1998) attempts to directly test the hypothesis that the effects of capital account liberalization depend on the strength of the financial system, the effectiveness of prudential supervision and regulation, and the quality of other policies and institutions. The results are not encouraging: the interaction of the quality of policy and institutions with financial openness is almost never posi-

23. Quinn's measure of capital account openness enters negatively, in other words, whereas the interaction between capital account openness and per capita income enters positively.

24. Using a different methodology, Quinn (2000) reaches a similar conclusion. He estimates bivariate vector autoregressions using growth rates and his measures of capital account liberalization individually for a large number of middle- and low-income countries. He finds scant evidence that capital account liberalization has had a positive impact on growth in the poorest countries, but some positive evidence for middle-income countries, especially those that have other characteristics likely to render them attractive to foreign investors.

25. Kraay uses the ratio of M2 to GDP and the ratio of domestic credit to the private sector relative to GDP as ex ante proxies for the level of financial development, and one minus the average number of banking crises per year as an ex post indicator of financial strength. As an indicator of the strength of bank regulation, he uses a measure based on whether banks are authorized to engage in nontraditional activities, such as securities dealing and insurance. To capture the broader policy and institutional environment, he uses a weighted average of fiscal deficits and inflation, the black-market premium, and indices of corruption and the quality of bureaucracy.
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tive and significant, and it is sometimes significantly negative.\textsuperscript{26} Arteta and others (2001) similarly interact the level of capital account openness with the liquid liabilities of the financial system as a measure of financial depth and with International Country Risk Guide's index of law and order as a measure of institutional development. Again, the results are largely negative. There is little evidence that the growth effects of capital account openness are shaped in robust and predictable ways by a country's level of financial and institutional development.

More important for shaping the effects of capital account liberalization, these authors suggest, is the sequencing of reforms. Countries that first complete the process of macroeconomic stabilization, allowing them to remove exchange controls and other distortions on the current account side, enjoy stronger growth effects of capital account openness. While some of the qualitative literature similarly suggests that sequencing is an important determinant of the effects of capital account opening, systematic cross-country empirical analysis has barely begun. (In other words, there do not appear to be other "large-n" studies like that of Arteta and others 2001 that address this question.)

One way of unraveling the mystery of why the growth effects of capital account liberalization do not seem to vary as expected with institutional and financial development is to determine whether these results are sensitive to the measures of policies and institutions used. Here, it will be evident, work is already under way. Another way is to pin down the mechanisms or channels through which capital account liberalization affects the economy, the approach examined next.

V. Channels Linking Capital Account Liberalization with Growth

The cross-country growth literature points to a number of factors that plausibly intermediate between capital account liberalization and growth. Investment, financial development, and the stability of macroeconomic policy, among other variables, have been shown to be positively related to an economy's rate of growth (see, for example, Levine and Renelt 1992; Levine 1997; Barro 1997). All of these variables create channels through which capital account liberalization can potentially exercise an effect. Studying the impact of capital account policy on these intermediate variables is thus a way of inferring its implications for growth. This section focuses on the impact of capital account policies on two of the channels that have received the most attention: investment and the depth and development of financial markets.

Many attempts have been made to analyze the connections between capital account policies and investment. Rodrik (1998) relates the investment to GDP

\textsuperscript{26} Note that the test here is for whether the effects of capital account openness are conditional on these measures of institutional development. These measures are not simply used as additional controls in the growth equation; rather, they are entered interactively.
ratio to the IMF’s measure of capital account openness, again finding no trace of an effect. Kraay (1998) similarly finds no impact on gross domestic investment as a share of GDP, using the IMF index, the Quinn index, and gross inflows and outflows as alternative measures of financial openness. He considers the possibility that capital account openness positively affects investment only in countries where risk-adjusted returns exceed the world average—that is, where liberalization will cause capital to flow in rather than out. Using the average balance on the financial account of the balance of payments as a proxy for risk-adjusted returns, he reports a positive impact on investment when this variable is interacted with capital account openness. However, the coefficient in question differs significantly from zero for only one of Kraay’s three measures of capital account openness.27

Because the evidence on investment does not speak clearly, it is logical to strip off another layer and consider variables like real interest rates and financial depth—factors on which investment plausibly depends. Governments have used capital controls in support of administrative measures designed to keep interest rates low with the express purpose of stimulating investment. And a substantial number of studies have confirmed that capital controls are associated with lower real interest rates (see, for example, Alesina and others 1994; Grilli and Milesi-Ferretti 1995; Bordo and Eichengreen 1998; Wyplosz 1999). But whether there are benefits for growth is a separate question. The literature on financial repression—especially the recent literature—is skeptical that interest rate ceilings, even if they reduce the cost of investment, succeed in nurturing growth. Although artificially low real rates reduce the required return on investment, they impede financial development. And financial development presumably increases the efficiency of investment as well as financing and otherwise facilitating experimentation with new technologies.28

Klein and Olivei (1999) find that capital account openness stimulates financial depth (measured variously as the change in the ratios of liquid liabilities to GDP, claims on the nonfinancial private sector, and bank domestic assets in deposit money to the sum of bank domestic assets in deposit money and central bank domestic assets). But the correlation between capital account openness and financial deepening is limited to the OECD countries; the relationship dissolves

27. The measure in question is actual (gross) inflows and outflows. Because the interaction term is then gross inflows and outflows times net inflows and outflows, one suspects that it is dominated by cases where investment reacted to exceptional surges of capital inflows. In addition one worries about the near-tautological nature of using a variable that essentially captures whether or not capital flowed in as a way of determining whether the policy affected investment. Kraay’s findings also appear to be sensitive to the estimator used and the sample period: He obtains different results depending on whether he estimates his investment equation by ordinary least squares or instruments his measures of capital account restrictions to control for their endogeneity.

28. The literature on the link between financial development and growth is vast—even more so than that on the topic surveyed here. Attempting to review the controversies and contributions would not be realistic. The reader may refer to Levine (1997) for a full-scale review of the topic.
when these countries are excluded from the sample. Thus where researchers like Kraay (1998) and Arteta and others (2001) find little evidence that an open capital account does more to stimulate growth in high-income countries, Klein and Olivei conclude that it may do more in the advanced industrial countries to stimulate certain inputs into growth—specifically, well-developed financial markets. That the effect is indirect (an open capital account encourages financial development, which in turn encourages growth) and contingent (presumably) on a range of intervening factors may be why it has been so difficult to document a direct link from the capital account to growth that varies between high- and low-income countries.

But not all investigators agree that the influence of capital account liberalization on financial development is limited to high-income countries. Levine and Zervos (1998) find for 16 developing economies that stock markets become larger and more liquid after the capital account is opened. To be sure, this study focuses on a different aspect of financial development, namely, stock markets rather than bank intermediation. But why the evidence for different financial markets is apparently contradictory is not clear. It could be that Levine and Zervos’s 16 countries, selected for having functioning stock markets, were already relatively advanced financially, so that capital account liberalization could then have a positive and powerful impact on their further deepening and development. Alternatively, it could be that banking systems typically are already relatively well developed when capital accounts are opened, so that the main effect of liberalization is on stock markets whose development is still at an earlier stage.

Sorting through this controversy may require more sophisticated measures of capital account liberalization. Whether liberalization favors the development of banks or securities markets plausibly depends on how liberalization proceeds—on whether restrictions on offshore borrowing by banks are relaxed first, as in the Republic of Korea, or measures limiting foreign investment in domestic securities markets are eased instead, as in Malaysia. Implementing such distinctions will also require measures of the development of the information and contracting environment, because asymmetric information and poor contract enforcement are thought to favor banks over securities markets.30

Another set of studies builds on the observation that controls are disproportionately used by countries with chronic macroeconomic imbalances (see, for example, Alesina and others 1994; Grilli and Milesi-Ferretti 1995; Wyplosz 1999; Garrett 1995, 1998, 2000). The motivation is presumably to limit capital flight and contain the threat from these imbalances for the stability of financial markets.31 By now it will be clear that more than a few studies advancing such conclusions have identification problems. Whereas countries suffering from chronic

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29. Edwards (2001) is an exception in this regard, as noted above.

30. The argument being that banks are in the business of internalizing transactions that cannot take place at arm’s length due to such market imperfections (Baskin and Miranti 1997).

31. Along with the seigniorage-related rationale reviewed previously.
Macroeconomic imbalances are more likely to resort to controls, governments and central banks enjoying the additional policy autonomy that controls confer may indulge in more expansionary policies. That few studies have addressed this identification problem may reflect the difficulty of finding plausible instruments for the endogenous variables.

One response by those concerned with the impact of controls on the public finances has been to move from the budget balance to its components (the expenditure and tax sides and different categories of taxes and spending), where the causality running from controls to budgetary outcomes is presumably easier to identify. Garrett and Mitchell (2000) find that public spending is lower when the capital account is open, which they interpret as capital mobility applying fiscal discipline. Garrett (2000) finds that this effect is specific to the exchange rate regime: Governments come under less pressure to limit spending when the exchange rate is allowed to float, but the combination of fixed rates and an open capital account has a strong disciplining effect.

A particular mystery is the impact of capital account liberalization on taxes on profits and other returns to capital. The idea that capital account liberalization, which increases the effective elasticity of supply of capital, should put downward pressure on the rate of capital taxation is one of the most fundamental corollaries of the theory of public finance. But the evidence to this effect is surprisingly weak. Quinn (1997), Swank (1998), Garrett (2000), and Garrett and Mitchell (2000) find that rates of capital taxation are unchanged or even higher in countries with open capital accounts. Because most countries with open capital accounts are relatively high-income, it may simply be that they have large public sectors (by Wagner’s Law) and high tax rates. But Quinn, Swank, Garrett, and others go to considerable lengths to control for income and other country characteristics that may independently influence the level of capital taxation, and none of their extensions makes this finding go away. Clearly, this is a puzzle requiring further study.

Finally, a number of researchers, motivated by the association of short-term foreign debt with crises and, in particular, by the perception that debt runs played a role in many episodes of serious turbulence in emerging markets in recent years, have asked whether controls can be used to lengthen the maturity structure of foreign obligations. Using data for a cross-section of countries, Montiel and Reinhart (1999) find that controls reduce the share of portfolio and short-term capital flows in total inflows, while increasing the share of foreign direct invest-

32. Quinn (1997) reports a positive association between public spending and capital account liberalization but concludes that the correlation is not robust.

33. On the association of short-term debt with crises, see Rodrik and Velasco (1999). Readers whose sensitivities will have been heightened by the preceding discussion to the causality problems arising in other contexts will not be surprised that the same issue arises here. Rather than short-term debt causing crises, in other words, it has been argued that anticipations of crises leads to a shortening of the maturity structure of the debt.
ment and leaving the overall volume of capital inflows unchanged. This general-
izes the findings of detailed studies for Chile, many of which conclude that its
holding period tax on capital inflows reduced the volume of short-term inflows
but in a way that was fully compensated for by increased long-term flows. (In
other words, only the maturity structure and not the level of the flows was af-
fected by these controls.34)

Controls like Chile’s, with the potential to reduce the risk of currency and
financial crises, have their advocates in the scholarly and policymaking com-
munities. But is this advocacy justified? Answering this question requires determin-
ating whether controls in fact reduce crisis risk.

VI. CRISES AND LIBERALIZATION OF THE CAPITAL ACCOUNT

The currency and banking crises of the 1990s did much to encourage the belief
that capital account liberalization raises the risk of financial instability. The relax-
ation of capital controls in Europe following the implementation of the Single
European Act made the realignment of currencies participating in Europe’s Ex-
change Rate Mechanism more difficult, allowing competitiveness problems to build
up, exposing governments and central banks to speculative pressures, and culmi-
nating in the crisis of 1992 (Eichengreen and Wyplosz 1993). Capital account lib-
eralization was implicated in Asia’s crisis insofar as the selective opening of capi-
tal accounts allowed banks to respond to the moral hazard created by government
guarantees and to lever up their bets (Furman and Stiglitz 1998). China’s suc-
cess in insulating itself from this instability by the use of capital controls is widely
seen as the exception that proves the rule.35 These assertions are controversial;
scholars continue to debate the causes of the European and Asian crises and the
role of capital flows. But it is curious, given the intensity of the debate, how few
cross-country studies have sought to systematically weigh the evidence.

One reason may be that problems of reverse causality are severe in this con-
text. Countries experiencing financial turbulence may impose or reinforce con-
trols, as did Malaysia following the outbreak of the Asian crisis. Or they may
relax their controls in an effort to restore investor confidence, as did Thailand
in January 1998 and the Republic of Korea several months later. The absence of
controls may or may not heighten crisis risk, but the fact that crisis risk some-
times prompts changes in the capital account regime makes it hard to distinguish
cause from effect.

34. Studies that reach this conclusion include Soto (1997), De Gregorio and others (1998), and

35. China restricted borrowing by Chinese entities, restricted portfolio outflows by Chinese citi-
zens and inflows by foreigners, and banned futures trading in yuan. While cautioning that controls
were probably only one of several factors making for the resiliency of the Chinese economy, Fernald
and Babson (1999) conclude that without a freely accessible onshore futures market, speculation against
the yuan would be difficult and that controls on outflows make it harder for Chinese investors to con-
vert their yuan if they expect the currency to weaken.
In fact, contrary to the intuition described at the beginning of this section, the cross-country evidence generally suggests that controls *heighten* currency crisis risk. Glick and Huchinson (2000) combine data on the presence or absence of controls at the end of one year (from the IMF's *Exchange Arrangements and Exchange Restrictions*) with data on the occurrence of currency crises in the next. In both bivariate and multivariate analyses they find a *positive* correlation between capital controls and crises. Leblang (2000) uses the narrative accounts in *Exchange Arrangements and Exchange Restrictions* to code changes in capital controls monthly and finds that controls are associated with an increased probability of currency crises. He also finds evidence that controls influence the likelihood that governments and central banks will successfully defend the currency against attack.

An interpretation, following Bertolini and Drazen (1997a, b) and Drazen (1997), is that countries maintaining or imposing controls send a negative signal to the markets. Investors may suspect a country that resorts to controls of reluctance to commit to the rigorous course of fiscal and monetary treatment to maintain stability. They may worry that a government inclined to resort to controls will be particularly willing to compromise investor rights. The signal may incite investors to flee and, if the control regime is less than watertight, enable them to do just that.

But have these researchers identified the direction of causality? If governments impose controls in anticipation of looming financial problems, then timing cannot identify the direction of causality. And, even more than in other contexts, there is reason to question the conclusions of an analysis that lumps all controls together. Controls of different intensity may not be equally effective in containing threats to currency stability, and different *types* of controls and different *forms* of liberalization may have different implications for financial stability. Liberalizing banks' access to offshore funding but not also permitting foreign access to domestic equity and bond markets may be more destabilizing than doing the reverse; it may cause foreign funds to flow in through the banking system, the weakest link in the financial chain. This is a common conclusion drawn from the crisis in Korea, which liberalized offshore bank funding before permitting foreign access to its securities markets. Even if inflow controls can reduce crisis risk by preventing banks and firms from becoming excessively dependent on short-term foreign debt, outflow controls,

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36. For example, Thailand introduced partial controls in May 1997, prior to its crisis, and later extended their coverage several times: in June, July, and September 1997 and January 1998. That Glick and Hutchinson relate the presence or absence of controls in one year to crises in the next may convince some readers that they have finessed this problem; surely controls imposed fully a year before a crisis are not the response of the authorities to subsequent difficulties. In fact, however, Glick and Hutchinson relate the presence or absence of controls *at the end of year* t to the presence or absence of a crisis *any time* in year *t* + 1, so that the time between the observation of controls and the occurrence of a crisis is *at most* a year—and in practice can be considerably less.
except of the most draconian sort, may be incapable of restraining capital flight if panic breaks out.\(^{37}\)

In addition, different controls may send different signals. Inflow controls like Chile's can be justified as prudential measures—a way of reinforcing regulations designed to stabilize the financial system (Eichengreen and others 1998). They may then be perceived as a signal that the authorities take seriously their commitment to currency and banking stability. Outflow controls, in contrast, may suggest only that the authorities are desperate. Using data for 15 developing countries, Rossi (1999) finds that outflow controls heighten the risk of currency crises but that inflow controls reduce it. Outflow controls similarly are associated with an increased risk of banking crises, whereas inflow controls have no discernible effect.

VII. FROM RESEARCH TO POLICY AND FROM POLICY TO RESEARCH

Turning from research to policy, one finds greater consensus on the lessons of international experience. That the G7 countries all have open capital accounts is regarded as telling. For those who emphasize this fact, capital account liberalization is just another manifestation of the policies of financial deregulation that countries adopt as they develop economically and institutionally, and specifically as they acquire the capacity to operate market-led financial systems. In other words, the relaxation of statutory restrictions on international financial transactions and the growth of cross-border financial flows reflect the same forces that encourage the removal of repressive domestic financial regulations and facilitate reliance on domestic financial markets to guide the allocation of resources.

The same arguments suggesting that domestic financial deepening and development enhance the efficiency of investment, facilitate experimentation with new technologies, and encourage growth and efficiency generally similarly support the presumption that international portfolio diversification and cross-border portfolio investment should encourage efficiency and growth. Capital account liberalization can be counterproductive, to be sure, if it takes place before severe policy-related distortions have been removed and before domestic markets, institutions, and the administrative capacity of the prudential authorities have developed enough to generate confidence that foreign finance will be channeled in productive directions. This qualification may be too frequently neglected—as the unconditional advocacy of capital account liberalization heard in the mid-1990s and the Asian crisis that quickly followed remind us to our chagrin—but this caveat, too, is now an integral part of the conventional wisdom.

But if caveats like this one complicate the journey, the destination, from all appearances, remains the same. Officials and their advisers may differ on precisely when and how to liberalize international financial transactions so as to

\(^{37}\) The fact that outflow controls tend to be the dominant variety in crisis-prone countries may therefore be another part of the explanation for why previous cross-country studies have found a positive association between controls and crisis incidence.
best ensure that capital inflows are channeled in productive directions, in other words, but there is little support for refusing to liberalize or (Malaysia in 1998-99 notwithstanding) for reversing previous liberalization measures. International financial liberalization, to paraphrase Marx, may be just another instance of the more developed economies showing their less developed counterparts an image of their future.

Given the breadth of support commanded by this synthesis, the lack of empirical substantiation of its fundamental tenets is worrisome indeed. If the evidence is really not there, then it is high time to rethink the conventional wisdom. With these stakes, priority should be attached to research with immediate promise for solving the key empirical puzzles. Empiricists need to better distinguish among controls—between inflows and outflows and between transactions involving banks and those involving securities markets. They need to develop more informative measures of the legal, contracting, and information environments that plausibly shape the effects of capital account liberalization. They also need to construct better indicators of the other policy initiatives with which capital account liberalization is sequenced.

These extensions can be undertaken in the context of existing macro-oriented cross-country research. Admittedly, operationalizing them presumes a not inconsiderable investment in data, constructed in ways that are consistent across countries and over time. The call for more and better data is standard fare in surveys like this one; here, however, is a case where it warrants its place of prominence.

But could it be that the problem is with the framework and not with the data and methods used to operationalize it? The literature on capital account liberalization has been written by macroeconomists, for macroeconomists, with an emphasis on the macroeconomics of growth and crisis. Perhaps the microeconomic level offers more definitive evidence of the effects of capital account policies. A growing body of firm-level evidence and analysis, surveyed by Karoly (1998) and Stulz (1999), suggests that this may be the case. Some examples illustrate the kinds of questions asked and answers found. Tandon (1994) shows a reduction in the required rate of return on equity for firms offering bonds on international markets. Smith and Sofianos (1997) show that firms listing abroad experience an increase in trading volume, consistent with the argument that financial integration leads to greater liquidity and hence a lower cost of capital. Lins and others (2000) show that firms from emerging markets listing in the United States are able to relax capital constraints—that is, the cash-flow sensitivity of their investment declines—but no such change is evident for firms from industrial countries, where capital constraints are presumably less.

More remains to be learned from a microeconomic perspective. That said, answering the big questions like how growth and crises are affected by capital account liberalization will ultimately require mapping the findings of microeconomic studies back into the macroeconomic framework adopted by the researchers whose work was reviewed in this survey.
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Where Has All the Education Gone?

Lant Pritchett

Cross-national data show no association between increases in human capital attributable to the rising educational attainment of the labor force and the rate of growth of output per worker. This implies that the association of educational capital growth with conventional measures of total factor production is large, strongly statistically significant, and negative. These are “on average” results, derived from imposing a constant coefficient. However, the development impact of education varied widely across countries and has fallen short of expectations for three possible reasons. First, the institutional/governance environment could have been sufficiently perverse that the accumulation of educational capital lowered economic growth. Second, marginal returns to education could have fallen rapidly as the supply of educated labor expanded while demand remained stagnant. Third, educational quality could have been so low that years of schooling created no human capital. The extent and mix of these three phenomena vary from country to country in explaining the actual economic impact of education, or the lack thereof.

To be a successful pirate one needs to know a great deal about naval warfare, the trade routes of commercial shipping; the armament, rigging, and crew size of potential victims; and the market for booty.

To be a successful chemical manufacturer in early twentieth century United States required knowledge of chemistry, potential uses of chemicals in different intermediate and final products, markets, and problems of large scale organization.

If the basic institutional framework makes income redistribution (piracy) the preferred economic opportunity, we can expect a very different development of knowledge and skills than a productivity-increasing (a twentieth century chemical manufacturer) economic opportunity would entail. The incentives that are built into the institutional framework play the decisive role in shaping the kinds of skills and knowledge that pay off.

—Douglass North (1990)

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People with more education have higher wages. This is probably the second (after Engel's law) most well-established fact in economics. It would seem to follow naturally that if more individuals are educated, average income should rise; if there are positive externalities to education, average income should rise by even more than the sum of the individual effects. The belief that expanding education promotes economic growth has been a fundamental tenet of development strategy for at least 40 years. The post–World War II period has seen a rapid, historically unprecedented expansion in educational enrollments. Since 1960, average developing country (gross) primary enrollments have risen from 66 to 100 percent, and (gross) secondary enrollments from 14 to 40 percent.

How has this experiment in massive educational expansion turned out? Is there now strong evidence of the growth-promoting externalities to education? This is an area where growth theory and empirical estimates are potentially important. Positive externalities should mean that the impact of education on aggregate output is greater than the aggregation of the individual impacts. To test for externalities, we need macroeconomic and microeconomic models of education's impacts that are consistent. The augmented Solow model is just such a model because it predicts that the "no externality" impact of education should be the share of educational capital in factor income. This impact can be estimated from microeconomic evidence on the wage increments to capital. Within the augmented Solow model, the estimated growth impact of education is consistently less than would be expected (rather than more) from the individual impacts. The cross-national data suggests negative externalities and present something of a "micro-macro" paradox.

The path to resolving this paradox begins with an acknowledgment that the impact of education on growth has not been the same in all countries (Temple 1999). I discuss three possibilities for reconciling the macro and micro evidence and explaining the differences across countries. The first possibility is North's (1990) metaphorical piracy: Education has raised productivity, and there has been sufficient demand for this more productive educated labor to maintain or increase private returns, but the demand for educated labor comes, at least in part, from individually remunerative yet socially wasteful or counterproductive activities. In this case, the relative wage of each individual could rise with education (producing the micro evidence), even while increases in average education would cause aggregate output to stagnate or fall (producing the macro evidence). The second possibility is that expansion of the supply of educated labor when demand is stagnant could cause the rate of return to education to fall rapidly. In this case, the average Mincer returns (Mincer 1974) estimated in the 1960s and

1. The idea that either the "new" growth theory or the "neoclassical revival" has "discovered" the importance of human capital is belied by even a casual reading of Kuznets (1960), Lewis (1956), or Dennison (1967). Gunnar Myrdal's (1975) Asian Drama, written mostly in the late 1950s, already treats the importance of human capital along with physical capital in development as the conventional wisdom.
1970s overstated the actual marginal contribution to output from educational expansion in those instances where the demand for educated labor did not expand rapidly enough. Third, schooling quality may be so low that it does not raise cognitive skills or productivity. This could even be consistent with higher private wages if education serves as a signal to employers of some positive characteristics, such as ambition or innate ability.

I. Expansion of Education and Growth-Accounting Regressions

The first approach is to do what we would do if we did not know it was not going to work. That is, we will take the standard production function specifications of growth at the macroeconomic level, build aggregate measures of education capital from microeconomic data on education and its returns, and then examine the relationship between them.

How Much Should Education Matter? The Augmented Solow Model

Mankiw and others (1992) extend the Solow aggregate production function framework to include educational capital:

\[
Y_t = A(t) * K_t^{\alpha_k} * H_t^{\alpha_h} * L_t^{\alpha_l},
\]

assuming constant returns to scale \((\alpha_k + \alpha_h + \alpha_l = 1)\), normalizing by the labor force, and taking natural logs to produce a linear equation in levels. But this “linear in log levels” specification can also be expressed in rates of growth. Because estimation in levels raises numerous problems (to which I return below), I focus on the relationship among percent per annum growth of output per worker \((\dot{y} = \frac{d\ln (Y/L)}{dt})\), growth of physical capital per worker, and educational capital per worker:\(^2\)

\[
\dot{y} = \dot{a} + \alpha_k * \dot{k} + \alpha_h * \dot{h}.
\]

In the context of this model, \(\dot{a}\) is the growth rate of the growth-accounting residual—and I will reluctantly follow convention and call this total factor productivity (TFP), even though it is not (Pritchett 2000a).

\[
\dot{TFP} = \dot{y} - \alpha_k * \dot{k} - \alpha_h * \dot{h}.
\]

The extended Solow approach facilitates simple nonregression-based estimates of how much the expansion of educational capital “ought” to matter. Because

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2. Growth for each variable is calculated as the logarithmic least squares growth rate over the entire period for which the data are available. This makes the estimates of growth rates much less sensitive to the particular endpoints than if changes from the beginning period to the end period were calculated. This means the time period over which I calculate the growth rate does not always correspond exactly to the time period for the education data, but because both are per annum growth rates, this difference does not matter much.
the weights in the aggregate Cobb-Douglas production function represent the factor shares of national income, the coefficient on educational capital in a growth-accounting regression ought to be equal to the share of educational capital in gross domestic product (GDP) that can be estimated based on microeconomic data.

With constant returns to scale, labor share is one minus the physical capital share. A physical capital share of around 0.4 is somewhat high, but is consistent with a variety of evidence—the estimates from national accounts and from regression parameters—and with capital output ratios (if the capital-output ratio, $K/Y$, is 2.5 and the rate of return to capital is 16 percent, then the share of capital, $rK/Y$, is 40 percent). This implies a labor share of 0.6.

How much of the labor share is due to human (or educational) capital? One simple way of estimating the share of the wage bill attributable to human capital is to use the ratio of the unskilled—or “zero human capital”—wage, $w_0$, to the average wage, $w$:

$$ HUMAN \ \ CAPITAL \ \ SHARE \ \ (FROM \ \ WAGES) = 1 - \frac{w_0}{w}. $$

A calculation based on the distribution of wages in Latin America estimates a human capital share of wages of between 50 and 75 percent. Mankiw, Romet, and Weil (1992) use the historical ratio of average to minimum wages in the United States to estimate that half of wages are due to human capital. Either of these calculations suggests a human capital coefficient ($\alpha_h$) of at least 0.3.

Another approach to estimating the educational capital share is to assume a wage increment to education (taking the micro evidence discussed below at face value), and then use data on the fraction of the labor force in each educational attainment category to derive the educational capital share. Table 1 shows the results of two calculations. The top half shows the fraction of the labor force in various educational attainment categories in various regions. One can calculate the share of the wage bill due to educational attainment by assuming a wage premium for each attainment category and applying equation 5:

$$ EDUCATIONAL \ \ CAPITAL \ \ SHARE \ \ OF \ \ WAGES \ \ BILL = \frac{\sum_{i=1}^{K} (w_i - w_0) \gamma_i}{wL}, $$

where $i$ represents each of the seven educational attainment categories and $\gamma_i$ are the shares of the labor force in each educational attainment category.

3. Using data on the distribution of workers’ earnings (World Bank 1993a), we take the ratio of the average wages up to the 90th percentile (to exclude the effect of the very long tails of the earnings distribution) to the wage of those workers in either the 20th or 30th percentile (to proxy for the wage of a person with “no” human capital). The estimates of human capital share of the wage bill are 62 and 47 percent, respectively. If the top 10th percentile is included (and I take the ratio of average wages to the 20th or 30th percentile), the estimates of human capital share are even higher—74 and 63 percent, respectively. Although these are considerably higher that other estimates, they are estimates of all human capital, not just educational capital. In the United States, the ratio of the average to the minimum wage (taken as a proxy for the “unskilled” wage) has hovered around 2.
### Table 1. Share of Educational Capital in Wage Bill

<table>
<thead>
<tr>
<th>Wage premia by educational attainment under assumption set:</th>
<th>Share of work force by educational attainment, 1985 (percent except where noted)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Developing countries</td>
</tr>
<tr>
<td>No schooling</td>
<td>1.00</td>
</tr>
<tr>
<td>Some primary</td>
<td>1.40</td>
</tr>
<tr>
<td>Primary complete</td>
<td>1.97</td>
</tr>
<tr>
<td>Some secondary</td>
<td>2.77</td>
</tr>
<tr>
<td>Secondary</td>
<td>3.90</td>
</tr>
<tr>
<td>Some tertiary</td>
<td>5.47</td>
</tr>
<tr>
<td>Tertiary</td>
<td>7.69</td>
</tr>
<tr>
<td>Average years of schooling</td>
<td>3.56</td>
</tr>
</tbody>
</table>

Calculated share of wage bill due to educational capital across regions under each assumption \( t, \ldots, n \)

| Assumption set A | 36 | 26 | 43 | 30 | 62 |
| Assumption set B | 49 | 38 | 56 | 42 | 73 |

*(wage increment is constant at 10 percent)

*(wage increments are: primary 16 percent, secondary 12 percent, tertiary 8 percent)

Source: Data on educational attainment by region from Barro and Lee (1993).
Under assumption set A (constant wage increment of 10 percent per year of schooling), the educational share of the wage bill varies across regions, from 26.3 percent in Sub-Saharan Africa (SSA) to 62.1 percent in the Organisation for Economic Cooperation and Development (OECD); and it is 36.4 percent for developing countries as an aggregate. Under assumption set B (wage increments are proportionately higher for a year of primary than for a year of secondary, and higher for secondary than for tertiary, at 16, 12, and 8 percent, respectively), the share of educational capital in the total wage bill averages 49 percent—almost exactly half—for all developing economies, varying from 38 percent in SSA to 73 percent in OECD. Both methods suggest that the educational capital share of the wage bill should be between 0.35 and 0.7. Hence the growth-accounting regression coefficient on educational capital \( (\alpha_k) \) ought to be between 0.21 and 0.42—with 0.3 in the middle of the range.

Data and Specification for Physical and Educational Capital

Using two recently created cross-national time-series data sets, I create estimates of the growth rate of per worker educational capital. The two data sets use different methods to estimate the educational attainment of the labor force. Barro and Lee (1993) estimate the educational attainment of the population age 25 and above using census or labor force data where available and create a full panel of five yearly observations over the period 1960–85 for a large number of countries by filling in the missing data using enrollment rates. Nehru and others (1995) use a perpetual inventory method to cumulate enrollment rates into annual estimates of the stock of schooling of the labor force-aged population, creating annual observations for 1960–87.

From these estimates of years of schooling of the labor force, I create a measure of educational capital from the microeconomic specification of earnings used by Mincer (1974). I assume the natural log of the wage (or more generally, earnings per hour) is a linear function of the years of schooling:

\[
\ln(w_N) = \ln(w_0) + r \cdot N,
\]

where \( w_N \) is the wage with \( N \) years of schooling, \( N \) is the number of years of schooling, and \( r \) is the wage increment to a year’s schooling. The value of the stock of educational capital at any given time, \( t \), can then be defined as the discounted value of the wage premia due to education:

\[
HK(t) = \sum_{t}^{T} \delta^t \cdot (w_N - w_0),
\]

where \( w_0 \) is the wage of labor with no education. Substituting the formula for the educational wage premia (equation 6) into the definition of the stock (equation 7) and taking the natural log gives equation 8 for the log of the stock of educational capital, we get

\[
\ln(HK(t)) = \ln(\sum_{t=0}^{T} \delta^t) + \ln(w_0(t)) + \ln(e^{rN} - 1).
\]
Therefore, the proportional rate of growth of the stock of educational capital is approximately

$$hk(t) \approx \frac{d\ln(\exp^{RN(t)} - 1)}{dt}.$$ 

Based on existing surveys of the large number of micro studies, I calculate the growth of educational capital using equation 9, the data on years of schooling from either Barro and Lee (1993) or Nehru and others (1995), and an assumed $r$ of 10 percent constant across all years of schooling.

In addition to the measures of educational capital, I use two series created by a perpetual inventory accumulation of investment and an initial estimate of the “capital” stock, based on an estimate of the initial capital-output ratio (King and Levine 1994; Nehru and Dhareshwar 1993). As I have argued elsewhere, series constructed in this way cannot be treated as estimates of the physical capital stock relevant to the production function, because there is no underlying theoretical or empirical justification for doing so when governments are the main investors. Hence, they should be called by a purely descriptive acronym: cudie (cumulated, Depreciated Investment Effort) (Pritchett 2000a). The two CUDIE series are highly correlated and give similar results, with the principal difference being that King and Levine (1994) use investment data from the Penn World Tables, Mark 5 (PWT5; Summers and Heston 1991), while Nehru and Dhareshewar (1993) use investment data from the World Bank.

The dependent variable is growth of GDP per worker from PWT5. This is conceptually more appropriate in growth-accounting regressions than GDP per person or per labor force-aged person (but, as argued below, the findings are robust).

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4. There are two reasons this formula is only an approximation. First, the discount factor is assumed constant and hence is factored out in the time rate of change. It does depend on the average age of the labor force (because the discount is only until time $T$, retirement), which certainly varies systematically across countries, but I am assuming that changes in this quantity over time are small. The second, potentially more serious problem is that I dropped out the growth rate of $\ln(w(-))$—the evolution of the unskilled wage term. This means my growth rate of human capital is really that component of the growth of human capital due to changes in years of schooling. For instance, Mulligan and Sala-i-Martin (1997) estimate a human capital stock in which increases in unskilled wages reduce human capital; this is technically correct, but certainly counterintuitive.

5. A survey by Psacharopoulos (1993) shows wage increments by region: SSA 13.4 percent; Asia 9.6 percent; Europe, Middle East, and North Africa 8.2 percent; Latin America 12.4 percent; OECD 6.8 percent; and an unweighted average of 10.1. In any case, the cross-national differences in the growth rate of educational capital are very robust to variations in the value of $r$.

