
Does Trade Reform Promote Economic Growth? A Review of Recent Evidence

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Do trade reforms that significantly reduce import barriers lead to faster economic growth? In the twenty-five years since Rodríguez and Rodrik's (2000) critical survey of empirical work on this question, new research has tried to overcome the various methodological problems that have plagued previous attempts to provide a convincing answer. I examine three strands of recent work on this issue: cross-country regressions focusing on within-country growth, synthetic control methods on specific reform episodes, and empirical country studies looking at the channels through which lower trade barriers may increase productivity. A consistent finding is that trade reforms have had a positive impact on economic growth, on average, although the effect is heterogeneous across countries. Overall, these research findings should temper some of the previous agnosticism about the empirical link between trade reform and economic performance.

JEL Codes: F13, F43

Keywords: trade liberalization, trade reform, tariffs, economic growth.

Introduction

The breakdown of the Doha Development Round trade negotiations in 2008, followed by the global financial crisis of 2008–9, the rise of geopolitical tensions between the United States and China, and the return of economic nationalism all marked an end to a remarkable period of liberalization and economic reform that began in the mid-1980s.¹ In retrospect, the decade from 1985 to 1995 stands out as an unusual period in which developing countries were swept up in a dramatic wave of trade reform (Irwin 2022). These reforms led to much greater openness in many countries and gave shape to the globalized world in which we live today.

Enough time has passed so that economists can look back and take stock of the momentous policy changes that occurred during this period. A fundamental question is

The World Bank Research Observer

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<https://doi.org/10.1093/wbro/lkae003>

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whether the reduction in import restrictions and other trade barriers paid off in terms of faster growth, greater investment, or higher productivity for the countries that chose this path. Early empirical research, such as [Dollar \(1992\)](#), [Sachs and Warner \(1995\)](#), and [Edwards \(1998\)](#), found support for the idea that openness to trade was associated with better economic outcomes. But these papers were subjected to a wide-ranging critique by [Rodríguez and Rodrik \(2000, 266\)](#), who concluded that “the relationship between trade policy and economic growth remains very much an open question” and “is far from having been settled on empirical grounds.”²

Twenty-five years have elapsed since Rodríguez and Rodrik last surveyed the field and there are several reasons why this question deserves reexamination.³ First, many of the early papers in this literature had sample periods that ended in the late 1980s or early 1990s, around the time when many big reforms were just being implemented. Any assessment of these reforms would have been premature at that point, given the length of time it takes to determine if changes in policy have produced an economic payoff. Subsequent studies were able to use additional data to evaluate the consequences of trade reforms undertaken during this period.

Second, more countries have undertaken trade reforms, giving us a larger sample of country experiences than were considered in the earlier literature. For example, at the time of [Sachs and Warner \(1995\)](#), China and India were considered “closed” economies, as were Vietnam, Cambodia, and Bangladesh. Such countries provide additional evidence on the economic consequences of having a more open trade regime.

Third, recent work has employed new and varied empirical methods that address many of the methodological concerns raised about the earlier studies. Regression analysis has moved away from cross-sectional (between-country) comparisons to examining within-country growth following a reform episode. These studies have been supplemented with research using synthetic control methods that allow for a more structured counterfactual scenario against which to judge the outcome of reforms. In addition, empirical studies and model-based simulations of particular countries have focused on the channels through which a reduction in trade barriers might improve economic performance. These papers often use cross-industry variation in the reduction of trade barriers within a country to identify the impact of increased imports on domestic producers. This approach focuses on how the reduction in cost and increase in variety of imported intermediate goods improves the productivity of final goods producers.

This paper reviews the research on trade reform and economic growth in developing countries as a way of understanding what progress has been made in uncovering the link between the two.⁴ Developing countries have had much higher barriers to trade than high-income countries and also much greater opportunities for catch-up growth because they are farther away from the technological frontier. While many OECD countries adopted trade and other structural reforms in the 1980s and 1990s, developing

countries are more likely candidates to have large potential payoffs to increased participation in world trade.⁵

A consistent finding is that trade reforms in developing countries have a positive impact on economic growth, on average, but the effects differ considerably across countries. These results are fairly uniform across methods of analysis, different indicators of trade policy, and other dimensions. The microeconomic evidence that lower tariffs on intermediate goods lead to improved productivity performance of domestic final goods producers is particularly strong. Overall, these research findings suggest that the outcome of trade reforms can be seen in a more positive light than the agnosticism left in the wake of the Rodríguez and Rodrik critique.⁶

In focusing on the impact of trade reforms, usually meaning the unilateral reduction of trade barriers, the paper does not discuss research on several related issues, including the relationship between trade and income, a question examined by [Frankel and Romer \(1999\)](#), [Noguer and Siscart \(2005\)](#), [Feyrer \(2019\)](#), and others. The general finding of this research is that an exogenous increase in trade has a positive and potentially large impact on the level of national income.⁷ However, a policy decision to change trade barriers may not have the same impact on income as an exogenous increase in trade driven by other factors, such as declining trade costs. Furthermore, this paper does not examine the cross-country relationship between trade barriers and economic growth.⁸ The cross-sectional relationship between the level of tariffs and economic growth across countries, which is often found to be positive, cannot be interpreted as causal evidence. The relationship could reflect the fact that low-income countries have higher tariffs than high-income countries and have tended to grow faster for reasons other than their trade policy. Instead, the focus here is on how changes in a country's barriers (trade liberalization episodes) affect its own economic growth.⁹

This paper also does not examine the domestic distributional impact of trade reform, such as the implications for inequality (surveyed by [Pavcnik 2017](#); [Dix-Carneiro and Kovak 2024](#)), labor market adjustment (surveyed by [McLaren 2017](#)), or the incidence of poverty (surveyed by [Winters and Martuscelli 2014](#)).¹⁰ The paper does not look at the impact of a reduction in trade barriers that come about from joining the World Trade Organization ([Chang and Lee 2011](#); [Esteve-Pérez, Gil-Pareja, and Llorca-Vivero 2020](#)), entering into preferential trade agreements ([Baier, Yoto, and Zylkin 2019](#)), or becoming part of regional free trade areas, such as the European single market ([Campos, Coricelli, and Moretti 2019](#)).

This paper begins by documenting the wave of trade reform that swept through the developing world in the late 1980s and early 1990s. It then looks at the different methods used to assess the links between trade reform and economic performance, including cross-country regressions, synthetic control, and empirical or quantitative country studies, discussing the virtues and vices of each approach.

The Trade Liberalization Wave in Developing Countries

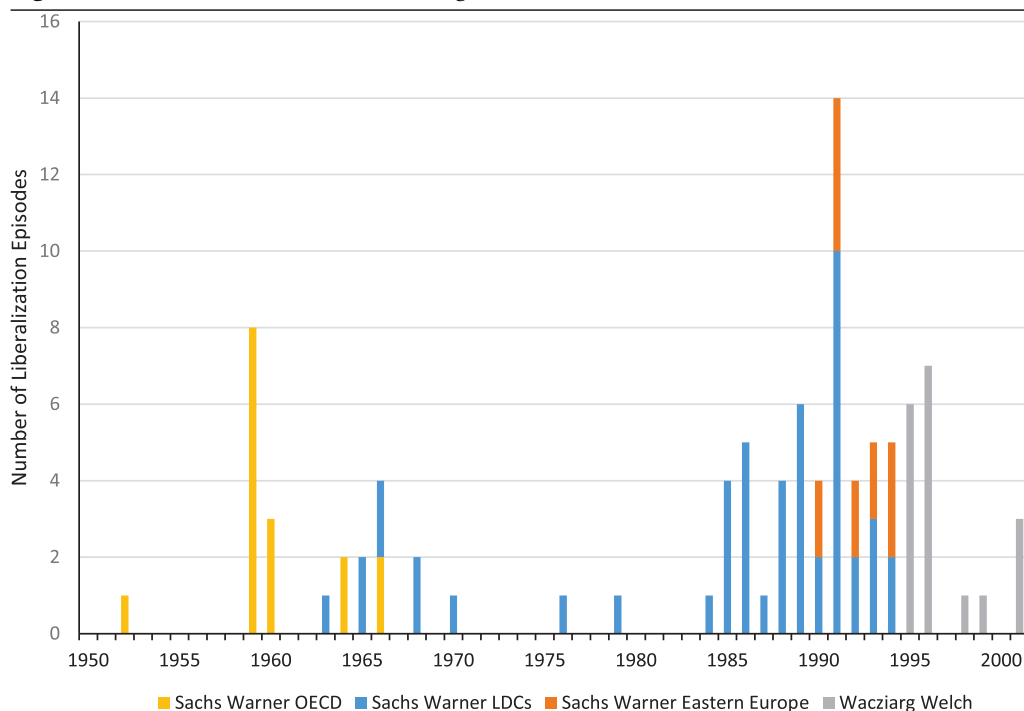
From the 1950s through the 1980s, most developing countries had extensive import restrictions in place (Krueger 1984). These policies went far beyond high import tariffs but included foreign exchange controls and payment restrictions; quantitative restrictions, such as import quotas and licenses; multiple exchange rates; and import surcharges. Many of these barriers dated from the 1950s when developing countries encountered balance of payments problems and needed to conserve foreign exchange. They were not necessarily imposed in a strategic manner to promote certain industries but rather arose at various times for various ad hoc reasons.¹¹

Early on, a few countries began to reduce these barriers to trade and foreign exchange transactions. In many cases, reforms were incomplete and sometimes later reversed. In other cases, there was a dramatic reorientation of a country's trade regime. Early examples of reforming economies include Taiwan (1958–62), the Republic of Korea (1964–68), Chile (1974–79), and a few others. Some countries, such as Brazil (1964–68), Indonesia (1966–71), Argentina (1976–82), and Sri Lanka (1977–83), had spells of liberalization followed by a return to restrictions. By the mid-1980s, however, the number of truly open developing countries was relatively small.

In the late 1980s and early 1990s, however, developing countries embarked on a wave of trade reforms, many of them moving surprisingly fast to open their markets (Dean, Desai, and Riedel 1994; Irwin 2022). They often began by devaluing highly overvalued currencies and unifying their exchange rates. They allowed exporters to retain foreign currency earnings at a competitive exchange rate and relaxed if not eliminated licensing and quantitative restrictions on imports. The reform process continued by gradually cutting import tariffs, usually on intermediate goods first and consumer goods later.

