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The Impact of Trade on Poverty Through Growth Effects

Chapter 4 provided estimates of the impact of multilateral trade liberalization on global poverty, based on one-time efficiency gains. This chapter focuses on the additional poverty alleviation that could be expected from the ongoing dynamic growth effects of higher levels of trade. This second tranche of dynamic gains is conceptually different from the "steady state" gains estimated in chapter 4 under the hypothesis of additional capital investment in response to new trade opportunities. These dynamic gains refer to increases in total factor productivity, rather than increases in output per worker from increased capital per worker.

There is an extensive literature on the interaction between trade and growth.[1] This chapter first surveys this literature to identify key parameters that can serve as a basis for estimating dynamic gains from increased trade. It then uses these parameters in combination with the estimates developed in previous chapters to arrive at a comprehensive calculation of the potential for global trade policy reform to reduce global poverty over time. Before doing so, however, it surveys the recent empirical debate on the link between trade policy and growth. The main controversy is about whether the liberalization of the developing countries' own trade regimes will spur, curb, or leave unaffected their own growth. In contrast, even trade policy skeptics will tend to agree that developing countries would be better off if industrial countries liberalized market access.

1. Recent surveys include Berg and Krueger (2003) and Baldwin (2003). Edwards (1993, 1998) and Sachs and Warner (1995) are prominent protrade empirical studies, while Rodriguez and Rodrik (2000) lead in the skeptical backlash.

Two components of the trade-growth relationship can be separated. The first is the influence of trade on growth. The second is the influence of trade policy on growth, presumably through its influence on trade. The discussion treats each in turn before synthesizing the parameters needed to estimate the growth and poverty effects of trade liberalization.

The Impact of Trade on Growth

The tradition that international trade enhances domestic growth goes back at least to Adam Smith. Trade permits increased specialization. Specialization permits increased attainment of economies of scale, especially for countries with relatively small domestic markets. It also permits fuller utilization of the country's abundant factor of production. Increased imports, for their part, provide increased competitive pressure that helps prompt domestic firms to improve their technologies. Increased economic integration with the outside world also stimulates technical change through the diffusion of new technologies, especially from more advanced countries at the technological frontier to developing countries. Increased imports also curb domestic monopoly power that holds production below and prices above socially optimal levels.

Cross-country regression analyses to determine the sources of growth have proliferated in recent years. A relatively early study that sounded a strong cautionary note was that by Levine and Renelt (1992). They tested for robustness in the by then already extensive literature. They first identify four core variables influencing growth: the share of investment in GDP, the initial secondary school enrollment rate, GDP per capita in 1960, and the rate of population growth. Using data for 119 countries for the period 1960–89, they find the predicted signs for all four variables and significance at the 5 percent level except for population growth. The results confirm conditional convergence (negative coefficient on initial income per capita), and a strong positive influence of capital accumulation and human capital. Their objective, however, is to explore the robustness of additional variables (especially policy variables) when they are added to the core set. The method is essentially to see whether a particular variable remains significant and of the right sign when an alternative set of up to three control variables is added.

For all the resulting sets of regressions including the variable of interest, Levine and Renelt identify the highest and lowest value of the coefficient that cannot be rejected at the 5 percent level (Leamer's "extreme bounds"; Leamer 1983). If the highest and lowest values estimated for the coefficient remain significant and of the same sign, the variable's results are considered robust. By this test, "almost all results are fragile." A broad array of fiscal, monetary, and political variables are found not robustly correlated with growth or the investment share.

Levine and Renelt's results for trade are of special interest to this chapter. They find that while a trade variable is not robust when entered in addition to the four core variables, there is "a positive and robust correlation between the share of investment in GDP and the average share of trade in GDP." They thus conclude that there is a "two-step" positive influence of trade on growth, working through the influence of trade on investment. They note that this is somewhat surprising, considering that usually the trade influence is thought to work more through technical change than through induced capital accumulation. Their results also show a significant influence of trade if the investment rate is left out of the core variable set. Moreover, they find that it does not matter whether trade is measured by the ratio of exports, imports, or their sum, to GDP. This is important, because whereas exports as a positive component of GDP would be expected to generate spurious correlation through a simultaneous-equation accounting relationship, imports as a negative component would be expected to generate a spurious negative relationship from this standpoint.

Although it will be suggested below that the Levine-Renelt extreme bounds tests may be too stringent, it is important to note that the influence of trade does seem to pass even this strong test for their results through the two-step interaction with investment. In particular, they estimate a coefficient of 17.5 on the investment variable, indicating that each percentage point of GDP in additional investment will boost the per capita growth rate by 0.175 percentage point. They also estimate a (robust) coefficient of 0.14 on exports as a fraction of GDP in an equation explaining investment share.[2] This implies that an extra 10 percentage points of GDP in the export ratio will boost per capita annual growth by 0.245 percent, raising the level of per capita income above the baseline by 2.5 percent over a decade and 5 percent over two decades.[3] The average share of exports in GDP in their sample is 0.32 for countries with per capita growth above the mean of 1.92 percent, and 0.23 for countries below mean growth.[4] Thus, the mean export ratio is about 0.28. In turn, a 0.1 rise in the export ratio is equivalent to a 36 percent increase against its mean. This in turn implies an elasticity of the level of output per capita with respect to the export ratio of 0.14 over two decades.[5]

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2. Their text says as a percent of GDP, but their specific example in a footnote makes it clear that a fraction is being used (p. 954).

3. That is, a 10-percent-of-GDP rise in the export variable generates a 0.1 rise in X (e.g., from 0.2 to 0.3, or from 20 to 30 percent of GDP). This rise in turn generates $0.1 \times 0.14 = 0.014$ increase in the investment share. The combined impact on the per capita growth rate is $0.014 \times 17.5 = 0.245$ percent per year.

4. The difference is statistically significant, with $t = 2.3$.

5. That is, there is a 5 percent increase in level over two decades, against a 36 percent increase in the export ratio. The translation of various studies' results into this long-term elasticity is discussed further below.

One of the most convincing tests of the influence of trade on growth in the more recent literature is the instrumental variable approach used by Frankel and Romer (1999) and Frankel and Rose (2000). This approach seeks to deal with the problem of reverse causality (i.e., from growth to trade rather than the other way around) by using a "gravity model" of trade to estimate an instrumental variable for the trade variable (exports plus imports relative to GDP) and then applying two-stage least squares (2SLS) to estimate the relationship between growth per capita and the trade ratio.

Concern about endogeneity in the trade-growth relationship is well placed. As shown in chapter 1 (figure 1.3), there is a clear positive relationship between export growth and GDP growth in international experience. But exports, after all, are part of GDP, so contemporaneous data will inherently have a "simultaneous equations" bias relating the two. More fundamentally, it has long been considered a standard pattern of consumption that imports tend to be "income elastic" goods, so as per capita incomes rise the share of imports (and hence, to pay for them, exports) in GDP would be expected to rise as a consequence rather than cause of growth.

Frankel and Romer (1999) cut through this identification problem by using the by now well-known empirical strength of a gravity trade model. The gravity model states that the exports of one nation to another are greater if the two countries are geographically closer to each other (especially if they share a border), and if they are larger and hence offer larger markets to each other. Their core gravity equation is simply that the logarithm of the ratio of exports plus imports to GDP (which will be called the "trade ratio" in this chapter) is a function of the logarithm of distance between the two (negative coefficient) and of the logarithms of the "size" of the two (positive coefficients). The equation is enhanced with dummy variables for landlocked countries and countries with a common border. Both population and land area variables are included to capture size.

With predicted trade ratios from the gravity model in hand as the "instrument" for the actual trade ratio, Frankel and Romer then regress the logarithm of per capita income on the trade ratio, log population, and log area. The trade ratio is statistically significant, and indeed it is larger with 2SLS than with ordinary least squares (OLS).[6] The authors then decompose the source of the trade influence, using 1960 as a base and estimating the influence of the trade share on the change in capital, education, and total factor productivity from 1960 to 1985. They find that trade influences growth both by increasing physical and human capital deepening and, even more, by boosting total factor productivity growth. Their overall estimate is that a rise in the trade ratio by 1 percentage point of GDP (e.g., from 0.7 to 0.71) raises per capita income by at least 1 percent.

6. If endogeneity were a major problem, the reverse would be true.

Frankel and Rose (2000) apply the same method to examine the influence of currency unions on growth, working through their influence on trade. Using panel data for 180 countries from 1970 through 1995, they refine the underlying estimate of the trade-growth relationship to estimate that "every one percent increase in trade (relative to GDP) raises income per capita by roughly ⅓ of a percent over twenty years."[7] Moreover, in an appendix they show that their result is robust to the inclusion of distance from the equator.[8] A related paper by Irwin and Tervio (2000) extends the Frankel-Romer analysis to eight benchmark years from 1913 to 1990 and generally confirms the results, but it finds that prior to the 1985 period examined by Frankel and Romer the inclusion of a latitude variable removes the significance of the trade ratio. The implication is that although climate may have dominated trade as an influence on economic performance through much of the 20th century, by its final two decades that was no longer the case.

Frankel and his coauthors are careful not to equate trade with trade policy. Indeed, a key feature of the instrumental variable approach is that it suggests an independent benefit from the good fortune of being located in the right spot for high trade. The further implication is that if some other influence, such as trade policy, can act as a substitute to attain comparable high trade, there will be economic benefits from that policy.

A second and even stronger finding that trade enhances growth is set forth in Alcalá and Ciccone (2001). Three features distinguish their study. First, they use Generalized Method of Moments (GMM) estimation rather than OLS or 2SLS.[9] The GMM approach is an alternative to 2SLS as a vehicle for addressing simultaneity. Second, and centrally, they place the purchasing power parity (PPP) valuation of GDP in the denominator of the trade ratio, rather than GDP at the national exchange rate. The reason for this approach is basically the Balassa-Samuelson effect. In this effect, as countries achieve catch-up growth, their productivity tends to grow more rapidly in tradables than in nontradables, so the very process of growth will tend to shrink the observed rise in the trade ratio by ballooning the relative price and therefore national valuation of nontradables in the denominator. There will be a downward bias in the measured influ-

7. Note that this time the percent change in the trade ratio is stated proportionately rather than as a percentage point of GDP.

