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The Value of Making Commitments Externally: Evidence from WTO Accessions

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Abstract

This paper studies the value of external commitment to policy reforms in the case of WTO/GATT accessions. The accessions often entail reforms that go beyond narrowly defined trade liberalization, and have to overcome fierce resistance in the acceding countries, as reflected in protracted negotiations. We study the growth and investment consequences of WTO/GATT accessions, with attention to a possible selection bias. We find that the accessions tend to raise income, but only for those countries that were subject to rigorous accession procedures. Policy commitments associated with the accessions were helpful, especially for countries with poor governance.

Keywords: Trade, Growth, Economic Reforms

JEL Classification: F1

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"It is surprisingly hard to demonstrate convincingly that the GATT and the WTO have encouraged trade."

Andrew Rose

American Economic Review, 2004

"WTO accession provides a predictable business environment and gives a powerful guarantee to investors that there will be no policy reversals."

Mamo Mihretu, advisor to the Ethiopian government on WTO accession

International Development Research Center, 2005

1. Introduction

One way a country can acquire strong commitment to pro-growth policy reforms and convince investors that it has done so is by making the commitment a part of its international obligations. Examples of such external commitment include tariff reductions in a treaty that governs the terms of a country's accession to the World Trade Organization (WTO), foregoing the right to impose capital controls in the future in a free trade agreement (FTA), a privatization scheme made as a part of the conditionality in a World Bank loan, or a tax reform plan made as a part of the conditionality in an International Monetary Fund (IMF) supported program. The value of such an external commitment is intuitive. While a government's unilateral announcement or implementation of a policy reform can be reversed or undone unilaterally, a policy reform embedded in an international treaty would involve a much higher cost of reversal. Non-fulfillment of an external commitment could trigger termination of loan disbursement from the World Bank or the IMF, or sanctions from the dispute settlement mechanism at the WTO or the FTA. In political economy terms, the benefits conferred by the multilateral organization (e.g., more secured access to foreign markets through the WTO, or loans from the IMF) can be used by the reform-minded government to buy political support from the originally anti-reform interest groups.

However, it is not a foregone conclusion that the value of such external commitment is positive. For example, some have accused IMF supported programs of having made some countries economically worse off, as they might advocate a rigid recipe of policy changes that may not be suitable for the countries (see, for example, views by Feldstein, 1999, and Stiglitz, 2002). A rigorous analysis by Barro and Lee (2005) that incorporates a clever strategy to model which countries receive IMF supported programs suggests that participation in IMF programs does not generally enhance a country's growth prospect and may have reduced it. So there is certainly room for the possibility of making external commitment to a wrong set of policies. This can be the case when the negotiating partners of the treaties do not necessarily have the country's best interest as their objective or simply misunderstand what is good for the country. Moreover, even if the commitments are good, there is a separate question of whether they can be enforced or sustained in the long run. In the case of IMF programs, the countries might reverse the prescribed reforms once the programs expire.

In this paper, we study the case of accessions to the WTO (or its predecessor, the General Agreement on Tariffs and Trade, GATT). Unlike policy commitments made in an IMF program, policy reforms mandated in a WTO accession agreement are legally binding as long as the country remains a member of the WTO. The accessions are sometimes reported with fanfare, as was the case for China in 2001. In recent years, the applicant countries have typically been required by existing members to undertake a wide range of policy changes before membership can be granted and to promise to do more within a certain timeframe after the start of membership. The required policy changes typically go beyond a reduction in tariff rates, and can encompass termination of state monopoly, greater transparency in policy making process generally, reduction in restrictions on payment and foreign exchange arrangement, and better protection of intellectual property rights.[2] As the second quote at the beginning of the paper indicates, WTO accession is thought to make it less likely for governments to reverse market-oriented reforms. Many of these policy changes would have to overcome fierce resistance from anti-reform interest groups within the acceding countries. This is reflected in lengthy and often contentious negotiations between the acceding countries and the existing members. For example, for countries that acceded to the WTO during 1995-2001, the median time it took between the initial application and the final accession was 71 months. The view that WTO accession brings about pro-growth reforms even if they may be politically difficult can be summarized by a Chinese adage: beneficial medicine may be bitter in one's mouth.

This view, however, is not universally shared. Some think that the membership is completely irrelevant. For example, Rose (2004) reports that WTO/GATT member countries do not appear to trade any more than non-members do. As Subramanian and Wei (2007) point out, since most developing-country members of the GATT/WTO acceded to the trade body at a time when very few reforms were required of them, it is not difficult to understand the irrelevance results. If WTO membership does not even lead to a more open trade regime, then it is hard to see how it could deliver beneficial reforms in other ways. So, in this case, the medicine is neither bitter nor effective.

