IS THE WASHINGTON CONSENSUS DEAD? GROWTH, OPENNESS, AND THE GREAT LIBERALIZATION, 1970S–2000S

Antoni Estevadeordal and Alan M. Taylor*

Abstract—According to the Washington Consensus, developing countries' growth would benefit from reductions in barriers to trade. However, the empirical basis for judging trade reforms is weak. Econometrics are mostly ad hoc, results are typically not judged against models, policies are poorly measured, and most studies are based on pre-1990 experience. We address these concerns by employing a model with capital and intermediate goods, compiling new disaggregated tariff measures, and employing treatment and control regression analysis with differences-in-differences. We find that a specific treatment, liberalizing tariffs on imported capital and intermediate goods, led to faster growth, consistent with the model.

Trade liberalization and tariff reforms have provided increased access to Indian companies to the best inputs available globally at almost world prices.

— Rakesh Mohan, managing director,
Reserve Bank of India, 2008[1]

I. Introduction

DOES trade policy liberalization promote economic growth? The question has been central to economic policy debates since the dawn of the new era of globalization in the 1990s. Yet the opinions of economists, once quite coherent, are now far from unanimous.

In the 1990s the so-called Washington Consensus (WC) promoted openness to trade as an essential policy reform to promote growth and higher incomes.[2] At first, absent statistical evidence, this view garnered support as practitioners looked back on the divergent economic fortunes of the fast-growing export-oriented new industrializing countries (NICs) of East Asia and the sluggish inward-looking economies of Latin America. Subsequently, a barrage of cross-country econometric studies seemed to lend weight to this view, including widely cited works by Dollar (1992), Sachs and Warner (1995), Edwards (1998), and Frankel and Romer (1999). It was in this intellectual climate that trade barriers fell in many developing countries in what we might call the Great Liberalization of the 1990s, coinciding with the GATT Uruguay Round.

A decade later, that erstwhile consensus had evaporated. A survey of the World Bank's self-review, *Learning from Reform* by Rodrik (2006), was entitled, "Goodbye Washington Consensus, Hello Washington Confusion." If there is a new consensus, it often seems the focus has shifted from getting policies right (the "policy view") to getting institutions right (the "institutions view"), although the practical implications of that shift are quite unclear (Easterly, 2005).[3]

In the research arena, the openness-growth linkage has also attracted a vast amount of attention, and the reversal of sentiment has been dramatic here too. Academic research has played no small part in this counterrevolution. Rodríguez and Rodrik (2001) replicated and extended the above heavily cited works from the 1990s to assess their robustness—and found them wanting. Theirs is now another widely referenced work that lends weight to an increasingly prevalent view that trade policies may have very little to do with economic performance.[4]

Moreover, in later work by Easterly, Rodrik, and others, institutions have been proposed as the "deeper determinants" that have been said to trump other factors such as trade policies (Easterly & Levine, 2003; Rodrik, Subramanian, & Trebbi, 2004; Easterly, 2005).

Yet for the debate about growth and trade policy, the reality is that the jury is still out. As Rodríguez and Rodrik (2001) noted, cross-section empirical work to date has depended on dubious and noisy data. Confidence intervals were generally large and not far from zero. It was therefore easy to find results that would disappear under alternative assumptions or with different controls. But imprecision does not mean that there is no effect, only that we have not

[1] Quoted in Goldberg et al. (2010).

[2] In fairness to Williamson (1990), his Washington Consensus recommendations were a broad and coherent package of ten reforms, of which trade reforms were just a part. We say "so-called" because the term *Washington Consensus* soon took on a life of its own. The ten reforms were summarized by Rodrik (2006) as fiscal discipline, reorientation of public expenditures, tax reform, financial liberalization, unified and competitive exchange rates, trade liberalization, openness to FDI, privatization, deregulation, and secure property rights.

[3] It would be a mistake to equate trade liberalization with the entire Washington Consensus; but of the ten reforms in the original package, trade policy seems to have attracted the most attention. Why? On trade policy, the political stakes appear high: protests and riots accompany WTO meetings, but there is not such violent agitation over other issues in the consensus package, and the other elements are mostly uncontroversial—for example, nobody is now arguing for fiscal indiscipline or insecure property rights.

[4] Rodríguez and Rodrik focus on the papers by Sachs and Warner, Frankel and Romer, and Edwards and Dollar because these papers were most influential in terms of impact. For example, the citation counts as of January 2013 were: Sachs and Warner, 4,354; Frankel and Romer, 3,505; Edwards, 1,754; Dollar, 1,939. Rodríguez and Rodrik had 2,984 citations, making theirs the dominant paper with the opposing viewpoint. Citation counts from scholar.google.com.

been able to model and measure precisely enough to answer the question. Thus the tariff growth literature has reached an apparent impasse. Unclear results mean that neither side can really claim victory, so arguments over the merits of a key element of the Washington Consensus are still unresolved.

Can we make any further progress? We think so, if only because so much time has now passed since the first round of empirical studies. As the academic and policy community now judges the Washington Consensus, one troubling aspect of the most cited papers in the literature on openness and growth, both pro and con, is that they all base their tests on data drawn from periods before 1990: before the Washington Consensus had even emerged, and before the Great Liberalization in trade policy in the 1990s had taken root. This problem affects the data sets in Frankel and Romer (spanning 1960–1985), Edwards (1960–1989), Sachs and Warner (1970–1989), and Dollar (1970–1989), as well as all of the robustness checks of these studies in Rodríguez and Rodrik (2001). But now, a decade after this last generation of studies, we ought to be in a position to judge the effects of the experiment that was the Washington Consensus as it has run from 1990 onward. And we ought to do so using sounder tests and harder data.[5]

In this paper, we do just that: we document the Great Liberalization experiment and, viewing it as a "treatment," study its correlation with before-and-after growth outcomes in two windows, 1975–1989 and 1990–2004. For skeptics, some studies, such as those by Rodríguez and Rodrik (2001) and Easterly (2005), have set a higher bar for empirical work, a bar that we now endeavor to surmount. We agree with them that the data and the techniques used by all sides in the debate thus far have been inadequate in a number ways, most notably:

- The data were scant in terms of spatial and temporal coverage; the latter encouraged OLS cross-section estimation, with associated omitted-variable and endogeneity problems.
- The trade policy measures were subject to measurement error or subjective bias.
- They were often endogenous "outcome" measures (like trade volume) that did not correspond to any policy instrument directly controlled by governments.
- They did not correspond to any trade policy measure that would be suggested by theory as having a causal impact on growth.
- They were not robustly correlated with growth and seemed to be trumped by institutions.

In this paper we confront these issues and develop an improved methodology and data set for studying the tariff growth question. We pay attention to theory and seek tariff measures that can be disaggregated into consumption, intermediate, and capital goods tariffs. It turns out, as noted by the Indian policymaker Mohan quoted in the epigraph, that both theory and empirics point to important gains resulting from lower input prices. Given concerns about the Sachs-Warner and other binary policy measures, we implement tests using both discrete and continuous measures. We also control for other policies, attend to inference and identification problems, and avoid omitted-variable biases with a difference-in-difference approach, using a treatment-and-control setup to see whether the liberalizers saw accelerated growth relative to nonliberalizers.

Using this policy experiment approach, we work in the time dimension rather than in cross-section. A few previous studies have used similar identification methods ("within" rather than "between") with a variety of trade openness indicators, but some are subject to the Rodríguez-Rodrik critiques, and some of them date from the pre–Washington Consensus era. Harrison (1996) studied only the pre–Washington Consensus period using liberalization proxies for approximately the 1960s to the 1980s, though not all were strict policy measures (for example, trade shares). Slaughter (2001) also studied that era and only the EEC, EFTA, and Kennedy Round liberalizations that primarily affected rich countries in the 1960s and 1970s. The study by Dollar and Kraay (2004) encompasses developing countries and runs through the 1990s, but identifies liberalizers using growth in nominal trade shares, an endogenous variable. As Kraay (2007, p. 139) admits, if one is looking for policy prescriptions, then "one can always object" to findings based on trade volumes rather than trade policies.[6] In recent studies using aggregate policy measures, Lee, Ricci, and Rigobon (2004) find weaker results using average tariff or duty measures than with trade volumes. Wacziarg and Welch (2008) find positive results but use a Sachs-Warner type of indicator.

Our approach is somewhat different. First, starting from theory, we focused on input tariffs, that is, border taxes on capital and intermediate goods. Then we painstakingly collected new and detailed disaggregated tariff data on consumption, capital, and intermediate goods from primary sources, using digital sources for recent years, but very

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[5] Even within the confines of cross-sectional empirical work, the debate has moved on from the work surveyed by Rodríguez and Rodrik (2001). More recent papers arguing for a positive effect of trade on growth include Alcalá and Ciccone (2004), Noguer and Siscart (2005), and Warner (2003). In turn, the skeptics have made further responses; see Rodríguez (2007).

[6] Dollar and Kraay (2004) do find that countries that experienced more rapid trade share growth also experienced sharper tariff declines. But as Rodrik points out in his October 2000 critique of their paper, "The authors combine a policy measure (tariff averages) with an outcome (import/GDP) measure in selecting countries. This is conceptually inappropriate, as policy makers do not directly control the level of trade. Saying that 'participation in world trade is good for a country' is as meaningful as saying that 'upgrading technological capabilities is good for growth' (and equally helpful to policy makers). The tools at the disposal of governments are tariff and non-tariff barriers, not import or export levels." See http://ksghome.harvard.edu/~drodrik/Rodrik%20on%20 Dollar-Kraay.pdf.

cumbersome and hitherto unused archival sources for the 1980s. Based on a difference-in-difference design, we then *do* find a significant correlation between tariff reductions and growth acceleration, one that is strong for tariffs on capital and intermediate goods and much weaker for consumption tariffs. The estimated impacts are large, but not too large—about 1 percentage point per year higher growth for liberalizers. The impacts are plausible and mesh with the predictions of our calibrated model.

Thus, one might say that our work is closer in spirit to two other important papers, written contemporaneously, which examine the country-specific growth impacts of disaggregated industry-level input-tariff changes—one for Indonesia (Amiti & Konings, 2007) and one for India (Goldberg et al., 2010). The study by Broda, Greenfield, and Weinstein (2006) sets out to measure the structural trade-growth linkages at the disaggregated level, with a crucial role played by expanding varieties. Our paper can be seen as complementary to these but distinct. Unlike these country studies, we do not have the detailed granularity offered by industry or product-level data. However, our study works at the cross-country level of analysis—for more than forty countries, not just two—providing some valuable external validity that bolsters the evidence from these within-country, cross-sectoral empirical studies.

In asking whether trade reform episodes were followed by increased growth rates, our method is also close in spirit to other contemporaneous work, including the study of growth accelerations by Hausmann, Pritchett, and Rodrik (2005). Our specific focus on differenced estimation and using changes in trade reforms as an acceleration trigger is also echoed in the work of Romalis (2007), though our sample and identification approaches differ. We are able to employ more recent data up to 2004. We also differ in using fifteen-year periods for this analysis, as growth rates over quinquennia are likely to be volatile and beyond the explanatory powers of a medium-run growth model (Easterly et al. 1993). The longer time horizon is also a harsher test: worthwhile policy recommendations need to generate enhanced growth performance over many years, not a flash in the pan. Like these authors, we find that sustained growth accelerations are correlated with economic reforms, although our focus is on trade policy—and a continuous measure, as opposed to the discrete Sachs-Warner indicator variable used by Hausmann et al. (2005).

In the next section, we develop a basic growth model where trade can play a role and use simple calibrations to estimate the plausible magnitude of policy impacts. Drawing on the theory, we then confront the need for more detailed tariff data than have been used to date and describe how we collected and collated these data from primary and secondary sources. In the final main section, we test the theory using the data by applying statistical methods of the treatment control type that avoid many of the problems common to cross-section methods. We also address endogeneity concerns using new arguments, since standard instruments are of no use in this context and changes in trade policy are more strongly correlated with changes in growth than changes in institutions or schooling. Finally, as a corroboration, we show that variations in policy changes across countries correlate with changes in import trends, as predicted by the theory.

