

Is the Washington Consensus Dead? Growth, Openness, and the Great Liberalization, 1970s–2000s*

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Abstract

A centerpiece of the Washington Consensus was the claim that developing countries' growth performance would benefit from a reduction in tariffs and other barriers to trade. But a backlash against this view has built up and the now-prevailing view of recent comparative economic history suggests that trade policies have little or no impact on growth. If "getting policies right" is wrong or infeasible, this leaves only the more tenuous objective of "getting institutions right" (Easterly 2005, Rodrik 2006). However, the empirical basis for judging recent trade reforms is remarkably weak. Econometrics are mostly ad hoc and not model-based; trade policies are poorly measured (or not measured at all, as when trade volumes are spuriously used as a measure of policy); and the most influential studies in the literature are based on pre-1990 experience which predates the "Great Liberalization" which followed the GATT Uruguay Round. We address all of these concerns—by using a model-based analysis which highlights tariffs on capital goods; by compiling new disaggregated tariff measures to apply the model; and by employing a treatment-and-control empirical analysis of pre- versus post-1990 performance of liberalizing and nonliberalizing countries. We find evidence that a specific treatment, liberalizing tariffs on imported capital goods, did lead to faster growth, and by a margin consistent with theory. Changes to other tariffs, e.g. on consumption goods, appear to have relatively little effect, as theory would also predict.

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Does trade policy liberalization promote economic growth? The question has been central to recent 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.¹ At first, absent statistical evidence, this view had garnered support as practitioners looked back on the divergence in economic fortunes between the fast-growing outward-oriented East Asian economies and the sluggish inward-looking Latin American economies. Subsequently, a barrage of cross-country econometric studies seemed to lend weight to this view, including well-known works by Dollar (1992), Sachs and Warner (1995) and Edwards (1998). It was in this intellectual climate that tariff barriers fell in many developing countries all over the world in the “Great Liberalization” of the 1990s.

A decade later, the econometric respectability of the argument, and whatever consensus there might have been, appears lost. A new survey of the World Bank’s self-review *Learning from Reform* (2005) is entitled “Goodbye Washington Consensus, Hello Washington Confusion” (Rodrik 2006). Rodrik notes that some still believe that the problem was too little reform, rather than too much; but once one gets past the few hold outs (e.g., the IMF), it seems that if there is any sort of new consensus in Washington it is one where 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). The one-line summary on Rodrik’s website neatly sums up the current mood: “The Washington Consensus is dead. What will take its place?”

It would be a mistake to equate trade liberalization with the entire Washington Consensus; but, of the ten reforms in the original WC 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 WC package; and

¹ In fairness to Williamson (1990), his WC 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, 978) 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.

no doubt the other elements of the WC are uncontroversial—e.g., nobody is now arguing for fiscal indiscipline or insecure property rights.

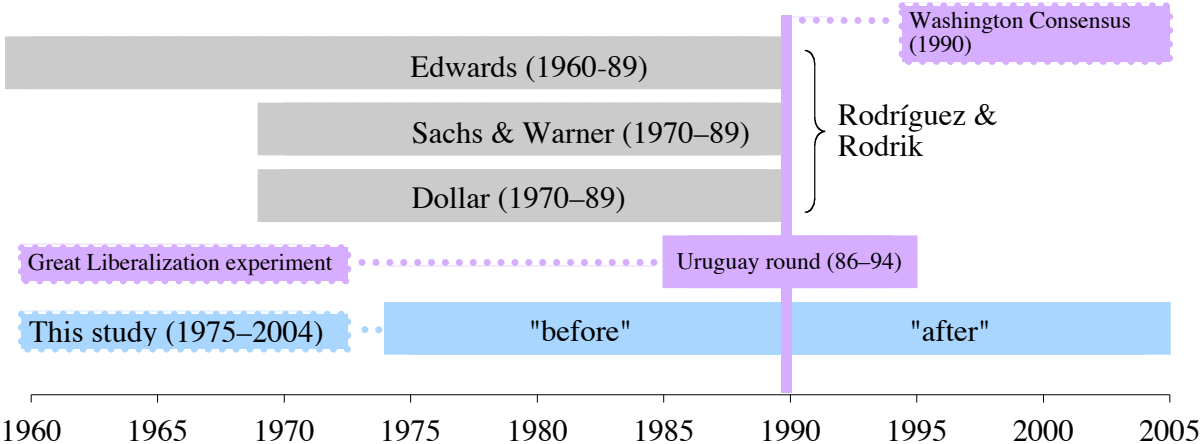
In the research arena, the openness-growth linkage has also attracted a vast amount attention, probably more than any other element of the Washington Consensus. It is also where the apparent reversal of sentiment has been most dramatic. Academic research has played no small part in this revolution. Rodríguez 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.² 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 and Levine 2003; Rodrik, Subramanian, and Trebbi 2004; Easterly 2005).

The latest findings might have laid to rest the debate about growth and trade policy, but in this paper we argue that the jury is still out. As noted by Rodríguez and Rodrik (2001), the 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 relatively easy to find results that would disappear under alternative assumptions or with a different set of controls. Fuzziness prevailed.

Of course, imprecision does not mean that there is no effect, only that our tests are weak and we have not yet been able to measure precisely enough to answer the question—and the coefficient estimates of Rodríguez and Rodrik (2001) are as subject to this critique as those of the earlier literature. Thus the tariff-growth literature has reached an apparent impasse. Unclear results meant that neither side can really claim victory and policy debates over the merits of a key element of the Washington Consensus are still unresolved. As far as trade policy is concerned, is it possible that the reports of the death of the Washington Consensus might be exaggerated?

² Rodríguez and Rodrik focus on the papers by Dollar, Sachs and Warner and Edwards because these papers were the most influential in terms of impact. For example, the citation counts as of February 2007 were: Sachs and Warner 1561; Edwards 610; Dollar 536. At the same date, Rodríguez and Rodrik had 921 citations, making theirs the dominant paper with the opposing viewpoint. Citation counts from scholar.google.com.

Can we make any 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 is that they all base their tests on data drawn from the period before 1990: a period *before* the Washington Consensus had even emerged, and before the “Great Liberalization” in trade policy in the 1990s had taken root in many developing countries—following the completion of the Uruguay Round and the rise of internal politics more sympathetic to free markets. The following timeline shows how the sample in our study differs from the earlier generation of studies:



We argue that now, more than 15 years after the end date of the 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 to 2005. And we ought to do so using sounder tests and harder data which actually include the 1990–2005 experimental period, rather than by extrapolating (out of sample) from fuzzy results based on fuzzy data from the 1970s and 1980s.

In this paper, we do just that—we document the “Great Liberalization” experiment and study its correlation with before-and-after growth outcomes in 1975–89 and 1990–2004. For sure, skeptical studies, notably Rodríguez and Rodrik (2001) and Easterly (2005), have set a higher bar for future empirical work, a bar which 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 are scant in terms of spatial and temporal coverage; the latter encouraged cross-section OLS estimation which is plagued by 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 trade policy measure that was directly controlled by policymakers;
- 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 outcomes, and appeared to be trumped by institutions.

Hence, in this paper we aim to confront all of these issues and develop an improved methodology for studying the tariff-growth question. We pay attention to theory and seek tariff measures that can be disaggregated into consumption and capital goods tariffs. We answer the right policy question, attend to inference and identification problems, and avoid biases by using the difference-in-difference approach. We painstakingly collect these tariff data from primary sources, using easy digital sources for recent years, but extremely cumbersome and hitherto unused archival sources for the 1980s. Based on this design we *do* find a significant correlation between trade liberalization and growth acceleration in the last 30 years. The estimated impacts are modest, but this is reassuring, for two reasons: they are plausible and they mesh with the predictions of a simple calibrated model. Finally, changes in trade policy turn out to be much more strongly correlated with changes in growth rates than changes in institutions or schooling.

In the next three sections we focus on three areas for methodological improvement relative existing cross-country studies. We first base our analysis on theory, using the most relevant, basic growth model where trade can play a role, and using simple calibrations to estimate what the plausible magnitude of the 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 biases and endogeneity problems common to the precious literature’s cross section methods.

1. Theory and Calibration

The question we seek to answer is: has tariff liberalization promoted growth in developing countries from the 1970s to the 2000s? Rather than start with an ad hoc econometric approach, we use theory to develop a suitable estimating framework.

A Model of Tariffs and Growth

It is now well accepted that any reasonable model of the tariffs and growth must be about more than the static gains resulting from the elimination of allocative inefficiencies (i.e., Harberger triangles): in any reasonably calibrated general-equilibrium model such gains are simply far too small to matter in this debate. Or, 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. Although the report does not quite put it in this way, what I think the authors have in mind is that market or government failures that affect accumulation or productivity change are much more costly, and hence are more deserving of policy attention, than distortions that simply affect static resource allocation.

For a theoretical growth model to be useful here it must include some basis for trade, that is, some difference in comparative advantage between developed and developing countries (aka, rich and poor, or North and South). The simplest production system for this purpose is the two-sector model of Mazumdar (1996). This model, a response to Baldwin (1992), features consumption and capital goods produced with Cobb-Douglas technologies with identical factor shares, so Hecksher-Ohlin/Stolper-Samuelson effects are absent and countries are completely specialized. Tariffs on imported capital goods can then be shown to retard growth and lower the steady state level of output—since the tariffs are a tax that distorts the relative price of capital (as in Jones 1994). Or, put another way, countries with a comparative disadvantage in producing capital goods will see the price of capital goods fall when autarky is replaced with free trade, and hence growth will speed up.

On the saving side, Mazumdar (1996) derives this result in both Solow and Ramsey frameworks, but the intuition is similar in each case. Several subsequent papers have used an analogous production structure. Some of these models focus exclusively on the steady state (e.g., Hsieh and Klenow 2003); but here, in the context of the debate over growth regressions, we will

be worried about transitional dynamics since any policy change will, ipso facto, put an economy out of steady state—assuming it was even in steady state to begin with.

We now briefly describe the Mazumdar (1996) model for the Solow case and explore some simple simulation exercises. We consider a small open economy in which sector $i=1$ produces consumption goods and sector $i=2$ capital goods, where the production functions are

$$y_i = A_i l_i^\alpha k_i^{1-\alpha},$$

y_i is sectoral output per worker, A_i is a sectoral productivity parameter, k_i is sectoral capital input per worker and l_i is sectoral labor input per worker. Since the factor shares α are the same in each sector, the factor intensities will be the same in each sector and also economywide, and must equal $k = K/L$ the aggregate capital labor ratio.

Suppose we are in an autarky steady-state where $k=k^*$, and both goods are produced competitively. The cost functions in each sector are identical up to a multiplicative constant given by the productivity parameter. Hence the autarky relative price of capital is given by

$$p^A = \frac{p_2}{p_1} = \frac{A_1}{A_2},$$

However, under free trade, the country will completely specialize, depending on the relative price in the world economy denoted p^* , according to comparative advantage

if $p^A > p^*$, the country specializes in consumption goods

if $p^A < p^*$, the country specializes in capital goods

Either way, in this Ricardian world, using world prices, the value of output (and hence of demand) of the country is higher under free trade than under autarky, as can be shown in a simple 2-good diagram as in Figure 1a.

In autarky, output corresponds to a point on the production frontier of the economy, which is a straight line KC with slope $-1/p^A$. The production frontier is also the consumption frontier, or budget constraint. Suppose the economy is at its steady state, and saves a fraction s of real

income. Geometrically, the line SX is drawn such that $OS/OK = OA/OC = s$. Thus, saving equals depreciation equals OS. Consumption equals AC.

Consider now the static and dynamic impact of a move to free trade when $p^A > p^W$, so the country has comparative advantage in the consumption good. The production frontier is still KC but the economy will specialize in producing consumption goods at point C, and will export consumption goods and import capital goods. The consumption frontier, or budget constraint, corresponds to a straight line K'C with slope $-1/p^W$, steeper than KC. Again, suppose the saving rate is s . Geometrically, the line S'X is drawn such that $OS'/OK' = OA/OC = s$. Thus, saving equals depreciation equals OS'. Consumption equals AC. OS' is larger than OS because of the gains from trade, so saving now exceeds depreciation.