According to some, accession to the WTO may even mean making counterproductive external commitments. The policy changes demanded by existing members of the WTO/GATT might narrow the "policy space," and force the acceding countries to choose inferior policies that they otherwise would not have chosen. In a book entitled, "Behind the Scenes at the WTO: the Real World of International Trade Negotiations," the authors Fatoumata Jawara and Aileen Kwa suggest that WTO negotiations place the interests of powerful developed countries ahead of everyone else and often coerce developing countries into signing something that they profoundly disagree with. By this view, the medicine is not only bitter but also poisonous.

[2] More examples of reform conditions in recent accession cases that have implications outside trade are given in Table 10 of the NBER working paper version.

In the first four decades of the GATT, developing countries were not asked to do much reform if they wanted to join the club. Indeed, many of them retained very high bound tariff rates even after becoming GATT members. However, the Uruguay Round of the GATT negotiations signifies a drastic change. One objective of the Uruguay Round was to bridge the gap between the developed and developing countries in terms of their degree of liberalization and obligations. New acceding countries are subject to much more stringent accession requirements. For instance, under the old GATT rules, an existing member might be able to invoke nonapplication only on the condition that it had never entered bilateral negotiations with the acceding country; however, under the new WTO regime, an existing member could opt to not extend its WTO-related benefits to the new member even after they had held bilateral negotiations. For example, the United States had invoked the nonapplication clause against the Kyrgyz Republic, Mongolia, and Georgia, even after it had held bilateral negotiations with them. The United States would not have been allowed to exercise nonapplication in such a situation in the GATT era (Drabek and Bacchetta, 2004). Such threat of ex-post nonapplication potentially strengthens the leverage of existing members over an acceding country during the bilateral negotiations, and thus enable them to extract more concessions from the new member.

Subramanian and Wei (2007) document that these new (i.e., post-Uruguay) members tend to be systematically more open than old developing country members of the GATT. On average, new developing country members of the WTO/GATT trade about 30 percent more than the old developing members. Thus, accessions to the WTO/GATT after the Uruguay Round offer an opportunity to empirically study the value to a country of making policy commitments externally.

Specifically, in this paper we investigate whether and how WTO/GATT accession between 1990 and 2001 alters a country's growth trajectories. The empirical method we employ is in spirit a difference-in-differences strategy: comparing the change in the growth rate of the acceding countries before and after accessions with the change in the growth rate of nonacceding developing countries. Our results show that, relative to other developing countries, countries that became WTO members did generally grow faster than before, and the increments in their ratios of investment to GDP were greater as well.[3]

Any good economist would instinctively ask whether there is any endogeneity bias in this result. Specifically, is it possible that only countries that would pursue pro-growth, open-trade policies anyway would apply for GATT/WTO membership? Researchers might find a positive association between accession and an increase in the growth rate even though the former may not cause the latter. In some sense, we are just as happy with the possible result that reforms designed to promote trade openness

[3] We choose to leave developed countries out of our analysis. About half of the developed countries were cofounders of the GATT. A majority of the remaining ones had joined the GATT by the mid-1950s. We do not want to make developed countries be part of the control group (when the treatment group consists of developing countries) as we wish to compare the like with the like. We do not want to make the developed countries be the treatment group since it is not possible to construct a meaningful control group that consists of other developed countries. An interesting paper by Staiger and Tabellini (1999) shows that developed countries did gain policy commitment by embedding policy reforms as part of the "concessions" made in the Tokyo Round of the GATT negotiation.

rather than WTO accession per se have increased growth. In this scenario, application for GATT/WTO membership is simply a demonstration of a government's resolve to switch to a more open trade regime. Our exercise can be seen simply as a new angle to check the consequence of trade reforms for growth.

We, however, document a number of patterns in the data that enhance our confidence that the WTO commitments may causally improve investment climate and help to raise the growth rate. Besides implementing a Heckman procedure that explicitly models the selection issue, we also make use of a number of economic and institutional features of the WTO accession process that turn out to be informative. We summarize these features below.