8. This further test answers queries by Hall and Jones (1999) and Rodriguez and Rodrik (2000).

9. See Wooldridge (2001) for a brief description of the Generalized Method of Moments. In essence, this method takes advantage of known or postulated relationships between the moments (e.g., mean and variance) of a distribution to obtain parameter estimates as a weighted average of alternatively based relationships in an overdetermined set (more equations than unknowns). The method is especially helpful for increasing the efficiency of estimates in nonlinear and time-series models. It also addresses heteroskedasticity (i.e., the size of the disturbance term not independent of the underlying variables), although for correction of this in OLS alternative methods are approximately as effective.

ence of the trade ratio on growth unless this is corrected for by using PPP GDP in the denominator of the trade ratio measure. Third, the authors include institutional quality in their explanatory variables, addressing the growing concern in this literature that institutions determine everything.

The equation estimated in Alcalá and Ciccone is as follows: The logarithm of per-worker PPP GDP is a function of the logarithm of real openness, an economic scale variable (logarithm of the total workforce), the logarithm of land area (for comparability with Frankel-Romer), an index of institutional quality, and geography controls (distance from equator and continent dummies). Once again, the gravity model is used to obtain the instrument for the trade variable. The data are cross-sectional for 1985 and 1990. Their central result is that the elasticity of average labor productivity with respect to real openness is 1.44. For example, a country moving from median real openness (31 percent) to the 60th percentile (39 percent, or a proportionate rise by $8/31 = 0.26$) increases productivity by 37 percent ($= 26 \times 1.44$). On a comparable basis, this is about twice as large as the Frankel-Romer estimate, even in the higher (2SLS) variant in that study.

Alcalá and Ciccone further explore the relationship of openness to trade policy. They use GMM estimation of the logarithm of observed real openness against the gravity model's predicted real openness instrument, "years open" between 1960 and 1985 (based on the measure used by Sachs and Warner 1995) as instrumented by English language incidence and population size, the fraction of GDP in mining and quarrying, and geography controls.[10] They find that these variables have a strong positive effect on real openness. To address Rodriguez and Rodrik's (2000) critique that various components of Sachs and Warner's variable measure something other than trade policy, they repeat the test replacing "years open" by an alternative measure of trade policy, the nontariff and tariff component of the Sachs and Warner variable. They find that doing so raises rather than reduces the influence of the trade policy variable on real openness. Pursuing this line further, they obtain similarly strong results using import duties as a percent of the value of imports to explain real openness. The basic finding is that not only does higher openness drive higher productivity but lower protection also drives higher openness.

A prominent study relating growth to trade is that by Dollar and Kraay (2001b). They use decade-average panel data for the 1980s and 1990s for about 100 countries to estimate growth in real per capita income (from the first decade to the second) as a function of the corresponding growth in the previous decade (1970s to 1980s), the change in a set of control variables from the 1980s to 1990s, and a decadal-shift "period effect" common to all countries. Among the control variables is the key "trade volume"

10. The incidence of the English language is used, somewhat heroically, to capture a favorable attitude toward free market policies, and population size is used to capture the scope for scale benefits, even without open trade.

variable, the ratio of imports plus exports to GDP. They argue that use of change in trade rather than trade level itself circumvents the problem that the level of the trade variable is highly influenced by such factors as geography, because the change over time is not.

Similarly, they argue that the decadal-difference approach avoids the problem of omitted variables, such as the rule of law or colonial history, because these change little over a decade. They maintain that their approach also deals naturally with the problem of reverse causation. In particular, they include the level of "trade volume" in the 1970s (which cannot be caused by economic shocks in the subsequent decades) as an instrument for trade openness, as well as the level of income in the 1970s as an instrument for lagged growth.

In their OLS regression, Dollar and Kraay estimate a relatively high coefficient of change in growth on change in trade. The coefficient indicates that a 100 percent increase in the ratio of trade to GDP has the effect of raising GDP per capita by 25 percent over a decade. When instrumental variables are included for initial income and trade ratios, the coefficient nearly doubles, from 0.25 to 0.48. The much larger coefficient in the 2SLS result is reminiscent of the same contrast found by Frankel and Romer (1999), in that it once again indicates that taking endogeneity into account tends to strengthen rather than reduce the trade-growth relationship. Robustness experiments with a number of additional variables tend to show them as insignificant and to leave the trade influence significant and intact. The authors interpret this pattern as confirming their expectation that the use of decadal differences will largely remove the influence of slow-moving omitted variable influences such as institutional quality.

In short, the Dollar-Kraay study appears to provide additional evidence that trade matters as an influence on growth. It is considerably less clear that it also shows that trade policy matters, because, as discussed below, many factors could have altered the trade ratios between the two decades other than trade policy.

Easterly (2003) provides additional information on the influence of trade on growth. In a dynamic panel regression of growth on inflation, the government budget balance, M2 relative to GDP, the logarithm of the black market premium, the logarithm of overvaluation, government consumption relative to GDP, private credit as a fraction of total credit, and a trade variable—exports plus imports divided by GDP—he estimates a surprisingly large and significant coefficient on trade. The coefficient averages 0.066 over four variants. A unit increase in the trade ratio would thus boost the long-term annual per capita growth rate by 6.6 percent. The mean trade ratio in the sample is 0.7, so a 10 percent rise in the trade ratio would amount to 0.07, which when applied to the coefficient would raise annual per capita growth by 0.46 percentage point, so that over the course of a decade per capita income would rise 4.7 percent, and over 2 decades, 9.6 percent.

This result is much larger than in corresponding OLS regressions (where the coefficient is only 0.01). It is notable that Easterly's GMM result is high, just as the Alcalá-Ciccone GMM result is high. At the same time, it should be noted that despite his seemingly strong results, Easterly is skeptical that trade or any of his other policy indicators affect growth.[11] The results, however, can also be interpreted as capable of speaking for themselves. It should also be noted that whereas he interprets the trade ratio as a policy indicator, for the discussion here it is considered as an economic characteristic but not necessarily a proxy for trade policy.

Choudhri and Hakura (2000) provide a methodologically attractive analysis of the influence of trade on total factor productivity growth. Their approach draws on Krugman's technology gap model, and thereby focuses on a central and concrete dimension of the openness argument: that it promotes technological transfer. Using panel data for 33 developing and 11 industrial countries for the period 1970–93, they conduct tests relating total factor productivity growth to sectoral shares and sectoral changes in trade ratios. They find a relatively strong impact of the import ratio in medium-technology industries (International Standard Industrial Classification categories 33, wood and wood products; 36, nonmetallic mineral products except fuel; and 37, basic metal industries). For these sectors, a rise by 0.1 in the ratio of imports to value added increases total factor productivity growth by 0.6 percent annually. If we place the median ratio of imports to value added at, say, 0.3 for these sectors, the implication is that a 33 percent rise in the openness indicator elicits a 6 percent rise in output in this part of the economy over a decade. For other sectors, however, there is no statistically significant relationship. Moreover, these medium-growth (and medium-technology) sectors account for only about 3 percent of GDP for developing countries.

The World Bank (2002a) has developed a computable general-equilibrium (CGE) model that incorporates the influence of trade on productivity growth. As set forth in chapter 3, when this dynamic effect is included, the World Bank's Global Economic Prospects (WBGEP; World Bank 2002a) model boosts the estimate of the impact of free trade by 2015 from $355 billion static gains annually (in 1997 prices) to $832 billion annually, or from 0.9 percent of global product to 2.1 percent (World Bank 2002a, 168–69). The study cites the following arguments for why these dynamic productivity effects should occur (p. 181): (1) Higher incomes from the static trade liberalization gains lead to higher saving and investment. (2) Tariffs are often imposed on investment goods, so liberalization will tend to boost investment. (3) Incorporating economies of scale and imperfect competition increases welfare gain estimates above the static estimates.

11. Easterly seems to reject the GMM approach summarily because he considers that it requires "the rather dubious assumption that the lagged right-hand side variables do not themselves enter the growth equation" (2003, 41).

(4) Opening trade increases the extent to which domestic firms can take advantage of new technologies and foreign research and development, and move toward international standards (which they call an "endogenous growth" or "productivity" effect). (5) International capital flows can enhance growth through capital deepening and improved technologies associated with foreign investment; by implication, some increase in capital flows is considered likely to arise from increased export opportunities.

To provide a basis for comparison of the welfare gain effect, ideally the WBGEP model's estimate of increased trade from the elimination of barriers would be used. Because this is not reported, the free trade impact estimates of chapter 4 in the present study can serve as a benchmark. The main free trade scenario generates import increases that, when weighted by base levels of imports, yield an aggregate increase in import volume of 1.53 percent globally. This implies that a 1.5 percent rise in the ratio of trade to GDP leads in the WBGEP model to a long-term productivity increase of 1.2 percent (the difference between the static gains as a percentage of GDP and the dynamic gains).

Finally, in a recent study on the sources of growth in 21 of its member countries, the Organization for Economic Cooperation and Development (OECD 2003f, 88) has estimated that a rise in the trade exposure ratio by 10 percent of GDP increases long-term per capita GDP by 4 percent. Trade exposure is close to, but not the same as, the sum of exports plus imports relative to GDP.[12] In an appealing refinement, the OECD researchers use an adjusted trade measure—the deviation of the trade exposure ratio from a cross-country regression line relating exposure to country population size—rather than the raw trade exposure measure itself, as the gauge of the influence of trade. This simple but powerful approach makes the trade variable in their regressions much closer to a policy variable than a state-of-nature parameter. In other words, if a country has considerably more trade than would be predicted by its size, the chances are that it has pursued policies that tend toward integration externally rather than toward closure or neutrality.