In the Solow model this would be depicted as a shift up in the aggregate production function $f(k)$ (measured at world prices), as shown in Figure 2 by the shift from the autarky production function $f^A(k)$ to the free trade production function $f^T(k)$. The economy will then traverse to the new long-run steady state capital stock k^{**} and output y^{**} . The one time jump in output followed by the traverse means that trade liberalization leads to “medium run” growth in this case. This result also holds in a Ramsey model with optimized consumption behavior, as shown by Mazumdar (1996).

What about the static and dynamic impact of a move to free trade when $p^A < p^W$, so the country has comparative advantage in the capital good, as we might expect in a rich country? The situation is shown in Figure 1b. The production frontier is still KC but the economy will specialize in producing capital goods at point K, and will export capital goods and import consumption goods. The consumption frontier, or budget constraint, corresponds to a straight line KC' with slope $-1/p^W$, less steep than KC. Again, suppose the saving rate is s . Geometrically, the line SX is drawn such that $OS/OK = OA'/OC' = s$. Thus, saving still equals depreciation and equals OS. Consumption equals A'C and has risen. But the economy remains in a steady state with a capital stock k^* : there is no dynamic output response. The gains from trade are purely static and take the form of an immediate increase in consumption. Again, the result also holds in a Ramsey model as shown by Mazumdar (1996).

How Big is the Impact of Trade Liberalization? Some Simple Calibrations

The theoretical model provides a mechanism through which tariff reductions—in some countries, the capital importers—will promote growth in the medium term. But without further calibration, we know only the direction of this effect, not its likely magnitude. Much of the empirical openness-growth literature has proceeded without any guidance from theory as to the plausible size of the effect, but a more convincing argument could be achieved if empirical estimates were not merely robust but also in line with model-based predictions. To that end, we now do a simulation based on a calibration of the Mazumdar model. This provides a model-based estimate of the likely impact of tariff reductions on medium-run growth.

We make the following assumptions. The growth model is a Solow model, with a savings rate of twenty percent of nominal output. The production function is Cobb-Douglas in capital and labor, with a capital share of one third (Gollin 2002). The depreciation rate is ten percent per annum. Simulation begins with the country at its long-run steady state and a tariff reduction is imposed at time zero. We ask what happens in the model over the subsequent 10 to 15 years.

Suppose first that we are in Figure 1a and the country is in autarky, with the price of capital and consumption equal to 1, producing 80 units of C good and 20 units of the K good. GDP is 100 at domestic prices, with $OC=OK=100$. Saving OS is 20. Suppose as a result of trade liberalization the price of capital falls by 1% due to a now zero tariff on imported capital goods. Now the price of capital falls to the world price of 0.99, with the price of consumption still set equal to 1 as the numéraire. The PPF has now shifted out with OK' at a level 1% higher than OK. The country specializes at C and produces 100 units of C. Measured at world prices, autarky output was 99.8 (80 plus 0.99 times 20), but is now 100. The static gain is due to specialization: the 20% of capital goods in GDP can now be acquired 1% more cheaply through trade, for a gain of 1% times 20% equals 0.2%. In addition, OS' is at a level 1% higher than OS, so saving is now about 1% higher, and equals about 20.2 in capital goods units. In Figure 2, saving now exceeds depreciation and the economy grows. Numerical simulations show that this economy reaches an output level of 100.15 after 5 years, 100.25 after 10 years, and 100.33 after 15 years. In the long run, the economy raises its GDP to 100.50, for a total gain of 0.7% in GDP in steady state. In the first 10 years of transition, the average growth rate of the economy is 0.045% per annum, and over 15 years is 0.035% per annum (including the initial jump).

Measured as a tariff-growth elasticity, the model thus predicts a value in the range -0.035 to -0.045 on a 10–15 year horizon. In what follows we shall see whether the empirical estimates fall within the range predicted by the theory. It should be noted that in one respect this is probably an overestimate of real world gains, because it assumes that the PPF is straight. If the PPF were convex, then extent of (and gains from) specialization would be smaller since economies would not move immediately to the corner of the PPF.

How Big is the Impact of Trade Liberalization? Data Problems

Theoretical simplicity is not our only concern when making the jump from the model to the real world. There may also be data problems that lead us to a mistaken inference about the tariff-growth linkage. We can think of two potential difficulties with the data, although they work in opposing directions.

On the one hand, changes in tariff and nontariff barriers (NTBs) may be reinforcing, and we may then underestimate the gains associated with real world tariff reductions. Suppose every 1% cut in tariffs in the GATT rounds is also typically accompanied by an $X\%$ (unknown and unmeasured) cut in tariff-equivalent NTBs.

If X were 1, the total reduction in trade distortions would be twice the measured tariff reduction, and our 15-year tariff-growth elasticity ought to double from -0.035 to -0.07 . Also, the model focuses only on one channel through which removing tariffs can raise output and growth, but there may be other pro-growth impacts of trade reform that are not considered here.

On the other hand, trade liberalization may be accompanied by offsetting policies that undo the pro-trade effects of tariff reductions. An “obfuscation” policy may take the form of changes in “core” NTBs or in “hidden” NTBs such as regulations for environmental certification, health and safety, and so on. Thus, although a country might scale back tariffs, it could well implement a range of NTBs that make trade policy even less liberal.

This practice appears to be quite widespread, although the effects may be strongest in the richer democratic countries (Kono 2006). In what follows we shall test the theory on the full sample but also on a developing country subsample to ensure that inference is not affected by possible biases driven by obfuscation effects.

2. Data

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

Growth rate: The dependent variable is GDP per worker (rgdpch) from the Penn World Table (PWT) database (<http://pwt.econ.upenn.edu/>; version 6.2). Rates of growth on 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, for which we use the mean tariff rate from the Economic Freedom in the World (EFW) 2005 database (<http://www.freetheworld.com/>; variable: Area 4-A(ii) Data). Data are available every 5 years from 1970 to 2000, plus annually for 2001, 2002, and 2003, and the sample size grows from 77 countries in 1970 to 122 in the year 2000.

Disaggregated Tariffs: Since theory makes an important distinction between capital and consumption tariffs we go beyond existing measures and compile data on disaggregated tariffs. We compiled data on disaggregated Most Favored Nation (MFN) applied tariffs for two benchmark dates: “early” meaning circa 1985 and “late” meaning circa 2000. For the late benchmark we rely on pre-compiled tariff data by the UNCTAD’s TRAINS (Trade Analysis and Information System). However, for the earliest benchmark, TRAINS only covers 11 countries in our sample. Tariff data for a number of European Union countries are based on TRAINS complemented with EU Tariff Schedules. For the rest of the sample we had no option but to collect the tariff data by hand, line by line, from national tariff schedules from the 1980s. Tariff data for all Latin American Countries comes from national customs schedules provided by ALADI. For the rest of the sample we used published national tariff schedules available at the Library of Congress (Washington, D.C.). When all these data were collected, average tariffs were compute for capital and consumption goods using a concordance mapping and weights to include each tariff line in constructing an average measure. Further details of our tariff data are provided in the Appendix.

Trade Data: Import and Export data was obtained using the United Nations Statistic Division’s (UNSD) COMTRADE data via the World Integrated Trade Solution (WITS) query facility for the years 1984-1988 for the early benchmark and 1999-2003 for the late benchmark wherever possible. These data are used to determine capital importer-exporter status.

Other Controls: In the course of our empirical analysis we need to include appropriate 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. According to the EFW definitions, 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 now common in growth regressions (see Easterly 2005), we might also include a measure of human capital, here proxied by total years of schooling from Barro and Lee (2000) and a catch-up term, initial income per worker (from PWT) to control for transitional dynamics.

3. Empirics: Design and Implementation

In this section we present an empirical design which differs from the previous cross-section literature, but which is—we think—better suited to the policy question at hand. In this design, we consider post-1985 trade liberalization as a treatment. We implement the design using two different methods. The first method controls for policy openness as a discrete (zero-one) treatment in the spirit of Sachs and Warner (1995), and uses a difference-in-difference estimator. The second method controls for openness as a continuous treatment, following most of the literature, using tariff policy measures as a proxy for policy openness in a regression in differences. Difference estimators 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.

We therefore turn to a discussion of the possible endogeneity of the treatment variable. We find no evidence that trade liberalization was correlated with institutional variables (in levels or differences). However, we do find that treatment is correlated with an instrument we construct called “GATT Potential.” This variable is constructed to be a predictor of both the *ability* and *willingness* of countries to lower tariffs in the Uruguay round—and it is defined as the interaction of tariff history (pre-Uruguay round tariffs) and the pressure to make a deal in GATT (either GATT membership in 1975 or a proxy for diplomatic pressure). These factors were likely to promote tariff reduction, given the politics and mechanics of trade negotiations under GATT. To see a big tariff cut, a country had to have high tariffs to be able to offer to cut them by a large

amount (tariffs cannot be negative); a country also had to enter the Uruguay round with a strong willingness to cut tariffs (which we proxy either by GATT membership at the time of the preceding Tokyo round or by diplomatic pressure).

On a priori grounds these GATT potential variables ought to be correlated with tariff cuts, and they are. They are not weak instruments. They are also, we argue, likely to be driven by political and historical factors uncorrelated with the error term in the growth regression and, hence, they are also valid instruments. Reassuringly, our findings are robust when we switch to IV estimation using this measure of GATT potential as an instrument.

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 noted by Rodríguez and Rodrik (2001), leaves little hope of a definitive conclusion—save the meager refuge offered by a null hypothesis of no effect.

In contrast, we think the right question is: did the rate of growth rise more in liberalizing (treatment group) countries as compared to nonliberalizing (control group) countries? This way of looking at the question has numerous benefits. It corresponds most closely to the policy question being asked by developing country governments before liberalizing, and it also corresponds most closely to the Washington Consensus claim that liberalizers would grow faster than they would have without liberalization.

The new way of looking at the question also leads to a much cleaner empirical design. It naturally leads to an estimation of growth rates in differences, which is immune from omitted variable bias arising from time-invariant characteristics; all country-specific fixed effects in the growth equation are swept away by differencing. A naïve 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. However, as in the cross section literature, we know that endogenous variables should not be included as regressors. The variables to be included should be either (a) exogenous or (b)

endogenous and properly instrumented. And of course this requirement applies to our treatment variable as much as any other control.

However, this new approach of studying trade liberalization as a treatment problem also raises the stakes empirically. To put it simply, if this kind of “experimental-style” design is going to work then, at the very least, there has to be some sort of experiment: enough developing countries need to receive the liberalization “treatment” in sample, and then enough time needs to elapse so that their growth can be observed in both pre- and post-treatment phases, and this in turn has to be compared to a control group on nonliberalizers. And here is the problem: before the Uruguay round almost no developing countries had engaged in any serious trade liberalizations, bar a few famous examples. Most of the fall in tariffs prior to the Uruguay round was achieved in the developed countries, these being the main participants in earlier GATT rounds or in other serious regional trade agreements (notably the EU) that fostered lower tariffs. The Kennedy round of 1962–67 included only 48 countries; the Tokyo round of 1973–79 included roughly 100 countries, including 20 non-GATT developing countries, but progress on reducing developing country trade barriers was limited. In contrast, the Uruguay round 1986–94 included 125 countries and focused strongly on tariff reductions in both developed and developing countries.

Figure 3(a) sums up what the Uruguay round achieved for the reductions in average tariff levels (EFW data). To show what happened we plot post-Uruguay (year 2000) tariffs against pre-Uruguay (year 1985) tariffs. And we divide the world (i.e., our sample) into two groups of countries as follows:

Nonliberalizers: This group of countries did not (or could not) lower tariffs. Hence, there are really two sets of countries hiding with this group.