There are several ways to provide a broad picture of the extensive changes made during this period. Figure 1 shows the number of countries that flipped from being “closed” economies to being “open,” according to the classification developed by Sachs and Warner (1995) and updated by Wacziarg and Welch (2008), as discussed below. While there were few reformers in the 1970s or early 1980s, the decade after 1985 saw a dramatic increase in the number of countries changing their policies.

Figure 2 presents the average tariff in developed and developing countries. The average tariff in developing countries declined steadily in the 1980s, dropped more sharply in the early 1990s, and continued to fall at a slower pace thereafter. Most of these reductions were undertaken unilaterally. About three-quarters of the decline in the weighted average tariff in developing countries, from 30 percent in 1983 to 11 percent in 2003, was due to unilateral action, according to Martin and Ng (2004). Only a quarter of the reduction reflected concessions given in the Uruguay Round negotiations, which were phased in starting in 1995.¹²

Figure 1. Number of Countries Undertaking Trade Reforms, 1950–2001

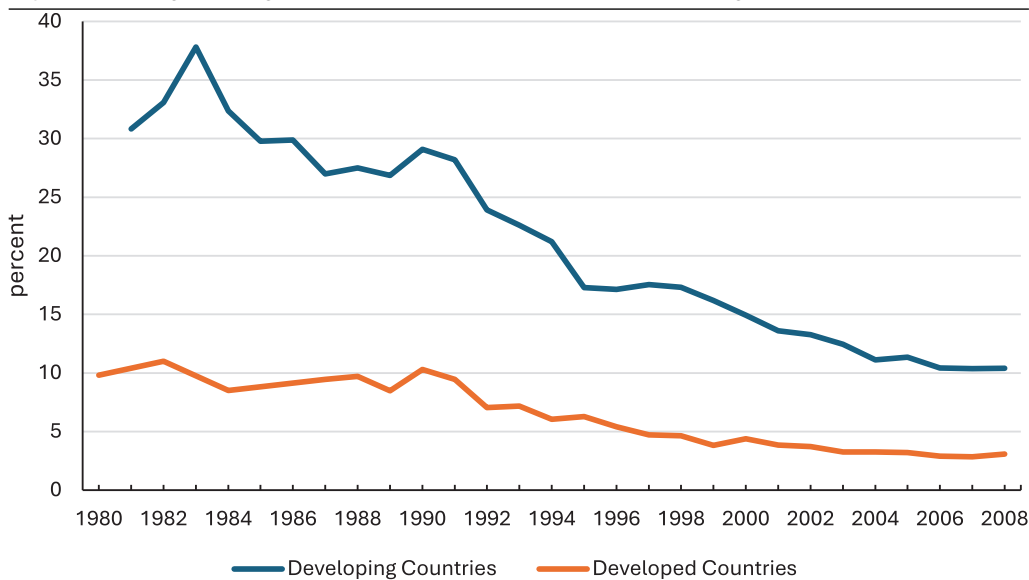
Sources: Author's compilation based on [Sachs and Warner \(1995\)](#); [Wacziarg and Welch \(2008\)](#).

[Figure 3](#) shows that not all regions experienced the same degree of tariff reductions. South Asian countries started with very high tariffs but reduced them sharply. Latin America and East Asia saw significant reductions as well. By contrast, countries in Africa and the Middle East did not change their policies as much as those in other regions.

One problem with focusing on tariffs is that nontariff barriers are often a more important impediment to imports. Nontariff barriers (NTBs) take many forms, from import licensing requirements, prohibitions, quotas, and administered pricing. All of these policies make it more difficult to import. These NTBs are inherently difficult to measure, let alone aggregate, but [Figure 4](#) presents the “coverage ratio”—the share of imports directly affected by such measures—in developing countries between the early 1990s and the late 1990s. For most regions, the coverage ratio declines over this period.

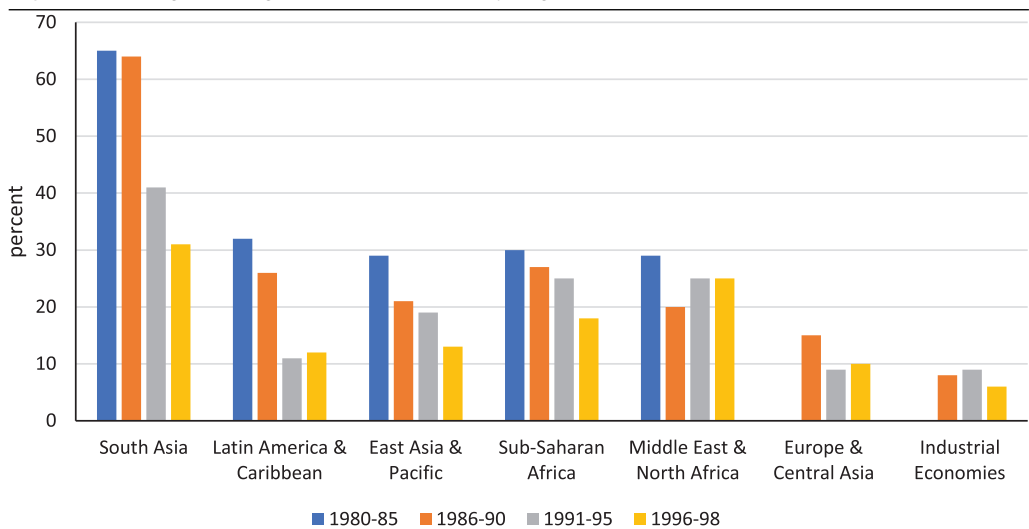
The trade regime of many developing countries was also affected by the freedom of payments on current account transactions and the exchange rate regime. For example, governments commonly restricted imports through the allocation of foreign exchange. Exporters were forced to surrender their foreign exchange earnings to the central bank at the official exchange rate, enabling officials to ration foreign exchange

Figure 2. Average Unweighted Tariff Rate in Developed and Developing Countries, 1980–2008

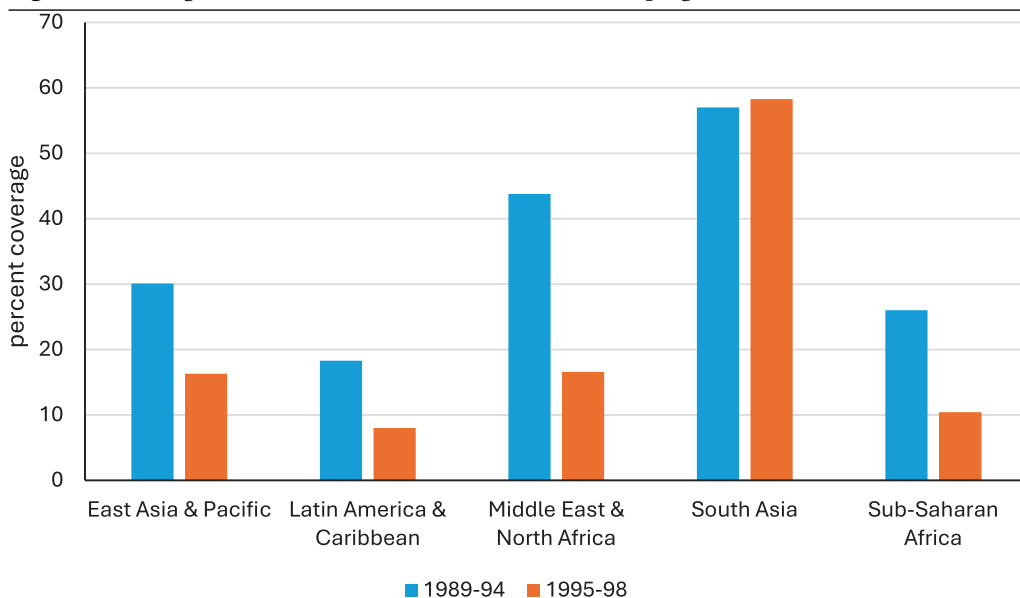


Source: Hoekman, Mattoo, and English (2002), p. 567, and World Bank tariff data.

Figure 3. Average Unweighted Import Tariffs, by Region, 1980–98



Source: World Bank (2001, 53).

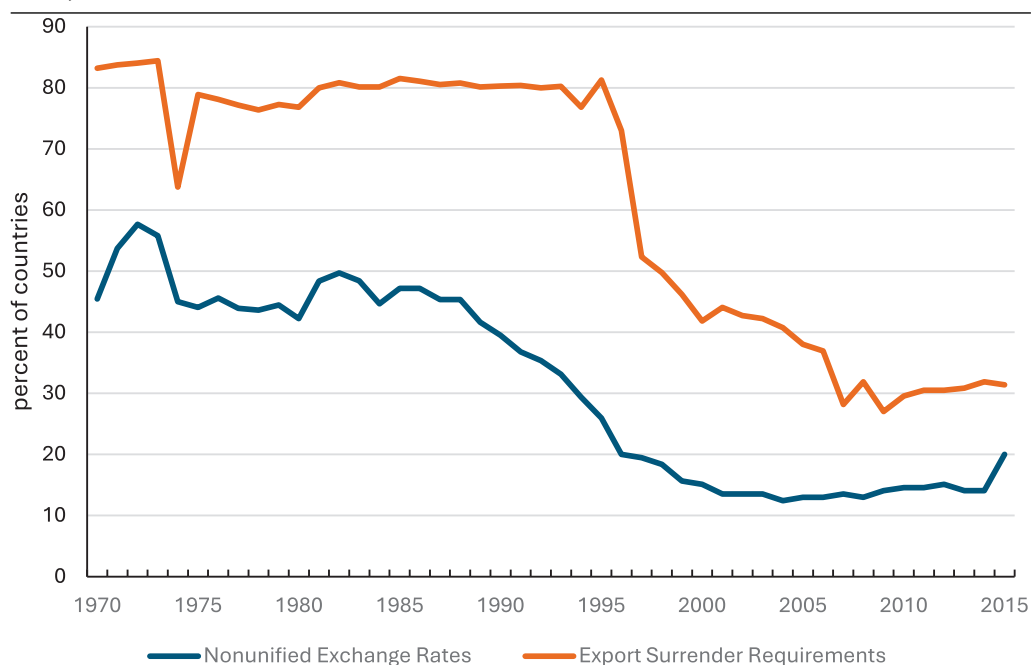
Figure 4. Coverage Ratio of Core Non-tariff Barriers in Developing Countries, 1989–98

Sources: World Bank (2001, 53).

and channel it to preferred importers through the issuance of discretionary import licenses (Bhagwati and Krueger 1973). Figure 5 shows the share of countries with export surrender requirements, which drops sharply in the late 1990s. The figure also presents the share of countries with a nonunified exchange rate (i.e., multiple exchange rates or a parallel market that exists alongside the official rate), another signal of government restrictions on foreign exchange. This indicator shows a steady drop from the mid-1980s to the mid-1990s, as developing countries moved to more market-oriented exchange rate systems.