1. Accessions with and without extensive reforms. If accession involves no binding commitments, then the endogeneity bias is highly plausible. However, plenty of evidence shows that the accession negotiations can be very demanding on the acceding countries, often with anti-reform interest groups resisting strenuously the reform requirements from the existing members. The long accession negotiations (with an average of about five years) indicate the immense political difficulty many acceding countries have in implementing various reforms required of them. In fact, an interesting difference among the accession countries is informative about whether accession-related reforms have helped to change the domestic investment climate. Up to the end of 1994, a subset of developing countries was eligible to join the GATT under Article XXVI 5(c) by essentially sending a notification to the GATT without having to promise reforms. Existing members could not block the accession and therefore could not impose demands that the acceding countries would feel reluctant to fulfill. In contrast, the rest of the developing countries would have to undergo rigorous negotiations with existing members because any of the latter countries could block the accession. Almost all Article XXVI 5(c)-eligible countries joined the GATT by 1994 without making extensive reform commitments. We will show that the positive impact of WTO/GATT accession comes entirely from countries that were required to undergo more rigorous accession negotiations.

2. Application vs. actual accession. It is possible that an application for membership may signal that the government has become reform-minded and may pursue pro-growth reforms regardless of the membership. Because a long and variable lag typically exists between the date of application and that of the eventual accession, we can exploit this gap to isolate the effect of accession-induced reforms from the effect of reforms that a government wants to implement anyway. We find that there is a (temporary) pickup in the growth rate subsequent to the initial application. However, even after accounting for this pattern, we continue to find a distinct growth spurt after the actual accession.

3. Modeling the selection effect. We also explicitly test for and quantify the effects of self-selection on economic performance by employing a two-stage procedure a la Heckman (1979). We do not find evidence of a strong selection bias that drives our result.

4. Effects of reform commitments on countries of different governance quality. If accession has no additional economic impact beyond signaling a government's resolve to pursue reforms unilaterally, then the association of accession with growth does not have to vary with the quality of public governance. We look at whether and how accession-induced policy reforms have differential impacts on countries of different governance quality. We find that the policy commitments through WTO accession appear to be more beneficial in countries with weak governance. This suggests that the external policy commitments may serve as a (partial) substitute for governance in promoting economic development.

These four features are based on economic as well as statistical arguments. Taken together, they suggest that WTO/GATT accessions, when rigorous reforms are required, have led countries to engage in a wide range of reforms, improve the general investment climate beyond narrowly defined trade areas, resulting in an acceleration of their growth rates around the time of the accession.

Note that the accession may lead only to a one-off increase in the income level (though with a transition period of several years), not necessarily to a permanent increase in the growth rate. Of course, a temporary increase in growth rates for a few years implies a permanently larger economy and a permanently higher living standard in the end. So it is still economically significant. In any case, given that WTO accession cases are relatively recent, available data would not allow us to discriminate between a growth effect and a level effect that spreads over several years.

Besides studying the value of commitments, this paper contributes to the literature on the effects of the WTO/GATT. Rose (2004), Subramanian and Wei (2007), and Goldstein, Rivers and Tomz (2007), among others, study the trade volume effects of the WTO (with different conclusions). Li and Wu (2004) explore the average effects of WTO/GATT accessions on growth during 1960 and 1998, but do not take into account the qualitative change in the nature of the accession process since the Uruguay Round, the role of Article XXVI 5(c), and the difference between applications and actual accessions. Ferrantino (2006) examine association between the accessions (and free trade agreements) and governance.

In the following section, we briefly describe the data and our empirical methodology before presenting our results. We discuss the selection issue in greater detail in section 3. Section 4 explores the role of policy commitments as a substitute for good governance. We conclude in section 5.

2. Empirical Evidence

2.1 Data and Empirical Specifications

The main variables employed in our regressions include per capita GDP, private investment, total investment, exports and imports of all the developing countries between 1981 and 2003. All these data, at annual frequency, are obtained from the IMF's *World Economic Outlook*. The panels are not always balanced, since some smaller countries might not have data for earlier years. The years the countries formally acceded to WTO/GATT are taken from the WTO's website. We exclude all OPEC and industrial countries. Table 1 lists all the countries in our treatment and control groups. In most regressions, we also exclude ten outliers from the control group, five from either end of the spectrum (however, as we will report later, our results are robust to not excluding the outliers). In later subsections, we will use additional variables such as governance indices and a measure of the extent of policy commitments. The sources and construction of those variables will be discussed in due course.