The first set inherited low tariffs and left them low and very nearly constant. These countries are close to the origin in Figure 3(a) and they are countries that never received the treatment because they have always been open. For example, in 1985, a very open economy like Singapore had tariffs as low as 2.2%, which obviously left very little room for further substantive tariff reductions after 1970. (Tariffs in Singapore fell to zero in 2000.) Clearly, the trade policy element of the Washington Consensus did not speak to this group of countries: they had practically converged to free before the Uruguay round and without any nagging from the

Beltway. Thus, no growth accelerations induced by trade policy could be expected in these early liberalizers 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 3(a) 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 and 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 any attention to it.

Liberalizers: In contrast, a third and final group of countries both *could* and *did* lower tariffs after 1985. They had large tariffs to begin with and cut them. These countries are below the diagonal on the lower right of Figure 3(a) and they are countries that received the treatment because they made a big move from being closed towards being 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, who also embarked on trade liberalization in the 1980s and 1990s. 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-WC trade policies placed an enormous “tax” on imports (including imports of capital goods) that was subsequently removed. The question is, did this group see any pay offs in terms of improved growth performance?

Figure 3(a) shows that we have suitable control groups and treatment groups for the proposed experimental design. Figure 3(b) replicates the approach using our more limited sample of capital goods tariffs from our newly collected disaggregated tariff data. Again, we can partition the sample to isolate the “off diagonal” countries that embraced the Washington Consensus, and those on the diagonal which did not liberalize.

To further describe the evolution of tariffs in these samples, Figure 4 plots the average tariffs for different subsets of countries from 1975 to 2000 using EFW average tariff data. Figure 4(a) shows averages for the whole sample, developed countries and developing countries. The

developed world started with lower tariffs and lowered them a little (about 10% falling to about 5%), but that the developing world lowered tariffs more dramatically in the 1980s and 1990s (about 35% falling to about 15%).

Figure 4(b) shows average tariffs for the two aforementioned groups. The nonliberalizers saw very little movement in their average tariff rate: it stayed about 15% to 20% throughout. 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 WC-era, after the Uruguay round, these countries cut average tariffs to a much lower level, around 15%.

Exploiting this contrast between liberalizers and nonliberalizers should allow us to identify any pro-growth impacts of trade liberalization. Our theory would say that we should see such an effect, if we employ a suitable empirical design, the natural choice being a treatment-control study. The theory also predicts that the effect ought to be more strongly evident under the following two conditions: (a) the sample is restricted to capital-importing countries; and/or (b) the tariff measure is restricted to tariffs on capital goods.

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 those countries 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 zero-one openness indicator used by Sachs and Warner (1995) and their followers. Thus, if we suppose the levels regression is written

$$growth_i = \alpha[openness_i] + \beta X_i + \varepsilon_i,$$

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

$$\Delta growth_i = \alpha[\Delta openness_i] + \beta \Delta X_i + v_i.$$

By replacing $\Delta openness_i$ with our indicator variable “Liberalizer” we hope to capture those countries which, in the WC-era, shifted to a more liberal trade regime.

The resulting difference-in-difference (DD) regressions offer a very clean and simple test of the hypothesis that WC-era liberalizers 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 2005. By splitting the sample into two 15 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 (e.g., business cycles, 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 only have one pre-reform observation on tariff rates. Finally, as noted by Bertrand, Duflo, and Mullainathan (2004), restricting analysis to two periods “before” and “after” treatment may be a blessing in disguise, since DD methods with $T>2$ run the risk of biased standard errors.

In asking whether trade reform episodes were followed by increased growth rates, our method is close in spirit to the study of growth accelerations by Hausmann, Pritchett, and Rodrik (2005). We are able to employ more recent data (they use PWT 6.1, but PWT 6.2 adds 4 extra years). We also differ in using 15-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. 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 will find that *sustained* growth accelerations are correlated with economic reforms, although our focus is on trade policy—and ultimately a continuous measure, as opposed to the updated discrete Sachs-Warner indicator variable used by Hausmann, Pritchett, and Rodrik (2005).

Table 1(a) reports the results of these difference regressions. The dependent variable is the annual average growth rate of per worker GDP from the Penn World Tables, version 6.2. The first set of regressions in columns 1 to 4 include no control variables other than “Liberalizer”, but “Liberalizer” is defined in two ways. The first definition sets Liberalizer =1 for countries where the average tariff rate falls by more than the median for the entire sample between period 1 and period 2. The second definition is like the first, except that the tariff on capital goods is used, rather than average tariffs, since this is the better measure of trade policy suggested by theory.

We run these regressions first on the entire sample in columns 1 and 4.

Then, to more strictly adhere to the predictions of theory, we re-run them on the subsample of capital importing countries in columns 2 and 5; net importers of capital goods are identified

using disaggregated trade data for the earliest available year, which is 1987 (see the appendix). All but one developing country is thus identified as a capital importer—Taiwan is the exception—and the ratio of net exports to imports of capital goods is below -20% for all developing countries. With the exception of Brazil, Malaysia, Korea and Mexico it is below -50% . For most developing countries this ratio is between -90% and -100% . Their ability to produce and export tradable capital goods is very limited indeed, and the model's reliance on complete specialization with zero output of tradable capital goods turns out to be not so outrageous an assumption. Even for some developed countries like Australia and New Zealand (both "liberalizers," we should note) the above ratio was close to -90% , showing that even some developed countries can be highly dependent on capital goods imports, and, hence, subject to the tariff-growth predictions of the theory.

Finally, we re-run the regressions on a subsample of only developing countries in columns 3 and 6. We shall repeat this kind of test on the three different samples in all discussion of empirical results that follow.

This use of different samples may be desirable on at least two grounds. First, theory says the growth effect should be stronger in capital importing countries, so including capital exporters might bias the results against finding a tariff-growth linkage. Using either the capital goods export indicator or the developing country indicator allows us to screen out countries that have comparative advantage in capital goods. Second, we might have concerns about the validity of the parallel trends assumption across the full sample. What if rich and poor countries experienced systematic divergence (or convergence) in period 2 as compared to period 1? Given that fear, and since the WC claim about tariffs and growth pertained only to developing countries, ensuring the results are robust when the sample is restricted to just developing countries might seem like a worthwhile check.

The use of the subsamples, like the use of capital tariffs, is thus a priori desirable. The only downside is that, whilst the choice of capital tariffs and a restricted sample may be preferred a priori on theoretical grounds, either choice (and especially both choices together) sharply reduce sample size given data availability in the primary (our own) and secondary (EFW) datasets.

Coefficients and absolute t-statistics are reported in this and all subsequent tables. So too are indicators of levels of statistical significance. We are judging the WC claim that liberalization promotes growth. Since theory gives a one-tailed hypothesis (lowering tariffs should increase the

rate of growth) the appropriate tests for the liberalization coefficient are one-tailed. To avoid confusion, all significance levels are shown based on one-tailed tests.

Table 1(a), columns 1 through 6 offer some weak support for the Washington Consensus prescription. Liberalizing developing countries grew about 1.4% to 1.5% per annum faster than nonliberalizers in this period, as shown in columns 3 and 6. Using the entire sample, or the capital importer sample, mutes the effect to between 0.7% to 1.1% per annum, as in Columns 1 and 3. Also, consistent with theory, the effect is found to be slightly larger when using the “correct” capital tariff data and restricting the sample to developing countries (N=22), as in column 6.

In columns 1 to 3, the one-tailed test has a statistical significance level of at least 5% (1% in column 1). In columns 4 to 6, the smaller samples with capital tariff data lead to much less precision, and the one-tailed test has a statistical significance level of between 5% and 10%, although in column 6, with only 22 observations, the one-tailed p-value is very close to 5%.

Table 1(b), columns 1 through 6 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 income (log), initial schooling, and initial institutions. Clearly endogenous variables such as investment 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. These are now added to the Table 1(a) difference regressions. 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).

As Table 1(b) shows, the inclusion of these controls neither dramatically strengthens nor weakens the previous results. Columns 1 through 6 show that the liberalization indicator coefficient still enters significantly at the 10% level (and now at the 5% level in columns 1 and 4 where the samples are largest). The size of the liberalization effect is slightly muted, but still about 1% per annum in columns 3 and 6 for the developing country sample, and 0.6% to 0.8% for the other samples. Again, the difference between the developing country results and the others appears consistent with the hypothesis that developing country tariff cuts may have understated the actual change in protection due to the stronger comovement of tariffs and NTBs.

In column 6, the point estimate of the liberalization indicator coefficient is also slightly larger than that in column 3, suggesting that using the capital tariff measure as opposed to an average tariff measure can make a difference and the bias goes in the expected direction.

The coefficient on the lagged growth variable corresponds to the convergence speed, and since time is measured in 15-year periods, a coefficient of 0.57 in Columns 1 to 3 implies an annual convergence speed of almost 4% in log levels. In columns 3 and 4, this coefficient drops to 0.45–0.48, implying around a 3% per annum convergence speed. These are not unreasonable convergence speeds, certainly compared to the empirical literature and the Solow model (the typical Ramsey model calibration does lead to much higher convergence speeds, however). Schooling is not statistically significant in any regression. Institutional change is weakly significant in columns 1 to 3 using average tariffs, but in columns 4 to 6 using capital tariffs the coefficient collapses in size and in statistical significance.

Our empirics have already surmounted another hurdle, for as Easterly (2005, 1056) had shown, many earlier results in openness-growth literature proved not to be robust to the inclusion of the catch-up term (here entered in its 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 extant 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.³ We also find, in contrast to the levels results discussed by Easterly, 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 run. 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 tariff schedules, rather than inferring reform events based on an amalgam of aggregated tariff data, black market premia, government monopoly measures and so on (Easterly 2005, 1050). Our tariff variable may be narrow, but it is cleanly defined and measured.

³ High trade shares may result from a variety of factors. In the gravity model, for example, being small and proximate to large trade partners can lead to very large trade shares, even in the presence of large trade frictions. Trade can also be high in nonmarket economies: famously, East Germany had a very high ratio of trade to GDP; did East Germany pursue “open” trade policies? It is unfortunate that the Penn World Table persists in labeling its trade share variable “openness”—a name that seems to suggest to some a policy measure, leading arguments astray.

Still, we do not dwell further on these results, since, like the Sachs-Warner openness measure, our use of a dichotomous liberalization treatment indicator can be faulted 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 tariff rates provide a *continuous* treatment measure, and so we switch to using difference regression with the change in $\ln(1+\text{tariff})$ replacing the liberalization indicator variable as the measure of policy change.

Openness as a Continuous Treatment: Difference Estimates

Echoing Table 1(a), Table 2(a) now reports results using the continuous treatment measure $\log(1+\text{tariff})$ and (for now) no other controls, where the tariff measure is constructed one of three ways: for average tariffs (t), tariffs on capital goods (t_k), and tariffs on consumption goods (t_c).

In addition the sample is again varied in two ways—columns 1, 4, 7 show the corresponding results for the full sample; columns 2, 5, 8 the capital importer sample; columns 3, 6, 9 the developing country sample. For average tariffs from the EFW database in columns 1 to 3 the samples are again large (75 total, 70 capital importing, and 52 developing). For the hand-collected disaggregated tariffs the samples are smaller (35 total, 30 capital importing, and 22 developing), so again we may have little reason to expect precisely estimated coefficients in columns 3 through 9.

However, the findings for the small sample using capital tariffs in columns 4 to 6 appear almost as strong (in statistical significance) as the results for all countries in columns 1 to 3. In all cases the coefficient is statistically significant with t ratios well above 2. But for average tariffs in columns 1 to 3 the sample is at least twice as large, so this is yet more suggestive evidence that capital tariffs could be the more relevant measure of protection in this setting.