6. One confusion (among many) in this literature is between the wage increment and the rate of return to education. The often-repeated assertion that “returns are higher to primary schooling” (as reported, for example, by Psacharopolous [1993]) seems true not because the increment to wages from a year of primary school is higher than for other levels, but because the opportunity cost of a year of primary schooling is much lower. This is due to the fact that the typical forgone wage attributed to a primary-age unschooled child is very low (Bennell 1996). What is relevant to growth accounting is the increment to wages, not the cost-inclusive return.

7. This output variable does raise one problem. My estimates of human capital are based on estimates of the educational capital of the labor force-aged population, whereas my output is output per estimated labor force (although not corrected for unemployment), so that systematic differences in the
Regression Results for Growth and TFP

The results for estimating the growth-accounting equation (2) for the entire sample of countries are reported in column 1 of Table 2. The partial scatter plot is displayed as figure 1. The estimates for cumulated physical investment (CUDIE) correspond reasonably well to national accounts-based estimates of the capital share (although 0.52 is somewhat on the high side) and are strongly significant \( t = 12.8 \). Very much on the other hand, the estimate of the impact of growth in educational capital on growth of per worker GDP is negative \(-0.049\) and insignificant \( t = 1.07 \). Adding the initial level of GDP per worker (column 2) has no impact on the negative estimates of the effect of education \(-0.038\).

Columns 8 and 9 of table 2 show the results of regressing TFP growth on the growth of physical CUDIE and educational capital. In column 9, the assumed factor shares used in creating TFP are 0.4 (physical) and 0.3 (educational). The growth of educational capital shows a large, statistically very significant \( t = 6.91 \) and negative \(-0.338\) effect on TFP growth. In column 10, I make the educational capital share as small as is consistent with growth accounting by assuming the physical capital share is on the high side (0.5) and the share of educational capital in the wage bill is on the low side (0.33), so that the educational capital share is as low as it can reasonably be \((1/2 \times 1/3 = 0.167)\). It is still the case that educational capital accumulation is strongly statistically significant and negatively related to TFP growth. Of course, except for fixing the physical capital share, this TFP regression is equivalent to a t-test that finds the estimated human capital share equal to 0.167. Using the results of column 1, this hypothesis is easily rejected \( t = [-0.049 - 0.167] / 0.046 = 4.72 \).

These TFP results are a simple arithmetic trick, but this trick is useful because it changes a typically uninteresting “failure to reject” to a convincing rejection of an interesting and policy-relevant hypothesis. The findings are not a “low-powered” failure to reject zero—they are a “high-powered” failure to reject, because although the data do not reject zero, they do in fact reject a wide range of interesting hypotheses—including the hypothesis that the growth impact is as large as the microeconomic data would suggest. After all, the primary reason to use aggregate data to estimate the impact of schooling is to find out whether the evolution of the labor force versus the labor force-aged population (say, through differential female labor force participation) could affect the results. The question of whether or not changes in female labor force participation (cross-national level differences would not affect the results) are an important part of the story is beyond the scope of this article. With the currently available gender-disaggregated data, this is an active research question, with some arguing that female education is more important for growth, and others arguing that it is less important, than male schooling.

8. Four countries have been dropped from all regressions because of obvious data problems: Kuwait, because PWT5 GDP data are bizarre; Gabon, because labor force data (larger than the population) are clearly wrong; Ireland, because the Nehru and others (1995) data report an average of 16 years of schooling (immigration has distorted these numbers); and Norway, because Barro and Lee (1993) report an impossible increase of 5 years in schooling over a period of 5 years.
<table>
<thead>
<tr>
<th>Dependent variable</th>
<th>1</th>
<th>2</th>
<th>3</th>
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<th>5</th>
<th>6</th>
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<td>GDPPW)</td>
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<td>[1995] edu.</td>
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<td>with test</td>
<td>whole</td>
<td>αₖ = 0.4,</td>
<td>αₖ = 0.5,</td>
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<td></td>
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<td>capital data)</td>
<td></td>
<td>scores)</td>
<td>sample)</td>
<td>αₜ = 0.3,</td>
<td>αₜ = 0.167</td>
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<td>Growth of education capital per worker</td>
<td>-0.049</td>
<td>-0.038</td>
<td>-0.091</td>
<td>-0.120</td>
<td>-0.088</td>
<td>0.058</td>
<td>0.136</td>
<td>-0.338</td>
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<td></td>
<td>(1.07)</td>
<td>(0.795)</td>
<td>(1.61)</td>
<td>(1.42)</td>
<td>(0.593)</td>
<td>(0.229)</td>
<td>(1.97)</td>
<td>(6.91)</td>
<td>(4.19)</td>
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<tr>
<td>Growth of CUDIE per worker</td>
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<td>0.526</td>
<td>0.458</td>
<td>0.460</td>
<td>0.527</td>
<td>0.592</td>
<td>0.612</td>
<td>0.126</td>
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<td></td>
<td>(12.8)</td>
<td>(12.8)</td>
<td>(10.19)</td>
<td>(10.18)</td>
<td>(12.42)</td>
<td>(6.78)</td>
<td>(14.88)</td>
<td>(3.08)</td>
<td>(0.651)</td>
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<td>ln (initial GDP per worker)</td>
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<td>0.0009</td>
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<td>(0.625)</td>
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<td>(0.625)</td>
<td>(0.0625)</td>
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<td>Test score (normalized, mean = 1)</td>
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<td></td>
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<td>(1.31)</td>
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<tr>
<td>Test score * EK</td>
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<td>-0.485</td>
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<td></td>
<td>(1.27)</td>
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<td></td>
<td></td>
<td>(1.27)</td>
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</tr>
<tr>
<td>Number of countries</td>
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<td>91</td>
<td>70</td>
<td>70</td>
<td>77</td>
<td>25</td>
<td>96</td>
<td>91</td>
<td>91</td>
</tr>
<tr>
<td>R²</td>
<td>0.653</td>
<td>0.655</td>
<td>0.611</td>
<td></td>
<td></td>
<td>0.71</td>
<td>0.909</td>
<td>0.419</td>
<td>0.205</td>
</tr>
</tbody>
</table>

Note: *Except in column 7, which uses levels.
Source: Author's calculations.
impact is higher (or lower) than expected from the microeconomic data, and hence to provide some indication of the presence (or absence) of externalities. But to speak to this question, growth regressions using aggregate data must demonstrate not only that the educational capital coefficient is not zero but that it is higher than the value expected, given the microeconomic evidence applied to the same growth model. This is a seemingly modest standard, but one that has never been met.

Before proposing explanations of this apparent micro-macro paradox of negative externalities, I first show that this result is robust to sample, data, and technique and that it is not the result of "pure" measurement error or failure to account for school quality.9

The estimated coefficient is not the result of a peculiar sample or a few extreme or atypical observations. To ensure robustness against outliers, individual observations identified as influential were sequentially deleted up to 10 percent of the sample size, with no qualitative change in results.10 The negative

9. I do not show that the results are robust to the introduction of other covariates (Levine and Renelt 1992). This is because I am interested in growth accounting within a specific growth model that takes a production function approach. Thus there is no scope to introduce other covariates arbitrarily, as in the "reduced form" literature.

10. An observation is identified as influential based on the difference in the estimate with and without the observation included (Belsley and others 1980). Temple (1999) working on a different data set,
coefficient on schooling growth persists if (a) only developing countries are used, (b) all observations from SSA are excluded, or (c) regional dummies are included.

The results are also robust to variations in the data used for education, CUDIE, or GDP. All the regressions in table 2 were also estimated using Nehru and others' (1995) estimates of educational capital, and the educational capital coefficient estimates are similar: consistently negative. Changing the data on growth and using World Bank growth rates of GDP in constant prices in local currency instead of the PWT5 GDP data gives similar results. Using growth of GDP per person or per labor force-aged population produces an even larger negative estimate for education. Relaxing the assumption of constant returns to scale does not alter the negative estimate on educational capital. Using weighted least squares with either (log of) population, GDP per capita, or total GDP because the weights also gives nearly identical results.

The finding using level-on-level specifications of the augmented Solow equation in table 2, column 7 shows a coefficient of 0.13 \( (t = 1.97) \)—which continues to reject \( H_0 : \alpha_g = 0.3, t = 2.37 \). However, there are good reasons to believe level-on-level coefficients will be biased upward. If this educational capital coefficient is biased upward by as much as the CUDIE results appear to be (by about 0.1), then the small negative coefficient in the growth-on-growth regressions are consistent with the small positive coefficients in the level-on-level regressions.

Although both sets of educational attainment data have been roundly criticized on a number of legitimate grounds (Behrman and Rosenzweig 1993, 1994), I use two different instruments to show that this particular result on educational capital is not the result of pure measurement error in the estimates of years of schooling. Using the growth of Nehru and others' (1995) educational capital as an instrument for Barro and Lee's (1993) educational capital (the correlation of the two series' growth rates is 0.67), the coefficient becomes slightly more negative: -0.12 (column 4 of table 1) versus -0.091 for ordinary least squares (OLS) in the same sample (column 3). In addition, I also match each country with a similar country, usually picking the geographically closest neighbor, based on the idea that educational capital growth rates in similar countries are likely to be correlated (the actual correlation was \( \rho = 0.316 \)), whereas the pure measurement error in similar countries' reported enrollment and attainment rates is plausibly uncorrelated (and certainly less than perfectly correlated). This IV coefficient in table 2, column 5 is also negative (-0.088). Correcting for pure

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11. These are reported in Pritchett (1996), an earlier version of this article. In that paper, the basic ordinary least squares regression using the other data set was \( \hat{y} = c + 0.501k_{15,41} + 0.104_{12,07} + b \), \( N = 79 \), \( r^2 = 0.557 \) (t-statistics in parentheses).
measurement error makes the estimates more negative (which is to be expected, as measurement error produces attenuation bias), and hence only deepens the puzzle.\(^{12}\)

Recently, Krueger and Lindahl (2000) have criticized Benhabib and Spiegel (1994), based on the latter's older estimates of educational stocks. Krueger and Lindahl (2000) claim that Benhabib and Spiegel's (1994) findings are not robust to pure measurement error. However, this criticism is not relevant to the present article (for which much of the work was done several years before the Krueger and Lindahl paper) for three reasons. First, I use newer data sets, not the Kyriacou (1991) data used in Benhabib and Spiegel (1994). Second, my use of IV to correct for measurement error is exactly the same conceptual approach as Krueger and Lindahl's (2000), and I do not find that IV reverses any findings. Third, Krueger and Lindahl (2000) focus particularly on the measurement error of growth rates over short (e.g., five-year) periods, and argue, rightly, that measurement error is a larger concern in differenced data. In any case, the results in Krueger and Lindahl's (2000) table 5, column 5, which are the most similar to those presented here (in that they control for physical capital with an unconstrained coefficient and instrument for the education variable), find an empirically modest but statistically insignificant impact of schooling ($t = 0.41$). The bound of two standard deviations on Krueger and Lindahl's estimate of the aggregate equivalent of the Mincerian rate of return ranges from negative 44 percent to positive 67 percent. The major difference between our results is that I use the percentage rate of growth in the value of educational capital (which is essentially a logarithmic specification; see equations 6–9), whereas they use absolute change in the years of schooling.

A different, deeper notion of measurement error is that while the years of schooling are correctly measured, the true problem is that years of schooling do not reflect learning. However, while differences in educational quality can account for heterogeneity in the impact of schooling, it should not explain a low average impact. In fact, due to the “general underlying positive covariance between quantity and quality of schooling” (Schultz 1988), one would expect that excluding quality would bias the estimated return upward, as more schooling is accumulated where quality is high.\(^{13}\) For lack of quality adjustment to explain the results or quantities in the aggregate, there would have to be a very strong inverse cross-national relationship between quality and the expansion of quantity—a relationship for which there is no evidence.

The quality of schooling across countries is impossible to measure without internationally comparable test examinations of comparable groups of students,

\(^{12}\) Using instruments for physical CUDIE and educational capital simultaneously, to correct for measurement error in both has very little impact on the estimates of educational capital.

\(^{13}\) For instance, Behrman and Birdsall (1983) have shown, for Brazil, that not controlling for school quality leads to overestimating the impact of years of schooling by a factor of two.
and these, unfortunately, exist for very few countries.\textsuperscript{14} Hanushek and Kim (1995) use test score data to show that test score performance has a positive and statistically significant coefficient as an independent variable in a growth regression.\textsuperscript{15} However, in this case the interest is in the impact of an increase in educational capital, and the expected functional form when schooling quality matters would be an interactive effect: the impact of an additional unit of educational capital is higher when the quality of schooling is higher. I estimate this functional form using a single observation on test scores for each of the 25 countries used by Hanushek and Kim (1995), normalized to a mean of one, to interact with the growth of the educational capital stock. As shown in table 2, column 7, while the estimated impact of education is higher with higher quality (although the interactive coefficient is statistically insignificant), it is still the case that, evaluated at the average level of quality (test score = 1), the education impact is substantially less than zero (0.06 – 0.48 = –0.42). This suggests that, as expected, the lack of control for quality causes an upward bias, so the negative estimates that do not control for quality are not negative enough.

Relationship to Other Empirical Results on Schooling

As surprising as these negative results may seem, they are similar to what other researchers have found when they examined the relationship between education and growth using either growth-on-growth or level-on-level regressions. Benhabib and Spiegel (1994) and Spiegel (1994) use a standard growth-accounting framework that includes initial per capita income and estimates of years of schooling from Kyriacou (1990), and find a negative coefficient on growth of years of schooling.\textsuperscript{16} Lau and others (1991) estimate the effects of education by level of schooling (primary versus secondary) for five regions and find that primary education has an estimated negative effect in Africa and Middle East North Africa,

\textsuperscript{14} One possible way out of the lack of quality measures is to use proxies for quality. However, there is no particular reason to believe that physical indicators (such as teacher-to-pupil ratio or resources expended per student) will adequately proxy quality, and many reasons to believe they will not. Hanushek and Kim (1995) explore the connections between these indicators and test scores to extrapolate a quality when it is not available, but with little success. Because schooling is typically publicly provided, there is no reason to believe that dollars spent will be closely associated with output (that is, one cannot apply the usual theory about the relationship between inputs and outputs derived from production theory of profit maximizers). There is a huge amount of literature on the impact on achievement of various physical and financial measures of resources expended per student, with generally ambiguous results (see, for example, Filmer and Pritchett [1999]).

\textsuperscript{15} However, one could easily suspect that any variable—for example, test scores—on which countries such as Singapore (the highest, 72.1) and Hong Kong (71.8) do well and countries such as Nigeria (38.9) and Mozambique (27.9) do poorly, might well be capturing more in a growth regression than just labor force quality.

\textsuperscript{16} Spiegel (1994) shows that the finding of a negative effect of educational growth is robust to the inclusion of a wide variety of ancillary variables (e.g., dummies for SSA and Latin America, size of the middle class, political instability, share of machinery investment, inward orientation), and to the inclusion of samples.
insignificant effects in South Asia and Latin America, and positive and significant effects only in East Asia. Jovanovich and others (1992) use annual data on a different set of capital stocks and Nehru and others' (1995) education data and find negative coefficients on education in a non-OECD sample. Behrman (1987) and Dasgupta and Weale (1992) find that changes in adult literacy are not significantly correlated with changes in output. The World Bank's World Development Report on labor also reports the lack of a (partial) correlation between growth and education expansion (World Bank 1995, figure 2.4). Newer studies using panels to allow for country-specific effects consistently find negative signs on schooling variables (Islam 1995; Caselli and others 1996; Hoeffler 1999).17

Some very early studies used enrollment rates in growth regressions (Barro 1991; Mankiw and others 1992), but this approach had and has two deep problems. First, especially in Mankiw and others (1992), secondary enrollment rates alone were used—but without any clear or compelling reasoning as to why both primary and tertiary enrollment rates should have been excluded. Second, enrollment rates are a terrible proxy for growth in years of schooling.18 The assumption that current (or average) enrollment rates adequately proxy a country’s steady-state stock is true only if enrollment rates are constant over time across countries—but this is contradicted by the massive recent expansion of schooling in developing countries (Schultz 1988). The correlation between the growth of educational capital and secondary enrollment rates is -0.41. This is because the growth of educational attainment depends not on the current enrollment rate but on the difference in the enrollment rate between the cohort leaving the labor force and the cohort entering the labor force.19

17. However, these studies are susceptible to the Krueger and Lindahl (2000) critique about exacerbation of measurement error in short (five-year) panels. Moreover, the dynamic properties of the educational series, which tend to have little time series variation within countries, make it difficult to identify impacts of education in any case (Pritchett 2000b).

18. This does raise the question of why, if they are not a valid proxy for accumulation of schooling, initial secondary enrollment rates are a reasonably robust correlate of subsequent growth rates. My conjecture is the nature of “conditional convergence” regressions—that is, both the initial level of income and initial secondary enrollment rate are on the right-hand side of the equation with growth on the left-hand side. It is not unreasonable to assume that high secondary enrollment rates conditional on income level may signal something good about a country’s growth prospects (e.g., the government’s provision of good schools might mean it does other things well, the country has a substantial middle class, or people anticipate the country will do well; but it could also mean income is temporarily low), quite independent of the impact via accumulation of educational capital.

19. Comparing Korea and Great Britain provides a simple illustration. Korea’s secondary enrollment rate in 1960 was 27 percent, while Great Britain’s was 66 percent. But the level of schooling of Great Britain’s labor force in 1960 was 7.7 years, and the level of Korea’s was 3.2 years. Subsequently, Great Britain’s enrollment rate increased to 83 percent by 1975 and then remained relatively constant, whereas Korea’s enrollment rate increased from 27 to 87 percent by 1983. Given these differences in initial stocks and the large changes in enrollment rates, Korea’s average years of schooling expanded massively from 3.2 to 7.8 by 1985, but Great Britain’s expanded only modestly from 7.7 to 8.6, even though Great Britain’s enrollment rate was higher than Korea’s for most of the period.
Another section of the literature uses the initial level of the stock of education to explain growth of output per capita. Benhabib and Spiegel (1994) show that if the initial level of education is added to a growth-accounting regression, the initial level of education is positive, whereas the mildly negative impact of the growth of educational capital persists. This finding of a level effect is actually much more puzzling than is generally acknowledged, as the spillover effects of knowledge that might be captured by an effect of the level of education in the endogenous growth literature should be in addition to rather than instead of the usual direct productivity effects. Finding only a spillover impact is grossly inconsistent with the microdata: If the entire return to education at the aggregate level is spillover effects, then why is the wage premium observed at the individual level?

Moreover, a regression with growth rates on the left-hand side and level of education on the right-hand side is either misspecified or a complicated way of imposing parameter restrictions. The obvious fact that growth rates are stationary (without drift) while the stock of education is nonstationary and secularly increasing implies there cannot be a stable relationship between the growth of output and the level of education (Jones 1995). Growth regressions that include initial levels of both education and output are only justified if education levels (nonstationary) are cointegrated with levels of income (nonstationary). But in that case, this specification still begs the original question, because to fully implement the error correction model one must still estimate the cointegrating relationship.

II. Why (and Where) Has Schooling Contributed to Growth?

So there is an apparent micro-macro contradiction. The microeconomic evidence is commonly (if naively) taken to mean that substantial wage increments from additional schooling are nearly universal and that additional schooling will lead to growth. The macroeconomic data in an entirely standard growth accounting model suggest that education has not uniformly had the growth impact the microeconomic data would suggest. The obvious resolution is that the impact of education has varied widely across countries (Temple 1999). The question

20. Ben-David and Papell (1994) use Angus Maddison's historical data and find that growth rates are stationary after allowing for one structural break. This criticism applies to all endogenous growth models that make growth rates a function of any nonstationary variable (such as the magnitude of research and development or the stock of knowledge) while growth rates are stationary (Jones 1995).

21. Not surprisingly, the data, when unconstrained, do not say that schooling has contributed to output to exactly the same degree in Korea, Zaire, Paraguay, and Hungary. Parameter homogeneity does not change the fact that the unconstrained estimates are well below the expected level, on average. Hence, there must be a number of countries for which education appears to have had less than the expected "standard augmented Solow model no externality" growth impact if wage increments were on the order of 10 percent.
is why. In those countries that have had substantial improvements in educational attainment of the labor force yet still face declining real wages and slow economic growth, the question must be asked: Where has all the education gone? I do not propose a single answer, but put forward three possibilities that could account for the results:

- The newly created educational capital has gone into piracy; that is, privately remunerative but socially unproductive activities.
- There has been slow growth in the demand for educated labor, so the supply of educational capital has outstripped demand and returns to schooling have declined rapidly.
- The education system has failed, so a year of schooling provides few (or no) skills.

These possibilities are not mutually exclusive; all are likely to be present to varying degrees in every country. I will discuss each briefly, with some indication of the evidence that would support or contradict each approach in a given country. (For a more extensive discussion, see Pritchett 1996.)

**Are Cognitive Skills Applied to Socially Productive Activities?**

Rent seeking in our [African] economies is not a more or less important phenomenon, as would be the case in most economies. It is the centerpiece of our economies. It is what defines and characterizes our economic life.

—Meles Zenawi, Prime Minister of Ethiopia, September 5, 2000

One way to reconcile high wage increments to schooling with a small (and differential) macroeconomic impact of education is to argue that social and private rates of return to education diverge due to distortions in the economy. North’s (1990) powerful metaphorical comparison of piracy and chemical manufacturing in the introduction suggests the problem. Rent seeking and directly unproductive activities can be privately remunerative but socially dysfunctional and reduce overall growth. If the improved cognitive skills acquired through education are applied to piracy, this could explain both the micro returns (rich pirates) and small macro impact (poor economies). Several pieces of evidence suggest this is at least part of the puzzle.

In many developing economies, the public sector has accounted for a large share of the expansion of wage employment in the 1960s and 1970s (table 3). This is not to equate government or the magnitude or growth of government employment with the magnitude of rent seeking. Nor am I saying that the expansion of education in government is necessarily unproductive. On the contrary, the most successful of developing countries have had strong and active governments and highly educated civil servants hired through a very competi-
TABLE 3. Share of Wage Employment Growth Accounted for by Public Sector Growth in Selected Developing Countries

<table>
<thead>
<tr>
<th>Country</th>
<th>Period</th>
<th>Average growth of wage employment (percent per annum)</th>
<th>Public sector (percentage of total increase)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Public</td>
<td>Private</td>
</tr>
<tr>
<td>Ghana</td>
<td>1960–78</td>
<td>3.4</td>
<td>-5.9</td>
</tr>
<tr>
<td>Zambia</td>
<td>1966–80</td>
<td>7.2</td>
<td>-6.2</td>
</tr>
<tr>
<td>Tanzania</td>
<td>1962–76</td>
<td>6.1</td>
<td>-3.8</td>
</tr>
<tr>
<td>Peru</td>
<td>1970–84</td>
<td>6.1</td>
<td>-0.6</td>
</tr>
<tr>
<td>Egypt</td>
<td>1966–76</td>
<td>2.5</td>
<td>-0.5</td>
</tr>
<tr>
<td>Brazil</td>
<td>1973–83</td>
<td>1.4</td>
<td>0</td>
</tr>
<tr>
<td>Sri Lanka</td>
<td>1971–83</td>
<td>8</td>
<td>0.9</td>
</tr>
<tr>
<td>India</td>
<td>1960–80</td>
<td>4.2</td>
<td>2.1</td>
</tr>
<tr>
<td>Kenya</td>
<td>1963–81</td>
<td>6.4</td>
<td>2</td>
</tr>
<tr>
<td>Panama</td>
<td>1963–82</td>
<td>7.5</td>
<td>1.8</td>
</tr>
<tr>
<td>Costa Rica</td>
<td>1973–83</td>
<td>7.6</td>
<td>2.8</td>
</tr>
<tr>
<td>Thailand</td>
<td>1963–83</td>
<td>6.3</td>
<td>5.5</td>
</tr>
<tr>
<td>Venezuela</td>
<td>1967–82</td>
<td>5.1</td>
<td>3.4</td>
</tr>
<tr>
<td>Unweighted mean</td>
<td></td>
<td>5.5</td>
<td>0.3</td>
</tr>
</tbody>
</table>

Source: Derived from Gelb and others (1991), table 1.

The question is not whether educated labor flows into the government, but why the government hires educated workers (actual need versus employment guarantee) and what they do once they are in the government (productive versus unproductive or rent-seeking activities).

Murphy and others (1991) present a simple model of the allocation of talent in which, if returns to ability are the greatest in rent seeking, then economic growth is inhibited by drawing the most talented people away from productive sectors into rent seeking. Anecdotal evidence that rent seeking attracts educated labor abounds. There is the possibly apocryphal (but nevertheless instructive) story of one West African nation with an employment guarantee for all university graduates. In a year when the exchange rate was heavily overvalued (and hence, there was a large premium on evading import controls), 60 percent of university graduates in all fields designated the customs service as their preference for government employment.

Explicit or implicit government guarantees of employment for the educated have been common and have led to large distortions in the labor market. In Egypt,

22. Wade (1990) asserts that college graduates are as likely to enter government service in Korea and Taiwan as in African economies.
government employment guarantees led to notoriously overstaffed enterprises and bureaucracies. In 1998, the government and public enterprises employed 70 percent of all university graduates and 63 percent of those with education at the intermediate level and above (Assaad 1997). Gersovitz and Paxson (1995) calculate that in 1986–88 in Côte d'Ivoire, 50 percent of all workers between age 25 and 55 that had completed even one grade of postprimary education worked in the public sector. Gelb and others (1991) built a dynamic general equilibrium model in which government responds to political pressures from potentially unemployed educated job seekers and becomes the employer of last resort for educated labor force entrants. They show that when both employment pressures are strong and the government is highly responsive to those pressures, the employment of surplus educated labor in the public sector can reduce growth of output per worker by as much as 2 percent a year (from a base case growth of 2.5 percent).

Stagnant Demand for Educated Labor

A second explanation for smaller growth returns from expanding education than from wage increments might suggest that the marginal return to adding an additional year of schooling economy-wide can be dramatically different from the average returns estimated from a cross-sectional Mincer (1974) regression on wage employment at a single point in time. Depending on the shift in the demand for and supply of educated labor, and on the mechanism of labor market adjustment, the wage premia can rise or fall. In different countries there is evidence of rising, falling, stable, or vacillating returns to schooling. Mincer coefficients in the United States have increased (at the median) from 0.063 to 0.096 (Buchinksy 1994). The returns to schooling in Egypt fell significantly in the 1980s (Assaad 1997). Funkhouser (1994) shows quite stable Mincer returns for five Central American countries over several years. Montenegro (1995) shows that the Mincer coefficient in Chile varied from 0.095 to 0.167 between 1960 and 1993—falling, then rising, then falling again over this period.

There are two basic stories to explain the demand for educated labor (including by the self-employed). One is that education conveys skills that make labor more productive. In this case, the demand for educated labor will rise when the skill intensity of the economy rises. The second is that more educated individuals are able to adapt more quickly to disequilibrium (Schultz 1975). In this case, the demand for educated labor will rise when there are greater gains to adapting to disequilibrium. These two stories of the source of returns to education are difficult to distinguish empirically, but both suggest that growth of educational capital would have a larger impact on output growth when policies are in place to ensure either that sectoral shifts lead to higher skill intensity, or that the creation or assimilation of knowledge is higher (even within the same sector), or both.

One can easily imagine a scenario in which a Mincer regression based on wage employment shows very high returns and yet, in the absence of expansion of the
wage employment sector (assume, for now, this is the skill-intensive sector), these returns could fall very fast so that the marginal return to additional education is very small. Table 4 (adapted from Bennell 1996) shows that in many African countries, expansion of the number of newly educated laborers has often exceeded expansion of wage employment by more than an order of magnitude. Under these conditions, the returns to education could fall very fast.

Even without sectoral shifts, the returns to education would be higher where technological progress was rapid, thus requiring constant adaptation to technologically induced disequilibrium. Schultz (1975) argues that in a technologically stagnant agricultural environment the production gains from education would be zero, as even the least educated could eventually reach the efficient allocation of factors. In this case, only when new technologies and inputs are available does education pay off, and then only in transition to the new equilibrium. Foster and Rosenzweig (1996) find that the return to five years of primary schooling versus no schooling in the average Indian district studied was a modest 11 percent (an average increase of 446 rupees in farm profits). However, returns to schooling were higher in those districts where agricultural conditions were intrinsically conducive to the adoption of Green Revolution technologies (which they proxy by the exogenous increase in average farm profits). In a district where farm profits are one standard deviation above the average due to technical

Table 4. Growth of Enrollments and of Wage Employment in Selected Sub-Saharan African Countries

<table>
<thead>
<tr>
<th>Country</th>
<th>Change in enrollments (thousands)</th>
<th>Change in wage employment (thousands)</th>
<th>Ratio, expansion of enrollment to wage employment</th>
<th>Wage employment as percentage of total labor force</th>
</tr>
</thead>
<tbody>
<tr>
<td>Enrollment growth positive, wage employment falling</td>
<td>Zambia 446</td>
<td>–4.3</td>
<td></td>
<td>13.1</td>
</tr>
<tr>
<td></td>
<td>Côte d'Ivoire 323</td>
<td>–7.7</td>
<td></td>
<td>9</td>
</tr>
<tr>
<td>Enrollment growth exceeds wage employment growth by an order of magnitude</td>
<td>Sierra Leone 257</td>
<td>8.9</td>
<td>29</td>
<td>4.9</td>
</tr>
<tr>
<td></td>
<td>Uganda 225</td>
<td>13.2</td>
<td>17</td>
<td>4.7</td>
</tr>
<tr>
<td></td>
<td>Ghana 1312</td>
<td>80</td>
<td>16</td>
<td>3.8</td>
</tr>
<tr>
<td></td>
<td>Burkina Faso 351</td>
<td>35.4</td>
<td>10</td>
<td>3.8</td>
</tr>
<tr>
<td></td>
<td>Lesotho 142</td>
<td>14.9</td>
<td>10</td>
<td>5.4</td>
</tr>
<tr>
<td>Enrollment growth higher by factor of 4</td>
<td>Senegal 180</td>
<td>45.4</td>
<td>4.0</td>
<td>5.5</td>
</tr>
<tr>
<td></td>
<td>Kenya 1709</td>
<td>436</td>
<td>3.9</td>
<td>14.1</td>
</tr>
<tr>
<td></td>
<td>Malawi 546</td>
<td>143</td>
<td>3.8</td>
<td>13.7</td>
</tr>
<tr>
<td>Rough equality of enrollment and wage sector growth</td>
<td>Botswana 157</td>
<td>122</td>
<td>1.3</td>
<td>50.4</td>
</tr>
<tr>
<td></td>
<td>Zimbabwe 135</td>
<td>111.1</td>
<td>1.2</td>
<td>36.6</td>
</tr>
</tbody>
</table>

Note: Growth rates of enrollments and wage sector growth are calculated from the beginning date of the study estimating Mincerian return to 1990 (or the most recent data).

Source: Bennell (1996), table 5.
progress, the return to primary schooling was 32 percent—almost three times higher. However, the converse of high returns with rapid progress is that the estimated returns to schooling were negative in those districts in which progress was low.\footnote{When average district farm profits were more than two-thirds of a standard deviation below the country average, the point estimate of education was negative. This explanation of the interaction of demand and supply for education due to different rates of technological progress might suggest the reason education appears not to have paid off in such places as SSA. Several recent studies have found very little return to education in farming in Africa (Gurgand 1995; Joliffe 1995). If there has been little exogenous change in the technical production functions appropriate for more educated farmers to adopt, it is because Green Revolution innovations were not appropriate for African agriculture.}

Rosenzweig (1996) uses data across districts of India to show the pitfalls in cross-sectional regressions when technological progress varies exogenously. In a cross-section of Indian districts, education is correlated with economic growth. But Rosenzweig (1996) shows that once varying exogenous technical progress is introduced, this technological progress explains both the higher economic growth and higher returns to education (and the higher returns lead to greater expansion in the amount of education). Although schooling has paid off handsomely where the Green Revolution brought technological advances, education has not been an important determinant of local growth in technologically stable areas, and the apparent impact of education from cross-district regressions disappears.

If some countries’ policies are more conducive to the creation or assimilation of technical progress or to development patterns that are skill intensive, then one could expect that the output impact of a given expansion of schooling could be higher or lower. For instance, many argue that more open trade regimes in developing countries would facilitate catch-up and lead to more rapid technical progress, and that the returns to education would depend, at least in part, on complementary policies such as reasonable outward orientation (World Bank 1994).

Did Schooling Create Skills?

Direct evidence from internationally comparable examinations shows substantial variation in schooling quality—and that children in some developing countries lag far behind OECD and East Asian countries. Low quality of schooling is consistent with the macroeconomic evidence and is obviously consistent with the household evidence of little or no wage increment from additional schooling.

However, in countries where there is a reliably demonstrated microeconomic return but no apparent macroeconomic impact of schooling, a more sophisticated “low quality” explanation of the paradox is needed. A signaling model of the labor market is consistent with schooling that creates few skills and yet substantial observed wage impacts. If workers with high initial (or innate) ability have an easier time staying in school than workers with low initial ability, employers will pay more for schooled workers even though schooling has no impact on skills or productivity (Spence 1976).
There is mixed evidence of a signaling function of schooling. "Sheepskin" effects—in which the completion of a level of education has substantially more labor market impact than would be expected from the skills acquired at that level—are common and can be taken as indication of schooling as a filter. However, there are at least three sources of evidence against an argument that the entire wage impact of schooling is signaling. First, several studies from developing countries with data on ability, skills, and schooling suggest that signaling effects are small (Knight and Sabot [1990], containing data on Kenya and Tanzania; Glewwe [1991] with data on Ghana; and Alderman and others [1996], with data on Pakistan). Second, the limited evidence of the impact of education on the productivity of farmers (Jamison and Lau 1982) or the self-employed is harder to explain by signaling. Finally, even for SSA countries, where one might suspect low educational quality, evidence from the Demographic and Health Surveys shows a 24 percent lower child mortality rate where women have a primary education as opposed to no education (Hobcraft 1993). This is hard to explain if schooling has no impact on knowledge.24

III. Conclusion

In the decades since 1960, nearly all developing economies have seen educational attainment grow rapidly. The cross-national data show, however, that on average, education contributed much less to growth than would have been expected in the standard augmented Solow model. Where did all the education go?

There are three possible explanations for the differences across countries in the impact of schooling on growth in economic output:

- In some countries, schooling has created cognitive skills and these skills have been in demand, but to do the wrong thing. In other countries, the institutional environment has been sufficiently bad that the bulk of newly acquired skills has been devoted to privately remunerative but socially wasteful or counterproductive activities—that is, the expansion of schooling has meant the country just has better-educated pirates.
- The rate of growth of demand for educated labor (due in part to different sectoral shifts, in part to policies, in part to exogenous differences in technological progress) has varied widely across countries, so countries with the same initial individual returns and equal subsequent expansions in the supply of educated labor could have seen the marginal returns to education fall dramatically, stay constant, or rise.
- In some countries, schooling has been enormously effective in transmitting knowledge and skills, while in others it has been essentially worthless and has created no skills.

