Center for Global Development

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economic growth and integration

By Andrew Warner

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This paper re-examines the evidence linking poor growth during the era of import substituting in-dustialization with trade restrictions. Recent work, notably Rodriguez and Rodrik (2000), asserts that all the evidence is fragile, implying that economists who believe that trade restrictions played a role in this poor performance do so largely on faith. This paper argues that this criticism ignores crucial evidence and tests regression specifications that are either not relevant for the question or are bound to have low statistical power. After revising and updating measures of trade openness used in Sachs and Warner (1995), the weight of the evidence argues that trade restrictions were indeed harmful to growth during this period. While there is certainly room for debate about how best to use the available data on trade restrictions, a large number of variants yield strong and consistent results. Furthermore, although some assert that it is impossible to empirically distinguish measures of trade restriction from other policies or institutions, the findings are not diminished even after controlling for many alternative eco-nomic policies or institutions.

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This paper re-examines the evidence linking poor growth during the era of import substituting industialization with trade restrictions. Recent work, notably Rodriguez and Rodrik (2000), asserts that all the evidence is fragile, implying that economists who believe that trade restrictions played a role in this poor performance do so largely on faith. This paper argues that this critcism ignores crucial evidence and tests regression specifications that are either not relevant for the question or are bound to have low statistical power. After revising and updating measures of trade openness used in Sachs and Warner (1995), the weight of the evidence argues that trade restrictions were indeed harmful to growth during this period. While there is certainly room for debate about how best to use the available data on trade restrictions, a large number of variants yield strong and consistent results. Furthermore, although some assert that it is impossible to empirically distinguish measures

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of trade restriction from other policies or institutions, the findings are not diminished even after controlling for many alternative economic policies or institutions.

1. Introduction

This paper revisits the issue of the impact of barriers to international trade on economic growth, referring especially to the 30 to 40-year global experiment with import-substituting industrialization between 1950 and 1990, when differences across countries in trade restrictions were truly dramatic. The paper argues that critics such as Rodriguez and Rodrik (2000), who assert that all previous evidence linking trade liberalization with faster growth during this period is fragile, have created a false impression. They do so in part by estimating inferior specifications of empirical models, which they then advertise inappropriately as checks of robustness. And they do so in part by adopting extreme positions on data choices, which then leads them to ignore crucial evidence. The result is that they base their claims on empirical specifications with low statistical power for testing the impact of trade restrictions on growth and development. This paper presents additional tests of the growth- openness relation based on specifications similar to Sachs and Warner (1995). The weight of the evidence argues that protection was very harmful to growth during this period.

It is helpful to start with a concrete example. Consider an enterprise that wishes to import foreign machinery. To calculate the real price incentives, one should include all factors that can impact the relative price of such machinery. These may well include fees at the border other than tariffs, fees associated with obtaining the currency of the country of production, bribes to officials for import licenses, transport costs and of course, the familiar barriers connected with tariffs and quotas.

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The point behind the specifications such as Sachs and Warner (1995), for example, is that data on traditional trade policy instruments such as tariffs and quotas only tell part of the story of protectionism. This fact is widely acknowledged in other discussions about trade policy, so why not also incorporate it in tests of the impact of protectionism on growth? Further, it is not hard to imagine examples in which alternative import barriers are perfectly equivalent to tariffs in their impact on import costs. Suppose the enterprise mentioned above could import foreign equipment at a cost of $100,000 and use it to deliver a revenue stream of 10,000 per year in domestic currency. If the exchange rate and the product price were normalized to 1, this investment would have an internal rate of return of 10 percent. If the government were to levy a ten percent tariff on imports, the internal rate of return would fall to 9.09 percent; exactly the same amount as if the government introduced restrictions on obtaining foreign currency and these served to create a premium of ten percent on obtaining foreign exchange through the black market. This is an example where tariffs and exchange controls would be perfect substitutes. Through this channel both tariffs and exchange controls can equally-well depress capital accumulation and growth. This, in a nutshell, is both the growth model and the argument behind empirical specifications such as Sachs and Warner (1995).

What are the implications regarding testing strategies? The main implication is that it is better to attempt to measure the different manifestations of protectionism and combine them rather than pretending that a single data series such as the average tariff adequately summarizes the state of affairs regarding protectionism. Imagine a world with four developing countries. The first follows open trade; the second maintains tariff levels of 100 percent; the third has no tariffs but a restrictive licensing system; the fourth has neither tariffs nor quotas but foreign exchange is only obtainable at twice the official exchange rate. The last three countries differ only in the

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form but not the fact of protection.

In such a world, regressions of anything on single measures of protectionism clearly have low-power for testing protectionism against alternatives. They have high power for testing for the impact of *specific forms* of protectionism but low power for testing for protectionism in general. The right regression for testing the impact of protectionism would seem to entail some aggregation of the policy instruments. In contrast, Rodriguez and Rodrik (2000), and Hanson and Harrison (1999) examine regressions that introduce trade policy variables one by one on the right hand side, and refer to individual coefficient estimates as though they are informative about the impact of protectionism overall. But clearly the estimated coefficients from such a regression are not informative about the effects of protection in general but rather the marginal impact of each of the policy instruments *holding constant the other policy instruments.*

This shift in the empirical specification makes all the difference in the world. Rather than a robustness test, it is a different empirical model, and one that suffers from a number of defects. The question about the impact of a particular policy instrument is second-order compared to the first-order question of the impact of protection. Further, the marginal impact of changes in any single policy variable should not be independent of the levels of the other variables. An extreme example would be a country with an inconvertible currency that prohibited its residents from holding foreign currency of any form. Tariffs and quotas could be anything in such a country and have no impact on imports. The average statutory tariff rate would be a meaningless indicator. In contrast another country in which the tariff were the only trade policy instrument, variations in tariffs would be highly relevant for protectionism. Finally, it is probably asking too much of the accuracy of trade policy data to estimate a second-order issue such as the marginal impact of policy instruments using cross-country data.

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Whatever these considerations suggest regarding proper specification, it is certainly not a one-by-one introduction of the various instruments of trade policy on the right-hand side of regressions. Even tests of the joint significance of the separate policy variables in such a specification would not be ideal because such a specification would needlessly estimate too many coefficients.

This paper proceeds as follows. Section 2 discusses a number of flaws in the claim by Rodriguez and Rodrik (2000) that there is no simple evidence linking high protection with slow growth. Section 3 then argues for the use of non-standard evidence on protection. This section includes a discussion of the historical importance of exchange controls and the merits of using data on the black market exchange rate premia and African export marketing boards and even geographical barriers. Section 4 presents a model of one important theoretical mechanism linking protectionism with slow growth and lower steady state income. Section 5 reviews the fragility of the Sachs and Warner (1995) rankings on a country-by-country basis and additional regression evidence. The section argues that in many cases the closed/open rating for countries does not depend on single indicators but can be corroborated with multiple indicators. The section however also identifies countries whose rating is indeed sensitive to single indicators so that they can be dropped in subsequent robustness tests. The section concludes with an examination of the sensitivity of the Sachs and Warner (1995) results to the inclusion of borderline countries, variations in the aggregation methods, and controls for additional variables. The paper concludes with a discussion of the overall message of the Sachs Warner (1995) results for the trade and growth debate.

2. Is there really no simple evidence linking trade restrictions with growth?

Rodriguez and Rodrik (2000) start their paper with an attempt to persuade us that there is no simple empirical evidence that shows an inverse relation between growth and trade restrictions.

Lets examine the basis for this claim. The simplest and most commonly used data on trade restrictions would have to be the average tariff rate taken from widely available Barro-Lee (1994) data set. A simple one-variable regression of the growth rate between 1970 and 1990 on the average tariff yields a coefficient of -1.6 with a t- ratio of -1.2. This appears to vindicate Rodriguez and Rodrik's position, since the estimated coefficient, though negative, is not statistically significant. But a glance at the data (figure 1) shows that India is a clear outlying observation. Dropping India from the sample and repeating the regression produces an estimated coefficient of -3.9 with a t-ratio of -2.1 (figure 2). This negative association between growth and tariffs is not driven by the presence of African countries in the data because it still holds if one excludes all African countries (figure 3).

If we continue along lines that are standard practice by controlling for initial income, the result strengthens (figure 4). The t-ratio on the average tariff rate is now -3.4. If we add the secondary enrollment rate the result also continues to hold: the estimated coefficient is -7 and the t-ratio is around -3 (figure 5).[1] Let us remind ourselves that Rodriguez and Rodrik claim that *there are no simple specifications* that show an inverse association between growth and

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[1] The introduction raised the issue that tariff data would not be a good summary measure of protection when tariffs were not binding (as would occur for example when other measures of protection were high). A regression of growth on tariffs and initial income for a sample of countries where other protectionist measures were not high yields an estimated negative coefficient of -7.8 and a t-ratio of -3.2. The sample excludes all countries with an average black market premia over 100, all African countries with a restrictive export marketing board, all countires with quota coverage greater than 40 percent, India due to its extremely high tariff, Zaire due to its civil war, and Mauritius due to its free trade zone with zero tariffs.