In regressions obtaining "pooled mean group estimators" for the 21 countries during the period 1971–98, the OECD authors obtain highly significant coefficients of per capita growth on prior year growth (negative coefficient indicating convergence), the investment rate, human capital, inflation (negative), the standard deviation of inflation (negative), government capital formation, taxes (negative), and adjusted trade exposure (p. 82). The coefficient of the logarithm of per capita income on the logarithm of adjusted trade exposure is persistently about 0.20 across six

12. The actual underlying measure is $[x + (1 - x)(M/\{Y - X + M\})]$, where x is the ratio of exports to GDP, X is exports, M is imports, and Y is GDP. Basically, an import penetration ratio against apparent consumption is used rather than imports relative to GDP for the import part of the trade measure.

model variants and also consistently significant (at the 1 percent level in four of the six variants). Given the log-log specification, this implies that the elasticity of per capita GDP with respect to trade exposure is 0.2. This is consistent with the OECD's summary interpretation that an increment of trade exposure by 10 percent of GDP yields a welfare gain of 4 percent of GDP if the average trade exposure ratio is 50 percent.[13]

Because the OECD study includes Hungary, Mexico, Poland, South Korea, and the Slovak Republic, as well as the main industrial countries, its results are of relevance to effects in developing countries more generally. At the same time, because at least one key component of the stimulus to growth from trade—the transfer of technological know-how—should be smaller for countries already at the technology frontier, one should expect that the OECD growth elasticity with respect to trade should be lower than that applicable to most developing countries. For the purposes of this study, then, the OECD estimate likely represents a lower-bound calculation of this elasticity.

It is possible to synthesize these various estimates more generally into a comparable "long-term elasticity of productivity with respect to trade intensity." The discussion of the estimates of Levine and Revelt (1992) above infers this elasticity for 20 years. Frankel and Rose (2000) explicitly give a 20-year horizon for their estimate; Alcalá and Ciccone (2001) and Easterly (2003) use cross-section data and do not specify a time horizon. Choudhri and Hakura (2000) provide an annual productivity growth estimate that can be cumulated for a decade. Dollar and Kraay's (2001b) results are for the difference between two decades and hence imply a decade as the time horizon. The WBGEP estimate is for the effects of free trade over the course of a decade. The OECD authors interpret their model to refer to "long-run" or "steady state" output per capita (p. 87). A horizon on the order of 20 years thus seems implied by their results.

Table 5.1 compiles the direct or implied long-term elasticity of productivity with respect to the trade/GDP ratio in these various studies. This is simply the ratio of the percent rise in productivity over the long term (typically one to two decades) divided by the percent rise in the ratio of trade to GDP, for each of the studies in question.

There is a striking feature of table 5.1: None of these estimates is negative.[14] Despite all the debate about whether openness contributes to

13. That is, a 10-percent-of-GDP rise would amount to a 20 percent rise in the trade exposure measure, which when applied to an elasticity of 0.2 would generate a 4 percent rise in per capita GDP.

14. Even the recent study by Yanikkaya (2003), which as discussed below purports to find that "trade barriers are positively . . . associated with growth" (p. 57), finds that there is a "strong and positive relationship between trade intensity ratios and growth" (i.e., the ratio of trade to GDP shown here in table 5.1), and thus that "it is probably safe to conclude that trade barriers may have negative repercussions on growth through reducing the size of the external sector of a country" (p. 69).

Table 5.1 Various studies' estimates of the long-term elasticity of output per capita with respect to trade relative to GDP (10–20 year horizon)

Study	Estimate
Levine and Revelt (1992)	0.14
Frankel and Rose (2000)	0.33
Alcalá and Ciccone (2001)	1.44
Dollar and Kraay (2001b)	0.25–0.48
Easterly (2003)	0.14–0.96
Choudhri and Hakura (2000)[a]	0.18
World Bank Global Economic Prospects (World Bank 2002)	0.80
OECD (2003f)	0.20

a. Medium-technology manufacturing.

growth, if the issue were truly one warranting nothing but agnosticism, we should expect at least some of these estimates to be negative (and statistically significantly negative). The uniformly positive estimates suggest that the relevant terms of the debate by now should be about the size of the positive influence of openness on growth, and probably also about how trade policy is related to observed openness, rather than about whether increased levels of trade relative to GDP have a positive effect on productivity and growth. The preponderance of theoretical considerations on this side of the argument reinforces this judgment, because theory should presumably weigh substantially in interpreting the econometric results (as indeed "Bayesian" econometrics formally recognizes).

The simple average of the central values of these estimates is a long-term elasticity of output on trade of 0.5. This represents a powerful influence of trade on growth and a major potential source of increased growth, especially for those economies with currently low trade ratios. It is unclear, however, how much of this relationship represents windfall gain from technical change (for example), as opposed to extra output purchased at the price of increased capital investment.

Several of the studies do seek to distinguish between these two sources, however, and they tend to find the greater part of the effect resides in the pure productivity (e.g., technological) shift effect. The Frankel and Rose (2000) study is one of the more explicit on this issue. Its "uncontrolled" productivity elasticity is 0.79 (OLS) to 1.22 (2SLS), but its central estimate of the elasticity (0.33) is from equations that control for the investment ratio (as well as original income and human capital variables). Alcalá and Ciccone (2001) conduct further analysis suggesting that the trade-growth relationship is driven by a pure labor productivity effect, whereas physical and human capital accumulation are driven by institutional quality.

Overall, a fair summary would seem to be that in the econometric estimates relating productivity change to the trade ratio, the bulk of the effect is from an efficiency shift rather than induced capital and/or human capital deepening. On this basis, an appropriate central estimate for the pure productivity gain elasticity might be on the order of 0.4 (i.e., the bulk of the 0.5 simple average).

Finally, an important recent study by Wacziarg and Welch (2003) suggests that the impact of trade on growth could be even higher than the range in table 5.1. This study focuses on the impact of trade liberalization on trade, growth, and investment. The authors find that, after controlling for time trend, for 133 countries over the full period 1950–98 annual growth in per capita income was 1.4 percentage point higher in postliberalization than in preliberalization periods. On average, liberalization raised the ratio of imports plus exports to GDP by 5 percentage points. Their formulation does not clarify how long the additional growth rate persists. Under an extreme endogenous growth model, it might persist indefinitely; under a more traditional neoclassical growth model, the boost to growth would only be transitional, as the economy moved from the original output baseline to a higher one thanks to increased efficiency. If we assume that the Wacziarg and Welch increment to growth persists for only 15 years, this amounts to an eventual rise in output by 23 percent from the baseline. For 118 countries with available data, the average ratio of exports plus imports to GDP in 1990 was 56 percent, and the median ratio was 48 percent (World Bank 2002d). So the 5-percentage-point increase identified by Wacziarg and Welch amounts to about a 10 percent rise in the trade/GDP ratio. This implies an output elasticity with respect to the trade ratio of 2.3 (23 percent divided by 10 percent), which is higher than any of the entries in table 5.1. Although the Wacziarg and Welch study implies a higher dynamic growth effect of openness than will be used here, it finds that about the same proportion of this effect is attributable to induced investment as is assumed here (one-fifth).

The Impact of Trade Policy on Growth

Although empirical studies often seem to leap from the observed trade ratio to an inference about trade policy, the two are separate. A larger economy will tend to have a lower ratio of trade to GDP even under free trade than a smaller economy, because of the greater potential for economies of scale in the domestic market. An economy with an impoverished natural resource base will tend to have a higher trade ratio than an economy with abundant agricultural land, minerals, and energy reserves, because it will need to specialize more in exporting manufactures to obtain food and fuel through imports.

Similarly, movements in the ratio of trade to GDP will not necessarily be driven by changes in trade policy. Commodity-dependent countries may exhibit a prolonged decline in the ratio of their trade to GDP even though they have no tariff or nontariff barrier (NTB) protection (or have not increased this protection), because of adverse trends in global commodity prices.

The challenge of moving from observed trade to inferred trade policy is illustrated by Dollar and Kraay's (2001b) analysis discussed above. The authors interpret an increase in the ratio of trade to GDP as an indication of an opening in trade policy. They divide their sample into three groups, by change in trade ratio. Their top one-third of developing countries by this measure increased their ratio of imports plus exports to GDP from 16 to 33 percent from 1980 to 2000. In contrast, the remaining two-thirds of developing countries experienced a *decline* in the trade/GDP ratio. Their "globalizers" had an increase in growth rates from 2.9 percent in the 1970s to 3.5 percent in the 1980s and 5 percent in the 1990s; the rest experienced a decline, from 3.3 percent in the 1970s to 0.8 percent in the 1980s and 1.4 percent in the 1990s.

After taking note of the Rodriguez-Rodrik critique of attempts to relate trade policy variables to growth, and after noting the difficulty of obtaining "clean measures" of trade policy, Dollar and Kraay settle upon "*changes* in the volume of trade as an imperfect proxy for *changes* in trade policy" (p. 3). Their statistical results linking growth to the trade ratio have been discussed above. However, their attribution of changes in trade ratio to changes in trade policy warrants further scrutiny.

Birdsall and Hamoudi (2002) cast considerable doubt on Dollar and Kraay's interpretation of the trade ratio as a trade policy variable. They argue that the change in this measure is biased by commodity price trends. In the early 1980s when commodity prices were high, developing countries borrowed and expanded imports. When prices collapsed, they were forced to cut back imports, shrinking the numerator of the trade ratio. Birdsall and Hamoudi conclude that using the trade/GDP ratio systematically classifies commodity-dependent exporting countries as "less open" in trade policies. Arguing that these are the same countries that have had low growth in the past two decades, they judge that Dollar and Kraay overstate the importance of trade policy in growth. When they add a commodity dependence dummy variable to the Dollar-Kraay growth equations, they reduce the size of the growth effect of openness by at least half.

Perhaps the most spirited econometric debate in this area, however, is that between the critics Rodriguez and Rodrik (2000) and the authors of two leading statistical analyses showing how open trade policy contributes to growth: Sachs and Warner (1995) and Edwards (1998). Consider first Sachs and Warner's results. They examine growth performance as a function of trade policy and other variables (investment rate, government spending as a percent of GDP, education, and number of revolutions and coups) for 79 countries during the period 1970–89. Their variable for trade policy is a binary dummy variable that is set to "closed" (zero) if any one of five conditions is met. First, the average tariff on capital and intermediate goods is more than 40 percent. Second, NTBs cover more than 40 percent of imports of capital goods and intermediates. Third, the country has a socialist economic system. Fourth, there is a state monopoly on the principal exports. Fifth, the black market premium on the official exchange rate exceeded 20 percent in the 1970s or 1980s.