II.   Theory and Calibration

Any reasonable model of the relationship between trade protection and growth must be about more than the static gains from the elimination of allocative inefficiencies. In any reasonably calibrated model, such gains are simply far too small to matter in this debate. For example, a much-cited study of Doha Round impacts estimated that the static gains for developing countries of completely free trade would amount to just 0.8% of income (Anderson & Martin, 2005, table 3). The World Bank (2005) stands behind these estimates. As Rodrik (2006, p. 976) puts it:

> One of the insights of [the World Bank's] *Learning from Reform* is that the conventional package of reforms was too obsessed with deadweight-loss triangles and reaping the efficiency gains from eliminating them, and did not pay enough attention to stimulating the dynamic forces that lie behind the growth process. Seeking efficiency gains does not amount to a growth strategy.... Market or government failures that affect accumulation or productivity change are much more costly, and hence are more deserving of policy attention.

We must move beyond static analysis and look at dynamics. Yet for a growth model to be useful here, it must include some basis for trade. A realistic and simple production system for this purpose is a model of at least two sectors where a developing country has a comparative disadvantage in producing (some) inputs to the production process, be they capital inputs (durable for many periods) or intermediate inputs (nondurable).[7]

An early and clear exposition of this sort of model is that of Mazumdar (1996), which was written as a response to Baldwin (1992). The model features consumption and capital goods (but not intermediates), and the goods are produced with identical factor shares, so that Hecksher-Ohlin and Stolper-Samuelson effects are absent. As a result, countries are completely specialized. This assumption of uniform factor intensities is now commonplace.[8] Using this type of model, one can explore both transitional dynamics and the steady state, where the dynamics of factor accumulation can follow either Solow or Ramsey mechanics.[9] For brevity, we focus on the Solow model, but almost identical implications derive from the Ramsey setup and are dis-

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[7] On North-South trade in capital goods, see Eaton and Kortum (2001).
[8] See, for example, Hsieh and Klenow (2007).
[9] Mazumdar (1996) examines both the Solow and Ramsey cases. In the first model of Eaton and Kortum (2001) and in Hsieh and Klenow (2007), the focus is on either Solow or Ramsey steady states.

cussed in the earlier working paper version of this paper (Estevadeordal & Taylor, 2008).

How does trade policy enter this kind of model? Tariffs on imported capital goods lower the steady-state level of output since they are a tax that distorts the relative price of capital (De Long & Summers 1991; Jones, 1994; Taylor, 1994, 1998). The developing country, with its comparative disadvantage in capital goods, sees the price of capital goods fall when trade is liberalized. In a neoclassical model, this leads to medium-run growth effects (growth speeds up in the transition to the new steady state) and a long-run level effect (the new steady state will have higher GDP). We now develop and calibrate a model of this type, with both traded and nontraded inputs, allowing for the inputs to include both capital goods and intermediates.[10]

A. A Calibrated Model

We assume a small open developing economy ("developing" here means an importer of capital and intermediates; some rich countries fit this description). Output is used for consumption, as a nontraded intermediate variety, as a nontraded capital variety, or is exported to obtain imports of traded intermediate and capital varieties. Output is made using two factors of production: labor L and capital K. The labor endowment is $L = 1$ fixed, but capital K can be accumulated.

The production of output Y given factors K, L and intermediate inputs X is

$$Y = \left(K^{\alpha}L^{1-\alpha}\right)^{1-\sigma}X^{\sigma},$$

and the Cobb Douglas form means that spending on intermediates X is equal to σY. Thus GDP, or value added, is given by $(1 - \sigma)Y$.

Trade is balanced. For simplicity, output is the numeraire and is exportable with no tax or friction and the world (and domestic) price of this good is set to 1, without loss of generality. Units are chosen so that world prices of the imported goods are also equal to 1. The imported capital goods have a domestic price P_I, and imported intermediates have a domestic price P_X, where $P_I = (1 + t_I)$ and $P_X = (1 + t_X)$ and the t_I and t_X are ad valorem tariffs. For simplicity we can assume that other transport costs are 0 (or that 1 is the c.i.f. price).

We look at two extreme cases to check the sensitivity of the response of growth to tariffs in this setting.

- Case 1. All I and X goods are traded and can be imported. Domestic output can only be used for consumption or exported. In this case, there is 100% pass-through from tariff changes to the domestic input prices of I or X. This will lead to a high estimate of the

impact of tariffs on growth and income, although the impacts would be larger if we allowed for gains from changes in the traded/nontraded margin.

- Case 2. A fraction of the I and X goods are imported varieties and the rest are nontraded and have to be produced from domestic output. For simplicity, the traded-nontraded goods are combined in Leontief fashion so that there is no change in the marginal good (the traded/nontraded boundary is fixed). In this case, there is only a limited pass-through of tariff changes to the domestic price index of I or X goods. This will lead to a low estimate of the impact of tariffs on growth and income.

In the model, spending on inputs is $\sigma Y = P_X X$, and spending on investment is $s(1 - \sigma)Y = P_I I$, where s is the exogenous savings rate. Consumption of domestic output absorbs $(1 - s)(1 - \sigma)Y$.

Substituting for intermediates X, we find

$$Y = \left(\frac{\sigma}{P_X}\right)^{\frac{\sigma}{1-\sigma}}\left(K^{\alpha}L^{1-\alpha}\right).$$

We see immediately that a fall in P_X is isomorphic to a rise in total factor productivity (A). This is one place where reductions in intermediate tariffs create dynamic gains.

Turning to capital goods, the dynamic equation for K is

$$\Delta K = \frac{s(1 - \sigma)Y}{P_I} - \delta K.$$

The steady state is when the level of investment equals depreciation so that

$$I = \frac{s(1 - \sigma)Y}{P_I} = \delta K,$$

which implies that

$$\frac{s(1 - \sigma)}{P_I}\left(\frac{\sigma}{P_X}\right)^{\frac{\sigma}{1-\sigma}}K^{\alpha} = \delta K.$$

Hence, the steady-state level of capital is

$$K^* = \left[\frac{s}{\delta}\frac{(1 - \sigma)}{P_I}\left(\frac{\sigma}{P_X}\right)^{\frac{\sigma}{1-\sigma}}\right]^{\frac{1}{1-\alpha}}.$$

We see that a fall in P_I is isomorphic to a rise in the savings rate s. This is where reductions in capital goods tariffs have the potential to create dynamic gains. We also see how reductions in intermediate goods tariffs also increase the steady-state capital through a TFP-like impact.

How does trade policy affect the prices of capital and intermediate goods? This depends on the traded and nontraded shares. In case 1, with all inputs imported, the tariff cut feeds one-for-one directly into the domestic price. In the more general case 2, this is not true, and the assump-

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[10] For a similar argument in an "AK" model, see Lee (1995) or Gallup, Sachs, and Mellinger (1999). In these cases, the tariff reduction has permanent effects on the growth rate of income, not its level.

tion of Leontief technology allows us to see how far intermediate input complementarities can amplify these development frictions.[11] We assume aggregate investment I is a Leontief composite of traded goods T and nontraded goods N, with

$$I = \min\left(\frac{I_N}{(1-\beta)}, \frac{I_T}{\beta}\right).$$

Since domestic output has a price of 1 and can be used as the nontraded investment good, the price index for one unit of investment is then $(1-\beta) + \beta P_I$.

We assume also that aggregate intermediate input is Leontief composite of traded goods T and nontraded goods N,

$$X = \min\left(\frac{X_N}{(1-\gamma)}, \frac{X_T}{\gamma}\right).$$

Since domestic output has a price of 1 and can be used as the nontraded intermediate good, the price index for one unit of intermediate input is then $(1-\gamma) + \gamma P_X$.

The analysis goes through as before except that there is less trade, and some home output now goes to make the nontraded capital and intermediate goods. Thus,

$$Y = \left(\frac{\sigma}{(1-\gamma) + \gamma P_X}\right)^{\frac{\sigma}{1-\sigma}} (K^\alpha L^{1-\alpha})$$

$$\Delta K = \frac{s(1-\sigma)Y}{[(1-\beta) + \beta P_I]} - \delta K$$

and

$$K^* = \left[\frac{s}{\delta} \frac{(1-\sigma)}{[(1-\beta) + \beta P_I]} \left(\frac{\sigma}{(1-\gamma) + \gamma P_X}\right)^{\frac{\sigma}{1-\sigma}}\right]^{\frac{1}{1-\alpha}}. \quad (1)$$

For calibration purposes we choose parameters representative of developing countries. We set $\alpha = 1/3$ following Gollin (2002). We assume $s = 0.25$ and $\delta = 0.06$. Following Jones (2011), we set $\sigma = 0.5$, so intermediates have a 50% share of output; this is more conservative than the 0.7 postulated by Gallup, Sachs, and Mellinger (1999). In the case of fully traded inputs, we set $\beta = 1$ and $\gamma = 1$. For the case with some nontraded inputs, we set $\beta = 0.3$ and $\gamma = 0.15$, which are close to the average values in the developing country data set we use in the empirical analysis later.[12] Note that for our purposes, the key results in equation (1) are not specific to the Solow model; similar steady-state elasticities relating K^* to deep parameters appear in a Ram

sey version of the model discussed in the working paper version of this paper; what differs are the transitional dynamics.[13]

B. Simulations

For the simulations, we compute GDP each year and in the final steady state, starting from an initial steady state with $P_I = P_X = 1.25$ (25% uniform tariffs) and where the policy change at time T is to remove these tariffs. As we shall see, these tariff changes are again comparable to the data for those countries that pursued liberalization in the Uruguay Round. Three types of trade liberalization policy experiments are considered:

X: Eliminate the 25% X tariff and reduce P_X to 1.
I: Eliminate the 25% I tariff and reduce P_I to 1.
XI: Eliminate the 25% X and I tariffs and reduce both P_I and P_X to 1.

Simulations of growth with fully traded goods are shown in Figure 1a. Investment tariff reductions (I) have no immediate impact, as they do not raise productivity. But they encourage accumulation. In the long run, output rises by 11.5%. In contrast, intermediate tariff reductions (X) have an immediate impact, as they raise productivity right away. They also encourage accumulation, like a productivity shock. In the long run, output rises by 39.4%. Finally, when both tariffs are removed, the effects are compounded: long-run output rises by 56%.

Simulations for the model with nontraded goods appear in figure 1b. These simulations show much smaller impacts (note the vertical scale change) because by assumption, there is no pass-through from tariff reductions to the prices of nontraded capital and intermediate goods. We set the traded share of capital goods to $\beta = 0.3$ and the traded share of intermediate goods to $\gamma = 0.15$, so we can see that to a first approximation, the impacts on the steady-state output level of reductions in I and X tariffs will be reduced by 70% and 85%, respectively. In fact, for investment tariff reductions (I), output rises by 3.7%. For intermediate tariff reductions (X), output rises by 5.7%. And when both tariffs are removed, output rises by 9.6% in the long run.

The tabulation below figure 1 documents the implied growth accelerations over a fifteen-year period to conform to our empirical design. Overall, the fully traded model simulation a suggests a high estimate for the growth impact of an extra 2.5 percentage points of growth per year in this window, or 0.1 percentage points of extra growth per 1% of

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[11] See Kremer (1993) and the more recent and general "weak links" argument in Jones (2011).

[12] To compute these parameters for the average country in our data set we gather import value data by types of goods from the UN COMTRADE database and then compute each type's share of nominal GDP.

[13] As is well known, for typical calibrations, the Ramsey model has a much faster convergence speed than the Solow model, and this is true here. Our calibrated Solow model converges at 4% per year to steady state, at the high end of empirical estimates of convergence speed (see Mankiw, Romer, & Weil 1992; Dowrick & Rogers 2002). In the working paper, our calibrated Ramsey model converges about twice as fast, at about 8% per year, a speed rarely seen in empirical work (Caselli, Esquivel, & Lefort, 1996).