The two principal sets of regressions we use look at the effects on growth and investment at annual frequency. They take the following forms.

$$G_{i,t,s} = \beta_0 \log(GDP\ per\ capita)_{t-1} + \sum\nolimits_s \beta_s + \beta_i + \beta_t + \varepsilon_{i,t,s}, \quad \text{and}$$

$$\log(Inv/GDP)_{i,t,s} = \sum\nolimits_s \beta_s + \beta_i + \beta_t + \varepsilon_{i,t,s}.$$

$G_{i,t,s}$ and $\log(Inv/GDP)_{i,t,s}$ are, respectively, annual growth of per capita GDP and the log of the investment/GDP ratio of country *i*, in year *t*, and *s* years away from accession. We refer to the set of *s*'s as the time profile of accession. In most of our specifications, *s* belongs to {null, -2, -1, 0, 1, 2, 3, 4, 5, beyond}; *s* is null if either the country is not in our treatment group or it would not accede until more than two years later. Correspondingly, β_s is set at zero when *s* is null; β_i and β_t are country and year fixed effects, respectively. The log of lagged per capita GDP is included in the growth regressions to take into account the long-term converging and short-term mean-reverting effects.

2.2 Benchmark Result (GATT/WTO Accessions During 1990-2001)

One of the objectives of the Uruguay Round was to raise the developing countries' obligations to adopt more open trade regimes. Even for countries that joined the GATT after the commencement but before the conclusion of the Uruguay Round, Subramanian and Wei (2007) show evidence that accessions have led them to become more liberalized relative to both pre-existing members as well as nonmembers.

Guided by Subramanian and Wei's results, we focus on countries that acceded between 1990 and 2001. The summary statistics on growth, trade and investment for this group of countries before and after accession are reported in the first column of Table 2.

We perform our growth and investment regressions as specified earlier. Figures 1 and 2 plot, respectively, the trajectories of the changes in growth and investment for the accession countries relative to the control group after taking into account other control variables in our regressions. The 90% confidence intervals are derived from robust standard errors clustered by country. As Figure 1 shows, in the year before accession countries are growing about 2.4 percentage points faster than before, relative to other countries. The growth rates stay high in the four subsequent years. These increases in growth are statistically significant. In comparison, as Figure 2 shows, while accessions are associated with an increase in the investment ratio, the effect is not statistically significant.

The regression details are reported in Table 3A. Ten outlying control-group countries are excluded from the first two columns of the table, while the last two columns report results without such exclusion. Both the coefficient estimates and their significance levels are essentially the same across the two sets of results. Following Wooldridge (2002) and Drukker (2003), we also perform an F-test for first-order serial correlation in the error term in our linear panel-data model, and find no evidence of first-order serial correlation at the 10% level (with F(1, 104)=2.31 and a p-value of 0.13).

One may wonder whether the growth effect of GATT accessions prior to the Uruguay Round is statistically and economically significant. The working paper version (Table 3B) reports the growth and investment regressions for these earlier accessions (compared with non-accession countries during the same period). In the growth regression, the point estimates are all positive, but much smaller than the estimates for the more recent accessions. Moreover, the growth effect of the earlier accessions is not statistically significant at the 10% level. Similarly, the effect of the earlier accessions on private investment is not statistically significant either. This contrast between the early and the more recent accessions is not surprising, since developing countries in the early episodes were typically exempted from undertaking comprehensive economic reforms under the principle of special and differential treatment. In fact, the early accessions did not even make these developing countries more open in the trade area, as documented by Subramanian and Wei (2007). In the rest of the paper, we will focus on accessions that take place since 1990.

Article XXVI 5(c) vs. Non-Article XXVI 5(c) Countries

The results shown in Figures 1 and 2 mask a substantial degree of heterogeneity among the countries in terms of their accession procedures. Before the WTO replaced the GATT in 1995, former colonies of the GATT members could, upon becoming independent, invoke GATT Article XXVI 5(c). The article had

allowed them to be converted to full members ("contracting parties") without having to undergo the kind of lengthy negotiations that often characterize the accession processes of other countries.[4]

Although once they had become full members they were required to fulfill more obligations (e.g., notifying GATT/WTO about any alteration of their trade policies to deal with balance of payments problems), policies of the countries acceding by Article XXVI 5(c) were not rigorously reviewed before the countries were granted accession. As a result, the extent of policy reforms those countries were required to commit to was substantially less. It is arguably a main reason why a host of countries that were eligible for Article XXVI 5 (c) flocked to accede to the GATT immediately before the WTO was established.

Between 1990 and 1994, 18 countries invoked Article XXVI 5(c) and acceded to the GATT.[5] Table 4 lists the countries acceding by Article XXVI 5(c) and those by normal procedures. In terms of changes in economic performance before and after accessions, the two groups differ considerably. As shown below, accessions seem to have much stronger impacts on the non-Article XXVI 5(c) countries than on the others. The comparison is between annual growth (or private investment/GDP) averaging over zero to two years after accession and annual growth averaging over eight years before accession, after controlling for year fixed effects.