The authors first conduct regressions relating growth in real per capita income during the period 1970–89 to initial per capita income, a binary variable for whether the economy was "open" during the full period, the investment rate, the relative price of investment goods, government consumption spending relative to GDP, and a series of variables for political stability. These regressions find a statistically significant coefficient on openness, which indicates that over the two decades examined real per capita income grew at 2.2 percentage points faster in "open" economies than in "closed" economies. A second set of regressions examines whether growth accelerated after a shift to "open" status for 38 noncommunist countries that opened after 1975 and sustained their open status through 1993. These results also show a statistically significant impact of opening, indicating an increase in growth from the country's past rate by about 1.2 percentage points.

In an important critical review, Rodriguez and Rodrik examine the Sachs and Warner study and three others. They judge that the studies have "serious shortcomings," leading them to conclude that there is "little evidence that open trade policies—in the sense of lower tariff and nontariff barriers to trade—are significantly associated with economic growth" (Rodriguez and Rodrik 2000, i).[15] Rodriguez and Rodrik rerun

15. The four studies are Sachs and Warner (1995), Edwards (1998), Dollar (1992), and Ben-David (1993). The early Dollar study sought to use purchasing power parity data to gauge protection by departures of domestic tradable prices from international levels, but this has a number of problems, including reliance on the empirically weak "law of one price." The Ben-David study focuses on whether trade liberalization generated convergence among European industrialized countries, and it is of less direct relevance to the purposes of this chapter than the other studies considered here. Slaughter (2001) also examines the impact of trade liberalization on income convergence, but three of his tests similarly involve European integration events. His fourth test, the Kennedy Round, involves using "all countries in the world not part of the Kennedy Round" as the control group. This may be necessary for the control group technique, but it means that a relatively unrepresentative collection of countries will serve as the control, given the widespread coverage of countries that did participate. Moreover, the convergence issue is distinct from the more direct question of whether liberalization affects growth.

the Sachs and Warner regressions separating the Sachs and Warner trade policy index into its five components (black market premium, BMP; state monopoly, MON; socialist, SOC; nontariff barriers, NTB; and tariffs, TAR). Only two of them, BMP and MON, turn out to be significant—although all three of the others retain the right sign. They next reaggregate into two components: BM comprising BMP and MON, and SQT as an index comprising the other three. If SQT is run separately, it has the right sign (for the Rodriguez and Rodrik reformulation) and is highly significant. The same is true of BM. If both are entered jointly, however, BM remains significant but SQT does not. Rodriguez and Rodrik conclude that SQT and thus the most unambiguous trade policy measures—tariffs and quotas—do not stand up to scrutiny as influencing growth.

Rodriguez and Rodrik then dismiss the state monopoly variable as failing to include such countries as Mauritius and Indonesia and instead acting as an Africa dummy. They then ask whether the BMP is in fact a measure of protection. They note that if all imports are purchased with foreign currency bought on the black market while all exporters must turn in earnings at the official rate, the BMP "works exactly like a trade restriction (by raising [the price of imports relative to the price of exports])." If, instead, exporters can sell on the black market, the BMP does not act like a trade restriction. Rodriguez and Rodrik do not state which is more likely, but surely mandatory exchange surrender by exporters is the rule rather than the exception in most regimes with a large BMP.

Nonetheless, Rodriguez and Rodrik judge that the BMP more fundamentally reflects prolonged macroeconomic and sociopolitical imbalances, and it is more likely that these factors are driving the low growth rate rather than the BMP's trade distortions. When Rodriguez and Rodrik insert variables for macroeconomic and political distress, they stress that the BMP loses significance. Well, yes and no. In two of six models, the BMP retains its original size (about –1) and significance. In three of the others, its sign remains the same, its size declines somewhat (to about –0.6 to –0.7), and its *t*-statistic drops below the conventional cutoff but is still not de minimis at about 1.6 (i.e., close to the 10 percent significance level). In only one of the six augmented models do the coefficient and its *t*-statistic collapse substantially further. So is the BMP really unimportant? It would seem of interest, in particular, that in the equations including either a terms-of-trade shock variable or a war variable the BMP coefficient remains essentially unchanged and significant.

Warner (2003) has provided a trenchant defense of the Sachs and Warner results. He argues persuasively that it is necessary to aggregate various protectionist variables rather than test them one by one, because different countries may be applying different means of protection and it is not the marginal effect of a single instrument but their joint effect that is of interest. Or, as Berg and Krueger (2003) put it in their support of Sachs and Warner, "the motivation for such a multivariate indicator in the first place" is to capture "the frequent substitution of one for the other method of protection" (p. 10).

Warner then supports the black market premium as a gauge of protection. He cites the case of Algeria, where the premium was in the range of 250 to 400 percent in the late 1980s, a period when Algeria had tight import restrictions despite an average tariff of only 13 percent. Inflation was a moderate 8.6 percent in the 1980s, so the black market premium hardly appears to be a symptom of macroeconomic disturbance in this example. He then shows a scatter diagram for 1,400 country years in the period 1971–92 showing no relation between the BMP and inflation. He argues that it is much more common historically for a high BMP to result from exchange controls, and further argues that these have tended to stem from rationing foreign exchange for imports rather than from dual exchange rates penalizing capital transactions.

Warner then notes that the marketing board monopoly variable is not a simple dummy for Africa, because only 28 of the 41 African countries in Sachs and Warner's sample trigger the protection dummy on this basis. To Rodriguez and Rodrik's point that Mauritius had a marketing board, Warner replies that it did not have a monopoly on selling foreign exchange for imports. He cites Mozambique as an example of a country with disastrous growth (–3 percent annually) having high protection through the marketing board according to country-study accounts, which will bias results if it is reported as open because of its moderate tariff (10 percent).

Warner's tour de force, however, is in his statistical results obtained even after bending over backward by removing several of the protection cases that tend to violate Rodriguez and Rodrik conditions. He drops 13 countries that were only classified closed because of the black market premium. He then further drops all African countries that were designated closed solely on the basis of their marketing board monopolies. The results still confirm the original Sachs and Warner statistical significance of a negative impact of a closed trade regime on growth.[16]

Reviewing the debate, Berg and Krueger (2003) judge that the Sachs and Warner index "represents a fairly successful effort to measure the overall importance of trade policy restrictions, though it does not differentiate degrees of restrictiveness" (p. 9). They consider Rodriguez and Rod-

16. Warner (2003) further queries the robustness of Rodriguez and Rodrik's estimates. Rodriguez and Rodrik state that there is no simple inverse relation between growth and protection. But they conduct a test using tariff revenue divided by imports, rather than the widely available Barro and Lee (1994) data on average tariff rates. Warner shows that the latter data do show a negative relationship of growth to protection, and this relationship is highly significant if a single outlier—India—is removed. He adds that by using tariff revenue data for the late 1990s, Rodriguez and Rodrik are missing the key period for testing trade regimes: the 1970s and early 1980s, before the wave of liberalization in developing countries removed much of the variation in protection.

rik's critique of the black market premium as overdone, because a premium acts like a tariff by driving a wedge between the exchange rate exporters receive and the rate paid by importers. They are comfortable, moreover, in attributing macroeconomic policy disarray to protection because such disequilibria are precisely the type of consequences seen associated with protection in the past. This is consistent with their more general position that although the influence of trade policy is admittedly difficult to disentangle from that of overall policy quality, it is not necessary to do so for the purposes of arriving at advice favoring open trade policies.

Other researchers have obtained positive growth effects using the Sachs and Warner index. Using data for 84 countries from 1960 to 2000, Bosworth and Collins (2003) estimate regressions of per capita growth on initial income per capita (finding a negative coefficient and thus conditional convergence), human capital as proxied by life expectancy in the initial year, change in terms of trade, institutional quality, geography (number of frost days and tropical area), change in inflation, budget balance, and Sachs and Warner openness. Their coefficient on the Sachs and Warner variable indicates a statistically significant increase of 0.82 percentage point in per capita growth for open versus closed economies.[17] Additional tests indicate that this works through the influence of openness on capital deepening, where the Sachs and Warner variable is significant, rather than a shift in total factor productivity, for which it is not.

Similarly, Greenaway, Morgan, and Wright (1998) apply the Sachs and Warner index at 0 prior to the year of opening and 1 thereafter. In GMM regressions after first differencing, they relate per capita GDP growth in 69 countries during the period 1975–93 to per capita income in 1965, secondary school enrollment in 1965, change in terms of trade, population growth, the ratio of investment to GDP, and the Sachs and Warner trade policy variable. They find a significant coefficient on the last indicating that a shift to openness boosts the rate of per capita GDP growth by a sustained 2.7 percent annually. The authors conduct similar panel regressions using two alternative binary measures of when liberalization occurred (both based on work conducted at the World Bank). They obtain significant liberalization effects with both. However, the cumulative impact on per capita income level is modest (about 2 percent) and far smaller than that from the Sachs and Warner variable (46 percent). They infer that the Sachs and Warner measure is a broader gauge of openness, whereas their other two variables are narrower measures of liberalization and tend to

17. In their text, the authors seem to prefer an interpretation to the effect that the Sachs and Warner variable loses significance when the number of "other conditioning variables" is increased. However, the main other conditioning variables are already present in one key set of results (their table 10), where Sachs and Warner is significant. The addition of regional dummies removes significance (their table 8), but in general, the information from an economic variable should be preferred to that from a regional dummy.

capture "first steps rather than final steps," thereby likely understating the eventual effect.

Harrison and Hanson (1999) carried out the same exercise as Rodriguez and Rodrik (2000) in reestimating growth equations with the individual components of Sachs and Warner rather than its aggregate dummy-variable form. When they include the full set of control variables used by Sachs and Warner and level of the black market premium, they do find tariffs, the marketing board dummy, and the socialist dummy significant individually. However, when they use a dummy variable for the black market premium, the tariff variable loses significance. They conclude that the Sachs and Warner variable "fails to establish a robust link" between more open trade policy and growth. They then consider that the problem may stem from Sachs and Warner's use of end-period tariff and quota data rather than average period rates. They create an alternative measure using tariff revenues relative to imports. Using this variable to measure tariffs, they find that it does have a statistically significant negative effect on growth. A 10-percentage-point rise in tariffs reduces average growth in GDP per capita by 0.5 to 0.6 percent. However, when they add dummy variables for the four East Asian tigers, Latin America, and India, the significance of this openness variable disappears. They infer that there is "limited validity of pure cross-section estimation" (p. 134). However, it would seem questionable to prefer the indirect measure of regional dummies to the direct measure of the openness variable. An alternative interpretation would be to accept the Harrison-Hanson result using duty collections as providing significant evidence of the adverse impact of protection on growth.