The World Bank and the International Monetary Fund (IMF) strongly supported these efforts by developing countries to open up their markets and seemed confident that there would be a significant payoff from doing so.¹³ Academic economists were more guarded at the time, as reflected in two prominent articles in the *Journal of Economic Perspectives*. Rodrik (1992, 90) argued that the presumption that a liberal trade regime would be good for economic development “is not tremendously helpful for policy at the present time,” adding that “in most of the countries that have undertaken radical trade reforms in the 1980s, the direct efficiency consequences of trade liberalization are still uncertain and likely to be small.” Dornbusch (1992, 73) welcomed the reform efforts, but noted that “measuring the benefits of trade reform has been a frustrating endeavor. Although the discussion of trade policy at times gives the impression that a liberal trade regime can do wonders for a country’s economy, and most observers

Figure 5. Percent Share of Countries with Export Surrender Requirements and Nonunified Exchange Rates, 1970–2015



Sources: Nonunified exchange rates are from [Ilzetki, Reinhart, and Rogoff \(2019\)](#); export surrender requirements are from IMF, Annual Report on Exchange Restrictions and Exchange Arrangements ([various years](#)).

believe firmly that trade reform is beneficial, yet systematic attempts at quantification fail to single out trade policy as a major factor in economic growth.”

These prominent economists were not alone in their skepticism of the existing evidence on trade reform and growth. Around the same time, [Krugman \(1995, 33\)](#) commented that “the widespread belief that moving to free trade and free markets will produce a dramatic acceleration in a developing country’s growth represents a leap of faith, rather than a conclusion based on hard evidence.”¹⁴ Meanwhile, [Taylor \(1991, 106, 113\)](#) asserted that “recent experience does not substantiate claims about [trade] liberalization’s beneficial effects” and “neither openness nor trade liberalization fosters income growth; the empirical and even theoretical linkages are simply not observed.”

Standard theory suggests that reducing trade barriers should lead to efficiency gains. But why might it be expected to increase economic growth as well? The strongest case is that trade reform can promote capital investment and technical efficiency in a way that increases potential GDP. This leads to a temporary increase in the rate of growth as the economy closes the gap between the current level of GDP and its higher potential level. The magnitude of the higher transitional growth rate depends on how much the efficiency gains (productivity improvements) and factor accumu-

lation (greater capital investment) increase potential GDP and the length of time it takes for current GDP to converge to its potential. Both of these factors differ considerably across countries. When policy reforms (trade related or otherwise) are limited and phased in, there is no reason to expect an immediate burst of growth. But if a country is significantly behind the technological frontier and new investment opportunities become available to it as a result of trade, some acceleration in growth would be expected (Lucas 2009).

Suppose trade reform did affect a country's growth rate. How would researchers be able to know it, given the many factors that affect a country's economic growth at the same time? Economists have long recognized that it is exceedingly difficult to isolate individual factors; social scientists cannot run natural experiments in which two identical countries differ only in their trade policies, as John Stuart Mill imagined would be a decisive test.¹⁵ To understand the impact of trade policies, therefore, economists have compiled different sorts of empirical evidence, identified trade reform episodes in different ways, and made various kinds of counterfactual comparisons.

Cross-Country Regressions

A seemingly straightforward way to evaluate the impact of a country's trade policy on its economic performance is to compare an outcome variable (growth in real per capita income) under different trade regimes, controlling for as many other covariates as possible. There are formidable obstacles to doing so, one of which is coming up with a single variable that represents a country's trade policy.¹⁶

In a widely cited paper, Sachs and Warner (1995) reduce trade policy to a single binary variable. They construct a dummy variable indicating whether a country is "open" or "closed" to trade, defining a country as closed if it had at least one of the following five characteristics: an average tariff of 40 percent or more, nontariff barriers covering 40 percent or more of trade, a black-market exchange rate that was at least 20 percent above the official rate, a state monopoly on major exports, or a socialist economic system. Figure 1 shows the number of countries that became "open" by this tally.

Sachs and Warner estimate a simple cross-sectional regression:

$$\Delta \log y_{i,1970-89} = \alpha \log y_{i,1970} + \gamma \text{Open}_{i,1980s} + X\beta + \varepsilon_i$$

that relates economic growth in real per capita income (y) between 1970 and 1989 to the initial level of income in 1970; the openness dummy variable (in the 1980s); and other political and economic control variables, such as schooling rates, investment, and government spending. The estimated coefficient on "open" is 2.4, implying that open economies grew 2.4 percentage points faster than closed economies. In this regression, the identification comes from comparing open with closed countries. This raises the problem of omitted variables, because the two sets of countries could differ on many dimensions that the controls do not capture. The results focus on a single-

period cross-section ending in 1989. They did not use the timing of the openness decision to look at the impact of a reform decision on a country's subsequent growth.

In this case, questions can be raised about both the dependent and the independent variables.¹⁷ In terms of the openness dummy variable, the question is whether a simple zero-one indicator accurately captures the broad stance of a country's trade policy. Sachs and Warner concede that their measure is crude but argue that it usefully places countries into two different and meaningful categories. [Winters and Masters \(2013, 1062\)](#) note that "the thresholds are not estimated nor justified, it is not clear which policies really matter and a liberalization is registered only if it flips a country across a threshold." A simple dummy variable does not capture the restrictiveness of a country's trade policies or the degree to which a country changes its policy at a given point in time.¹⁸ In addition, the variable is unidirectional (from closed to open) and does not capture countries that go through multiple periods of being open or closed. For example, Argentina's trade policy became more open during the 1970s, closed during the 1980s, open again in the 1990s, and more closed again in the 2000s.

Another question is which of the five components of the Sachs-Warner measure is most responsible for their empirical findings. [Harrison and Hanson \(1999\)](#) unpack the Sachs-Warner indicator and find that tariffs and quotas do not have any explanatory power on growth but exchange rate distortions do. When they try a different tariff measure—the average tariff measured by customs revenues divided by imports—they find that both tariffs and distorted exchange rates have a negative impact on growth.¹⁹

[Rodríguez and Rodrik \(2000\)](#) argue that the explanatory power of the Sachs-Warner indicator comes mostly from the black-market exchange rate premium and state monopoly of exports. In their view, neither of these factors clearly represents a trade policy variable: State monopoly is effectively a dummy variable for Africa, and a black-market premium is more reflective of macroeconomic distortions. They conclude that the most direct indicators of trade policy (tariffs and quotas) are not clearly linked to growth outcomes.²⁰

In fact, an overvalued currency, as reflected in a black-market premium, is almost invariably the breeding ground for restrictions in imports ([Shatz and Tarr 2002](#)). When developing countries were reluctant to embrace floating exchange rates, but opted instead for fixed rates or managed pegs, they often failed to devalue their currencies in line with their higher inflation rates. This led them to have overvalued currencies, which gave rise to foreign exchange shortages and balance of payments problems. Given the reluctance to adjust the exchange rate, government officials sought to stem the loss of foreign exchange reserves by adopting exchange controls, import surcharges, and other non-tariff measures to reduce spending on foreign goods (sometimes referred to as import compression policies). While difficult to measure, these foreign exchange controls are known to have an adverse effect on trade.²¹ Because of the importance of exchange rate policy, [Collier \(1993, 510\)](#) argues that "The heart of

liberalization is the conversion from using trade policy for payments balance to using the exchange rate.”

Therefore, the trade reform process almost always begins with a currency devaluation to eliminate any black-market premium and establish a realistic exchange rate.²² A devaluation encourages exports and discourages imports, which helps stabilize the level of foreign exchange reserves. This enables the government to end the rationing of foreign exchange and relax or even eliminate quantitative import restrictions and import licensing. Traditional import liberalization—namely, the reduction in import tariffs—usually occurs after these adjustments have been made.

As already noted, Sachs and Warner did not take advantage of the dates of specific trade reform episodes to investigate whether a move toward more open policies leads to an acceleration in economic growth. Yet the Sachs-Warner variable is probably more useful as a marker of the timing of a country’s trade reform process than it is as a way of categorizing countries as open or closed. Instead of comparing growth rates in open versus closed countries, one could examine what happens when a country moves from being closed to being open, which is what subsequent researchers have done.

Wacziarg and Welch (2008) updated the Sachs-Warner openness variable to include data from the 1990s (in what will be called the SWWW indicator) and find that the openness variable no longer separates high-growth from low-growth countries the way it could in the 1970s and 1980s. However, they use the dating of reform episodes to estimate the within-country impact of trade reform on growth and investment through the following regression:

$$\Delta \log y_{it} = \alpha + \beta LIB_{it} + \varepsilon_{it}$$

where y_{it} is per capita income between two periods and LIB is the date of the SWWW liberalization variable, which takes the value of 0 for “closed” economies” and 1 when a country becomes “open” according to the SW criteria. The error term is modeled as $\varepsilon_{it} = \nu_i + \eta_t + \mu_{it}$, where ν are country- and η are time-fixed effects.

Wacziarg and Welch find that trade reforms have a positive, economically large, and statistically significant impact on growth and investment within a country. Over the 1950–98 period, countries that liberalized their trade regimes experienced 1.4 percentage point higher growth than in the pre-reform period. Wacziarg and Welch also break out the coefficient by decade. The coefficient is 0.6 for 1950–70, 1.8 for 1970–90, and 2.5 for 1990–98. The implication is that countries that liberalized in the 1950s and 1960s saw little payoff, but countries that did so in the 1980s and 1990s saw large payoffs.²³

Wacziarg and Welch focus on the path of growth around the time of the reform. They find that growth was depressed three years before reform, rose slightly in the three years after reform but was indistinguishable from zero, increased to 1.44 percent in the period three-to-six years after a reform, and declined to 1.0 percent thereafter. These findings are consistent with a 1.0–1.5 percent increase in growth three years after reform. Wacziarg and Welch report that the investment rate rose by 1.5–2.0 percent-

age points over the pre-reform period and conclude that about 21 percent of the effect of liberalization on growth came from increased capital investment. They also find that liberalization increased openness (the trade-to-GDP ratio) by nearly 6 percentage points.