24. But it is not impossible to explain, as the education–health linkage might be entirely the result of intergenerationally correlated endowments or preferences.
No two countries follow exactly the same pattern, and each explanation contributes a different amount to explaining the overall impact of schooling on growth in different countries.

None of the arguments in this article suggest that governments should invest less in basic schooling, for many reasons. For one thing, most (if not all) societies believe that at least basic education is a merit good, so that its provision is not and need not be justified on economic grounds at all—a position with which I strongly agree. To deny a child an education because of a small expected economic growth impact would be a moral travesty. In addition, schooling has a large number of direct beneficial effects beyond raising economic output, such as lower child mortality. All education can raise cognitive skills, with everything that implies. The implication, therefore, of a poor past aggregate payoff from increased cognitive skills in a perverse policy environment is not “don’t educate,” but rather “reform now so that investments (past and present) in cognitive skills will pay off.”

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Measuring the Dynamic Gains from Trade

Romain Wacziarg

This article investigates the links between trade policy and economic growth in a panel of 57 countries between 1970 and 1989. It develops a new measure of trade policy openness based on the policy component of trade shares, using it in a simultaneous equations system to identify the effect of trade policy on several determinants of growth. The results suggest a positive impact of openness on economic growth, with the accelerated accumulation of physical capital accounting for more than half the total effect; enhanced technology transmission and improvements in macroeconomic policy account for smaller effects. This decomposition is robust with respect to alternative specifications and time periods. The article also successfully tests whether the model exhaustively captures the effects of trade policy on growth.

The relationship between trade openness and economic growth has been the subject of numerous empirical studies. Most uncover a positive empirical association between trade openness and per capita income growth; until recently, few economists challenged the findings. Although theories promoting inward-oriented development strategies flourished in the 1950s and 1960s, the policies' unsustainable effects had, by and large, discredited the idea that the costs of an open trade regime may outweigh its potential benefits.

Recently, however, Rodrik and Rodríguez (2000) have questioned the empirical results on trade and growth, pointing to methodological problems associated with the measurement of openness and the specification of estimated equations. In particular, the collinearity between trade protection and other measures of (possibly domestic) policy, such as the quality of macroeconomic policy, might lead researchers to conclude wrongly that trade protection depresses growth, when another omitted or poorly measured variable is in fact accounted for.

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2. Other recent studies casting doubt on a positive growth-openness link using macrodata include Rodrik (1998a) and Harrison and Hanson (1999). In a related literature, a study by Vamvakidis (1998) uncovers negative effects of regional arrangements, such as free trade areas, on growth in time-series data.

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for by trade openness.\textsuperscript{3} This challenge suggests two directions for research: improving existing measures of trade policy openness and being more explicit about how trade openness might affect growth by specifying more clearly the channels relating these variables. This allows for the possibility that negative channels may partially or fully outweigh positive ones. This article seeks to advance the literature on both fronts.

Theory points to a number of possible costs and benefits of trade openness, not mutually exclusive in general. Some theories stress technological spillovers and the international transmission of knowledge as a source of growth for open economies.\textsuperscript{4} More traditional, static theories invoke allocative efficiency, which can be achieved more easily with an open trade regime even when factors of production are assumed to be immobile. Higher levels of output are attained when countries specialize according to comparative advantage, so growth rates can be expected to increase in the transition that follows a liberalization episode.

The increased degree of market competition resulting from a wider scale of market interactions yields further gains in efficiency.\textsuperscript{5} More generally, by increasing the size of the market, trade openness allows economies to better capture the potential benefits of increasing returns to scale.\textsuperscript{6} Yet another set of theories points to the complementary aspects of virtuous policies: trade policy openness may create incentives for governments to adopt less distortionary domestic policies and more disciplined types of macroeconomic management. On the cost side, some theories suggest that when comparative advantage patterns would lead a country to specialize in goods where technological innovations or learning by doing are largely exhausted, opening up to trade might actually reduce long-run growth (Young 1991). Another potentially negative channel was suggested in Rodrik’s (1998b) findings on openness and government size: more open countries may face incentives to increase the size of government to insure agents in the face of foreign shocks. In turn, a larger government may distort resource allocation, to the detriment of economic growth.\textsuperscript{7}

There has been little empirical work to determine the relative roles of these different factors in explaining the observed overall impact of trade policy openness on growth. The finding that trade openness spurs growth tends to be interpreted according to the observer’s preferred theory, but two important possibilities are ignored: several forces may be operating simultaneously, and trade openness may also involve some dynamic costs, even if the benefits outweigh them. This be-

\textsuperscript{3} For example, Rodrik and Rodríguez (2000) criticize the Sachs and Warner (1995) contribution because much of the variance in their trade liberalization dummy is accounted for by the black market premium on the exchange rate, itself at least as much a measure of poor domestic policies as of a closed trade regime.

\textsuperscript{4} See, for instance, Grossman and Helpman (1991) and Barro and Sala-i-Martin (1997). This relies on the notion that more open economies are better able to import advanced technologies.

\textsuperscript{5} See, for instance, Wacziarg (1997).

\textsuperscript{6} See Alesina and others (2000).

\textsuperscript{7} Barro and Sala-i-Martin (1995) provide empirical evidence on this point.
comes especially important with increasing integration: by determining the source of the costs and benefits of trade liberalization, policymakers can hope to maximize the benefits and to minimize the costs.

This article employs a fully specified empirical model to evaluate the channels through which trade policy might affect growth. To this end, it presents two innovations. The first is a new measure of trade openness based on a weighted average of several indicators (tariff revenues, nontariff barriers, and an indicator of overall outward orientation). This new measure of trade policy openness corresponds to the policy-induced component of an average country’s trade to gross domestic product (GDP) ratio.

The second innovation is a set of equations describing the incidence of trade policy on several determinants of growth. Moving away from single-equation, reduced-form growth empirics, these equations capture different theoretical arguments on the potential costs and benefits of trade policy openness. Various channel variables are included in a growth regression. By multiplying the effects of trade policy on the channel and the effect of the channel on growth, the effect of trade policy on growth through that specific channel can be identified. The results suggest a positive effect of trade policy openness on economic growth, with accelerated accumulation of physical capital accounting for more than half the effect.

The article first analyzes the theoretical basis for the six channels and describes the empirical methodology for measuring the channel effects. It then discusses measurement issues, provides preliminary evidence on trade policy and growth, and describes the channel effects. The model’s robustness and exhaustiveness are also examined.

I. Theory and Methodology

This section discusses the six channel variables and outlines the article’s empirical methodology.

The Six Channels in Economic Theory

The six links between trade policy and economic growth incorporated in the empirical model are meant to capture the dominant theories concerning dynamic gains (or possibly losses) from trade. The underlying assumption is that together these six channels adequately capture most of the effect of trade policy on growth. These channels are broadly classified under government policy, domestic allocation and distribution, and technology transmission.

Government policy. Trade openness may create incentives for policymakers to pursue virtuous macroeconomic policies, either because of the threat of capital flight or because of international agreements, implicit or explicit, that act as a check on policy. Preserving a competitive environment for domestic firms engaged in foreign transactions may also require policies that maintain macroeco-
nomic stability. In turn, macroeconomic stability is likely to favorably affect growth by reducing price uncertainty and moderating public deficit and debt levels, thereby reducing crowding out and the likelihood of future tax increases and furthering the ability of domestic firms to compete on global markets (Fischer 1993).

Another way to capture the effects of trade openness on government activity is to consider the effect on the size of government. If more open economies are subject to larger exogenous supply and demand shocks, a larger government may be better able to provide insurance or consumption smoothing through redistribution or other forms of social programs (Rodrik 1998b). On the other hand, open economies may tend toward laissez-faire arguments and more limited taxation to preserve the economy's price competitiveness and attractiveness to foreign investors. The effect of trade policy openness on government size, measured by the public consumption of goods and services, is therefore theoretically ambiguous. On the other hand, theory points to a positive growth-maximizing size of government resulting from a tradeoff between the productive function of public activities and the distortionary nature of taxation (Barro and Sala-i-Martin 1992). In addition, Barro (1991) and Barro and Sala-i-Martin (1995) document the negative impact of a larger government on growth in a cross-section of countries.

ALLOCATION AND DISTRIBUTION. Open economies are less likely to have tradable goods prices that differ substantially from world market prices because free trade should facilitate price convergence of traded goods across countries. Open countries will tend to specialize according to their comparative advantage, so once the effect of nontradable goods on deviations from purchasing power parity has been eliminated, countries with open trade policies would be expected to have lower overall price levels (relative to some benchmark country, such as the United States) than closed economies (Dollar 1992). Hence, theory points to a lower degree of price distortion in open economies, and price distortions have been shown to adversely affect factor accumulation and growth (Easterly 1989, 1993).

Factor accumulation may also be of crucial importance. Much of the effect of trade policy on growth may well work through the domestic rate of physical investment, which is a determinant of economic growth in a nearly tautological sense (Levine and Renelt 1992; Baldwin and Seghezza 1996). The investment channel may capture several theories. First, investment may respond to openness through a size of the market effect. As first stressed by Adam Smith, market

8. However, some scholars question the direction of causality between investment and growth, based on Granger causality tests (see Blomstron and others 1996). But these tests are typically based on relatively high-frequency data, whereas this study examines long-term relationships between growth and its determinants. The Solow model predicts that the long-term relationship runs from investment rates to growth. This article also uses an instrumental variables estimator, which should limit the incidence of this type of endogeneity.
size imposes a constraint on the division of labor, so that more open countries are better able to exploit increasing returns to scale. Trade liberalization may thus provide the type of big push effect on capital accumulation that Murphy and others (1989) argued was required for less developed countries to move from a low growth equilibrium to a path of sustained industrialization.\textsuperscript{9} Using a related argument, Wacziarg (1997) shows that the extent of the market is an important determinant of product market competition. The entry of new firms in export markets after an episode of liberalization may well entail large fixed investments.

Second, trade liberalization may simply allow domestic agents to import capital goods that were previously unavailable (or produced locally but at higher costs), thus removing structural constraints on investment. Capital goods imports, which make up sizable proportions of the imports of many recently liberalized developing economies, also embody more recent technologies, a further source of growth. In a related argument, Baldwin and Seghezza (1996a, 2) state that “assuming that traded goods are an input into capital formation, protection raises the cost of new capital goods and thereby tends to lower the rate of return on investment. With intertemporal optimization, this lowers the steady-state capital stock and slows growth in the transition.”

Technological transmissions. The last two channels are drawn from the recent literature on endogenous growth: if knowledge spillovers are a driving force for sustained, long-run growth, and open economies are more exposed to a worldwide stock of productivity-enhancing knowledge, then trade openness can affect growth and convergence through technology transmissions (Barro and Sala-i-Martin 1997; Grossman and Helpman 1991).

One way openness can increase the exposure of the domestic economy to technology transmission is by making it easier, through more frequent and sustained international trade interactions, for domestic producers to imitate foreign technologies and incorporate that knowledge in their own productive processes (Edwards 1992). This increased exposure can stem from direct imports of high-tech goods or from greater interaction with the sources of innovation (through enhanced international communication and mobility brought forth by economic integration). This should translate into a higher capacity to compete with more advanced economies on world markets. Such a pattern was certainly part of the East Asian growth miracle, characterized by broad transformations in the product composition of output and exports from agriculture to heavy industry and finally to high-tech goods, through the imitation of technology originating in industrial countries.

\textsuperscript{9} Ades and Glaeser (1999) provided preliminary empirical evidence showing that the extent of the market boosts growth largely through an increase in the rate of capital accumulation, thus lending support to big push theories. The working paper version of this study (Wacziarg 1998) contains further evidence on this point.
A second channel for greater technology transmission is foreign direct investment (FDI), whether associated with joint ventures or not. FDI often transmits advanced types of technology, either through capital goods imports that are later imitated or through the diffusion of knowledge and expertise. However, it is unclear a priori that trade openness is associated with greater levels of FDI. FDI may act as a substitute for trade, because foreign investment is used to set up plants producing goods that cannot be imported because of trade restrictions (tariff-hopping). Or investors may view trade openness as a signal that a country is committed to stable and market-oriented economic policies, whereas trade openness allows them to import the intermediate goods required to initiate projects, expect repatriation of some profits, and export the goods they produce. Falling transport costs may allow a slicing up the value-added chain, so that firms can produce a good in stages in several locations, adding a little more value at each stage (Krugman 1995). In that case FDI may complement rather than substitute for trade openness. Indeed, evidence suggests that open economies attract more FDI than closed economies (Harrison and Revenga 1995).

In turn, FDI is likely to spur growth. Because the share of FDI in GDP is typically small (averaging about 1 percent), it is hard to argue that FDI would spur growth through traditional physical capital formation. If there is any significant dynamic effect of FDI, it likely captures the incidence of a certain type of technology transmission, an interpretation applied here to the FDI channel.\textsuperscript{10}

**Empirical Methodology**

The estimates presented in this article use a method first employed in a cross-country growth context by Tavares and Wacziarg (2001) to analyze the effects of democracy on growth.\textsuperscript{11} The underlying econometric theory is an extension of the three-stage least squares method of Zellner and Theil (1962) to panel data.

**The Structural Model.** The basic framework for the cross-sectional analysis consists of a simultaneous equations model aimed at identifying the effects of trade policy on growth. The model consists of an equation for the growth of per capita income, one for determining the nature of trade policy, and six channel equations describing the effects of trade policy on several growth determining variables. This constitutes the structural model, derived from economic theory: the channel variables are included in the growth regression, but the measure of trade policy openness appears only in the channel relationships.

\textsuperscript{10} However, Aitken and Harrison (1999), using plant-level data for Venezuela, show that foreign ownership adversely affects the productivity of domestically owned plants. I will use macroeconomic data to evaluate whether this result holds at the aggregate level.

\textsuperscript{11} Baldwin and Seghezza (1996) also employ three-stage least squares to estimate a system for the joint determination of growth and investment rates, as a function, among other variables, of the trade regime. Taylor (1998) uses a structural approach to growth empirics to study the impact of outward orientation on growth in Latin America.
To better understand the foundations for the channel analysis, consider a neo-
classical production function: \( Y = AK^{\alpha}H^{\beta}L^{1-\alpha-\beta} \), where \( A \) denotes the level of tech-
nology, \( K \) physical capital, \( H \) human capital, and \( L \) labor.\(^{12}\) Dividing by \( L \) and totally
differentiating with respect to time yields the traditional Solow decomposition:

\[
\frac{\dot{y}}{y} = \frac{\dot{A}}{A} + \frac{\dot{k}}{k} + \frac{\dot{h}}{h},
\]

where lowercase letters designate per worker quantities. Hence, the ultimate
drivers of per capita growth are technological growth and the (per capita) growth
of human and physical capital. Presumably, the nature of trade policy can affect
either of these factors. The channel methodology therefore consists of excluding
the trade policy index from the growth equation directly and examining its ef-
tections on the ultimate drivers of growth instead.

Limiting the number of ultimate growth determinants, however, may over-
simplify the model. To enrich the structural model and allow for the effects of
trade policy openness on growth through such factors as government policies or
technology transmission (the latter being only part of the \( A/A \) term), the list of
growth determinants can be augmented. For example, adding a measure of gov-
ernment consumption and macroeconomic policy allows consideration of the
corresponding channels. Hence, although the analysis here takes a step away from
purely reduced-form growth empirics, it stops short of a fully structural model.
Such a model would involve explicit consideration of the effects of, for example,
government consumption on factor accumulation and technological progress,
which are in turn the ultimate drivers of growth.\(^{13}\) In contrast, in the model
developed here, government consumption and factor accumulation appear jointly
in the growth equation, so any effects of openness mediated by government con-
sumption (including those going through investment) will be reflected in the
government consumption channel.

An equation is formulated relating trade policy and other determinants to each
channel variable under consideration, with the intention of fully exhausting the
ways that openness could affect growth. (Formal evidence concerning this issue
is provided in section III.) Finally, the equation describing the determinants of
trade policy openness explicitly deals with the endogeneity issues having to do
with the simultaneous determination of trade policy, growth, and the channel
variables. In particular, several channel variables may appear on the right-hand
side of the trade policy equation.

\(^{12}\) I am grateful to an anonymous referee for suggesting this interpretation.
\(^{13}\) In this case, the analysis would involve three rather than just two steps: a fully structural model
would consider the effect of trade policy on government consumption, the effect of government con-
sumption on technological and factor growth, and the effect of technological and factor growth on per
capita income growth. However, such a system would be extremely cumbersome and would involve
the estimation of a large number of parameters relative to the available data.
Estimation. The parameters of the structural model are estimated jointly using three-stage least squares. This method achieves consistency by appropriate instrumenting, and efficiency through optimal weighting. It combines features of instrumental variables, random effects, and generalized least squares models.

Each of the eight equations in the structural model is formulated for four time periods under scrutiny (1970–74, 1975–79, 1980–84, 1985–89). Joint estimation allows the derivation of a large covariance matrix for the error terms of all 32 equations. Hence, both cross-period and cross-equation error correlations are allowed to differ from zero. This ensures the efficiency of the estimates. Taking cross-period error correlations into account is similar to assuming that the error terms contain country-specific effects uncorrelated with the right-hand-side variables. The flexibility of the error covariance matrix allows for substantial efficiency gains relative to estimating each equation separately (that is, assuming zero cross-equation error covariances).

Because several endogenous variables appear on the right-hand side of the structural equations, endogeneity bias is a concern. Consistency requires instrumenting for every endogenous variable that appears as a regressor. This is done by first rewriting every endogenous variable as a function of all the exogenous variables in the system in the model's reduced form. The fitted values of each endogenous variable from ordinary least squares estimation of the reduced form equations will provide suitable instruments for each corresponding endogenous variable in the structural form. Because of concerns about the endogeneity of per capita income levels in the context of a random effects estimator with a lagged dependent variable, per capita income was excluded from the list of instruments (see Caselli and others 1996).

The second stage of the three-stage least squares procedure consists of estimating each equation in the structural model separately through instrumental variables (or two-stage least squares), using the instruments constructed in the first stage. This allows the derivation of a consistent covariance matrix for the error terms of the model. The third stage employs this covariance matrix as a weighting matrix as well as the instruments derived in the first stage to jointly estimate the equations in the structural model using instrumental variables-generalized least squares.

Identification and Restrictions. Some assumptions about specifications are required for this methodology to carry through. For each equation, enough instruments must be validly excluded for the order condition to be met: at least

---

14. Given the above specification of the baseline model, the instruments are male and female human capital, the island dummy variable, the log of population, the democracy index, the log of area, terms of trade shocks, population density, the secondary school completion rate, the share of population over age 65, the share of population under age 15, ethnolinguistic fractionalization, and postwar independence status, each taken at every time period when applicable.
as many exogenous variables must be excluded as regressors because there are endogenous variables included on the right-hand side.

The chosen specification is based on empirical work on the determinants of the endogenous variables under study. For instance, the growth and investment equations are based on common specifications used in the cross-country growth literature (Barro and Sala-i-Martin 1995). The specification of the government size equation is based on Rodrik (1998b) and Alesina and Wacziarg (1998). For other channels, priors were used to determine the set of exclusions.15 (Table C-1 in appendix C displays parameter estimates of each equation in the system for the baseline model, allowing readers to infer the specification of each of the equations in the system.)16

To assess the long-run effects of trade policy on growth in a unified manner, cross-period parameter equality restrictions are imposed: none of the estimates of the parameters in the structural model is allowed to vary across time. This allows efficiency gains through higher degrees of freedom, as the number of estimated parameters in the system is divided by four. Whether these cross-period parameter equality restrictions are justified is examined in section III.

II. MEASUREMENT ISSUES AND PRELIMINARY EVIDENCE

This section considers issues involved in measuring trade openness and the channel variables, and presents simple correlations between the main variables of interest.

Existing Measures of Trade Openness

Measuring the extent of trade openness is a major challenge for any study involving the analysis of trade policy, as suggested in Rodrik and Rodriguez (2000) and Pritchett (1996a). There are three broad categories of existing measures of trade openness.

Outcome Measures. Outcome measures describe the volume of trade or its components. This type of indicator is most subject to endogeneity problems with respect to growth (Frankel and Romer 1999), but because it measures actual exposure to trade interactions, it may account quite well for the effective level of integration. It may correlate only imperfectly, however, with attitudes or institutions relating to openness. Past research has tended to confuse outcome measures with policy attitudes (which are presumed to partly determine the outcome), largely because precise measures of actual trade policies were not widely available.

Because most theories about dynamic gains from trade have to do with policy measures, contrasting free trade to restricted trade or autarky, an index of trade

15. Tavares and Wacziarg (2001) discuss in more detail the specification search for the type of system that is considered here. A previous report on this study (Wacziarg 1998) describes the specification of each equation in the system.

16. Because each equation is estimated for four time periods, with estimated parameters constrained to equality across periods, the table reports $R^2$ statistics corresponding to each of these time periods.
policy had to be constructed for this study that adequately captures the nature of the policy regime for international trade. The use of outcome measures seems undesirable on these grounds, because they also reflect the gravity component of trade openness. The choice is then between direct policy indicators and effective protection measures.

**Policy Indicators.** Tariff rates, nontariff barriers, tariff revenues, and related matters describe the institutional features of a country's attitude toward the rest of the world with respect to trade and factor flows. As such, they are likely to be an important determinant of the outcome measures. However, there are endogeneity problems in their relationship with growth, and they tend to have limited availability. Furthermore, they may not directly reflect the degree of effective protection faced by domestic agents, but only the legal framework they confront.

The main drawback of such trade policy measures as tariff barriers, nontariff barriers, and broader measures of a country's liberalization status is that they are weakly correlated among themselves. Pritchett (1996a) showed that no such single policy measure adequately captures a country's outward orientation. Because various measures may reflect different aspects of a country's trade policy, using a single indicator may not be very informative. This suggests combining the variation in several measures to obtain an indicator of trade openness.

**Deviation Measures.** Deviations of observed trade volume from the predicted free-trade volume are also used to provide a measure of how restrictive the trade regime really is. Factor endowment and gravity models of trade generate predictions about a country's propensity to trade internationally. For instance, small country size, distance from major trading partners, and negative terms of trade shocks can be thought to affect trade volumes negatively. Similarly, relative endowments of skilled labor, unskilled labor, and capital and natural resources may have an impact on overall trade volumes. This type of variable can be used to predict a country's potential free trade volume of international commercial transactions.

There are three drawbacks to measures based on deviations. First, some determinants of potential trade may have been omitted, so the predicted level of trade may not adequately measure the volume of commercial transactions that would prevail under complete free trade. Second, some gravity or endowment determinants of potential trade may be highly correlated with policy attitudes, so the deviation of observed from potential trade may exclude some valid infor-

17. The working paper on this study (Wacziarg 1998) presents empirical evidence in favor of this choice: the growth effects of trade openness seem mostly due to the trade policy regime, rather than to the gravity component of trade shares.

18. The classic reference on such residual measures is Leamer (1988).

19. Frankel and Romer (1999) presents a state-of-the-art method for computing the gravity component of trade volumes by regressing bilateral trade on exogenous characteristics of country pairs, such as distance and common language.
mation about policy. Third, as long as the observed volume of trade contains a white noise disturbance term, deviations from predicted volumes will also contain a white noise disturbance (whose share of the variance in the total variance of the measure has increased due to the differencing), and its use will result in increased downward bias associated with measurement error.

Construction of the Trade Policy Openness Index

The approach used here attempts to avoid these problems with existing measures of trade openness. A country’s trade to GDP ratio can be viewed as resulting from policy, factor endowment, and gravity determinant variables. The trade policy index is computed by isolating the variation in trade shares attributable to a variety of trade policy measures.

More specifically, trade shares (the ratio of imports plus exports to GDP) are regressed on several openness-determining variables, including policy, gravity, and endowment variables. The estimated coefficients on the policy variables are used as weights in constructing a weighted average of these variables. This weighted average is the index of trade policy openness, equal to the portion of observed trade shares attributable to the effective impact of trade policy. This procedure avoids both the problem of measurement error due to the construction of the difference between observed and potential trade shares (because it is not constructed as a residual) and the problem of collinearity between gravity and endowment and policy factors.

Components of the Openness Index

The objective is to construct an openness measure that applies to a broad range of countries over the period 1970–89 and that adequately accounts for tariff barriers, nontariff barriers, and other policy attitudes toward international trade that capture outward orientation.

Tariff Barriers. The effects of tariff barriers are captured by the share of import duty revenues in total imports (from the International Monetary Fund’s [IMF] Government Finance Statistics Yearbook). This has three advantages. First, it better captures the effective degree of tariff restrictions. Direct overall measures of tariff protection obtained from the U.N. Conference on Trade and Development (UNCTAD) are unweighted averages of goods-specific tariff rates. However, duty revenues are by construction weighted by the composition of imports. Second, officially declared tariff rates and effectively implemented rates may vary substantially. Duty revenues once again avoid this problem by measuring the tariff revenues actually collected. Third, data based on revenues are available for more countries and a wider time span than direct measures of tariff rates.20

20. Unweighted tariff rates were available for the period 1980–93 only, and for approximately 50 countries.
One potential limitation of the use of tariff revenues is that prohibitive tariff rates tend to reduce revenues through a Laffer curve effect applied to imports. However, the problem is likely greatly attenuated by the fact that duty revenues are treated as a share of total imports (high tariff rates work to reduce revenues by deterring imports, so the ratio of the two should roughly reflect effective tariff rates). Correlations between tariff revenues and tariff rates, for the dates and countries available for both measures, are relatively high, ranging from 66 (64?) percent to 83 percent (table 1).

NONTARIFF BARRIERS. Existing measures of nontariff barriers are highly imperfect, dealing mainly with coverage rates (percentage of goods affected by quotas, voluntary export restraints, and the like) and ignoring whether the constraints are binding. Furthermore, there is no consistent panel data set for nontariff barriers. The measure used here is an unweighted coverage ratio for the pre-Uruguay Round time period published by UNCTAD. Although the extent of nontariff barriers has no doubt varied across time, like tariffs it is likely to be highly autocorrelated within countries. The data do not permit accounting for this time-series variation, because there is only one observation for the 20 years under consideration. Presumably, this type of measurement error weakens the relationship of nontariff barriers with trade volumes and correspondingly reduces the weight of this indicator in the overall index.

LIBERALIZATION STATUS. A third component for the index of trade policy was developed to capture the overall attitude of policymakers. Dummy variables were constructed for a country's liberalization status for each year using the list of trade liberalization episodes compiled by Sachs and Warner (1995) for a large sample of countries. These were then averaged over the four time periods under study. Insofar as this indicator receives some weight in the index, it captures factors other than just tariffs and nontariff barriers; in particular, it may help account for the effect of time variations in nontariff barriers, which cannot be explicitly accounted for because of data unavailability.

Rodrik and Rodríguez (2000) have recently raised strong doubts about the indicator used in Sachs and Warner (1995), arguing that much of the variation in the liberalization dummy variable is attributable to the black market premium on the exchange rate (a proxy for distorted macroeconomic management as much

21. These dates were constructed by examining trade policy data and by conducting a systematic analysis of the literature concerning the trade regimes of specific countries. The sources for the dates for each country are reported in the appendix to their article. Note that the dates of liberalization computed by Sachs and Warner (1995) and their cross-sectional liberalization dummy (for the mid-1980s) are derived using different methodologies. In particular, because much of the tariff and nontariff data were not available for periods before the 1980s, Sachs and Warner (1995, 24, n. 44) resorted to a literature search to determine when countries opened their trade regimes, rather than to the five formal criteria used to derive their well-known liberalization dummy variable (the latter is computed for the mid-1980s only).
Table 1. Correlations between Duty Revenues and Unweighted Tariff Rates

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<tr>
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<tbody>
<tr>
<td>1980-84</td>
<td>0.667</td>
<td>0.744</td>
<td>0.725</td>
</tr>
<tr>
<td>1985-89</td>
<td>0.638</td>
<td>0.754</td>
<td>0.717</td>
</tr>
<tr>
<td>1990-94</td>
<td>0.802</td>
<td>0.837</td>
<td>0.831</td>
</tr>
</tbody>
</table>

Note: 50 observations.

as for the degree of openness) and the existence of an export marketing board (a characteristic mainly of slow-growing African economies). Hence, they argue that the Sachs and Warner variable is constructed in a way that is conducive to finding a positive effect of openness on economic growth. For this reason, results are also presented here based on an index of openness that excludes the Sachs and Warner liberalization status.

Correlations between these underlying components of the trade policy index are displayed in table 2. The signs of the correlations are as expected. The nontariff barriers measure is most weakly correlated with the other indicators, suggesting either that its inclusion may provide useful information about trade policy or that it is a poor measure of openness. Insofar as the nontariff barrier measure poorly reflects the true orientation of trade policy, however, it should receive a small weight in the overall index.

Trade Shares Regressions

Estimates pertaining to the determination of trade shares are shown in table 3.23 The explanatory variables feature the three policy indicators (import duties as a share of total imports, the pre–Uruguay Round nontariff barriers coverage ratio, and the Sachs-Warner liberalization status indicator averaged over the relevant five-year time periods). The regression also features gravity components, such as log of land area and log of population, as well as the growth rate of per capita GDP.24

As expected, trade shares are positively affected by liberalization status and negatively affected by tariffs and nontariff barriers. The lack of precision of the

22. This study uses an indicator based on Sachs and Warner’s liberalization dates rather than on their (purely cross-sectional) liberalization dummy. This may reduce the incidence of the Rodrik and Rodriguez critique, insofar as the liberalization dates are based on the broad survey of the literature on specific countries’ trade regimes. Entirely removing this indicator from the index, however, allows the Rodrik and Rodriguez critique to be addressed more fully.

23. The three-stage least squares estimator described earlier is used to obtain these estimates.

24. The working paper on this study (Wacziarg 1998) provides evidence of reverse causation from growth to trade shares, justifying the inclusion of economic growth in the equation for the trade to GDP ratio.
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<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Duty 1970-74</td>
<td>1.000</td>
<td></td>
<td></td>
<td></td>
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<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>Duty 1975-79</td>
<td>0.944</td>
<td>1.000</td>
<td></td>
<td></td>
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<td></td>
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<tr>
<td>Duty 1980-84</td>
<td>0.825</td>
<td>0.887</td>
<td>1.000</td>
<td></td>
<td></td>
<td></td>
<td></td>
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<td></td>
</tr>
<tr>
<td>Duty 1985-89</td>
<td>0.753</td>
<td>0.808</td>
<td>0.935</td>
<td>1.000</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Nontariff barriers</td>
<td>0.190</td>
<td>0.232</td>
<td>0.157</td>
<td>0.212</td>
<td>1.000</td>
<td></td>
<td></td>
<td></td>
<td></td>
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<td></td>
<td></td>
</tr>
<tr>
<td>Liberalization 1970-74</td>
<td>-0.467</td>
<td>-0.460</td>
<td>-0.470</td>
<td>-0.459</td>
<td>-0.120</td>
<td>1.000</td>
<td></td>
<td></td>
<td></td>
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<td></td>
<td></td>
</tr>
<tr>
<td>Liberalization 1975-79</td>
<td>-0.471</td>
<td>-0.465</td>
<td>-0.474</td>
<td>-0.455</td>
<td>-0.085</td>
<td>0.994</td>
<td>1.000</td>
<td></td>
<td></td>
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</tr>
<tr>
<td>Liberalization 1980-84</td>
<td>-0.470</td>
<td>-0.464</td>
<td>-0.473</td>
<td>-0.446</td>
<td>-0.048</td>
<td>0.978</td>
<td>0.994</td>
<td>1.000</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Liberalization 1985-89</td>
<td>-0.429</td>
<td>-0.469</td>
<td>-0.469</td>
<td>-0.479</td>
<td>-0.182</td>
<td>0.900</td>
<td>0.890</td>
<td>0.870</td>
<td></td>
<td></td>
<td></td>
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</tr>
</tbody>
</table>

*Note: 57 observations.*
TABLE 3. Trade Shares Regression
(three-stage least squares estimates)

<table>
<thead>
<tr>
<th>Independent variable</th>
<th>Trade Policy 1: baseline index</th>
<th>Trade Policy 2: excluding the Sachs and Warner variable</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant</td>
<td>182.561</td>
<td>186.830</td>
</tr>
<tr>
<td></td>
<td>(9.70)</td>
<td>(8.55)</td>
</tr>
<tr>
<td>Growth of per capita income</td>
<td>0.322</td>
<td>0.444</td>
</tr>
<tr>
<td></td>
<td>(1.12)</td>
<td>(1.40)</td>
</tr>
<tr>
<td>Log of land area</td>
<td>-8.029</td>
<td>-9.164</td>
</tr>
<tr>
<td></td>
<td>(-3.69)</td>
<td>(-2.23)</td>
</tr>
<tr>
<td>Log of population</td>
<td>-9.121</td>
<td>-8.052</td>
</tr>
<tr>
<td></td>
<td>(-3.42)</td>
<td>(-2.31)</td>
</tr>
<tr>
<td>Import duties / total imports</td>
<td>-34.733</td>
<td>-60.912</td>
</tr>
<tr>
<td></td>
<td>(-1.16)</td>
<td>(-1.78)</td>
</tr>
<tr>
<td>Pre-Uruguay Round</td>
<td>-0.217</td>
<td>-0.239</td>
</tr>
<tr>
<td>nontariff barrier coverage</td>
<td>(-0.73)</td>
<td>(-0.74)</td>
</tr>
<tr>
<td>Sachs/Warner</td>
<td>11.262</td>
<td></td>
</tr>
<tr>
<td>liberalization status</td>
<td>(2.12)</td>
<td></td>
</tr>
<tr>
<td>Adjusted $R^2$</td>
<td>0.60 0.55</td>
<td>0.60 0.54</td>
</tr>
<tr>
<td></td>
<td>0.53 0.49</td>
<td>0.50 0.47</td>
</tr>
<tr>
<td>Number of observations</td>
<td>71 (4)</td>
<td>71 (4)</td>
</tr>
<tr>
<td>(number of periods)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Note: The dependent variable is imports plus exports as a share of GDP. Numbers in parentheses are t-statistics. Because each equation is estimated for four time periods, with estimated parameters constrained to equality across periods, the table reports $R^2$ statistics corresponding to each of these time periods. The instruments used were: initial income; population density; dummy variables for religion, oil producers, postwar independence; log of population; share of population over 65; and log of area.

estimates, largely due to collinearity between the policy measures, is not really a concern since the objective is only to generate rough weights for how the three components affect trade shares. Minor variations in these weights are not likely to influence the final results.