Figure 1
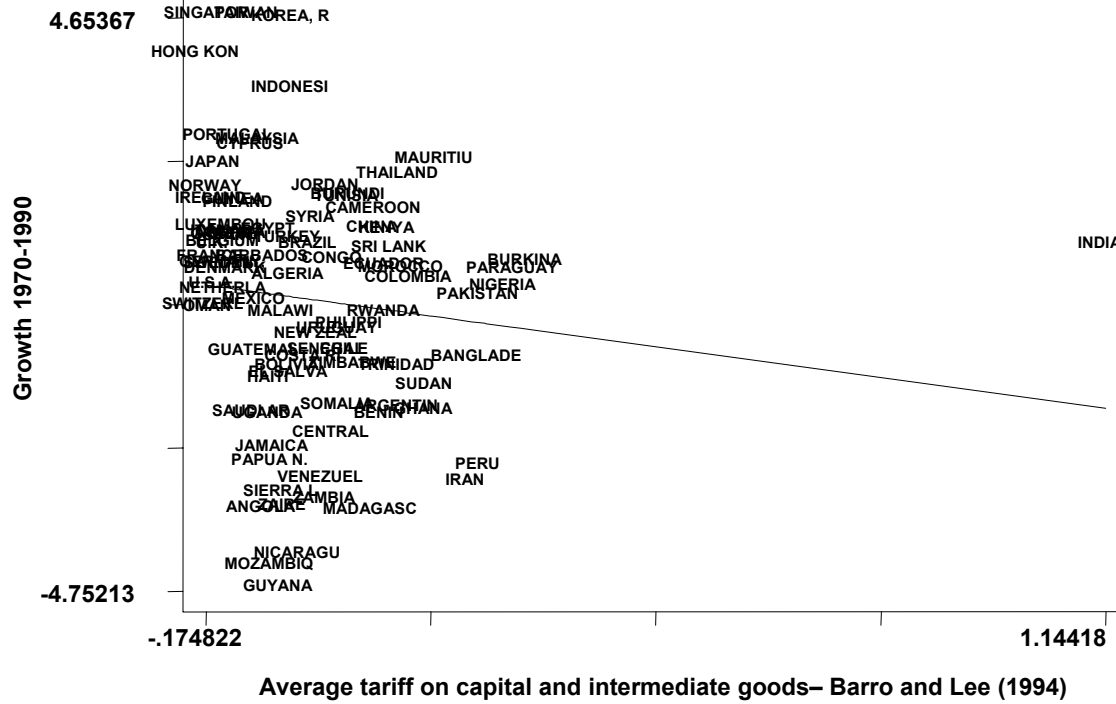
coef = -1.5305401, se = 1.2412887, t = -1.23

Growth 1970-1990 (y-axis, range 4.65367 to -4.75213)

Average tariff on capital and intermediate goods– Barro and Lee (1994) (x-axis, range -.174822 to 1.14418)

Figure 2

coef = -3.8427342, se = 1.7346077, t = -2.22

Figure 3

coef = -4.7046845, se = 1.9377646, t = -2.43

Figure 4

coef = -7.4554376, se = 2.1762401, t = -3.43

Figure 4 — scatter plot of Growth 1970-1990 (y-axis, from -5.15295 to 4.33136) versus Average tariff on capital and intermediate goods– Barro and Lee (1994) (x-axis, from -.154217 to .284776)

Figure 5

coef = -7.0675444, se = 2.2773724, t = -3.1

Scatterplot with y-axis labeled "Growth 1970-1990" (range from 4.08716 to -4.88642) and x-axis labeled "Average tariff on capital and intermediate goods– Barro and Lee (1994)" (range from -.154105 to .263564).

Country labels plotted: SINGAPOR, TAIWAN, KOREA, R, PORTUGAL, INDONESI, MALAYSIA, FINLAND, JAPAN, NORWAY, TUNISIA, SYRIA, SPAIN, THAILAND, IRELAND, FRANCE, GERMANY, JORDAN, GREECE, U.K., TURKEY, BRAZIL, ALGERIA, BELGIUM, DENMARK, MEXICO, SWITZERL, ECUADOR, AUSTRIA, CANADA, COLOMBIA, PARAGUAY, GUATEMAL, NETHERLA, SWEDEN, COSTA RI, URUGUAY, TRINIDAD, PAKISTAN, EL SALVA, PHILIPPI, SRI LANK, CHILE, NEW ZEAL, BANGLADE, ARGENTIN, HAITI, U.S.A., VENEZUEL, IRAN, BOLIVIA, JAMAICA, PAPUA N., PERU, NICARAGU, GUYANA
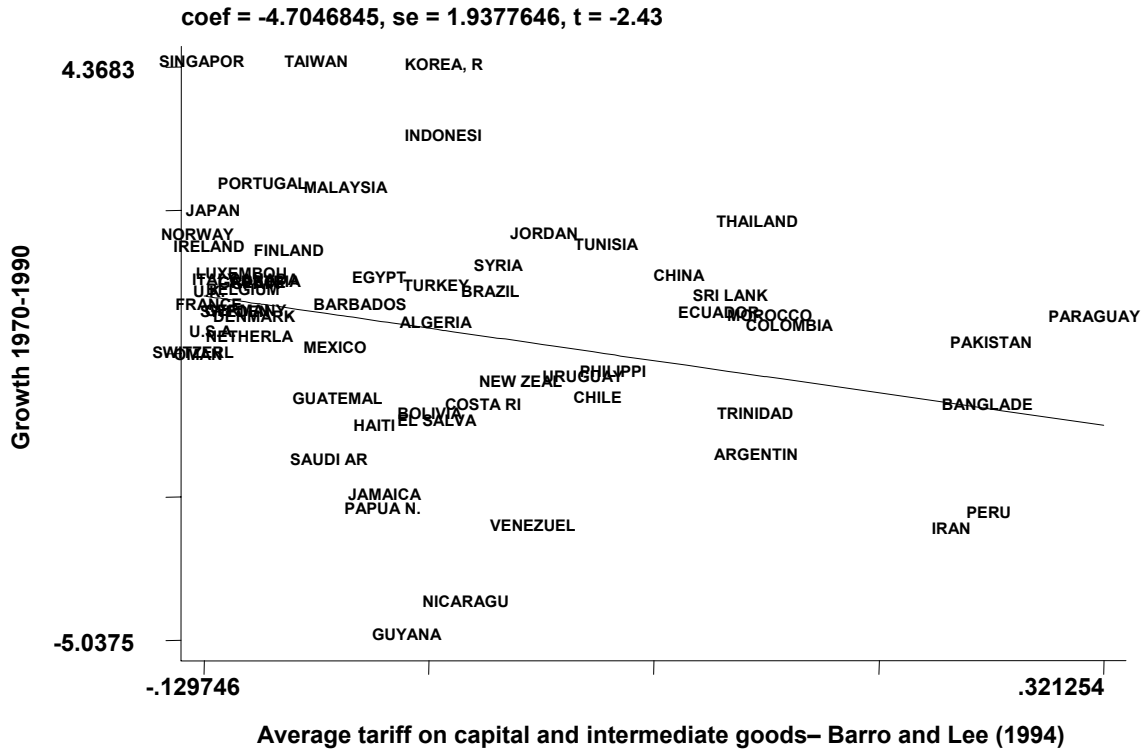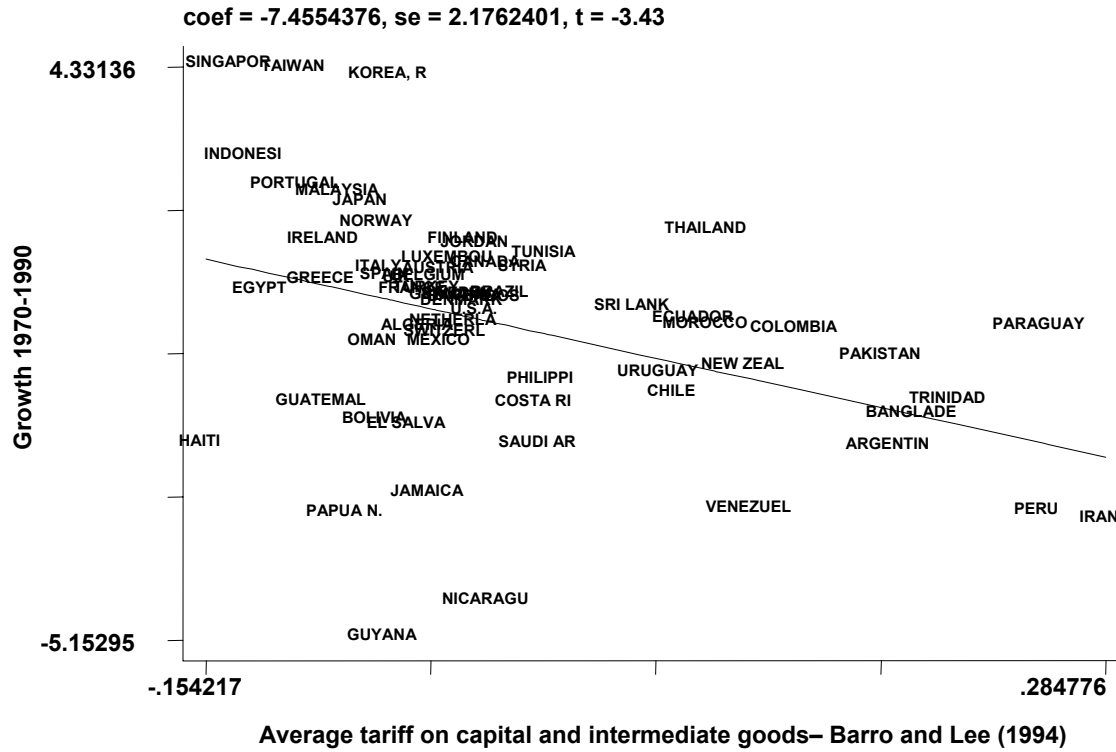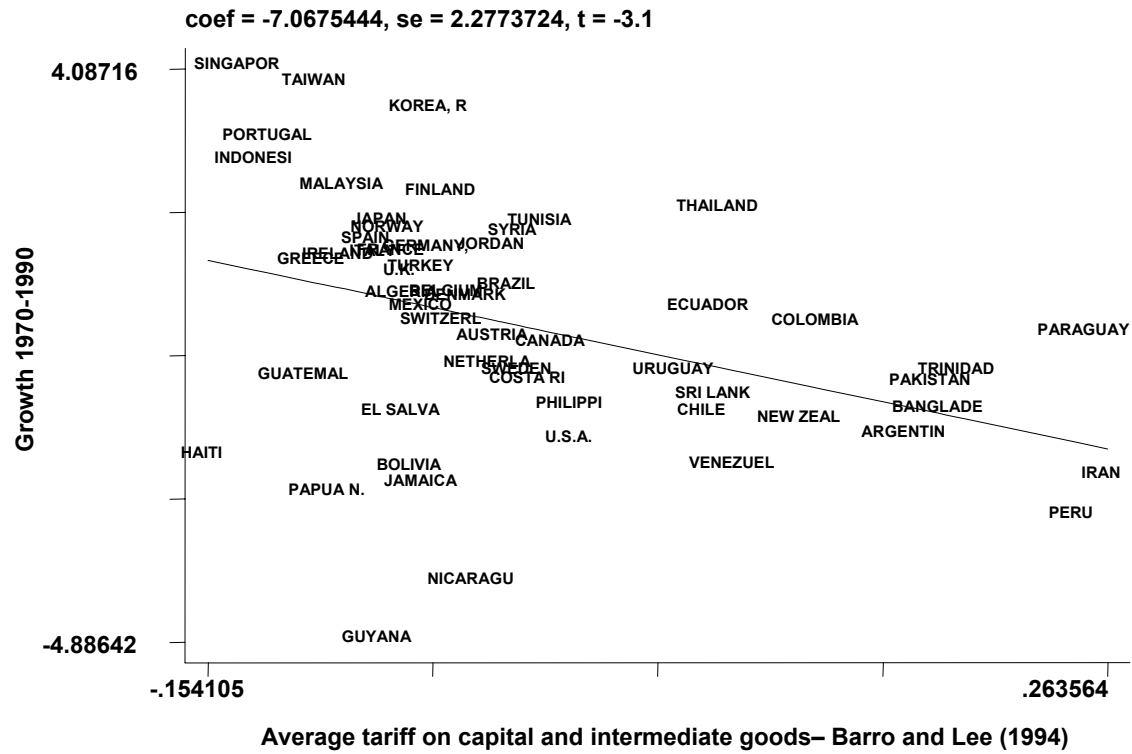
protection.