Buttressing this argument is the finding in columns 7 to 9 that consumption tariffs are seemingly irrelevant for growth outcomes. This is as our theory would predict—although reductions in consumption tariffs under GATT were probably correlated with reductions in capital tariffs, most likely leaving some spurious correlation present. Since average tariffs are “polluted” by having some weight on consumption tariffs, the results in this table are all of a piece. Using average tariffs we find a tariff-growth elasticity of -0.049 in developing countries in column 3, but this rises to -0.093 using the purer measure of capital tariffs in column 6. For the

full sample in columns 1 and 4, the elasticities are -0.052 and -0.059 . The best fit is in columns 4 to 6, with an R-squared as high as 0.21 in column 6; using average tariffs R-squared statistics are around 0.08, and for consumption tariffs smaller still, near 0.05

Following Table 1(b), Table 2(b) now reports results using the continuous treatment measure $\log(1+\text{tariff})$ with the additional controls comprising lagged growth, change in schooling and change in institutions. Compared to Table 2(a), the tariff coefficients are now reduced in magnitude and statistical significance, but the only control variable that consistently enters with statistical significance is, once more, the catch up term using lagged growth. Again, neither institutional change nor changes in schooling appear to have drive growth accelerations across these two periods. The catch up term again takes values in the -0.45 to -0.6 range which, for 15 year periods, again implies convergence speeds of 3%–4% per annum.

Turning to the main coefficient of concern, columns 1 to 3 suggest an average tariff-growth elasticity of near -0.025 , significant at the 5% level in full sample, and 10% level in the smaller samples. Columns 4 to 6 suggest a much larger capital tariff-growth elasticity of near -0.05 , also significant at the 5% level in full sample and capital importer samples, and at just the 10% level in the smaller developing country sample. This estimated elasticity is *consistent with the model-based estimate* of -0.035 to -0.07 , a figure based on the idea that some tariff cuts will also spill over into (unmeasured) NTB cuts of equivalent or smaller magnitude. Once again, we note that these are much smaller samples than in columns 1 to 3, so imprecision is to be expected. Again, consumption tariffs appear to be less relevant as seen in columns 7 to 9, where t-statistics are below 1.5 and coefficients small.

Summary of Core Results

What should we conclude? We think these results show that there is some support for the trade policy prescriptions of the Washington Consensus. The WC claimed that lowering tariffs would promote growth in the developing world. Theory suggests a mechanism: lower tariffs will lead to cheaper capital imports in capital importing countries. The only 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 didn’t. In other words, we need to employ a classic treatment-and-control comparison.

Perhaps surprisingly, our results run contrary to the widespread 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 tariffs levels and growth rates in the pre-1990 (pre-WC) period. That may have been the case then, but these earlier studies could not examine post-WC policy changes and economic performance. As Figure 4 shows, the Great Liberalization of the 1980s and 1990s constitutes 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. WC supporters have been faulted for expecting implausibly high impacts from their policy prescriptions. For example, the idea that a regression can tell us that such and such policy reform will solve world poverty has been heavily criticized—even by the person who ran the regression (Easterly 2005, speaking of Collier and Dollar 2001).

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 like some of the claims encountered in the WC debate, but at the same time our effects are still large enough to make a nontrivial cumulative difference in outcomes over the long run. Our model said that each 1% cut in tariffs leads to a long-run increase in GDP of 0.5%; so a large tariff cut as seen in our samples of say 40%, would, on its own, contribute 20% to the increase of GDP in the long run.

Is the cup half full or half empty? This 20% impact may not sound like a lot, and it is small compared to what “institutional convergence” might deliver, by which poor-country TFP levels could be raised to OECD levels. But who has a prescription for that? And what other single policy prescription of the last 20 years can be argued to have contributed +20% to developing country incomes?

4. Endogenous Treatment?

One reservation skeptical readers may harbor is that our coefficient on the tariff measure may be subject to endogeneity bias. If institutions “rule” than tariff policy is just a symptom, not a cause, of better economic performance. But if the effect we have measured is robust, then dismissal of the policy view in favor of the institutions could represent a false dichotomy. As we show in this section, there is no clear and robust relationship between institutions and trade policy reform 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. Moreover, when we then try to construct better instruments for trade policy changes, they fare much better and they support the findings from the previous section.

Endogeneity, Round 1: Institutions as “Deep Determinants”

The recent literature has argued that “institutions” are key determinants of income levels (Acemoglu, Johnson, and Robinson 2001). The only difficulty is that good institutions may cause higher income but there may also be reverse causality. The same may also be true of other proximate determinants of growth, such as economic policies. And policies and institutions may in turn affect each other, a circular causality. To escape, researchers have had to find 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, and these have been shown to have a causal effect on incomes (Acemoglu, Johnson, and Robinson 2001); so too is legal origin (Glaeser et al. 2004).⁴

These ideas have certainly advanced the rigor of “levels accounting”. But the problem for us is these causal chains and instruments are not relevant once we change the experimental model to a difference estimator for medium-term “growth accounting.” First, consider the instruments. Suppose (lagged) settler mortality predicts institutional quality in 1985 in levels; then it will do 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-19th century data; even if there were, we suspect it would not provide a plausible theory of institutional change over the 15-year period from 1985 to 2000. The same problem would arise more starkly if we switched to say, legal origin, as the preferred instrument: legal origins don’t change so differencing them isn’t an option. To sum up, time invariant “deep determinants” are useful for levels analysis, but inappropriate for growth analysis using difference estimators.

⁴ 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 standard instrument for trade volume in the gravity framework (Rodrik, Subramanian, and Trebbi 2004).

The absence or irrelevance of deep determinants is troubling, but we should recall that the main concern about our pro-WC results in Tables 1 and 2 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. We now show that, even absent instruments, we can find little *prima facie* evidence that changes in institutions might really be driving everything.

Table 3 suggests shows that institutions measured in levels might have affected trade policies measured in levels. But the same is not true for differences. We focus panel (a) with the full sample. In columns 1 and 4, the level of tariffs in 1985 (measured as $\ln [1+t]$) is regressed on the levels of “institutions” in two ways: first, using our previous measure of institutions-as-protection (EFW legal & property rights index); and then using a “deeper” measure of institutions-as-democracy (Freedom House political liberty index). The relationship is strong at the 1% significance level, and it is consistent with the standard story in levels—countries with “better institutions” had lower tariffs. The relationship also survives in the developing country sample in Panel (b), but only for the EFW measure and at only the 5% significance level.

Thus, levels of institutions correlate with levels of tariffs. But back in panel (a), columns 2 and 3 show that institutional data are not correlated with *changes* in tariffs in the manner suggested by the prevailing theories. Column 2 shows that changes in tariffs are negatively correlated with initial levels of institutions. But we shouldn’t really regress changes on levels. In a strict interpretation of the conventional wisdom, differencing both sides would lead us to regress changes in tariffs on changes in institutions and get a positive result: and if we did, this would support the view that the Washington Consensus tariff reforms were nothing but a symptom of deeper institutional changes that were probably doing all the work in raising growth rates. Unfortunately, column 3 offers no support for that conjecture. The sign is even wrong: positive changes in institutions were associated with *increases* in tariffs. The same holds in the developing country sample in panel (b).

Columns 4 through 6 in each panel repeat the exercises using the political liberty definition of institutions, but the results are if anything less robust. 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; 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 the level or trend of institutional quality.

Endogeneity, Round 2: World War II, GATT, and the Political Economy of Postwar Trade Deals

Now we look in new directions for exogenous variation, since institutions seem irrelevant and their deeper determinants unavailable in differenced form.

We take the view that the main exogenous shock to trade policy in the last 100 years was the period of the Great Reversal, from 1914 to 1945, when war and depression caused almost every country to retreat behind trade barriers (Kindleberger 1989). We feel pretty safe asserting that war was exogenous to the economic system, and though it certainly affected important economic outcomes for us, like trade levels, there is no evidence that there was any major reverse causation from trade to war (Glick and Taylor 2005). Thus, we take those great convulsions as exogenous, and the effect they had in each country was then mediated via a multitude of political economy channels leading to a protectionist environment in 1945 that was a far cry from the liberal world order of 1913. Tariffs were much higher than in 1913 in most places, and whilst 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) in 1947, succeeded much later, in 1995, by the World Trade Organization (WTO). GATT organized multiple rounds of multilateral bargaining to reduce tariffs among member states. As noted above, early GATT rounds mainly involved only developed countries. It was the Tokyo round of 1973–79 that expanded the involvement of developing countries, but it was not until the Uruguay round of 1986–94 that large tariff reductions were agreed in both developed and developing countries. 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” mechanism.

Thus, to capture exogenous variation in tariffs before and after the Uruguay round we propose to use the following variables:

- Tariff level in 1985. To offer a big cut in tariffs, the initial tariff must be big. Otherwise there is nothing to cut. We view this initial value as a legacy of previous GATT rounds, which means that in developing countries this is largely a legacy of the 1914–1945 Great Reversal episode (plus any subsequent non-GATT reforms). To the extent that countries ramped up protection in that pre-1975 era, we take this to be determined by political economy forces outside our 1975–2000 growth model. To be part of a valid instrument, this variable must be assumed orthogonal to the error term in the growth regression. This would be valid if we can assume that trade policy pre-1985 was set by political economy forces which neither systematically favored fast or slow growth trade policy regimes. This should be no surprise if we view trade policy in the real world as typically divorced from social welfare maximization, and driven by distribution; or, as Rodrik (1995) has put it: “Saying that trade policy exists because it serves to transfer income to favored groups is a bit like saying Sir Edmund Hillary climbed Mount Everest because he wanted to get some mountain air.” If distributional issues trump everything else in trade policy, then there is little a priori reason to think that the best interests of favored groups are subject to much reverse causality from expected future growth outcomes.
- Participation in Previous Tokyo Round of GATT. We employ an indicator if the country was a member of GATT in 1975, the middle of the Tokyo round (from Rose 2004). We take this variable as an indicator of a country’s willingness to engage in tariff reductions in the Uruguay round a decade later. Since GATT membership is largely an internal political matter, this would seem to be a good way to capture the willingness of countries to cut tariffs as a result of *internal pressure*. Is it exogenous for the purposes of our regression? It is certainly pre-determined, so the only concern would be whether early GATT members had systematically better growth possibilities. But if one takes the view that trade policy is set by political economy forces, then this decision in 1975 may be orthogonal to subsequent growth, depending again on who the groups are that are likely to be favored by GATT outcomes.

- External diplomatic pressure. In the event that our internal pressure variable is polluted by reverse causality, we sought a measure of *external pressure* for openness, based on the work of Rose (2007). He finds that diplomatic pressure, measured by the presence of more diplomats, or more embassies and consulates, seems to promote trade between countries. Especially for the G7 countries, with large and well-funded foreign service corps, and large markets as a bargaining chip, these projections of diplomatic power allow countries to secure better bilateral trade deals with partner countries. We reason that the same trade-promoting diplomatic pressures would be indicative of the probability, within multilateral GATT negotiations, of the same pair of countries to reach a deal on tariff reductions. For example, if the G7 has a lot of diplomats working on trade deals in Brazil, then we are assuming that they will also be more inclined to strike a deal with Brazil in the GATT round also. Of course, large diplomatic missions can exist for all kinds of noneconomic reasons, e.g. geopolitics, particularly in large countries. This makes the variable attractive since it is more plausibly exogenous with respect to the growth model. In particular, there is no a priori reason for G7 diplomatic presence to be correlated with future growth.

Using these three variables we construct two instruments as follows for each country, which we think should be strong instruments for potential for tariff reductions under the Uruguay round of GATT between 1985 and 2000, the early and late periods in our study:

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

$$\text{GATT Potential 2} = \ln(1 + \text{tariff}_{1985}) \times \left[\begin{array}{c} \text{Average Number of} \\ \text{G7 Embassies and Consulates} \end{array} \right].$$

The logic for these instruments is that in order to have the potential for a large tariff cut in the GATT round, a country had to enter with high tariffs *and* some pressure, internal or external, to lower those tariffs.

Tables 4 and 5 report IV estimates of our differenced growth regression using these two instruments. The first row in each panel shows the coefficient and t-statistics on the instrument in

the first-stage regression where the dependent variable is the change in tariffs. The t-statistics are high. If we are willing to accept that the instruments are valid, then they are certainly not weak.

The remainder of the table presents the IV estimates of the second-stage differenced growth regression. The results are sometimes weaker than the OLS results when the aggregated average tariff measure is used, as seen in Panel (a) of each table. However, when we use our newly collected disaggregated data for capital tariffs, the results remain strong (despite a small sample size) as shown in Panel (b) of each table. This conclusion holds even when additional controls are added.