Two indices of trade policy were computed using estimates from the two regressions in table 3 as weights on the various policy measures. For each period, the trade policy openness indices were computed as:

Trade Policy 1 = $-34.73$ (Import Duty Share) $- 0.22$ (Nontariff Barriers) $+ 11.26$ * (Liberalization Status)

Trade Policy 2 = $-60.91$ (Import Duty Share) $- 0.24$ (Nontariff Barriers)

Correlating the baseline index (Trade Policy 1) with its three components (table 4) gives an idea of the relative weights attached to each. For all the components, correlations with the overall index are larger than 0.449 in absolute value, but the duty revenue component dominates with a correlation ranging from 0.634 to 0.790, depending on time period. As expected, the nontariff barriers component received the smallest weight. Correlations between the two indices of trade
Table 4. Correlations between Trade Policy 1 and Underlying Components

<table>
<thead>
<tr>
<th></th>
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<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Duty 1970–74</td>
<td>-0.713</td>
<td>-0.703</td>
<td>-0.690</td>
<td>-0.643</td>
</tr>
<tr>
<td>Duty 1975–79</td>
<td>-0.705</td>
<td>-0.733</td>
<td>-0.725</td>
<td>-0.704</td>
</tr>
<tr>
<td>Duty 1980–84</td>
<td>-0.645</td>
<td>-0.673</td>
<td>-0.747</td>
<td>-0.737</td>
</tr>
<tr>
<td>Duty 1985–90</td>
<td>-0.634</td>
<td>-0.654</td>
<td>-0.724</td>
<td>-0.790</td>
</tr>
<tr>
<td>Nontariff barriers</td>
<td>-0.507</td>
<td>-0.501</td>
<td>-0.449</td>
<td>-0.526</td>
</tr>
<tr>
<td>Liberalization 1970–74</td>
<td>0.867</td>
<td>0.862</td>
<td>0.860</td>
<td>0.752</td>
</tr>
<tr>
<td>Liberalization 1975–79</td>
<td>0.851</td>
<td>0.854</td>
<td>0.860</td>
<td>0.733</td>
</tr>
<tr>
<td>Liberalization 1980–84</td>
<td>0.826</td>
<td>0.837</td>
<td>0.850</td>
<td>0.706</td>
</tr>
<tr>
<td>Liberalization 1985–90</td>
<td>0.810</td>
<td>0.818</td>
<td>0.812</td>
<td>0.838</td>
</tr>
</tbody>
</table>

Note: 57 observations.

openness used in this study correlations are always greater than 80 percent (table 5). Although high, this shows that the exclusion of the Sachs and Warner liberalization status from the index can be expected to have some impact on the results.

Summary Statistics for Growth and the Openness Index

Summary statistics for growth and the trade policy index provide preliminary insights into the relationship between them. Tables 6 and 7 display first and second moments for per capita GDP growth and the policy index for five-year averages during 1970–89. The simple contemporaneous correlations between growth and Trade Policy 1 are positive, but their magnitudes are somewhat small, especially for 1975–79, when the oil shock may have negatively affected the relationship between openness and growth (table 7). Furthermore, the simple correlations between growth and Trade Policy 2 are small in magnitude and negative in three out of four periods. Overall these correlations suggest that the relationship between trade policy openness and growth, if any, will be conditional on other determinants of growth.

Measurement of Channel Variables

Three of the channel variables considered in section I—FDI inflows as a share of GDP, government consumption as a share of GDP, and the domestic investment rate—can be captured in fairly uncontroversial ways as far as measurement is concerned.

The other three channels are captured by composite indices or approximated using available data. The quality of macroeconomic policy is captured by an index that gives equal weight to each of three decile rankings of policy characteristics for each country for each time period: level of public debt as a percentage of GDP, level of government deficit as a share of GDP, and growth of M2 net of total real output growth (higher numbers signal better policies). The rankings are averaged to obtain an index of overall macroeconomic policy quality, which
Table 5. Correlation between the Two Trade Policy Indices

<table>
<thead>
<tr>
<th></th>
<th>Trade Policy 1</th>
<th></th>
<th></th>
<th>Trade Policy 2</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Trade Policy 1, 1975-79</td>
<td>0.991</td>
<td>1</td>
<td></td>
<td>0.991</td>
<td>0.991</td>
<td>1</td>
</tr>
<tr>
<td>Trade Policy 1, 1980-84</td>
<td>0.967</td>
<td>0.982</td>
<td>1</td>
<td>0.967</td>
<td>0.982</td>
<td>0.991</td>
</tr>
<tr>
<td>Trade Policy 1, 1985-89</td>
<td>0.908</td>
<td>0.919</td>
<td>0.930</td>
<td>0.908</td>
<td>0.919</td>
<td>0.930</td>
</tr>
<tr>
<td>Trade Policy 2, 1970-74</td>
<td>0.806</td>
<td>0.795</td>
<td>0.758</td>
<td>0.763</td>
<td>0.806</td>
<td>0.795</td>
</tr>
<tr>
<td>Trade Policy 2, 1975-79</td>
<td>0.787</td>
<td>0.805</td>
<td>0.772</td>
<td>0.796</td>
<td>0.787</td>
<td>0.805</td>
</tr>
<tr>
<td>Trade Policy 2, 1980-84</td>
<td>0.763</td>
<td>0.782</td>
<td>0.817</td>
<td>0.846</td>
<td>0.763</td>
<td>0.782</td>
</tr>
<tr>
<td>Trade Policy 2, 1985-89</td>
<td>0.731</td>
<td>0.746</td>
<td>0.785</td>
<td>0.870</td>
<td>0.731</td>
<td>0.746</td>
</tr>
</tbody>
</table>

Note: 57 observations.

reflects a country’s position relative to others. This avoids the problem of having to characterize a “good” macroeconomic policy in absolute terms.25

The extent of technology transmission is approximated by the share of manufactured exports in total merchandise exports, admittedly an imperfect proxy.26 Countries able to compete effectively on world markets for manufactured goods and to produce at world standards are likely to incorporate more of the existing modern technologies in their productive processes. The crucial point is that technological advances and knowledge embodied in existing goods must make their way into production processes to truly qualify as technology transmission. The share of manufactured imports in merchandise imports, another possible measure, was not used because imports of manufactures may act as a substitute rather than a proxy for technology transmission.27

The black market premium on the official exchange rate is used as a measure of price distortions prevailing within the economy, to capture the effect of trade policy on the efficiency of the price system. The black market premium is widely used in cross-country analyses. Barro and Sala-i-Martin (1995) argue that the black market premium on foreign exchange is a widely available and apparently accurate measure of a particular price distortion and can serve as a proxy for government distortions of markets more generally.28

25. The working paper on this study (Wacziarg 1998) presents greater detail on the method used to compute the macroeconomic policy quality index.

26. The share of manufactures in merchandise exports was used as a proxy for technology transmission in World Bank (1996).

27. It attempts to employ the share of manufactured imports to total merchandise imports as a proxy for technology transmission, instead of the share of manufactured exports, no statistically significant relationship was found between this variable and growth on the one hand and trade policy openness on the other, even when controlling for a diverse set of variables.

28. I am grateful to an anonymous referee for pointing out that the black market premium is a component of the Sachs and Warner dummy and that estimates of the coefficient on a variable that includes black market premium in a black market premium equation will be tainted by endogeneity bias. There are three answers to this objection in the context here: first, as stated above, this study employs an indicator based on the Sachs and Warner liberalization dates, rather than their dummy.
Table 6. Summary Statistics for Growth and the Trade Policy Indices

<table>
<thead>
<tr>
<th></th>
<th>Mean</th>
<th>Standard deviation</th>
<th>Minimum</th>
<th>Maximum</th>
</tr>
</thead>
<tbody>
<tr>
<td>Growth 1970–74</td>
<td>3.990</td>
<td>2.520</td>
<td>-0.499</td>
<td>12.351</td>
</tr>
<tr>
<td>Growth 1975–79</td>
<td>2.333</td>
<td>2.845</td>
<td>-6.688</td>
<td>10.433</td>
</tr>
<tr>
<td>Growth 1980–84</td>
<td>0.380</td>
<td>2.740</td>
<td>-8.277</td>
<td>6.018</td>
</tr>
<tr>
<td>Trade Policy 1, 1975–79</td>
<td>-0.937</td>
<td>8.460</td>
<td>-18.716</td>
<td>10.781</td>
</tr>
<tr>
<td>Trade Policy 1, 1980–84</td>
<td>-0.712</td>
<td>8.663</td>
<td>-19.358</td>
<td>10.784</td>
</tr>
<tr>
<td>Trade Policy 2, 1980–84</td>
<td>-8.896</td>
<td>7.188</td>
<td>-30.746</td>
<td>-0.528</td>
</tr>
</tbody>
</table>

Note: 57 observations.

Simple statistics for openness, growth, and the channel variables, averaged over the period under consideration, provide preliminary evidence of the relevance of the choice of channels (tables 8 and 9). Unconditional correlations suggest that all of the channels involve a positive effect of trade on economic growth (first column of table 9). The largest correlations are in the investment and manufactured exports channels. Overall, this shows that the trade policy index is positively related to FDI as a share of GDP, macroeconomic policy quality, manufactured exports as a share of merchandise exports, and the domestic investment ratio. Each of these is positively correlated with growth. Trade policy openness is negatively related to the black market premium and government size, and each of these is negatively associated with growth. Although these simple correlations are suggestive, results obtained when controlling for other determinants of growth and the channel variables are likely to differ greatly.

III. Empirical Results

Table 10 reports summary effects of each channel on growth, the effect of openness on each channel, and the product of the two coefficients for the baseline model for 57 countries for 1970–89.29 Trade policy openness works positively
for growth through five out of six channels, three of which—investment, FDI, and macroeconomic policy quality—involve statistically significant effects at the 90 percent level. In each case these involve a positive effect of trade policy on the channel variable and a positive effect of the channel variable on growth.

The remaining channel estimates are statistically insignificant at the 90 percent level, although government size comes close to being a significantly negative channel (the $p$-value associated with the $t$-statistic on the channel effect is 13 percent). For price distortions, this is due to the absence of a significant effect of trade openness on the black market premium once other determinants of this variable (such as per capita income) are held fixed. However, the black market premium was found to bear a negative relationship to economic growth. For manufactured exports, the absence of a statistically significant overall channel effect is due to the fact that this variable does not seem to affect growth in the model specification. However, trade openness was found to be positively associated with the share of manufactured exports in total exports.

The overall effect of all the channels is significant at the 99 percent level. The magnitude of the effects is small for some channels: reduced distortions account for roughly 3 percent of the net effect of trade policy openness on growth and are statistically insignificant due to the absence of a significant estimated effect of trade policy on the black market premium. This is a surprising result in light of the importance accorded allocative efficiency in arguments about static and dynamic gains from trade. The same holds for manufactured exports, meant to capture technology transmission. Government size works negatively for growth,
although the effect is weak for both magnitude and significance. Differences in the quality of macroeconomic policy and in the ratio of FDI to GDP appear to be relatively important channels, each accounting for roughly 20 percent of the total effects of trade policy on growth.

The most important channel by far is investment rate, which accounts for close to 63 percent of the total effect of trade policy on growth. Several theoretical arguments point to the potential direct impact of trade policy openness on investment, such as those outlined in section I. It is also possible that measurement error in some of the channel variables leads to overstatement of the effect of trade policy through the investment rate. For instance, if investment is positively correlated with technology transmission and if the share of manufactured exports in total merchandise exports is a weak proxy for the extent of technology transmission, part of this effect may be accounted for by the investment channel. However, the scope of this argument is somewhat limited by the use of a wide set of instruments for all of the channel variables: if measurement errors in the instruments are independent of measurement errors in the channel variables, attenuation bias will be reduced.

To summarize, this model provides evidence for a beneficial total effect of trade policy on growth. An 8.5 percentage point increase in the trade policy measure, corresponding roughly to one standard deviation, is associated with a 0.601 percentage point increase in the annual growth rate once all channels of influence are brought into the picture. This effect is estimated with great precision. The most important channel by far seems to be through investment (63 percent of the total effect). Technology transmission explains 22.5 percent of the overall positive effect of trade on growth; macroeconomic policy quality accounts for

30. This is in line with empirical results in Baldwin and Seghezza (1996a) and Levine and Renelt (1992), who found evidence of trade-induced, investment-led growth.
### Table 9. Correlation Matrix for Main Variables

<table>
<thead>
<tr>
<th></th>
<th>Growth</th>
<th>Trade Policy 1</th>
<th>Macro policy quality</th>
<th>Black market premium</th>
<th>Government consumption</th>
<th>Manufactured exports</th>
<th>Investment rate</th>
<th>FDI</th>
<th>Human capital</th>
</tr>
</thead>
<tbody>
<tr>
<td>Trade Policy 1</td>
<td>0.331</td>
<td>1</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Macro policy quality</td>
<td>0.384</td>
<td>0.420</td>
<td>1</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Black market premium</td>
<td>-0.408</td>
<td>-0.404</td>
<td>-0.304</td>
<td>1</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Government consumption</td>
<td>-0.421</td>
<td>-0.265</td>
<td>-0.594</td>
<td>0.390</td>
<td>1</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Manufactured exports</td>
<td>0.387</td>
<td>0.602</td>
<td>0.393</td>
<td>-0.484</td>
<td>-0.268</td>
<td>1</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Investment rate</td>
<td>0.483</td>
<td>0.674</td>
<td>0.441</td>
<td>-0.498</td>
<td>-0.428</td>
<td>0.556</td>
<td>1</td>
<td></td>
<td></td>
</tr>
<tr>
<td>FDI</td>
<td>0.503</td>
<td>0.263</td>
<td>0.155</td>
<td>-0.255</td>
<td>-0.296</td>
<td>-0.012</td>
<td>0.342</td>
<td>1</td>
<td></td>
</tr>
<tr>
<td>Human capital</td>
<td>0.185</td>
<td>0.554</td>
<td>0.361</td>
<td>-0.357</td>
<td>-0.334</td>
<td>0.487</td>
<td>0.522</td>
<td>0.116</td>
<td>1</td>
</tr>
<tr>
<td>Log income</td>
<td>0.266</td>
<td>0.743</td>
<td>0.469</td>
<td>-0.530</td>
<td>-0.504</td>
<td>0.648</td>
<td>0.754</td>
<td>0.188</td>
<td>0.750</td>
</tr>
</tbody>
</table>

*Note: 57 observations.*
Table 10. Summary of the Channel Effects Using Trade Policy 1

<table>
<thead>
<tr>
<th>Channel variable</th>
<th>Effect of channel on growth</th>
<th>Effect of channel on openness</th>
<th>Effect of Trade Policy 1 on growth</th>
</tr>
</thead>
<tbody>
<tr>
<td>Price distortions</td>
<td>-0.0066</td>
<td>-0.3445</td>
<td>0.0023</td>
</tr>
<tr>
<td></td>
<td>(-9.08)</td>
<td>(-0.63)</td>
<td>(0.63)</td>
</tr>
<tr>
<td>Government consumption</td>
<td>-0.0425</td>
<td>0.1539</td>
<td>-0.0065</td>
</tr>
<tr>
<td></td>
<td>(-1.57)</td>
<td>(3.73)</td>
<td>(-1.52)</td>
</tr>
<tr>
<td>Manufactured exports</td>
<td>0.0036</td>
<td>0.6345</td>
<td>0.0023</td>
</tr>
<tr>
<td></td>
<td>(0.45)</td>
<td>(4.59)</td>
<td>(0.45)</td>
</tr>
<tr>
<td>Investment rate</td>
<td>0.1425</td>
<td>0.3173</td>
<td>0.0452</td>
</tr>
<tr>
<td></td>
<td>(6.86)</td>
<td>(6.72)</td>
<td>(5.12)</td>
</tr>
<tr>
<td>FDI</td>
<td>0.3203</td>
<td>0.0450</td>
<td>0.0144</td>
</tr>
<tr>
<td></td>
<td>(4.68)</td>
<td>(4.01)</td>
<td>(3.79)</td>
</tr>
<tr>
<td>Macro policy quality</td>
<td>0.4887</td>
<td>0.0267</td>
<td>0.0130</td>
</tr>
<tr>
<td></td>
<td>(4.22)</td>
<td>(2.19)</td>
<td>(1.90)</td>
</tr>
<tr>
<td>Total effect</td>
<td></td>
<td></td>
<td>0.0707</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(5.94)</td>
</tr>
</tbody>
</table>

Note: Numbers in parentheses are t-statistics based on heteroscedastic-consistent (White robust) standard errors.

18 percent of the effect. The only negative channel, government size, is significant at the 87 percent level only.

Robustness Analysis

The model was tested for robustness to the choice of liberalization indicator, to model specification, and to time coverage.

Excluding the Sachs and Warner indicator. Table 11 replicates the estimation of the baseline model replacing Trade Policy 1 with Trade Policy 2 as a measure of openness. Trade Policy 2 excludes the Sachs and Warner liberalization status variable critiqued by Rodrik and Rodríguez (2000). The magnitude and precision of the overall estimated channel effects fall, although the investment effect and the overall effect are still positive and statistically significant. A one-standard-deviation change in Trade Policy 2 (8 percentage points) is now associated with a 0.264 increase in the annual growth rate of per capita GDP. The proportional contributions of most channels remains roughly unchanged, with the investment channel accounting for the bulk of the effect.

The main change in the channel effects is the disappearance of the macroeconomic policy quality channel, now statistically indistinguishable from zero. This is due entirely to the fact that Trade Policy 2 now bears no relationship to the

31. Referring explicitly to the earlier working paper on this study (Wacziarg 1998), they state that “we are skeptical that the Sachs-Warner measure, on which the Wacziarg indicator is partly based, is a meaningful indicator of trade policy. . . . We would have preferred to see estimates based only on tariff and [nontariff barrier] indicators." I am grateful to them and to anonymous referees for this suggestion.
TABLE 11. Summary of the Channel Effects (Using Trade Policy 2)

<table>
<thead>
<tr>
<th>Channel variable</th>
<th>Effect of channel on growth</th>
<th>Effect of openness on channel</th>
<th>Effect of Trade Policy 2 on growth</th>
</tr>
</thead>
<tbody>
<tr>
<td>Price distortions</td>
<td>-0.0068 (-9.63)</td>
<td>0.4886 (0.65)</td>
<td>-0.0033 (-0.65)</td>
</tr>
<tr>
<td>Government consumption</td>
<td>-0.0497 (-1.69)</td>
<td>0.2030 (6.14)</td>
<td>-0.0101 (-1.60)</td>
</tr>
<tr>
<td>Manufactured exports</td>
<td>0.0033 (0.41)</td>
<td>-0.0653 (-0.52)</td>
<td>-0.0002 (-0.33)</td>
</tr>
<tr>
<td>Investment rate</td>
<td>0.1365 (6.09)</td>
<td>0.2086 (4.39)</td>
<td>0.0285 (3.67)</td>
</tr>
<tr>
<td>FDI</td>
<td>0.3066 (4.38)</td>
<td>0.0805 (5.41)</td>
<td>0.0247 (4.22)</td>
</tr>
<tr>
<td>Macro policy quality</td>
<td>0.4989 (4.13)</td>
<td>-0.0129 (-0.87)</td>
<td>-0.0064 (-0.83)</td>
</tr>
<tr>
<td>Total effect</td>
<td></td>
<td>0.0331 (2.50)</td>
<td></td>
</tr>
</tbody>
</table>

Note: Numbers in parentheses are t-statistics based on heteroscedastic-consistent (White robust) standard errors.

index of macroeconomic policy. This is consistent with the Rodrik and Rodríguez (2000) critique of the Sachs and Warner indicator for proxying distorted domestic policies, which may not be the case for the other measures of trade openness used to construct the index. The result is also consistent with an alternative view, more favorable to the baseline model: that the Sachs and Warner liberalization dates reflect a component of a country’s trade orientation that is only weakly related to direct measures of trade policy, such as tariffs and nontariff barriers, but is nonetheless causally linked to the quality of macroeconomic policy.

Robustness to the Specification. Table 12 displays several modifications of the baseline model to examine its sensitivity to changes in specification and estimation method. It presents t-statistics and Wald tests for the significance of the products of coefficients. The Wald statistics are asymptotically distributed as $\chi^2$ variables with 1 degree of freedom. As the table shows, the p-values implied by the t-tests and those obtained from the Wald tests are very similar. Figure 1 displays the six channels graphically, for each of the five models in table 12.

The third column examines the robustness of the model with respect to estimation method, employing the seemingly unrelated regression estimator. This estimator, though inconsistent (no instruments are used), is characterized by greater efficiency and may provide some indication of the model’s robustness. It shows that changing the estimator does not greatly affect the sign or magnitude of the estimated effects. In fact, the overall effect of trade policy is roughly the same as in the baseline model.

The fourth column restricts the sample to developing countries. The magnitude of the effect of Trade Policy 1 on economic growth increases when the sample
<table>
<thead>
<tr>
<th></th>
<th>Base model 1970–89</th>
<th>Using Trade Policy 2</th>
<th>Seemingly unrelated regression estimates</th>
<th>Developing economies</th>
<th>Regional dummy variables</th>
</tr>
</thead>
<tbody>
<tr>
<td>Distortions</td>
<td>0.0023</td>
<td>-0.0033</td>
<td>0.0046</td>
<td>0.0090</td>
<td>0.0072</td>
</tr>
<tr>
<td>(p-value)</td>
<td>(0.63)</td>
<td>(-0.65)</td>
<td>(1.73)</td>
<td>(2.51)</td>
<td>(1.71)</td>
</tr>
<tr>
<td>Wald test</td>
<td>0.3986</td>
<td>0.4222</td>
<td>2.9825</td>
<td>6.3154</td>
<td>2.9240</td>
</tr>
<tr>
<td>(p-value)</td>
<td>(0.53)</td>
<td>(0.52)</td>
<td>(0.08)</td>
<td>(0.01)</td>
<td>(0.09)</td>
</tr>
<tr>
<td>Government consumption</td>
<td>-0.0065</td>
<td>-0.0101</td>
<td>-0.0044</td>
<td>-0.0009</td>
<td>-0.0107</td>
</tr>
<tr>
<td>(p-value)</td>
<td>(-1.52)</td>
<td>(-1.60)</td>
<td>(-1.57)</td>
<td>(-1.14)</td>
<td>(-1.93)</td>
</tr>
<tr>
<td>Wald test</td>
<td>2.3087</td>
<td>2.5534</td>
<td>2.4766</td>
<td>1.2913</td>
<td>3.7085</td>
</tr>
<tr>
<td>(p-value)</td>
<td>(0.13)</td>
<td>(0.11)</td>
<td>(0.12)</td>
<td>(0.26)</td>
<td>(0.05)</td>
</tr>
<tr>
<td>Manufactured exports</td>
<td>0.0023</td>
<td>-0.0002</td>
<td>0.0049</td>
<td>-0.0023</td>
<td>-0.0025</td>
</tr>
<tr>
<td>(p-value)</td>
<td>(0.45)</td>
<td>(-0.33)</td>
<td>(1.11)</td>
<td>(-1.00)</td>
<td>(-0.70)</td>
</tr>
<tr>
<td>Wald test</td>
<td>0.2011</td>
<td>0.1103</td>
<td>1.2284</td>
<td>0.9943</td>
<td>0.4901</td>
</tr>
<tr>
<td>(p-value)</td>
<td>(0.65)</td>
<td>(0.74)</td>
<td>(0.27)</td>
<td>(0.32)</td>
<td>(0.48)</td>
</tr>
<tr>
<td>Investment rate</td>
<td>0.0452</td>
<td>0.0285</td>
<td>0.0326</td>
<td>0.0394</td>
<td>0.0222</td>
</tr>
<tr>
<td>(p-value)</td>
<td>(5.12)</td>
<td>(3.67)</td>
<td>(4.37)</td>
<td>(5.20)</td>
<td>(3.54)</td>
</tr>
<tr>
<td>(p-value)</td>
<td>(0.00)</td>
<td>(0.00)</td>
<td>(0.00)</td>
<td>(0.00)</td>
<td>(0.00)</td>
</tr>
<tr>
<td>Foreign direct investment</td>
<td>0.0144</td>
<td>0.0247</td>
<td>0.0129</td>
<td>0.0231</td>
<td>0.0101</td>
</tr>
<tr>
<td>(p-value)</td>
<td>(3.79)</td>
<td>(4.22)</td>
<td>(3.46)</td>
<td>(4.90)</td>
<td>(2.37)</td>
</tr>
<tr>
<td>Wald test</td>
<td>14.3848</td>
<td>17.8191</td>
<td>11.9667</td>
<td>24.0583</td>
<td>5.6374</td>
</tr>
<tr>
<td>(p-value)</td>
<td>(0.00)</td>
<td>(0.00)</td>
<td>(0.00)</td>
<td>(0.00)</td>
<td>(0.02)</td>
</tr>
<tr>
<td>Macro policy quality</td>
<td>0.0130</td>
<td>-0.0064</td>
<td>0.0161</td>
<td>0.0169</td>
<td>0.0040</td>
</tr>
<tr>
<td>(p-value)</td>
<td>(1.90)</td>
<td>(-0.83)</td>
<td>(2.84)</td>
<td>(3.36)</td>
<td>(1.18)</td>
</tr>
<tr>
<td>Wald test</td>
<td>3.6089</td>
<td>0.6917</td>
<td>8.0783</td>
<td>11.2935</td>
<td>1.4016</td>
</tr>
<tr>
<td>(p-value)</td>
<td>(0.06)</td>
<td>(0.41)</td>
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<tr>
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<td>0.0853</td>
<td>0.0303</td>
</tr>
<tr>
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<td>(5.73)</td>
<td>(7.85)</td>
<td>(2.38)</td>
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<tr>
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</table>

Note: Numbers in parentheses are t-statistics based on heteroscedastic-consistent (White robust) standard errors.

... is restricted to developing economies. This is due to the fact that the distortions channel is now significant and represents roughly 10 percent of the overall effect. The other channels are preserved.

The last column shows the results of adding regional dummy variables to every equation to account for time-invariant region-specific effects that can covary with the right-hand-side variables. To account for the possibility that regional specificities might be the driving force of the results, regional dummy variables for Latin America, Sub-Saharan Africa, Southeast Asia, and coun-
tries in the Organisation for Economic Co-operation and Development (OECD) were added to each of the channel equations and to the list of instruments. Because accounting for fixed effects tends to wipe out much of the cross-sectional variation (a fixed-effects estimator uses only the variation within regions across time), the inclusion of regional dummy variables would be expected to lower the estimated effects of trade policy. The total effect of trade policy is reduced by the inclusion of region-specific dummy variables, but each channel’s shares are roughly preserved. In particular, the dominant role of physical capital formation is maintained, and the overall effect remains statistically significant.

**Robustness to Time Coverage.** Three issues related to the study’s time coverage were also examined (table 13). First, the cross-equation parameter equality restrictions may not be warranted. Second, a wider time span, though reducing the number of countries for which the data are available, might provide a further robustness check on the results. Third, the use of five-year averages, though increasing the number of data points in the estimation, may highlight short-term variability in the data (due, for example, to business cycle effects) and obscure the long-run relationships.32

32. Rodrik and Rodriguez (2000), referring to this article, state that “we are not sure that the regularities revealed by the data over time horizons of five years or less are particularly informative about the relationship between trade policy and long-run economic performance. It would be interesting to see if the results hold up with averages constructed over a decade or more.” I am grateful to them for this suggestion.
<table>
<thead>
<tr>
<th></th>
<th>I</th>
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<th>III</th>
<th>IV</th>
<th>V</th>
<th>VI</th>
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<td>Distortions</td>
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<td>0.0008</td>
<td>0.0045</td>
<td>0.0431</td>
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<td>(-1.15)</td>
<td>(-0.37)</td>
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<td>(0.07)</td>
<td>(7.28)</td>
<td>(1.78)</td>
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<tr>
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<td>0.1332</td>
<td>1.6789</td>
<td>0.0052</td>
<td>53.0421</td>
<td>3.1682</td>
</tr>
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<td>(p-value)</td>
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<td>(0.72)</td>
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<td>(0.94)</td>
<td>(0.00)</td>
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<td>-0.0066</td>
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<td>(0.62)</td>
<td>(-1.53)</td>
<td>(-5.85)</td>
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<td>(0.60)</td>
<td>(0.12)</td>
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<td>Investment rate</td>
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<td>0.0206</td>
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<td>0.1078</td>
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<td>(p-value)</td>
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<td>(0.07)</td>
<td>(0.04)</td>
<td>(0.00)</td>
<td>(0.00)</td>
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<td>0.0040</td>
<td>0.0118</td>
<td>0.0155</td>
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<td>(1.17)</td>
<td>(2.43)</td>
<td>(2.41)</td>
<td>(6.02)</td>
<td>(2.23)</td>
</tr>
<tr>
<td>Wald test</td>
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<td>1.3678</td>
<td>5.9239</td>
<td>5.8298</td>
<td>36.2359</td>
<td>4.9735</td>
</tr>
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<td>(0.01)</td>
<td>(0.02)</td>
<td>(0.00)</td>
<td>(0.03)</td>
</tr>
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<td>Macro policy quality</td>
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<td>0.0009</td>
<td>0.0076</td>
<td>0.0089</td>
<td>0.0111</td>
<td>-0.0258</td>
</tr>
<tr>
<td>(p-value)</td>
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<td>(0.78)</td>
<td>(1.08)</td>
<td>(4.24)</td>
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<td>17.9800</td>
<td>1.2340</td>
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<tr>
<td>(p-value)</td>
<td>(0.05)</td>
<td>(0.91)</td>
<td>(0.43)</td>
<td>(0.28)</td>
<td>(0.00)</td>
<td>(0.27)</td>
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<tr>
<td>Total effect</td>
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<td>0.1078</td>
<td>0.0612</td>
<td>0.0595</td>
<td>0.0459</td>
<td>0.1890</td>
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<td>(3.87)</td>
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<td>(2.53)</td>
<td>(11.71)</td>
<td>(3.25)</td>
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<td>6.3990</td>
<td>137.2148</td>
<td>10.5605</td>
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<td>(p-value)</td>
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<td>(0.00)</td>
<td>(0.00)</td>
<td>(0.01)</td>
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<td>(0.00)</td>
</tr>
</tbody>
</table>

**Note:** Numbers in parentheses are t-statistics based on heteroscedastic-consistent (White robust) standard errors.
First, each of the four time periods was excluded from the baseline model one at a time (columns one to four in table 13). This should greatly reduce the precision of the parameter estimates, because a quarter of the data is being excluded. Indeed, the t-statistics on most of the channel effects are considerably lower when only three time periods are used for estimation. For example, the macroeconomic policy and government size channels are no longer statistically significant. However, both the signs and magnitudes of the estimates are remarkably close to those in the baseline model. The investment effect is preserved in all specifications, and in all but one case the overall effect of trade policy remains of the same magnitude.

When the timespan is widened by adding 1990–92, distortions and government size become statistically significant channels, although still relatively small in magnitude. The addition of this time period reduces the number of observations from 57 to 50, while raising the number of data points used to estimate each parameter, thus improving the precision of the estimates. The signs and relative magnitudes of most of the effects are maintained. The reduction in the overall effect, from 0.71 to 0.46, is almost entirely due to a reduction in the magnitude of the investment channel.

With respect to the third issue, results are quite robust when 10-year averages of the data are used rather than 5-year averages (last column of table 13). In particular, the investment channel remains statistically significant and still accounts for over half the total effect of trade policy on economic growth. Moreover, the total effect is more than double the previous magnitude, although as expected it is estimated with lower precision. One interpretation of the increase in magnitude is that data averaged over 5 years reflect to some extent short-term variability in the data, analogous to measurement error, whereas data averaged over 10 years are more likely to reflect long-term relationships.

Exhaustiveness of the Model

The last concern is whether the six channels fully capture the total effect of trade policy on growth. The omission of one or more channels could lead to an incomplete characterization of the effects of trade policy and to potential biases in the estimates of the included channels (insofar as the omitted channel variables covary with the included ones in the growth regression).

Other Possible Channels

Among other possible channels for the effect of trade policy on growth, this study looked briefly at human capital, income inequality, and corruption.

HUMAN CAPITAL. The accumulation of human capital might be one of the channels linking trade policy and economic growth. If trade openness modifies the relative returns to factors, it may create greater incentives to accumulate human

---

33. Furthermore, the exogenous variables corresponding to the excluded period were removed from the list of instruments.
capital. For instance, if an open trade policy spurs technology transmission and if technology and skills are complements, then trade openness will increase the returns to accumulating human capital. However, no significant linkage effect was found when a human capital channel was specified: the coefficient on the trade policy variable was essentially zero once other determinants of human capital formation, such as per capita income, were held constant. This was robust with respect to the inclusion of a diverse set of controls. Furthermore, the effects of human capital on growth are not robust in the model’s growth specification, a problem compounded by the opposite signs of male and female human capital.\(^{34}\) Hence, human capital did not appear to be an important channel linking trade policy and growth.

**Income inequality.** Neoclassical trade theory provides several tools for the analysis of income distribution in relation to trade openness. The simple factor endowments theory of Hecksher-Ohlin-Samuelson predicts that returns to unskilled labor should increase in relative terms, with presumed positive effects on income distribution, when a relatively unskilled labor-abundant country moves from autarky to free trade. There are reasons to believe that inequality has an effect on growth, although the direction of this effect appears a priori ambiguous. Alesina and Perotti (1996), among others who have studied the issue of distribution and growth, argue that when the poor have a larger weight in political decisionmaking, they tend to vote for transfer schemes that involve distortive (growth-reducing) taxation. Empirically, they report that more unequal societies tend to display lower growth rates, once other determinants of growth are held constant. However, including a measure of income inequality (the Gini coefficient) in the basic growth regression gave rise to an insignificant effect. Furthermore, the effect of trade policy on income inequality, controlling for the level of per capita income, was found to be essentially zero. Hence, the income inequality channel does not appear to operate either, although the poor quality of cross-country inequality data may be the source of this result.

**Corruption.** Ades and Di Tella (1999) show convincingly that enhanced openness to international trade may limit corruption by increasing the degree of internal market competition and reducing opportunities for local bureaucrats to demand bribes. Mauro (1995) provides evidence that corruption has adverse effects on growth. When Mauro’s data (from Business International) were included, however, there was evidence of an effect of trade openness on corruption, but the effect of corruption on growth, though negative, was insignificant and not robust to alternative specifications. This may be due to the fact that the

\(^{34}\) This is consistent with estimates in Barro and Sala-i-Martin (1995) and Pritchett (1996b). Pritchett (1996b, 1) documents that “cross-national data on economic growth rates show that increases in educational capital resulting from improvements in the educational attainment of the labor force have had no positive impact on the growth rate of output per worker.”
index of corruption, based on survey methods (and hence likely subject to measurement error), was entered in the growth regression along with government size, the black market premium, and the quality of macroeconomic policy, each of which may proxy in part for corruption. Adding the corruption channel also resulted in a loss of degrees of freedom. The corruption data used in Mauro are available only for the 1980s, forcing abandonment of two of four time periods from the estimation and a loss of five countries.\footnote{35. Results for the income inequality, human capital, and corruption channels are available from the author on request.}

**Unconditional Effect of Trade Policy Openness**

Further evidence of the model’s exhaustiveness is provided by comparing the total effect under the channel methodology with the unconditional effect of trade policy on growth obtained by removing all of the channel variables from the growth regression and using only the trade policy index. The resulting estimates suggest a strong association between the trade regime and growth: a 10 percentage point increase in the trade policy index is associated with a 0.66 percentage point increase in the annual growth rate in the baseline model (table 14).