Wacziarg and Welch also called attention to the high variance in the outcomes: Although the average effect was positive, about half of the countries did not experience more rapid growth after opening.²⁴ They attribute this heterogeneity to a variety of factors: “Countries that experienced negative or no effects on growth tended to have suffered from political instability, adopted contractionary macroeconomic policies in the aftermath of reforms, or undertaken efforts to counteract trade reforms by shielding domestic sectors from unnecessary adjustments.”

Growth effects could differ across countries for other reasons as well. The extent that countries undertake reforms could vary significantly in a way that the binary openness variable does not capture. In addition, if complementary reforms, such as freeing the labor market from regulatory restrictions or ensuring greater competition in the service sector, are not undertaken, the gains from trade may be limited (Chang, Kaltani, and Loayza 2009). These reasons will be further discussed below, as this issue is a worthwhile area for further research.

One problem with the SWWW dates is that they do not allow for policy reversals—a frequent occurrence in Latin America—and the use of a binary indicator means that the speed and depth of policy changes are not captured. Feyrer and Irwin (2024) employ a continuous time indicator of current account openness (updated from Quinn 1997) that reflects the speed and depth of such episodes as well as accounting for changes in foreign exchange controls. The variable also allows for the possibility that countries (such as Argentina) go through cycles of opening and closing. Impulse response functions to a change in current account openness indicate that most of the increased growth occurs in the first five years after a reform episode, after which the impact diminishes. For countries going from closed to open, income is about 10–15 percent higher a decade after the reform.

Estefania-Flores et al. (2022) also employ a measure of trade and payments restrictions based on the IMF’s *Annual Report on Exchange Arrangements and Exchange Restrictions*. Using the report, they code annual indicator variables on a host of policy measures related to current account transactions for up to 157 countries from 1949 to 2019. They find large and persistent reductions in GDP as a result of an increase in these restrictions. They also find that closing generates larger income losses than opening leads to income gains, a result consistent with Furceri et al. (2022) which looks at tariff shocks.

Another problem in interpreting the economic growth that occurs after a SWWW reform date is that many countries opened to trade as part of a larger package of economic policy reforms, making it difficult to attribute the growth outcomes exclusively to changes in trade policy. To account for this issue, Wacziarg and Welch focus on a sub-

sample of 22 relatively “pure” trade liberalization episodes, mainly developing countries in the 1980s, eight of which occurred without other major shifts in domestic policy. The results are roughly the same as the results based on the full sample, leading the authors to conclude that it is plausible that their findings are in large part attributable to external sector reform.

Prati, Onorato, and Papageorgiou (2013) also try to separate out the contributions of different policies in a reform package to economic performance in a cross-country context. Focusing on the period 1973–2006, they use different indicators of real (trade, agriculture, and networks) and financial (banking, finance, capital account) policy. For trade they use the average tariff and an indicator of current account restrictions, both of which are scaled from 0 to 10. In the baseline regression, the coefficient on trade is 1.9 and the coefficient on the current account is 3.3, indicating that both sets of reforms independently improve growth prospects, although it is difficult to interpret the precise meaning of these coefficients.

Other researchers explore similar questions using different variables to represent trade reform.²⁵ Salinas and Aksoy (2006) take an indicator of trade reform from the World Bank Trade Assistance Evaluation, an ex-ante signal of World Bank assistance in helping to initiate a trade reform. This dating marks the start of a trade reform, not the crossing of some arbitrary threshold.²⁶ Their variable also includes other countries (such as China, Croatia, and India) that are considered closed by SWWW.²⁷ In a cross-country panel regression with country and time fixed effects, they find that economic growth is 1.2 percentage points faster after the start of a liberalization than before. Widening the window of the reform period still yields growth that is about 1 percentage point faster. This finding is robust to the inclusion of many other variables that might affect growth, and the results are positive for Sub-Saharan African countries. They find that reform has no impact on industrialization but does increase the share of manufactured exports and reduce export concentration. Overvalued real exchange rates limit the supply response to trade reform.

Estevadeordal and Taylor (2013) point out that not all tariff reductions should be expected to increase growth: Reducing tariffs on final consumption goods might increase economic welfare, but it would not necessarily increase a country’s potential growth in the same way that a reduction in tariffs on capital and intermediate goods could increase the capital stock and improve technology. Because broad measures such as average tariffs would conflate the different impacts, they focus on the differential growth impact of these different tariffs. They look at a long difference-in-difference regression comparing liberalizing and non-liberalizing countries in two long periods, 1990–2004 and 1975–89, in order to avoid contaminating the results with short-run business cycle fluctuations, crises, or lags in policy implementation.

Using an openness indicator variable like SWWW, they find that reduced tariffs on capital and intermediate goods resulted in a 1 percentage point growth acceleration for liberalizing countries. Switching to data on actual tariff changes, which provides

greater variation in the policies across countries, they find stronger growth effects for countries that reduced tariffs on intermediate and capital goods than for countries that reduced tariffs only on consumption goods. Their key finding is that a 25 percent reduction in the tariff on capital or intermediate goods is associated with a 0.75–1 percentage point increase in economic growth for liberalizers compared with non-liberalizers. They show a dramatic divergence in the path of real per capita GDP between the two groups: By 2004 the liberalizers were 10 percent above the 1975–98 trend of both and non-liberalizers had fallen almost 10 percent below trend, creating a 15–20 percent gap between the two sets of countries.²⁸

The Estevadeordal and Taylor findings are important because they confirm that using actual tariff data (on intermediate goods) leads to results that are similar to those from the SWWW indicator. In both cases, trade reform is associated with about a 1 percentage point increase in economic growth relative to the baseline.

A related strand of research examines whether “growth accelerations” are systematically related to economic reform episodes. Hausmann, Pritchett, and Rodrik (2005) identify about 80 episodes of rapid acceleration in economic growth that were sustained for at least eight years during the period 1957–92.²⁹ Of many potential factors, they find that accelerations occur when there is an increase in trade, an increase in investment, and a large depreciation in the exchange rate. Using the SWWW dates, they find that economic reform is a statistically significant predictor of sustained growth accelerations but that most instances of economic reform do not produce growth accelerations. Only about 20 percent of sustained growth episodes are preceded or accompanied by economic liberalization, suggesting that many other factors are at work and our ability to predict growth acceleration episodes is low.

Jong-A-Pin and De Haan (2011) argue that the filter Hausmann, Pritchett, and Rodrik use to select the starting dates of the growth accelerations leads to some anomalous results.³⁰ By simply requiring that economic growth in the first year of the acceleration be higher than the year before, they identify 89 accelerations over the period 1957–93 and report stronger evidence that growth accelerations are preceded by economic liberalization.³¹

Pritchett et al. (2016) document some of the largest and most important growth accelerations (in Taiwan [1962], Brazil [1967], China [1991], India [1991], and Poland [1991]), without noting that they seemed to occur around the time of major trade reforms. The magnitude of the gains from these growth accelerations was enormous. The decade-long growth acceleration in India after 1993, for example, amounted to an extra \$1.1 trillion in national income. Even if a more open trade policy was responsible for just a fraction of the resulting income gains in these cases, the reform would have yielded enormous benefits for these countries.

Related work by Freund and Pierola (2012) uses the SWWW dates as a potential trigger for “export surges.” They find that periods of rapid export growth are more likely to occur in open or liberalizing countries, are usually preceded by a large depreciation

of the real exchange rate, and that new exports (on the extensive margin) account for much of the acceleration. About 58 percent of export surges occurred in open countries or countries that opened within five years before a surge, 24 percent occurred in closed countries, and 19 percent occurred before a country's opening.

Finally, mention should be made of recent work on the impact of positive tariff shocks, although this is the opposite of trade liberalization. Using a large panel of countries from 1963 to 2014, [Furceri et al. \(2020\)](#) find that tariff increases are associated with large and persistent declines in domestic output and productivity.

To conclude, a consistent finding of many cross-country regression studies, often using a variant of the SWWW indicator variable, is that countries that relax significant import restrictions usually experience a pickup in economic growth. [Table 1](#) summarizes the key findings of these papers and some others that are not discussed. The results may not hold for every country, but on average the results are positive and of an order of magnitude of 1 percentage point or more. While attributing all of the immediate postreform growth to trade policy alone is difficult, researchers have made reasonable, if imperfect, efforts to try to isolate the impact of trade policy changes from other changes in policy.

In interpreting the results from such regressions, [Winters and Masters \(2013\)](#) make a critical point: Researchers tend to focus on statistical significance, asking whether we have confidence that an estimated coefficient is different from zero, whereas policymakers care about the distribution of possible outcomes. These are very different questions. To drive home this point, they consider the following hypothetical scenario: Suppose a policymaker is considering whether to implement a 10-percentage point tariff reduction when the estimated elasticity of income with respect to the tariff is 1 and the standard error is 0.7. The null hypothesis that the coefficient is 0 cannot be rejected at the 5 percent confidence level, leaving the econometrician agnostic about the outcome.

But for a policymaker, it is much more relevant that 91 percent of the probability mass of outcomes is positive, and the best guess is that the tariff reduction would raise income by about 10 percent. The authors of virtually all of the papers considered in this section focused on statistical significance rather than the distribution of outcomes. In considering the probabilities of various outcomes, the expected value of a trade reform is positive and potentially quite large for countries that have not reformed.

Synthetic Control Methods

Focusing on within-country growth as a result of trade liberalization is an improvement over a cross-sectional comparison, but it still does not quite get at the key issue. The appropriate question is not whether growth is faster after a reform episode than before but whether growth performance is stronger than it would have been in the absence of the reform. Answering this sharper counterfactual depends on an explicit

Table 1. Selected Studies of Trade Reform and Economic Growth

Study	Period	Policy indicator	Sample size	Main finding
Growth regressions				
Greenaway, Morgan, and Wright (2002)	1975–93	Various dichotomous indicators	69 developing countries	≈ +2.7 percentage point increase in growth
Salinas and Aksoy (2006)	1970–2004	World Bank episodes	39 developing countries	≈ +1.2 percentage point increase in growth
Wacziarg and Welch (2008)	1950–98	Updated Sachs-Warner indicator	≈ 133 countries	≈ +1.4 percentage point increase in growth
Falvey, Foster, and Greenaway (2012)	1970–2003	Wacziarg-Welch indicator	39 developing countries	≈ +1.8 percentage point increase in growth
Falvey, Foster-McGregor, and Khalid (2013)	1970–2005	Wacziarg-Welch indicator	≈ 50 developing countries	≈ +1.7 percentage point increase in growth
Estevadeordal and Taylor (2013)	1975–2004	Average import tariff (aggregate, disaggregated)	≈ 44 developed and developing countries	≈ +1 percentage point increase in growth, income 15–20 percent higher after 20 years
Feyrer and Irwin (2024)	1950–2015	Updated Wacziarg-Welch, current account openness	≈ 125 countries	Income ≈ 10–20 percent higher after 5–10 years
Synthetic control				
Billmeier and Nannicini (2013)	1963–2005	Wacziarg-Welch indicator	30 developing-country cases	Positive but heterogeneous effect of trade reform on growth
Billmeier and Nannicini (2011)	1993–2005	Wacziarg-Welch indicator	5 transition economies	Positive but heterogeneous effect of trade reform on growth
Country studies				
Connolly and Yi (2015)	1962–89	Tariffs and quantitative restrictions	Korea, Rep.	Tariff reductions explain 17 percent of Korea’s catchup
Alessandria and Avila-Montealegre (2023)	1989–93	Tariffs	Colombia	Welfare gain of 5 percent; long-run consumption gain of 6 percent

Source: Author’s compilation.

modeling of what would have happened to an economy had a trade reform not taken place.