Since we have proposed two candidate instruments we can also verify the exclusion restrictions by taking each one in turn as the instrument in the first stage, and then including the other as an additional regressor in the second stage. In all cases the exclusion restriction is satisfied and the candidate instruments enter the second stage with very low statistical significance (absolute t-statistics less than 1 and on average below 0.7). By this test, our candidate instruments do appear to be uncorrelated with the error term, as required.

The IV results bolster the theory that capital tariffs suppress growth in capital importing countries. The results for average tariffs only confirm the importance of adopting the correct measure of trade policy suggested by theory and then collecting the relevant data as necessary.

5. Corroborations

This study has focused on the reduced-form relationship between trade liberalization and growth implied by the simplest two-sector open economy growth model. 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 all of the structural linkages suggested by theory are also verified in the data, otherwise the correlation of policy change and growth may be spurious. To that end, Table 6 examines some correlated variables that we would expect to see changes as a corollary of the purported liberalization-growth mechanism. It is, by now, unnecessary to explain that we view all of these variables through the difference-in-difference framework: we examine the before-and-after levels in liberalizers and nonliberalizers and look for the right direction of change. The sample is restricted to capital importing countries, as per the theory.

Panel (a) looks at the PWT price index for investment goods. If the theory is correct then capital importers who liberalize should see the domestic price of investment goods fall relative to nonliberalizers. In a more general model, this effect may be muted due to the presence of nontraded goods in the bundle, but the direction of change should be the same. The results conform to theory: liberalizers saw their investment goods price index fall by 21 points relative to nonliberalizers (−17 versus +4).

Panel (b) looks at the investment to GDP ratio. In the Solow variant of the model this would be constant, by assumption. But in the Ramsey variant, a rise in the marginal product of capital (due to the fall in the “tax” on investment) should occasion an increase in the investment share. The same would also be true in an open economy model with internationally mobile capital and costs of investment: with saving and investment decisions separable, and with the marginal product of capital rising, investment would also rise. The results conform to theory: liberalizers saw their investment share rise by 1.5 points relative to nonliberalizers (−1.7 versus −3.2). Of course, both groups saw their investment shares fall, which might reflect a slowdown in global investment from one period to the next, especially for emerging markets (for many developing countries this was the end of an overborrowing period that culminated in an emerging market debt crisis in the mid-1980s). But the difference-in-difference cares not about factors common to both treatment and control groups, only their differences.

Panel (c) looks at the current account to GDP ratio. This variable is not relevant in the immobile capital model discussed earlier, but again we stress the predicted outcomes in an open economy model with internationally mobile capital. Positive news for investment and growth would, all else equal, increase investment. It could also decrease saving (due to intertemporal smoothing against higher future income). The results conform again: liberalizers saw their current account to GDP ratio fall by −0.9 points relative to nonliberalizers (+2.1 versus +2.9). Of course, both groups saw their current accounts rise after the end of the 1970s–1980s borrowing boom. But what is striking is that although nonliberalizers saw deficits (−1.8%) turn around into surpluses (+1.2%), the liberalizers managed to remain net recipients of capital (−3.7% rising to −1.7%). In an era when the United States in particular emerged as a major borrower, and when capital seemed to flow mysteriously “uphill” from poor to rich countries, the liberalizing countries in our sample stand out as a rare example of continued “downhill” capital flow, as theory would predict.

6. Conclusions

Despite the predictions of theory, it is now unfashionable to argue that lower trade barriers will make developing countries better off, via faster growth in the medium run and higher output in the longer run. 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 relatively little experimentation with trade policy actually occurred. We seek to overcome these obstacles by using a more suitable experimental design, collecting and assembling new data, and focusing on the contrast between pre- and post-reform periods that roughly bracket the watershed event in trade policy as it affected developing countries, the Uruguay Round of GATT.

Our results show that liberalizers have grown faster than nonliberalizers after this “experiment”—a result that survives in multiple specifications. Our findings are summarized in Figure 5, which shows growth accelerations for liberalizers and nonliberalizers. The level of GDP per worker is shown relative to the baseline trend for each group during the 1975–89 “before” period. Liberalizers clearly accelerated, whilst nonliberalizers stagnated. After 15 years, the difference between the two is about 0.12–0.15 in log levels, implying a differential acceleration of about 1% per annum in favor of the liberalizers, a result which duplicates the very first difference-in-difference results we saw in Table 1.

The results make sense and, without being oversold, they make a telling point about trade policy in developing countries. Most of these countries produce and export virtually zero traded capital goods; demand for these goods, such as equipment and machines, is satisfied almost wholly by imports. Long ago, Díaz Alejandro (1970) pointed out that if—like the Argentines—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.

The post–Uruguay trade liberalizations—and their intellectual underpinnings, whether we label them the “Washington Consensus” or not—should take some credit for unwinding many of those barriers from the 1980s to today. Where those barriers have dropped growth acceleration has been higher than where barriers have remained, no matter what the level or change in the quality of institutions. Some countries have reaped the benefits. Many more could yet to do so—but this may be much less likely to happen if any new consensus says that trade policy doesn’t matter.

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Table 1
Discrete Treatment Variable: Difference in Difference Regressions

(a) no controls

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	All	Capital importers	Devel- oping	All	Capital importers	Devel- oping
Liberalizer (average tariffs)	0.011 (2.48)**	0.010 (2.11)*	0.014 (2.01)*			
Liberalizer (capital tariffs)				0.007 (1.35)+	0.008 (1.32)+	0.015 (1.69)+
Observations	75	70	53	35	30	22
R-squared	0.08	0.06	0.07	0.05	0.06	0.12

(b) controls

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	All	Capital importers	Devel- oping	All	Capital importers	Devel- oping
Liberalizer (average tariffs)	0.007 (1.96)*	0.006 (1.61)+	0.009 (1.63)+			
Liberalizer (capital tariffs)				0.007 (1.72)*	0.008 (1.66)+	0.011 (1.54)+
Growth lagged	-0.574 (7.29)**	-0.567 (6.92)**	-0.566 (6.09)**	-0.482 (4.62)**	-0.464 (4.26)**	-0.458 (3.59)**
Change in schooling	-0.000 (0.13)	-0.001 (0.21)	-0.002 (0.44)	0.000 (0.06)	0.000 (0.07)	0.003 (0.56)
Change in institutions	0.027 (1.90)*	0.027 (1.83)*	0.027 (1.58)+	0.009 (0.51)	0.015 (0.72)	0.007 (0.28)
Observations	70	65	49	35	30	22
R-squared	0.57	0.57	0.61	0.48	0.49	0.51

Dependent variable is difference in the average change per annum of log GDP per worker

Period 1 is 1975–1989 and period 2 is 1990–2004

Tariffs are measured in 1985 and 2000 or closest date thereto.

Absolute value of t statistics in parentheses

+ significant at 10%; * significant at 5%; ** significant at 1% (one tailed test)

Table 2
Continuous Treatment Variable: Difference Regressions

(a) no controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All	Capital importers	Devel- oping	All	Capital importers	Devel- oping	All	Capital importers	Devel- oping
$\Delta \ln(1+t)$	-0.052	-0.049	-0.049						
	(2.72)**	(2.47)**	(2.16)*						
$\Delta \ln(1+tk)$				-0.059	-0.066	-0.093			
				(2.16)*	(2.16)*	(2.33)*			
$\Delta \ln(1+tc)$							-0.023	-0.023	-0.028
							(1.35)+	(1.26)	(1.23)
Observations	75	70	53	35	30	22	35	30	22
R-squared	0.09	0.08	0.08	0.12	0.14	0.21	0.05	0.05	0.07

(b) controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All	Capital importers	Devel- oping	All	Capital importers	Devel- oping	All	Capital importers	Devel- oping
$\Delta \ln(1+t)$	-0.027	-0.025	-0.024						
	(1.84)*	(1.63)+	(1.42)+						
$\Delta \ln(1+tk)$				-0.042	-0.045	-0.057			
				(1.82)*	(1.78)*	(1.54)+			
$\Delta \ln(1+tc)$							-0.020	-0.021	-0.022
							(1.34)+	(1.32)+	(1.05)
Growth lagged	-0.576	-0.565	-0.576	-0.453	-0.432	-0.419	-0.477	-0.457	-0.454
	(7.30)**	(6.88)**	(6.20)**	(4.26)**	(3.91)**	(3.17)**	(4.47)**	(4.10)**	(3.42)**
Δ schooling	-0.000	-0.001	-0.001	0.001	0.001	0.003	0.002	0.002	0.003
	(0.11)	(0.23)	(0.31)	(0.16)	(0.18)	(0.57)	(0.48)	(0.44)	(0.60)
Δ institutions	0.023	0.025	0.021	0.009	0.014	0.008	0.015	0.021	0.021
	(1.69)+	(1.71)+	(1.33)	(0.49)	(0.70)	(0.30)	(0.72)	(0.97)	(0.77)
Observations	70	65	49	35	30	22	35	30	22
R-squared	0.57	0.57	0.60	0.48	0.50	0.51	0.46	0.47	0.47

Dependent variable is difference in the average change per annum of log GDP per worker

Period 1 is 1975–1989 and period 2 is 1990–2004

Tariffs are measured in 1985 and 2000 or closest date thereto.

Absolute value of t statistics in parentheses

+ significant at 10%; * significant at 5%; ** significant at 1% (one tailed test)

Table 3
 Good Institutions and Liberal Trade Policy: Correlation in Levels but Not in Changes

(a) All countries

	(1)	(2)	(3)	(4)	(5)	(6)
	ln(1+t), lagged	ln(1+t), change	ln(1+t), change	ln(1+t), lagged	ln(1+t), change	ln(1+t), change
Institutions, lagged	-0.415 (5.86)**	0.211 (3.09)**				
Institutions, change			0.159 (1.49)*			
Political liberty, lagged				-0.148 (3.73)**	0.053 (1.42)*	
Political liberty, change						-0.054 (0.96)
Observations	91	83	83	84	79	74
R-squared	0.28	0.11	0.03	0.14	0.03	0.01

(b) Developing countries

	(1)	(2)	(3)	(4)	(5)	(6)
	ln(1+t), lagged	ln(1+t), change	ln(1+t), change	ln(1+t), lagged	ln(1+t), change	ln(1+t), change
Institutions, lagged	-0.277 (2.25)*	0.231 (1.82)+				
Institutions, change			0.136 (1.08)			
Political liberty, lagged				0.019 (0.32)	-0.023 (0.37)	
Political liberty, change						-0.036 (0.52)
Observations	69	61	61	62	57	52
R-squared	0.07	0.05	0.02	0.00	0.00	0.01

Dependent variable is change or lag of log(1+average tariff)

Period 1 is 1975–1989 and period 2 is 1990–2004

Tariffs are measured in 1985 and 2000 or closest date thereto.