Share of acceding countries growing faster after the accession than before:

	Grew Faster than Before
Non-Article XXVI5c Countries	72% (18/25)
Article XXVI5c Countries	47% (8/17)

Share of acceding countries investing more output after accession than before:

	Invested More than Before
Non-Article XXVI5c Countries	59% (13/22)
Article XXVI5c Countries	38% (6/16)

Further summary statistics of ArticleXXVI5(c) and non-ArticleXXVI5(c) countries before and after accession are reported in the second and third columns of Table 2. We note that the pre-accession growth behaviors between the two groups of countries are quite similar (the difference in the mean of their pre-accession growth is not statistically significant at the 5 percent level); but the average post-accession growth of the non-Article XXVI 5(c) countries is significantly faster than that of the Article XXVI 5(c) countries.

[4] The full text of Article XXVI 5 (c) is as follows: "If any of the customs territories, in respect of which a contracting party has accepted this Agreement, possesses or acquires full autonomy in the conduct of its external commercial relations and of the other matters provided for in this Agreement, such territory shall, upon sponsorship through a declaration by the responsible contracting party establishing the above-mentioned fact, be deemed to be a contracting party."

[5] Cambodia and Algeria were the only two countries that were eligible for Article XXVI 5(c) but did not use it, probably because both were reluctant to ask France to sponsor their accessions – a requirement for invoking Article XXVI.

About half of the countries in the Article XXIV5(c) group are small island economies. This suggests that it is important to include country fixed effects in a regression framework. We perform the same regressions as those that are reported in Table 3, except that we now use dummies to separate the set of countries that acceded to the world trade body through Article XXVI 5(c) from the rest. The results on growth and investment are plotted in Figures 3 and 4, respectively. In contrast with the results for the whole sample, non-Article XXVI 5(c) countries grow significantly faster than before ever since one year before accession. The growth performance of non-Article XXVI 5(c) countries is generally stronger than that of the Article XXVI 5(c) countries. Moreover, the accession effect on growth seems longer-lasting. Its economic and statistical significance persists even beyond the fifth year after accession.[6] On the other hand, accessions have only a very weak effect, if at all, on the Article XXVI 5(c) countries. For instance, in the second year after accession, the Article XXVI 5(c) countries grew only 0.8 percentage points faster than before, and it is not statistically different from zero.

Distinction between the two groups is also apparent in Figure 4. Compared with before, non-Article XXVI 5(c) countries invested more of their output than before, relative to other countries. For example, in the third year after accession, this group of countries on average increased their investment/GDP ratio by about 18 percent from before. In contrast, Article XXVI 5(c) countries on average increased their investment/GDP ratio by only 3 percent from before in year 3 post-accession, and this increase is not statistically significant. The results suggest that the extensive policy commitments a government has to make before accession appear to play an important role in raising output and investment. The regressions are also reported in detail in Table 5. Ten outlying control-group countries are excluded from the first two columns, while no outlying countries are excluded in the last two columns. The two sets of results are basically identical.

As shown by Sala-i-Martin and others (2004), some variables are robustly correlated with growth. These variables include investment price, fraction of GDP in mining, government consumption share and real exchange rate.[7] In regressions reported in the working paper version (Table 6), we re-run our growth regression but with these variables added in as control variables. Although not shown to be robustly related to growth in Sala-i-Martin and others (2004), we also include dummies for revolution, coup and major cabinet change on the right hand side to capture social and political spillovers to the economy.[8] Our results – both the coefficient estimates and their significance level – are basically unchanged with the inclusion of the revolution, coup, and cabinet change dummies, although each of these additional control variables is statistically significant. Next, we include also the four variables motivated by Sala-i-Martin and

[6] Countries that were not eligible for Article XXVI 5(c) acceded by either GATT Article XXXIII (mostly before 1995) or Marrakesh Article XII (mostly after 1995). In a regression not reported here, we find that there is no statistically significant difference in post-accession performance between these two groups of non-Article XXVI 5(c) countries.

[7] While there are other variables that are shown to be robustly correlated with growth in Sala-i-Martin and others (2004), they vary little over time, and their effects are already mostly captured by the country fixed effects, which are included in our regressions.

[8] Source: Banks Cross-National Time-Series Archive.

others (2004). While the coefficient estimates are somewhat lowered, in a sense it is not surprising since these four additional control variables are each likely to be affected by WTO/GATT accessions. We also note that most of the coefficient estimates on the accession time profile still remain statistically significant at the 10 percent level. Lastly, we also control for changes in the total trade to GDP ratio. Although the coefficient estimates are reduced further, the statistical significance of most of them still remain above the 10 percent level. The result suggests that WTO/GATT accessions might affect a country's growth through affecting its trade volume, but this is not the only channel.