It is puzzling that Rodriguez and Rodrik never show the reader results using this average tariff data. After all, what could be simpler than regressing growth on the average tariff rate from the Barro and Lee data? Instead, they offer a slight of hand that is not immediately obvious: they use revenues from tariffs divided by imports as their data on tariffs. The big question is why the switch to data nobody else uses, when they are advertising themselves as objective arbiters of previous evidence? The tariff data they use is known to be inferior due to the fact that high tariffs may depress imports and therefore tariff revenue and make tariffs seem small. A highly protectionist country may appear open by this measure.

In addition, this implicit tariff data was taken from the World Bank's World Development Indicators from 1998, which means that in all likelihood, the data were measured *after* the growth rates in their regressions of 1975-1994 and *after* most of the major trade liberalizations of the 1980's in the developing world. The tariff data should be measured before, or at least contemporaneous with, the growth in question, and certainly not after the growth happened. With the data that Rodrik uses, a country that was protectionist in the 1970's and 1980's but liberalized in the 1990's would appear to have been an open economy during the 1975-1990 period when growth is being measured.

In one example of a more recent series of articles, Rodrik (2001) has continued with this theme by presenting the graph we reproduce in figure 7. The data on growth in this figure is growth in the 1990's and the tariff rate is from the 1990's. The time period is again *after* the major trade liberalizations in the developing world of the late 1980's and early 1990's. These liberalizations have eliminated most of the previously large cross-country variation in tariff rates. The current tariff data therefore lack sufficient variation with which to estimate the effects of

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trade liberalization. For testing the impact of protection, better to use data from an earlier period when the variation in protection across countries was truly dramatic.[2]

3. Why Narrow the Range of Admissible Evidence?

In addition to advocating questionable data and specifications that miss the point about substitution between policy instruments, Rodriguez and Rodrik (2000) advocate a radical narrowing of the admissible evidence. They ask us to focus only on the tariffs and quotas of textbook trade policy, ignoring inconvertible currencies and a wide range of other barriers.

In this section we discuss the kinds of data they want us to ignore. The list includes data on black market premia as an indicator of exchange controls, data indicating monopoly providers of foreign exchange in Africa, and data on geographic barriers to trade. Indeed the retreat from all the evidence goes even further. In a recent paper, Rodrik, Subramanian and Trebbi (2002) abandon even standard data on protection in favor of one single measure of openness to trade: exports plus imports as a share of GDP. Since countries such as Canada have small import shares due to their large size and dispersed geography, trade shares are an even more problematic indicator of protection. Canada is simply not a highly protectionist economy as this indicator would suggest.

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[2]A further criticism by Rodriguez and Rodrik (2001), namely that protectionism *could* be correlated with other policies, has a rather well-known solution: measure the other policies and introduce them in the regression. When other policy variables are controlled for in addition to the Sachs Warner variable the Sachs-Warner variable virtually invariably remains robustly associated with growth (see for example Sala-I-Martin 1997).

3.1. Exchange Controls Were Not Trivial

Far from being an exotic curiosity, administrative restrictions on obtaining foreign currency to buy imports became a common and pervasive form of import control during the post WWII period. Given that effective customs or border controls were costly to maintain, it is not surprising that relatively poor countries eventually came to rely on currency restrictions for import control. In Guyana the regime was quite simple: every single foreign transaction had to be personally approved by the President of the Central Bank. But even if protection was not the primary motivation, high protection can nevertheless emerge as a passive by-product of regulations. We are all aware of how the mere existence of an administrative requirement can eventually become an invitation for rent seeking and corruption.

The issue for testing trade restrictions is how to measure the intensity of administrative barriers. There is no cross-country data that directly measures the intensity of exchange controls. Such data is very hard to back out from the narratives in the IMF's Annual Report on Exchange Restrictions and Exchange Controls, in part because the narratives do not discuss this issue for all countries, in part because they often provide only a snapshot, but most importantly because the narratives do not quantify the intensity of exchange controls nor indicate the costs of complying with them. Faced with this situation, data on the exchange rate premium on the black market, if interpreted with the help of the narrative evidence and evidence on inflation can be quite helpful.

In addition to being relevant, exchange controls could cause large increases in the price of imports relative to domestic prices or relative to the price of the import converted at the official exchange rate. Consider the case of Algeria. According to the IMF Report mentioned above and

the World Currency Yearbook, Algeria had tight import restrictions for decades before a brief liberalization in 1991. In the early 1980's, after import restrictions were tightened in response to a balance of payments crisis, the black market premium rose from 100 percent (in the late 1970's) to 388 percent in 1985, 246 percent in 1986, and 418 percent in 1987. The official average tariff in the 1980's according to UNCTAD data was a mere 13.2 percent while inflation averaged 8.6 percent. The World Currency Yearbook reports that smugglers sold imports at markups of 1000 percent (over the European price converted at the official exchange rate, see page 29). Taken at face value, this figure suggests that prices of imports in Algeria were fully ten times the prices in Europe. This may seem high, but even if the markup were half of that figure, the effect on the domestic price of imports would be equivalent to a tariff of 500%. It strains credulity to conduct an analysis where the 13 percent tariff is the only indicator of Algeria's trade regime. In effect such an analysis stubbornly rates Algeria as a relatively open economy, in the face of additional evidence to the contrary.

Good cross-country data on such price differentials would serve as a sufficient statistic for the various trade restrictions. Such data is not readily available on a consistent basis across countries. But specific cases such as the Algerian example above certainly warn us against using the tariff as the sole indicator. Data from Pakistan provide another example where the use of tariffs alone would underestimate the price differentials. Islam (1981) reports such price wedges in Pakistan under their import control regime during the 1960's. His figures on prices for capital goods show that the regular tariff rate was 47 percent in 1963/4 (1963 for short). The price differential however was 1.8 time as high at 86 percent. During the next five years the average tariff rose and the average price wedge rose even more, so that by 1968/9 the price wedge was 2.4 times the average tariff. These figures illustrate the potential importance of non-tariff

items in determining rates of protection in real life.

Tariff Rates and Percentage Price Differentials: Pakistan	1963	1968
Tariff Rate on Capital Goods	47%	62%
Percentage price differential: domestic vs world	86%	150%
Ratio (price diff/tariff)	1.8	2.4

Source: Islam (1981, Table 5.7, Page 65.)

If the prices wedges themselves have not been recorded sufficiently for useful cross-country analysis, it is not more scientific to rely on tariffs as the sole source of the price differentials. The price differentials were a function of multiple restrictions. Failing direct observation of the price differentials, an attempt should be made to measure all of these sources.

3.2. Is the Black Market Premium a Good Indicator of Exchange Restrictions?

Rodriguez and Rodrik (2000) also assert that a high black market premium indicates macroeconomic chaos rather than trade barriers. This is only one of several possibilities so the assertion lacks some clarity and evidence. It is true that rampant inflation with all nominal prices rising except for a lagging official exchange rate would produce an exchange rate premium. In the special case in which no transactions were conducted at the official rate, this premium would affect no relevant relative price and should not be considered an import barrier. But if the importer of the capital good in our earlier example had to buy the imported good at the official exchange rate, then even in this high-inflation scenario the exchange rate premium would be a good proxy for the rise in the relative price of imports, and would be a relevant proxy for the tax on investment. So the mere observation that high inflation coupled with some lagging price adjustment can produce a black market premium is not sufficient to toss out the evidence. It is

extreme to flatly deny that there could be any trade implications behind a high black market premium and to rule it out of any study on trade restrictions.

Evidence on whether black market premia correlate with inflation rates is certainly relevant to this issue. In figure 6 we show a scatter that establishes no relation between the two variables in a large cross section of countries.. The sample covers 1400 country-years between 1971 and 1992. Indeed the reader may be surprised that many countries had a high black market premium and *low* inflation - just the opposite of the pattern that Rodriguez and Rodrik assume. As an empirical matter, it has been more common historically to see high black market premia emerge as a consequence of exchange controls imposed after a balance of payments crisis than to see high black market premia arise after expansionary macroeconomic policies. But even if a macroeconomic demand expansion produces a high black market premia, this in no way diminishes the fact that a by-product of the high premia can be higher costs of acquiring foreign exchange.

A more important criticism would be that a black market premium may arise from foreign currency restrictions that have no immediate implications for growth such as restrictions on acquiring foreign exchange to purchase foreign assets (capital controls) or restrictions on acquiring foreign exchange to purchase special luxury consumer goods. In other words, the mere existence of a premium is not sufficient to prove that the relative price of growth-relevant imports such as capital goods were affected. A country with a positive black market premium may have maintained a dual exchange rate system that insulated importers of capital goods from any adverse relative price changes.