Absolute value of t statistics in parentheses

+ significant at 10%; * significant at 5%; ** significant at 1% (one tailed test)

Table 4
IV Regressions part 1

(a) Using Average Tariffs

	(1)	(2)	(3)	(4)	(5)	(6)
	All	Capital importers	Devel- oping	All	Capital importers	Devel- oping
First stage						
GATT potential 1	-0.393 (5.02)**	-0.383 (4.69)**	-0.350 (3.71)**	-0.369 (4.79)**	-0.364 (4.50)**	-0.338 (3.57)**
Second stage						
Change in $\ln(1+t)$	-0.059 (1.62)+	-0.055 (1.42)+	-0.056 (1.17)	-0.032 (1.10)	-0.027 (0.90)	-0.027 (0.74)
Growth lagged				-0.570 (6.61)**	-0.562 (6.32)**	-0.573 (5.76)**
Change in schooling				-0.000 (0.10)	-0.001 (0.23)	-0.001 (0.30)
Change in institutions				0.024 (1.66)+	0.025 (1.65)+	0.022 (1.29)
Observations	75	70	53	70	65	49
R-squared	0.09	0.08	0.08	0.57	0.57	0.60

(b) Using Capital Tariffs

	(1)	(2)	(3)	(4)	(5)	(6)
	All	Capital importers	Devel- oping	All	Capital importers	Devel- oping
First stage						
GATT potential 1	-0.433 (5.53)**	-0.403 (4.87)**	-0.347 (4.06)**	-0.452 (5.87)**	-0.430 (5.04)**	-0.339 (3.44)**
Second stage						
Change in $\ln(1+tk)$	-0.094 (2.32)*	-0.104 (2.26)*	-0.127 (2.10)*	-0.065 (2.04)*	-0.069 (1.91)*	-0.094 (1.57)+
Growth lagged				-0.428 (3.87)**	-0.408 (3.53)**	-0.383 (2.67)*
Change in schooling				0.000 (0.04)	0.000 (0.13)	0.004 (0.70)
Change in institutions				0.012 (0.64)	0.016 (0.77)	0.004 (0.13)
Observations	35	30	22	35	30	22
R-squared	0.08	0.09	0.18	0.46	0.48	0.48

Dependent variable is difference in the average change per annum of log GDP per worker
GATT Potential 1 is "Tokyo" (GATT membership in 1975) times 1985 level of $\ln(1+\text{tariff})$
Period 1 is 1975–1989 and period 2 is 1990–2004
Tariffs are measured in 1985 and 2000 or closest date thereto.
Absolute value of t statistics in parentheses
+ significant at 10%; * significant at 5%; ** significant at 1% (one tailed test)

Table 5
IV Regressions part 2

(a) Using Average Tariffs

	(1)	(2)	(3)	(4)	(5)	(6)
	All	Capital importers	Devel- oping	All	Capital importers	Devel- oping
First stage						
GATT potential 2	-0.126 (5.08)**	-0.125 (4.88)**	-0.118 (4.03)**	-0.126 (5.18)**	-0.125 (4.96)**	-0.120 (4.11)**
Second stage						
Change in $\ln(1+t)$	-0.026 (0.69)	-0.024 (0.62)	-0.025 (0.52)	-0.053 (1.89)*	-0.052 (1.78)*	-0.056 (1.65)+
Growth lagged				-0.540 (6.15)**	-0.530 (5.84)**	-0.541 (5.32)**
Change in schooling				-0.000 (0.07)	-0.000 (0.16)	-0.001 (0.20)
Change in institutions				0.028 (1.90)*	0.030 (1.90)*	0.026 (1.52)+
Observations	74	69	53	69	64	49
R-squared	0.07	0.06	0.06	0.55	0.54	0.57

(b) Using Capital Tariffs

	(1)	(2)	(3)	(4)	(5)	(6)
	All	Capital importers	Devel- oping	All	Capital importers	Devel- oping
First stage						
GATT potential 2	-0.103 (4.73)**	-0.095 (4.19)**	-0.082 (3.42)**	-0.104 (4.72)**	-0.098 (4.22)**	-0.082 (3.45)**
Second stage						
Change in $\ln(1+tk)$	-0.062 (1.40)	-0.069 (1.37)	-0.090 (1.37)	-0.067 (1.83)*	-0.074 (1.79)*	-0.093 (1.56)+
Growth lagged				-0.425 (3.71)**	-0.402 (3.34)**	-0.383 (2.68)**
Change in schooling				-0.000 (0.01)	0.000 (0.09)	0.003 (0.69)
Change in institutions				0.012 (0.63)	0.016 (0.75)	0.004 (0.14)
Observations	34	29	22	34	29	22
R-squared	0.12	0.14	0.21	0.46	0.47	0.48

Dependent variable is difference in the average change per annum of log GDP per worker

GATT Potential 2 is diplomatic pressure (avg. number of G7 embassies and consulates) times 1985 level of $\ln(1+\text{tariff})$

Period 1 is 1975–1989 and period 2 is 1990–2004

Tariffs are measured in 1985 and 2000 or closest date thereto.

Absolute value of t statistics in parentheses

+ significant at 10%; * significant at 5%; ** significant at 1% (one tailed test)

Table 6
Other Correlates

(a) Price Index of Investment Goods (Penn World Table)

	Nonliberalizers	Liberalizers	Difference
Before	88.6	95.0	6.3
After	92.7	78.0	-14.7
Difference	4.0	-17.0	-21.1

(b) Investment to GDP ratio (percent, world prices, Penn World Table)

	Nonliberalizers	Liberalizers	Difference
Before	22.3	15.7	-6.5
After	19.1	14.0	-5.1
Difference	-3.2	-1.7	1.5

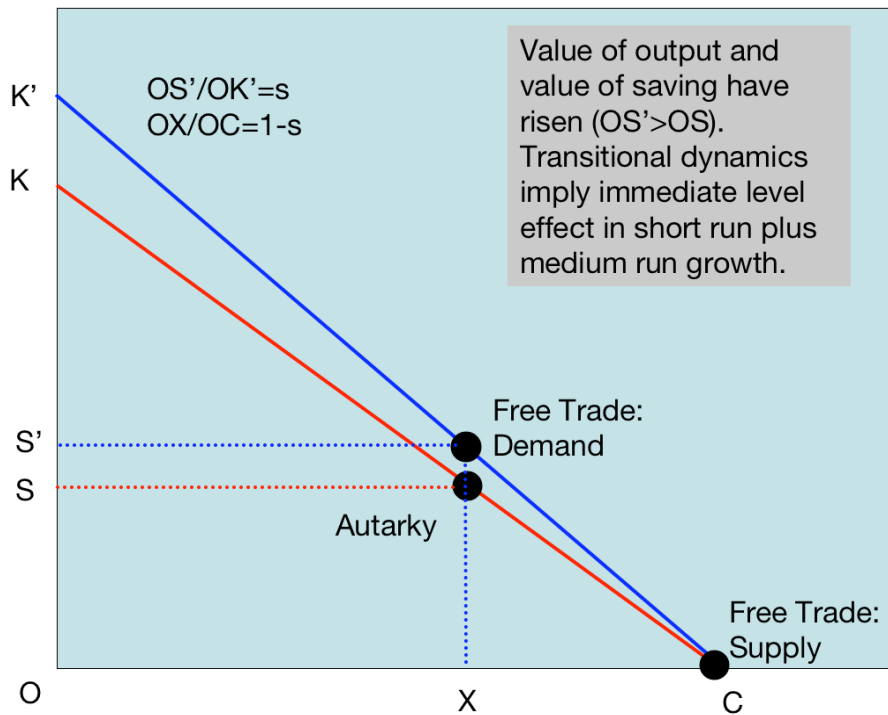
(c) Current Account to GDP ratio (percent, IFS)

	Nonliberalizers	Liberalizers	Difference
Before	-1.8	-3.7	-2.0
After	1.2	-1.7	-2.8
Difference	2.9	2.1	-0.9

Notes: Liberalizers and nonliberalizers defined by average tariff. Before is 1975–1989. After is 1990–2004.

Figure 1: Trade Liberalization in a Two-Sector Ricardian Model: Supply and Demand