With the exclusion of many variables from the growth equation, the trade policy index captures much of their effect on growth that is not necessarily linked to trade policy. However, this coefficient is useful in that it provides a rough order of magnitude against which to compare the total effect of trade policy computed above. Indeed, in all five models, the unconditional effect of trade policy is roughly of the same magnitude as the total effect of trade policy computed earlier.

**Tests Based on the Residuals from the Growth Equation**

A more formal test of exhaustiveness can be carried out by regressing the residual vector obtained from the system estimates of the growth regression on the index of trade policy. A correlation between the estimated residual and the measure of trade openness could indicate that a significant channel has been left out of the growth regression. The results, based on a seemingly unrelated regression estimator, show that this is not the case (table 15).\footnote{36. Again, this should not be taken as an absolute proof of exhaustiveness. To the extent that potentially omitted channels covary with the included ones, the included variables will pick up the effects of trade policy that should be accounted for by the missing channels; this would be reflected by a lower correlation between the growth residual and trade policy openness. However, this test provides yet another indication that no major channel has been omitted.} In most of the models, the residual effect of trade policy is generally positive but not significantly different from zero at any reasonable level of significance. This again reinforces confidence in the exhaustiveness of the model. That the estimate is positive in the baseline model shows, if anything, that the channel methodology has uncovered a lower bound on the total effect of trade openness. In all cases, the residual effect is statistically insignificant.
TABLE 14. Unconditional Effect of Trade Policy in the Growth Equation

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<th></th>
<th>Baseline 1970–89</th>
<th>1970–92</th>
<th>Seemingly unrelated regression estimates</th>
<th>Developing economies</th>
<th>Regional dummy variables</th>
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<td>(2.24)</td>
<td>(2.34)</td>
<td>(1.13)</td>
<td>(1.61)</td>
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<td></td>
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<td>(0.38)</td>
<td>(-1.12)</td>
<td>(0.03)</td>
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<td>Male human capital</td>
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<td>0.9481</td>
<td>0.6709</td>
<td>1.8925</td>
<td>-0.2851</td>
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<td></td>
<td>(2.11)</td>
<td>(5.30)</td>
<td>(2.18)</td>
<td>(13.54)</td>
<td>(-0.92)</td>
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<td>Female human capital</td>
<td>-0.9261</td>
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<td></td>
<td>(-3.04)</td>
<td>(-8.02)</td>
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<td>East Asia dummy variable</td>
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<td>Sub-Saharan Africa dummy variable</td>
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### Note:
Numbers in parentheses are t-statistics based on heteroscedastic-consistent (White robust) standard errors. Because each equation is estimated for four time periods, with estimated parameters constrained to equality across periods, the table reports $R^2$ statistics corresponding to each of these time periods. Five time periods were reported for 1970–92.

IV. Conclusion

This article is a first attempt, in a cross-country context, to evaluate empirically various theories of dynamic gains from trade in explaining the observed positive impact of trade openness on economic growth. Trade openness affects growth mainly by raising the ratio of domestic investment to GDP. Depending on the specification, the rate of physical capital accumulation explains between 46 percent and 63 percent of the impact of trade policy on economic growth. FDI, as a proxy for technology transmission, and the quality of macroeconomic policies each account for roughly 20 percent of the overall effect. There is also weak evidence that the size of government, measured by the ratio of public consumption to GDP, constitutes a channel through which trade policy affects economic growth negatively.

The lack of statistically significant results for manufactured exports and price distortions may be due to measurement problems. Measurement, although improving on past attempts, is still subject to considerable shortcomings. The black
market premium may be a weak proxy for the overall efficiency of the price system. International technology transmission is extremely hard to measure as well, perhaps downwardly biasing estimates for this channel and overstating the others. Future research should seek to improve on the measures used in this study.

The important role of investment in physical capital poses a theoretical challenge. Some theories about gains from trade predict positive effects of openness on the rate of return to capital, but some of these effects should be captured either by the price distortions or technological transmission channel. Furthermore, theories based on dynamic gains from technology transmission and efficiency improvement focus on the improvement of the overall productivity of factors, rather than on accelerated accumulation. If specialization is limited by the extent of the market, under increasing returns to scales trade openness should allow entrepreneurs to undertake previously unprofitable investments. Similarly, if trade liberalization involves procompetitive effects, the entry of new firms may entail large fixed capital costs. Applying such theories to the study of the growth effects of trade openness may provide useful insights into the nature of dynamic gains from trade.

### References


APPENDIX A.

TABLE A-1. List of Countries

<table>
<thead>
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<th>OECD</th>
<th>Asia</th>
<th>Latin America</th>
<th>Africa</th>
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<td>United Kingdom</td>
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</table>

APPENDIX B. DATA SOURCES AND DESCRIPTION


Import duties as a percentage of total imports. Source: IMF. Description: Import duties in local currency as a percentage of total imports in local currency (percentage points).

Pre-Uruguay Round nontariff barrier coverage. Source: UNCTAD. Description: Coverage rate of nontariff barriers pre-Uruguay Round (percentage points).

Sachs and Warner liberalization status. Source: Sachs-Warner (1995). Description: For each year, a dummy variable was constructed based on the years of liberalization in Sachs and Warner (1995). Takes a value of 1 for liberalized countries and 0 for closed countries. The data were averaged over the relevant five-year subperiods.


FDI ratio. Source: IMF. Description: Ratio of gross foreign direct investment inflows to GDP (percentage points).

Democracy. Source: Gastil (Freedom in the World Reports). Description: Index of how democratic institutions are (regular elections, broad franchise, wide access to office, and relevance of elected officials). Takes values from 0 (nondemocracy) to 1 (country with fully developed democratic institutions).

Human capital. Source: Barro and Lee (1993). Description: Average years of secondary and higher education in the total population over age 25.

Secondary school completion rate. Source: Barro and Lee (1993). Description: Percentage of the total population that has completed secondary school.

Macroeconomic policy quality. Source: World Bank and IMF. Description: Index of macroeconomic policy quality. Constructed by ranking countries according to their public debt to GDP ratio, deficit to GDP ratio, and growth of M1 net of total output growth and assigning values from 1 to 10 to each decile, then averaging the three resulting indicators. Index also ranges from 1 to 10. Higher numbers signal better policies.

Black market premium. Source: Tavares and Wacziarg (2001) data set, initially World Currency Yearbook and IMF. Description: Black market premium on the official exchange rate (black market rate minus official rate/official rate as a percentage).


Population over 65. Source: Barro and Lee (1994). Description: Share of population aged over age 65 in the total population (percent).


Ethnolinguistic fractionalization. Source: Mauro (1994). Description: Probability that two randomly selected people from a given country will not belong to the same ethnolinguistic group.

Postwar independence. Source: Barro and Lee (1994). Description: Takes on a value of 1 if the country gained independence after World War II and 0 otherwise.
### Table C-1. Baseline Specification of the Structural System (57 countries, 1970–89)

<table>
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<tr>
<th></th>
<th>Growth</th>
<th>Openness</th>
<th>Distortions</th>
<th>Government Consumption</th>
<th>Manufactured Exports</th>
<th>Investment</th>
<th>FDI</th>
<th>Macro policy quality</th>
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Note: Numbers in parentheses are t-statistics based on heteroscedastic-consistent (White robust) standard errors. Because each equation is estimated for four time periods, with estimated parameters constrained to equality across periods, the table reports $R^2$ statistics corresponding to each of these time periods.

Source: See Appendix B.
Ownership and Growth

*Thorvaldur Gylfason, Tryggvi Thor Herbertsson, and Gylfi Zoega*

This article suggests how state enterprises can be incorporated into the theoretical and empirical growth literature. Specifically, it shows that if state enterprises are less efficient than private firms, invest less, employ less skilled labor, and are less eager to adopt new technology, then a large state enterprise sector tends to be associated with slow economic growth, all else remaining the same. The empirical evidence for 1978–92 indicates that, through a mixture of these channels, an increase in the share of state enterprises in employment by one standard deviation could reduce per capita growth by one to two percentage points a year from one country to another.

The debate over private versus public enterprise has played an important part in the history of economic ideas and of the world. State ownership of all factors of production was a cornerstone of communism, as practiced in the former Soviet Union and its satellites. Even under capitalism, the state (especially European states) has sometimes been deeply involved in economic affairs. The state in developing economies has been particularly inclined to take a prominent role in producing goods and services and allocating resources to investment and other economic needs.

Despite valiant efforts by many governments in recent years to get bureaucrats out of business, state enterprises remain prominent around the world. The unweighted average share of state enterprises in nonagricultural economic activity in 40 developing economies reporting to the World Bank (1995, table A2) was 13 percent in 1991, the same as in 1978. The comparable figure for eight industrial countries in 1988 was 7 percent, down from 8 percent in 1979. The unweighted average share of state enterprises in gross domestic investment in 55 developing countries was 18 percent in 1991, down from 23 percent in 1978. For 10 industrial countries it was 11 percent in 1988 and 13 percent in 1978.

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I. A Model with State Enterprises

Recent worldwide interest in privatization derives, in part, from empirical evidence that seems to indicate that private enterprise is generally more efficient than state enterprise. This evidence was reviewed in detail in World Bank (1995). Phelps (1993) provides a useful classification by suggesting five main reasons for the superior efficiency of private enterprise. Private firms may be more entrepreneurial. Managers of private firms may find it easier to act on their intuition about what products or production processes will be successful. State enterprises may be more susceptible to pressure from interest groups, whereas private firms can focus solely on maximizing profits. Private investors generally have a longer time horizon for acquiring assets that can be sold, whereas politicians’ electoral assets tend to be more fleeting. Last, private firms may have more difficulty getting public assistance, so the penalty for failing to maximize profits is harsher, though the fruits of success may also be sweeter. In his presidential address at the 110th meeting of the American Economic Association, Arnold Harberger (1998, 23) airs similar views: “In most countries state-owned enterprises operate under a series of constraints that seriously get in the way of real cost minimization in a comparative-static sense and real cost reduction in a dynamic sense.”

Even so, several empirical studies have reported mixed evidence of the relative efficiency of public and private firms (see Stiglitz 1988 for a review of this evidence). The dearth of unambiguous empirical evidence is not surprising in view of the long-standing debate on the relative merits of public and private enterprise, especially when the inefficiency that can arise from principal-agent (owner-manager) relations in private industry is taken into consideration. The public sector has no monopoly on inefficiency in production.

But if transferring state property to more productive uses in the private sector enhances efficiency, by replacing soft budget constraints with harder ones, for example, then the composition of corporate ownership would be expected to play a role in generating and sustaining long-term economic growth. This is a direct implication of the theory of endogenous growth: virtually anything that increases static efficiency stimulates growth. This result follows particularly clearly from endogenous growth models featuring constant returns to capital (the so-called AK model where $A$ denotes the output to capital ratio, which may be viewed as a measure of macroeconomic efficiency, and $K$ denotes capital stock).

In these models, the long-run rate of growth of output per head equals the multiple of the saving rate, $s$, and efficiency, $A$, less the depreciation rate, $\delta$: $g = sA - \delta$. Any policy undertaking or external event that increases static efficiency by increasing the amount of output that can be made from given capital thus also increases the rate of economic growth, permanently. In the neoclassical theory of economic growth, increasing efficiency increases economic growth, possibly for a long time,

1. Important contributions to this debate include Vickers and Yarrow (1988), Laffont and Tirole (1993), and Stiglitz (1994), among others.
but eventually the rate of growth returns to its exogenously determined initial equilibrium value. Either way, this link between efficiency and growth explains why, for example, education is good for growth. It also explains why liberalization, stabilization, and—yes, why not?—privatization are probably also good for growth.

Which brings up the question of private or public ownership and economic growth. Using an index of private ownership from Milanovic (1989), Palia and Phelps (2000) find for a sample of 43 countries that a strong private sector is good for growth. Rather than appealing to the simple framework of an AK-type model, which might mask the more complex interactions that have been debated in the literature on the efficiency of state enterprises (see Rama 1999), this article presents a more fully articulated model. It goes beyond the AK model to show how efficiency can be related to growth by incorporating into an endogenous growth framework the idea that state enterprises may be less inclined to invest and employ skilled labor and less innovative than private firms. State enterprises sometimes fail to adopt new products and processes invented in the private sector, reducing their efficiency. The article places this hypothesis within a clear conceptual framework, to facilitate discussion of how growth may be affected by the form of corporate ownership. The model developed here bridges the analytical literature on the static efficiency of state enterprises and the empirical tests of dynamic efficiency and economic growth reported below. This modeling strategy rests on a microeconomic foundation and derives testable macroeconomic hypotheses.

These hypotheses about the linkages among state enterprises, efficiency, investment, education, and economic growth are tested on new data from the World Bank (1995) on the share of state enterprises in employment for a cross-section of 34 developing economies for 1978–92. A significant inverse relationship emerges between the size of the state enterprise sector and economic growth, partly through investment and partly through education.

In a similar attempt to find a relationship between the size of the state enterprise sector and economic growth, the World Bank (1995, 52) reports that “there was insufficient time-series data on state enterprise sector size for enough countries over a sufficiently long time to conduct satisfactory growth regression analysis.” Even so, the World Bank concludes that “the microeconomic evidence, the experience of the centrally planned economies, and the strong negative effect SOEs [state enterprises] have on fiscal deficits all collectively support the premise that large SOE sectors can hinder growth. Moreover, because SOE sectors tend to be larger in low-income countries, SOEs are likely to be most costly in the countries that can least afford them” (50–51). The empirical findings reported here support that conclusion.

It is important in this kind of analysis to distinguish the adverse growth effects of state involvement in production and in the allocation of resources from any effects of big government on growth. The effects of government spending, taxes, and transfers on growth depend on how the government spends tax rev-
enue (see Barro 1990). It is possible for increased government expenditure to boost growth despite a concurrent negative relationship between the size of the state enterprise sector and economic growth (on education, for example, see Glomm and Ravikumar 1992).

The first step is to embed state enterprises in one model of economic growth to demonstrate that ownership can matter for growth and thus belongs in growth theory. In this model, derived from Romer (1990), growth arises from an expanding variety of inputs. Because the hypotheses will be tested using data for developing economies, it is natural to think in terms of the adoption or adaptation of leading-edge technology rather than the invention of new technology. The model is intended only to illustrate some—but by no means all—of the channels through which efficiency influences economic growth. The intention is to show by example how state enterprises can be incorporated into the growth literature and the standard determinants-of-growth regression framework. The empirical tests presented later are not meant as tests of the particular model selected as the vehicle, because other points of entry, such as the AK model, could as well have been chosen.

In this version of the Romer model, output is produced in both private and public sectors, and in both sectors, output levels are set to maximize profits. Unlike private firms, however, state enterprises have to satisfy further constraints and objectives (regulations on work hours, on where to buy inputs, and the like) that affect labor productivity and the propensity to adopt new inputs. These firms are thought of as being run by bureaucrats on whom political authorities have imposed multiple goals and constraints.

The model features full employment, free entry in the competitive private sector, and infinite substitutability between private and public output. With free entry, the inherent static and dynamic inefficiency of state-owned firms means that they must be kept afloat by a government subsidy financed by a tax on private firms. State enterprises may produce goods and services (such as cars and computers, as in France, and banking services, as in India) that are no different from similar goods and services produced by more competitive, privately owned companies, but cost more to produce. So why do state enterprises exist? Because of their size, inefficient state enterprises may be important for the local economy: firms that employ workers that are not easily employable elsewhere are tempting targets for politicians striving to gain popularity with job-saving measures. An even stronger motivation for public ownership is the strategic importance of certain industries, such as aircraft, utilities, and armaments. Other examples abound, especially in developing economies, where export and import-competing industries are often of great importance to the local economy, yet face stiff foreign competition.

In sum, this modeling strategy is intended to draw the attention of growth analysts to public versus private ownership and to shed some light on the consequences of state enterprises, which, while competing with the private sector at home or abroad, are saddled with an inefficient cost structure and social respon-
sibility for the local economy. The model is illustrative; it is not intended as a general framework for studying the *raison d'être* of state enterprises.

### Preferences and Utility Maximization

Consumers derive utility from the consumption of final output, which is sold in a perfectly competitive market. Public output \( Y^s \) and private output \( Y^p \) are perfect substitutes. Total consumption equals \( C = C^p + C^s \). Though indifferent between consuming private and public output, the typical consumer maximizes the present discounted value of lifetime utility from total consumption. As in Blanchard (1985), workers face a constant probability of death \( \pi \), and new cohorts are continuously being born. This prevents Ricardian equivalence. This matters because inefficient state enterprises are often responsible for mounting public debt, which may reduce saving. Preferences are described by an isoelastic utility function, 

\[
u = c^{1-1/\sigma} / (1 - 1/\sigma),
\]

where \( c \) is per capita consumption and \( \sigma \) is the elasticity of intertemporal substitution. This gives the following Euler equation (equation 1) for the optimal aggregate consumption profile \( C = cL \), where \( L \), the total labor force, is fixed.

\[
\frac{\dot{C}}{C} = \sigma(r - \rho) - \left\{\sigma \rho + \pi - (\sigma - 1) \pi \right\} \frac{W}{C}
\]

where \( r \) is the real interest rate, \( \rho \) is the pure rate of time preference, and \( W \) is total wealth, which consists of the total value of firms and outstanding public debt, \( D \). This debt has been accumulated to sustain the operation of state enterprises in the past.

### Technology and Profit Maximization

Both sectors, private and public, use labor and other inputs, which are produced solely by private firms. With Romer (1990) as a starting point, production technology in the two final-goods sectors is shown in equations 2 and 3.

\[
Y^p = A^p (e^p L^p)^{1-\alpha} \sum_{j=1}^{N} (X^p_j)^\alpha
\]

\[
Y^s = A^s (e^s L^s)^{1-\alpha} \sum_{j=1}^{N} (X^s_j)^\alpha
\]

where \( L^i \) is employment in sector \( i \), \( e^i \) is the efficiency of labor in that sector, \( X^i_j \) is the use of input \( j \) in that sector, \( i = s, p \). \( N \) is the number of inputs produced and used in the private sector, and \( p \) is the probability that a new input will be adopted by state enterprises. Possible sources of (static) inefficiency in the public sector are:

1. Public enterprises may be less efficient—waste more resources—than private firms. This means that \( A^s < A^p \) in equations 2 and 3. Managers of state enterprises may not have the same incentive as management in private firms
do to organize production efficiently and to invest in sound projects, partly because the penalty of failure is less threatening when the state coffers are within reach and partly because the rewards of success are typically smaller.

2. State firms may be less efficient in organizing labor within the firm and may employ less well educated labor than private firms. Therefore, $e^s < e^p$. Wages are generally lower in state enterprises than in the private sector (see Gyourko and Tracy 1988). Also, wage setting in the public sector tends to be less flexible and thus less incentive compatible—less conducive to increased work effort and improved efficiency—than in the private sector (World Bank 1995). Moreover, in many countries, the public sector tends to be overstaffed because state enterprises do not make hiring and firing decisions solely on the basis of profitability. Workers in state enterprises generally enjoy greater protection from cyclical layoffs than do workers in the private sector.

3. Each newly invented input is bought by private firms, but in the public sector this occurs with probability $p$. The hypothesis is that state enterprises are not as innovative as private firms—and so not as likely to invest in new machinery and equipment that embodies new and productive technology (Phelps 1993). Thus there is a fixed probability $p < 1$ that a new input will be adopted by state enterprises. For this reason, fewer types of inputs—less high-tech capital—may be used in the public sector than in the private sector: $pN < N$.

With free entry, private firms enter until average profits in the sector are driven to zero. The question of the viability of public enterprises is bound to arise in light of the constant returns to scale nature of the production technology in both sectors. This issue is resolved by assuming that state enterprises receive a subsidy $s$ per unit of output from the government financed by a tax $t$ on the output of private firms. The effective subsidy $s + t$ is then equal to the difference between average long-run costs in public and private enterprises and can be written (see equation 4) for the case of $N = 2$ and $p = 1$.

$$s + t = \left( \frac{w}{e^s} \right)^{1-\alpha} \left( \frac{1}{A^s} \right) - \left( \frac{w}{e^p} \right)^{1-\alpha} \left( \frac{1}{A^p} \right) \lambda P^\alpha$$  \hspace{1cm} (4)

where $\lambda = \left( \frac{1-\alpha}{\alpha} \right)^{1-\alpha} + \left( \frac{1-\alpha}{\alpha} \right)^{-1+\alpha}$ and $P$ is the real price of an input. The government budget constraint is added next to solve for the subsidies and taxes (equation 5).

$$sY^s + rD = tY^p$$  \hspace{1cm} (5)

The effective subsidy is a decreasing function of $e^s$ and $A^s$ and an increasing function of $e^p$ and $A^p$ as expected. If $e^s = e^p$ and $A^s = A^p$, then $s + t = 0$ by equation 4. This system of taxes and subsidies is the basis for the continued existence of state enterprises, given their presumed inefficiency.

Firms in both sectors decide on employment and the use of other inputs to maximize profits. The first-order conditions for labor (in efficiency units) and other inputs are shown in equations 6 and 7.
(6) \[ w = (1 - \alpha) \frac{Y_P(1 - t)}{e^t L} = (1 - \alpha) \frac{Y^s(1 + s)}{e^t L} \]

(7) \[ X^P_j = e^t L \left( \frac{\alpha A^P(1 - t)}{P_j} \right)^{\frac{1}{1 - \alpha}} \]
\[ X^S_j = e^t L \left( \frac{\alpha A^S(1 + s)}{P_j} \right)^{\frac{1}{1 - \alpha}} \]

where \( P_j \) is the real price of input \( j \).

The price of inputs is set by the monopolists that invented them, but the wage \( w \) is determined by supply and demand in labor markets, so that \( L = (1 - \alpha) \frac{Y^P(1 - t) + Y^S(1 + s)}{e^t w} \), where \( L \), the labor force, is fixed.

**Input Pricing, Output, and Growth**

Each intermediate input is produced by its inventor, who has a permanent monopoly in production. The production technology involves turning one unit of the final good into a unit of input at zero cost. The monopolists' profits can be written as \((P_i - 1)X_i\), where \( P_i \) is the (monopoly) real price of the input in terms of final goods. The monopolist then sets the price of the input to maximize current profits by taking factor demand (equation 7) into account; no intertemporal considerations enter the pricing decision. The monopoly price is \( P_i = 1/\alpha \). Plugging this price into factor demand equations 7 yields the steady-state value of a new invention, assuming a constant rate of real interest, \( r \) (equation 8).

(8) \[ V = \left[ \frac{e^t L (A^P[1 - t])^{\frac{1}{1 - \alpha}} + \rho e^t L (A^S[1 + s])^{\frac{1}{1 - \alpha}}}{\alpha} \right]^{\frac{1}{1 - \alpha}} \]

In a steady state with a growing variety of inputs and free entry, the expected value of a new invention has to equal the cost of inventing a new input, \( \eta \). The total value of firms is therefore equal to \( N\eta \). This gives the equilibrium interest rate and, through equation 1, the rate of economic growth (equation 9)

(9) \[ g = \sigma \left[ \left( \frac{e^t(1 - v)L}{\eta} (A^P[1 - t])^{\frac{1}{1 - \alpha}} + \rho \frac{e^t L}{\eta} (A^S[1 + s])^{\frac{1}{1 - \alpha}} \theta - r \right) \right] \]
\[ - \left( (\sigma \rho + \pi - (\sigma - 1) \pi) \frac{N\eta}{C} + D \right) \]

where \( v = L^P(L^P + L^S) \), \( L^S \), and \( L^P \) are determined by equation 6 and \( \theta = \frac{(1 - \alpha)}{\alpha^{\frac{1}{1 - \alpha}}} \).

Apart from the usual effects of changes in the cost of innovation, \( \eta \), the size of the total labor force, \( L = L^P + L^S \), and the rate of time preference, \( \rho \), on growth, the equation suggests that

1. The rate of growth is a decreasing function of the size of the state sector, \( v \), as long as \( A^P(1 - t)/A^S(1 + s) > (pe^t/e^t)^{1-\alpha} \), because the transfer of labor from the private sector to the public sector reduces demand for inputs. Thus \( A^P(1 - t) \geq A^S(1 + s) \) and \( e^t \geq e^s \) are a sufficient but not necessary condition
for the expansion of the state enterprise sector from one time or place to another to reduce economic growth, as long as \( p < 1 \). This is the main hypothesis.

2. The rate of growth is an increasing function of the productivity of labor in state enterprises, \( e^s \) (as well as in private firms, \( e^p \)), for given taxes and subsidies.

3. The rate of growth is an increasing function of the level of technology and the efficiency of organization in state enterprises, \( A^s \) (as well as in private firms, \( A^p \)), for given taxes and subsidies.

4. The rate of growth is an increasing function of the probability that state enterprises adopt newly invented inputs, \( p \), which is interpreted as a sign of their willingness to invest.

5. The rate of growth is a decreasing function of public debt, \( D \), which is assumed to have been, at least in part, accumulated by state enterprises in the past. A higher level of debt increases consumption and hence leaves less output for investment in research and development of new types of inputs.

II. Empirical Evidence

Under ideal conditions, the next task would be to gather the data and test all the hypotheses derived from growth equation 9. This is an impossible task, however, because several of the variables that drive economic growth in this model cannot be directly observed: the efficiency of organization \( (A^s \text{ and } A^p) \), the productivity of labor \( (e^s \text{ and } e^p) \), and the probability that state enterprises adopt newly invented inputs \( (p) \).

Hypothesis 1 can be tested directly by estimating the partial correlation between the state enterprises' share in the labor force, \( v = L^s/(L^s + L^p) \), for which data are widely available from the World Bank (1995), and economic growth, controlling for other potential determinants of growth. If the conjecture that this correlation is negative is confirmed, that is an indication that the state enterprise sector is less well organized, less efficient, or less innovative than the private sector in such proportions that \( A^p(1-t)/A^s(1+s) > (pe^p/e^p)^{1-s} \).

Hypotheses 2–5 can be tested only indirectly, however, because of lack of data. To test hypotheses 2 and 3, labor productivity and efficiency of organization are assumed to vary directly with investment and the education of the labor force. To test hypothesis 4, the propensity to invest is assumed to reflect, in part, the willingness to adopt newly invented inputs. This is the case when the intermediate inputs are capital goods. Therefore, if state enterprises are generally more prone than private firms to waste resources on unproductive investments ("white elephants") and to divert government spending from social needs, including education (Mauro 1998), and less willing to adopt new inputs, as conjectured, then this is an additional link between the size of the state enterprise sector and economic growth.

Together, hypotheses 1–4 imply that increased state enterprise activity can hurt economic growth directly as well as indirectly through investment and edu-
cation. A fifth hypothesis is that the impact of investment on growth varies inversely with the size of the state enterprise sector. To test hypothesis 5, state enterprises are assumed to bear responsibility for a substantial part of public external indebtedness. The empirical results reported below need to be viewed in the light of these qualifications.

A Preview

These hypotheses are tested using cross-sectional data from the Penn World Tables (see Summers and Heston 1991) and the World Data Bank (World Bank 1997) covering 1978–92 (1978–91 for state enterprises). Table 1 reports summary statistics for the share of state enterprises in employment \((SOE/Labor)\) and in nonagricultural GDP \((SOE/GDP)\) and for the external debt of state enterprises as a proportion of GDP \((SOE/Debt)\).

The share of state enterprises in employment was remarkably steady, averaging 12 percent in both the first and last years of the period. Several countries significantly downsized their state enterprise sector. Chile reduced the sector’s share in employment from 4 percent to 1 percent and its share in nonagricultural GDP from 12 percent to 8 percent. Argentina reduced the employment share from 4 percent to 2 percent and the share in GDP from 6 percent to 2 percent, and Botswana reduced the public sector’s share in employment from 3 percent to 2 percent and the sector’s share in GDP from 9 percent to 6 percent. At the other end of the spectrum, Ghana increased the share of state enterprises in employment from 29 percent to 45 percent, while their share in GDP declined from 8 percent to 7 percent.

It would be unwise, however, to ascribe rapid growth in Chile since the mid-1980s and in Argentina since the early 1990s until recently in part to privatization (or, for that matter, to ascribe slow growth in Ghana in part to the failure to privatize). For one thing, causation can run both ways. Although the model suggests a link from privatization to growth, and privatization was an important ingredient of the reforms that started in Chile in the 1970s and in Argentina in the 1980s, it also seems reasonable to suppose that brisk growth in Chile and Argentina may have helped create conditions favorable to further privatization and other reforms. Even so, the high unemployment that accompanied the rapid growth in Argentina and Chile, by exerting political pressure not to endanger jobs in the state sector, seems likely to have weakened this reverse linkage from growth to privatization. By the same token, sluggish growth and high unemployment in Ghana (and elsewhere, no doubt) contributed to the expansion of employment in the state enterprise sector, even if the sector’s share in GDP was declining.

The main point, however, is this: if private enterprise is good for growth, as hypothesized, that does not mean that growth is not good for private enterprise. The same argument applies to other potential determinants of economic growth: trade, investment, education, and so on. The discussion that follows emphasizes

2. Due to gaps in the data, the first year and the last year vary from country to country.
the link from privatization to growth, even though the relationship between the two may well be more complex.

**Correlation Analysis**

Correlation analysis can further illuminate the relationships between the size of state enterprises and growth. Some key bivariate correlations are between the relative size of the state sector, measured by state enterprises’ share of employment (SOE/Labor), and the accumulation of physical and human capital—two key determinants of economic growth.

A scatterplot of state enterprise employment and the share of investment in GDP across countries, both measured as averages over the period 1978–1991/92, shows the relationship to be economically and statistically significant (figure 1.)3 The regression line in figure 1 is based on robust estimation to reduce the weight of potential outliers (the same applies to figures 2–3). An increase in the employment share of state enterprises by one standard deviation is associated with a decrease in investment of 4.5 percent of GDP, all else remaining the same. The correlation \( r = -0.51 \) \( (t = 3.6) \). Similar results \( (r = -0.42, t = 2.9) \) obtain when the initial rather than average value is used to measure SOE/Labor. This suggests that causation runs from SOE/Labor to investment rather than the other way round. This result supports the hypothesis that state enterprises are less inclined than private firms to invest in new machinery and equipment and to adopt new technology and may thus impede economic growth.

A second scatterplot shows the correlation of state enterprise employment during 1978–91 and the rate of enrollment in secondary schools in the base year, 1978, a commonly used measure of education in the growth literature (figure 2). An increase in the employment share of state enterprises of one standard deviation goes along with a decrease in secondary school enrollment of 1.5 percentage points, all else remaining the same. The correlation is \( -0.58 \) \( (t = 4.1) \). The pattern is similar \( (r = -0.51, t = 3.4) \) when the initial rather than average value is used to measure SOE/Labor. This pattern seems consistent with the hypothesis that state enterprises are less inclined than private firms to employ skilled labor and perhaps less likely to adopt new technology and thus may inhibit economic

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3. SOE/Labor is exceptionally large in Guinea (70 percent). This outlier is excluded from figures 1–3 and from equations 2 and 3 in table 2.
growth. Other interpretations are also conceivable; for example, low standards of education may generate unemployment and thus exert pressure on the authorities to create jobs through state enterprises.

In sum, the data suggest that state enterprises may slow economic growth by discouraging investment (figure 1) and education (figure 2). Figure 3 confirms this: it shows an inverse correlation (−0.35, \( t = 2.2 \)) between state enterprises’ employment share and economic growth across countries. An increase in state enterprises’ employment share of one standard deviation is associated with a decrease in the annual rate of economic growth of about 1 percent. The economic and statistical significance of this correlation is preserved when economic growth is regressed on the state enterprises’ employment share and initial GDP using ordinary least squares (OLS) and when the initial rather than average value is used to measure SOE/Labor (\( r = −0.29, \ t = 1.8 \)).

These correlations suggest that a small state enterprise sector (where state enterprises account for 5 percent or less of employment) can be associated with both rapid growth, as in Indonesia, the Republic of Korea, Taiwan (China), and Thailand, and slow or even negative growth, as in Bolivia, Madagascar, and Peru. A large state enterprise sector, however, generally goes hand in hand with slow

**Figure 1. State Enterprises and Investment (percent)**

Source: Penn World Tables and the World Bank.
Note: Country abbreviations are defined in the Appendix.
growth, as in Ghana and Zambia. Except for Sri Lanka, all countries whose state enterprise sector accounted for 10 percent or more of total employment had economic growth of less than 2 percent a year on average over the period (a majority had negative growth).

Does this inverse correlation between the employment share of state enterprises and economic growth stem from inefficiency in the state enterprise sector, as hypothesized? Or does the size of the state enterprise sector simply reflect flaws in economic policy that hinder economic growth? If the second, the share of state enterprises in employment could be expected to be positively correlated with inflation, a common measure of policy failure. That is not the case, however. The correlation between state enterprises’ share in employment (SOE/Labor) and a measure of inflation distortion (defined as $\frac{\pi}{1+\pi}$ where $\pi$ is the rate of inflation) is −0.31 ($t = 1.9$).\(^4\)

4. The correlation between SOE/Labor and the share of government expenditure in GDP in the sample is 0.48 ($t = 3.2$), but government expenditure is not in itself a sign of policy weakness or inefficiency, certainly not if the government spends its tax revenue mostly on productive infrastructure, education, and health care.
Next, the simple correlations between the size of the state sector and investment, education, and growth are subjected to closer econometric scrutiny.

**Regression Analysis**

The model is estimated as a system using seemingly unrelated regression (SUR).\(^5\) That allows the marginal processes for investment, education, and growth to be modeled simultaneously to investigate the direct and indirect effects of state enterprises on economic growth. First, however, a basic Barroian growth regression is estimated to explain the growth rate alone.