FIGURE 1.—MODEL SIMULATIONS OF REAL GDP
INITIAL VALUE OF REAL GDP BEFORE POLICY CHANGE = 1.0

(a) When all goods are traded

(b) When most X and I goods are nontraded
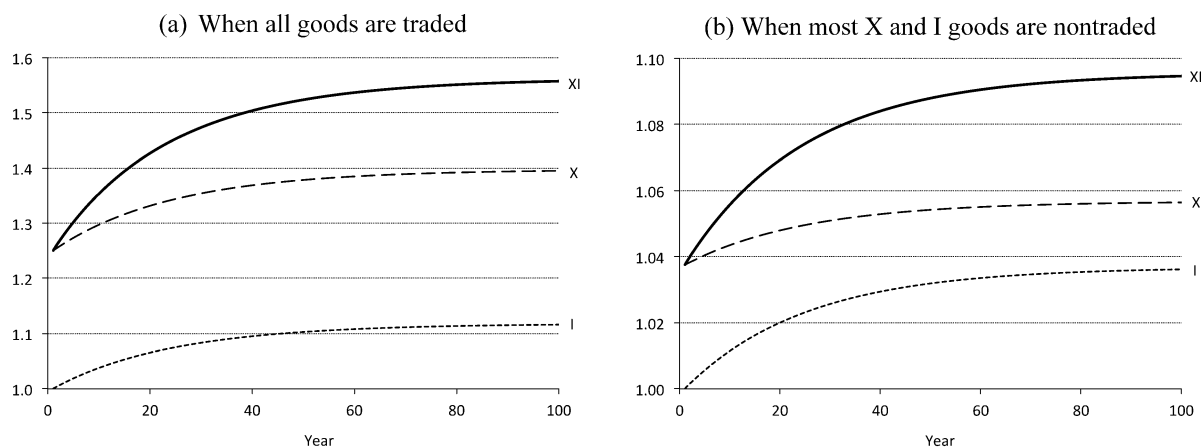
Increase in average real GDP growth rate (% per year) over first 15 years	Remove 25% I tariff	Remove 25% X tariff	Remove 25% I and X tariffs
(a) When All Goods Are Traded	0.36%	1.85%	2.25%
(b) When Most X and I Goods Are Nontraded	0.11%	0.30%	0.42%

The simulations are labeled as follows. I: Remove 25% I (capital goods) tariff. X: Remove 25% X (intermediate goods) tariff. XI: Remove 25% I and X tariffs.

tariff reduction. When inputs are nontraded the effects in simulation b are about one-fifth as big and suggest a low estimate for the growth impact of a 25% tariff removal would be about 0.5 percentage points of growth per year in this window, or 0.02% of extra growth per 1% of tariff reduction. In both cases, a large part (two-thirds) of these effects is felt via the intermediate goods channel.

C. Summary: From Theory to Empirics

The simulations guide our interpretation of empirical results and put a fresh perspective on key works from the trade and growth debate. Static CGE models estimating trivial one-shot gains of 1% or less appear too pessimistic, since they fail to take into account the dynamic gains from cheaper capital and intermediate inputs. Our low estimate suggests a level effect of 9.6%, an order of magnitude bigger, and our high estimate of 56% is almost another order of magnitude bigger still. These larger gains from trade could be far from trivial for those developing countries (most of them), which must import key intermediate and capital goods.

In our view, the plausible growth and level effects ought to be somewhere between our high and low estimates. Although many goods are nontraded, we can think of several factors that are not included in our simple model or in the empirical work we present below, which could cause a higher growth response: the possibility of more aggressive

substitution toward cheaper inputs (not allowed by our Leontief specification); shifts in the traded/nontraded margin (which was assumed fixed in the model); new goods on the extensive margin of trade (ditto);[14] the possibility of induced higher productivity through imports (for example, by learning or new inputs); and the removal of nontariff barriers (absent in this model but that, in reality, are also removed during trade reforms).[15]

Thus, if even our low-end estimate of growth impacts is around an extra 0.5 percentage points per year for fifteen years, we might take the view that estimates above 0.5 and perhaps up to 1.0 to 1.5 percentage points per year might be reasonable given these extra factors. And indeed, in the empirical work that follows, we shall find an impact consistent with this range.

III. Data

We compiled, and in some cases hand-collected, the following core data to test the theory.

Growth rate: The dependent variable is the growth in GDP per capita in PPP constant 2000 international dollars, from the World Bank's World Development Indicators

[14] One recent study, for the case of India, finds a large response of imported inputs to tariff changes on both the extensive and intensive margins (Goldberg et al., 2010).
[15] Although obfuscation may sometimes lead to a rise in nontariff barriers, which offset tariff reductions (Kono, 2006).

(online, September 2005 edition) based on annual data from 1975 to 2004. Rates of growth over long periods are calculated in continuous fashion, using differences in log levels divided by years elapsed.

Average tariffs: Three tariff measures are employed. The first is an average tariff measure, the mean tariff rate from the Economic Freedom in the World (EFW) 2005 database (http://www.freetheworld.com/; Area 4-A(ii) Data). Data are available every five years from 1970 to 2000, plus 2001, 2002, and 2003, and sample size grows from 77 countries in 1970 to 122 in the year 2000. In our regressions, we employ samples of up to 75 countries from this database.

Disaggregated tariffs: Our theory makes an important distinction between capital, intermediate, and consumption tariffs, so we go beyond existing measures and compile data on disaggregated most favored nation (MFN) applied tariffs for 49 economies at two benchmark eras: "early" or "before," meaning circa 1985 (in practice, between 1985 and 1993), and "late" or "after," meaning circa 2000 (in practice, between 1999 and 2001). Note that these applied tariff rates may differ from ceilings set by bound tariff rates, the objects of negotiation in the GATT/WTO process.

For the late benchmark, we rely on tariff data from the UNCTAD's TRAINS (Trade Analysis and Information System). However, for the early benchmark, TRAINS fully covers only 23 economies in our sample. Data for eight EU economies are based on TRAINS, with gaps filled using EU tariff schedules. For the other eighteen economies, we had no option but to collect tariff data by hand, line by line, from national tariff schedules from the 1980s. Data for Latin American countries came from schedules provided by ALADI. For the rest, we used published tariff schedules available at the Library of Congress (Washington, DC). This was a nontrivial exercise, for some economies, there were many thousands of tariff lines to be collected.

Finally, unweighted average tariffs were computed for consumption, capital, and intermediate goods. This avoids the criticism of import-weighted tariffs that they may understate protection when tariff rates are at or near prohibitive levels (cf. Warner, 2003; Rodríguez, 2007).

Further details of our new tariff data are provided in the online data appendix.

Other basic controls: In the course of our benchmark empirical analysis, we add some standard control variables in the growth regressions. We measure institutional quality as legal and property rights according to the Economic Freedom in the World 2005 database: Area 2-AB composite score, where 2-A is an index of judicial independence ("the judiciary is independent and not subject to interference by the government or parties in disputes"), and 2-B is an index of impartial courts ("a trusted legal framework exists for private businesses to challenge the legality of government actions or regulation"). As is common in growth regressions (see Easterly, 2005), we also include a measure of human capital, proxied by total years of schooling, from

Barro and Lee (2000), and a catch-up term, log initial GDP per person, to control for transitional dynamics.

IV. Empirics: Design and Implementation

In this section we present an empirical design that differs from the previous cross-section literature but is better suited to the policy question at hand. In this design, we consider post-1990 trade liberalization as a treatment and using two different methods. The first method treats openness as a discrete (0-1) treatment in the spirit of Sachs and Warner (1995) or Wacziarg and Welch (2008) and uses a difference-in-difference estimator. The second method treats openness as a continuous treatment, following most of the literature, using tariff rates as a proxy for openness in a regression in differences. Difference estimators can avoid omitted-variable problems that plague cross-section analysis, at least when the omitted regressors are time-invariant country characteristics. But the treatment variable must be exogenous, and in a later section, we consider endogeneity issues and instrumental variables.

A. Empirical Design and the Great Liberalization

In the older literature critiqued by Rodríguez and Rodrik (2001), the dominant question was: Do liberalized countries grow faster than nonliberalized countries in a given period, all else equal? But this is, we believe, *the wrong question.* At the very least, it is probably an empirically unanswerable question, since ensuring that all the proper controls are included is likely an impossible task. Thus, the results in this literature are fraught with omitted variable bias, and their resulting fragility, as Rodríguez and Rodrik (2001) noted, leaves little hope of a precise and definitive answer save the meager refuge offered by a null hypothesis of no effect.

In contrast, we think the right question is: *Does the rate of growth accelerate more in a liberalizing (treatment group) country as compared to nonliberalizing (control group) country?* This way of looking at the question has numerous benefits. It corresponds most closely to the policy question being asked by policymakers before liberalizing, and it also corresponds most closely to the claim embedded in the Washington Consensus that liberalizers would grow faster then they otherwise would have without liberalization—notwithstanding the fact that many other factors, some unobservable and many unalterable, could still intervene to cause differences in growth rates between countries, whether they were liberalizing or not.

A naive difference equation with no controls could then be perfectly satisfactory provided the parallel trends assumption is met. More generally, we may have to worry about any variables that change over time in the growth equation. Also, as in the cross-section literature, we know that endogenous variables should not be included as regres-

sors. The variables to be included should be either exogenous or endogenous and properly instrumented.

However, studying trade liberalization as a treatment control problem also raises the stakes empirically. Put simply, if an experimental design is going to work, then there has to be some sort of experiment: enough countries need to receive the liberalization "treatment" in sample; enough time needs to elapse so that growth can be observed in both pre- and posttreatment phases; and growth then has to be compared to a control group of nonliberalizers.

And here is the key problem for older studies: before the Uruguay Round, very few developing countries had engaged in any serious trade liberalizations. Some had begun the process of trade reform unilaterally (the NICs and Chile, for example). But for the most part, it was the developed countries that had been the main participants in earlier GATT rounds or in other serious regional trade agreements (notably the EU) that had fostered lower tariffs. For example, the GATT's Kennedy Round of 1962–1967 included only 48 countries. And although the Tokyo Round of 1973–1979 encompassed roughly 100 countries, including twenty non-GATT developing countries, the progress made in reducing developing country trade barriers was negligible. In contrast, the Uruguay Round, 1986–1994, included 125 countries and focused strongly on tariff reductions in both developed and developing countries. This, we would argue, is the experiment that we have been waiting for.
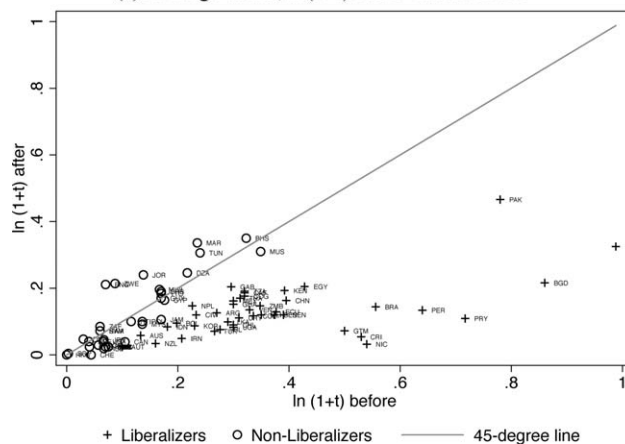
Figure 2a sums up what the Uruguay Round achieved for the reductions in average tariff levels (EFW data). To show what happened, we plot post–Uruguay Round (year 2000) tariffs against pre–Uruguay Round (year 1985) tariffs. In that follows, our empirical design will exploit differences in the change in tariffs pre- versus post–Uruguay Round to identify the effects of trade policy changes on growth (and other) outcomes. We begin with a simple discrete treatment approach based on a two-way sample split and then later move on a continuous treatment framework.

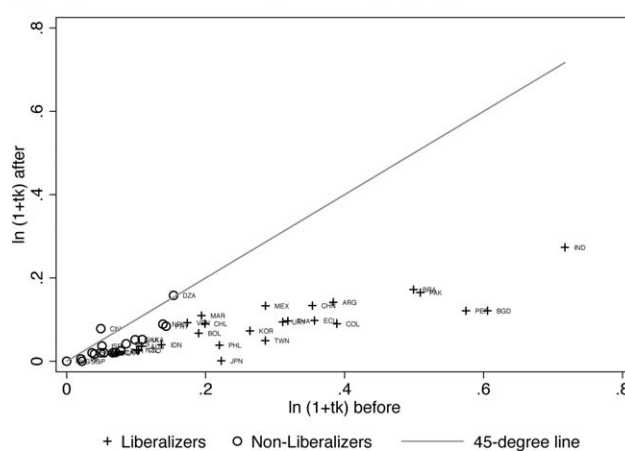### B. Constructing a Liberalization Indicator

For our initial empirical work we start simple and divide the set of countries equally into two discrete bins of countries, creating an indicator or binary dummy variable to identify liberalizers, based on whether the change in a country's tariff level was above or below the sample median:

Nonliberalizers are defined as those that did not (or could not) lower tariffs between the early (circa 1985) and late (circa 2000) periods. Clearly, there are really two sets of countries hiding with this group. The first set inherited low tariffs and left them low. These countries are close to the origin in Figure 2a. They never received the treatment because they were always open in the sample period. For example, in 1985, Singapore had tariffs as low as 2.2%, which left little room for further substantive tariff reductions after 1970. (Tariffs in Singapore fell to zero in 2000.)