Overall, despite the Rodriguez and Rodrik critique, the Sachs and Warner results seem to represent a relatively durable benchmark study showing that protection does adversely affect growth. Consider next Edwards (1998), who makes another important attempt to examine the strength of the empirical relationship between trade policy and growth. He first uses estimates of capital and labor stocks for 93 developing and industrial countries during the period 1960–90 to estimate growth in total factor productivity (TFP), which is a residual in a regression of real GDP growth on growth in capital and labor (with factor-share coefficients constrained to sum to unity). The median rate of TFP growth was 1.1 percent, or 1.0 percent in a version adding a human capital variable.

Edwards then tests the relationship between TFP growth and trade policy openness, using nine alternative measures of the latter. These include the Sachs and Warner variable (discussed above), the "outward orientation index" of the World Bank, Leamer's (1988) index of openness (the country's average residual from disaggregated trade flow regressions), the average black market premium, the average tariff in 1982, the average coverage of NTBs (both from Barro and Lee 1994), the Heritage Foundation Index of Distortions in International Trade, the Collected Trade Taxes ratio for the period 1980–85, and a regression-based index of import distortions in 1985 prepared by Wolf (1993). For each measure, the benchmark value for each country is typically for the early 1980s.

Edwards then conducts cross-country regressions of TFP growth on each of the alternative trade policy indicators, in samples ranging from 32 observations for the World Bank index to 75 for the black market premium. He uses weighted least squares, weighting by GDP per capita. A second round of regressions adds the initial per capita income and initial level of human capital as control variables. In 17 of the 18 resulting regressions, the trade policy openness variable has the right sign, and in 13 of these 17 it is statistically significant. He considers the results as "quite remarkable" evidence of a "tremendous consistency that there is a significantly positive relationship between openness and productivity growth" (p. 391). Many readers are likely to agree with him, although one might worry about his having only a single point estimate of policy openness for each country for the full time span.

Complementing their critique of Sachs and Warner, Rodriguez and Rodrik also criticize Edwards (1998). Their principal critique is that his use of weighted least squares gives undue weight to countries with a higher per capita income. They acknowledge that heteroskedasticity is likely to be present, but they prefer an alternative (White's) form of correction. When they apply this alternative, only four of the nine least-squares models show a significant trade policy variable. They then go on to find fault with individual components of Edwards's variables (property rights should be included) and data (India's collected import taxes are too high to be believable).

How can we be sure, however, that there is not still another weighting system to address heteroskedasticity that will not increase rather than reduce the significance of Edwards's results? How many combinations and permutations would Rodriguez and Rodrik really need to run to arrive at "confidence interval" robustness (see the discussion of Sala-i-Martin 1997) with these different approaches? In particular, my own sense is that there should be far more effort in the cross-country regression literature toward country weighting, considering that otherwise the large number of small economies may give a false impression of how economies really work for the great majority of the world's population. So there are surely alternative dimensions of weighting that neither Rodriguez and Rodrik nor Edwards have tried. Should we reject Edwards's findings on grounds that Rodriguez and Rodrik find weaker results with an alternative technique? Or for that matter, should we look at Rodriguez and Rodrik's tests as showing the glass half empty or half full, with five of Edwards's eight significant and correct signs still significant and correct in one of Rodriguez and Rodrik's corrections for heteroskedasticity (using the log of per capita GDP weighting rather than per capita GDP weighting) and in four of the other alternative (White's)?

More generally, an underlying problem is that there do not seem to be standardized rules of the game for confirming or rejecting earlier statistical results. In particular, adding additional variables can remove the statistical significance of the trade policy variable, but with additional variables is the comparison between the two sets of results a fair one? As Sala-i-Martin (1997, 179) points out, "one is bound to find one regression for which the estimated coefficient changes signs if enough regressions are run." He cites the earlier results of Levine and Renelt (1992), indicating that few or none of the large stable of explanatory variables in international growth equations stand up to Leamer's (1983) extreme-bounds test of robustness. Sala-i-Martin points out that by this test, if a single regression can be found in which the sign of a coefficient changes or even becomes insignificant, the variable is not robust. He argues persuasively that this is simply too extreme a test of robustness.

Sala-i-Martin offers an alternative approach that identifies a weighted mean and variance of each parameter estimate from the full set of alternative models, weighting by likelihood. He proposes that a variable be judged significantly correlated with growth in a "robust" way if the cumulative density function of its coefficient around this mean, within 1 standard deviation, is 0.95 or higher. He then suggests that about 60 variables are reasonable candidates for growth correlations, but that choosing one variable at a time and letting all the others compete for entry in a total of seven right-hand-side variables would require 3.2 billion regressions. After he narrows the field by selecting three core variables to appear in all the regressions, he still has 2 million regressions to run (hence his title). When he does so, he finds that by the Leamer extreme-bounds test only 1 variable passes robustness, but by his confidence-interval approach 22 out of 59 variables are robustly "significant."[18]

The main lesson of Sala-i-Martin's exercise should be to make us wary of throwing out the baby with the bathwater of yet another regression that happens to turn insignificant a key variable previously found to be significant. This in turn suggests that there should be a relatively high hurdle for overturning a significant result that confirms a relatively well-established theory, and the simple presentation of another regression in which the variable is insignificant (and especially if the sign remains un-

18. For a recent examination of the Rodriguez and Rodrik-Sachs and Warner controversy that adopts the spirit of Sala-i-Martin's warning, see Jones (2000). He suggests that a "best estimate" approach to robustness is preferable to an approach in which a single reversal destroys the diagnosis. He examines approximately 100 specifications, selects the 13 he considers most appropriate, and averages the relevant coefficients and probability values. He notes that adding a quality of institutions variable often removes significance of the trade policy variable, but he suggests that there are problems with this variable (Knack-Keefer) as well. He concludes that for all but the worst specifications, the best estimate of the effect of a large adverse change in trade policy (e.g., a move from 1 to 0 on Sachs and Warner) is a 40 to 70 percent decline in steady state income, and the decline is 13 to 24 percent in his "worst" specifications.

changed) should probably not be sufficient to surmount this hurdle, especially if the number of variables has been increased.[19]

As an illustration of this problem, consider Rodriguez and Rodrik's critique of Sachs and Warner. First, they run the Sachs and Warner test separately on each of the five subcomponents of the Sachs and Warner trade policy index, and they find that only two are significant. This procedure, however, does not deal with the economic problem that the original index is designed to capture the combined effect of alternative substitute approaches to protection (as discussed above). Then they replace the Sachs and Warner trade policy variable with two intermediate indexes comprising parts of the five subcomponents. Both subaggregates are significant when run individually, but one loses significance when they enter jointly. But this test increases the total number of variables, raising the question of whether robustness is really disproven in the sense of Sala-i-Martin's confidence-interval approach. The general problem is the risk of the unwarranted rejection of a meaningful econometric result by changing the number or composition of variables.

Wacziarg and Welch (2003) provide an important extension of the Sachs and Warner analysis by shifting the focus from cross-section to time-series and panel analysis. They replicate and update the Sachs and Warner measure of openness, and as a first step confirm the Sachs and Warner cross-sectional results for the 1950–90 period. However, they find that for the period 1991–98 liberalization's impact on growth disappears. Nonetheless, this could simply be because the growth effect of liberalization is transitional (standard neoclassical growth model) rather than permanent (endogenous growth model), and by the 1990s many of the liberalizing countries had already maintained liberalized regimes for several years. Moreover, far from concluding that Sachs and Warner overestimated the impact of liberalization (even though Wacziarg and Welch agree with Rodriguez and Rodrik about the dominant subcomponents of the Sachs and Warner protection measure), Wacziarg and Welch go on to estimate large liberalization effects on the basis of the time-series performance of individual country growth before and after liberalization.

Wacziarg and Welch set the date of trade regime opening for each country, and they find that for 133 countries during the period 1950–98, 32 percent of the country years had liberalized trade regimes. As noted above, they estimate that growth for postliberalization country periods was on average 1.4 percent higher than in preliberalization periods. The thrust of the study is to provide strong reinforcement for the view that trade liberalization has a positive impact on growth, even though the summary growth impact is somewhat lower than the original Sachs and Warner es-

19. A caveat to this caveat is the standard concern that researchers may tend to conduct their search until they find the "right" result.

timate of a difference in annual growth of 2.2 percentage points between open and closed economies (as discussed above).[20]

Overall, it would seem that the weight of the empirical evidence is on the side of those who judge that more open trade policies lead to better growth performance.[21] The Rodriguez and Rodrik analysis has served an important role in sharpening the debate (e.g., see Baldwin 2003), but it would not appear to warrant their outright rejection of meaningful evidence on the link. Perhaps more important, no one has provided persuasive statistical evidence showing support of better growth results using protective regimes.[22] The most compelling evidence on the broad alternative of using high protection to foster import-substituting industrialization remains the spectacular failure of the later stages of this strategy in Latin America by the 1970s, which led to a wave of unilateral liberalization by the late 1980s.

The Dynamic Productivity Effects of Trade Liberalization on Global Poverty

It is possible to synthesize the dynamic productivity effects of trade surveyed above with the impact of global trade's liberalization on the trade of developing countries to obtain the implied long-term effects on growth and poverty. The central elasticity of long-term output per capita with respect to the ratio of trade to GDP in table 5.1 turns out to be 0.5. For ex-

20. Note, however, that the Wacziarg and Welch estimate of 1.4 percent is quite close to the comparable concept estimated by Sachs and Warner: 1.2 percent increase in growth from the preliberalization to postliberalization period, estimated for a smaller set of countries and years.