The results for regression 1 in table 2 are for a cross-sectional OLS regression of average growth on the logarithm of the initial level of GDP and the average share of investment in GDP. The negative coefficient on initial income (although not statistically significant) is a sign of \(\beta\)-convergence, but quite slow: it implies a convergence speed of 0.3 percent a year rather than the 2–3 percent rate usually reported in the literature (Barro and Sala-i-Martin 1995). However, this result

\(^5\) The models were also estimated independently using OLS (not reported). The results remained virtually the same.
### Table 2. Empirical Results

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<tr>
<td></td>
<td>(1) (2a) (3a) (4a) (5)</td>
<td>(2b) (3b) (4b) (2c)</td>
<td>(3c) (4c)</td>
</tr>
<tr>
<td>Initial GDP</td>
<td>-0.003 (-0.008 -0.010 -0.008 -0.007)</td>
<td>0.024 (0.023 0.053</td>
<td>0.147 (0.147 0.240</td>
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<tr>
<td>Investment</td>
<td>-0.172 (0.131 0.213 0.208 0.162</td>
<td>0.172 (0.131 0.213 0.208 0.162</td>
<td>0.147 (0.147 0.240</td>
</tr>
<tr>
<td>Secondary education</td>
<td>-0.029 (0.014 0.914 0.023</td>
<td>-0.029 (0.014 0.914 0.023</td>
<td>-0.029 (0.014 0.914 0.023</td>
</tr>
<tr>
<td>SOE/Labor</td>
<td>-0.267 (2.58) (2.68)</td>
<td>0.078 (1.73) (1.70)</td>
<td>0.036 (1.70)</td>
</tr>
<tr>
<td>SOE/GDP</td>
<td>-0.096 (2.48)</td>
<td>-0.096 (2.48)</td>
<td>-0.096 (2.48)</td>
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<tr>
<td>SOE/Debt</td>
<td>-0.695 (2.06)</td>
<td>-0.695 (2.06)</td>
<td>-0.695 (2.06)</td>
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<tr>
<td>SOE/Labor × Investment</td>
<td>-0.219 (2.30)</td>
<td>-0.219 (2.30)</td>
<td>-0.219 (2.30)</td>
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<tr>
<td>SOE/GDP × Investment</td>
<td>-0.219 (2.30)</td>
<td>-0.219 (2.30)</td>
<td>-0.219 (2.30)</td>
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<tr>
<td>Constant</td>
<td>0.003 (0.16) 0.038 (1.46) 0.056 (1.42) 0.032 (1.11) 0.034 (1.11) 0.034</td>
<td>-0.016 (0.14) -0.011 (0.10) -0.272 (5.11) -0.730 (2.79) -0.731 (2.79) -1.463 (10.00)</td>
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<tr>
<td>SE</td>
<td>0.22 (0.22) 0.25 (0.25) 0.36 (0.36) 0.26 (0.26) 0.22 (0.22) 0.33 (0.33) 0.33 (0.44) 0.44 (0.55) 0.55 (0.55) 0.72</td>
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<tr>
<td>Adj. $R^2$</td>
<td>0.22 (0.22) 0.25 (0.25) 0.36 (0.36) 0.26 (0.26) 0.22 (0.22) 0.33 (0.33) 0.33 (0.44) 0.44 (0.55) 0.55 (0.55) 0.72</td>
<td></td>
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</tr>
<tr>
<td>Number of countries</td>
<td>96 88 34 67 71 39 39 74 35 35 69</td>
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<td></td>
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<tr>
<td>Estimation method</td>
<td>OLS SUR SUR SUR OLS SUR SUR SUR SUR SUR SUR</td>
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*Note: t values appear within parentheses below the coefficients.*
is in line with other studies when such variables as human capital, trade, and political instability are excluded.

The higher the share of investment in GDP, the more rapid is economic growth in all the regressions; this effect is quite robust. According to point estimates, increasing the investment ratio from 20 to 30 percent from one country to another increases growth by 1.3 to 2.1 percent, all else remaining the same. These estimates are broadly similar to those reported by Levine and Renelt (1992), Sachs and Warner (1995), Gylfason and Herbertsson (1996), and Gylfason (1999).6 The exclusive focus here on developing economies, where diminishing returns to capital have not yet set in fully, may explain why investment in some cases appears to have a slightly stronger effect on growth than in some of the above-mentioned studies (Sachs and Warner, in particular), which include industrial as well as developing countries.

In regression 2a, the education variable is the usual secondary school enrollment rate from Barro and Lee (1993), measured at the beginning of the sample period (1978), as is customary to avoid simultaneity bias. The effect of education on growth is statistically—and economically—significant: an increase in the initial secondary school enrollment rate from 50 to 80 percent increases the average rate of growth by almost a whole percentage point, all else remaining the same.

Regression 2a is estimated as part of a system of three equations, in which equations 2b and 2c describe the dependence of investment and secondary education on initial income and the share of state enterprises in employment (recall figures 1 and 2). An increase in state enterprise employment discourages both investment (regression 2b) and education (regression 2c)—education only marginally, however—and thus reduces growth, as shown in regression 2a. The total indirect effect of an increase in the employment share of state enterprises on economic growth is $0.131 \times (-0.267) + 0.029 \times (-0.419) = -0.035 - 0.012 = -0.047 \ (t = 2.5)$.7 The indirect effect of state enterprises on growth through investment is statistically significant ($t = 2.1$), but the indirect effect through education is not ($t = 1.3$).8 When initial rather than average values of the SOE/Labor variable are used to guard against the possibility of reverse causation and omitted-variable bias, a broadly similar pattern emerges. This reduces the likelihood that the results are driven by the effects of economic growth and investment on the size of the state enterprise sector (for example, growth slowdowns that make governments more willing to expand state enterprise employment, or investment booms that make state enterprise employment and growth increase simultaneously).

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7. The composite $t$ values are computed by Taylor expansion following Staiger and others (1997).
8. The total effect of initial income on growth is, by similar arithmetic, smaller than the direct effect, as is reasonable: conditional convergence does not necessarily generate absolute convergence.
Regression 3a adds the multiple of the employment share of state enterprises and the investment ratio to test for the direct impact of state enterprises' employment share on growth. This makes the effect of investment on growth dependent on the size of the state enterprise sector. The idea is that state enterprises tend to buy inferior capital, which adds less to output. The coefficient on the SOE/Labor term is significant and implies that a one-standard-deviation increase in state enterprises' employment share (0.14) reduces economic growth by $-0.695 \times 0.153 \times 0.14 = -0.015$, or 1.5 percentage points, evaluated at the sample mean of the investment ratio (0.153). The investment rate survives the introduction of the interaction term involving employment share, but the education variable drops in both size and significance. Auxiliary regressions 3a and 3b are similar to regressions 2b and 2c. The total effect of an increase in state enterprise employment on economic growth is $-0.695 \times 0.153 + 0.213 \times (-0.278) + 0.014 \times (-0.416) = -0.106 - 0.059 - 0.006 = -0.171 \ (t = 3.4)$.9

Therefore, when the share of state enterprises in employment increases by one standard deviation, economic growth decreases by 2.4 percentage points, all else remaining the same, directly as well as through investment. The indirect growth effect of state enterprises through investment is economically and statistically significant ($t = 2.2$), but the indirect effect through education is not ($t = 0.5$). The visual impression conveyed by figures 1 and 3 is confirmed. Again, a broadly similar pattern is observed when initial rather than average values of the SOE/Labor variable are used: the total effect of an increase in state enterprise employment on growth is now $-0.123 \ (t = 2.5)$. So far, the size of the state enterprise sector has been measured by its share in total employment rather than by its share in GDP. This is because the inefficiency associated with state enterprises is often manifested in overstaffing. (Recall the case of Ghana, where the share of state enterprises in employment rose by half during 1978-91, while their share in GDP declined.) It is nevertheless interesting to explore whether there is a significant relationship between the share of state enterprises in GDP (SOE/GDP) and economic growth.

Regression 4a shows that an increase in the share of state enterprises in GDP has a significantly negative direct effect on economic growth, a result that also holds when initial rather than average values of SOE/GDP are used. There are no indirect effects, however, at least not through education (see regression 4c). True, the coefficient on SOE/GDP in investment regression 4b is marginally significant, but its sign is wrong in view of the model. Even entertaining the possibility that an increase in the share of state enterprises in GDP stimulates investment does not materially change the result: the total effect of SOE/GDP on growth is still significantly negative.

9. When SOE/Labor appears as an independent variable on its own in regression 3a, without interacting with investment, its direct effect on growth is still negative, but not significant ($t = 1.1$). In other respects, the results remain virtually unchanged.
Regression 5 includes the external debt of state enterprises as a proportion of GDP (SOE/Debt). This result also holds when initial rather than average values of SOE/Debt are used.

III. Conclusion

In the simple model developed here of endogenous growth in an economy with state enterprises as well as private firms, a large state sector tends to be associated with slow economic growth, all else remaining the same, if state enterprises are less efficient than private firms, invest less, employ less skilled labor, and are less eager to adopt new technology. The main empirical finding is that, across countries, investment and economic growth during 1978–92 were inversely related to the size of the state enterprise sector, measured by its share of total employment. Specifically, a one-standard-deviation increase in the state sector’s share of total employment from one country to another reduces the ratio of investment to GDP by about four percentage points and reduces per capita growth by about one to two percentage points, all else remaining the same. Thus, too great a reliance on state enterprises may stand in the way of both static and dynamic efficiency—and consequently also investment and economic growth. Even so, the results need to be interpreted with caution in view of the limited data coverage across countries and over time.

APPENDIX. COUNTRY ABBREVIATIONS USED IN FIGURES 1–3.

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<td>ZMB</td>
<td>Zambia</td>
</tr>
<tr>
<td>KOR</td>
<td>Korea, Rep.</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
REFERENCES


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Infrastructure, Geographical Disadvantage, Transport Costs, and Trade

Nuno Limão and Anthony J. Venables

The authors use different data sets to investigate the dependence of transport costs on geography and infrastructure. Infrastructure is an important determinant of transport costs, especially for landlocked countries. Analysis of bilateral trade data confirms the importance of infrastructure and gives an estimate of the elasticity of trade flows with respect to the trade cost factor of around $-3$. A deterioration of infrastructure from the median to the 75th percentile raises transport costs by 12 percentage points and reduces trade volumes by 28 percent. Analysis of African trade flows indicates that their relatively low level is largely due to poor infrastructure.

The real costs of trade—the transport and other costs of doing business internationally—are important determinants of a country's ability to participate fully in the world economy. Remoteness and poor transport and communications infrastructure isolate countries, inhibiting their participation in global production networks. For example, in 1995 landlocked countries on average had an import share in gross domestic product (GDP) of 11 percent, compared with 28 percent for coastal economies. Eight of the top 15 nonprimary export performers for 1965–90 are island countries, and none is landlocked (World Bank 1998). As liberalization continues to reduce artificial trade barriers, the effective rate of protection provided by transport costs is now, in many cases, considerably higher than that provided by tariffs. To bring countries further into

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1. Increasing trade in components and the geographical fragmentation of some production processes make transport costs even more important. See Feenstra (1998) and the references quoted therein for evidence of the increase in the importance of intermediate goods trade. Radelet and Sachs (1998) show how sensitive value added is to transport costs in a vertically fragmented activity.


3. See Finger and Yeats (1976) for U.S. post-Kennedy round data on nominal and effective rates of protection afforded by tariffs and transport costs. See Hummels (1998b) for recent data on nominal rates for Argentina, Brazil, New Zealand, and the United States.
the trading system, it is important to understand both the determinants of transport costs and the magnitude of the barriers to trade that they create.

Here we study the determinants of transport costs and show how they depend both on countries' geography and on their level of infrastructure. The importance of geography has been established by Hummels (1998b) as well as by Moneta (1959). We focus on the distance between countries, whether they share a common border, whether they are landlocked, and whether they are islands. The infrastructure measures relate to the quality of transport and communications infrastructure. Although the importance of infrastructure for transport costs is well established in regional and transport economics, the few empirical studies of international transport costs often neglect this and focus on geographical and product characteristics.

We show that infrastructure is quantitatively important in determining transport costs, a finding with important policy implications for investment in infrastructure. Poor infrastructure accounts for 40 percent of predicted transport costs for coastal countries and up to 60 percent for landlocked countries. An improvement in own and transit countries' infrastructure from the 25th percentile to the 75th percentile overcomes more than half of the disadvantage associated with being landlocked.

Our research uses different sources of transport cost data. The first is shipping company quotes for the cost of transporting a standard container from Baltimore, Maryland, in the United States, to selected destinations. The advantages of this measure are that it is the true cost of transporting a homogeneous good and that it gives both the city of landfall and the final destination city. This enables us to compare the transport costs of land and sea legs of a journey, finding that the former is around seven times more costly per unit distance. The disadvantage of this data set is that it is not clear how the experience of Baltimore generalizes, because charges are affected by particular routes, frequencies, and opportunities for backhauling and exploiting monopoly power. Our second data set uses a cross section of the ratio of carriage, insurance, and freight (CIF) to free on board (FOB) values that the International Monetary Fund (IMF) reports for bilateral trade between countries. These are representative insofar as they cover the entire imports of each reporting country. However, the measure is an aggregate over all commodity types imported, and there are some questions, which we address, regarding the quality of the data.

In addition to the determinants of transport costs, we want to know the extent to which transport costs choke off trade. To do this we undertake a gravity modeling exercise, incorporating the same geographical and infrastructure measures that we use in estimating trade costs. This analysis strongly confirms the

4. Hummels (1998b) has undertaken a thorough study of the implications of geography for freight rates on disaggregated commodity imports of New Zealand, the United States, and five Latin American countries.

5. An exception to this is Radelet and Sachs (1998), where port quality is entered as an explanatory variable for transport costs.
importance of these variables in determining trade and enables us to compute estimates of the elasticity of trade flows with respect to transport costs. We find that this elasticity is large, with a 10-percentage-point increase in transport costs typically reducing trade volumes by approximately 20 percent.

Taken together, our approaches provide a rather consistent picture of the determinants of transport costs, in particular the importance of infrastructure in source and destination countries and in any transit countries used by landlocked economies. We draw out the implications of our findings by looking in some detail at trade and transport costs in Sub-Saharan Africa. Our measures indicate that many of these economies have extremely high transport costs. We show how taking infrastructure into account explains part of the relative trade performance of these countries.

In section I we discuss the determinants of transport costs and present estimates for the transport cost equation using the shipping data and the CIF/FOB data. In section II we present the gravity results. In section III we compare and contrast the results from the transport cost and gravity analyses and derive an estimate of the elasticity of trade flows with respect to transport costs. We show that improvements in the infrastructure of landlocked countries and their transit countries can dramatically increase trade flows. We analyze trade and transport costs in Sub-Saharan Africa in section IV, finding that infrastructure accounts for much of Africa’s poor performance. Section V concludes and summarizes our main quantitative findings.

I. Transport Costs

The Determinants of Transport Costs

Let $T_{ij}$ denote the unit cost of shipping a particular good from country $i$ to country $j$. We suppose that it is determined by

$$T_{ij} = T(x_{ij}, X_i, X_j, \mu_i),$$

where $x_{ij}$ is a vector of characteristics relating to the journey between $i$ and $j$, $X_i$ is a vector of characteristics of country $i$, $X_j$ is a vector of characteristics of country $j$, and $\mu_i$ represents all unobservable variables.

What are the relevant observable characteristics of countries and the journeys between them? For the journey, we use two types of measures, both standard in the literature. The first is whether the countries share a common border, and the second is the shortest direct distance between the countries. The importance of distance for transport costs is obvious, but why should sharing a border reduce transport costs after controlling for distance? First, neighboring countries typically have more integrated transport networks that reduce the number of transshipments, for example, from rail to road or across different types of rail gauge. Second, neighboring countries are more likely to have transit and customs agreements that reduce transit times and translate into lower shipping and insurance costs. Finally, the higher volume of trade between neighboring countries dra-
matically increases the possibilities for backhauling, allowing the fixed costs to be shared over two trips.

For country characteristics, we focus on geographical and infrastructure measures. The main geographical measures are simply whether the country is landlocked and whether it is an island. The infrastructure measure \((inf)\) we use is designed to measure the costs of travel in and through a country. It is constructed as an average of the density of the road network, the paved road network, the rail network, and the number of telephone main lines per person. In our regressions, we always work with an inverse measure of this index; so an increase in the variable \(inf\) is expected to be associated with an increase in the costs of transport. Details on the construction of this and other variables are given in the appendix.

**Shipping from Baltimore**

Our first results are based on the costs of shipping a standard 40-foot container from Baltimore to different destinations around the world. A firm that handles forwarding for the World Bank provided the data, which cover 64 destination cities, 35 of which are in landlocked countries (see appendix tables A-2, A-4, and A-5). This source of data has two major advantages. One is that journeys can be broken down into component parts—the data gives the landfall city for each journey as well as the final destination city—allowing separate estimation of the effect of land and sea distance. The other is that the good shipped is homogeneous, avoiding compositional problems that can occur in aggregate data.\(^6\)

We estimate a linear version of equation 1 both for the entire journey and for the journey divided into the sea journey (to the port) and the land journey (from the port). More specifically, we estimate:

\[
T_{ij} = \alpha + \beta' x_i + \gamma' x_j + \delta' y_i + \nu_{ij},
\]

where \(i\) corresponds to Baltimore and \(j\) represents the destination city. The error term \(\nu_{ij}\) is assumed to be independent of the explanatory variables and normally distributed.

The most appropriate functional form is not clear a priori. On one hand, we are adding over the different legs of the trip. That is, the cost of going through the infrastructure of the importer and the exporter and the cost of shipping between them suggests a linear form. On the other hand, it is possible that there are interactions among the cost variables that would make a nonlinear form more suitable. The simplest example is that an increase in land distance should increase the cost of going through a given infrastructure. For this reason, we also experi-

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\(^6\) UNCTAD (1995, p. 58) presents similar data for a sample of four coastal countries and nine landlocked countries in Sub-Saharan Africa. Livingstone (1986) uses quotes made by regular shippers to the Crown agents from the United Kingdom to eight African countries. The small size of the sample in both studies does not allow for a systematic examination of the determinants of transport costs.
mented with some nonlinear forms, but they were rejected by the data.\footnote{This is true even when quadratic distance terms are added to capture any nonlinearity. These terms are insignificant, further justifying the use of the linear land and sea distance measures. We also estimated equation 2 including the per capita income of the destination country, because low-income countries might have high transport costs for a variety of reasons other than infrastructure. It was not significant.} Therefore, table 1 presents the ordinary least squares (OLS) estimation results of the linear form given by equation 2.

The first two columns in table 1 give results excluding the infrastructure variables. There are three main conclusions. First, being landlocked raises costs by $3,450—compared with a mean cost for nonlandlocked countries of $4,620. Second, breaking the journey into an overland component and a sea component (the second column in table 1) considerably improves the fit of the equation. It also gives a much larger coefficient for the overland portion of the trip compared with the sea distance. An extra 1,000 km by sea adds $190, whereas a similar increase in land distance adds $1,380. When this value is compared with the $380 per 1,000 km predicted by total straight-line distance (the first column), it becomes clear that using the latter measure leads to a large underestimate of the impact of overland distance on transport costs. Third, the additional transport cost from being landlocked is not fully explained by the extra overland distance that must be overcome to reach the sea. Although the final city destination for landlocked countries is on average four times further from the sea than the final city destination for coastal countries in this sample, the landlocked dummy remains significant after controlling for land distance. There are several possible reasons for this, arising from border delays or transport coordination problems, uncertainty and delays creating higher insurance costs, and direct charges that may be made by the transit country.\footnote{For example, Kenya charges a transit goods license for road transit of $200 (per entry or 30 days) and tolls on trucks (UNCTAD 1997, p. 11).}

The third and fourth columns in table 1 introduce our measures of the inverse infrastructure of the destination (inf) and, for landlocked countries, the transit country (infran) for the smaller sample covered by these data.\footnote{The landlocked dummy is not included because of its multicollinearity with transit infrastructure.} The signs of these are as expected, inferior infrastructures leading to higher transport costs. We can also ask what proportion of the predicted value is explained by infrastructure versus distance. For coastal economies, own infrastructure explains 40 percent of the predicted cost; for landlocked countries, own infrastructure explains 36 percent and transit infrastructure 24 percent of the cost.

The final specification (the fourth column) breaks distance into the overland and sea components. The coefficients on these distance variables are very similar to those in the full sample (the second column). Splitting the distance variable makes the coefficient for transit infrastructure smaller and insignificant because of the variable’s high positive correlation with land distance. Moreover, transit and own infrastructure are also highly correlated. This multicollinearity
Table 1. The Cost of Shipping a 40-Foot Container from Baltimore, 1990

<table>
<thead>
<tr>
<th>Variable</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
</tr>
</thead>
<tbody>
<tr>
<td>Infrastructure (inf&lt;sup&gt;a&lt;/sup&gt;)</td>
<td>1.31**</td>
<td>1.56***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(2.51)</td>
<td>(2.92)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Infrastructure of transit country (infran&lt;sup&gt;a&lt;/sup&gt;)</td>
<td>1.34**</td>
<td>0.67</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1.93)</td>
<td>(0.88)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Landlocked country dummy (ldldummy&lt;sup&gt;b&lt;/sup&gt;)</td>
<td>3.45***</td>
<td>2.17***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(4.75)</td>
<td>(2.94)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Distance between trading partners</td>
<td>0.38**</td>
<td>0.29*</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(2.60)</td>
<td>(1.84)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Sea distance (distsea)</td>
<td>0.19**</td>
<td></td>
<td>0.18*</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(2.12)</td>
<td></td>
<td>(1.74)</td>
<td></td>
</tr>
<tr>
<td>Land distance (distland)</td>
<td>1.38***</td>
<td>1.49*</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(4.66)</td>
<td></td>
<td>(1.77)</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>1.10</td>
<td>2.06*</td>
<td>0.11</td>
<td>-0.10</td>
</tr>
<tr>
<td></td>
<td>(0.95)</td>
<td>(1.85)</td>
<td>(0.093)</td>
<td>(-0.07)</td>
</tr>
<tr>
<td>Sample size</td>
<td>64</td>
<td>64</td>
<td>47</td>
<td>47</td>
</tr>
<tr>
<td>R&lt;sup&gt;2&lt;/sup&gt;</td>
<td>0.32</td>
<td>0.47</td>
<td>0.38</td>
<td>0.43</td>
</tr>
<tr>
<td>F-test (p-values)</td>
<td></td>
<td></td>
<td>0.00</td>
<td></td>
</tr>
</tbody>
</table>

<sup>a</sup>Significant at the 10 percent level.
<br><sup>**</sup>Significant at the 5 percent level.
<br><sup>***</sup>Significant at the 1 percent level.

*Note:* The dependent variable is transport cost, $T_{in}$ in thousands of U.S. dollars. The sample used in specifications 3 and 4 is reduced to the countries for which the infrastructure variables are also available. For specifications 1 and 3, the standard errors were adjusted to correct for heteroskedasticity. t-statistics are in parentheses. The F-tests are for the pairs of variables indicated; the p-values show the level at which the null of no joint significance is rejected. See table A-2 for the countries included in the sample.

<sup>a</sup>Values for the infrastructure variables are averages for 1990–95 (the latest year available).
<br><sup>b</sup>ldldummy = 1 if the country has no access to the sea, 0 otherwise.

*Source:* Authors' calculations.

poses problems for identifying the separate effects of the two variables. However, the tests of significance at the bottom of table 1 confirm the importance of the transit variable when considered jointly with either own infrastructure or land distance. To reemphasize the relative importance of infrastructure, an improvement of inf from the 75th percentile to the median is equivalent to a distance reduction of 3,466 sea km or 419 land km.\(^\text{10}\)

10. For 20 landlocked countries in the sample, we have both the costs of shipping to the port and the full cost of shipping to the landlocked destination (for example, the cost of shipping from Baltimore to Durban and that from Baltimore to Harare via Durban). This enables us to look at the determinants of the incremental costs associated with the final stage of the journey. Final destination infrastructure is significant and positive, although incremental distance and port infrastructure are not. This is due both to the small number of observations and to details that become apparent on inspection of the data. For example, shipping from Baltimore to Durban costs $2,500; shipping the 1,600 km further to Lusaka costs an additional $2,500, whereas the 347 km from Durban to Maseru (Lesotho) costs an additional $7,500. This points to the importance of details of geography, market structure, and trade volumes, in addition to the broader picture painted by the econometrics.
Our second set of experiments is based on the \textit{CIF/FOB} ratio as derived from the IMF's \textit{Direction of Trade Statistics} (IMF various years). Importing countries report the value of imports from partner countries, inclusive of \textit{CIF}, and exporting countries report their value \textit{FOB}, which measures the cost of the imports and all charges incurred in placing the merchandise aboard a carrier in the exporting port. Denoting the \textit{FOB} price of goods shipped from \textit{i} to \textit{j} by \(p_{ij}\), we define \(t_{ij}\), the ad valorem transport cost factor, as

\begin{equation}
\begin{aligned}
t_{ij} &= \frac{\text{cif}_{ij}}{\text{fob}_{ij}} = \frac{p_{ij} + T_{ij}}{p_{ij}} \\
&= t(x_{ij}, X_i, X_j, \mu_{ij})
\end{aligned}
\end{equation}

where the determinants of \(T_{ij}\) are given in equation 1.

The ratio \textit{CIF/FOB} provides the measure of transport costs on trade between each pair of countries. In theory, the \textit{FOB} and \textit{CIF} prices are border prices, and thus it would seem that own and trading partner infrastructures as defined here should not affect these rates. There are three reasons why they are indeed relevant. First, road, rail, and telephone infrastructures are likely highly correlated with port infrastructure (for which we have no data) and the latter would be important even if the prices were pure border prices. Second, the insurance component reflects the total time in transit, that is, from door to door, not just border to border; total transit time is likely to be a function of own and partner infrastructure. Finally, according to UN experts on customs data, the \textit{FOB} and \textit{CIF} figures rarely measure actual border prices, instead measuring the prices at the initial point of departure and final destination, respectively.\footnote{E-mail correspondence with Mr. Peter Lee at the United Nations.} Thus, own and partner infrastructure should be included in the estimation.

Assuming that \(t\) can be approximated by a log linear function up to some measurement error, we can write the observed transport cost factors \(t_{ij}\) :

\begin{equation}
\begin{aligned}
\ln t_{ij} &= \alpha + \beta x_{ij} + \gamma \ln X_i + \delta \ln X_j + \omega_i
\end{aligned}
\end{equation}

where the tildes distinguish this set of parameters from those in equation 2. The final term, \(\omega_i\), contains unobserved variables, which we assume are uncorrelated with the explanatory variables, and random measurement error. As in the previous section, functional form is to a large degree an empirical question. There are good reasons why \(t_{ij}\) may be nonlinear in its determinants. For example, if country \(j\) does not have a container port, then country \(i\) will not benefit from its own container facilities in exporting to \(j\).\footnote{Even if the true transport cost function \(T^*\) is linear, there is no reason for the reduced form of the transport cost rate \(t^*\) to have the same functional form. The reason for this is that for small exporters (facing a perfectly elastic demand) the \textit{FOB} price, \(p_{ij}\), will itself depend on the average transport cost between themselves and their importers, an effect captured by the reduced form of \(t_{ij}^*\).} We found that the log linear form fitted the \textit{CIF/FOB} data considerably better than the linear one.

Several questions have been raised about the use of this \textit{CIF/FOB} transport cost data.\footnote{See Hummels (1998a).} The first is that the measure aggregates over all commodities imported,
so it is biased if trade on high transport cost routes systematically involves lower transport cost goods. This suggests that our estimates in fact will underestimate the true magnitude of transport costs. The second is the presence of measurement error, arising particularly from the fact that exports are not always accurately reported. To the extent that this measurement error is uncorrelated with the explanatory variables, this should not be a problem.

We deal with three other data problems as follows. First, approximately 25 percent of potential bilateral trade flows are dropped because of missing data from one of the partner countries. Second, some countries had CIF import values lower than the corresponding FOB export values, which would imply negative costs; we dropped all such observations. Third, we also dropped values when they were imputed by the IMF for a CIF/FOB ratio of 1.10. Table A-1 provides further details on sample selection. In section III, we compare the results obtained using the CIF/FOB data with those from the shipping cost data. The comparison indicates that the CIF/FOB data contain information about the cross-sectional variation in transport costs that is consistent with the shipping cost data.

The model is estimated with 1990 data for a sample of 103 countries. Deleting observations that are missing, estimated, or give negative transport costs leaves 4,615 observations. Approximately 22 percent of all country pairs in our sample are reported to have no trade. One important reason for this is that at high enough transport costs two countries will not find it profitable to trade. This implies that for these countries, the transport cost measure is censored at some upper limit and this motivates our use of an upper limit Tobit. We assume that for those countries that report zero trade, the transport cost of trading takes the value of the upper limit in the sample.

**Estimation Results**

Table 2 gives the results from the estimation of equation 4. The first two rows of the table are characteristics of the journey between i and j; the log of distance, \( \ln(\text{distance}) \), and whether i and j share a common border (\( \text{border} \)). The remainder are characteristics of the importer country and its trading partner; a dummy for an island (\( \text{isldummy} \) and \( \text{pisldummy} \)); the per capita income of the importing and exporting countries, \( \ln(Y/cap) \) and \( \ln(pY/cap) \). Finally, the infrastructure measures (\( \ln(\text{inf}) \) and \( \ln(p\text{inf}) \)) and the infrastructure of transit countries (\( \ln(1 + \text{infran}) \) and \( \ln(1 + p\text{infran}) \)).

The first column of the table gives the effect of distance alone, and the second column gives a specification with journey and country characteristics, apart from infrastructure. Distance and border effects are as expected. Being or trading with an island reduces transport costs (although these effects are barely significant), and high per capita income reduces transport costs. The infrastructure variables are included in the third column, and all are significant with the

14. Hummels (1998b) discusses the cross-commodity variation in transport costs using disaggregated data for four countries.
TABLE 2. The Bilateral Transport Cost Factor, 1990

<table>
<thead>
<tr>
<th>Variable</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
</tr>
</thead>
<tbody>
<tr>
<td>Distance (ln(distance))</td>
<td>0.25***</td>
<td>0.23***</td>
<td>0.21***</td>
<td>0.38***</td>
</tr>
<tr>
<td></td>
<td>(6.74)</td>
<td>(6.02)</td>
<td>(5.65)</td>
<td>(10.17)</td>
</tr>
<tr>
<td>Common border (border)</td>
<td>-1.35***</td>
<td>-1.36***</td>
<td>-1.02***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-7.77)</td>
<td>(-7.78)</td>
<td>(-6.30)</td>
<td></td>
</tr>
<tr>
<td>Island (isldummy)</td>
<td>-0.12***</td>
<td>-0.12</td>
<td>-0.09</td>
<td>-0.06</td>
</tr>
<tr>
<td></td>
<td>(-1.73)</td>
<td>(-1.23)</td>
<td>(-0.94)</td>
<td></td>
</tr>
<tr>
<td>Island (pisldummy)</td>
<td>-0.16**</td>
<td>-0.12*</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-2.18)</td>
<td>(-1.65)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Per capita income (lnY/cap)</td>
<td>-0.31***</td>
<td>-0.23***</td>
<td>-0.24***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-19.97)</td>
<td>(-9.64)</td>
<td>(-10.78)</td>
<td></td>
</tr>
<tr>
<td>Per capita income (lnpY/cap)</td>
<td>-0.45***</td>
<td>-0.30***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-27.94)</td>
<td>(-12.84)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Infrastructure (lninf)</td>
<td>0.34***</td>
<td>0.36***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(3.92)</td>
<td>(4.47)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Partner infrastructure (lnpinf)</td>
<td>0.66***</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(7.64)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Infrastructure of transit country ln(1 + infran)</td>
<td>0.21**</td>
<td>0.36***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(2.15)</td>
<td>(4.07)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Infrastructure of partner's transit country ln(1 + pinftran)</td>
<td>0.24***</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(2.51)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pseudo-R²</td>
<td>0.10</td>
<td>0.46</td>
<td>0.48</td>
<td>0.60</td>
</tr>
<tr>
<td>σ</td>
<td>1.92</td>
<td>1.70</td>
<td>1.69</td>
<td>1.53</td>
</tr>
</tbody>
</table>

*Significant at the 10 percent level.
**Significant at the 5 percent level.
***Significant at the 1 percent level.

Note: The dependent variable is ln transport cost factor CIF/FOB, ln Int,. All variables are in natural logs, except for the border variables and the island dummies. The sample size is 4,516; Tobit estimates. The pseudo-R² is given by the correlation of actual and predicted Int,. Constants are included but not reported. Exporter fixed effects are included in column four but not reported. σ is the standard error of the Tobit estimate. t-statistics are in parentheses. The Tobit coefficients correspond to the marginal effects for the full sample, including the zeros. See table A-1 for data descriptions and sources and table A-2 for the countries included in the sample. The original transit variable, infran, ranges from 0 for the coastal economies to approximately 1.7. Before taking the log, we add 1 to the measure to correctly reflect that coastal economies bear no extra infrastructure transport cost. To compare the own and transit elasticities, we need to multiply the coefficient of lninftran (reported above) by infran / (1 + infran). This ratio ranges from 0.40 to 0.63 for landlocked countries in this sample.

Source: Authors' calculations.

The results contain several important messages. The first is the quantitative importance of the infrastructure effects. If a country could improve its infrastructure from the median to the top 25th percentile, then its CIF/FOB factor would fall from 1.28 to 1.11, this being equivalent to becoming 2,358 km
closer to all its trading partners.\textsuperscript{15} Conversely, deterioration in infrastructure from the median to the 75th percentile raises the predicted CIF/FOB factor from 1.28 to 1.40, equivalent to becoming 2,016 km further away from all trading partners.

We can ask a similar question for the border effect. How much closer must two otherwise identical countries be if they do not share a border and are to have the same transport costs? The answer is that they would need to be 932 km closer—compared with a mean distance between capitals of bordering countries of 1,000 km.\textsuperscript{16} Thus the positive border effect on trade—which is typically found in gravity model estimates—is very important for transport cost reasons other than distance, suggesting that trans-shipment costs and the integration of transport networks are quite important. We turn to the cost of being landlocked in more detail in section III.

Finally, it is worth comparing our estimates with those using distance, the simple and most commonly used proxy for transport costs. As shown by the pseudo-\(R^2\), using distance alone explains only 10 percent of the variation of transport costs, compared with almost 50 percent when the remaining geography and infrastructure measures are added. Clearly, distance fails to explain a significant part of the variation in transport costs.

\section{II. Trade Volumes}

Instead of looking directly at trade costs, we now look at the trade flows they support, by estimating a gravity model including the infrastructure variables used above. There are two main reasons for doing this. First, the variables identified as being important in the transport cost equations should also be important in the trade equations, and we want to confirm that this is so. Second, by using the same variables in estimating transport costs and trade equations, we are able to compute estimates of elasticities of trade flows with respect to transport costs.

The gravity equation is the standard analytical framework for the prediction of bilateral trade flows. Its empirical use in the context of international trade dates back to the early 1960s and theoretical underpinnings were developed later.\textsuperscript{17} Despite the abundant number of theoretical derivations of the gravity equation, the majority of the authors do not model transport costs explicitly, exceptions being Bergstrand (1985) and Deardorff (1998). More recently, Bougheas and others (1999) incorporate transport infrastructure in

\textsuperscript{15} This uses estimates from the fourth column in table 2, and evaluated at the median CIF/FOB ratio of 1.28 and the median distance of 7,555 km, respectively, so \(1.11 = 1.28 \times (0.95/1.41)^{0.36}\) and \(2,358 = 7,555 \times 7,555^{**} (0.95/1.41)^{0.36/0.38}\).