The issue hinges on the extent to which such multiple exchange rate systems did in fact achieve effective separation between the various uses of foreign exchange. In the Algerian case

mentioned earlier, if the administrators of the exchange rate controls were to try to provide foreign exchange at the official rate for importers of capital goods, they personally would face a large opportunity cost for not selling the same foreign exchange on the black market. From this perspective, the premium is also the return that an administrator of the dual exchange rate system can earn by selling the foreign exchange on the black market. One justification for a threshold such as the 20 percent used by Sachs and Warner (1995) for the black market premia would be the belief that a dual system would brake down beyond this threshold. The exact threshold is of course debatable. Several countries, including Algeria, were far above this threshold. If the equilibrium bribe in Algeria was only half of the official premium, the increase in import costs still would be substantial. Going back to the example of the $100,000 capital import, any premium above 100 percent would mean a negative internal rate of return.

While there is plenty of scope for debate about the extent to which dual rate systems were in fact implemented cleanly, and about the precise threshold to use when rating countries closed or open, there appears to be little basis for the Rodriguez and Rodrik assertion that high exchange rate premia have absolutely no bearing on the cost of imports. Blocking access to foreign currency or implementing measures that lead to higher prices for foreign currency can be just as effective in restricting trade as any tariff or quota.

3.3. African Protectionism

In several African countries the international marketing of exports were administered by centralized export marketing boards. Given the high concentration of African exports in single commodities, these boards usually had a near-monopoly position when it came to control over foreign exchange earnings. Bauer (1953,1964) provides a detailed analysis of how the boards op-

erated in West Africa. Such boards originated during the late 1940's under British colonial rule with the ostensible purpose of stabilizing prices for African farmers. They instead accumulated large cash surpluses, increased political power, and evolved into linchpins of the managed-trade regimes in West Africa. During the post-colonial period these boards became in part the administrators of, and in part the lobbyists for, the system of import control in post-colonial Africa. Aided by their accumulated wealth, they became closely aligned with governments implementing import licensing and foreign exchange rationing, with all the attendant extra fees and charges of such systems. Given their near-monopoly on exports, they had a near-monopoly on control over the pool of foreign exchange available for importers. Sachs and Warner (1995) regard the existence of an restrictive export marketing board, namely one which was given a maximum score of 4 (on a 1 to 4 scale) as presumptive evidence of a restrictive trade regime. This decision is debatable, but nevertheless it is one that is backed-up by secondary sources on how export marketing boards actually operated (see also Bates 1981). Rodriguez and Rodrik's (2000) once again refuse to admit the evidence.

Since traditional trade restriction data on Africa is often not available, the practical consequence of ruling out such non-standard evidence is simply that Africa vanishes from studies on the impact of trade restrictions. Since African countries are widely acknowledged to have been highly protectionist, a point which is not-controversial for any expert on Africa, and since Africa has been a region with very slow growth, the decision to rule out African countries stacks the deck in favor of finding no negative effects of trade restrictions.

3.4. Why not Natural Barriers Too?

To the extent that the determining factor in growth is the price of imports relative to the price of domestic goods, there is no conceptual reason for treating natural barriers to imports any differently from other barriers. Natural barriers are not commonly included in trade studies (and were not considered in our 1995 study), but using the example of the $100,000 capital good import, can have exactly symmetric consequences on investment and growth. A similar point may be made about the impact of delays due to red tape. If the useful life of the capital good in the earlier example were 15 years, the internal rate of return would be 5.6 percent. However, if the project were delayed by two years so that the revenue stream started in year 3 rather than year 1, the return would immediately drop to 4 percent.

To quantify the potential cost of geographic remoteness, consider recent data from Bolivia[3]. The example concerns a capital good bought in Canada for $100,000. The additional payments associated with this good were as follows. Maritime transport costs to Bolivia were $20,850, insurance was $211, port costs $2,702 and transport costs to the Bolivian border $16.980. The pre-tariff price at the border was therefore 40.7 percent of the Canadian price. If this was a consumer good a ten percent tariff would have been levied on the price at the border, but capital goods were exempt. The value added tax of 14.94 percent adds a further $21,027 to the cost, after which transport costs to the capital city ($33,469) and fees for inspections and certificates add a further 2-3 percent. The final price in La Paz minus the value added tax was $178,354, fully 78 percent higher than the Canadian price even though there were no tariffs, bribes, costs for financial transactions or costs for acquiring foreign exchange. Even if a tariff was levied at

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[3]Based on data in Marcela de Guzman (2001) Cuadro No. 6, pp.25-26. She in turn relies on data from the Central Government of Bolivia and the National Chamber of Commerce (Camara Nacional de Comercio).

the ten percent rate it would have been a relatively minor part of the full costs of importing.

The Bolivian example is used to illustrate a case of relatively high transport costs. The general point however is that even when transport costs are a fraction of the Bolivian example they can still be significant compared to the costs of official tariffs and quotas. This example raises the question of why Rodriguez and Rodrik (2000) wish to rule out the evidence in Frankel and Romer (1997) that uses geographic remoteness as an instrument to estimate the impact of import restrictions on growth. This is valid evidence that is fully consistent with the idea that import restrictions can depress growth by increasing the cost of capital, and reducing the incentives to invest and grow. All sources of a higher cost of capital should be equally admissible as evidence on this proposition.

4. A Model of Trade and Growth

It is sometimes claimed that there is little theoretical basis for the proposition that trade liberalization can boost growth (see for example Rodriguez and Rodrik (2000)). However Panagariya (2002) reviews a number of ways in which trade liberalization can raise the level of income through static efficiency gains. And Bhagwati and Srinivasan (2002) argue that trade liberalization can also raise growth through two channels: greater variety in intermediate inputs and greater returns to investment through access to a larger global marketplace. Other authors have stressed channels such as improved technological transfer through international trade and improved incentives to innovate and upgrade in reaction to greater international competition.

Models in the style of Helpman and Grossman (1991) derive growth implications from trade liberalization to the extent that liberalization shifts resources towards the sector that is the

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growth engine of the model. If the growth engine is research and development activity the results can depend on whether R&D is conducted at home or imported from abroad. In a variant of the infant industry argument, liberalization can depress growth in these models if liberalization shifts resources from research and development activity. In the Matsuyama (1992) - style models, human capital accumulation depends on the share of the labor force in certain sectors. Liberalization can raise or lower growth depending on whether it shifts resources into or out of such sectors. These models can justify either infant industry considerations or trade liberalization, depending on which sectors are assumed to be characterized by increasing returns to scale.

This section shows a model that draws attention to the relative price channel. The model provides a framework that can justify a variety of variables used in previous empirical work. It applies to countries where crucial capital goods are typically imported. In such countries trade restrictions or anything else that raises the relative price of machinery such as costs connected to climate geography and corruption can exert symmetric effects. This kind of model can motivate both the Sachs-Warner (1995) approach, since a variety of trade restrictions can affect this relative price, as well as the Frankel and Romer (1999) approach of using geographical remoteness as an instrument for trade barriers, since higher transport costs can also affect this relative price.

Consider an investment technology that combines the imported machine with domestic capital to produce a composite capital good according to $k = \min[\theta k^f, k^d]$, with the superscripts standing for foreign and domestic capital. With this technology, it is optimal to combine the two kinds of capital goods in fixed proportions: $\theta k^f = k^d$. Therefore, to increase the composite capital stock by one unit, the enterprise must purchase θ units of the foreign capital good at a

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price of p, and 1 unit of the domestic capital good at a price of p^d. Setting $p^d = 1$, the cost of an extra unit of the composite capital good is therefore $1 + \theta p$. For simplicity assume further that the domestic capital good and the domestic output are identical and that the cost of installing one unit of the capital good is $C(I)$ units of the domestic good. Specifically, let $C(I) = \frac{I^2}{2}$, and assume production takes place according to $y = Ak$. Then the representative firm solves the following Hamiltonian:

$$\max \int_0^\infty e^{-rs} \left[Ak - (\theta p + 1) \frac{I^2}{2} \right] ds + q(I - \delta k)$$

From which we obtain,

$$\dot{q} - (r + \delta)q = -A$$

and

$$I = \frac{q}{\theta p + 1}.$$

Integrating...we have

$$q = \frac{A}{r + \delta}$$

Therefore,

$$I = \frac{A}{(\theta p + 1)(r + \delta)}$$

From the production function $y = Ak$, we have the familiar two sources of growth: productivity growth or technological progress and capital accumulation $(g = \frac{\dot{A}}{A} + \frac{\dot{k}}{k})$. We suppress the

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productivity growth term to focus on the essential points in the discussion below, hence.

$$g = \frac{\dot{k}}{k}.$$

Growth in the capital stock comes from the accumulation equation. Given,

$$\dot{k} = I - \delta k,$$

growth in the capital stock is

$$\frac{\dot{k}}{k} = \frac{I}{k} - \delta.$$

The path of the capital stock over time is therefore

$$k(t) = k(t_0)e^{-\delta(t-t_0)} + \frac{I}{\delta}(1 - e^{-\delta(t-t_0)}).$$

Note that in the limit as $t \to \infty$, $k(t) \to I/\delta$.

Since output is

$$y(t) = A * k(t).$$

the path for output is:

$$y(t) = A * k(t_0)e^{-\delta(t-t_0)} + \frac{A^2}{(\theta p + 1)\delta(r + \delta)}(1 - e^{-\delta(t-t_0)}).$$

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Differentiating with respect to time, growth is given by:

$$g = \frac{A}{k(t)(\theta p + 1)(r + \delta)} - \delta.$$

Therefore both growth and the level of income along the entire path to the steady state depend inversely on the relative price of imported equipment, and inversely on trade protection through this channel. The tariff data used in the empirical work below are tariffs on investment goods.