Abadie and Gardeazabal (2003) propose a way of setting up a reasonable counterfactual scenario against which a reform episode can be evaluated. A synthetic control

is a weighted combination of non-reforming comparison countries constructed to fit the pre-reform economic growth path of the country that undertakes liberalization. This “synthetic” country acts as a control, representing what would have happened to the reforming country had it not reformed. The path of the synthetic control countries can then be compared to the actual post-reform outcomes to determine the impact of reform. This method comes closest in spirit to addressing the problem of finding a counterfactual for what would have occurred in a country in the absence of a reform.

The synthetic control method rests on identification assumptions that are weaker than the those required by estimation techniques commonly used in the trade and growth literature. Panel models, for example, control only for confounding factors that are time invariant (fixed effect) or share a common trend (difference-in-differences). The synthetic control allows the effect of unobservable confounding factors to vary with time.

Billmeier and Nannicini (2013) use the SWWW reform dates and apply the synthetic control method to 30 trade liberalization episodes from 1963–2005. In each case, they choose a set of appropriate comparison countries in which the pre-reform growth paths in the reforming country and the weighted nonreforming countries are closely matched. The pooled regression-based approaches yield only an average treatment effect across all countries, around which there are successes and failures that are not clearly revealed. In contrast, the synthetic control approach is essentially a quantitative case study in which each episode constitutes its own individual case.

Billmeier and Nannicini conclude that trade reforms had a positive impact on income, but with much heterogeneity across country and time. This heterogeneity, they suggest, can be attributed to wide differences in the scale of reforms adopted by different countries and whether complementary policies (such as labor market reforms) were in place. In general, they find that economic reforms in Asia and Latin America had positive outcomes; growth in real per capita GDP was higher than the counterfactual composite after the reform. Some early reformers in Africa also experienced gains, but that was less true for late reformers. The results were inconclusive for the few reformers in the Middle East and North Africa.

In this approach, no single number quantifies the average impact of trade reform on growth because each country is a separate case. Indonesia, for example, is presented as an example of an economic liberalization episode (in 1970) that had a large economic payoff. The average income in the years before liberalization was nearly identical to that of the synthetic control, a weighted average of Bangladesh (41 percent), India (23 percent), Nepal (23 percent), and Papua New Guinea (13 percent). After trade reforms, Indonesia’s per capita GDP soared. It was 40 percent higher than the estimated counterfactual after five years and 76 percent higher after 10 years. These results are robust to placebo testing, as none of the “fake” experiments for the eight (regional) potential comparison countries showed treatment effects that were larger than the baseline estimates.

This example also illustrates one of the pitfalls of the synthetic control approach. Indonesia is endowed with abundant reserves of petroleum, which became very valuable after the 1973 oil shock, a resource that the synthetic control countries (chosen for their performance prior to the shock) lacked. The posttreatment growth path may be contaminated by factors unrelated to trade reform.³²

Turning to other Asian countries, Korea (1968) is also deemed a success story, with income about twice as high as in the counterfactual case after 10 years. The results are also positive but not as great for the Philippines (1988) and Nepal (1991). In each case, the credibility of the results depends almost entirely on whether the synthetic control is a plausible representation of what would have happened had there been no reform, something that can be judged only on a case-by-case basis.

The results are also generally positive in Latin America. In Barbados, Colombia, Costa Rica, and Mexico, economic outcomes improved after economic liberalization. A decade after liberalization, per capita income was about 57 percent higher in Barbados, 23 percent higher in Colombia, 26 percent higher in Costa Rica, and 21 percent higher in Mexico than in the synthetic control. The placebo tests confirm that these findings are largely robust. Chile turns out to be a hard case to assess, because a pre-reform collapse in income and the financial crisis in 1982 make it difficult to find a suitable synthetic control.

The results are more mixed in Africa. Broadly speaking, only the early liberalizations seem to have had a positive impact on per capita income; almost all of the later attempts had only slightly positive or no effects. The authors suggest that the late liberalizers in Africa adopted gradual reform strategies, leading to attenuation bias in the results. In the Middle East and North Africa, the results are inconclusive and sometimes the treated countries perform poorly in comparison to the synthetic control. In general, Billmeier and Nannicini conclude that trade reforms were generally positive and that late liberalizations had a lower payoff or lacked the complementary policies needed to ensure growth.

Billmeier and Nannicini (2011) also use synthetic control methods to examine five transition economies from the former Soviet Union: Armenia and Azerbaijan, which liberalized in 1995; Georgia and Tajikistan, which liberalized in 1996; and Uzbekistan, which failed to liberalize. Ten years after liberalization, real per capita GDP was 44 percent higher in Georgia and almost 100 percent higher in Armenia compared with each country's synthetic control. Placebo tests support these results. The results for Azerbaijan were confounded by civil conflict and a drop in natural resource extraction. For Tajikistan no set of countries formed an acceptable synthetic control in which the pretreatment fit was adequate. For the one nonliberalizer, Uzbekistan, the counterfactual synthetic control suggests that real per capita income would have been 75 percent higher had it liberalized trade.

Another use of synthetic control in conjunction with the SWWW dates is the Olper, Curzi, and Swinnen (2018) study of the impact of trade liberalization on child mortal-

ity. They find that trade liberalization significantly reduced child mortality in approximately half of the cases, and by as much as 20 percent in a majority of these cases. In most other cases there was no significant effect. Trade reform reduced child mortality more in democracies than autocracies, when initial incomes were higher, and when the reform reduced the taxation of farmers.

Despite the benefits of the method, there are few other studies of trade reform using synthetic controls. One reason is that [Billmeier and Nanncini \(2013\)](#) in a single paper exhausted most of the cases, leaving little for other researchers to follow up on. A larger literature uses synthetic control methods to evaluate the impact of more general market-oriented reform packages. [Marrazzo and Terzi \(2017\)](#) use a synthetic control approach to examine the effect of 29 structural reform episodes between 1961 and 2000. They find that, on average, reforms had a significant positive impact on per capita GDP, but only after five years. After 10 years, per capita GDP was about 6 percentage points higher than the synthetic counterfactual scenario. However, they consider only reform packages (including trade policy, measured by the average tariff, and other policies together) and cannot quantify the impact of a single policy measure. This makes it difficult to determine how much of the growth is coming from the trade reforms as opposed to other economic policy changes. Of course, if even a fraction of the income gains can be attributed to the trade component of the reforms, the value of the reform appears to be very large. To the extent the reform package involves a macroeconomic austerity program, such as tighter fiscal and monetary policies, one might expect growth performance to have been worse in the short run.

A common finding across these various studies is the heterogeneity of outcomes to trade liberalization efforts. There are many possible explanations for the divergence in outcomes across countries and more research is needed to understand these differences. Researchers have often pointed to frictions in labor markets, financial markets, and services (non-traded inputs) as possible reasons why some countries benefit more than others from trade reform.³³ For example, [Kohn, Leibovici, and Szkup \(2023\)](#) suggest that low financial development substantially limits the gains from trade liberalization. Studying Colombia, they find that reduction in capital goods tariffs between 1991 and 1995 explained more than half of the observed GDP growth in Colombia during that period but argue that the country would have benefited even more if it had better financial markets. In their counterfactual scenario, if Colombia had the level of financial development of the United States, then its GDP would have been 4.6 percent higher than it was in 1995, mostly due to a larger investment boom following the trade reforms. They conclude that the welfare gains from trade liberalization are larger in financially developed economies (3.0 percent in consumption-equivalent units vs. 0.5 percent in financially underdeveloped economies).

Domestic services are a critical input for exports. Having a high-cost, uncompetitive service sector can act as a drag on trade. Therefore, service sector reforms to increase competition can be helpful in getting more benefit from a trade opening. In the case of

India in the 1990s, [Arnold, et al. \(2016\)](#) find that service sector liberalization—in banking, telecommunications, insurance and transport—all had significant positive effects on the productivity of manufacturing firms and increased the value of trade reform. [Arnold et al. \(2011\)](#) reach similar conclusions for the Czech Republic. Finally, rigid labor market institutions that impede labor mobility across regions and sectors can diminish the gains from and intensify the costs of reform, as [Topalova \(2010\)](#) shows in the case of India.

Channels of Impact: Tariffs on Intermediate Goods

[Rodríguez and Rodrik \(2000\)](#) suggested that the search for a general empirical relationship between trade policy and economic growth was “futile.” They were more hopeful that microeconomic evidence could reveal the channels by which trade policy might affect productivity at the industry or firm level. An important body of microeconomic research has made significant progress on this question over the past 20 years.³⁴

The increased availability of firm-level data in developing countries has allowed researchers to understand the economic adjustments that were made in the wake of trade reforms or the opening of export opportunities. This approach does not depend on an aggregate indicator trade policy or the single-year dating of a trade reform. Instead, specific measures, such as import tariffs at the product level, provide a detailed picture of the variation in protection levels across different sectors of the economy and how those barriers changed over time, creating a measure of differential exposure to foreign competition across industries. This tariff variation helps identify the impact of lower import barriers on such outcome variables as sectoral output or labor productivity. Furthermore, there are multiple margins on which these trade policy measures differ across sectors, such as the height of the initial tariffs and the differential speed with which they are reduced.

Like any method, this approach is not without problems. A detailed investigation of one country is required, at the potential cost of external validity. The firm- or industry-level outcome variables, such as productivity, can be difficult to measure. For example, a standard method has been to estimate a production function (using revenue deflated by a price index as a proxy for output) and treat as total factor productivity the difference (residual) between actual and estimated production (based on capital, labor, and material inputs). [De Loecker \(2011\)](#) argues there may be a spurious relationship between this way of measuring productivity and openness to trade, given the impact of policy changes on prices and demand. Fortunately, new methods have improved upon past practices.