\textsuperscript{16} Evaluated at the mean distance for bordering countries of 1,000 km, as new distance = 1,000 \* \exp(-1.02/38).

\textsuperscript{17} See Frankel (1997) for a discussion of earlier references. For different theoretical underpinnings, see Anderson (1979), Bergstrand (1985).
a two-country Ricardian model and show the circumstances under which it affects trade volumes.\textsuperscript{18}

Bilateral imports, \( M_{ij} \), depend on GDP in countries \( i \) and \( j \) (\( Y_i \) and \( Y_j \)) in the standard way, and on the transport cost factor, \( t_{ij} \), which we model in terms of the geographical and infrastructure measures used in the preceding analysis. Therefore, we have

\[
M_{ij} = \Phi Y_i^{\phi_1} Y_i^{\phi_2} t_{ij} \epsilon_{ij}, \quad \text{or}
\]

\[
\ln M_{ij} = \phi_0 + \phi_1 \ln Y_i + \phi_2 \ln Y_j + \tau [\beta \ln x_i + \gamma \ln x_j + \delta \ln x_{ij}] + \eta_{ij},
\]

where the second equation is obtained by taking logs and substituting out the true transport cost rate as given by equation 4. We estimate the second equation in expression 5 in the form:

\[
\ln M_{ij} = \phi_0 + \phi_1 \ln Y_i + \phi_2 \ln Y_j + \phi_3 \ln \text{distance}_{ij} + \phi_4 \text{border}_{ij} + \phi_5 \text{isldummy}_j + \phi_6 \text{isldummy}_i + \phi_7 \ln \text{inf} + \phi_8 \ln \text{inftran} + \phi_9 \ln (1 + \text{inftran}) + \phi_{10} \ln (1 + \text{inftran}_i) + \phi_{11} \ln (Y_i / \text{cap}) + \phi_{12} \ln (Y_j / \text{cap}) + \eta_{ij},
\]

where \( M_{ij} \) represents country \( j \)'s imports from \( i \) valued cif, \( Y_i \) is GDP, distance is distance between countries, border is whether they share a border, isldummy is a dummy for island countries, inf is the infrastructure measure, inftran is the infrastructure measure for the transit country, and \( Y/cap \) is per capita GDP.\textsuperscript{19}

The model is estimated by Tobit using the same data set as for transport costs. In the sample used, 22 percent of all observations are reported as zeros, in which case the import values are set equal to the censoring point, which is the minimum value in the sample.

\textbf{Estimation Results}

Table 3 contains the results of the estimation. Income, distance, border, and island effects have the expected signs, as usual in gravity estimates. The striking result is the strong performance of the infrastructure variables used in the preceding analysis. First, all infrastructure variables (importer, exporter, and transit if either country is landlocked) have the correct sign and are significant at the 1-percent level. Moreover, they have sizable effects on trade volumes. Moving from the median to the top 25th percentile in the distribution of infrastructure raises trade volumes by 68 percent, equivalent to being 2,005 km closer to other countries.\textsuperscript{20} Moving from the median to the bottom 75th percentile reduces trade

\textsuperscript{18} Bougheas and others (1999) estimate augmented gravity equations for a sample limited to nine European countries. They include the product of partner's kilometers of motorway in one specification and that of public capital stock in another and find that these have a positive partial correlation with bilateral exports.

\textsuperscript{19} The transit infrastructure variables are adjusted for neighboring countries, so if \( i \) and \( j \) are neighbors and \( j \) (\( i \)) is landlocked, then \( \text{inftran}_j \) (\( \text{inftran}_i \)) is set to zero since no transit country must be used. So, to be more precise, in equation 5' we should write for \( j \) \( \text{inftran}_j \cdot (1 - \text{border}_{ij}) \) not \( \text{inftran}_j \), and similarly for \( i \).

\textsuperscript{20} This uses estimates from the fourth column, and evaluated at the median distance of 7,555 km, so \( 1.68 = (0.95/1.41)^{(-1.32)} \) and \( 2,005 = 7,555 - 7,555 \cdot (0.95/1.41)^{(-1.32/1.69)} \).
TABLE 3. The Gravity Model of Bilateral Imports, 1990

<table>
<thead>
<tr>
<th>Variable</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
</tr>
</thead>
<tbody>
<tr>
<td>Income (lnY)</td>
<td>1.28***</td>
<td>1.05***</td>
<td>0.99***</td>
<td>1.03***</td>
</tr>
<tr>
<td></td>
<td>(53.51)</td>
<td>(30.30)</td>
<td>(28.04)</td>
<td>(31.30)</td>
</tr>
<tr>
<td>Income of trading partner (lnpY)</td>
<td>1.55***</td>
<td>1.35***</td>
<td>1.28***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(60.57)</td>
<td>(37.48)</td>
<td>(34.67)</td>
<td></td>
</tr>
<tr>
<td>Distance (Indistance)</td>
<td>-1.65***</td>
<td>-1.43***</td>
<td>-1.37***</td>
<td>-1.69***</td>
</tr>
<tr>
<td></td>
<td>(-24.07)</td>
<td>(-18.70)</td>
<td>(-18.03)</td>
<td>(-22.40)</td>
</tr>
<tr>
<td>Common border (border)</td>
<td>2.45***</td>
<td>2.52***</td>
<td>1.85***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(7.03)</td>
<td>(7.25)</td>
<td>(5.67)</td>
<td></td>
</tr>
<tr>
<td>Island (isldummy)</td>
<td>0.48***</td>
<td>0.35**</td>
<td>0.41***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(3.23)</td>
<td>(2.46)</td>
<td>(3.06)</td>
<td></td>
</tr>
<tr>
<td>Island (pisldummy)</td>
<td>0.48***</td>
<td>0.40***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(3.34)</td>
<td>(2.78)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Per capita income (lnY/cap)</td>
<td>0.41***</td>
<td>0.16***</td>
<td>0.12**</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(8.78)</td>
<td>(2.96)</td>
<td>(2.28)</td>
<td></td>
</tr>
<tr>
<td>Per capita income (lnpY/cap)</td>
<td>0.34***</td>
<td>0.16***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(7.29)</td>
<td>(3.04)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Infrastructure (lninf)</td>
<td>-1.32***</td>
<td>-1.32***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-7.49)</td>
<td>(-8.07)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Partner infrastructure (lnpinf)</td>
<td>-1.11***</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-6.26)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Infrastructure of transit country ln(1 + infran)</td>
<td>-0.60***</td>
<td>-0.77***</td>
<td>-0.45**</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-3.04)</td>
<td>(-4.18)</td>
<td>(-2.26)</td>
<td></td>
</tr>
<tr>
<td>Infrastructure of partner’s transit country ln(1 + pinftran)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Pseudo-$R^2$ 0.79 0.80 0.80 0.83
$\sigma$ 3.47 3.39 3.34 3.08

*Significant at the 5 percent level.
**Significant at the 1 percent level.

Note: The dependent variable is bilateral imports, $\ln M_{ij}$. The sample size is 4,516; Tobit estimates. The pseudo-$R^2$ is given by the correlation of actual and predicted $\ln M_{ij}$. Constants are included but not reported. $\sigma$ is the standard error of the Tobit estimate. All variables and sample selection are as in table 2. $t$-statistics are in parentheses.

Source: Authors' calculations.

volumes by 28 percent, equivalent to being 1,627 km further away from trading partners.

III. COMPARISON AND QUANTIFICATION

In this section, we compare the results in a way that facilitates the assessment of the quantitative importance of infrastructure and geographical location for transport costs and trade.

The Cost of Being Landlocked

Table 4 shows the disadvantage of being landlocked, relative to being an average coastal country, for different values of own and transit country infrastruc-
TABLE 4. The Cost of Being Landlocked, Relative to a Coastal Economy, 1990

<table>
<thead>
<tr>
<th>Transit infrastructure percentile</th>
<th>Own infrastructure percentile</th>
</tr>
</thead>
<tbody>
<tr>
<td>25th</td>
<td>1.33 1.48 1.67</td>
</tr>
<tr>
<td>Median</td>
<td>1.41 1.55 1.74</td>
</tr>
<tr>
<td>75th</td>
<td>1.51 1.65 1.84</td>
</tr>
<tr>
<td>Shipping data: transport cost ratio</td>
<td>1.31 1.43 1.65</td>
</tr>
<tr>
<td>Median</td>
<td>1.34 1.46 1.69</td>
</tr>
<tr>
<td>75th</td>
<td>1.37 1.49 1.72</td>
</tr>
<tr>
<td>CIF/FOB data: (CIF/FOB - 1) ratio</td>
<td>0.55 0.42 0.26</td>
</tr>
<tr>
<td>Median</td>
<td>0.53 0.40 0.25</td>
</tr>
<tr>
<td>75th</td>
<td>0.50 0.38 0.24</td>
</tr>
</tbody>
</table>

Note: The construction of the variables for the shipping and CIF/FOB data is as follows: we calculate the predicted transport cost for landlocked countries allowing \( \text{inf} \) and \( \text{infran} \) to vary as well as the landlocked dummy, but keeping all other variables at the level of the representative coastal country (median value over nonislands). This is then divided by the predicted transport cost (or by CIF/FOB - 1) for the representative coastal country. For the trade volume data, a similar procedure is used. The percentiles are taken over the sample of landlocked countries. The specifications used are column 3 in table 1, column 3 in table 2, and column 3 in table 3. See table A-2 for the countries included in the sample.

Source: Authors' calculations.

The shipping data indicate that the median landlocked country has transport costs 55 percent higher than the median coastal economy. However, improving own infrastructure to the level of the best 25th percentile among landlocked countries cuts this cost penalty to 41 percent, improvement by the transit country cuts the penalty to 48 percent, and if both improvements are made the penalty drops to 33 percent. Using the CIE/FOB measure, table 4 reports ratios of CIE/FOB – 1 for landlocked countries relative to the median coastal economy. This gives slightly smaller cost penalties, with the median landlocked economy's transport costs 46 percent higher than the median coastal economy's. Improving own and transit country infrastructure to the 25th percentile reduces this penalty to 34 percent and 43 percent, respectively; if both are improved the penalty drops to 31 percent.

Comparison of these results assures us that the estimates from our different data sources are consistent, and that the cross-sectional variation in the CIE/FOB measure does contain useful information regarding transport costs. Although the CIE/FOB data predict relative costs that are 9 percentage points lower than the shipping data at the median infrastructure values, the partial effects of the own and transit infrastructure variables are similar across the data sets, as illustrated.
in figure A-1 in the appendix. The similarity between the predicted effects on relative transport costs is particularly striking in the case of own infrastructure.

Table 4 undertakes an analogous experiment for trade volumes, asking how the volume of trade of representative landlocked economies compares with the average coastal economy at the same income levels and distance. The difference is dramatic, with the median landlocked economy having only 40 percent of the trade volume. Improvements in own infrastructure from the median to the 25th percentile increase the volume of trade by 13 percentage points, improvement in transit country infrastructure increase the volume by 2 percentage points, and a simultaneous improvement leads to an increase of 15 percentage points in the volume of trade.

**The Elasticity of Trade with Respect to Transport Costs**

It is natural to link our estimates of trade volumes and transport costs by computing the elasticity of trade volumes with respect to the transport cost factor as given by the parameter $\tau$ in equation 5. In this subsection we offer two approaches to doing this, one based on comparison of the estimates of the CIF/FOB and gravity models, and the other based on regression of trade volumes on predicted trade costs.

The estimates from the CIF/FOB and gravity models (equations 4 and 5) provide overidentifying restrictions for $\tau$, one for each of the determinants in the transport cost equations. We focus on the estimates of distance, border, and own and transit country infrastructure. The elasticities previously found in the gravity estimation and the CIF/FOB estimation are reproduced in the first two columns of table 5. The last column gives the predicted elasticity of trade with respect to the transport cost factor, $\hat{\tau}$, obtained as the ratio of the gravity and CIF/FOB elasticities.

The point estimates of $\tau$ vary quite widely, from $-6.47$ on the distance variable, to $-1.67$ for the price of infrastructure. The likely reason for this is that some of the variables influence trade volumes through channels other than measured transport costs. For example, distance and border effects might be expected to influence trade volumes through such channels as information flows and language and cultural ties, which would not show up in measured transport costs.

Our second approach is to use predicted values of transport costs (from equation 4) as independent variables in the gravity model (equation 5). In estimating this, we exclude variables that, a priori, we think only affect trade volumes.

21. Of the other two variables, it is likely that income per capita may enter the gravity equation for reasons other than transport costs, and the island dummy is not significant.

22. Geracci and Prewo (1977) estimate $\tau$ for a sample of 18 Organisation for Economic Co-operation and Development (OECD) countries. They find a higher elasticity ($\tau = -10$) than the one we find. This is possibly because of the restriction of their sample to high-income countries. More important perhaps is the fact that they do not estimate an upper limit Tobit for the transport cost. This is likely to lead to an underestimate of the predicted transport cost factor and a consequent upward bias of the transport cost elasticity.
TABLE 5. Estimates of Import Elasticity with Respect to the Transport Cost Factor, 1990

<table>
<thead>
<tr>
<th>Variable</th>
<th>Gravity</th>
<th>CIF/FOB</th>
<th>Trade</th>
</tr>
</thead>
<tbody>
<tr>
<td>Distance (Indistance)</td>
<td>-1.37</td>
<td>0.21</td>
<td>-6.47</td>
</tr>
<tr>
<td>Import country infrastructure (lninf)</td>
<td>-1.32</td>
<td>0.34</td>
<td>-3.86</td>
</tr>
<tr>
<td>Transit country infrastructure ln(1 + infran)</td>
<td>-0.60</td>
<td>0.21</td>
<td>-2.87</td>
</tr>
<tr>
<td>Common border (border)</td>
<td>2.52</td>
<td>-1.36</td>
<td>-1.85</td>
</tr>
<tr>
<td>Partner infrastructure (lnpinf)</td>
<td>-1.11</td>
<td>0.66</td>
<td>-1.67</td>
</tr>
<tr>
<td>Infrastructure of partner's transit country ln(1 + pinftran)</td>
<td>-0.45</td>
<td>0.24</td>
<td>-1.84</td>
</tr>
</tbody>
</table>

Note: We also calculate upper and lower bounds for the trade elasticities using the 95 percent confidence intervals for the gravity and CIF/FOB coefficients. These are distance (-4.28, -10.98); lninf (-1.90, -9.75); ln(1 + infran) (-0.53, -53.65); border (-1.08, -3.15); lnpinf (-0.91, -2.94), and ln(1 + pinftran) (-0.14, -15.64).

'Gravity elasticities correspond to the estimates in column 3 in table 3.

CIF/FOB elasticities correspond to the estimates in column 3 in table 2.

Source: Authors' calculations.

through transport costs (the infrastructure measures), leaving in those that might affect trade volumes directly. Thus, table 6 reports regressions of trade volumes on predicted values of the transport cost factor, incomes, per capita incomes, and distance and border effects. The first column uses predictions of the transport cost factors from the third column in table 2, whereas the second column has partner-country fixed effects, so it uses predictions from the fourth column in table 2.

The coefficient on the predicted transport cost factor, $\hat{\tau}$, measures the elasticity of trade volume with respect to the transport cost factor, $\tau$, and, in column 1 in table 6, this is $-2.24$. Distance remains highly significant, although the coefficient falls markedly compared with the gravity estimates in table 3. This suggests that distance affects trade volumes both through transport costs and independently through other channels, such as information, which could account for the large value of $\hat{\tau}$ associated with the distance coefficients in table 5. Of the other variables, the border coefficient is insignificant, while incomes per capita enter with a negative sign, suggesting that, controlling for transport costs, countries with low per capita income trade more than countries with high per capita income. The second column reports analogous results when partner-country fixed effects are included. The main difference is that this increases the absolute value of the estimated elasticity $\tau$ to $-3.11$, while reducing further the independent role of distance.

Taking tables 5 and 6 together enables us to make an informed judgment about the quantitative importance of transport costs in determining trade flows. Re-

23. Because this is the transport cost factor, an increase from, say, 1.1 to 1.2 is a 9 percent increase, not a doubling.
TABLE 6. Trade Volumes and Predicted Import Costs, 1990

<table>
<thead>
<tr>
<th>Variable</th>
<th>Based on full modela</th>
<th>Based on fixed-effects modelb</th>
</tr>
</thead>
<tbody>
<tr>
<td>Transport cost factor ln((t_))</td>
<td>-2.24</td>
<td>-3.11</td>
</tr>
<tr>
<td>Import country income lnY</td>
<td>1.01</td>
<td>1.03</td>
</tr>
<tr>
<td>Export country income lnpY</td>
<td>1.26</td>
<td></td>
</tr>
<tr>
<td>Import country per capita income (lnY/cap)</td>
<td>-0.25</td>
<td>-0.59</td>
</tr>
<tr>
<td>Export country per capita income (lnpY/cap)</td>
<td>-0.57</td>
<td></td>
</tr>
<tr>
<td>Distance (ln(distance))</td>
<td>-0.87</td>
<td>-0.51</td>
</tr>
<tr>
<td>Common border (border)</td>
<td>-0.50</td>
<td>-1.39</td>
</tr>
<tr>
<td>Pseudo-R(^2)</td>
<td>0.80</td>
<td>0.83</td>
</tr>
<tr>
<td>(\sigma)</td>
<td>3.35</td>
<td>3.08</td>
</tr>
</tbody>
</table>

Note: The dependent variable is bilateral imports, lnM. The standard error of ln(\(t_\)) is not adjusted for the fact that it is a predicted variable, and therefore underestimates the true estimate error.
aThe dependent variable is from column 3 in table 2.
bThe dependent variable is from column 4 in table 2.
Source: Authors’ calculations.

Results suggest an elasticity of trade flows with respect to the transport cost factor in the range of -2 to -3.5. Taking a value of -3 means that doubling transport costs from their median value (that is, raising the transport cost factor from 1.28 to 1.56) reduces trade volumes by 45 percent. Moving from the median value of transport costs to the 75th percentile (transport cost factor 1.83) cuts trade volumes by two-thirds.

IV. TRANSPORT COSTS, INFRASTRUCTURE, AND SUB-SAHARAN AFRICAN TRADE

Our results show how poor infrastructure and being landlocked damage trade. We now extend the quantitative implications of our findings by applying them to Sub-Saharan African (SSA) trade.24

24. Evidence of the importance of transport costs for Africa’s export performance is given by Amjadi and Yeats (1995) and Amjadi, Reincke, and Yeats (1996). In the former study, it is reported that, according to balance of payments statistics, SSA’s net insurance and freight payments amounted to 15 percent of the value of the exports. By comparison, for all developing countries the payments averaged 5.8 percent. Collier and Gunning (1999, p. 71) provide a brief description of the quantity and quality of infrastructure in SSA.
Is SSA Trade Too Low?

There is a common belief that Africa trades “too little” both with itself and with the rest of the world. Frankel (1997) reports intraregional trade shares in 1990 of 4 percent for Africa compared with 44 percent for East Asia. Amjadi, Reincke, and Yeats (1996) discuss the marginalization of SSA in world trade. The poor performance is typically attributed to protectionist trade policies (Collier 1995; Collier and Gunning 1999) and high transport costs due to poor infrastructure and inappropriate transport policies (Amjadi and Yeats 1995).

This view has been contested by Foroutan and Pritchett (1993), who show that the low level of intra-African trade is explained by the usual determinants of a gravity equation. Similarly, Coe and Hoffmaister (1998) conclude that bilateral trade between SSA countries and industrial countries in the 1990s was not unusually low. Finally, Rodrik (1998) finds that the trade/GDP ratios of SSA countries are comparable to those of countries of similar size and income, and that Africa’s marginalization is mainly due to low income growth.

What evidence does our data provide on this, and to what extent can it be accounted for by the infrastructure variables we have identified as being so important? To answer this we reestimated the baseline and infrastructure specifications of our transport cost and gravity models, augmenting them with African dummy variables: African importer (Africa), African exporter (pAfrica), African importer and exporter (AA), and an interaction of the latter with distance (AAdistance). Tables 7 and 8 provide the estimates for the transport cost equation and the gravity equation, respectively.

Intra-SSA trade costs are substantially higher and trade volumes substantially lower than those for non-SSA countries. In tables 7 and 8, the Africa factor gives the combined effects of the Africa dummies. Intra-SSA transport costs are 136 percent higher (2.36 = exp(0.08 + 0.52 + 0.26) from table 7) and trade volumes are 6 percent lower (0.94 = exp(-0.23 - 0.59 + 0.76) from table 8). Thus the basic specification cannot account for the poor performance of African trade, even when it controls for both geographical variables (border and island dummies) and per capita income.

In tables 7 and 8, the third and fourth columns add the infrastructure measures. The key finding is that infrastructure accounts for nearly half the transport cost penalty borne by intra-SSA trade. The penalty attributable to the Africa dummies drops from 136 to 77 percent. The Africa penalty on trade flows is actually overturned, suggesting that, once we control for infrastructure intra-SSA trade is 105 percent higher than would be expected.

It is sometimes claimed that poor communications infrastructure in Africa entails higher transport costs per kilometer within SSA than elsewhere. We investigate this with the interaction variable AAdistance, which is zero for trade involving one non-African country, and equal to distance for trade between a pair of African countries. Foroutan and Pritchett (1993) use a similar variable and find that it is insignificant, which leads them to conclude that “the gravity model gives little evidence that in fact distance is a greater barrier to intra-SSA
Table 7. Transport Costs of Sub-Saharan African Countries, 1990

<table>
<thead>
<tr>
<th>Variable</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
</tr>
</thead>
<tbody>
<tr>
<td>Income</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Import country (lnY)</td>
<td>0.29***</td>
<td>0.23***</td>
<td>0.26***</td>
<td>0.20***</td>
</tr>
<tr>
<td>Export country (lnpY)</td>
<td>(7.38)</td>
<td>(5.67)</td>
<td>(6.57)</td>
<td>(4.88)</td>
</tr>
<tr>
<td>Distance (indistance)</td>
<td>0.29***</td>
<td>0.23***</td>
<td>0.26***</td>
<td>0.20***</td>
</tr>
<tr>
<td>Common border (border)</td>
<td>(7.38)</td>
<td>(5.67)</td>
<td>(6.57)</td>
<td>(4.88)</td>
</tr>
<tr>
<td>Island dummy</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Import country (isldummy)</td>
<td>0.13*</td>
<td>-0.12*</td>
<td>0.10</td>
<td>0.10</td>
</tr>
<tr>
<td>Export country (pisldummy)</td>
<td>(1.78)</td>
<td>(1.68)</td>
<td>(1.36)</td>
<td>(1.29)</td>
</tr>
<tr>
<td>Per capita income</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Import country (lnY/cap)</td>
<td>0.29***</td>
<td>0.29***</td>
<td>0.23***</td>
<td>0.20***</td>
</tr>
<tr>
<td>Export country (lnpY/cap)</td>
<td>(15.31)</td>
<td>(15.36)</td>
<td>(9.36)</td>
<td>(9.36)</td>
</tr>
<tr>
<td>Infrastructure</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Import country (lninf)</td>
<td>0.32***</td>
<td>0.32***</td>
<td>0.32***</td>
<td>0.32***</td>
</tr>
<tr>
<td>Export country (lnpinf)</td>
<td>(3.47)</td>
<td>(3.59)</td>
<td>(3.59)</td>
<td>(3.59)</td>
</tr>
<tr>
<td>Infrastructure of transit countries</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Import country ln(1 + infran)</td>
<td>0.21**</td>
<td>0.18*</td>
<td>(2.13)</td>
<td>(1.81)</td>
</tr>
<tr>
<td>Export country ln(1 + pinfran)</td>
<td>0.14</td>
<td>0.11</td>
<td>(1.43)</td>
<td>(1.09)</td>
</tr>
<tr>
<td>Africa dummies</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>African importer Africa</td>
<td>0.08</td>
<td>0.09</td>
<td>-0.02</td>
<td>0.00</td>
</tr>
<tr>
<td>African exporter pAfrica</td>
<td>(0.36)</td>
<td>(1.15)</td>
<td>(-0.26)</td>
<td>(0.00)</td>
</tr>
<tr>
<td>African importer and exporter AA</td>
<td>0.52***</td>
<td>0.53***</td>
<td>0.37***</td>
<td>0.39***</td>
</tr>
<tr>
<td>Interaction of AA and distance (ln(1,000 km))</td>
<td>0.81***</td>
<td>0.80***</td>
<td>0.80***</td>
<td>0.80***</td>
</tr>
<tr>
<td>Africa factor</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Africa (1,000 km)</td>
<td>1.18</td>
<td>0.92</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Africa (3,000 km)</td>
<td>2.87</td>
<td>2.11</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Critical distance</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>826</td>
<td>1,110</td>
<td>826</td>
<td>1,110</td>
<td></td>
</tr>
</tbody>
</table>

*Significant at the 10 percent level.
**Significant at the 5 percent level.
***Significant at the 1 percent level.

Note: The dependent variable is ln transport cost factor CIF/FOB. The sample size is 4,516. t-statistics are in parentheses. The pseudo-R^2 is given by the correlation of actual and predicted imports. Constants are included but not reported. \( \sigma \) is the standard error of the Tobit estimate. All variables and sample selection are as in table 2.

aAfrica factor = exp(Africa + pAfrica + AA), or exp(Africa + pAfrica + AA + AAdistance * ln(#km)).
bCritical distance, \( x_c \), is given by: \( 1 - \exp(Africa + pAfrica + AA + AAdistance * \ln(x)) = 0 \).

Source: Authors' calculations.
### TABLE 8. The Gravity Models for Sub-Saharan African Countries, 1990

<table>
<thead>
<tr>
<th>Variable</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Income</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Import country (lnY)</td>
<td>1.05***</td>
<td>1.05***</td>
<td>1.02***</td>
<td>1.02***</td>
</tr>
<tr>
<td></td>
<td>(27.44)</td>
<td>(27.45)</td>
<td>(26.96)</td>
<td>(26.99)</td>
</tr>
<tr>
<td>Export country (lnpY)</td>
<td>1.31***</td>
<td>1.31***</td>
<td>1.28***</td>
<td>1.28***</td>
</tr>
<tr>
<td></td>
<td>(33.47)</td>
<td>(33.45)</td>
<td>(32.69)</td>
<td>(32.70)</td>
</tr>
<tr>
<td>Distance (lndistance)</td>
<td>1.39***</td>
<td>-1.31***</td>
<td>-1.29***</td>
<td>-1.21***</td>
</tr>
<tr>
<td></td>
<td>(17.45)</td>
<td>(16.06)</td>
<td>(-16.29)</td>
<td>(-14.93)</td>
</tr>
<tr>
<td>Common border (border)</td>
<td>2.34***</td>
<td>1.87***</td>
<td>2.42***</td>
<td>1.98***</td>
</tr>
<tr>
<td></td>
<td>(6.70)</td>
<td>(5.14)</td>
<td>(6.96)</td>
<td>(5.49)</td>
</tr>
<tr>
<td>Island dummy</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Import country (isldummy)</td>
<td>0.45***</td>
<td>0.44***</td>
<td>0.35**</td>
<td>0.34**</td>
</tr>
<tr>
<td></td>
<td>(3.14)</td>
<td>(3.07)</td>
<td>(2.41)</td>
<td>(2.37)</td>
</tr>
<tr>
<td>Export country (pisldummy)</td>
<td>0.42***</td>
<td>0.41***</td>
<td>0.37***</td>
<td>0.37***</td>
</tr>
<tr>
<td></td>
<td>(2.89)</td>
<td>(2.83)</td>
<td>(2.57)</td>
<td>(2.53)</td>
</tr>
<tr>
<td><strong>Per capita income</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Import country (lnY/cap)</td>
<td>0.41***</td>
<td>0.41***</td>
<td>0.16***</td>
<td>0.16***</td>
</tr>
<tr>
<td></td>
<td>(8.62)</td>
<td>(8.64)</td>
<td>(2.92)</td>
<td>(2.90)</td>
</tr>
<tr>
<td>Export country (lnpY/cap)</td>
<td>0.32***</td>
<td>0.32***</td>
<td>0.17***</td>
<td>0.17***</td>
</tr>
<tr>
<td></td>
<td>(6.85)</td>
<td>(6.93)</td>
<td>(3.11)</td>
<td>(3.16)</td>
</tr>
<tr>
<td><strong>Infrastructure</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Import country (linf)</td>
<td></td>
<td></td>
<td>-1.44***</td>
<td>-1.45***</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(-7.92)</td>
<td>(-7.99)</td>
</tr>
<tr>
<td>Export country (linpinf)</td>
<td></td>
<td></td>
<td>-1.10***</td>
<td>-1.10***</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(-6.03)</td>
<td>(-6.06)</td>
</tr>
<tr>
<td>Infrastructure of transit countries</td>
<td></td>
<td></td>
<td>-0.62***</td>
<td>-0.58***</td>
</tr>
<tr>
<td>Import country ln(1 + infran)</td>
<td></td>
<td></td>
<td>(-3.13)</td>
<td>(-2.91)</td>
</tr>
<tr>
<td>Export country ln(1 + pinfran)</td>
<td></td>
<td></td>
<td>-0.40**</td>
<td>-0.36*</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(-2.02)</td>
<td>(-1.80)</td>
</tr>
<tr>
<td><strong>Africa dummies</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>African importer Africa</td>
<td>-0.23</td>
<td>-0.25</td>
<td>0.15</td>
<td>0.13</td>
</tr>
<tr>
<td></td>
<td>(-1.29)</td>
<td>(-1.43)</td>
<td>(0.86)</td>
<td>(0.71)</td>
</tr>
<tr>
<td>African exporter pAfrica</td>
<td>-0.59***</td>
<td>-0.62***</td>
<td>-0.31**</td>
<td>-0.34*</td>
</tr>
<tr>
<td></td>
<td>(-3.46)</td>
<td>(-3.58)</td>
<td>(-1.78)</td>
<td>(-1.93)</td>
</tr>
<tr>
<td>African importer and exporter AA</td>
<td>0.76***</td>
<td>9.18***</td>
<td>0.88***</td>
<td>9.00***</td>
</tr>
<tr>
<td></td>
<td>(2.61)</td>
<td>(4.92)</td>
<td>(3.03)</td>
<td>(-4.89)</td>
</tr>
<tr>
<td>Interaction of AA and distance (ln(1,000 km))</td>
<td>-1.08***</td>
<td>-1.04***</td>
<td>(-4.56)</td>
<td>(-4.46)</td>
</tr>
<tr>
<td><strong>Pseudo-R²</strong></td>
<td></td>
<td></td>
<td>0.79</td>
<td>0.79</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>0.80</td>
<td>0.80</td>
</tr>
<tr>
<td><strong>σ</strong></td>
<td></td>
<td></td>
<td>3.38</td>
<td>3.38</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>3.33</td>
<td>3.33</td>
</tr>
<tr>
<td><strong>Africa factor a</strong></td>
<td></td>
<td></td>
<td>0.94</td>
<td>0.94</td>
</tr>
<tr>
<td>Africa (1,000 km)</td>
<td></td>
<td></td>
<td>2.34</td>
<td>2.34</td>
</tr>
<tr>
<td>Africa (3,000 km)</td>
<td></td>
<td></td>
<td>0.71</td>
<td>0.71</td>
</tr>
<tr>
<td>Critical distance b</td>
<td></td>
<td></td>
<td>2.196</td>
<td>4.684</td>
</tr>
</tbody>
</table>

*aSignificant at the 10 percent level.
**Significant at the 5 percent level.
***Significant at the 1 percent level.

*Note:* The dependent variable is bilateral imports, InM. The sample size is 4,516. t-statistics are in parentheses. The pseudo-R² is given by the correlation of actual and predicted imports. Constants are included but not reported. σ is the standard error of the Tobit estimate. All variables and sample selection are as in Table 2.

aAfrica factor = exp(Africa + pAfrica + AA), or exp(Africa + pAfrica + AA + AA distance * ln(#km)).

bCritical distance, x, is given by: 1 - exp(Africa + pAfrica + AA + AA distance * ln(x)) = 0.

*Source:* Authors’ calculations.
trade than it is for other countries. This result goes against the apparently common feeling that the poor quantity and quality of communications and transport infrastructures between SSA countries is a major obstacle to intra-SSA trade.”

We find the opposite, with the second and fourth columns in table 7 indicating that the variable is significant in raising transport costs, and the second and fourth columns in table 8 indicating that it is significant in reducing trade volumes.\(^{25}\) Thus, controlling for infrastructure, African transport costs are 8 percent lower on journeys of 1,000 km, but 121 percent higher on journeys of 3,000 km. One way to summarize the results, including the interaction variable, is to calculate the critical distance above which a pair of African countries faces a penalty compared with a pair of non-African countries. Looking at transport costs, the distance is 826 km, rising to 1,110 km once we control for infrastructure. Looking at trade volumes, the distance is 2,196 km, rising to 4,684 km once infrastructure is included. It is interesting to note that including the infrastructure measures more than doubles the critical distance for trade and that the majority of country pairs in SSA on opposite coasts exceed that critical distance.

Pulling our Africa results together, there are several main conclusions. First, intra-African transport costs are higher and trade volumes lower than would be predicted by a simple model (column one in tables 7 and 8). However, much of this can be attributed to poor infrastructure and to the particularly high cost of distance in Africa. Our results confirm the fact that intra-African trade is concentrated at the subregional level with less east-west trade than would be expected between a pair of otherwise similar countries in the rest of the world.

V. Conclusion

Transport costs and trade volumes depend on many complex details of geography, infrastructure, administrative barriers, and the structure of the shipping industry. In this article, we have used several sources of evidence to explain transport costs and trade flows in terms of geography and the infrastructure of the trading countries, and of countries through which their trade passes.

Table 9 summarizes some of the main results on the impact of infrastructure, reporting levels and changes from the median infrastructure. The results are strongly consistent, although they come from different data sets and measure different things. Thus, deterioration in infrastructure from that of the median country to the 75th percentile raises costs, according to our shipping data, by an amount equivalent to 3,466 km of sea travel or 419 km of overland travel. Using the CIF/FOB ratio, the equivalent distance is 2,016 km. The impact on trade volumes is equivalent to an extra 1,627 km distance.