Since $k(t)$ is in the denominator, growth will decelerate as capital accumulates. In the long run this model has the familiar property that capital accumulation will reach a steady state where new investment is just sufficient to compensate for the depreciation of the capital stock so that real output stops growing. In other words, given that A is constant, growth is entirely due to transitional dynamics. This feature of the model may be an appropriate simplification for a developing country far from the technological frontier. Technological progress, represented by A, would be determined by innovation at the global level and thus would be pre-determined from the perspective of a small developing country.

4.1. The equivalence of trade control measures

An crucial term in the previous model is the term $R = \frac{1}{\theta p + 1}$, which stands for the ratio of the price of the domestic good to the price of the composite capital good. This section spells out more formally how a number of policies, including tariffs, quotas, black market premiums and monopolization of exports can affect both growth and the level of income through this term.

A unit of output can be sold at the local currency price of 1, and there is potentially a tariff

levied on the imported capital good at rate τ. There is a dual exchange rate system in place with an official exchange rate E^o and a black market exchange rate E^B, and hence a black market premium of $(\frac{E^B}{E^O} = 1 + BMP)$. Without loss of generality we assume that the firms import a fraction δ of their import needs at the official rate and the rest $(1 - \delta)$ at the black market rate. Let p represent the foreign currency cost of a unit of the imported capital good. Then the ratio R may be expressed as:

$$R = \frac{1}{\theta(1 + \tau)p[\delta E^o + (1 - \delta)E^B] + 1}.$$

or, expressing this as a function of the black market premium:

$$R = \frac{1/E^o}{\theta(1 + \tau)p[\delta + (1 - \delta)(1 + BMP)] + 1/E^o}.$$

In the limit as $\delta \Rightarrow 0$, the BMP enters this expression exactly like a tariff. The claim that the existence of a black market premium has nothing to do with import costs or has nothing to do with growth is an extreme case corresponding to $\delta = 1$.

If we think for a minute about what would determine δ, it is very likely to be a function of the black market premium itself. Suppose a country found itself in the polar case where its dual exchange rate system had achieved perfect separation of the capital account from the current account so that all imports of capital goods were transacted at the official exchange rate. Suppose in addition that there was a high black market premium. Any firm or bank official with foreign exchange would have an incentive to sell it on the black market rather than the official market, and this incentive would rise with the level of the black market premium. The amount of foreign exchange available on the official market would shrink and it is very likely that importers

would increasingly have to transact at the black market rate or equivalently would have to pay bribes or suffer delays to get official foreign exchange. These are essentially equivalent to a declining δ.

This framework may be extended to analyze the impact of a number of other trade-related restrictions apart from tariffs and quotas. These include export surrender requirements, transport costs, port fees, license fees, the opportunity cost of waiting, inventory costs and extra-legal or informal payments.

5. Another Look at the Sachs-Warner Data and Results

5.1. The Black Market Premia

If we go back to the original Sachs-Warner sample there were 56 countries with an average black market exchange rate premium in excess of 20 percent for the full decade of the 1970s or the 1980s. Of these, 15 also had high tariffs or quotas, 13 had restrictive export marketing boards, and 8 were socialist planned economies. Six countries belonged to more than one of these groups. Therefore 30 countries (15+13+8-6) did not depend solely on the black market premia for their open/closed rating.

Of the remaining 26 countries, one was eliminated for lacking GDP data (Sudan) and another for an ambiguous trade status (Swaziland), leaving 24 countries. Of these 24 countries, 10 can now be rated as closed based on corroborating evidence that has been published after 1995 (and will be reproduced below). Out of the original list of 56 countries then, we are left with a group of 14 that need to be examined more closely (open countries rated as open in part because they were clean on the black market premium would not have their rating changed by dropping the

BMP criterion).

Let us consider these 14 countries more closely. They are Algeria, Bolivia, Botswana, Chile, El Salvador, Guyana, Israel, Iraq, Myanmar, Nepal, Papua New Guinea, South Africa, Sri Lanka, and Uruguay. In many cases we are not talking about modest black market premia. In fact seven of the countries had average BMP's that exceeded 100 percent for a full decade (Algeria, Bolivia, Chile, El Salvador, Guyana, Iraq, and Myanmar). Eight countries had average black market premia that exceeded five times their average inflation rate (Algeria, El Salvador, Guyana, Iraq, Myanmar, Papua New Guinea, South Africa, and Sri Lanka). There are also two mid-stream liberalizers in the group, Chile starting in 1976 and Sri Lanka starting in 1977. Few writers would dispute the closed rating before the liberalizations. Bolivia and Nepal are landlocked countries that have considerable natural barriers to trade. In the case of Nepal, it is difficult to justify rating them as open when major trade routes had to cross communist China, or India with average tariffs in excess of 100 percent. Nevertheless, in spite of these points, we will drop these countries from some of the empirical work later in this paper to check the sensitivity of the results to the black market premium criterion.

5.2. The African Export Marketing Boards

There were 41 African countries in the Sachs-Warner sample. Of these, 28 were listed as having the most restrictive version of the export marketing board (according to The World Bank (1994)). Of the remaining 13, 11 were rated as closed economies based on quota data (Tunisia) black market premia data (Algeria, Angola, Egypt, Ethiopia, Kenya, Somalia, Sudan, Tunisia, and Zaire) or the international embargo against Apartheid (South Africa and by extension via its customs union, Botswana). One African country was rated to be open (Mauritius). Morocco

was a special case that would have been classified as open based on the tariff and quota data from the mid-1980s. However, since this data was observed after their liberalization in 1984, and the liberalization was extensive, Morocco was judged to have been closed before 1984. Morocco's average growth over the full period was a relatively fast 2.4 percent per year, so in rating Morocco as closed we were leaning against the case for open economies. Rodriguez and Rodrik's (2000) claim that the export marketing board variable is an Africa dummy variable that takes the value 1 for Africa and 0 everywhere else does not appear to be accurate. This variable is coded 0 for 13 of the 41 African countries in the sample and it is coded 1 only for countries with marketing boards given the most restrictive rating of 4 on a 1-4 scale. It happens that most of the boards rated in the study were judged to be extremely restrictive, but that is a fact about African trade policies.

As discussed earlier, writings on colonial and post-colonial African economic policy emphasize the central role played by the state marketing agencies or export marketing boards such as the Cocoa Marketing Board in Ghana or the Coffee Board of Kenya in the restrictive trade regime. Typically these boards were monopsonies that set the prices from which they bought agricultural output from farmers to be re-sold on world markets. The marketing boards were also near-monopolists in the re-sale of foreign exchange. Hence the existence of a monopoly export marketing board is an indication that there was administrative allocation of foreign exchange. Bribes and favoritism were pervasive in the process of obtaining foreign exchange or licenses for importing. Although such extra payments do not show up in the data on official tariffs or black market premia, they serve as protective measures that can be equally restrictive. Given the widespread discussion of the protection that such boards engendered, this is a potentially useful indicator of protection and a solution to the problem of lack of data for several poorer

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African countries.

Rodriguez and Rodrik (2000) appear to flatly deny that restrictive marketing boards may indicate import restrictions through controlled allocation of foreign exchange. They mention that Mauritius has a marketing board, appearing to misunderstand the point that it is the monopoly nature of the board and its control over the re-sale of foreign exchange that is crucial to the argument. The Sugar Marketing Board in Mauritius did not have a monopoly position in the selling of foreign exchange for imports. Indeed, Mauritius has had several active export processing zones that are widely accessible for firms that want to import parts duty free - these zones have been set up precisely to make importing as easy as possible with no restrictions on foreign exchange. Canada also has export marketing boards, but such Boards play absolutely no role in import control like the African Boards. The crucial point is that Marketing Boards played a special role in Africa.

If we were nevertheless to disregard information about restrictive export marketing boards and black market premia, as Rodriguez and Rodrik (2000) recommend, we would be left with a highly incomplete and distorted picture of African protection. In such regressions, 10 African countries would simply drop from the sample due to lack of data tariffs or quotas. Of the remaining countries which do have data on tariffs and quotas, 14 would have qualified as 'open' if we were to apply our 1995 criteria, due to the sole reliance on tariffs as a criterion. Four of these countries, Guinea, Mozambique, Sierra Leone, and Uganda, would actually appear to have been relatively open since their average tariffs were in the 10 percent range. Since Mozambique recorded an average annual growth of -3.28 during this period, the decision by Rodriguez and Rodrik to focus only on tariffs and thus rate Mozambique to have been open certainly helps their case. But it runs counter to a lot of additional data and independent writings on Mozambique.

To mention just one source, Ng and Yeats (1998) are hardly equivocal in showing Mozambique to have been highly protectionist.

This decision to stick only to tariff data simply has the practical effect of introducing measurement error. Secondary sources are full of discussions of high protection in Africa; and for some of the countries, the low tariff data simply flies in the face of all the other evidence. Although there have been liberalizations in Africa in the 1990's, current commentaries on trade policies still emphasize that Africa is highly protectionist and also emphasize the important role played by factors other than tariffs. These factors presumably were if anything even more restrictive during the 1970-1989 period before the liberalizations. Ng and Yeats (1998) contain a number of useful summaries of particular countries. Angola for example is rated as a "virtually a closed market." "Its market is highly protected behind a wall of trade quotas and import licenses which are required of all imports. Corruption in the customs service hamper imports." "Cameroon's average tariff is about 30 percent. Cameroon also applies countervailing and anti-dumping duties to protect inefficient domestic industries. Import licenses are required and at least 100 items are subject to import quotas." In Chad, "..the biggest deterrent to trade remains an unsafe and unnavigable road system. Overall travel after dark is highly dangerous." "Congo's average tariff is unknown. Tariffs on intermediate and consumer goods range from 35 to 50 percent. The biggest non-tariff barrier is red tape, an inefficient customs, and theft of imported goods by government officials." "Ethiopia applies a maximum tariff rate of 80 percent. The customs bureaucracy is cumbersome and inefficient."