FIGURE 2.—THE GREAT LIBERALIZATION: TREATMENT AND CONTROL GROUPS

(a) Average tariffs, $\ln(1+t)$: after versus before

+ Liberalizers   ○ Non-Liberalizers   —— 45-degree line

(b) Capital and intermediate goods tariffs, $\ln(1+t_{ki})$: after versus before

+ Liberalizers   ○ Non-Liberalizers   —— 45-degree line

Clearly, the trade policy element of the Washington Consensus did not speak to this group of countries: they had practically converged to free trade before the Uruguay Round and without any nagging from the Beltway. Thus, no growth accelerations induced by trade policy could be expected in these cases after 1985. A second set of countries inherited high tariffs and left them alone, or even raised them. These countries are close to the diagonal on the upper right of figure 2a, and they are countries that never received the treatment because they have always been closed. For example, Jordan had an average tariff of 13.8% in 1985, rising to 24% in 2000, according to EFW. Jamaica's measured tariff fell from 17% in 1985 to just 10.6% in 1999—a cut in tariffs, but not a big one. Clearly, the Washington Consensus potentially could have spoken to these countries, only they did not pay much attention to it.

Liberalizers are those countries that both could and did lower tariffs between the early (circa 1985) and late (circa 2000) periods. They had large tariffs to begin with and cut them. These countries are below the diagonal on the lower right of figure 2a, and they received the treatment, going from closed toward open. They are selected as countries

with an above-median decrease in tariffs between 1985 and 2000. For example, Argentina had average tariffs of 27% in 1985, falling to 12.6% in 2000. This group also includes some developed countries—for example, Australia and New Zealand, which also embarked on trade liberalization in the 1980s and 1990s (they are rich countries but also are net importers of capital and intermediate goods, so they fit the rubric of our model). Another classic example would be India with tariffs as high as 98.8% in 1985, falling to 32.5% in 1999—still high, but a whole lot lower than before. It is obviously to this third group of countries that the Washington Consensus spoke. Their pre–Washington Consensus trade policies placed an enormous tax on imports (including imports of capital and intermediate goods) that was subsequently removed. In fact, the true extent of liberalization was probably larger than shown here, given the way the tariff data were sampled and our inability to measure changes in nontariff barriers.[16] The question is, Following this liberalization, did the treatment group see improved growth performance?

Figure 2a shows the control and treatment groups for the proposed experimental design using average tariffs. Figure 2b repeats the approach using our more restricted sample of capital and intermediate goods tariffs from our newly collected data set. Again, we can partition the sample to isolate "off-diagonal" countries that embraced the Washington Consensus and those on the diagonal that could not or did not liberalize, and similar patterns emerge.

To further describe the evolution of tariffs in these samples, figure 3 plots the average tariffs for different subsets of countries from 1975 to 2000 using EFW average tariff data. Figure 3a shows averages for the whole sample: developed and developing countries. The developed world started with lower tariffs and lowered them a little (about 10% falling to about 5%), but the developing world lowered tariffs more dramatically in the 1980s and 1990s (about 35% falling to about 15%), confirming that the main action was in the poorer countries.

Figure 3b shows average tariffs for the two groups. The nonliberalizers saw very little movement in their average tariff rate; it stayed on average at about 15% to 20% throughout (recall, this is a mix of low- and high-tariff

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[16] Our tariff variable probably measures a lower bound on tariffs in the early period and an upper bound in the late period. Early tariffs are mostly measured near the end of the period (mid-late 1980s) and tariffs were probably much higher in the mid-1970s in some countries. Similarly, our late tariffs exclude further tariff declines for selected trading partners that resulted from the explosive growth of preferential bilateral and regional free trade agreements (FTAs). The differences here may be more substantial in some cases. Although such agreements are seen by some as inimical to the GATT/WTO process (Bhagwati, 2008), and perhaps also contrary to the spirit of the Washington Consensus, evidence suggests that there are substantial spillovers from FTA tariff reductions to subsequent unilateral (ex-WTO) MFN tariff reductions. Estevadeordal, Freund, and Ornelas (2008) find that a country that provides preferential tariff-free access in a sector with a 15% MFN tariff is likely to subsequently lower its MFN tariff by 3 percentage points, suggesting that the WTO mechanism encourages countries to set MFN rates close to prevailing applied rates.

FIGURE 3.—THE GREAT LIBERALIZATION: TRENDS IN TARIFFS

(a) Average tariffs in Developed versus Developing
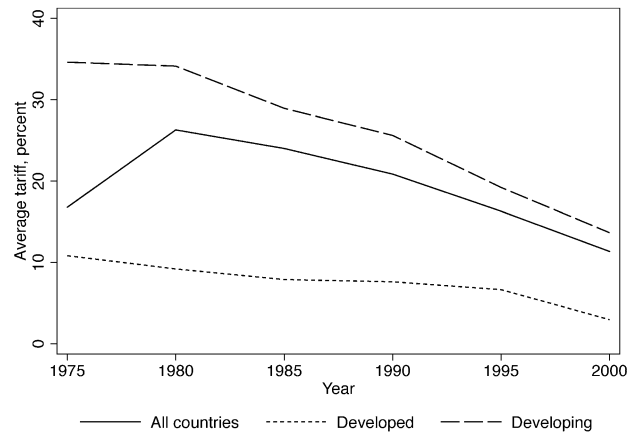
(b) Average tariffs in Liberalizers versus Non-Liberalizers
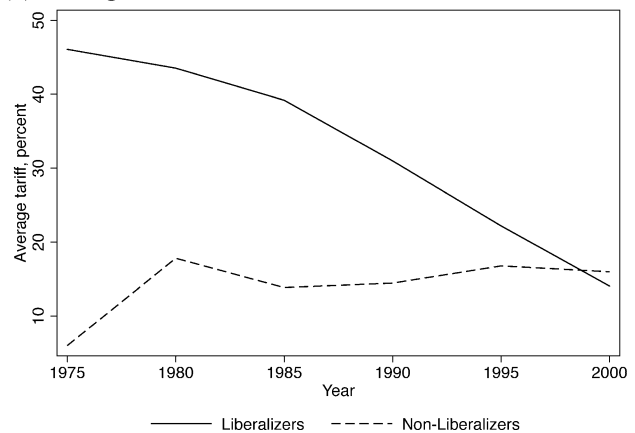
The samples are as follows (* = developing country).

Liberalizers: Austria, Turkey*, Australia, New Zealand, Argentina*, Bolivia*, Brazil*, Chile*, Colombia*, Costa Rica*, Ecuador*, Guatemala*, Mexico*, Nicaragua*, Paraguay*, Peru*, Uruguay*, Venezuela*, Iran*, Egypt*, Bangladesh*, Sri Lanka*, India*, Indonesia*, South Korea*, Nepal*, Pakistan*, Philippines*, Thailand*, Central African Republic*, Republic of Congo*, Benin*, Gabon*, Ghana*, Côte d'Ivoire*, Kenya*, Mali*, Senegal*, Tanzania*, Uganda*, Zambia,* and China.*

Nonliberalizers: United States, United Kingdom, Belgium, Denmark, France, Germany, Italy, Luxembourg, Netherlands, Norway, Sweden, Switzerland, Canada, Japan, Finland, Greece, Iceland, Ireland, Spain, South Africa*, Oman*, United Arab Emirates*, Hong Kong*, Malaysia*, Singapore*, Botswana*, Namibia*, Haiti*, Bahamas*, Barbados*, Guyana*, Jamaica*, Trinidad & Tobago,* Cyprus*, Jordan*, Algeria*, Malawi*, Mauritius*, Morocco*, Zimbabwe*, Tunisia*, Papua New Guinea,* and Poland.*

countries). The control group thus saw very little tariff change from 1975 to 2000. The really dramatic change is seen in the treatment group, the liberalizers: initially in 1975, 1980, and 1985, their average tariff rates exceeded 40%. But in the Washington Consensus era, after the Uruguay Round, these countries cut average tariffs to a much lower level, around 15%, a cut of about 25 percentage points (similar to the cut used in our earlier model simulations). Exploiting this contrast between liberalizers and nonliberalizers should allow us to identify any progrowth impacts of trade liberalization.

C. Openness as a Discrete Treatment: Difference-in-Difference Estimates

We begin our empirical work using the simplest notion of treatment, a dichotomous variable to capture countries

Table 1.—Discrete Treatment Variable: Difference in Difference Regressions
Dependent Variable: Difference in Growth, Δgrowth

Sample Tariff measure	(1) Full Average	(2) Restricted Consumption	(3) Restricted Capital	(4) Restricted Intermediate	(5) Restricted Capital and Intermediate
	A. With Growth Model Controls				
Liberalizer indicator	0.0072**	0.0084**	0.0086**	0.0090**	0.0095**
	(0.0037)	(0.0038)	(0.0037)	(0.0036)	(0.0036)
Growth lagged	−0.574***	−0.532***	−0.518***	−0.547***	−0.522***
	(0.079)	(0.094)	(0.094)	(0.092)	(0.092)
Change in schooling	−0.00035	0.00149	0.00129	0.00110	0.00149
	(0.0027)	(0.0030)	(0.0030)	(0.0030)	(0.0029)
Change in institutions	0.0271*	0.0212	0.0178	0.0171	0.0188
	(0.014)	(0.018)	(0.017)	(0.017)	(0.017)
Observations	70	44	44	44	44
R^2	0.57	0.52	0.53	0.54	0.54
	B. Excluding Institutions and Schooling				
Liberalizer indicator	0.0037	0.0079**	0.0077**	0.0091**	0.0095***
	(0.0035)	(0.0035)	(0.0035)	(0.0034)	(0.0034)
Growth lagged	−0.586***	−0.496***	−0.486***	−0.512***	−0.487***
	(0.073)	(0.088)	(0.088)	(0.086)	(0.086)
R^2	0.51	0.47	0.46	0.49	0.49
Observations	75	47	47	47	47

Dependent variable is difference in the average change per annum of log GDP per worker. "Pre" is period 1, 1975–1989; "post" is period 2, 1990–2004. Full sample uses EFW trade taxes as a tariff proxy. Restricted sample uses disaggregated tariff data. Tariffs are measured in 1985 and 2000 or closest date thereto. Standard errors in parentheses. Constant terms not reported. Significant at *10%, **5%, and ***1%.

thought to have liberalized (liberalizer = 1) versus those that have not (liberalizer = 0), using the definitions of the previous section. This approach is the difference-regression analog of the 0-1 openness indicator Sachs and Warner (1995) used and their followers. Thus, if we suppose the levels regression is written

$$growth_i = \alpha[openness_i] + \beta X_i + \varepsilon_i, \qquad (2)$$

where X is a vector of control variables, then the differenced regression can be written

$$\Delta growth_i = \alpha[\Delta openness_i] + \beta \Delta X_i + v_i. \qquad (3)$$

By replacing $\Delta openness_i$ with our indicator variable Liberalizer, we hope to capture those "treated" countries that, in the Washington Consensus era shifted to a more liberal trade regime.

Unlike the widely seen levels regressions of the form in equation (2), we think that difference-in-difference (DD) regression estimates of the form in equation (3) offer a clean and simple test of the hypothesis that liberalizers in this era grew faster than nonliberalizers using data from reasonable sample periods. In our work, there are two periods ($T = 2$). Period 1 is 1975 to 1989, and period 2 is 1990 to 2004. By splitting the sample into two fifteen-year periods, we can hope to avoid any pollution of our conclusions as a result of lags in policy implementation and short-run output fluctuations such as business cycles or crises, and yet the time frame is sufficiently short that (as our simulations have shown) we should still be able to detect medium-term post-reform growth accelerations. Using two periods is also justified by data limitations since we have only one prereform observation on tariff rates. Still, restricting analysis to two periods before and after treatment may be a blessing in dis-

guise, since DD methods with $T > 2$ run the risk of biased standard errors (Bertrand, Duflo, & Mullainathan 2004).

Table 1A reports the results of these difference regressions, with various controls added. The dependent variable is the annual average growth rate of GDP per person (from WDI). This first set of regressions in columns 1 to 5 includes no control variables other than liberalizer, but liberalizer is defined in various ways. The first definition in column 1 sets liberalizer = 1 for countries where the average tariff rate (from EFW) falls by more than the median for the entire sample between periods 1 and 2 ($N = 75$). Other definitions in columns 2 to 5 are like the first, except that the tariff on consumption goods, capital goods, intermediate goods, or the average of capital/intermediate goods is used, based on our new data for a smaller set of countries ($N = 47$).