Linking transport costs to trade volumes, we estimate an elasticity of trade flows with respect to the transport cost factor of around \(-3\). Table 10 summa-

\(^{25}\) The finding in Foroutan and Pritchett (1993) is most likely because the dummy for African countries that export and import and the interaction variable are multicollinear and thus they are not able to identify either. In our sample the correlation between these variables is over 0.9.
TABLE 9. Predicted Effects of Infrastructure on Trade Costs and Trade Volumes, 1990

<table>
<thead>
<tr>
<th>Variable</th>
<th>Infrastructure percentile</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>25th</td>
</tr>
<tr>
<td>Shipping data</td>
<td></td>
</tr>
<tr>
<td>Transport costs, US$</td>
<td>4,638</td>
</tr>
<tr>
<td>Sea km, equivalent change</td>
<td>-3,989</td>
</tr>
<tr>
<td>Land km, equivalent change</td>
<td>-481</td>
</tr>
<tr>
<td>CIF/FOB</td>
<td></td>
</tr>
<tr>
<td>CIF/FOB ratio</td>
<td>1.11</td>
</tr>
<tr>
<td>Kilometers, equivalent change</td>
<td>-2,358</td>
</tr>
<tr>
<td>Gravity</td>
<td></td>
</tr>
<tr>
<td>Trade volume, percentage change</td>
<td>+68</td>
</tr>
<tr>
<td>Kilometers, equivalent change</td>
<td>-2,005</td>
</tr>
</tbody>
</table>

Note: Shipping data are from column 4 in table 1, CIF/FOB data are from column 4 in table 2, and gravity data are from column 4 in table 3. Source: Authors' calculations.

rizes the implications of this. It indicates, for example, how a doubling of transport costs (from the median value) reduces trade volumes by 45 percent.

The article also presents results on the disadvantages faced by landlocked countries and by African countries. From both the shipping and the CIF/FOB data sets, we see that landlocked countries are disadvantaged. The representative landlocked economy has transport costs 50 percent higher and trade volumes 60 percent lower than the representative coastal economy. However, landlocked countries are able to overcome a substantial proportion of this disadvantage through improvements in their own and their transit countries’ infrastructure. Looking at SSA, we see that transport costs are relatively high, and that trade flows are lower than would be predicted by standard gravity modeling both for intra-SSA trade and for African countries’ external trade. We find that most of this poor performance is explained by poor infrastructure and by a particular penalty on long-distance (typically cross-continental) trade in Africa.

TABLE 10. Predicted Effects of the Transport Cost Factor on Trade Volumes, 1990

<table>
<thead>
<tr>
<th>Transport cost factor, τ, selected values</th>
<th>Predicted change in trade volume from median (percent)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1.11 (25th percentile)</td>
<td>+53</td>
</tr>
<tr>
<td>1.14</td>
<td>+42</td>
</tr>
<tr>
<td>1.28 (Median)</td>
<td>0</td>
</tr>
<tr>
<td>1.56</td>
<td>-45</td>
</tr>
<tr>
<td>1.83 (75th percentile)</td>
<td>-66</td>
</tr>
</tbody>
</table>

Note: τ = -3. Source: Authors’ calculations.
Shipping costs of the magnitudes reported here have a major impact on income, both because of the direct cost they impose, and because of the gains from trade forgone. However, our results also point to the potential for reducing these costs through investment in infrastructure.

Appendix. Construction of Variables

Own Infrastructure

Each country’s infrastructure is measured by an index constructed from four variables: kilometers of road, kilometers of paved road, kilometers of rail (each per square kilometer of country area), and telephone main lines per person. These measures are highly correlated among themselves and identifying each of their influences on transport costs separately is not possible. One possibility would have been to build an index using principal components. However, we have data on all of the measures for only 51 countries. Thus, we first normalize the variables to have the same mean, one, and then take the linear average over the four variables, ignoring missing observations. This is equivalent to assuming that roads, paved roads, railways, and telephone lines are perfect substitutes as inputs to a transport services production function.

Taking the mean over the available observations implicitly assumes that the missing variables take on average the same value as the available variables. This measure was raised to the power \(-0.3\). The reason for this is that infrastructure is an input to a transport services production function that, if Cobb Douglas, might be written as \(Y = K^\alpha L^\beta I^\chi\) where \(I\), the index of infrastructure, is exogenous to the transport sector firm. Then for a given output the reduced form of the cost function will be \(T = \phi \lambda^\alpha (\alpha+\beta)\) where \(\phi\) is a function of the factor prices of private inputs, the technology, and the target output. If there are constant returns to scale to the private inputs, \(K\) and \(L\), then our assumption is that \(\chi = 0.3\). According to the data, this value implies that the transport cost per kilometer of the worst infrastructure is approximately ten times that of the best one. In the log-linear specifications this scaling is only a choice of units.

Transit Infrastructure

Let \(L\) denote a given landlocked country and \(L_t\) the set of transit countries \(L\) uses to reach the sea (table A-3). Ideally, we weight transit countries’ infrastructure by their share of the transit trade. However, available data report solely whether a country is used for transit, so if country \(L\) uses \(n\) transit countries, the variable \(\text{infran}\) gives an equal weight of \(1/n\) to the infrastructure index of each of those countries. Two caveats should be noted. First, we are assuming that no trade (or the same share of trade for all countries) goes by air. Although this is clearly unrealistic and the share of trade that is airborne is rising, it is still small enough for landlocked countries to justify this assumption. Second, the transport cost from landlocked to neighboring countries should not include transit country costs and thus, when necessary, our variable is adjusted to reflect this fact.
FIGURE A-1. The Transport Cost of Landlocked Countries Relative to an Average Coastal Country
<table>
<thead>
<tr>
<th>Variable</th>
<th>Description</th>
<th>Source</th>
<th>Use</th>
</tr>
</thead>
<tbody>
<tr>
<td>distance</td>
<td>Great circle distance between trading partners (1000s km unless ln is used).</td>
<td>Fitzpatrick (1986), authors’ calculations</td>
<td>All</td>
</tr>
<tr>
<td>distsea</td>
<td>Sea distance around continents from Baltimore to the sea port of landfall (1000s km).</td>
<td>DMA (1985), authors’ calculations</td>
<td>Shipping</td>
</tr>
<tr>
<td>distland</td>
<td>Great circle distance from sea port of landfall to capital of destination (1000s km).</td>
<td>Authors’ calculations</td>
<td>Shipping</td>
</tr>
<tr>
<td>border</td>
<td>Dummy variable = 1 if two countries are contiguous or are separated by less than 40 km, 0 otherwise.</td>
<td>CIA (1998)</td>
<td>C IF/FOB, gravity</td>
</tr>
<tr>
<td>inf</td>
<td>Inverse of the index of road, paved road and railway densities and telephone lines per capita. A higher value indicates worse infrastructure (see below for more details).</td>
<td>Canning 1998, authors’ calculations</td>
<td>All</td>
</tr>
<tr>
<td>inftran</td>
<td>Average value of infrastructure for the transit countries if a country is landlocked, zero otherwise. Table A-3 below lists the landlocked countries with respective transit countries used.</td>
<td>Canning 1998, UNCTAD, authors’ calculations</td>
<td>All</td>
</tr>
<tr>
<td>ldldummy</td>
<td>Dummy variable = 1 if the country is landlocked, 0 otherwise.</td>
<td>CIA (1998)</td>
<td>All</td>
</tr>
<tr>
<td>isl dummy</td>
<td>Dummy variable = 1 if the country is an island, 0 otherwise.</td>
<td>CIA (1998)</td>
<td>Gravity, CIF/FOB</td>
</tr>
<tr>
<td>T_i</td>
<td>Cost of shipping a 40’ container from i = Baltimore to country j (1000s US$, 1999). The mode is surface (as opposed to air), type is freight (as opposed to household goods) and packing is loose (as opposed to lift van where the cargo is packed into wooden containers). The cost does not include insurance.</td>
<td>Panalpina (private communication)</td>
<td>Shipping</td>
</tr>
<tr>
<td>M_j</td>
<td>Aggregate imports (inclusive of insurance and freight, CIF) of country j from country i 1000s current (1990) US$.</td>
<td>IMF (various years)</td>
<td>Gravity</td>
</tr>
<tr>
<td>X_j</td>
<td>Aggregate exports (free on board value ) of country i to country j 1000s current (1990) US$.</td>
<td>IMF (various years)</td>
<td>Gravity</td>
</tr>
<tr>
<td>t_j</td>
<td>M_j /X_j.</td>
<td>IMF (various years)</td>
<td>CIF/FOB</td>
</tr>
<tr>
<td>Y/cap</td>
<td>Y/population.</td>
<td>World Bank (1998)</td>
<td>All</td>
</tr>
</tbody>
</table>

Note: In the text In variable stands for the natural logarithm of variable, pvariable stands for the trade partner’s variable. There are 103 countries in the sample used in sections I and II. This implies 10,712 potential bilateral pairs. The sample is greatly reduced because 2,759 of the pairs had missing import or export values; 555 had positive imports of j from i, but exports of zero from i to j; 2,494 had nonpositive transport costs; and 195 had CIF/FOB between 1.0909 and 1.101.
### Table A-2. List of Countries in the Samples
(sorted by quality of own infrastructure)

<table>
<thead>
<tr>
<th>Shipping data sample</th>
<th>IMF data sample</th>
</tr>
</thead>
<tbody>
<tr>
<td>Belgium</td>
<td>Mauritius</td>
</tr>
<tr>
<td>Netherlands</td>
<td>Oman</td>
</tr>
<tr>
<td>Switzerland</td>
<td>Costa Rica</td>
</tr>
<tr>
<td>Austria</td>
<td>Turkey</td>
</tr>
<tr>
<td>Italy</td>
<td>Austria</td>
</tr>
<tr>
<td>Germany</td>
<td>Japan</td>
</tr>
<tr>
<td>Hungary</td>
<td>Hong Kong, China</td>
</tr>
<tr>
<td>Rwanda</td>
<td>Denmark</td>
</tr>
<tr>
<td>Uruguay</td>
<td>Germany</td>
</tr>
<tr>
<td>Turkey</td>
<td>United Kingdom</td>
</tr>
<tr>
<td>India</td>
<td>United Arab Emirates</td>
</tr>
<tr>
<td>South Africa (25th)</td>
<td>United States (25th)</td>
</tr>
<tr>
<td>South Africa (50th)</td>
<td>United States (50th)</td>
</tr>
<tr>
<td>Switzerland (75th)</td>
<td>United States (75th)</td>
</tr>
<tr>
<td>Austria (50th)</td>
<td>Australia</td>
</tr>
<tr>
<td>Italy</td>
<td>Brazil</td>
</tr>
<tr>
<td>Germany</td>
<td>Chile</td>
</tr>
<tr>
<td>Hungary</td>
<td>Crimea</td>
</tr>
<tr>
<td>Rwanda</td>
<td>India</td>
</tr>
<tr>
<td>Switzerland</td>
<td>China</td>
</tr>
<tr>
<td>Austria</td>
<td>Ethiopia</td>
</tr>
<tr>
<td>Italy</td>
<td>Finland</td>
</tr>
<tr>
<td>Germany</td>
<td>Romania</td>
</tr>
<tr>
<td>Hungary</td>
<td>Korea, Rep. of</td>
</tr>
<tr>
<td>Rwanda</td>
<td>Greece</td>
</tr>
<tr>
<td>Switzerland</td>
<td>United Arab Emirates</td>
</tr>
<tr>
<td>Austria</td>
<td>Uruguay</td>
</tr>
<tr>
<td>China</td>
<td>United States (75th)</td>
</tr>
</tbody>
</table>

*Excluded from columns 3 and 4 in table 1 due to missing data for own or transit infrastructure.

**Note:** Not all country pairs were used due to missing data. Countries with infrastructure values closest to the corresponding sample values are labeled 25th, 50th, and 75th.

**Source:** For shipping data, see text; for IMF data, IMF (various years).
TABLE A-3. List of Transit Countries for Landlocked Countries in the Shipping Data Sample and the IMF Data Sample (sorted by quality of transit country infrastructure)

<table>
<thead>
<tr>
<th>Landlocked countries</th>
<th>Transit countries</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Shipping data sample</strong></td>
<td></td>
</tr>
<tr>
<td>Austria</td>
<td>Germany</td>
</tr>
<tr>
<td>Hungary</td>
<td>Germany</td>
</tr>
<tr>
<td>Switzerland</td>
<td>Germany</td>
</tr>
<tr>
<td>Bhutan</td>
<td>India</td>
</tr>
<tr>
<td>Nepal</td>
<td>India</td>
</tr>
<tr>
<td>Botswana (25th)</td>
<td>South Africa</td>
</tr>
<tr>
<td>Lesotho (25th)</td>
<td>South Africa</td>
</tr>
<tr>
<td>Swaziland (25th)</td>
<td>South Africa</td>
</tr>
<tr>
<td>Zimbabwe (25th)</td>
<td>South Africa</td>
</tr>
<tr>
<td>Zambia</td>
<td>South Africa, Zimbabwe</td>
</tr>
<tr>
<td>Paraguay (50th)</td>
<td>Brazil</td>
</tr>
<tr>
<td>Bolivia</td>
<td>Chile</td>
</tr>
<tr>
<td>Malawi</td>
<td>South Africa, Zimbabwe</td>
</tr>
<tr>
<td>Burundi</td>
<td>Kenya</td>
</tr>
<tr>
<td>Uganda</td>
<td>Kenya</td>
</tr>
<tr>
<td>Rwanda (75th)</td>
<td>Kenya, Tanzania</td>
</tr>
<tr>
<td>Central African Republic</td>
<td>Cameroon</td>
</tr>
<tr>
<td>Chad</td>
<td>Cameroon</td>
</tr>
<tr>
<td>Burkina Faso</td>
<td>Côte d'Ivoire</td>
</tr>
<tr>
<td>Mali</td>
<td>Côte d'Ivoire</td>
</tr>
<tr>
<td>Niger</td>
<td>Benin</td>
</tr>
<tr>
<td><strong>IMF data sample</strong></td>
<td></td>
</tr>
<tr>
<td>Switzerland</td>
<td>Germany, Italy, Netherlands</td>
</tr>
<tr>
<td>Hungary</td>
<td>Austria, Italy</td>
</tr>
<tr>
<td>Austria</td>
<td>Germany, Italy</td>
</tr>
<tr>
<td>Laos PDR</td>
<td>Thailand, Vietnam</td>
</tr>
<tr>
<td>Zambia (25th)</td>
<td>Mozambique, Tanzania, South Africa</td>
</tr>
<tr>
<td>Zimbabwe (25th)</td>
<td>Mozambique, Tanzania, South Africa</td>
</tr>
<tr>
<td>Nepal</td>
<td>Bangladesh, India</td>
</tr>
<tr>
<td>Paraguay</td>
<td>Argentina, Brazil, Chile, Uruguay</td>
</tr>
<tr>
<td>Bolivia</td>
<td>Argentina, Brazil, Chile, Peru</td>
</tr>
<tr>
<td>Central African Republic (50th)</td>
<td>Cameroon; Congo, Rep. of; Congo, Dem. Rep. of</td>
</tr>
<tr>
<td>Burundi</td>
<td>Kenya, Tanzania, Uganda</td>
</tr>
<tr>
<td>Mali</td>
<td>Burkina Faso, Côte d'Ivoire, Senegal</td>
</tr>
<tr>
<td>Rwanda</td>
<td>Burundi, Kenya, Tanzania, Uganda</td>
</tr>
<tr>
<td>Chad (75th)</td>
<td>Cameroon, Nigeria</td>
</tr>
<tr>
<td>Malawi</td>
<td>Botswana, Mozambique, Zambia, Zimbabwe</td>
</tr>
<tr>
<td>Niger</td>
<td>Benin, Burkina Faso, Nigeria, Togo</td>
</tr>
<tr>
<td>Burkina Faso</td>
<td>Côte d'Ivoire, Togo</td>
</tr>
<tr>
<td>Uganda</td>
<td>Kenya, Tanzania</td>
</tr>
</tbody>
</table>

Note: 25th, 50th, and 75th denote the countries with transit infrastructure values closest to these percentile values.

*Transit countries coincide with the port of entry reported by the shipping company. In the case of Zambia and Malawi, Zimbabwe is also a transit country. The countries for which there are no transit or own-infrastructure data (see note in table A-2) are not included here as they were not used in the restricted sample.

*Without specific knowledge of the source of the import and transit route, we must take the average infrastructure measure over all the transit countries reported by UNCTAD (see table A-1).
### Table A-4. Summary Statistics for Shipping Data Sample, 1998

<table>
<thead>
<tr>
<th>Variable</th>
<th>Full sample</th>
<th>Mean</th>
<th>Standard deviation</th>
<th>Landlocked countries</th>
<th>Coastal countries</th>
</tr>
</thead>
<tbody>
<tr>
<td>Whole sample</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Transport cost $T$</td>
<td></td>
<td>6.59</td>
<td>3.50</td>
<td>8.21</td>
<td>4.62</td>
</tr>
<tr>
<td>Distance</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Total</td>
<td></td>
<td>9.58</td>
<td>2.39</td>
<td>9.76</td>
<td>9.37</td>
</tr>
<tr>
<td>Over sea</td>
<td></td>
<td>10.5</td>
<td>3.75</td>
<td>10.10</td>
<td>10.90</td>
</tr>
<tr>
<td>Over land</td>
<td></td>
<td>0.979</td>
<td>1.27</td>
<td>0.979</td>
<td>0.353</td>
</tr>
<tr>
<td>Income per capita$^a$</td>
<td></td>
<td>4.01</td>
<td>8.11</td>
<td>3.57</td>
<td>4.56</td>
</tr>
<tr>
<td>Number of countries</td>
<td></td>
<td>64</td>
<td>64</td>
<td>35</td>
<td>29</td>
</tr>
<tr>
<td>Restricted sample$^b$</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Transport cost, $T$</td>
<td></td>
<td>5.98</td>
<td>3.49</td>
<td>7.95</td>
<td>4.38</td>
</tr>
<tr>
<td>Distance</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Total</td>
<td></td>
<td>9.75</td>
<td>2.60</td>
<td>10.20</td>
<td>9.37</td>
</tr>
<tr>
<td>Over sea</td>
<td></td>
<td>11.20</td>
<td>3.92</td>
<td>11.60</td>
<td>11.00</td>
</tr>
<tr>
<td>Over land</td>
<td></td>
<td>0.63</td>
<td>0.57</td>
<td>1.00</td>
<td>0.34</td>
</tr>
<tr>
<td>Income per capita$^a$</td>
<td></td>
<td>4.21</td>
<td>8.24</td>
<td>3.54</td>
<td>4.76</td>
</tr>
<tr>
<td>Number of countries</td>
<td></td>
<td>47</td>
<td>47</td>
<td>21</td>
<td>26</td>
</tr>
</tbody>
</table>

$^a$Average for 1990–95.

$^b$Countries for which infrastructure data are available. Note that the infrastructure data are available only until 1995. Here we use the average for 1990–95.

Source: Authors' calculations.

### Table A-5. Summary Statistics for the IMF Data Sample, 1990

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean</th>
<th>Standard deviation</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\ln M$</td>
<td>2.89</td>
<td>2.80</td>
</tr>
<tr>
<td>$\ln t$</td>
<td>0.49</td>
<td>0.62</td>
</tr>
<tr>
<td>$\ln inf$</td>
<td>0.23</td>
<td>0.47</td>
</tr>
<tr>
<td>$\ln inf tran$</td>
<td>0.11</td>
<td>0.29</td>
</tr>
<tr>
<td>$\ln distance$</td>
<td>8.49</td>
<td>1.54</td>
</tr>
<tr>
<td>$\ln border$</td>
<td>0.03</td>
<td>0.16</td>
</tr>
<tr>
<td>isldummy</td>
<td>0.16</td>
<td>0.36</td>
</tr>
<tr>
<td>llddummy</td>
<td>0.15</td>
<td>0.36</td>
</tr>
<tr>
<td>Africa$^b$</td>
<td>0.26</td>
<td>0.44</td>
</tr>
<tr>
<td>AA$^b$</td>
<td>0.07</td>
<td>0.26</td>
</tr>
<tr>
<td>$\ln distance$</td>
<td>7.68</td>
<td>0.91</td>
</tr>
<tr>
<td>$\ln Y$</td>
<td>24.29</td>
<td>2.26</td>
</tr>
<tr>
<td>$\ln Y/cap$</td>
<td>7.80</td>
<td>1.66</td>
</tr>
</tbody>
</table>

Note: See table A-1 for variable descriptions and sources. See table A-2 for the country sample.

$^a$These values correspond to the uncensored values of the variables. The sample size is 3,577 for those statistics. The median for $t$ is 1.28.

$^b$The statistics correspond to African partners only. Similar statistics hold for the partner country variables.

Source: Authors' calculations.
### Table A-6. Quartile Values for the IMF Data Sample, 1990

<table>
<thead>
<tr>
<th>Variable</th>
<th>Percentile</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>25th</td>
<td>50th</td>
<td>75th</td>
</tr>
<tr>
<td>CIF/FOB (all sample)</td>
<td>1.11</td>
<td>1.28</td>
<td>1.83</td>
</tr>
<tr>
<td>CIF/FOB (coastal)</td>
<td>1.10</td>
<td>1.29</td>
<td>1.82</td>
</tr>
<tr>
<td>CIF/FOB (landlocked)</td>
<td>1.10</td>
<td>1.23</td>
<td>1.91</td>
</tr>
<tr>
<td>inf (landlocked)</td>
<td>1.48</td>
<td>1.82</td>
<td>2.61</td>
</tr>
<tr>
<td>infran</td>
<td>1.18</td>
<td>1.37</td>
<td>1.59</td>
</tr>
<tr>
<td>inf</td>
<td>0.95</td>
<td>1.41</td>
<td>1.81</td>
</tr>
<tr>
<td>distance</td>
<td>4,536</td>
<td>7,555</td>
<td>10,729</td>
</tr>
<tr>
<td>distance (landlocked)</td>
<td>4,078</td>
<td>6,742</td>
<td>9,922</td>
</tr>
</tbody>
</table>

*Note:* See table A-1 for variable descriptions and sources. All variables correspond to all country samples except for first and last row, which refer to landlocked importers only.

*Source:* Authors' calculations.

### References


In the past two decades, in a series of banking crises around the world, banks have become systematically insolvent. These crises have occurred in developed and developing economies alike. To make such financial system breakdowns less likely and to limit their costs if they occur, policymakers feel the need for financial safety nets. These include such policies as implicit or explicit deposit insurance, a lender of last resort function of the central bank, bank insolvency resolution procedures, and bank regulation and supervision. Of these policies, explicit deposit insurance has been gaining popularity in recent years. Since the 1980s the number of countries with explicit deposit insurance schemes almost tripled, with most OECD countries and an increasing number of developing economies adopting some form of explicit depositor protection. In 1994 deposit insurance became the standard for the newly created single banking market of the European Union. Establishing an explicit deposit insurance scheme became part of the generally accepted best practice advice given to developing economies.

I. THE ORIGIN OF THE DATABASE

Given the complexities involved in safety-net design and operation, policymakers often request technical assistance from the World Bank, particularly on the design of deposit insurance. Until recently, bank staff were unable to give sound policy advice because of the absence of a cross-country data set on deposit insurance characteristics and a lack of empirical evidence on how different deposit insurance designs affect banking outcomes. A recent World Bank research project has started to fill this gap by collecting a cross-country data set and using it to develop much-needed empirical evidence (Demirgüç-Kunt and Kane 1998).

This article presents this data set on deposit insurance system arrangements currently in place around the world. A large section of the data set is constructed using the survey results of an International Monetary Fund study by Garcia (1999) and earlier sources such as Kyei (1995) and Talley and Mas (1990). Additionally, information from other country sources is also compiled to double-check the data sets. Most of the data are coded through dummy variables to represent the presence or absence of the deposit insurance features. A few other features that are not suitable for binary coding are categorized using a range of
numeric values. The main motive for presenting the data in this format is to enable researchers to process the data sets for quantitative analysis using computer applications. The database (a Microsoft® Excel spreadsheet), further details on its construction, including data sources and individual country notes, are available on the Web site www.worldbank.org/research/interests/confs/upcoming/deposit_insurance/home.htm.

II. Features of the Deposit Insurance System Database

Table 1 provides information on deposit insurance design features for the 71 countries with explicit schemes. The following section describes deposit insurance features and information on the presentation methodology of the database.

Explicit or Implicit Deposit Insurance

Type. The first variable in the database identifies the form of the deposit insurance, whether explicit or implicit. Deposit insurance is explicit if some form of legislation, such as the central bank law, banking law, or the constitution, establishes a guarantee scheme for deposits. In the absence of such formal arrangements, we assume that the country has an implicit deposit insurance system. Countries with explicit deposit insurance systems are coded 1, and the other countries are coded 0. The database has information on 178 countries, but the table reports only the design features of 71 explicit schemes.

Date enacted or revised. This variable identifies the year in which an explicit deposit insurance system was enacted and the year any revisions were made. Further details are provided in country notes.

Coverage Variables

Deposit insurance systems vary in the extent and amount of coverage that they provide depositors. The schemes specify the types of deposits, types of institutions, and the maximum amount of deposits guaranteed.

Extent of coverage. Systems offering coverage to deposits denominated in foreign currencies are coded 1 and systems excluding such deposits are coded 0. Most countries with explicit insurance systems do not extend coverage to interbank deposits. Deposit insurance systems extending coverage to interbank deposits are coded 1 and systems that do not are coded 0.

Amount of coverage. The amount of coverage provided by deposit insurance schemes varies. The database provides different coverage variables and a variable for the existence of coinsurance arrangements.

Coverage limits. This variable provides the coverage limits of the insurance schemes that were in effect during the first half of 1999 in U.S. dollars or ECU.
COVERAGE RATIOS. This variable shows the ratio of the coverage limits to 1998 GDP per capita.

COINSURANCE. This variable is coded 1 if there is coinsurance, and 0 otherwise.

Funding Variables

FUNDING TYPE. Deposit insurance schemes fall into two categories in the way they are funded by banks. The most conventional type is the funded system, in which the member institutions make periodic contributions to an established, permanent fund. The alternative type, the unfunded system, has no permanently maintained fund, and members are required to contribute to the fund after a bank failure. Funded systems are coded 1, and unfunded systems are coded 0.

ANNUAL PREMIUMS. This variable provides the banks’ annual premiums that are applied to the assessment base (generally deposits or insured deposits). If contributions are made after banking problems, this is indicated.

RISK-ADJUSTED PREMIUMS. Banks contribute to the fund by paying periodic premiums at either variable or fixed rates. Risk-adjusted systems are coded 1; fixed premiums are coded 0.

SOURCE OF FUNDING. In addition to the premiums collected from the banks, most insurance schemes can also resort to public funds when needed. Systems exclusively funded by the banks are coded 0. Systems exclusively funded by public funds are coded 2. Systems with access to both sources are classified as “jointly funded” and coded 1.

Administration and Membership Variables

ADMINISTRATIVE FORM. Systems administered by official authorities are coded 1, those administered by private authorities are coded 3, and those administered jointly are coded 2. Private administrators typically have limited authority. These are mentioned in the country notes.

TYPE OF MEMBERSHIP. Membership in deposit insurance systems may be compulsory or voluntary. As of spring 1999 a majority of the deposit insurance schemes were compulsory. The compulsory systems are coded 1, and the voluntary systems are coded 0.

III. Concluding Remarks

The cross-country database of deposit insurance design features described herein is part of a broader research project to understand the impact of deposit insurance design on bank stability, market discipline, and financial development. This database can be used to investigate a wide range of issues including when devel-
<table>
<thead>
<tr>
<th>Country</th>
<th>Type explicit = 1</th>
<th>Date enacted/ revised</th>
<th>Foreign currencies yes = 1 no = 0</th>
<th>Interbank deposits yes = 1 no = 0</th>
<th>Coverage limits-1 US$ or ECU</th>
<th>Coverage ratios</th>
</tr>
</thead>
<tbody>
<tr>
<td>Argentina</td>
<td>1</td>
<td>1979/1995</td>
<td>1</td>
<td>0</td>
<td>30,000</td>
<td>3</td>
</tr>
<tr>
<td>Austria</td>
<td>1</td>
<td>1979/1996</td>
<td>1</td>
<td>0</td>
<td>$24,075 but coinsurance for businesses</td>
<td>1</td>
</tr>
<tr>
<td>Bahrain</td>
<td>1</td>
<td>1993</td>
<td>1</td>
<td>0</td>
<td>5,640</td>
<td>1</td>
</tr>
<tr>
<td>Bangladesh</td>
<td>1</td>
<td>1984</td>
<td>0</td>
<td>0</td>
<td>2,123</td>
<td>6</td>
</tr>
<tr>
<td>Belgium</td>
<td>1</td>
<td>1974/1995</td>
<td>1</td>
<td>0</td>
<td>15,000 ECU until year 2000</td>
<td>1</td>
</tr>
<tr>
<td>Brazil</td>
<td>1</td>
<td>1995</td>
<td>1</td>
<td>0</td>
<td>17,000</td>
<td>4</td>
</tr>
<tr>
<td>Bulgaria</td>
<td>1</td>
<td>1995</td>
<td>1</td>
<td>0</td>
<td>1,784</td>
<td>1</td>
</tr>
<tr>
<td>Cameroon</td>
<td>1</td>
<td>1999</td>
<td>0</td>
<td>1</td>
<td>5,336</td>
<td>9</td>
</tr>
<tr>
<td>Canada</td>
<td>1</td>
<td>1967</td>
<td>0</td>
<td>1</td>
<td>40,770</td>
<td>2</td>
</tr>
<tr>
<td>Central African Republic</td>
<td>1</td>
<td>1999</td>
<td>0</td>
<td>1</td>
<td>3,557</td>
<td>13</td>
</tr>
<tr>
<td>Chad</td>
<td>1</td>
<td>1999</td>
<td>0</td>
<td>1</td>
<td>3,557</td>
<td>15</td>
</tr>
<tr>
<td>Chile</td>
<td>1</td>
<td>1986</td>
<td>1</td>
<td>0</td>
<td>demand deposits in full and 90% coinsurance to UF 120 of $3,600 for savings deposits in full until 2001, then coinsurance to $5,500</td>
<td>1</td>
</tr>
<tr>
<td>Colombia</td>
<td>1</td>
<td>1985</td>
<td>0</td>
<td>1</td>
<td>15,300</td>
<td>2</td>
</tr>
<tr>
<td>Croatia</td>
<td>1</td>
<td>1997</td>
<td>1</td>
<td>0</td>
<td>coinsurance to $11,756</td>
<td>3</td>
</tr>
<tr>
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<td>Annual premiums</td>
<td>Risk-adjusted premiums</td>
<td>Source of funding</td>
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<td>0</td>
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<td>1</td>
</tr>
<tr>
<td>Switzerland</td>
<td>1</td>
<td>1984/1993</td>
<td>0</td>
<td>0</td>
<td>19,700</td>
<td>1</td>
</tr>
<tr>
<td>Taiwan, China</td>
<td>1</td>
<td>1985</td>
<td>0</td>
<td>0</td>
<td>38,500</td>
<td>3</td>
</tr>
<tr>
<td>Tanzania</td>
<td>1</td>
<td>1994</td>
<td>0</td>
<td>0</td>
<td>376</td>
<td>2</td>
</tr>
<tr>
<td>Thailand</td>
<td>1</td>
<td>1997</td>
<td></td>
<td></td>
<td>Blanket guarantee</td>
<td></td>
</tr>
<tr>
<td>Trinidad and Tobago</td>
<td>1</td>
<td>1986</td>
<td>1</td>
<td>1</td>
<td>7,957</td>
<td>2</td>
</tr>
<tr>
<td>Turkey</td>
<td>1</td>
<td>1983</td>
<td>1</td>
<td>0</td>
<td>in full</td>
<td></td>
</tr>
<tr>
<td>Uganda</td>
<td>1</td>
<td>1994</td>
<td>0</td>
<td>0</td>
<td>2,310</td>
<td>8</td>
</tr>
<tr>
<td>Ukraine</td>
<td>1</td>
<td>1998</td>
<td>1</td>
<td>0</td>
<td>250</td>
<td>0</td>
</tr>
<tr>
<td>United Kingdom</td>
<td>1</td>
<td>1982/1995</td>
<td>1</td>
<td>0</td>
<td>Larger of 90% co-insurance to $33,333 or 22,222 ECU</td>
<td>1</td>
</tr>
<tr>
<td>United States</td>
<td>1</td>
<td>1934/1991</td>
<td>1</td>
<td>1</td>
<td>100,000</td>
<td>3</td>
</tr>
<tr>
<td>Venezuela, RB de</td>
<td>1</td>
<td>1985</td>
<td>0</td>
<td>0</td>
<td>7,309</td>
<td>2</td>
</tr>
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</table>

*Source: Demirgüç-Kunt and Sobaci 2000.*
<table>
<thead>
<tr>
<th>Co-insurance</th>
<th>Permanent fund</th>
<th>Annual premiums</th>
<th>Risk-adjusted premiums</th>
<th>Source of funding</th>
<th>Administration</th>
<th>Membership</th>
</tr>
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<tbody>
<tr>
<td>yes = 1</td>
<td>funded = 1</td>
<td>0.9375</td>
<td>0</td>
<td>1</td>
<td>1</td>
<td>1</td>
</tr>
<tr>
<td>no = 0</td>
<td>unfunded = 0</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

| 0 | 1 | 0.005 of assets and 0.01 of total deposits | 0 | 1 | 3 | 1 |
| 1 | 1 | 0.02 | 0 | 1 | 1 | 1 |
| 0 | 1 | risk-based from 0.65 to 1.45 | 1 | 1 | 2 | 1 |
| 0 | 1 | 0.2 | 0 | 1 | 1 | 1 |
| 1 | 1 | not more than 0.4 | 0 | 1 | 1 | 1 |

| 1 | 1 | risk-based, 0.08 to 0.12 + more in emergencies | 1 | 1 | 1 | 1 |
| 0 | 1 | risk-based: 0.15% of deposits + 0.5% of net non-performing loans | 1 | 1 | 2 | 0 |
| 0 | 1 | risk-based: 0.3 to 0.6 | 1 | 1 | 2 | 1 |
| 0 | 1 | 0.1 to 0.3 for banks | 0 | 1 | 2 | 1 |
| 0 | 1 | maximum of 0.2 | 0 | 1 | 2 | 1 |

| 0 | 1 | 0.15 | 0 | 1 | 1 | 0 |
| 0 | 1 | risk-based, 0.5 now, 0.1 later (future date is not available) | 1 | 1 | 1 | 1 |
| 0 | 0 | on demand | 0 | 0 | 3 | 0 |
| 0 | 1 | 0.015 | 0 | 1 | 1 | 0 |
| 0 | 1 | 0.1 | 0 | 1 | 3 | 1 |
| 0 | 1 | 0.2 | 0 | 1 | 1 | 1 |
| 0 | 1 | risk-based 1.0 to 1.2 | 1 | 1 | 1 | 1 |
| 0 | 1 | 0.2 | 0 | 1 | 1 | 1 |
| 0 | 1 | 0.5 plus special charges | 0 | 1 | 1 | 1 |
| 1 | 0 | on demand | 0 | 0 | 3 | 1 |

| 0 | 1 | risk-based, 0.00 to 0.27 | 1 | 1 | 1 | 1 |
| 0 | 1 | 2 | 0 | 1 | 1 | 1 |
Opening countries should adopt explicit insurance schemes and how these schemes should be designed (see Demirgüç-Kunt and Detragiache 2000; Demirgüç-Kunt and Huizinga 2000; Cull and others 2000; Kane 2000).

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