Most of this literature focuses on whether lower tariffs (on both intermediate goods and final goods) lead to within-industry efficiency gains as domestic producers face intensified competition.³⁵ A reduction in trade barriers could improve productivity in two ways. First, greater head-on competition forces firms to become more efficient and

reduce their costs to compete in the same market. Second, increased trade in intermediate goods gives domestic purchasing firms access to a wider array of less expensive intermediate goods that they can use to produce final goods. The increased availability of intermediate goods can affect final goods producers through lower prices, increased quality, and increased variety of inputs, all of which improve efficiency. The improved productivity of final goods producers as a result of the reduction in tariffs on intermediate goods has been a primary focus of this work.³⁶

Table 2 summarizes a few of the studies in this area. A pioneering study by Pavcnik (2002) examines the response of Chilean manufacturing to increased competition in 1979–86 to document the productivity effects arising from a reduction in import barriers. She compares sectors facing liberalized trade (import-competing and export-oriented sectors) to the nontraded-goods sector in order to distinguish the productivity effects stemming from more open trade from other sources. She finds that the productivity of plants in import-competing sectors grew 3–10 percent faster than in nontraded-goods sectors. Greater competition was presumably the source of these productivity gains, although much of the increase came from the forced exit of existing plants, which were about 8 percent less productive on average than the plants that continued to produce.

In a similar vein, Fernandes (2007) examines a period of substantial variation in protection levels across manufacturing industries in Colombia (1977–91) to see whether increased exposure to foreign competition generated productivity gains. She finds a strong positive impact of tariff liberalization on plant productivity, even after controlling for plant and industry heterogeneity, real exchange rates, and cyclical effects. Productivity improvements under trade liberalization are linked to increases in imports of intermediate inputs, skill intensity, and machinery investments and to reallocations of output from less to more productive plants. The impact was stronger for plants in industries initially facing less competition.

Brazil has been another country of focus, with some divergent results using firm-level data.³⁷ Muendler (2004) finds evidence that the pressure of foreign competition raised manufacturing productivity markedly but that the use of foreign inputs played only a minor role in productivity change. Schor (2004) reports that increased exposure to competition led to productivity improvements but that access to new inputs embodying better foreign technology also contributed to productivity gains.

More refined data on the use of imported intermediate goods have allowed researchers to trace that channel more clearly. Amiti and Konings (2007) use plant-level data on imported inputs from Indonesia during 1991–2001. They find that the largest productivity gains arise from lower input tariffs: A 10 percentage point reduction in input tariffs led to a 12 percent productivity gain for importing firms; the productivity gain associated with a 10 percentage point reduction in output tariffs was 1–6 percent. Although it is not possible to determine the channel that gives rise to this higher productivity, the large impact for importing firms compared with nonimporting firms sug-

Table 2. Selected Studies of Trade Reform and Industry Productivity

Country/study	Period	Measure of productivity	Main finding
Chile			
Pavcnik (2002)	1979–86	Plant total factor productivity (TFP) (revenue)	Productivity of import-competing sector 3–10% higher than the productivity of nontraded goods.
Kasahara and Rodrigue (2008)	1979–96	Plant TFP (revenue)	Imported intermediates boosted productivity 2.6%.
Garcia-Marin and Voigtländer (2019)	1996–2007	Plant TFP (quantity)	5% increase in productivity within plants after tariff-induced export exposure; marginal costs lower by 15–25%
Colombia			
Fernandes (2007)	1977–91	Plant TFP (revenue)	A 10 percentage point reduction in tariffs increased TFP by 0.8–1.2% in affected industries.
Indonesia			
Amiti and Konings (2007)	1991–2001	Plant TFP (revenue)	A 10 percentage point reduction in input tariffs was associated with a 12% increase in TFP.
India			
Topalova and Khandelwal (2011)	1987–2001	Plant TFP (revenue)	A reduction in final goods tariffs increased TFP by 1.7%. A reduction in input tariffs increased it by 10.6%.
Goldberg et al. (2010)	1987–97	Plant TFP (revenue)	A reduction in input tariffs increased product scope; new imported varieties reduced the price index for intermediate goods by 4.7% a year.
Ecuador			
Bas and Paunov (2021)	1997–2007	Firm TFP (revenue and quantity)	8-percentage point reduction of output tariffs associated with an increase in 10–17% increase in TFP; 7-percentage-point reduction of input tariffs associated with a 6–9%
China			
Brandt et al. (2017)	1994–2007	Plant TFP (revenue)	A reduction in final goods tariffs reduced markups (procompetitive effect); a 1 percentage point reduction in input tariffs reduced output prices by 1.6%.
Amiti et al. (2020)	1998–2007	Firm TFP (revenue)	A reduction in input tariffs increased firm TFP.
<i>Source: Author's compilation.</i>			

gests that direct benefits may accrue from the technology embodied in the imported inputs.³⁸

Using data from Chile, [Kasahara and Rodrigue \(2008\)](#) determine that a plant can immediately improve productivity by switching from being a nonimporter to being an importer of foreign intermediate goods. Although the point estimates differ across estimators, even the within-group estimate, which they suspect is downward biased, indicates a 2.6 percent positive productivity effect from importing. They also find some evidence of a positive dynamic effect from the use of imported materials.

Several detailed studies look at India's trade reforms in the 1990s. [Topalova and Khandelwal \(2011\)](#) use the variation in liberalization across sectors to evaluate the effect of reforms on firm-level productivity. The availability of cheaper inputs was a more significant driver of productivity than increased final goods competition. For the tariff reductions undertaken between 1989 and 1996, the cuts in final goods tariffs accounted for a 1.7 percent increase in productivity, and the cuts in intermediate goods tariffs accounted for a 10.6 percent increase in productivity. Firms in heavily regulated industries did not enjoy productivity benefits, either because they did not respond to competitive pressure or because they lacked the freedom to adjust their production technology after the reforms.

[Goldberg et al. \(2010\)](#) link the decline in India's input tariffs to an expansion in a downstream firm's product scope, finding that industries experiencing the greatest decline in input tariffs introduced more new products. Lower input tariffs accounted for a third of the observed increase in firms' product scope. They also improved firm performance, measured by output, total factor productivity, or research and development spending. The authors separate changes into a "price" and a "variety" channel and find substantial gains from access to new varieties of imported inputs. Accounting for new imported varieties lowers the import price index for intermediate goods by an additional 4.7 percent a year beyond the conventional gains through lower prices of existing imports. The availability of more varieties relaxed the technological constraints facing producers, who were able to source new and better inputs that were not available before liberalization.

Other papers focus on China, another big reforming country. [Brandt et al. \(2017\)](#) examine China's tariff reductions as it sought entry into the World Trade Organization (WTO) in the mid-to-late 1990s. They find that lower tariffs increased competition in the industry directly facing foreign competition. Their point estimate of this effect is similar to that for India: Each percentage point reduction in output tariffs lowered markups by 0.10–0.15 log points. In particular, state-owned firms were forced to improve their efficiency (and reduce markups) to avoid bankruptcy when faced with increased competition. The pro-competitive effects were most important among incumbents. New entrants were poised to achieve greater efficiency gains. [Brandt et al.](#) show that lower input tariffs increased efficiency in downstream purchasing indus-

tries, with a 1 percentage point lower tariffs on inputs reducing output prices by 1.6 percent.

Lower input tariffs have also been found to reduce the costs of domestic producers in a way that increases exports. Using detailed firm-level data from Argentina, [Bas \(2012\)](#) finds that the probability of entering the export market was higher for firms producing in industries that experienced greater input tariff reductions. Looking at cross-industry variation in tariff reductions, [Amiti et al. \(2020\)](#) find that lower input tariffs reduced the costs to purchasers, boosting Chinese firms' productivity and their exports.

Finally, there is an indirect channel from import liberalization to export growth, and from exports to productivity. Economists have long questioned whether more efficient firms select to enter export markets, or whether firms learn from exporting. [Garcia-Marin and Voigtländer \(2019\)](#) argue that identifying export-related efficiency gains within plants is difficult because commonly used revenue-based productivity measures are downward biased as more efficient producers tend to charge lower prices. Using better productivity data for Chilean, Colombian, and Mexican manufacturing plants, they find sizable efficiency gains after export entry (induced by lower tariffs) owing to a complementarity between exporting and investment in technology. Evidence from Slovenia ([De Loecker 2013](#)), Spain ([Manjón, Máñez, and Rochina-Barrachina 2013](#)), Indonesia ([Blalock and Gertler 2004](#)), and other countries points to learning by exporting as a modest but potentially important source of productivity gains.

In sum, while reducing tariffs on final goods increases the pressure of competition on domestic producers to improve their productivity, a growing body of evidence points to the benefits of reducing tariffs on intermediate goods and other inputs. Domestic producers benefit from lower prices, improved quality, and greater variety of imported inputs, all of which help them improve their productivity performance.³⁹

Unfortunately, this literature has not succeeded in providing an aggregate or economy-wide measure of the productivity improvement resulting from this channel. While improved productivity performance in manufacturing is an important driver of overall productivity in many developing countries, the contribution of trade openness at the aggregate level has yet to be established. There is still debate about the role of trade in expanding or shrinking the size of the manufacturing sector in developing countries. The benefits of achieving greater productivity in labor-abundant countries that export manufactured goods have been readily apparent. In contrast, for developing countries that export natural resources or agricultural goods, an increase in openness to trade could shrink the overall size of the manufacturing sector. If it does, aggregate productivity growth could fall, because improvements in firm-level productivity would be offset by a decline in the size of the manufacturing sector.

Qualitative and Quantitative Country Studies

A perennial problem with empirical evidence, particularly across countries, is that questions can always be raised about the quality of the data, the method of analysis, the robustness of the results, and so forth. [Srinivasan and Bhagwati \(2001\)](#) go so far as to reject all cross-country regression methods, arguing that the most compelling evidence on the consequences of trade reform only come from careful case studies of policy regimes in different countries.