Will the use of different tariff measures matter? One might worry that the results could be similar whatever tariff measure is used, making our pursuit of disaggregated tariff data rather pointless, since the average tariff would be information enough. Why? Although the measures involving capital or intermediate goods tariffs should be the better measures of the growth-enhancing effects of trade policy according to our theory, in practice countries may tend to lower tariffs of all kinds simultaneously during trade reforms. To examine this problem, we can look at the correlations between the different discrete liberalization indicators. The correlation between the liberalization indicator using average tariffs and that using consumption tariffs was 0.66; the correlation between the liberalization indicator using average tariffs and that using capital and intermediate tariffs was only 0.57. Clearly countries did tend to lower all tariffs together, but the fact that these correlations are much less than 1 shows that the information contained in average

tariffs may be polluted by different shifts in consumption tariffs, an element of noise that would confound the tariff-growth correlation if our theory is any guide.

Table 1A offers support for the Washington Consensus prescription, but it does depend on the tariff measure used. Liberalizing countries grew about 0.7 to 0.95 percentage points per annum faster than nonliberalizers in this period. Columns 1 through 5 repeat the exercise with the inclusion of widely used control variables from the growth literature. Following the state of the art, standard reduced-form estimates of a growth regression should include only those few controls X that are putatively exogenous, such as initial GDP per person (log), initial schooling, and initial institutions (Barro, 1991). In this view, clearly endogenous variables such as investment or trade flows must be omitted from the right-hand side. Putting the three controls into difference form implies that ΔX should contain the lagged level of growth, the change in schooling, and the change in institutions. Schooling is total years of schooling (variable: tyr) from Barro and Lee (2000). Institutional quality is measured by the EFW legal and property rights score (variable: area 2ab).

The coefficient of about 0.55 on the lagged growth variable is statistically significant and plausible. Over a fifteen-year window, it implies an annual convergence speed of about 3.5% (= 0.55/15). Estimated convergence speeds in this range are not unreasonable, certainly compared to the empirical literature and the Solow model (the typical Ramsey model calibration implies higher convergence speeds, however). Change in schooling is not statistically significant in any regression. Institutional change is weakly significant only in column 1 at the 10% level.

This leads us to our preferred OLS specifications in table 1B, which exclude the not significant institutions and schooling variables. In this panel, the results line up closely with our a priori expectations. Using crude average tariffs produces a smaller and insignificant coefficient on tariffs, which might explain the often weak and nonrobust results in the prior literature. Switching to disaggregated tariffs results in a larger and statistically significant coefficient on tariffs. In the final column of the table, where the combined capital and intermediate tariffs are used, the coefficient is at its largest, implying a 1 percentage point per annum growth acceleration for liberalizers, and the effect is significant even at the 1% level.[17]

These results warrant a few further comments. Our empirics have also surmounted another hurdle, for as Easterly (2005) had shown, many earlier results in the openness-growth literature proved not to be robust to the inclusion of the catch-up term (here in a differenced form). The failure of Easterly's regressions to be robust may not be too surprising, however, given that (for survey purposes) he follows the literature in using trade share as a measure of

openness—when, of course, this is an inappropriate outcome variable rather than a direct measure of policy. We also find, in contrast to the levels results Easterly discussed, that our growth results *are* robust to the inclusion of institutional controls, which is not too surprising since institutions change very little in the short to medium run. Likewise, changes in schooling policies seem to be either too small to matter or otherwise uncorrelated with acceleration outcomes. We would also argue that in the face of Easterly's warning about the "arbitrary" measures of episodes of policy change (he refers explicitly to Sachs and Warner, 1995), we have found a very direct measure of trade policy change by looking directly to the changes in trade taxes rather than inferring reform events based on an amalgam of aggregated tariff data, black market premiums, government monopoly measures, and so on (Easterly 2005). Our tariff variable may be narrow, but it is cleanly defined and measured.

D. Robustness: Testing for Preexisting Differences Using a Placebo Treatment

As a basic robustness check on the difference-in-difference results, we now perform a test using a "placebo" treatment as a way to test for systematic differences between the treatment and control groups that may have predated our benchmark sample window, the two fifteen-year periods 1975–1989 or period 1, and 1990–2004 or period 2. For this purpose we introduce a new period, 1960–1974 and call it 0. We then repeat our empirical exercise examining growth acceleration from period 0 to period 1, but using the actual circa 1990 binary treatment-control seen at the end of period 1 as a regressor.

Under this placebo-style experiment, a group of countries receives the placebo in the form of a counterfactual trade liberalization in 1975, equivalent to the actual treatment they got in 1990. The purpose of this experiment is to ensure that the countries that we classify as treated in period 2 were not in fact already systematically different from the control group and experiencing different (accelerated) economic performance in period 1.

The results of this placebo treatment are shown in table 2 (where here we use PWT real GDP per capita, chain method, since the WDI data do not stretch back before 1975). The results are reassuring. The treatment group of post-1990 liberalizers shows no growth acceleration in period 1 relative to period 0, as compared to the control group of post-1990 nonliberalizers. In terms of preexisting behavior, as revealed by the placebo, the two groups show no systematic difference in their growth dynamics in the two periods prior to the actual experiment.

A simple summary of this important finding, and indeed of the main result in this paper, is shown in Figure 4. Here we take the treatment and control groups, and calculate the 1975–1989 (period 1) trend of average log GDP per person in each group. We detrend the actual values in all years using these trends respectively for both groups.

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[17] Again, similar coefficients are obtained when the sample is restricted to developing countries, although precision again suffers (results shown in the working paper version of this paper).

TABLE 2.—"PLACEBO" DIFFERENCE IN DIFFERENCE REGRESSION FOR 1960–1974 AND 1975–1989 WITH COUNTERFACTUAL POST-1990
TREATMENT CONTROL IMPOSED 1975–1989
DEPENDENT VARIABLE: DIFFERENCE IN GROWTH, $\Delta growth$

Sample Tariff Measure	(1) Full Average	(2) Restricted Consumption	(3) Restricted Capital	(4) Restricted Intermediate	(5) Restricted Capital and Intermediate
Liberalizer indicator	−0.0092*	0.0018	−0.0018	0.0025	0.0001
	(0.0053)	(0.0060)	(0.0060)	(0.0059)	(0.0060)
Growth lagged	−0.812***	−0.492***	−0.507***	−0.495***	−0.500***
	(0.14)	(0.18)	(0.18)	(0.18)	(0.18)
R^2	0.32	0.16	0.16	0.16	0.15
Observations	71	46	46	46	46

See table 1. "Pre" is period 0, 1960–1974. "post" is period 1, 1975–1989. The liberalizer dummy is for the 1990 date, as in table 1. Standard errors in parentheses. Constant terms not reported. Significant at *10%, **5%, and ***1%.

FIGURE 4.—THE GREAT LIBERALIZATION AND GROWTH ACCELERATION
LOG INCOME PER WORKER (PWT) RELATIVE TO 1975–1989
TREND IN LIBERALIZERS AND NONLIBERALIZERS
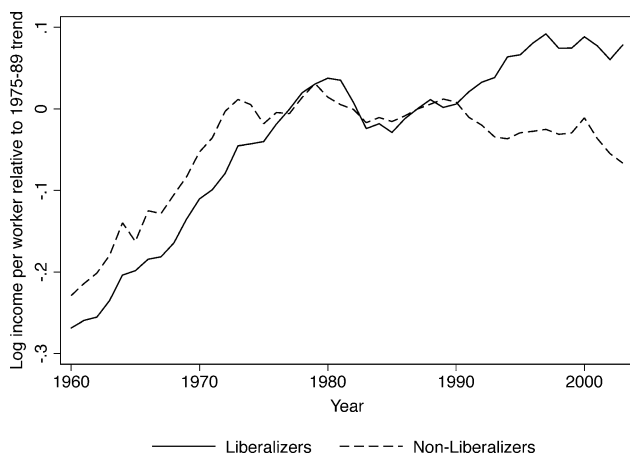
The samples are as in figure 3, based on changes in tariffs for capital and intermediate goods.

If the two groups were truly similar ex ante, we might expect similar cyclical behavior too, as idiosyncratic country shocks are averaged out and common "global shocks" remain. This is indeed what we find. The two groups' GDP per capita patterns tracked each other closely, both in period 0 (above trend growth) and period 1 (on trend, by construction). Their trends were barely distinguishable: future liberalizers grew about 0.2% per annum faster than future nonliberalizers, a trivial difference. We can also see that for the two groups, economic cycles show strong correlations, with some corresponding to well-known global boom-bust episodes (for example, the recessions in the 1970s and the 1980s crises).

If our argument has merit, then after the treatment, in period 2, these two groups of countries ought to have diverged dramatically. Indeed they did, as we see from the figure. The difference in the two groups' trends was now a nontrivial 1% per year.[18] By 2004, liberalizers were almost 10% above the 1975–1989 trend, but nonliberalizers had fallen almost 10% below trend, creating a 15% to 20% gap in detrended GDP per person outcomes. There is even a

suggestion of cyclical decoupling, with very different business cycle behavior in the two groups.

Figure 4 therefore reinforces the results of the placebo experiment in table 2. Before the Great Liberalization and going back to the 1960s, the treatment and control groups were not that different in terms of growth. After 1990, however, divergent dynamics clearly separated the two groups.

E. Openness as a Continuous Treatment: Difference Estimates

We do not dwell further on the results from table 1 since, like the Sachs-Warner openness measure, our use of a dichotomous liberalization treatment indicator can be faulted by skeptics for throwing away too much information by reducing a variety of policy stances to an on-off dummy variable. In what follows, we maintain our basic empirical design based on differencing, but now we make use of the fact that changes in tariffs (t) provide a continuous treatment measure, and so we switch to using a difference regression with a continuous variable, the change in $\ln(1 + t)$, replacing the liberalization indicator variable as the measure of policy change.

Echoing table 1a, table 3a now reports results using the continuous treatment measure $\log(1 + t)$ but again with additional controls comprising lagged growth, change in schooling, and change in institutions. We see that use of the consumption tariff clearly biases the coefficient toward zero. Using the theoretically "correct" input tariffs leads to stronger results.

However, once more the only control variable that consistently enters with statistical significance is the catch-up term using lagged growth. Again, neither institutional change nor changes in schooling appear to drive growth accelerations across these two periods at conventional significance levels. The catch-up coefficient takes values in the range from −0.5 to −0.6, so implied convergence speeds are again a reasonable 3% to 4% per annum.

Following table 1b, we turn in table 3b to a parsimonious specification that excludes the insignificant institution and schooling variables and constitutes our preferred set of OLS results for the continuous treatment case. Again, the same patterns hold. Using average tariffs or consumption tariffs

[18] Although our methods for identifying liberalizations are different, as noted earlier, this 1% per annum impact is broadly consistent with other recent findings based on treatment-control or difference-in-difference methods (Dollar & Kraay, 2004; Wacziarg & Welch, 2008).

TABLE 3.—CONTINUOUS TREATMENT VARIABLE: DIFFERENCE REGRESSIONS
DEPENDENT VARIABLE: DIFFERENCE IN GROWTH, $\Delta growth$

Sample Tariff Measure	(1) Full Average	(2) Restricted Consumption	(3) Restricted Capital	(4) Restricted Intermediate	(5) Restricted Capital and Intermediate
			A. With Growth Model Controls		
$\Delta \ln(1 + tariff)$	−0.0270*	−0.0199*	−0.0378*	−0.0285*	−0.0333*
	(0.015)	(0.011)	(0.019)	(0.014)	(0.016)
Growth lagged	−0.576***	−0.541***	−0.524***	−0.541***	−0.534***
	(0.079)	(0.096)	(0.096)	(0.095)	(0.095)
Change in schooling	−0.00030	0.00179	0.00094	0.00059	0.00072
	(0.0027)	(0.0031)	(0.0030)	(0.0030)	(0.0030)
Change in institutions	0.0234†	0.0180	0.0123	0.0120	0.0123
	(0.014)	(0.018)	(0.017)	(0.017)	(0.017)
R^2	0.57	0.50	0.51	0.51	0.51
Observations	70	44	44	44	44
		B. Excluding Institutions and Schooling			
$\Delta \ln(1 + tariff)$	−0.0226	−0.0210**	−0.0430**	−0.0325**	−0.0382**
	(0.014)	(0.0100)	(0.018)	(0.013)	(0.015)
Growth lagged	−0.579***	−0.501***	−0.487***	−0.507***	−0.499***
	(0.072)	(0.088)	(0.088)	(0.087)	(0.087)
R^2	0.52	0.46	0.47	0.47	0.48
Observations	75	47	47	47	47

See table 1. Standard errors in parentheses. Constant terms not reported. Significant at *10%, **5%, and ***1%.