21. A recent study by Yanikkaya (2003) finds to the contrary that there is a positive relationship between per capita growth and protection, for 80 countries in the period 1970–97. It is unclear how much weight to give these results, however. His estimates finding a positive coefficient of growth on tariff collections relative to import values contradict negative relationships estimated for the same variable in the studies by Edwards (1998), Harrison and Hanson (1999), and Jones (2000). Moreover, this measure is subject to the Sachs and Warner critique that it fails to capture nontariff restrictions, and to the problems that it will understate protection where corruption diverts duties payable and where the tariff is so high that it suppresses imports and revenue. Alternative measures of protection suggested by Yanikkaya either seem questionable (bilateral payments arrangement) or have the wrong sign for his unconventional finding that protection spurs growth (incidence of restrictions on current account payments).

22. Noland and Pack (2003) find, moreover, that even outward-oriented (i.e., export-oriented) strategies that rely on a combination of interventions seeking to encourage exports and at least tacitly curb imports, identified with such cases as Japan and South Korea, do not stand up to empirical scrutiny as supporting success of the "industrial policy" strategy, which some would categorize as within the family of protection even if the result is to foster "openness" as measured by the ratio of exports to GDP.

ample, if a country's exports plus imports rise from 40 to 40.4 percent of GDP (an increase of 1 percent in the ratio), the cumulative dynamic productivity effect over the long term (10–20 years) will be an increase in the level of per capita GDP by 0.5 percent.

Chapter 4 has already considered the quasi-dynamic gains from trade by examining the "steady state" results on output if capital investment is allowed to respond to the increased trade opportunities to the point where the return on investment is driven back down to its preliberalization level. This chapter seeks instead a concept that is equivalent to technical change without additional capital. As discussed above, most of the trade-productivity studies tend to find this the dominant source, rather than capital deepening. However, to allow for the likely inclusion of some capital-deepening contribution to the overall productivity elasticity average identified above, it is appropriate for the purposes of the calculations below to apply a somewhat more moderate central estimate. The parameter used is 0.4, which means that one-fifth of the productivity impact in the field of estimates comes from capital deepening and four-fifths comes from TFP increase.

Table 5.2 first repeats the estimates of the number of poor and the "poverty elasticity" applicable to each of the countries in the Poverty Effects version of the Harrison-Rutherford-Tarr, or PEHRT, model, developed in chapter 4. It next reports the percent increase in real aggregate (Armington) imports estimated from that model for the static free trade scenario (corresponding to table 4.1). These estimates are aggregated from the individual product level, weighting by base period shares of the products in total imports.

The average increase in real imports for all developing countries is 3.0 percent, weighting by base-period imports, and 5.4 percent weighting by poverty. The rise in trade is above average in several countries with large poor populations, including Bangladesh, China, India, Indonesia, and Pakistan.

The next step is to apply the elasticity of 0.4 to the percent increase in trade, to obtain the percent increase in long-term per capita output. This percentage is then multiplied by the poverty elasticity, and the product is multiplied by the number of people in poverty to obtain the long-term change in poverty for each country. The result is that an estimated 202 million people would be lifted out of poverty over the long term through the dynamic productivity effects of free trade. This is in addition to the central estimate of a 114 million reduction in poverty from the real income effects (mainly in unskilled wages) arising from the static effects of free trade (table 4.5). Thus, *a total of 316 million people would be lifted out of poverty* over the next decade or two through the static and dynamic productivity gains from free trade, even without taking account of further poverty reduction from induced capital investment, and even using a conservative estimate of agricultural liberalization effects (e.g., the chap-

Table 5.2 Poverty impact of dynamic productivity gains from free trade

Region and economy	Number of poor (millions)	Poverty elasticity	Increase in trade (percent)	Long-term productivity gain (percent)	Change in poverty (millions)
Asia					
Bangladesh	99.3	3.5	25.6	10.24	–35.6
China	673.2	3.5	5.0	2.00	–47.1
India	859.9	3.5	6.2	2.48	–74.6
Indonesia	136.8	3.5	4.6	1.84	–8.8
South Korea	0.9	3.5	3.1	1.24	0.0
Malaysia	5.6	3.2	5.9	2.36	–0.4
Pakistan	114.2	3.5	14.3	5.72	–22.9
Philippines	29.2	3.0	8.4	3.36	–2.9
Thailand	17.0	3.5	3.0	1.20	–0.7
Hong Kong, Taiwan, and Singapore	0.0	2.0	2.1	0.84	0.0
Other	84.8	2.0	4.2	1.68	–2.8
Subtotal	2,020.9				–196.0
Latin America					
Argentina	5.4	3.1	2.2	0.88	–0.1
Brazil	29.2	1.0	–0.3	–0.12	0.0
Central America and Caribbean	26.6	2.0	5.4	2.16	–1.1
Mexico	41.1	2.0	0.7	0.28	–0.2
Other	45.9	2.0	–0.4	–0.16	0.1
Subtotal	148.2				–1.3
Europe					
Central and Eastern Europe	92.2	2.0	1.0	0.40	–0.7
Middle East, North Africa					
Turkey	11.6	3.5	3.7	1.48	–0.6
Other	76.4	2.0	3.5	1.40	–2.1
Subtotal	88.0				–2.7
Sub-Saharan Africa					
Mozambique	13.6	1.4	4.1	1.64	–0.3
South Africa	15.1	1.2	–0.8	–0.32	0.1
Tanzania	19.7	1.0	0.5	0.20	0.0
Uganda	16.6	2.3	0.5	0.20	–0.1
Other	328.4	2.0	0.3	0.12	–0.8
Subtotal	393.2				–1.2
Total	2,742.5				–201.9

Source: Author's calculations.

ter 4 estimates rather than the chapter 3 estimates). This is surprisingly close to the estimate by the World Bank (2002a, 174) that free trade would reduce global poverty by 320 million by 2015 from its baseline level. The World Bank authors use a different CGE model and somewhat different methodology to arrive at the poverty estimates.

The Combined Static, Dynamic, and Induced-Investment Effects

Finally, the estimates in chapters 3 and 4 and in the present chapter can be combined to obtain an overview of the scale and composition of potential poverty reduction resulting from global trade liberalization. Appendix 3C used a stylized-parameter model combined with country detail on the share of the poor located in the rural sector to arrive at an estimate of the impact of free trade in agriculture on global poverty. This estimate is considerably higher than the implied agricultural-sector contribution to chapter 4's PEHRT model estimate of the poverty reduction resulting from the static effects of free trade. We can thus obtain a "central" static effect from the PEHRT results, and a "high" static effect replacing the agricultural liberalization component of these results with chapter 3's stylized-parameter estimates for the impact of agricultural liberalization. To do so, it is necessary to subtract from the PEHRT static poverty-impact estimates (table 4.5) the fraction broadly associated with agricultural liberalization, as inferred from the ratio of agricultural-sector welfare gains to total gains (from table 4.1).[23] This leaves the nonagricultural static poverty effects, which are then added to chapter 3's agricultural effects to obtain the "high" estimate of the static poverty effects in table 5.3.

The other major step in consolidating the various estimates is to decide how to treat the quasi-dynamic gains in the "steady state" results requiring increased capital investment. As noted in chapter 4, these net welfare and poverty reduction effects are large, even after fully deducting capital costs at a relatively high imputed real interest rate (7 percent). However, they would require additional capital amounting to about 40 percent of base-period GDP in developing countries. Nonetheless, because the consolidated estimates of this chapter seek to gauge the long-term effects over a horizon of perhaps 15 years, at least a substantial part of the increased capital stock would seem feasible.

The consolidated long-term estimates include *one-half* of the incremental welfare and poverty effects in chapter 4's steady state (SS) results above its static results.[24] This implies the mobilization of about 20 percent of base-period GDP (and 15 percent of average real GDP after accounting for reasonable growth of 4 percent annually) in additional capital stock over a period of 15 years. The increase to the investment rate would be only 1 percentage point of GDP (e.g., from 20 to 21 percent), and a significant

23. More precisely, the contribution to welfare gains from agricultural liberalization is divided by the total welfare gains minus the interaction effect, to obtain the proportionate contribution of agricultural liberalization to welfare gains. It is assumed that the same proportion applies to poverty effects.

24. For the purposes of table 5.3, this is based on the central (lower) case of the two alternative steady state estimates in table 4.7.

Table 5.3 Combined long-term static and dynamic effects of free trade on poverty (millions, change from baseline)

Region and economy	Static		Dynamic		Total	
	Central	High[a]	Productivity effect	One-half additional net steady-state effect[b]	Central	High
Asia						
Bangladesh	–4.1	–13.3	–35.6	–3.6	–43.3	–52.6
China	–24.7	–87.2	–47.1	0.0	–71.8	–134.3
India	–28.3	–74.8	–74.6	–109.1	–212.1	–258.5
Indonesia	–6.1	–15.1	–8.8	–7.4	–22.3	–31.3
South Korea	0.0	0.1	0.0	0.0	–0.1	0.0
Malaysia	–0.2	–0.2	–0.4	–0.6	–1.1	–1.1
Pakistan	–7.9	–13.0	–22.9	–46.1	–76.9	–82.0
Philippines	–2.3	–2.0	–2.9	–1.4	–6.6	–6.4
Thailand	–4.1	–4.0	–0.7	–9.2	–14.0	–13.9
Hong Kong, Taiwan, and Singapore	0.0	0.0	0.0	0.0	0.0	0.0
Other	–2.1	–8.8	–2.8	–5.9	–10.8	–17.5
Subtotal	–79.7	–218.3	–196.0	–183.3	–459.0	–597.6
Latin America						
Argentina	–0.7	0.1	–0.1	–0.1	–0.9	–0.1
Brazil	–1.0	–0.4	0.0	–0.3	–1.3	–0.7
Central America and Caribbean	–3.2	–2.9	–1.1	–1.6	–5.9	–5.6
Mexico	–0.4	0.4	–0.2	–0.1	–0.7	0.1
Other	–2.7	–2.2	0.1	–3.3	–5.8	–5.4
Subtotal	–7.9	–5.0	–1.3	–5.3	–14.6	–11.7
Europe						
Central and Eastern Europe	–2.4	–1.0	–0.7	–0.2	–3.3	–1.9
Middle East and North Africa						
Turkey	–1.3	0.0	–0.6	–1.1	–3.0	–1.8
Other	–3.8	–4.4	–2.1	–6.4	–12.3	–12.9
Subtotal	–5.1	–4.4	–2.7	–7.5	–15.4	–14.7
Sub-Saharan Africa						
Mozambique	–0.8	–0.4	–0.3	–0.8	–1.9	–1.5
South Africa	–0.6	–1.1	0.1	–0.1	–0.6	–1.2
Tanzania	–1.4	–3.1	0.0	–0.5	–2.0	–3.6
Uganda	–0.7	–2.5	–0.1	–0.8	–1.5	–3.4
Other	–15.6	–24.8	–0.8	–24.6	–41.0	–50.1
Subtotal	–19.1	–31.9	–1.2	–26.8	–47.0	–59.8
Total	–114.3	–260.7	–201.9	–223.1	–539.3	–685.7

a. Adjusts for chapter 3's agricultural-sector estimates; see the text.
b. One-half increment of steady state (induced capital investment) effects above static.