3. The Selection Issue and Other Robustness Checks

One might cast doubt on the exogeneity of accessions. In particular, do our results for the non-Article XXVI 5(c) countries simply reflect the possibility that countries more likely to experience stronger future growth anyway self-select to accede to WTO/GATT? In this section, we tackle this issue with economic as well as statistical arguments.

3.1 The Gap between Applications and Accessions

We exploit the long and variable lag between the dates of application and the dates of actual accession. Suppose the concern is that only pro-growth governments would apply for WTO membership, then the change in the growth rate associated with the event of application would capture this "government type" effect. We can then investigate if there is any additional increment in the growth rate around the time of actual accession after taking into account whatever happens around the time of application. The results on growth and investment are reported in Figures 5 and 6, respectively. As the figures show, there is indeed an increase in growth and the investment/GDP ratio in the two to four years after application, and this might be associated with the government's pursuit of various reforms that might or might not be related to WTO/GATT accession. However, from that point on the improvement dies down as time progresses. Most interestingly, the positive effects pick up again as the country approaches the time of accession. The coefficient estimates are also reported in Table 6. These results strongly suggest that accessions make independent contributions in encouraging investment and raising output.

Alternatively, one might proxy for the political difficulty the government faces in carrying out unilateral reforms (and thus its likelihood of carrying out pro-growth reforms independent of the accession) by the time length of negotiations with the Working Party. Presumably, the stronger the resistance the interest groups put up against reforms, the less likely the government has enough support to accept the Working Party's terms, thus the lengthier the negotiations would become. However, we do not find any significant relationship between length of negotiations and growth. In any case, inclusion of this variable does not alter the qualitative aspect of our results (not reported to save space). This seems to validate the independent effects of accession.

Separating the timing of application and that of actual accession also helps address another endogeneity concern: a government might choose to join the WTO/GATT only when it is more politically expedient to do so (e.g., during an economic upturn). Anecdotal observations suggest, however, that there is generally a long and uncertain gap between the date of WTO membership application and the date of actual accession. Although a government might well strategically time its application to the WTO, the actual accession date is often driven mostly by the politics and economics of the Working Party members instead. Take the Russian WTO application as an example. While the timing of the initial application has to do with politics in Russia, the date of final accession is mostly controlled by the United States, the last country in the Working Party to sign a bilateral agreement. In other words, the timing of the actual accession is less likely to be driven by the business cycle and politics of the applicant country. For the purpose of dealing with the interpretation of strategic timing of joining the WTO/GATT, we can make the extreme assumption that all the positive growth effect at the time of initial application reflects the endogenous nature of the application (which is likely to be an overkill). Conditional on the effect of the application, we would argue that the positive growth effect of the eventual accession is less likely a result of domestic politics and business cycle features of the accession country.

The Heckman approach discussed below is another attempt to net out the effects of strategic timing by the accession countries. As panel (and not only cross-country) data are utilized, the strategic timing factors can be captured in our econometric setup.

3.2 Testing for Selection Bias with Heckman Procedure

To the extent that WTO/GATT membership status might not be strictly exogenous, it is possible that our results are biased by some unobserved or omitted variables that affect both the membership status and changes in the countries' economic performance. To see whether this is the case, we employ a two-step procedure pioneered by Heckman (1979) with modifications tailored for panel data as suggested by Wooldridge (1995). Specifically, we first carry out a probit regression estimating the WTO/GATT membership status of a country (member or nonmember) as a function of observable country features (the country's lagged log per capita GDP and lagged log trade to GDP ratio). The choice of the independent variables is guided by the theoretical literature on the benefits of WTO/GATT membership (as commitment to trade liberalization: e.g., Maggi and Rodriguez-Clare, 1998 and 2007; as neutralization of terms-of-trade effects: e.g., Bagwell and Staiger, 1999); please see the appendix in the NBER working paper version for more discussion. Then for each country-year observation we compute the inverse Mills ratio, which contains information about the unobserved factors that also affect the country's membership status in that particular year. In the second stage, we add in the inverse Mills ratio as an independent variable in our estimation of growth or investment regressions. The inclusion of the ratio is supposed to control for the effects of the unobserved factors from the first stage on the dependent variable in the second stage, thus ensuring that the coefficient estimates in the second stage are purged of biases

resulting from the endogenous nature of membership status. On the other hand, if selection bias is absent – i.e., the dependent variable in the second stage is not affected by the unobserved factors affecting the membership status – the coefficient estimate of the inverse Mills ratio would not be statistically different from zero. In such a case, our original specification would have little bias, and our benchmark results would be valid.