TABLE 4.—CONTROLLING FOR INFLUENTIAL OBSERVATIONS: OLS VERSUS ROBUST REGRESSION ESTIMATES OF THE TARIFF COEFFICIENT
DEPENDENT VARIABLE: DIFFERENCE IN GROWTH, $\Delta growth$

Sample Tariff Measure	(1) Restricted Capital	(2) Restricted Intermediate	(3) Restricted Capital and Intermediate
(a) OLS regression estimate of coefficient on $\Delta \ln(1 + tariff)$	−0.0430**	−0.0325**	−0.0382**
	(0.018)	(0.013)	(0.015)
(b) Robust regression estimate of coefficient on $\Delta \ln(1 + tariff)$	−0.0331**	−0.0240*	−0.0289**
	(0.016)	(0.012)	(0.014)

The OLS regression in is the same one reported in table 3B, columns 3–5. Standard errors in parentheses. Constant terms not reported. Significant at *10%, **5%, and ***1%.

muddies the waters in columns 1 and 2: the tariff coefficient is smaller than one might expect (about −0.2) and of borderline statistical significance. In columns 3 through 5, the coefficient is larger, of a theoretically more plausible magnitude (−0.4), and is clearly statistically significant at well below the 5% level.[19]

F. Robustness: Are the Results Driven by a Few Influential Observations?

To guard against the OLS results being driven by a few influential data points, we also reestimated our preferred continuous model using the robust regression method, whereby a reweighting of the observations is applied (using Huber weights) to downweight influential observations in an iterated OLS estimation procedure. This did not weaken our results very much, and the key coefficient on the tariff variable was still large and statistically significant.

Table 4 shows these findings. The first row shows the plain vanilla OLS estimate of the coefficient on the continu-

ous tariff measure estimated in our preferred model (taken from Table 3B, columns 3–5). For comparison, on the second row is the same coefficient on the continuous tariff measure estimated using Huber weights. The coefficient magnitude or elasticity falls somewhat (from about −0.04 to −0.03), but the standard errors are virtually unchanged. All coefficients are statistically significant at the 5% level, except in column 2, where the significance level is marginally above 5%. In quantitative terms, the smaller elasticity means that, for example, a −0.25 log point change in the tariff measure would induce a growth rate change of +0.75 percentage points per year rather than +1 in the OLS case.

To sum up, the main lesson is that even on a small sample, growth impacts of a reasonable size are detectable using theoretically grounded tariff rates for capital and intermediate goods, but using the wrong tariff measures can lead to an understatement of those gains and bias findings toward a null effect. Even when we use robust regression methods and downweight influential observations, we find the result still holds. The point estimates suggest that a representative tariff change of −25% would raise growth by between three-quarters and 1 percentage point per year, a nontrivial effect and one consistent with our earlier model.

[19] Again, similar coefficients are obtained when the sample is restricted to developing countries, although precision again suffers (results shown in the working paper version of this paper).

G. Robustness: Was Growth Acceleration Really Due to Other Policy Reforms?

So far we have argued that the liberalization of trade policy in a select treatment group of emerging market economies was associated with a growth acceleration comparable to that predicted by a calibrated open economy growth model, and we shall shortly argue, moreover, that the relationship is causal. We have also dismissed the objection that these changes might have been related to changes in putative deep determinants of growth like institutions and schooling, as no such correlations appear in the data. Thus, the institutions view is not particularly helpful for understanding this episode.

However, we must first deal with another obvious objection: that it was not policy change in the form of trade liberalization per se that drove growth acceleration in these countries; rather, it could have been any number of other policy changes. The laundry list of the original Washington Consensus urged countries to undertake many reforms on ten different dimensions. The emerging markets of the 1990s took up many of these ideas, and this could raise a serious omitted variables problem for our analysis. Even skeptics who may be ready to concede that our policy view is the right way to think about the post-1990 divergence may raise the concern that it could be policies other than trade liberalization that mattered more. But which ones?

A natural place to start is to look at other quantifiable policy reforms that have been claimed as promoting growth. We examine three such reforms: financial openness, monetary stability, and fiscal stability. The first of these will show whether it was openness to trade rather than finance that spurred growth. The second and third will help us find out if growth was driven by sounder domestic macroeconomic conditions rather than by trade.

To measure these other policy changes, we take differences in five indicators between the early 1975–1989 and late 1990–2004 periods:

- The change in the average Edwards (2007) measure of financial openness
- The change in the average Chinn-Ito (2008) measure of financial openness
- The change in the standard deviation of log (PWT 6.2) consumer prices
- The change in the standard deviation of log (PWT 6.2) nominal exchange rates
- The change in the standard deviation of (PWT 6.2) government spending shares

The results are shown in table 5. Panel A reports results based on the discrete treatment model of table 1. The basic specification in column 1 includes lagged growth, which was always significant in table 1, and the liberalizer dummy based on average capital and intermediate tariffs. Columns 2 to 7 add each of the other policy controls one at a time, and finally all at once. None of the other policy controls is

ever significant, either singly or jointly, and the coefficient on the trade liberalizer dummy is stable, robust, and always significant at the 1% level, in the range 0.81% to 0.97% extra growth per annum, consistent with our previous results.

For completeness, panel B repeats the exercise with the continuous treatment model of table 3, where trade liberalization is measured by the change in the log of 1 plus the average of capital and intermediate tariffs, and again lagged growth is always significant. Column 8 is the basic specification, and columns 9 to 14 include added controls. In columns 11 and 12, there is very weak evidence that growth was enhanced to some degree by increased monetary stability as measured by the volatility prices or exchange rates, but these effects are significant only at the 10% level and they vanish in column 14 when other controls are added. None of the other policy controls is ever significant, either singly or jointly, and the coefficient on the trade liberalizer dummy is stable, robust, and always significant at the 1% level, an elasticity that takes values in the range of −0.415 to −0.499, consistent with our previous results.

We take these results to show that just as growth accelerations in the trade liberalizer group could not be attributed to changes in institutions and schooling, they cannot be attributed to favorable changes in financial openness and macroeconomic policies either.

H. Robustness: Sensitivity to Sample Choice and Trade Structure

We performed many robustness checks on our results, but space does not permit the inclusion of all of them. Some appear in the working paper version. Two are shown in the online appendix.

The first check (appendix table 1) asks whether the core result—the coefficient on the liberalizer variable, whether discrete or continuous—is driven by the mix of developed and developing countries. We find (panel A) that the results are robust when the sample is restricted to the developing countries. Unfortunately this lowers the sample size to 31 countries for our disaggregated tariff data, so precision is lost. But these coefficients are not significantly different from the core results in tables 1 and 2. In another check (panel b), the full sample is used as in tables 1 and 2, but the log level of GDP per capita is added as an other control variable to capture differences due to the level of development. Again, the coefficients are not significantly different from the core findings.

The second check (appendix table 2) looks at whether the core results are robust when the liberalizer variable is interacted with a measure of the capital or intermediate goods intensity of imports. One prediction of the model is that the growth impact of liberalization should depend on how much a country is using trade to access imported inputs. Unfortunately, the scarcity of disaggregated import data (we use the date 1987 in the middle of the sample period)

TABLE 5.—CONTROLS FOR OTHER POLICIES
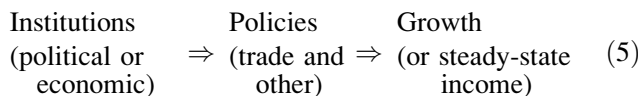DEPENDENT VARIABLE: DIFFERENCE IN GROWTH, $\Delta growth$

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	A. Discrete measure of trade liberalization						
Liberalizer dummy	0.0095***	0.0087**	0.0097***	0.0086**	0.0084**	0.0096***	0.0081**
	(0.0034)	(0.0034)	(0.0034)	(0.0034)	(0.0034)	(0.0034)	(0.0037)
Growth lagged	−0.487***	−0.460***	−0.466***	−0.429***	−0.427***	−0.485***	−0.414***
	(0.086)	(0.088)	(0.089)	(0.092)	(0.092)	(0.087)	(0.098)
Δ financial openness (E)		0.00014					0.00011
		(0.00012)					(0.00015)
Δ financial openness (CI)			0.00170				−0.00002
			(0.0020)				(0.0027)
Δ inflation volatility				−0.00250			0.00048
				(0.0016)			(0.014)
Δ exchange rate volatility					−0.00252		−0.00264
					(0.0016)		(0.014)
Δgovernment spending volatility						−0.00036	−0.00024
						(0.00084)	(0.00088)
R^2	0.49	0.51	0.50	0.52	0.52	0.50	0.53
Observations	47	47	47	47	47	47	47
	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	B. Continuous Measure of Trade Liberalization						
$\Delta \ln(1 + tariff)$	−0.0382**	−0.0344**	−0.0393**	−0.0356**	−0.0349**	−0.0391**	−0.0326*
	(0.015)	(0.016)	(0.016)	(0.015)	(0.015)	(0.016)	(0.017)
Growth lagged	−0.499***	−0.471***	−0.479***	−0.433***	−0.430***	−0.497***	−0.415***
	(0.087)	(0.089)	(0.091)	(0.093)	(0.093)	(0.088)	(0.099)
Δ financial openness (E)		0.00014					0.00010
		(0.00012)					(0.00016)
Δ financial openness (CI)			0.00171				0.00010
			(0.0021)				(0.0028)
Δ inflation volatility				−0.00278*			0.00407
				(0.0016)			(0.014)
Δ exchange rate volatility					−0.00284*		−0.00652
					(0.0016)		(0.014)
Δgovernment spending volatility						−0.00043	−0.00029
						(0.00086)	(0.00089)
R^2	0.48	0.49	0.48	0.51	0.51	0.48	0.52
Observations	47	47	47	47	47	47	47

See table 1. Standard errors in parentheses. Constant terms not reported. Significant at *10%, **5%, and ***1%.

brings the sample size to just 35 countries, but despite that, the results are still favorable, and the preferred specification (capital and intermediates, final column) still clears the 5% significance level in both the discrete and continuous estimates ($p = 0.045$ and 0.062, respectively).

V. Endogenous Treatment?

One reservation skeptical readers may still harbor is that our coefficient on the tariff measure may be subject to endogeneity bias. If institutions "rule," then tariff policy is just a symptom, not a cause, of better economic performance. This kind of relationship, at least in levels, is often summed up in causal diagrams in the following way:

$$\begin{array}{ccccc} \text{Institutions} & & \text{Policies} & & \text{Growth} \\ \text{(political or} & \Rightarrow & \text{(trade and} & \Rightarrow & \text{(or steady-state} \quad (5) \\ \text{economic)} & & \text{other)} & & \text{income)} \end{array}$$

Such diagrams may also be augmented by other causal arrows and other factors, such as geography (see similar diagrams in Acemoglu, Johnson, & Robinson, 2001, or Rodrik et al., 2004).

Based on this kind of causal logic and new empirical work, the institutions view has supplanted the policy view

in recent years. But while the causal relationship in equation (3) may sometimes find support in levels (cross section), it does not find support in differences (time series). We find there is no clear and robust relationship between institutional changes and trade policy changes in our sample, so it is hard to argue that fixing one trumps fixing the other: countries with "bad" (or worsening) institutions have managed to engage in trade reform; countries with "good" (or improving) institutions have also failed.

We therefore seek to construct better instruments for trade policy changes, based on different historical reasoning. Our new instruments fare much better and support the findings from the previous section. Indeed, they strengthen the findings, since the use of instrumental variable (IV) estimation has the added benefit of addressing problems of measurement error, which can be serious when using tariffs as a measure of liberalization (e.g., given the problem of unmeasured changes in quotas and other nontariff barriers).