There have been several multivolume country studies of trade reform and the lessons to be derived therefrom. In the late 1960s, the Organisation for Economic Cooperation and Development (OECD) sponsored a series of books on trade policy and industrialization in selected developing countries.⁴⁰ In the early 1970s, the National Bureau of Economic Research (NBER) published 10 country volumes on foreign trade regimes and economic development, along with two summary volumes, by [Bhagwati \(1978\)](#) and [Krueger \(1978\)](#). Both the OECD and NBER studies documented the costs of inward-oriented, import substitution trade regimes and the benefits of outward-oriented, export promotion policies.⁴¹

In 1991 the World Bank published the seven-volume series *Liberalizing Foreign Trade*, which covers 36 liberalization episodes in 19 countries over 1950–84 (the only reformers from the 1980s were Turkey and New Zealand). The overview volume, by [Papageorgiou, Choksi, and Michaely \(1990, 41\)](#) confidently reported that “[trade] reform can work anywhere, regardless of initially unfavorable circumstances, and without serious short-term drawbacks. Governments with highly distorted trade regimes need not fear the consequences of a well-designed liberalization program.”

This massive effort provided an abundance of informative detail on different reform episodes. But such sweeping conclusions drew criticism. [Greenaway \(1993\)](#) argued that “the conviction with which the conclusions are reported is misleading” and stated that the narrative approach led to “impressionistic” conclusions about the consequences of various trade policies, because it was hard to disentangle the impact of trade policies from other policies pursued at the same time. [Collier \(1993\)](#) lamented the lack of any analytical framework guiding the World Bank effort and feared that the sample of countries chosen was small and not random. He also worried that “there seem to be few simple empirical regularities” because every country seemed to have its own path and face its own difficulties, making broad generalizations difficult.

The qualitative nature of the country-study approach has often provided useful information, but it has never been entirely satisfactory to most economists. This approach seems to have fallen out of favor, as there has been no major attempt to provide a complete record of reforms undertaken during the late 1980s and early 1990s.⁴²

Of greater interest to economists are quantitative country studies based on detailed structural models. These models can be built to incorporate industry dynamics (such as entry decisions and capital accumulation), calibrated to match country data for a

pre-reform year, and then used to simulate counterfactual scenarios in which particular policies are changed to examine their impact on various economic outcomes. These are not the older computable general equilibrium (CGE) models that used to yield small welfare gains from simulations of tariff reductions, usually in a static setting with perfectly competitive sectors of the economy (Srinivasan and Whalley 1986; Dervis, De Melo and Robinson 1982). There are only a few examples of such new dynamic models, partly because they require a relatively large investment and yield information for only one country.

An example of this type of research is the Connolly and Yi (2015) study of the Republic of Korea. Korea moved from economic isolation (caused in part by heavy protection) to export promotion in the mid-1960s. Exports as a share of GDP rose from 2 percent in 1962 to 30 percent in less than two decades, and its growth experience has been miraculous. But the Korean case is also controversial because there is a debate about the contribution of trade policy to its growth performance and whether industrial policy and other factors played a significant role.⁴³

To help untangle the role of trade policy, Connolly and Yi develop a two-sector neoclassical growth model, calibrated with Korean data, to simulate the trade policy changes that occurred between early 1962 and 1989. The model can explain almost all of Korea's trade growth and most of its increase in imports of investment goods. They then examine three distinct trade policy changes: (a) the elimination of tariffs on intermediate goods and capital equipment for goods produced for export in the mid-1960s, (b) the reduction in general tariffs from about 40 percent in early 1970s to about 13 percent by 1989, and (c) the reduction in foreign tariffs on Korean exports as a result of the Kennedy and Tokyo Rounds of the General Agreement on Tariffs and Trade (GATT) in the 1960s and 1970s. Taking these changes together, their model suggests that these tariff reductions can explain 17 percent of Korea's catch-up to advanced countries in value added per worker in the manufacturing sector. Although more than 80 percent of the catch-up is left unexplained, trade policies alone account for a sizable share of the productivity improvement. Trade reforms also account for a 19 percent increase in consumption per worker (their measure of welfare).

These efficiency and welfare gains are driven by two channels: multistage production and imported investment goods. Connolly and Yi argue that the results from their neoclassical growth model constitute a lower bound on the contribution of trade policy reform to Korea's growth miracle, because it does not address other mechanisms by which trade could affect growth, such as learning or technological spillovers that were enhanced through exporting and importing activities or the impact on human capital formation. The model also does not allow them to examine factors such as industrial policies and credit subsidies.

Alessandria and Avila-Montealegre (2023) examine Colombia's unilateral tariff reforms in 1989–93 in the context of a quantitative trade model. They use a multi-sector, heterogeneous firm set up with a dynamic exporting decision and input-output link-

ages, calibrated with Colombian data. They find that the reforms boosted welfare by nearly 5 percent and increased long-run consumption by almost 6 percent. The bulk of the gains come from lower tariffs on capital goods and equipment. They emphasize the critical importance of modeling choices: a collapsed two-sector, static version of yields much lower gains, whereas accounting for the dynamics of exporter participation and other factors are key to their larger results.

While quantitative models have become increasingly popular, the results depend crucially on modeling assumptions and tend to be focused on a single country. External validity is always a question: What happened in Korea or Colombia might not be expected to happen in Cameroon or Laos. Unless other economists develop such models for other countries, there will still be uncertainty about how trade reform affects economic structure and performance in different contexts. Nevertheless, the detailed approach in the setting of one country helps separate the contributions of particular reforms to economic outcomes and is a useful alternative to a simple indicator variable in a multicountry regression.

Conclusion

Economists have been interested in the relationship between trade policy and economic growth since the time of Adam Smith. The trade reform wave of the late 1980s and early 1990s provides new historical evidence on the matter. There is no one ideal empirical method that can provide decisive evidence on this question, so researchers have tried a variety of different approaches to understand the relationship.

The findings from recent research, however, have been remarkably consistent. For developing countries that are behind the technological frontier and have significant import restrictions, there appears to be a measurable economic payoff from more liberal trade policies. As [Table 1](#) reports, a variety of studies using different measures of policy have found that economic growth is roughly 1.0–1.5 percentage points higher for countries that undertake trade reforms. Several studies suggest that this gain cumulated to about 10–20 percent higher income after a decade. The effect is heterogeneous across countries, because countries differ in the extent of their reforms and the context in which reform took place. Understanding that heterogeneity, which is sometimes attributed to labor market rigidities, financial frictions, or service-sector inputs, merits further research.

At a microeconomic level, the gains in industry productivity from reducing tariffs on imported intermediate goods are even more sharply identified. They show up time and again in country after country.

Some questions remain about how much of the economic growth following trade reform can be attributed to trade policy changes alone, as other market reforms are sometimes adopted at the same time. Even if the reduction of trade barriers accounts for only a part of the observed increase in growth, however, the cumulative gains from

reform appear to be substantial. As [Estevadeordal and Taylor \(2013\)](#) ask, “Is there any other single policy prescription of the past twenty years that can be argued to have contributed between 15 percent and 20 percent to developing country income?”

Conflict of Interest

None declared.

Data Availability Statement

Data available from the author upon request.

Notes

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1. In a textual analysis of IMF Article IV reports, [Cherif, Engher, and Hasanov \(2020\)](#) show that discussion of the terms stabilization, liberalization, and privatization (i.e., the “Washington Consensus”) started rising around 1983 and fell off after 1997.

2. [Edwards \(1993\)](#) was equally critical of the research undertaken in the 1970s and 1980s, much of which consisted of country case studies and simple regressions linking exports to growth. [Easterly \(2001\)](#) reported that there did not seem to be any observable payoff to reforms undertaken in the 1980s.

3. Standard surveys, such as the *Handbook of International Economics* ([Atkin and Davidson 2022](#)) or the *Handbook of Commercial Policy* ([Bagwell and Staiger 2016](#)), do not focus on the growth experiences of developing countries stemming from their trade reform experiences.

4. [Winters and Masters \(2013\)](#) provide a shorter, less comprehensive review of some of this literature. Other surveys, such as [Baldwin \(2002\)](#) and [Winters \(2004\)](#), focus more on the relationship between “openness” and growth rather than the specific question of “changes in trade policy” and growth.

5. Several OECD countries took significant steps to open further to trade in the 1980s. Australia and New Zealand began scaling back trade protection, and several countries, such as Spain, had to open their markets to more competition when they joined the European Union. On the payoff from these and other structural reforms, see the survey by [Campos, De Grauwe, and Ji \(2024\)](#), as well as [Prati, Ornorato, and Papageorgiou \(2013\)](#); [Marrazzo and Terzi \(2017\)](#); and [Campos, Coricelli, and Moretti \(2019\)](#).

6. Similarly, [Easterly \(2019\)](#) has noted that—in contrast to his earlier work ([Easterly 2001](#))—policy reforms in developing countries did eventually produce better outcomes. In fact, in a remarkable development, economic growth in the developing world began to pick up enough in the mid-1990s that many countries began converging to higher income countries ([Kramer, Willis, and You 2021](#); [Patel, Sandefur, and Subramanian 2021](#)).

7. Rodríguez and Rodrik argued that the Frankel and Romer result is not robust to controlling for omitted variables, such as institutions or distance from the Equator. [Feyrer \(2019\)](#) overcame this problem by generating a time-varying geographic instrument (based on air versus sea travel). This time-series variation allows controlling for country fixed effects, eliminating the bias from time-invariant variables such as historically determined institutions or distance from the Equator. Trade has a significant effect on income, with an elasticity of roughly one-half (meaning that a 10 percent increase in trade leads to a 5 percent increase in national income). For the period 1870-1913, [Pascali \(2017\)](#) finds that only countries

with inclusive institutions benefited from increased economic integration. Other studies assess the gains from increased market access, such as lower transportation costs. Donaldson (2015) surveys this literature, most of which focuses on domestic regional gains from the extension of railroads in India and the United States, the expansion of the highway network in China and the United States, and similar episodes.

8. Yanikkaya (2003) finds a positive relationship between a country's average tariff and its economic growth for a cross-section of countries for 1970–97. DeJong and Ripoll (2006) look at the same relationship in greater detail. They find a negative relationship for high-income countries and a positive relationship for low-income countries. Nunn and Trefler (2010) document a positive correlation between the skill bias of a country's tariff structure and its long-term growth in per capita income. Whatever the case, by using the level of tariffs rather than the change in tariffs, these studies are not examining within-country growth as a result of trade reform episodes.

9. The impact of a foreign market opening on an exporting country, such as the impact of the United States granting market access to Vietnam (McCaig 2011), is also not addressed.