The tests of the selection bias are presented in Table 7. In the growth and investment regressions in Columns 1 and 2 (for which lagged GDP, lagged trade/GDP, and lagged proxies for constraint on government executive power and for political tie with the US are included as the first-stage independent variables) and in columns 3 and 4 (for which lagged average statutory tariff imposed on imports is included as an additional first-stage independent variable), the coefficient estimates of the inverse Mills ratio are all statistically insignificant (the *p*-values are 0.18, 0.30, 0.46, and 0.53, respectively). Therefore, there is no evidence of a quantitatively significant amount of selection bias present. This is perhaps not surprising because all recent accession cases (except those that were able to invoke Article XXVI 5(c)) involve substantial policy changes that the countries would not have embarked on if they had been left alone. In any case, when we include the inverse Mills ratio from the selection equation, accessions still appear to have significant positive impacts on growth and investment.

Subsample for Which the Error Term in the Selection Equation is Normally Distributed.

In usual instrumental variable regressions, it is absolutely necessary for instruments to satisfy relevant exclusion restrictions. While one or more of our first-stage independent variables (e.g., degree of checks and balance in the government, UN voting record) might qualify as an excluded variable, one useful statistical property associated with the Heckman selection procedure – different from instrumental variable regressions – is that identification can also be achieved through the non-linearity of the inverse Mills ratio in the second stage if the error term in the first stage probit regression follows a normal distribution.[9] We now seek to take advantage of this property.

We perform a Lagrange multiplier test proposed by Bera, Jarque, and Lee (1984) to our first-stage probit. The null of this test is that the error term is normally distributed, and the test statistics follow chi-squared distribution with two degrees of freedom (for which the critical value at the 10 percent rejection level is 4.61). For regressions 2-4, the null is not rejected, thus supporting the assumption that the error terms in the first-stage probit regressions are normally distributed. While the normality assumption is rejected for the full sample (2,166 observations used in regression 1), it is not rejected for the sub-sample (1,832 observations) for which we have private investment data (i.e., the sample for regression 2). Specifically, the *p*-value of the Bera-Jarque-Lee test statistics is 0.64 for the subsample. We rerun our selection-test procedures for growth, but now based on the subsample for which the first-stage error term can be

argued to be normally distributed. The results are reported in column 5 of Table 8. The coefficient estimates on the accession time profile and the Article XXVI 5(c) interaction terms in column 5 are similar to those in column 1. Equally interesting, in neither case is the coefficient estimate on the inverse Mills ratio statistically significant. Finally, we note that we obtain essentially the same results if we jointly estimate the selection and main regressions with maximum likelihood (not reported to save space).

3.3 Comparability of Treatment and Control Groups

One might think that prior to accessions there is maybe an intrinsic difference between the structures of growth paths followed by the treatment group (i.e., the acceding countries) and the control group (i.e., the nonacceding countries), thus rendering the comparison of growth performance between the two groups inappropriate. To assess this concern, we test whether the residuals of the growth regression (controlling for log of lagged GDP per capita, year fixed effects, and country fixed effects) for the treatment-group countries at least three years prior to their accessions and those for the control-group countries appear to be similarly distributed. There are 152 and 1,272 observations from which the residuals for the treatment group and control group are computed, respectively. We find that the means of residuals for the two groups are both essentially zero. The standard deviation of the treatment-group residuals is 0.07 while that of the control-group residuals is 0.06.

We also perform a Kolmogorov-Smirnov test on the distributions of the residuals. The p-vale of the test is 0.14, and thus one cannot reject the null that the two sets of residuals are drawn from the same distribution. In other words, after taking account of the control in our growth-regression specification, the growth behaviors of the acceding countries prior to their accessions appear to be similar to those of the non-acceding countries. Therefore, our results are unlikely to be attributed to the accession countries' ex ante difference from the control-group countries.

3.4 Transition Economies

There are 14 transition economies in our sample of 25 non-Article XXVI 5(c) countries. There is a possibility that the transition economies are different from other developing countries. We separately track the effects of WTO accessions for transition and non-transition economies by adding a dummy, TE, and its interactions with a sequence of time dummies. In this specification, the first half of the coefficients describes the growth trajectory after WTO membership for non-transition economies. In the table reported in the working paper version, it can be seen that there is a statistically significant increase in growth rates in the first two years following accession. Therefore, at least some of the positive growth effects of WTO membership are independent from the transition economies. The results on private investment are

[9] The procedure, however, makes no assumption about the distribution of the error term in the second stage (see Wooldridge, 1995).

somewhat weaker – the coefficient estimates are positive and statistically significant only in year 4 (and "beyond").