A. Endogeneity, Round 1: Institutions as Deep Determinants

The recent literature argues that institutions are key determinants of income levels (Acemoglu et al., 2001). The

TABLE 6.—INSTITUTIONS AND TRADE POLICY: CORRELATION IN LEVELS BUT NOT IN CHANGES

Dependent variable	(1) ln(1+t), Lagged	(2) ln(1+t), Change	(3) ln(1+t), Change	(4) ln(1+t), Lagged	(5) ln(1+t), Change	(6) ln(1+t), Change
			A. All Countries			
Institutions, lagged	−0.415 (0.071)**	0.211 (0.068)**				
Institutions, change			0.159 (0.106)			
Political liberty, lagged				−0.148 (0.040)**	0.053 (0.370)*	
Political liberty, change						−0.054 (0.056)
R^2	0.28	0.11	0.03	0.14	0.03	0.01
Observations	91	83	83	84	79	74
			B. Developing Countries Only			
Institutions, lagged	−0.277 (0.123)**	0.231 (0.127)*				
Institutions, change			0.136 (0.126)			
Political liberty, lagged				0.019 (0.058)	−0.023 (0.060)	
Political liberty, change						−0.036 (0.069)
R^2	0.07	0.05	0.02	0.00	0.00	0.01
Observations	69	61	61	62	57	52

See table 1. Institutions measure is from Economic Freedom in the World (Area 2ab rescaled to 0-1). Political Liberty measure is from Freedom House (rescaled to 0-1). Developing countries are those with IMF IFS codes 186 (Turkey), and 199 (South Africa), plus all codes 200 and higher. Standard errors in parentheses. Constant terms not reported. Significant at *10%, **5%, and ***1%.

difficulty is that good institutions may cause higher income, but there may also be reverse causality as in equation (5). The same may also be true of other proximate determinants of growth such as policies. Researchers have sought creative sources of exogenous variation and creative chains of causation to perform IV estimation. Thus, settler mortality long ago is now a popular instrument for the quality of institutions today (Acemoglu et al., 2001); another widely used instrument is legal origin (Glaeser et al., 2004).[20]

These ideas have certainly advanced the rigor of levels accounting. But the problem for us is that these causal chains and instruments are not relevant once we change the experimental model as here to a difference estimator for medium-term growth accounting.

First, consider the instruments. Suppose (lagged) settler mortality predicts institutional quality in 1985 in levels; it does so for 2000 also. But taking first differences, we would find (to a first approximation) that the change in institutions (one of our regressors) is a function of the change in (lagged) settler mortality. But there is likely no change in lagged settler mortality, and certainly none in the available pre-nineteenth-century data; even if there were, it would not provide a plausible theory of institutional change over the fifteen-year period from 1985 to 2000. The same problem would arise if we switched to legal origin as the preferred instrument: legal origins do not change, so differencing them is not an option. Similar problems apply to latitude, disease environment, other geography variables,

religion, and so on. Time-invariant deep determinants may be useful for levels analysis but not for growth analysis using difference estimators.

The absence or irrelevance of deep determinants is troubling, but we should recall that the main concern about our pro–Washington Consensus results in tables 1 and 3 is that changes in trade policy might be endogenous and really just a proxy for improved institutions. If so, the Rodrik-Subramanian-Trebbi (2004) critique would bite, and the nonsignificance of our institutional change variables would just be the result of misspecification. We now show that even absent instruments, we can find little prima facie evidence that changes in institutions might really be driving everything.

Table 6 shows that institutions measured in levels might affect trade policies measured in levels. But the same is not true for differences. We focus on panel A with the full sample. In columns 1 and 4, the level of tariffs in 1985 as measured by ln (1 + t) is regressed on the levels of institutions in two ways: first, using our previous measure of economic institutions, or institutions-as-protection (EFW legal and property rights index), and then using a deeper measure of political institutions, or institutions-as-democracy (Freedom House political liberty index). The levels relationship is strong at the 1% significance level and consistent with the standard story: countries with better institutions had lower tariffs. The relationship also survives in a restricted developing country sample in panel B, but only for the EFW measure.

But for assessing the causes of policy change, we care about changes in tariffs. Columns 2 and 5 regress changes in tariffs (our variable of interest) on levels of institutions. The relationship is significant only in the latter case, and in both cases, it has a perverse positive sign. Better initial institutions were associated with smaller tariff reductions.

[20] To enter institutions into a horse race with trade volumes, settler mortality as an IV has been joined up with distance as an IV, the latter being the standard instrument for trade volume in the gravity framework (Rodrik et al., 2004).

However, if the widely used causal ordering, equation (3), is correct, it should hold in differences too, and then we should not really regress changes on levels. Differencing both sides would lead us to regress changes in tariffs on changes in institutions. If this yielded a robust association, then one might conjecture that the Washington Consensus tariff reforms were nothing but a symptom of deeper institutional changes that were—directly or indirectly—the main reason for accelerating growth rates. We find no support for that conjecture. Columns 3 and 6 show that institutional changes were not correlated with changes in tariffs in the manner suggested by a causal ordering such as equation (3). In both panels A and B the coefficients are small and statistically insignificant.

There is absolutely no stable or predictable relationship between levels or changes in institutions and changes in tariff policy. Perhaps this is not too surprising. The failure to find a change-change relationship could have been anticipated: during the Great Liberalization, as we have seen, tariff policies changed dramatically in many countries, but it is well known that institutions, in contrast, are highly persistent.

The failure to find a level-change relationship could also have been anticipated. It is a truism of contemporary political economy that we see trade reforms in a variety of institutional environments, under regimes with good and bad governance, and under dictatorships and democracies. The regressions are telling us that when the Great Liberalization experiment happened in the treatment group, it was not a biased sample of countries in terms of either the level or trend of institutional quality.

B. Endogeneity, Round 2: The Great Depression, GATT, and Reglobalization

Now we look in new directions for exogenous variation in 1980s and 1990s trade policy, since contemporary changes in institutions seem to have been mostly irrelevant.

Instead, we take the view that the main exogenous shock to trade policy in the last 100 years was the period of the so-called Great Reversal, from 1914 to 1945. Wars damaged trade, but even more damage was done by policy reactions during and after the Great Depression (Kindleberger, 1989; Estevadeordal, Frantz, & Taylor, 2003; Glick & Taylor, 2010). Policies were mediated via a multitude of political economy channels leading to a persistent protectionist environment after 1945 that was a far cry from the liberal world order of 1913. Tariffs were much higher than in 1913 in most places, and while almost nobody had seen NTBs (quotas) in 1913, they were in widespread use by 1945.

Into this autarkic scene came the postwar international organization charged with rebuilding a broken world trading system: the General Agreement on Tariffs and Trade (GATT) created in 1947 and succeeded much later, in 1995, by the World Trade Organization (WTO). GATT organized multiple rounds of multilateral bargaining to reduce tariffs among member states. In order to achieve tariff reductions under GATT, countries had to engage in a negotiation game where they exchanged proposed lists of tariff cuts with partners; if such cuts were then agreed, they were extended to all parties via the most favored nation (MFN) mechanism. But with most developing countries not taking any serious part in GATT until the Uruguay Round, and with very few of them engaging in major unilateral trade liberalization that was robust and enduring (the main exceptions being the East Asian NICs), the majority entered the Uruguay Round with tariff levels that could be traced back through a history of domestic postwar policymaking to the great shifts in trade policies in the 1930s.

How can we use this set of historical disturbances to the global economy to address the possible endogeneity of our liberalization treatment variables? We argue that a useful causal ordering from which we can extract an exogenous component for the speed and extent of tariff reforms in the 1980s and 1990s is as follows:

$$
\begin{array}{ccccc}
\text{Less} & & \text{Less intensity/} & & \text{Greater} & & \text{Greater} \\
\text{severe} & & \text{persistence} & & \text{reductions} & & \text{acceleration} \\
\text{Great Depression} & \Rightarrow & \text{of protection} & \Rightarrow & \text{in tariffs} & \Rightarrow & \text{in growth from} \\
\text{in the} & & \text{under postwar} & & \text{in the} & & \text{1975–1990 to} \\
\text{1930s} & & \text{political economy} & & \text{Uruguay Round} & & \text{1990–2005}
\end{array}
$$

$$(6)$$

In essence, we argue that the world was disturbed by the interwar shocks in such a way that beliefs changed and all countries moved away from liberal economic policies. But how far and how long they did so was in part decided by how much of an adverse shock they suffered in the Great Depression.[21] We now explain and defend this identification strategy and show how we construct two instruments called GATT Potential, which are good candidate predictors of both the ability and willingness of countries to lower tariffs in the Uruguay Round.

The first GATT potential variable is the interaction of an indicator of GATT membership in 1975 with the pre–Uruguay Round tariff level. These two factors were likely to promote tariff reduction, given the mechanics of negotiations under GATT. To see a big tariff cut, a country had to have high tariffs to start with (tariffs cannot be negative); a country also had to enter the Uruguay Round with a strong

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[21] A related argument is made by Buera, Monge-Naranjo, and Primiceri (2011). They construct and calibrate a Bayesian learning model whereby initial conditions in 1950 divide the world into closed and open economies. Beliefs about the growth performance under closed and open regimes evolve depending on what policymakers observe in their own country and in neighboring "similar" countries and subject to some frictions (political costs of making a policy change). This creates regional dynamics where some regions (East Asia) learn and open up faster than other regions (Latin America). But the model differs from our approach in that the 1950 prior for each group is flat, and there is no causal origin explaining the initial membership of the open and closed groups. In contrast, our argument is that 1950 beliefs and policies were related to the size of the adverse shock in the 1930s, and that shock left its mark in terms of countries' desire to adopt and persist with closed policies in the postwar period.

willingness to cut tariffs (which we proxy by 1975 GATT membership). In essence, the construction of this variable focuses on the second arrow in the causal ordering of equation (6), using the decision of countries to enter GATT earlier as being supposedly indicative of a deeper and historically determined inclination to liberalize.

Still, one problem with this instrument is that we might ask: Why were countries willing to enter GATT by 1975 and then cut tariffs in the Uruguay Round? The two decisions could have been correlated, and so our first variable may not be a valid instrument, as the exclusion restriction might fail. Perhaps some countries knew in 1975 that good trade-related growth opportunities had opened up for them. In that case, we might need a deeper historical determinant of attitudes to trade reform, which exploits the first causal arrow in the ordering of equation (6). Here we rely on arguments from the political economy literature—concerning the long-lasting effects of the Great Depression on policymakers—that are familiar but have been rarely used as sources of exogenous variation in contemporary economic policies.[22]

Writing in midcentury, Polanyi (1944) argued that the Great Depression marked a turn away from the market and a return to a "natural" state of the world where markets were embedded in a social order. In his view, the liberal, laissez-faire era of the long nineteenth century was a historical aberration and the freewheeling globalization it spawned was not sustainable in the long run as a political-economic equilibrium. The interwar slump was the breaking point. As many have noted, the perceived failure of a free market system changed beliefs fundamentally and for decades to come; according to Stanley Fischer, one of the Washington Consensus protagonists,

> It is not hard to see why views on the role of the state changed between 1914 and 1945.... A clear-headed look at the evidence of the last few decades at that point should have led most people to view the market model with suspicion, and a large role for the state with approbation—and it did.[23]

However, if Polanyi and those sympathetic to his view expected the extreme autarky of the 1930s and 1940s to persist forever, they were to be disappointed.

From the 1950s to the 1970s, the global economy was gradually rebuilt under international cooperation, under the auspices of the OECD, GATT, IMF, IBRD, EEC, and a host of other acronyms. Still, this new construct was not necessarily the same as a return to the supposed laissez faire of the pre-1914 years. Trade barriers were dismantled only slowly. An influential characterization of the postwar era is that of Ruggie (1982), who argues that the persistence of postwar trade protection was part of a broader social con-

tract, an "embedded liberalism" where the market economy sat within a managed system that allowed policymakers to contain what they saw as the adverse distributional impacts and volatility created by unfettered free markets.

Moving forward in time, however, as Rodrik (2000) noted, this embedding seems to have weakened considerably as the next great globalization, or reglobalization, unfolded in the 1980s, 1990s, and 2000s. For some, this recent trend suggests a reversion toward a nineteenth-century free trading world, which would permit us to characterize the long sweep of the twentieth century as a liberal order in 1900, followed by a massive deglobalization shock that took the system toward protectionism, and which then slowly unwound as, at their own different speeds, countries gradually relaxed their autarkic stance and reembraced a more open posture by the year 2000.[24]

However, this crude description of a "temporary" deviation from laissez faire hides many details. What determined the action and re-action? Why did the speeds vary? What had been the impetus for that sea change in economic thinking, given the orthodox free trade views handed down by posterity through the long nineteenth century and up to the 1920s?