Given this kind of commentary, the use of tariff data that in effect rates African countries as open requires more corroboration than Rodriguez and Rodrik provide. It also seems especially important when dealing with Africa to broaden the measurement beyond tariffs and quotas

to capture the true extent of closure. Although none of the African countries in the study had particularly low tariffs, even in the relatively low-tariff countries the commentary usually emphasizes some additional non-tariff barrier. In Ghana, handling and customs delays were frequent. In the Ivory Coast, customs fraud was extensive. In Kenya the customs system was prone to corruption. In Mozambique the customs system was riddled with corruption. In Namibia all imports required an associated letter of credit. In Somalia a major impediment was corrupt customs officials confiscating goods. Overall, Ng and Yeats (1998) summarize the data as follows: " .. for the 28 African countries included 16 (57 percent) received the lowest possible rating .. for their trade policies, while an additional 9 (34 percent) received a 'very poor' rating of 4 or higher.".. African policies relating to trade, therefore appear to be even more onerous than the generally very repressive measures applied in other areas like banking, taxation, or security of property rights."

For further evidence on African trade policies, table 2 shows data on trade restrictions in Africa from the World Bank (1994) and Ng and Yeats (1998), the latter of course was made available after Sachs and Warner (1995). The first two columns report additional information from the 1994 World Bank study on the number of import items subject to non-tariff barriers and the percent of foreign exchange allocation that is (administratively) controlled. These two columns indicate a number of countries where the earlier 'closed' classification receives further support. In the remaining six columns, we report data on average tariffs, average tariffs and para tariffs, and non-tariff measures. The table shows that even the simple tariff and quota data provide a picture of relatively restrictive trade policies. In addition, in 11 of the countries, fully 100 percent of foreign exchange allocation was controlled. However, in order to check robustness, we will nevertheless drop 14 African countries whose rating relies on the export marketing board

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from the empirical work in the next section of this paper.

5.3. Regression evidence

This section reviews regression evidence that tests the robustness of the growth-openness association using the original Sachs-Warner (1995) openness index along with four additional variants. The baseline results are given in the regression below. Growth in real GDP per-person during the 1970-1990 period is regressed on the log of GDP in 1970, the openness index, and an interaction term consisting of the openness index times initial GDP.

$$G7090 = -0.08 * LGDP70 + 14.8 * OPEN1 - 1.4 * LGDP70 * OPEN1$$

$N = 110\check{\ } R^2 = 44\%$

Even though this regression contains no further controls, it serves as a useful benchmark to discuss the magnitude of the estimated coefficients. The t-ratios on the three estimated coefficients are -0.3, 4.2 and 3.2. The results imply at face value that open economies grew faster and also converged on the incomes of richer countries faster than closed economies. Predicted growth for a closed economy such as Ghana, with ppp-adjusted GDP in 1970 of approximately $1000 (1985 international dollars per capita), was one-half percent per annum. If Ghana were open, predicted growth would have risen to 5.6 percent, for an increase of 5.1 percentage points. The same calculation for a country such as Mexico, with GDP of approximately $4000, would predict a rise in growth of 3.2 percentage points. Of course, we are not suggesting that this would have happened, rather we are simply providing illustrious numbers to help in evaluating the estimates. Note also that there are 110 countries in the regression sample above. In Sachs

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and Warner (1995) the original sample of 135 countries in version 5 of the Summers and Heston (1991) data set was reduced to 111 due to the lack of data on GDP or to the lack of sufficient information on protection. Here, one further country, Iraq, for which Sachs and Warner (1995) used 1985 GDP data is excluded for lack of good GDP data, reducing the sample to 110 countries.

The first regression in table 3 simply adds the institutions variable and secondary education variable to the simple specification shown above. The sample falls to 86 countries. The essential results concerning the openness variable are unchanged. The institution variable is significant but its presence does not diminish the magnitude or significance of the open variable. In particular, there is no evidence here that openness is just a relabelled institutions variable.

The first significant modification to the Sachs-Warner variable is to exclude from the analysis all countries whose open/closed rating relied exclusively on the black market premium data. These 13 countries are Algeria, Bolivia, Botswana, Chile, El Salvador, Guyana, Israel, Myanmar, Nepal, Papua New Guinea, South Africa, Sri Lanka, and Uruguay (Iraq and Swaziland also qualify for this list but have already been eliminated from the sample for other reasons). In Sachs and Warner (1995) South Africa was rated as a closed economy due to the international trade embargo imposed on the Apartheid regime, and Swaziland was excluded as being an inherently ambiguous case. Of the other countries in this list, Chile and Sri Lanka were two unusual countries in that they liberalized trade in the middle of the 1970-1990 period, Chile after 1976 and Sri Lanka starting in 1977. Algeria and Guyana have already been mentioned for having exchange controls but are excluded for the sake of the argument. Botswana was well known to have had a relatively open trade regime during this period but is also excluded for the sake of the argument. Israel and Uruguay are true borderline cases that may well have been classified as open under a slightly different scheme.

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The second regression in table 3 shows that the results concerning the open variable are not diminished by the introduction of this slimmed-down version of the openness variable, dubbed "OPEN2". The regression results continue to argue that open economies had higher mean growth and converged faster than closed economies.

The next modification to the open variable is to exclude not only all the countries mentioned in the previous section but also all of the African countries whose open/closed rating relied exclusively on the restrictive export marketing board rating or both the EMB and BMP rating. These 14 countries are Burundi, Central African Republic, Chad, Cote d'Ivoire, Gabon, Gambia, Guinea Bissau, Mauritania, Mali, Niger, Senegal, Togo, Uganda, and Zambia (Four of these, Burundi, Mauritania, Uganda and Zambia also had high average black market premia).

The third regression in table 3 shows that the essential results of the open variable continue to hold with the introduction of this modification, even though the sample is now reduced to 66 countries. The results are not sensitive to the use of the black market exchange rate criterion or the export marketing criterion for a few borderline countries. All the countries that remain in the sample for regression 3 rely on two or more criteria for their open/closed rating.

The next robustness check is to lower the tariff and quota threshold from 40 percent to 20 percent. Now countries are only considered open if all of the following conditions are satisfied: average tariffs below 20 percent, quota coverage percentage below 20 percent, average black market premia from the 1970's and 1980's below 20 percent and non-socialist. We continue to eliminate countries whose rating would rely exclusively on either the black market premium criterion or the export marketing board criterion. In other words, all the countries that are rated as closed economies and remain in the sample for this fourth regression do so thanks to a closed rating on two or more criteria. This modification narrows the set of countries that

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qualify as open economies. It was chosen in particular because it switches the classification from open to closed of four notable fast-growing economies, namely Mauritius, Taiwan, Thailand and Cyprus. Given that each of these economies was arguably fairly open, and each grew rapidly, this modification is a large concession to those who wish to diminish the evidence in favor of trade liberalization.

The fourth regression in table 3 shows that even this concession to the other side of the debate does not alter the essential results. The OPEN variable remains significant despite the fact that Mauritius, Taiwan, Thailand and Cyprus are classified as closed economies. These results underline the point that the empirical case in favor of open economies does not hinge on reliance on the black market exchange rate criterion or the export marketing board criterion as the sole criteria, nor does it hinge on the classification of countries such as Taiwan and Thailand as open economies. (Korea would not qualify as a closed economy even if the tariff threshold were lowered further to ten percent since according to the UNCTAD data South Korea's average tariff was 9 percent and her quota coverage percentage was also 9 percent). The fifth regression drops New Zealand from the sample to show that the results do not hinge on its earlier classification in Sachs and Warner as a closed economy.

5.4. Is it openness or something else?

When faced with these results, readers often claim that the openness variable must be proxying for other correlated policies such as institutional strength or overall macroeconomic stability. To investigate this, we consider a number of additional controls.

Above we have already shown that the openness variables are significant when controlling for institutions, so the evidence rejects the view that openness is simply a proxy for good

institutions. The regression evidence also rejects the view that trade openness only works once good institutions are in place. If this were the case, one would expect that interaction variables between institutions and openness would statistcally dominate the simple openness variables, but the evidence in table 4 contradicts this. The first regression in table four is a simple specification with growth regressed on initial income, OPEN5, (the most narrow version of the openness index from the previous table), institutions, and an interaction term consisting of openness times the institutions variable. The first reported regression appears to offer evidence in favor of the interaction effect. However the estimated coefficient has the wrong sign, suggesting, if taken at face value, that better institutions harm growth in open economies (or equivalently that openness harms growth when combined with good institutions). The second regression shows that this result is a by-product of the fact that institutions are positively correlated with GDP. When the regression is forced to choose between the argument that there is interaction with institutions and the argument that openness promotes convergence to higher incomes, the regression prefers the argument that openness promotes convergence.

The openness variable survives many controls in addition to institutions. Table 5 shows that the openness results withstand controls landlocked status, a tropical climate, fiscal policy (measured by the central government surplus in percent of GDP), the national saving rate, average inflation, financial depth, and indeed a sub-Saharan African dummy variable. Some of these additional variables are themselves significant, some are not, but the openness results remain significant in all the specifications.

But the reader does not need to rely on the results here. Consider for example the additional checks of robustness in Sala-i-Martin (1997) or Doppelhofer, Miller and Sala-i-Martin (2000). These studies rank the openness to trade variable in the top five in terms of robustness to the

inclusion of other controls.

In sum, it would be naive to argue based on regression results alone that openness can work well all by itself, or that other policies do not help complement the beneficial effects of trade liberalization. Nevertheless, critics have asserted that the evidence is a slam dunk in their favor; that openness is not reliably correlated with growth when compared with institutional variables or that the evidence shows that openness only works when combined with strong institutions. Although these arguments sound plausible a-priori, it is worth pointing out that they are not backed up clearly by the evidence.

6. Conclusions

The simplest reading of the evidence is that, between 1970 and 1990, countries that were open to trade grew faster than countries that were closed to trade. In other words there was a significant difference in growth between open and closed economies. This difference was larger for poorer countries. Open developing countries with GDP per capita of $5000 or less in 1970 grew by 4.8 percent per year while closed developing countries grew by 0.8 percent per year. Although the size of this estimated gap in growth rates depends in part on the income of the country and the other controls in the estimation, a 3-4 percentage point gap is a fair summary of the evidence. It is hard to argue that this estimated effect of openness is a spurious result due to associations with other variables because it survives numerous controls for other variables. It is hard to argue that this result is due to a special way in which closed and open are defined because it survives alterations to the definition. It is hard to argue that this result is due to reverse causality from growth to trade policies since it is difficult to point to specific cases where countries opened,

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grew slowly and then closed again as a consequence of growing slowly (see Sachs and Warner 1995 for the event studies behind this).