(a) Capital Goods Importer



(b) Capital Goods Exporter

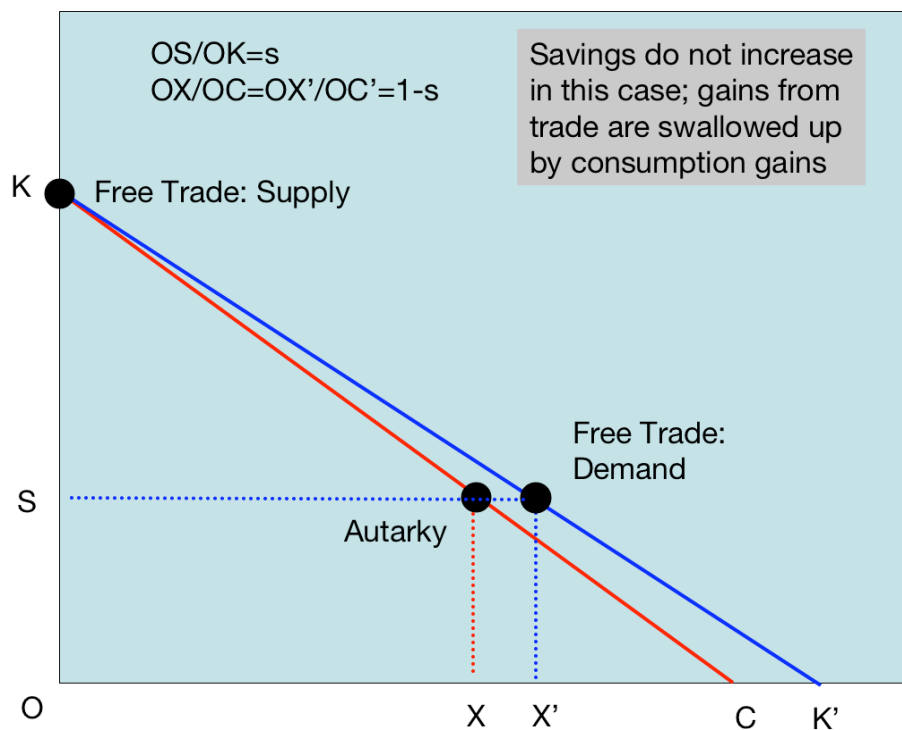


Figure 2: Trade Liberalization in the Capital Importer: Medium-Run Growth

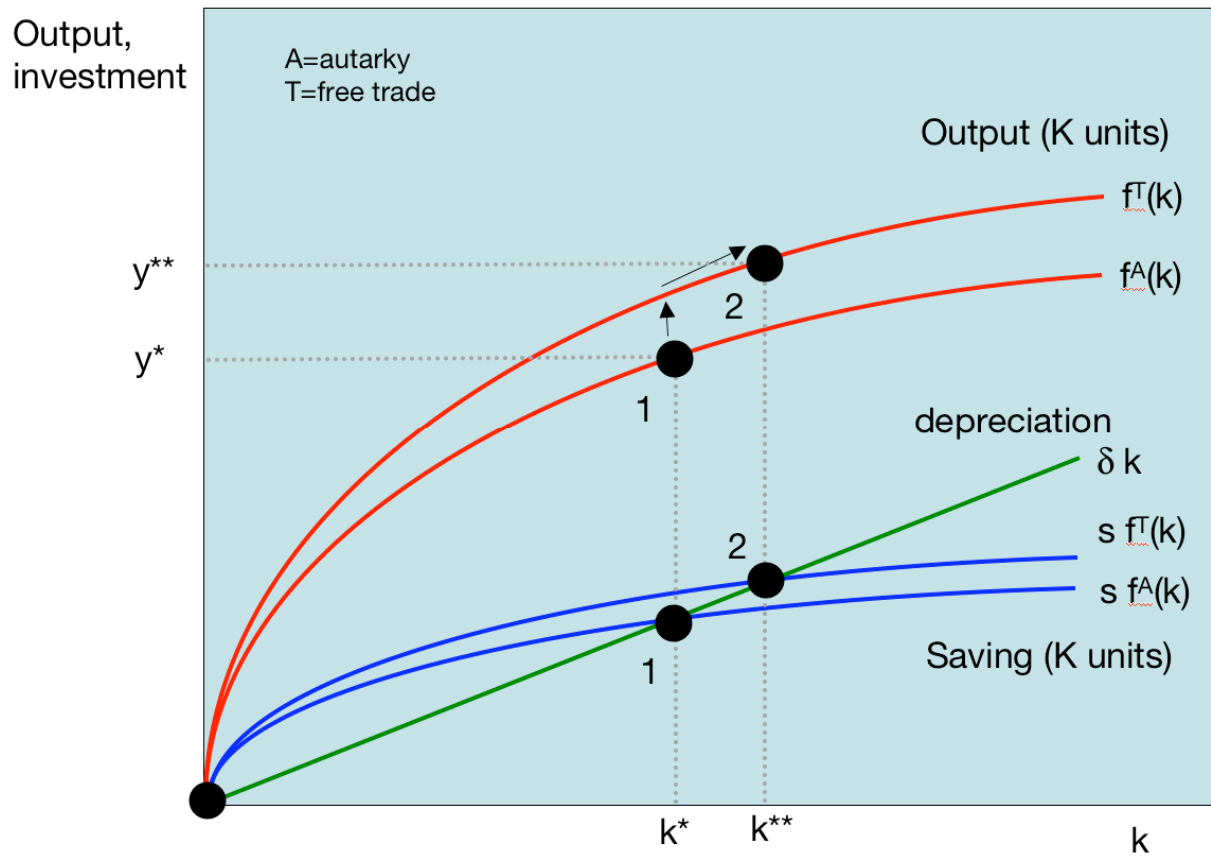
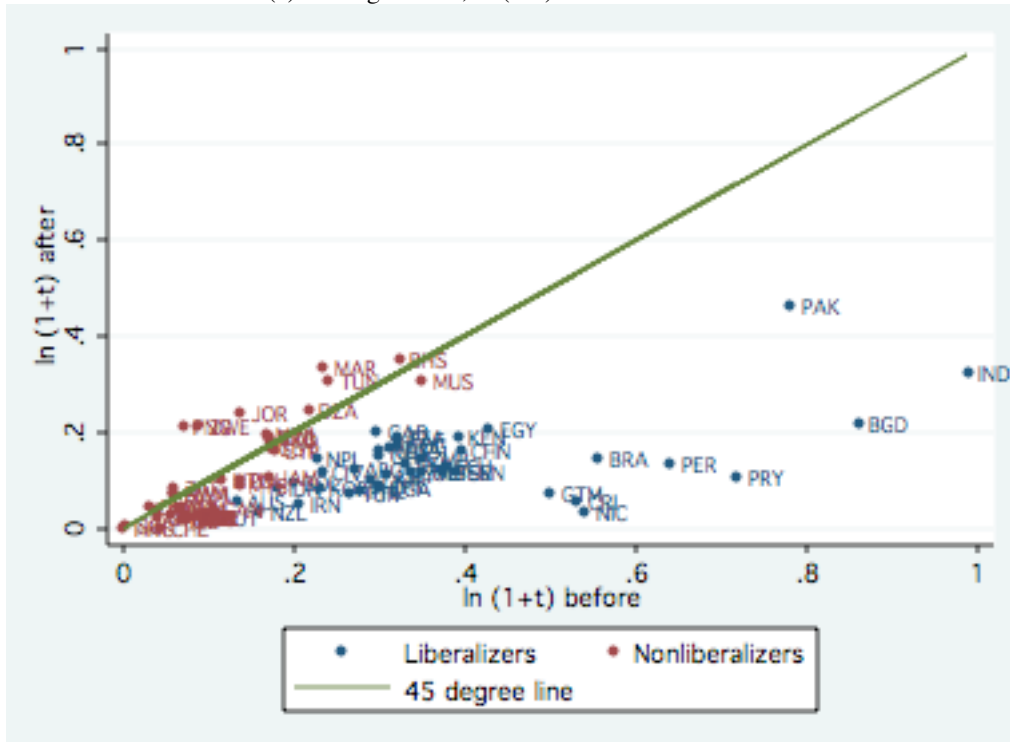


Figure 3: The Great Liberalization: Treatment and Control Groups

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



(b) Capital goods tariffs, $\ln(1+tk)$: after versus before

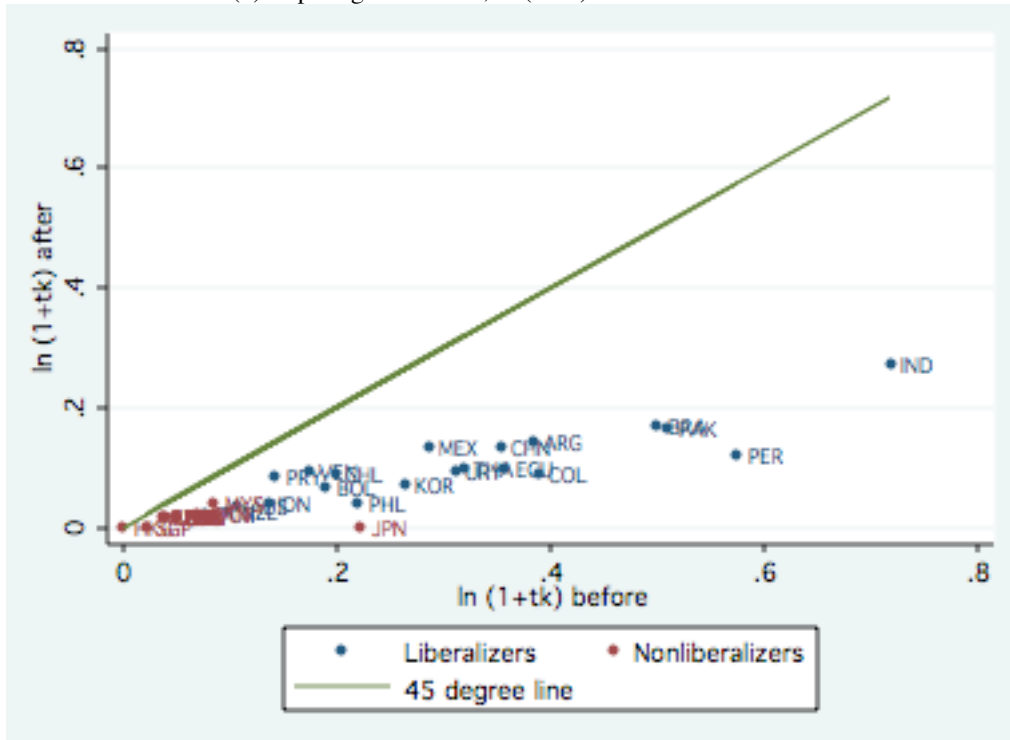
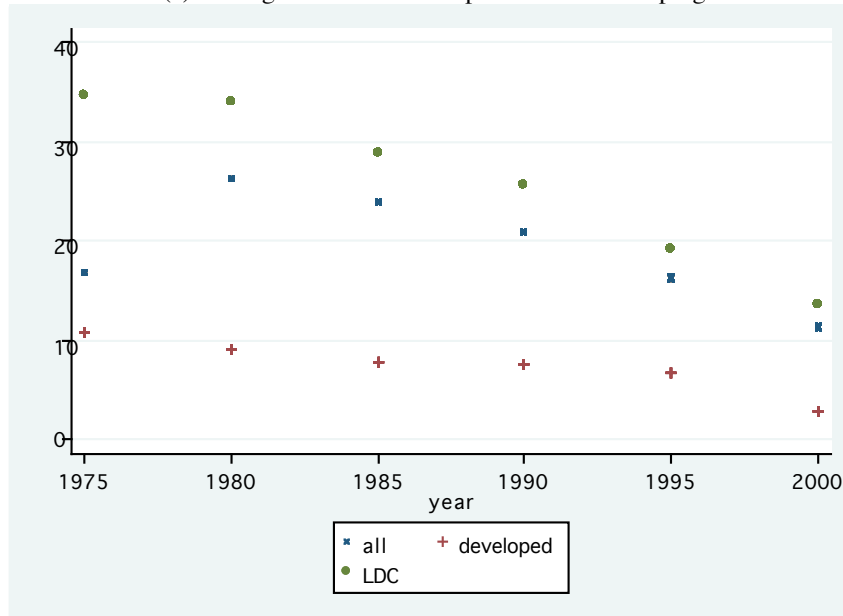
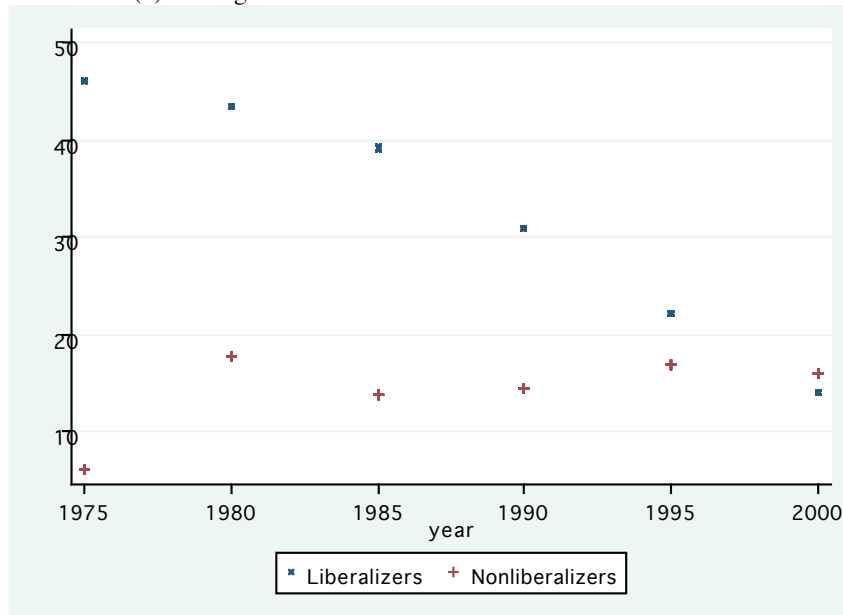


Figure 4: The Great Liberalization: Trends in Tariffs

(a) Average tariffs in Developed versus Developing



(b) Average tariffs in Liberalizers versus Nonliberalizers



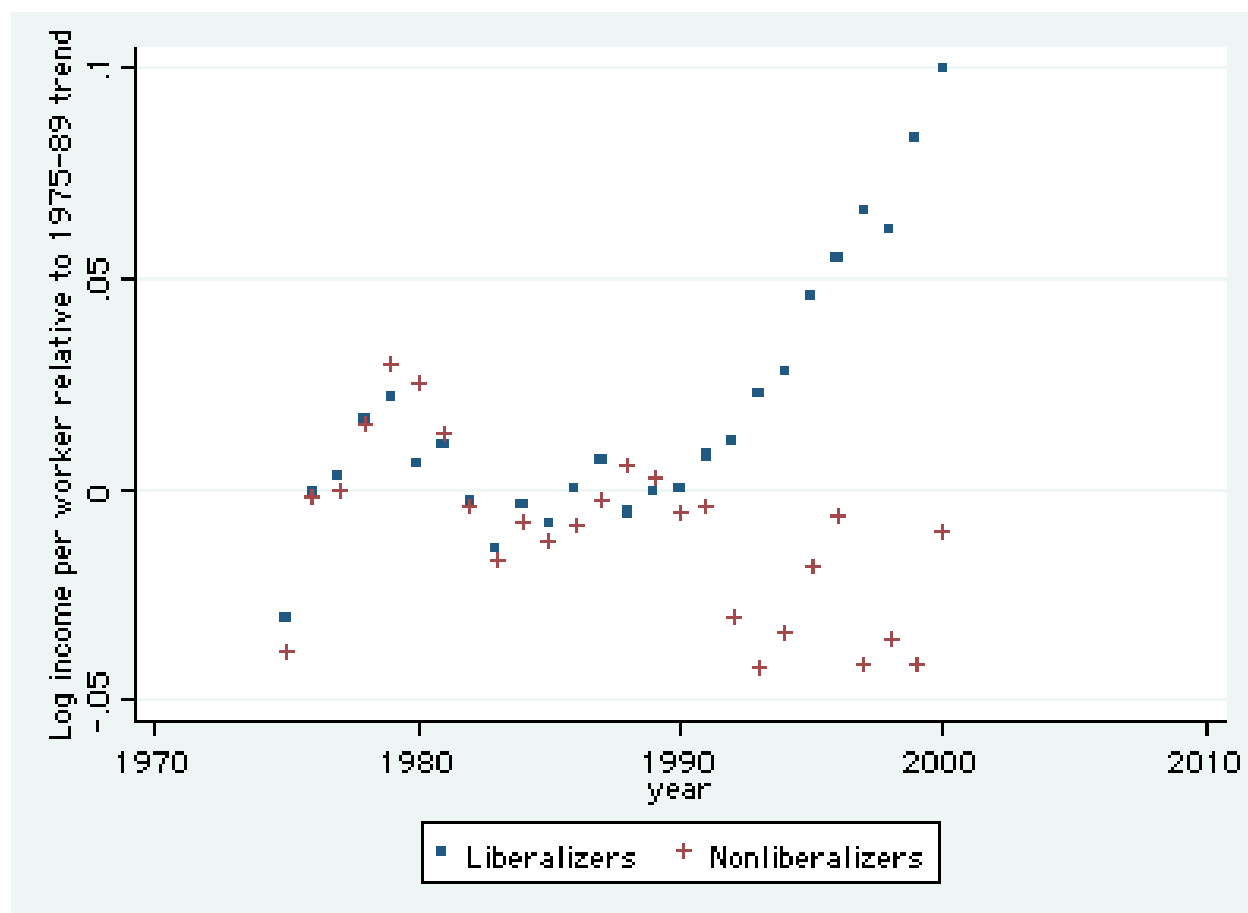
Notes: The samples are as follows (*=developing country).