The conventional answer is: the Great Depression. The crisis of world capitalism generated a political economy response. However, if this argument is valid, we would not expect it to create an identical response in all countries, and we ought to be able to find enduring legacies of the Great Depression in subsequent postwar policy choices. Specifically, we test this Great Depression hypothesis to see if the depth of the 1930s downturn can serve as a predictor of slow trade liberalization later on, as in the causal ordering of equation (6) above. Why? Given the account of history by Polanyi, Ruggie, Rodrik, and many others, we would expect the embrace of globalization by developing countries not to be uniform but to be conditioned by their own history—with attitudes toward free trade ultimately mediated by politics, the power of interest groups, the persistence of beliefs, and so on.

Thus, our second GATT Potential instrument is calculated as the interaction of Great Depression intensity with our initial period tariff level. Since 1930s growth experience was far removed from 1980s and 1990s growth experience and related to many factors specific to that era, such as terms of trade shocks in the commodity lottery and the collapse of the gold standard, we have good reason to believe that the exclusion restriction will be valid for this instrument, there being no direct link from 1930s experiences to growth accelerations circa 1990.

We construct two instruments as follows for each country, which we think should be strong instruments for poten-

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[22] An illuminating exception is Siegler and Van Gaasbeck (2005), who find a cross-country correlation between the depth of the output trough during the 1930s Great Depression and the weight placed on output in a standard monetary policy Taylor rule in the Great Inflation of the 1970s.

[23] Quoted in Buera et al. (2011, p. 1).

[24] Prior to the nineteenth century, the role of the market is also hotly disputed, with some historians seeing a dominant role for mostly free markets back to the medieval period. See, for example, the arguments of Greg Clark and Fred Block at http://economistsview.typepad.com/economistsview/2008/06/polanyis-the-gr.html.

TABLE 7.—TWO-STAGE (IV) REGRESSIONS

	(1)	(2)	(3)	(4)
	First Stage $\Delta \ln(1 + t_{Capital\&Intermediate})$	Second Stage $\Delta growth$	First Stage $\Delta \ln(1 + t_{Capital\&Intermediate})$	Second Stage $\Delta growth$
Dependent Variable				
A. Change in Tariffs Instrumented by GATT Potential 1 (Based on 1975 GATT Membership)				
GATT Potential 1	−0.517***		−0.548***	
	(0.072)		(0.067)	
$\Delta \ln(1 + t_{Capital\&Intermediate})$		−0.0537**		−0.0482**
		(0.021)		(0.021)
Growth lagged	−0.0453	−0.496***	0.159	−0.524***
	(0.58)	(0.088)	(0.56)	(0.096)
Change in schooling			−0.00648	0.00055
			(0.018)	(0.0031)
Change in institutions			0.291***	0.0146
			(0.10)	(0.017)
R^2	0.56	0.46	0.64	0.50
Observations	44	47	44	44
B. Change in Tariffs Instrumented by GATT Potential 2 (Based on Depth of Great Depression)				
GATT Potential 2	−0.650***		−0.659***	
	(0.025)		(0.027)	
$\Delta \ln(1 + t_{Capital\&Intermediate})$		−0.0541**		−0.0507**
		(0.020)		(0.022)
Growth lagged	0.146	−0.394***	0.0942	−0.409***
	(0.20)	(0.11)	(0.21)	(0.12)
Change in schooling			0.00119	0.00079
			(0.0061)	(0.0033)
Change in institutions			−0.0447	−0.00697
			(0.035)	(0.018)
R^2	0.96	0.43	0.96	0.44
Observations	31	31	31	31

See table 1. The regressor $\Delta \ln(1 + t_{Capital\&Intermediate})$ is instrumented by the GATT Potential variable in columns 2 and 4. Standard errors in parentheses. Constant terms not reported. Significant at *10%, **5%, and ***1%.

tial for tariff reductions under the Uruguay Round of GATT between 1985 and 2000, the early and late periods in our study:

$$\text{GATT Potential 1} = \ln(1 + tariff_{1985})$$
$$\times [\text{GATT member in 1975}];$$

$$\text{GATT Potential 2} = \ln(1 + tariff_{1985})$$
$$\times \begin{bmatrix} \text{Average deviation of 1930–35} \\ \text{GDP level from 1929 level} \end{bmatrix}.$$

The former uses the Rose (2004) database for GATT membership dates. The latter uses the Maddison (2001) database for historical GDP data.

Reassuringly, our findings are robust when we use these measures of GATT Potential as an instrument, and table 7 reports IV estimates of our differenced growth regression using each of the two instruments, in panels A and B, respectively. The instruments appear strong. On a priori grounds, we thought the GATT Potential variables ought to be correlated with tariff cuts, and they are. Column 1 in each panel shows the coefficient and standard errors on the instrument in the first-stage regression where the dependent variable is the change in capital and intermediate tariffs. The t-statistics are high, around 7 or 8. If we are willing to accept the instruments as valid, they are certainly not weak.

Column 2 in each panel presents the IV estimates of the second-stage differenced growth regression on the full sample. The coefficients on tariff change are similar to and

never more than slightly larger than the OLS coefficients, suggesting that bias problems are not severe. Most important, the IV coefficients are significant at the 5% level, even in the restricted sample using disaggregated tariffs. As a robustness check, in columns 3 and 4 of each table, we add schooling and institutions variables, and the results are unaffected, with the tariff-growth elasticity still estimated at about −0.05 and significant at the 5% level. Identical coefficients obtain from regressions restricted to the developing country sample (not shown), but with lower precision.[25]

VI. Verifying the Import Channel

This study has focused on the reduced-form relationship between trade liberalization and growth implied by a simple two-sector open economy growth model. We have seen that empirical evidence from the post–Uruguay Round Great Liberalization experiment offers support for the model. Liberalizers grew faster than nonliberalizers. But is this finding fully persuasive?

Easterly (2005) warns that evidence of this sort should be carefully scrutinized to ensure that the structural linkages suggested by theory are also verified in the data; otherwise, the correlation of policy change and growth may be spur-

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[25] Precision suffers especially when we use the Great Depression instrument, because we have only $N = 16$ developing country observations in the Maddison GDP database for the period 1929 to 1935.

TABLE 8.—IMPORT VOLUME TRENDS: DIFFERENCE-IN-DIFFERENCE ESTIMATES

Dependent Variable	(1) ln(capital goods imports)	(2) ln(intermediate goods imports)
Post	0.29**	0.03
	(0.13)	(0.04)
Year	0.12***	0.08***
	(0.02)	(0.01)
Post × Year	−0.05***	−0.05***
	(0.02)	(0.01)
Post × Liberalizer	0.12	−0.02
	(0.19)	(0.06)
Year × Liberalizer	−0.02	−0.02
	(0.03)	(0.01)
Post × Year × Liberalizer	0.10**	0.04**
	(0.05)	(0.02)
R^2	0.94	0.97
Observations	1,073	1,083

Liberalizers and nonliberalizers are defined by above and below median change in capital and intermediates tariffs. See text. Pre is 1975–1989. Post is 1990–2004. Standard errors in parentheses. Constant terms not reported. Significant at *10%, **5%, and ***1%.

ious. To that end, table 8 looks for changes in the trends in disaggregated import volumes that we would expect to see given the purported mechanism. This provides a direct test of the theoretical channel from trade reform to growth as we have described it.

For this work, we collected annual data on disaggregated imports of each type of input for every country over the entire sample period. This required classifying every type of import into the same capital and intermediate bins as used in the tariff analysis, and then summing up to get a time series for total capital and intermediate inputs measured in a common currency, current U.S. dollars. The source for these import data was UN COMTRADE.

If our theory is correct about the import-growth linkage at work, we should see a divergent trend after the treatment for both capital and intermediate imports: the "post-trend" for the treated group of liberalizers should be higher than the "post-trend" for the nonliberalizers. To test for this, we take logs of the import levels for each type of input and regress them on a set of standard dummies and interaction terms. These terms are: a Post dummy to capture level shifts; a Year dummy to capture general inflation or other trends; a Post-Year interaction to capture shifts in such trends; a Post-Liberalizer interaction, to allow differential level shifts; a Year-Liberalizer interaction, to allow differential trends within the two groups; and finally—the key term–the Post-Year-Liberalizer interaction, a three-way term that captures any divergent trends after the experiment in the treated group relative to the control group.

The results in table 8 show a strong relative acceleration in imported inputs in the treatment group as predicted by our theory. In column 1, there is a step up in capital goods imports levels for the Post period (+0.29 log points, row 1). A strong Pre-trend (+0.12 log points per year, row 2) gives way to a milder Post-trend (−0.05 log points, lower, row 3); this shift is likely due in part to lower levels of general dollar price inflation in the Post period. The key term of

+0.10 in the final row shows the divergent Post-trend: capital goods imports grew 10% per year faster in the treatment group of countries (Liberalizers) as compared to the control group of countries (Nonliberalizers). In column 2, we find that similar results obtain for intermediate goods, where imports grew 4% faster in the treatment group relative to the control group. Both of these Post-Year-Liberalizer effects are statistically significant at the 5% level and provide evidence consistent with the view that a key channel was from trade liberalization, to imported capital and intermediate inputs, to growth.

VII. Conclusion

Despite the predictions of theory, it is now unfashionable to argue that lower trade barriers will make developing countries better off. The argument seems to have failed because little robust evidence has been assembled—a result of poor empirical design, data scarcity, and a focus on sample periods when little experimentation with trade policy actually occurred. We can overcome these obstacles through better empirical design, new data, and focusing on the watershed event in trade policy for developing countries, the Uruguay Round of GATT.

Our results show that there is quite strong support for the trade policy prescriptions of the Washington Consensus. The consensus claimed that lowering tariffs would promote growth in the developing world. Theory suggests a mechanism: lower tariffs will lead to cheaper capital and intermediate imports. The way to test the claim is after the fact, by looking at which countries took this "medicine" and how they fared relative to those that did not, using a classic treatment-and-control method to detect acceleration effects. The results run contrary to the view that trade liberalization has failed to deliver growth benefits. They also contrast with influential cross-country empirical work that documents a weak or nonexistent relationship between tariff levels and growth rates in the pre-1990 (pre–Washington Consensus) period. But these earlier studies were surely hobbled by omitted variables and certainly could not examine post-Uruguay, post-Consensus policy changes and economic performance. The Great Liberalization of the 1990s constitutes what is probably the great trade policy experiment of our era, and only now can we begin to evaluate its impact.

We have to be concerned not to oversell these results. Washington Consensus supporters have been faulted for expecting implausibly high impacts from their policy prescriptions. Based on our empirics, all we can say is that the impact of tariff reduction looks quite beneficial and has a plausible magnitude consistent with theory. The effects we find are not so large as to be dismissed as implausible, but at the same time, our effects are still large enough to make a nontrivial cumulative difference in outcomes over the longer run. An extra 1% of growth each year may not sound like a lot. It is surely small compared to what "institutional convergence" might deliver, by which poor-country pro-

ductivity levels could be raised to OECD levels—but there are few credible prescriptions to achieve that goal. Moreover, is there any other single policy prescription of the past twenty years that can be argued to have contributed between 15% and 20% to developing country incomes? To see the impact in a different perspective, we can also consider the growth needed to reduce poverty in accordance with the Millennium Development Goals. As noted by Kraay (2007), an extra 1% of growth is sufficient on its own to meet that goal in several developing countries, and it would make a contribution of between one-half and one-third to achieving that goal in many other countries. The impact is hardly negligible, and for the mass of people clustered near the poverty line, a 15% to 20% GDP boost over fifteen years will make a tangible difference.

Last, but not least, the results affirm a key point about trade policy in developing countries. It is the *structure* of protection, as much as its *level,* that matters for growth. Poor countries are net importers of capital goods, and most are net importers of intermediate goods. Demand for some goods, such as advanced equipment and machines, has to be satisfied by imports. Long ago, Díaz Alejandro (1970) pointed out that if you double the price of a machine via trade barriers, then you are placing an enormous tax on investment and accumulation that will depress output. Historical evidence accords with his view (De Long & Summers, 1991; Jones, 1994; Taylor, 1994, 1998). Consumption tariffs may have limited or ambiguous impacts on growth (welfare is another matter), but capital and intermediate tariffs impose a very clear cost on national efficiency. Recent trade liberalizations should take some credit for unwinding many of those inefficiencies from the 1980s to today. Where those barriers have dropped growth accelerations have been significantly higher than where barriers have remained. Some countries have reaped the benefits. More could yet do so and enjoy higher incomes and lower poverty rates, but this is less likely to happen if any new consensus says that trade policy does not matter very much.

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