A more technical and accurate reading of the evidence is that open economies were on a different growth trajectory than the closed economies. Their growth path had a higher steady state level of income and faster convergence to that steady state. This result offers an explanation for the "twin peaks" result emphasized in other studies, in which the time series pattern of cross-country growth is consistent with some countries converging on a low level of income and others on a high level of income. The suggestion here is that these twin peaks are related to fundamental policy differences.

According to our results, will open trade policies guarantee fast growth for poorer countries? If the stress is placed on the word *fast,* not necessarily. The difference in growth performance between closed and open countries during the post-war period is both about the poor record of the closed group as the good record of the open group. Many poor countries with high trade restrictions saw their economies decline during this period. In contrast no poor country that was open (by the standards in this paper) suffered declining growth. The evidence is as much about what *not* to do as what to do.

Does this mean that poor countries that open to trade will be guaranteed positive growth for all time? Not necessarily either. The results here are an empirical summary of the evidence from the past, not a guarantee for all time.

A possibly important drawback in the approach here is the lack of discrimination between degrees of protectionism and/or the nature of trade policy. Although the dichotomous rating of openness allows the data to discriminate at a broad level between inward versus outward development strategies, it is obviously silent about small variations in trade policies. However,

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research about broad differences in trade policy does not exclude other research about finer differences in trade policies. This paper is about the former.

The evidence in this paper is also silent about differences in the degree of activism in trade policy. This refers both to the sectorial composition of liberalization as well as the degree of export promotion. Our evidence does not exclude the possibility that what discriminates poor open economies that grew very fast from those that grew only moderately fast was the effectiveness of export promotion, or differences in the ways in which opening was implemented over time or across sectors.

Nevertheless it is clear that the overriding objective of Rodriguez and Rodrik (2000), in both its tone and content, is to belittle the importance of open trade in the development process. This paper is an attempt to remind ourselves that the empirical record of the post-war experience with inward looking development is a testament to the dangers of inward-oriented growth strategies as well as the benefits of trade integration. There are several questions that remain open for debate, but to imply somehow that the entire post-war record is a whitewash as far as the evidence in favor of trade integration is not supported by the empirical record.

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Table 1.

Country	Original Sachs-Warner Data					UNCTAD, GATT/WTO, Ng and Yeats Data							IADB Data	
	bmp 70s	bmp 80s	tariff	quota		Un-weig-hted Average tariffs	Un-weighted Average para+tariffs	Import weighted Average tariffs	Import weighted Average para+tariffs	Un-weighted Average all Non-Tariff-Measures	Import Weighted Average Non-tariff-measures		Average tariffs pre-reform (IADB)	Items subject to non-tariff barriers (pre-reform, in percent)
ALGERIA	0.76	2.27	0.13	0.00		22.9	24.9	16.4	18.9	9.5	6.9			
BOTSWANA	0.33	0.14	.	.										
EGYPT	0.77	0.36	0.10	0.25		33.5	33.5	22.9	22.9	45.2	49.9			
KENYA	0.23	0.21	0.28	0.20		43.7	44.7	36.8	37.8	37.8	29.8			
SUDAN	0.15	0.68	0.33	0.08		56.6	56.6	47	47	10	8			
TUNISIA	4.33	0.07	0.22	0.54		27.5	30.6	24	26.7	32.7	42.5			
ZAIRE	1.06	1.30	0.12	0.38		20.7	20.7	17.1	18.2	100	100			
COSTA RICA	0.21	0.40	0.16	0.70		21.1	61.7	16.4	37.2	0.8	4.1		50	
DOMINICAN REP.	0.28	0.50	.	.									90	
EL SALVADOR	0.22	1.01	0.13	0.04		21.1	21.1	15.8	15.8	19.2	10.7		22	
JAMAICA	0.41	0.56	0.11	0.10		17.3	17.5	16.2	16.7	6.6	16.7		42	
MEXICO	0.02	0.21	0.08	0.06		13.4	16.9	12.3	15.8	3.9	19		29	72
TRINIDAD&TOBAGO	0.22	0.45	0.29	0.25		17.3	43.1	16.2	41.6	23.4	33.5		82	
ARGENTINA	0.38	0.34	0.29	0.05		10.6	19.4	9.7	16.6	0.2	3.1		43	
BOLIVIA	0.23	1.19	0.13	0.04		16.7	16.7	16.5	16.5	2	3.5		21	
BRAZIL	0.19	0.36	0.16	0.05		13.2	15.4	14.7	16.7	1.5	14.3		70	62
CHILE	2.12	0.19	0.21	0.10		10.9	19.9	10.7	21.2	0.1	0.4		35	
ECUADOR	0.08	0.51	0.28	0.40		9.3	11.2	8.2	10.2	63.6	52.2		41	
GUYANA	0.47	2.09	0.12	0.01		17.4	17.5	16.3	16.4	16	15.5			
PARAGUAY	0.17	0.68	0.46	0.01		15.4	15.4	12.9	12.9	1.8	4.6		19	43
PERU	0.45	0.04	0.41	0.37		48.9	66.3	39.4	56.1	53.4	55.5		68	
URUGUAY	0.27	0.02	0.21	0.03		27.5	28.5	26.6	27.6	14.1	20.6		25	
VENEZUELA	0.00	0.47	0.18	0.00		16.4	17.4	14.6	16.2	2.4	2.8		41	
BANGLADESH	1.06	0.81	0.41	0.50		81.2	83.2	65.1	67.1	49.4	55.1			
INDIA	0.29	0.15	1.32	0.89		53	95.2	42.6	76.6	62.6	61.3			
IRAN	0.11	3.19	0.39	0.86		20.7	100.9	15.1	70.1	99.3	98.8			
IRAQ	0.17	1.10	0.09	0.18										
ISRAEL	0.25	0.10	.	.										
MYANMAR	2.83	3.79	.	.										
NEPAL	0.28	0.27	0.10	0.05										
PAKISTAN	0.56	0.35	0.41	0.08		61.1	73.3	56.2	68.7	14.5	24.7			
SRI LANKA	0.58	0.21	0.28	0.08		26.1	29.2	23.7	26.1	3.8	11.2			
SYRIA	0.08	0.44	0.16	0.54		14.8	27.5	12.6	24.5	36.6	36.2			
PAPUA N.GUINEA	0.12	0.27	0.11	0.00		7	14.2	6.3	13.1	2.6	1.3			

Table 2. Trade Policy Indicators: Africa								
Country	Import items subject to non-tariff barriers.	Percent of foreign exchange allocation that is controlled	Unwgt. Av. Tariffs	Unwgt Ave Para+ Tariffs	Import Wgt Ave Tariffs	Import Wgt Ave Para+ Tariffs	Unwgt Ave all NTMs	Imp Wgt Ave All NTMs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Benin	All		37.4	49.4	30.7	42.2	17.0	31.1
Burkina Faso	Hundreds		60.8	76.8	52.8	67.5	80.6	86.8
Burundi	All	100	36.9	37.9	28.9	29.9	0.3	0.8
Cameroon	Hundreds		32.0	42.2	27.0	37.0	20.7	15.3
Central African R			32.0	39.3	26.7	33.7	5.1	16.7
Chad								
Congo	33 categories		32.0	33.2	26.7	28.7	4.6	5.3
Gabon	30							
The Gambia		100						
Ghana	All	100	29.6	33.0	29.3	31.0	48.4	38.4
Guinea-Bissau		100						
Cote D'Ivoire	37 percent		23.3	25.3	21.5	22.9	6.6	20.6
Madagascar	All	100	6.1	40.1	5.5	36.0	1.7	1.7
Malawi	All	100	15.2	15.2	13.1	13.1	91.3	91.8
Mali	58							
Mauritania	Hundreds							
Mozambique		100	15.6	25.6	14.5	24.5	56.9	61.0
Niger	Hundreds							
Nigeria	All	100	32.8	39.8	26.5	33.5	8.8	7.6
Rwanda	Nearly All							
Senegal	Hundreds		34.2	34.2	29.8	29.9	7.2	14.9
Sierra Leone			25.8	25.8	21.8	21.8	100.0	100.0
Tanzania	Nearly All	100	29.8	29.8	22.8	22.8	79.7	80.4
Togo	20							

Uganda		100	19.9	19.9	18.1	18.1	13.9	19.7
Zambia		100	29.9	29.9	25.8	25.8	0	0.1

Of the 26 countries in this table, 18 have corroborating evidence in the sense that they are also classified as having had either "All", "Nearly All" or "Hundreds" of import items subject to non-tariff barriers, or that 100 percent of foreign exchange allocation was controlled.

Of the remaining countries, Congo was classified as Socialist by Kornai (1992) and thus would be rated as closed under our criteria for that measure, and Sierra Leone had an average black market exchange rate premium of 411 percent during the second half of the 1980s. That leaves six countries for which we simply do not have additional, published information about openness.

Table 3. Regressions testing robustness of different definitions of openness to trade.

	(1) gr7090	(2) gr7090	(3) gr7090	(4) gr7090	(5) gr7090
LGDP70	-0.49 (1.67)	-0.24 (0.77)	-0.54 (1.66)	-0.66 (1.97)	-0.55 (1.70)
OPEN1	20.75 (5.65)**				
OPEN2		21.30 (5.77)**			
OPEN3			19.90 (5.44)**		
OPEN4				21.34 (5.53)**	
OPEN5					25.27 (6.29)**
LGDP70 * OPEN1	-2.30 (4.93)**				
LGDP70 * OPEN2		-2.36 (4.97)**			
LGDP70 * OPEN3			-2.22 (4.73)**		
LGDP70 * OPEN4				-2.47 (5.07)**	
LGDP70 * OPEN5					-3.03 (5.86)**
Institutions	0.47 (3.56)**	0.47 (3.09)**	0.58 (3.86)**	0.71 (4.64)**	0.85 (5.42)**
Sec Educ	0.40 (1.26)	0.16 (0.46)	0.05 (0.16)	0.24 (0.74)	0.42 (1.32)
Constant	1.84 (0.93)	0.12 (0.06)	2.15 (0.94)	2.48 (1.06)	0.96 (0.41)
Observations	86	74	66	69	68
R-squared	0.56	0.60	0.63	0.56	0.60

Absolute value of t-statistics in parentheses
* significant at 5%; ** significant at 1%

Table 4. Regressions testing impact of openness to trade and Institutions, in particular whether openness only works in combination with good institutions.