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 Afr. Rep.*, Congo, Rep. Of*, Benin*, Gabon*, Ghana*, Cote d'Ivoire*, Kenya*, Mali*, Senegal*, Tanzania*, Uganda*, Zambia*, 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*, Unit. Arab Em.*, Hong Kong*, Malaysia*, Singapore*, Botswana*, Namibia*, Haiti*, Bahamas*, Barbados*, Guyana*, Jamaica*, Trinidad & Tob.*, Cyprus*, Jordan*, Algeria*, Malawi*, Mauritius*, Morocco*, Zimbabwe*, Tunisia*, Pap. New Guinea*, Poland*.

Figure 5: The Great Liberalization and Growth Accelerations

Log income per worker (PWT) relative to 1975–89 trend in liberalizers and nonliberalizers



Notes: The samples are as in Figure 4, based on change in average tariff.

Appendix

Summary

Appendix Tables 1 and 2 give details of sources and data for the early (ca. 1985) and late (ca. 2000) benchmark dates for our restricted 35-country sample.

Appendix Table 1 shows the countries in the sample, which are considered developing, and which are considered capital importers based on the sign of net capital goods imports for the first benchmark year with data for all countries, in this case 1987.

Appendix Table 2 shows the sources for disaggregated data on trade and tariffs for this sample of countries in the early (ca. 1985) and late (ca. 2000) benchmark years.

Detailed Methods and Sources on Disaggregated Tariff Data

Compilation of disaggregated tariff data is a key component of our empirical research strategy in this study. We use a newly constructed tariff dataset for 37 countries in two benchmarks periods: a pre-liberalization period (circa mid-1980s) and a post liberalization period (circa 2000). The sample includes: Argentina, Australia, Belgium-Luxembourg⁵, Bolivia, Brazil, Canada, Chile, China⁶, Colombia, Denmark, Ecuador, France, Germany⁷, Hong Kong (China), India, Indonesia, Italy, Japan, Korea (Republic of), Malaysia, Mexico, Netherlands, New Zealand, Pakistan, Paraguay, Peru, Philippines, Singapore, Spain, Sweden, Taipei (China), Thailand, Turkey, United Kingdom, United States, Uruguay, and Venezuela.

We use for both periods Most Favored Nation (MFN) applied customs duty. For the late benchmark we rely on pre-compiled tariff data by the UNCTAD's TRAINS (Trade Analysis and Information System). However, for the earliest benchmark, UNCTAD's TRAINS only covers 11 countries in our sample. Tariff data for a number of European Union countries are based on UNCTAD's TRAINS complemented with EU Tariff Schedules. For the rest of the sample we had no option but to collect the tariff data by hand, line by line, from national tariff schedules from the 1980s. Tariff data for all Latin American Countries (Argentina, Bolivia, Brazil, Chile, Colombia, Ecuador, Mexico, Paraguay, Peru, Uruguay, and Venezuela) comes from national customs schedules provided by ALADI.

For China, Korea, Pakistan, Philippines, Japan, Singapore, Taipei (China) we used (scanned and translated) published national tariff schedules available at the Library of Congress (Washington, D.C.):

- China: *The official Chinese customs guide*, General Office of the Customs General Administration of the People's Republic of China, 1985/1986.
- Japan: *Customs tariff schedules of Japan*, Nihon Kanzei Kyokai, 1985.
- Korea (Republic): *Korea customs law and regulations*, Korea Customs Research Institute, 1984.

⁵ Belgium and Luxembourg are treated as separate countries starting in 1999. Trade for both countries was queried separately and then aggregated for years 1998 and earlier.

⁶ 1987 is first year with truly reliable year trade data for China as earlier years lack reliable disaggregated SITC data.

⁷ East Germany (DDR) excluded from Germany for years 1990 and prior due to lack of compatible data.

- Pakistan: *Pakistan customs tariff and import trade guide*, Karachi Law Publishers, 1985-1986.
- Philippines: *Commentaries on the revised Tariff and Customs Code of the Philippines: P.D. no. 1464, as amended by Executive order no. 688*, Central Lawbook Pub. Co., 1983.
- Singapore: *Singapore Trade Classification and Customs Duties*, Trade Development Board (Singapore), 1985
- Taipei (China): *Customs import tariff of the Republic of China: revised edition, Jan. 1985.*, Hai guan zong shui wu si shu, 1985.

In general, the tariff data were obtained in national nomenclatures based on the Harmonized System nomenclature (HS). The data had to be averaged out into 6-digit HS Sub-headings, and then each 6-digit tariff line were linked to their corresponding codes using the UN Broad Economic Categories (BEC) classification by means of conversion tables for the 1988 and 2006 versions of the HS, also obtained from WITS. Finally, the BEC categories were mapped out to the System of National Accounts (SNA) categories to distinguish between capital, intermediate and consumption goods.

For the purposes of this research project we had to make some adjustments of the SNA categories. The concordance procedure for the early benchmark was especially difficult since it varied according to the source of the data. Tariff data for Latin American countries was usually provided in NALADISA nomenclature and it had to be correlated first into Standard International Trade Classification (SITC, Rev. 2), and then converted into BEC and SNA. Tariff data for other countries (national schedules) used generally the Customs Cooperation Council Nomenclature (CCCN), the predecessor to the HS. For these data, a conversion table had to be produced manually connecting the CCCN to the SNA categories via the SITC and BEC. In particular, tariff schedules for China and Pakistan were not fully consistent with the standard CCCN, so ad-hoc conversions and assumptions had to be made for these two countries.

Appendix Table 1: Restricted Sample and its Subsamples

Country	ISO code	Developing	Capital importer	Ratio of net exports to imports of capital goods (1987)
Japan	JPN	0	0	572.95%
Germany	DEU	0	0	97.91%
Italy	ITA	0	0	30.25%
Sweden	SWE	0	0	17.86%
Taiwan	TWN	1	0	16.79%
United Kingdom	GBR	0	0	0.70%
France	FRA	0	1	-3.57%
Denmark	DNK	0	1	-5.71%
Singapore	SGP	1	1	-7.21%
Netherlands	NLD	0	1	-12.73%
Belgium-Lux.	BLX	0	1	-14.78%
Brazil	BRA	1	1	-23.18%
Malaysia	MYS	1	1	-23.63%
United States	USA	0	1	-24.25%
Korea, Rep.	KOR	1	1	-30.24%
Mexico	MEX	1	1	-41.30%
Philippines	PHL	1	1	-53.25%
Canada	CAN	0	1	-53.52%
Hong Kong	HKG	1	1	-58.46%
Spain	ESP	0	1	-59.35%
Thailand	THA	1	1	-60.79%
Turkey	TUR	1	1	-76.29%
Argentina	ARG	1	1	-81.18%
India	IND	1	1	-83.44%
New Zealand	NZL	0	1	-88.54%
China	CHN	1	1	-91.25%
Australia	AUS	0	1	-91.29%
Pakistan	PAK	1	1	-95.20%
Colombia	COL	1	1	-96.08%
Uruguay	URY	1	1	-96.29%
Chile	CHL	1	1	-98.12%
Peru	PER	1	1	-98.36%
Venezuela	VEN	1	1	-98.99%
Indonesia	IDN	1	1	-99.02%
Ecuador	ECU	1	1	-99.51%
Paraguay	PRY	1	1	-99.97%
Bolivia	BOL	1	1	-99.99%

Appendix Table 2: Sources for Disaggregated Tariffs and Trade-Weight Data

Country Name	MFN Tariff Data ¹			Early Benchmark in TRAINS	Trade Data ²	
	Late Benchmark	Early Benchmark	Source for Early Benchmark		Late Benchmark	Early Benchmark
Argentina	2000	1985	NAT	1992	1999-2003	1984-1988
Australia	2000	1991	TRAINS		1999-2003	1984-1988
Belgium-Lux.*	2000	1988	TRAINS-EU		1999-2003	1984-1988
Bolivia	2000	1985	NAT	1993	1999-2003	1984-1988
Brazil	2000	1985	NAT	1989	1999-2003	1984-1988
Canada	2000	1989	TRAINS		1999-2003	1984-1988
Chile	2000	1985	NAT	1992	1999-2003	1984-1988
China***	2000	1985	NAT	1992	1999-2003	1987-1988
Colombia	2000	1985	NAT	1991	1999-2003	1984-1988
Denmark	2000	1988	TRAINS-EU		1999-2003	1984-1988
Ecuador	1999	1985	NAT	1993	1999-2003	1984-1988
France	2000	1988	TRAINS-EU		1999-2003	1984-1988
Germany**	2000	1988	TRAINS-EU		1999-2003	1985-1988
Hong Kong	1998	1988	TRAINS		1999-2003	1984-1988
India	1999	1990	TRAINS		1999-2003	1984-1988
Indonesia	2000	1989	TRAINS		1999-2003	1984-1988
Italy	2000	1988	TRAINS-EU		1999-2003	1984-1988
Japan	2000	1985	NAT	1988	1999-2003	1984-1988
Korea, Rep.	1999	1985	NAT	1988	1999-2003	1984-1988
Malaysia	2001	1988	TRAINS		1999-2003	1984-1988
Mexico	2000	1985	NAT	1991	1999-2003	1984-1988
Netherlands	2000	1988	TRAINS-EU		1999-2003	1984-1988
New Zealand	2000	1992	TRAINS		1999-2003	1984-1988
Pakistan	2001	1985	NAT	1995	1999-2003	1984-1988
Paraguay	2000	1985	NAT	1991	1999-2002	1984-1988
Peru	2000	1985	NAT	1993	1999-2003	1984-1988
Philippines	2000	1985	NAT	1988	1999-2002	1984-1988
Singapore	2001	1985	NAT	1989	1999-2003	1984-1988
Spain	2000	1988	TRAINS-EU		1999-2003	1984-1988
Sweden	2000	1988	TRAINS		1999-2003	1984-1988
Taiwan	2000	1985	NAT	1989	1999-2002	1984-1988
Thailand	2000	1989	TRAINS		1999-2001/03	1984-1988
Turkey	1999	1993	TRAINS		1999-2003	1984-1988
United Kingdom	2000	1988	TRAINS-EU		1999-2003	1984-1988
United States	2000	1989	TRAINS		1999-2003	1984-1988
Uruguay	2000	1985	NAT	1992	1999-2003	1984-1988
Venezuela	2000	1985	NAT	1992	1999-2003	1984-1988

Notes to Appendix Table 2:

1. Source of MFN tariff data is TRAINS for the late benchmark, and ALADI, TRAINS, and National Sources in the early benchmark. EU tariff data is disaggregated by product at a greater level of digits in TRAINS than trade data.

2. Source of trade data is COMTRADE, and the data was based on the SITC, Rev. 1 nomenclature. Trade Availability is reported only when both Imports and Exports are available.

* Belgium and Luxembourg are treated as separate countries starting in 1999. Trade for both countries was queried separately and then aggregated for years 1998 and earlier.

TRAINS = UNCTAD - TRAINS (Trade Analysis and Information System).

TRAINS-EU = The EU schedule of tariffs according to TRAINS.

NAT = National sources.

** East Germany (DDR) is treated as separate from Germany for years 1990 and prior; BEC and SITC Rev.1 leaf-level data is not available for DDR.

*** China 1987 trade data is first truly reliable year; earlier years in both SITC1 and SITC2 are missing disaggregated data, and disaggregated data do not sum to totals.