Source: Author's calculations.

portion of this increment could be financed by external capital flows in a benign global economic environment.[25] Of particular importance, the inclusion of only half of the net SS welfare and poverty effects has the additional advantage of making some downward allowance for the possibility that the model calibration overstates the extent of induced investment, as discussed in chapter 4.

The resulting consolidated estimate is that over the long horizon of about 15 years, *global free trade could reduce global poverty by a central estimate of 540 million people, and perhaps as many as 685 million*, from the baseline it would otherwise reach. As noted in chapter 3, the World Bank (2002a) has estimated that even without trade liberalization, global poverty should moderate from 2.9 billion today to 2.13 billion in 2015. So the estimates here suggest that *free trade would reduce global poverty by about one-fourth or more from its baseline levels by 2015 and after*.

The bulk of this poverty reduction would occur in Asia. This partly reflects the fact that a large fraction of the world's poor are in Asia: approximately 74 percent (table 5.2). However, an even higher portion of the number of people lifted out of poverty from free trade, almost 85 percent, would be in Asia. This would correspond to 23 percent of today's number of poor people in Asia (central estimate), and an even larger fraction against the baseline number by 2015. In comparison, the consolidated poverty reduction estimates in table 5.3 for Latin America are relatively modest at about 15 million, or only about 10 percent of the number of poor people in this region today. The corresponding proportionate reductions in the number of the poor would be 3.6 percent in Eastern Europe, 17.5 percent in the Middle East and North Africa, and 12 percent in sub-Saharan Africa (again, compared with today's number of poor rather than the somewhat lower 2015 baseline numbers and using the lower "central" estimate).

The long-term poverty reduction estimates (in millions of persons) here are close to those in World Bank (2002a, 175) for the Middle East and North Africa and for Latin America.[26] However, the estimates here are far larger for Asia, and smaller for sub-Saharan Africa and Eastern Europe. The World Bank estimate for 2015 is that free trade would reduce the number of poor by 65 million in East Asia and the Pacific and 118 million in South Asia, or 183 million for the region. In contrast, the central estimate in table 5.3 is 460 million for all of Asia, more than twice as large. Conversely, the central estimate here for sub-Saharan Africa is 47 million, less than half the World Bank estimate of 107 million; and the estimate for the Middle East and North Africa is also smaller (at 3.3 vs. 9 million).

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25. That is, 20 percent of average GDP over 15 years amounts to 1.33 percent of GDP annually.

26. Specifically, the numbers are 15 million here vs. 19 million as estimated by the World Bank for the Middle East and North Africa, and 15 million vs. 17 million for Latin America.

A major reason for the difference in Asia especially is that the present study applies an estimated poverty elasticity specific to each country or region. For most of Asia, this elasticity is high, frequently constrained to the ceiling value of 3.5 (table 5.2). In contrast, the World Bank uses a standard poverty elasticity of 2.0. The theoretical basis for a higher elasticity is set forth in appendix 1B above. It is shown there that in the lognormal distribution of income, the poverty elasticity depends positively on the ratio of mean income to poverty-line income, and negatively on the degree of income concentration (Gini coefficient). Because mean income is relatively high in comparison with the poverty line (e.g., about 3 to 1 in India and Indonesia, almost 10 to 1 in China, and about 2 or 2.5 to 1 even in Bangladesh and Pakistan), and because income concentrations in the region tend to be moderate to intermediate (Gini coefficients of about 0.3 to 0.4), the predicted poverty elasticities tend to be relatively high.[27]

Similarly, one reason the sub-Saharan African estimates here are lower than those of the World Bank is that the low level of mean income relative to poverty income in this region results in a relatively low poverty elasticity where it is estimated directly (averaging around 1.2), although for the large bloc of "other" countries in the region, the same elasticity is used as that by the World Bank (2.0).

The more fundamental difference between the consolidated estimates here and those of the World Bank, however, is the inclusion here of a conceptually additive effect not present in the World Bank model. The estimates here comprise three components: static effects, dynamic productivity effects, and dynamic capital investment effects. The World Bank model captures only the first two effects. The estimates of the first two effects in the present study are indeed surprisingly close to those of the World Bank, as reviewed above. However, gains from induced capital investment in the Steady State model of Harrison, Rutherford, and Tarr (1996, 1997a) used in chapter 4 are not present in the WBGEP model.

Moreover, as elaborated above, the dynamic productivity gain used in the calculations of this chapter has been conservatively gauged to refer only to total productivity gains (analogous to the World Bank's model) rather than incorporating capital deepening, treated separately here through the SS effects. It should be emphasized again that for the latter, a conservative approach is taken by incorporating only half of the PEHRT model estimates, after considering plausible levels of additional investment and making some allowance for possible calibration overstatement. The overall effect, however, is that the central estimate of long-term poverty reduction from global free trade in this study is nearly two-thirds larger than that by the World Bank, at 540 million people instead of 320 million (table 5.3).

27. See appendix table 1A.1. The poverty line used here is $2 per day.

Finally, it is important to consider the welfare gain estimates that correspond to the consolidated poverty-impact estimates. As indicated in chapter 4, global free trade would confer static welfare gains estimated at $87 billion annually for developing countries, at 1997 prices (table 4.1). Welfare gains under the Steady State model would be $161.6 billion annually (table 4.5). Half the increment above the static effects would be $37 billion annually. The welfare gains from the long-term productivity effect can be calculated by applying the percentage productivity gains estimated for each country (table 5.2) to the 1997 base figure for GDP (Dimaranan and McDougall 2002, 2-8, 2-9). When this is done, the welfare gains from the long-term productivity effect amount to $79 billion annually. Summing the three components of total long-term gains (static, half the incremental steady state or induced-investment gains, and productivity-effect gains), the combined long-term welfare gains to developing countries from global free trade would amount to $203 billion annually at 1997 prices and economic scale, or 3.2 percent of GDP.

Global concessional assistance from industrial countries to developing countries amounts to about $50 billion annually. The calculations here suggest that the move to global free trade would confer gains on developing countries that are about four times as large. As set forth in chapter 4, at least half of the developing countries' gains from global free trade would stem from the removal of protection in industrial country markets. On this basis, it can be concluded that if industrial-countries eliminated protection, the resulting welfare gains to developing countries would amount to $100 billion or more annually, or twice the annual benefits currently being conveyed from industrial to developing countries through concessional assistance. The distribution of the trade gains would be much more oriented toward middle-income countries than the existing flows of concessional assistance, however, which tend to go to low-income countries.

Caveats

Globalization skeptics and development pessimists may find these estimates too large. It is useful to consider some of their likely concerns.

Inequality Impact in the 1990s?

Some appear to consider it a stylized fact that the opening of trade regimes in developing countries in the 1990s was generally associated with a widening rather than narrowing of inequality, contradicting expectations based on the Heckscher-Ohlin model and its prediction that open trade should boost the relative price of the abundant factor, unskilled labor in the case of developing countries. If so, might further globalization through a move toward multilateral free trade yield results far less favorable for the global poor than those estimated in this study?

The first point to be made is that the liberalization initiatives of developing countries in recent years have mainly been unilateral, whereas the estimates of this study refer to the potential effects of multilateral liberalization, and in particular to the thoroughgoing liberalization of industrial-country markets for goods exported by developing countries. Typically, studies that find a globalization-inequality link attempt to identify a unilateral liberalization episode and then examine whether it was associated with narrowing or widening income inequality.

Whatever else one should expect from such episodes, it is certain that their terms-of-trade effects will be less favorable for the liberalizing countries than will be the case in multilateral liberalization. Even if the record of the past decade or two were unambiguous in concluding that liberalization by developing countries had contributed to widening inequality and even increased poverty, it would by no means follow that multilateral liberalization would do the same, because there would be additional terms-of-trade benefits.

Surely it is far from clear, however, that the liberalization episodes in recent years aggravated inequality and especially poverty. There was simply too much else going on for a simple coexistence of liberalization and widening inequality to be attributed to the adverse effects of liberalization. In particular, the debt crisis in Latin America in the 1980s and the round of financial crises in the second half of the 1990s must have had much greater impacts on poverty. Moreover, whether inequality in fact worsened is often unclear given data uncertainties.

Wood (1997) has provided a useful review of this issue. He asks why the East Asian experience in the 1960s and 1970s, in which more openness to trade tended to narrow the wage gap between skilled and unskilled workers, appears to have been contradicted by the Latin American experience since the 1980s, in which greater openness has been "accompanied by rising rather than falling wage inequality" (p. 33). He judges that the difference primarily reflects the differences between the 1960s and the 1980s, especially the entry of China into the world market, and perhaps the increase in the latter period of technical change biased against unskilled workers.

Wood first makes the point that when there are more than two countries, two goods, and two factors, the Heckscher-Ohlin prediction becomes ambiguous. For countries with intermediate skill endowments, exports are of goods with medium skill requirements while imports are of goods at both ends of the skill spectrum, so increased trade could either decrease or increase relative unskilled wages. The basic idea here is that Latin America is less abundant in unskilled labor than China, in particular, so it is in this intermediate position.