	(1) gr7090	(2) gr7090
LGDP70	-0.56	-0.29
	(1.82)	(0.94)
OPEN5	8.01	24.66
	(4.86)**	(4.22)**
Institutions	0.73	0.68
	(3.70)**	(3.61)**
Institutions*OPEN5	-0.93	0.19
	(3.66)**	(0.42)
LGDP70 * OPEN5		-3.04
		(2.96)**
Constant	1.58	-0.20
	(0.72)	(0.09)
Observations	81	81
R-squared	0.42	0.49

Absolute value of t-statistics in parentheses
* significant at 5%; ** significant at 1%

Table 5. Regressions testing robustness of the already robust version of the openness variable to further controls.

	(1) gr7090	(2) gr7090	(3) gr7090	(4) gr7090	(5) gr7090
LGDP70	-1.08 (3.92)**	-1.26 (3.97)**	-0.70 (2.04)*	-1.20 (2.39)*	-1.78 (5.06)**
OPEN5	21.90 (6.15)**	20.14 (5.46)**	25.11 (6.40)**	22.26 (4.04)**	17.59 (4.60)**
LGDP70 * OPEN5	-2.59 (5.56)**	-2.37 (4.98)**	-3.04 (5.99)**	-2.65 (3.62)**	-2.09 (4.25)**
Institutions	0.76 (5.47)**	0.53 (3.49)**	0.70 (4.73)**	0.77 (3.79)**	0.91 (6.85)**
sec educ	0.27 (1.09)	0.37 (1.36)	0.47 (1.60)	0.77 (1.66)	0.33 (1.21)
Landlocked	-0.81 (1.76)	-0.62 (1.35)	-0.68 (1.29)	-1.02 (1.29)	-0.62 (1.34)
Tropics	-1.46 (3.64)**	-1.17 (2.92)**	-1.08 (2.49)*	-1.15 (1.78)	-0.69 (1.70)
C Gov Surplus	0.14 (3.42)**				
Saving Rate		0.08 (3.42)**			
infl7090			-0.02 (2.64)*		
fin depth				0.24 (0.19)	
SS Africa Dummy					-2.41 (4.31)**
Constant	6.26 (2.93)**	7.76 (3.26)**	4.11 (1.66)	6.80 (1.65)	10.96 (4.15)**
Observations	61	63	63	45	66
R-squared	0.77	0.71	0.68	0.65	0.74

Absolute value of t-statistics in parentheses
* significant at 5%; ** significant at 1%

Figure 1

Low import tariffs are good for growth? Think again

Source: All data are averages for the 1990s, and come from the Dollar and Kraay (2000) data set. Specifications are based on Dollar and Kraay (2000), replacing trade/GDP with tariff levels and controlling separately for initial income, government consumption/GDP and inflation rate.
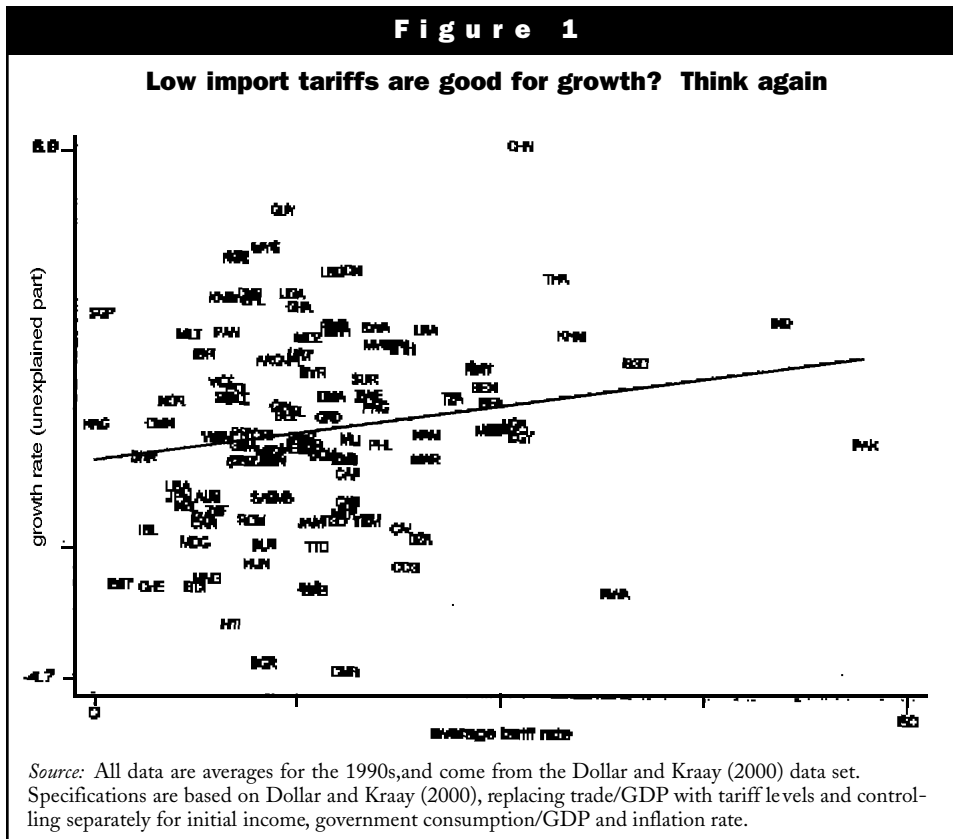
1995), Francisco Rodriguez and I found a major gap between the policy conclusions that are typically drawn and what the research has actually shown. A common problem has been the misattribution of macroeconomic phenomena (e.g., overvalued currencies or macroeconomic instability) or geographic location (e.g., in the tropical zone) to trade policies. Once these problems are corrected, any meaningful relationship across countries between the level of trade barriers and economic growth evaporates (see also Helleiner 1994).

In practice, the relationship between trade openness and growth is likely to be a contingent one, dependent on a host of internal and external characteristics. The fact that practically all of today's advanced countries embarked on their growth behind tariff barriers, and reduced protection only subsequently, surely offers a clue of sorts. Moreover, the modern theory of endogenous growth yields an ambiguous answer to the question of whether trade liberalization promotes growth, one that depends on whether the forces of comparative advantage push the economy's resources towards activities that generate long-run growth (research and development, expanding product variety, upgrading product quality, etc.) or divert them from such activities.

No country has developed successfully by turning its back on international trade and long-term capital flows. Very few countries have grown over long periods of time without experiencing an increase in the share of foreign trade in their national product. In practice, the most compelling mechanism that links trade with growth in developing countries is that imported capital goods are likely to be significantly cheaper than those manufactured at home. Policies that restrict imports of capital

Figure 6
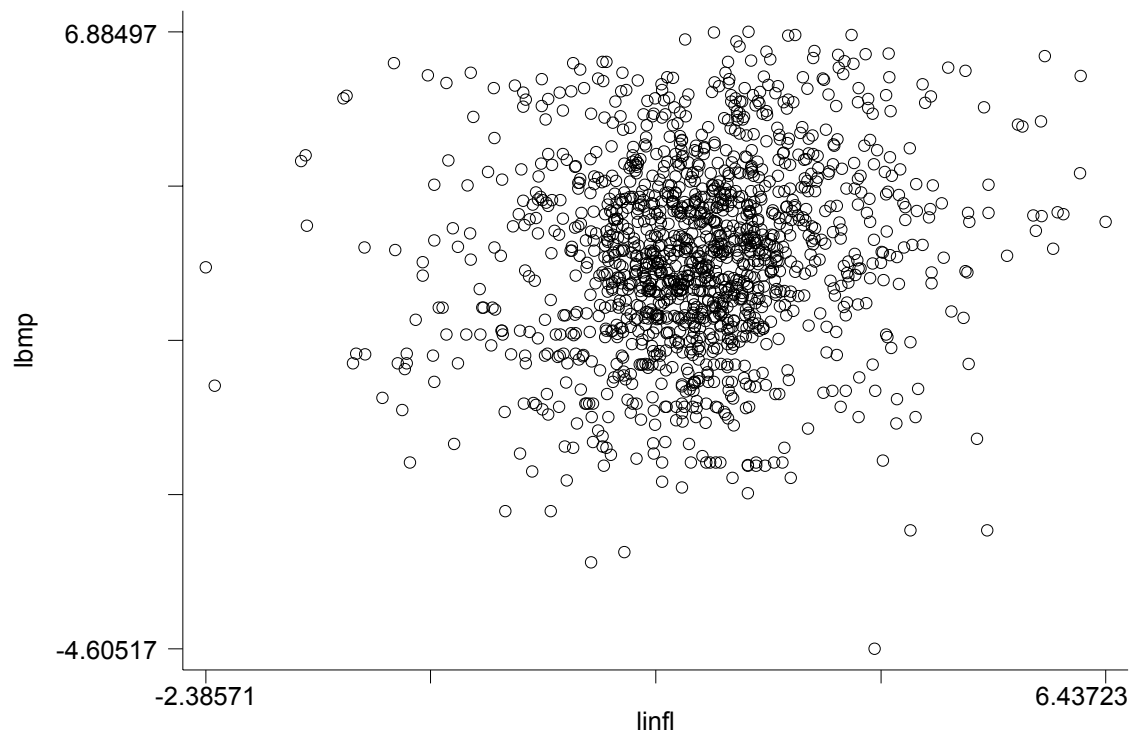
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