10. Artuc, Porto, and Rijkers (2019) study of the tradeoff between the aggregate income gains from trade liberalization and the costs of increased inequality is worth noting. They calculate the *static* gains from trade for many countries, look at how it affects household income, and assess the outcome using an Atkinson social welfare function. They find average income gains for 45 countries and average losses for 9 countries, with the static gains amounting to 1.9 percent of average household expenditures (with no accounting for any growth benefits from freer trade). Inequality rises in most countries as a result of trade liberalization, but the income gains typically more than offset the losses from increased inequality as reflected in the social welfare function.

11. As Balassa (1971, xv) put it, “The policy of import substitution followed by most developing countries since World War II has had the twin objectives of improving the balance of payments position of these countries and providing for the development of their manufacturing industries. . . . Whatever the intrinsic merits of this policy, its application has rarely been based on a consistent program of action. Rather, the existing system of protection in many developing countries can be described as the historical result of actions taken at different times and for different reasons. These actions have been in response to the particular circumstances of the situation and have often been conditioned by the demands of special interest groups.”

12. As Finger, Ingco, and Reincke (1996) document, most developing countries did not reduce their applied tariffs as a result of the Uruguay Round. Their concessions amounted to reductions in bound tariffs, which were substantially above their applied tariffs.

13. See Edwards (1997), Krueger and Rajapatirana (2003), and Jinjarak, Salinas, and Tsikata (2013) on World Bank support for trade liberalization and Wei and Zhang (2010) for evidence on IMF support for trade liberalization. For a general assessment, see Irwin (2023).

14. However, he added, “All this does not mean that trade liberalization is not a good idea. It almost certainly is. Nor does it necessarily mean that the modest conventional estimates of the gains from such liberalization tell the whole story.”

15. As Mill (1844, 148) noted, “How, for example, can we obtain a crucial experiment on the effect of a restrictive commercial policy upon national wealth? We must find two nations alike in every other respect, or at least possessed, in a degree exactly equal, of everything which conduces to national opulence, and adopting exactly the same policy in all their other affairs, but differing in this only, that one of them adopts a system of commercial restrictions, and the other adopts free trade. This would be a decisive experiment, similar to those which we can almost always obtain in experimental physics. Doubtless this would be the most conclusive evidence of all if we could get it. But let anyone consider how infinitely numerous and various are the circumstances which either directly or indirectly do or may influence the amount of the national wealth, and then ask himself what are the probabilities that in the longest revolution of ages two nations will be found, which agree, and can be shown to agree, in all those circumstances except one?”

16. Pritchett (1996) shows how various measures of trade policy—tariffs, nontariff barriers, coverage ratios, black-market premiums on foreign exchange markets—are essentially uncorrelated with one another.

17. The question is whether real per capita GDP is the appropriate measure for the impact of a trade reform. In theory, a tariff reduction should lead to an increase in welfare or real consumption, not necessarily real GDP, which might actually decline, if the changing production mix is evaluated at pre-reform prices (Burstein and Cravino 2015).

18. For example, a country that goes from complete autarky to complete free trade would be represented by the same transition from 0 to 1 as a country that goes from 41 percent tariffs (and other barriers) to 39 percent tariffs (and other barriers).

19. Sachs and Warner used the average tariff on intermediate goods (gathered from the mid-1980s, the middle of the sample period) collected by the United Nations Conference on Trade and Development (UNCTAD).

20. In response, Warner (2003) points out that Africa would be categorized as closed even without the state monopoly variable. He further argues that the black-market premium does not simply represent macroeconomic distortions, as it shows no correlation with inflation. For a counter response, see Rodríguez (2007).

21. Wei and Zhang (2007) find that a 1 standard deviation increase in foreign exchange controls has the same effect on trade as a 14- percentage point increase in tariffs. Eichengreen and Irwin (2010) show that during the Great Depression of the 1930s, imports were 23 percent lower in countries imposing exchange controls than in comparable non-control countries, after controlling for GDP.

22. Not just establishing but maintaining a realistic exchange rate is usually critical for the survival of the trade reform.

23. These results are not driven by two of the largest countries that opened up, China and India, both of which the SWWW indicator still classified as “closed.”

24. Kneller, Morgan, and Kanchanahatakij (2008) explicitly identify some of the big winners and losers from these liberalization episodes, meaning improvement in growth experience before and after reform. Some of the biggest winners were Nicaragua, Guyana, and Mozambique; losers included Guinea-Bissau, the Gambia, and Cape Verde. They do not successfully explain why some countries did well and others did not.

25. Several other regression-based studies deserve brief mention. Based on panel regression, Greenaway, Morgan, and Wright (1998) conclude that 32 countries that reformed after 1985 (identified by Dean, Desai, and Riedel [DDR] 1994) did not have stronger growth than other countries. Greenaway, Morgan, and Wright (2002) published a similar exercise, using a panel of 69 developing countries with several binary indicators (SWWW, DDR, World Bank). All of the indicators had a qualitatively similar result: Reforming countries experienced 2.7 percentage point faster growth, with a two-year lag, than other countries. Using similar methods, Falvey, Foster, and Greenaway (2012) look at whether trade reform that occurs during a crisis (internal versus external) affects its subsequent growth impact. They do not find that a crisis matters much: Liberalization in both crisis and noncrisis periods raised subsequent growth. Falvey, Foster-McGregor, and Khalid (2013) look at the dynamic adjustment of various variables to trade reforms over the period 1970–2005. They find that the impact on the trade share appears after four years and the impact on investment is strongest after seven years.

26. However, the dating is very similar to the SWWW dates. Of the 39 reforming countries, the two measures coincide for 25 country years, with most divergences less than three years.

27. They exclude from the sample transition economies, oil exporters, and small countries (countries with fewer than 1 million people), as well as countries with internal conflict. They also rule out years of crises (such as hyperinflation), which they argue artificially reduces the growth rate. After culling, their sample includes 39 countries with non-reversed trade liberalization between 1970 and 2004.

28. Their basic results survive several placebo checks (such as using 1960–75 as the pretreatment period) and the endogeneity of the treatment (the decision to liberalize). Checking to see if the findings are confounded by other policy reforms, they find that the higher growth “cannot be attributed to favorable changes in financial openness and macroeconomic policies.”

29. They define a growth acceleration as meeting three requirements: Growth must be at least 3.5 percent over an eight-year period, growth must be at least 2 percentage points higher than in the previous

eight years, and the level of real GDP must be higher at the end of the acceleration than in all years before it.

30. For example, they point out that the Hausmann, Pritchett, and Rodrik method picks up 27 growth accelerations in which the country experienced higher growth in the year before the start of the acceleration.

31. This result holds not just for the SWWW indicator, but also for the Economic Freedom indicator of [Gwartney and Lawson \(2008\)](#). The authors also argue that Hausmann, Pritchett, and Rodrik introduced a coding error in the political regime variable that led to the erroneous conclusion that political regime changes but not economic policy changes lead to growth accelerations. Using another definition of growth acceleration, [Peruzzi and Terzi \(2018\)](#) identify 135 episodes between 1962 and 2002. Using the [Giuliano, Mishra, and Spilimbergo \(2013\)](#) measure of structural reforms (trade, product market, financial sector, etc.), they find that almost 60 percent of growth accelerations were preceded by large changes in economic policy.

32. That said, Indonesia's economic performance improved dramatically after 1966, when increased growth could not be attributed to high oil prices. According to the SWWW dating method, Indonesia becomes "open" in 1970, although the reform process began in 1966.

33. One could also include internal trade barriers (as in India) as an obstacle to reaching full potential with external trade reform ([Leemput 2021](#)).

34. For a recent survey, see [De Loecker and Van Biesebroeck \(2018\)](#).

35. Several papers document the consumer gains from lower tariffs on final goods. [Tovar \(2012\)](#) examines the reduction in Colombia's tariff on automobiles. He finds consumer gains of about \$3,000 per vehicle. [Sheu \(2014\)](#) studies the gradual elimination of the 20 percent tariff on imported printers in India, which resulted in a large increase in imports. She disentangles three factors—lower prices, higher quality, and greater variety—all of which could boost consumer welfare. She finds that the higher quality of imports was the most important factor in generating gains from trade. The contribution of price was slightly smaller; variety lagged farther behind. The strength of these effects varied across buyers, with gains largest for small businesses.

36. [Feenstra, Markusen, and Zeile \(1992\)](#) discuss the theory that access to new intermediate goods and cheaper existing goods can lead to higher productivity in the production of final goods. Most research is unable to directly measure the adoption of new technology as a result of trade. One exception is [Bustos \(2011\)](#), who studies the impact of Brazilian tariff reductions on Argentine exporting firms. She finds that Argentine firms in industries facing greater reductions in Brazil's tariffs increased investment in technology faster than firms facing smaller tariff reductions. This situation is different from looking at the impact of changes in Argentinian trade policy on firms in Argentina; it looks at export market access rather than the effect of changing a country's own import policy.

37. At the industry level, [Ferreira and Rossi \(2003\)](#) estimate that Brazil's early liberalization, in 1988–90, led to a 6 percent increase in the growth rate of total factor productivity and had a similar impact on labor productivity.

38. Similar effects have been found in more advanced economies. [Halpern, Koren, and Szeidl \(2015\)](#) investigate the effect of imported inputs on productivity using microdata from importers in Hungary. They find that importing all input varieties would increase a firm's revenue productivity by 22 percent, with about half of the increase arising from imperfect substitution between foreign and domestic inputs. They attribute one-quarter of Hungary's productivity growth over the 1993–2002 period to increased imports of intermediate goods.

39. Other studies have focused on the positive productivity effects emanating from trade reforms in Mexico ([Luong 2011](#)), Vietnam ([Ha 2015](#)), and Uruguay ([Casacuberta and Zaclicever 2016](#)). The results are broadly similar.

40. The two most famous volumes to emerge from that effort were Bhagwati and Desai's book *India: Planning for Industrialization* and Little, Scitovsky, and Scott's summary volume *Industry and Trade in Some Developing Countries*, both published in 1970. See [Bhagwati and Desai \(1970\)](#), [Little, Scitovsky, and Scott \(1970\)](#).

41. At the World Bank, [Balassa \(1982, 57\)](#) also looked at country case studies of the early reformers in the 1960s and 1970s. He concluded that “trade orientation has been an important factor contributing to intercountry differences in the growth of income.”

42. One incomplete effort, focused more on the political economy of the reforms than their economic consequences, is [Williamson \(1994\)](#).

43. For example, [Westphal \(1990\)](#) argues that “import liberalization was not an important element of the reforms that put Korea on the path of export-led development.” It is unclear whether his statement included the liberalization of intermediate goods or referred only to liberalization of final goods. The latter is certainly true in the 1960s and 1970s; the former would certainly be contested.

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