Wood relies primarily on a series of Latin American studies by Robbins and various coauthors (e.g., Robbins 1996) for the stylized fact that a trade regime change was associated with widening wage differentials.[28] For seven episodes, wage differentials widened in five, narrowed in one, and fluctuated in one. However, the first episode for Argentina ends in 1982, when surely the dominant influence was the country's entry into an acute debt crisis. The episode of widening for Chile is in the 1970s, too early for the general pattern (predating the entry of China). One episode (Chile, 1984–92) is solely "devaluation," which surely is not a test of trade liberalization. Two of the other episodes intermix devaluation with liberalization. Of the seven episodes, only one (Uruguay, 1990–95) is a pure test of liberalization. This is a slim basis indeed for enshrining as a stylized fact that trade liberalization in Latin America systematically increased wage inequality.

The other case usually cited in this direction is that of Mexico, where skill differentials in wages widened after the mid-1980s. Both Feenstra and Hanson (1995) and Revenga and Montenegro (1998) have attributed this outcome to the impact of trade liberalization, and they have searched for explanations of why this result contradicted Heckscher-Ohlin expectations. However, I have emphasized that a far more direct explanation of the widening real wage dispersion was the macroeconomic stabilization package adopted in late 1987. The Solidarity Pact imposed a wage and price freeze along with an exchange rate freeze, reversing the previous practice of indexing wages. The real minimum wage fell sharply as inflation declined only with a lag, and despite the small fraction of the labor force directly receiving the minimum wage, it is likely that low-end wages were eroded relative to skilled wages as a result. The data show no change in the skilled-wage differential during the period 1984–87, but a rise of about one-third in 1988–90, implicating the stabilization program rather than trade opening (Cline 1998, 335).

A prominent feature of Mexico's opening to trade, moreover, was the sharp increase in employment of unskilled workers in the *maquiladora* sector. This suggests a positive rather than negative demand effect for unskilled wages from the opening to trade.

Wood notes that protection was initially high in labor-intensive sectors such as clothing and footwear in Latin America, so that liberalization could have been expected to affect unskilled workers more adversely than skilled. He emphasizes, however, that Bangladesh, China, India, Indone-

28. Ironically, in Robbins's underlying data, the skilled-wage differential is actually declining in each of the episodes (Robbins 1996, table 5). The interpretation of rising differential associated with liberalization depends on his imputation of the net effect after calculating the amount of decline in the relative wage that would have been expected in view of the rise in the relative supply of skilled labor, a pervasive pattern. This approach is vulnerable to overstatement of the decline that could be expected from rising relative skills supply (e.g., by understating the elasticity of substitution between the two types), leaving a residual that has the wrong sign when related to the labor demand shift resulting from liberalization.

sia, and Pakistan had all been largely closed to trade in the 1960s and 1970s but were opening to trade by the mid-1980s, led by Indonesia and China. This was likely to have changed the comparative advantage of middle-income countries toward goods of intermediate skill intensity.

On this basis, whereas the opening of trade by middle-income countries in the 1960s would have tended to reduce wage inequality, by the 1980s it would have been likely to do the opposite. Wood also cites rising skill-intensive technical change in the 1980s and after as an alternative plausible explanation for rising skill differentials in wages, perhaps in an interactive effect in which such technologies became more available as trade regimes were opened. With technical change, Wood emphasizes, the real wage of unskilled workers could rise even if their relative wage fell.

Overall, the historical experience of Latin America in the past two decades would seem an unconvincing basis on which to reject the estimates of potential poverty reduction using the CGE modeling approach of the present study. The stylized facts are largely on relative wages rather than their implications for poverty, and they are at best sketchy in support of any general proposition that liberalization, rather than other influences including financial crises and skill-biased technical change, was the driving force in any widening of wage differentials.

Moreover, with respect to the influence of the entry of China and other large low-income countries into international trade, the CGE approach is ideally suited to taking this effect into account, because it captures the fact that these countries have an even greater relative abundance of unskilled labor than do Latin American countries. Finally, as noted, global liberalization opening foreign markets should be expected to have more positive effects in reducing poverty than unilateral liberalization because of terms-of-trade implications.

Governance and Institutions

A second strand of concern about the potential for poverty reduction through global free trade is likely to be that of the development pessimists, who emphasize that a succession of past panaceas have failed to secure development in much of sub-Saharan Africa because of governance and institutional obstacles. It is certainly true that the calculations of this study represent potential effects under assumptions of a reasonable supply response to new opportunities. It might be possible to recalibrate the model by imposing lower supply elasticities, for example, for countries considered to have governance and institutional problems.

It is certainly true that global trade liberalization is no more a panacea for, especially, countries with chronic governance problems than any of the precursor candidates for solving the problem of economic development. In practice, however, the principal effect of any adjustments for these obstacles would likely be to reduce the projected reduction of poverty in sub-Saharan Africa and perhaps such countries as Pakistan. In sharp contrast, both China and India, which bulk large in the total poverty reduction estimates, have amply demonstrated the governance and institutional capacities to achieve high and sustained growth. In particular, if the central total estimates of poverty reduction in table 5.3 are cut in half for all of sub-Saharan Africa and Pakistan, and cut by one-third for Indonesia and Bangladesh, the effect is to reduce the global estimate for long-run poverty reduction from 540 million to 455 million. This leaves the qualitative conclusion of a major impact unchanged.

Transmission Mechanisms

A reasonable question concerns identifying the mechanisms by which open trade stimulates growth. Even if the statistical results linking productivity growth to trade (table 5.1) are accepted, it may fairly be asked what are the mechanisms underlying the relationship, so that there can be greater confidence in the conclusion that they might apply going forward for developing countries and hence be relied upon as a basis for estimating the poverty effects of global trade liberalization.

More than two centuries ago, Adam Smith set forth two of the key mechanisms: gains in efficiency from specialization and economies of scale. Few would doubt that the traditional static specialization gains remain valid, including for developing countries. Most developing countries will obtain wide-bodied aircraft more cheaply by importing them from Boeing or Airbus with earnings from exports of agricultural goods or labor-intensive manufactures than by attempting to build them domestically. Most developing countries, moreover, are even more acutely dependent on the world market for economies of scale than are the larger industrial countries. Costa Rica has large exports of computer chips from its Intel plant, but it would not have the scale to produce chips efficiently if it did not have access to the external market.

Similarly, the antimonopoly mechanism for a beneficial impact of open trade is likely to be even more important for developing countries than for industrial countries, again because the domestic market will tend to be smaller and hence more susceptible to monopolization. As for the concern about a conflict between static and dynamic comparative advantage (the infant-industry argument), and in particular concern about developing countries' being locked into monoculture—excessive dependence on a handful of traditional tropical or mineral exports—it is perhaps instructive to recall examples such as that of Chile, which moved from high protection in the 1950s and 1960s to open trade by the late 1970s yet did not remain condemned to a monoculture. Copper as a share of exports fell from 52 percent in 1980 to 44 percent by 1990 and 30 percent in 2000

(UNCTAD 2003, 116), while new product lines such as grapes and other fruits and vegetables became major sources of export growth.

Thinking about the mechanisms relating trade to growth is especially important for interpreting the dynamic productivity effects emphasized in this chapter. It might be asked, how can contact with the world market enhance total factor productivity in an agricultural product such as cotton (West Africa) or soybeans (Brazil)? It should be kept in mind that agriculture is a sector in which technical change has been extremely important, as shown by the Green Revolution in Asia based on new seed varieties and improved practices. Today's version of this same point concerns the spread of genetically modified crops in countries such as Brazil. It is far more likely that such advances will be made by countries in close contact with global markets than by countries isolated from them.

The productivity-impact point applies more broadly. One of the more robust stylized facts about trade and growth is summed up by Paul Krugman as follows:

> The raw fact is that every successful example of economic development this past century—every case of a poor nation that worked its way up to a more or less decent, or at least dramatically better, standard of living—has taken place via globalization; that is, by producing for the world market rather than trying for self-sufficiency. (Krugman 2003, 368)

Grossman and Helpman (1994, 40) spell out several ways in which integration with the world economy can help boost productivity growth:

> First, residents of a country that is integrated into world markets are likely to enjoy access to a larger technical knowledge base than those living in relative isolation. Trade itself may help the process of technological dissemination, if foreign exporters suggest ways that their wares can be used more productively or foreign importers indicate how local products can be made more attractive to consumers in their country. . . . Second, exposure to international competition may mitigate redundancy in industrial research. Whereas a firm that develops a product for a protected domestic market need only make use of technologies that are new to the local economy, one that hopes to compete in the international market-place will be forced to generate ideas that are truly innovative on a global scale. . . . [Third,] by expanding the size of the potential customer base, international integration may bolster incentives for industrial research.[29]

29. Note that Grossman and Helpman (1991) are sometimes misinterpreted as showing that protection can be beneficial to a country that otherwise would specialize in labor- and natural-resource-intensive sectors at the expense of human-capital-intensive sectors with high research and development. Given the role of research and development (R&D) in their model, this can indeed lead to slower long-run growth. But they emphasize that "output growth rates do not measure economic welfare." Instead, "a country that lacks the size and technological experience to support a world class R&D effort, or one that has the endowments appropriate to activities like agriculture and mining, typically will gain from specializing in the production of goods that do not require the latest technologies . . . [thereby being] better off trading . . . for manufactured goods than it would if it tried to develop the latest high-technology goods itself" (Grossman and Helpman 1994, 41).

Overall, there is no evident reason why the transmission mechanisms from more open trade to the dynamic gain of factor productivity growth should be any less applicable to developing countries than to industrial countries. Certainly for the two countries with the largest poverty populations in the world, China and India, the experience of the past two decades has been resoundingly consistent with the diagnosis that opening to the world economy fosters more rapid productivity growth.

Underestimation from the Exclusion of Services

Finally, those who are concerned that the central estimates of this study may overstate the potential for reduction in global poverty through free trade should take comfort in the fact that the estimates exclude the liberalization of trade in services. As noted in chapter 3, the World Bank (2002a) has estimated that removing global protection in services trade would generate welfare gains for developing countries that are more than four times as large as the gains from removing protection in merchandise trade. For the reasons set forth in chapter 3, I suspect that such estimates are exaggerated. Nonetheless, they strongly suggest that significant additional gains, and hence additional poverty reduction, are possible if trade is liberalized in services as well as goods.