The second part of the coefficients tracks the growth effects of WTO membership for the transition economies. Judged by both the point estimates and the t-statistics, these effects tend to be larger and more significant than their counterparts for non-transition economies. We do not think these positive growth effects simply reflect an economic rebound in the early stage of the transition. After the collapse of communist regime in 1990-91, most transition countries applied for GATT membership in 1993-94. However, they did not become members until an average of 5.6 years after their applications (see Table 1 for more information), or 8.7 years after their political regime change. Such a long interval renders it unlikely that the direct effects of regime change account for the increases in their growth and investment around the time of WTO accessions. Moreover, while the political regime change in these countries happened around the same time, their dates of WTO/GATT accession vary widely between 1994 and 2001. Note also Hungary, Poland, and Romania acquired their GATT membership before 1990, and therefore are part of the control group.

On the other hand, the growth effects of the transition economies could partly reflect the consequence of a large number of economic agreements between these economies and the European Union (e.g., Europe Agreements, Partnership and Cooperation Agreements, TCECA, Stabilization and Association Agreements, EU application) (mostly signed between 1993 and 1996). While it is difficult to isolate the effect of WTO accessions from that of the EU agreements for these countries, the general idea that external treaties could enhance the commitment ability of these governments is still valid.

In the working paper version, we report results by focusing only on non-transition economies and compare their average economic performance eight years before accession relative to that three years after accession (the sample becomes too short if we go beyond three years). We find significant improvement in the countries' growth rates after accession. This again confirms the conclusion that the positive growth effects of WTO membership is not unique to transition economies.

3.5 Consistency of Estimates

Since our data do not have long time series, our panel fixed-effect estimates are potentially inconsistent. In particular, in the growth regression, the log of lagged per capita GDP regressor might be endogenous. To check if this could bias our result, we adopt a two-step procedure. In step one, we use Blundell and Bond's system generalized method of moments (GMM) to estimate the following relationship

$$G_{i,t,s} = \beta_0 \log(GDP\ per\ capita)_{t-1} + \beta_i + \beta_t + \varepsilon_{i,t,s},$$

based on a sub-sample of non-acceding countries. The estimated $\hat{\beta}_0$ is consistent and equal to -0.21 for our sample. In step two, we impose the estimate on our original growth regression to estimate β_s – the coefficients on the time-profile of accession. The results are reported in the working paper version. Although the $\hat{\beta}_0$ based on Arellano-Bond GMM estimation is different from that in the panel fixed-effect estimation, the coefficient estimates on the time-profile of accession and its interaction with the transition-economy dummy are virtually unaffected. There continue to be positive and significant pickups in growth even for non-transition economies during the first two years after accession. Moreover, the residuals from step two (with the Blundell-Bond system GMM estimate of β_0 imposed on the growth regression) for the control group and the treatment group for the pre-accession years continue to appear to be similarly distributed. The *p*-value of the Kolmogorov-Smirnov test is 0.32, indicating little evidence that the two sets of residuals are differently distributed.

4. Is External Commitment a Partial Substitute for Better Governance?

Poor public governance including corruption and deviation from rule of law appears to inhibit economic development in many countries. We now examine the interactions between policy commitments made under WTO accessions and the quality of a country's public governance. Ex ante, there are two opposing possibilities. The first hypothesis posits that poor-governance countries benefit more from the external commitment. These countries are least likely to enact and carry through reforms unilaterally. So the external commitment can induce them to do more than they otherwise would have done. On the opposite side, a second possibility is that the countries with weak governance may have lower capacity to carry out any given reform commitments in the accession agreement. Which of the two possibilities dominates is an interesting empirical question.

We focus on the 15 countries that have joined the WTO since 1995 in order to take advantage of the standardized format of the Working Party reports that list the reform commitments of these countries. The summary statistics on growth, investment and trade before and after accession for this group of countries are shown in the last column of Table 2.

Upon receiving an application for the WTO membership, a Working Party composed of any interested existing members is formed to negotiate with the government for a series of commitments to policy changes, which broadly fall into two categories. One is market-access commitments that dictate the extent to which the domestic markets for goods and services are open to other WTO members. The other type of commitments concerns the government's other internal policies that may be trade-related but may also have considerable impacts on many other economic fronts. These commitments cover a wide range

of topics. For instance, a country might be required to commit to 1) not restrict any private firms' ability to import or export, 2) make transparent its future privatization plans, 3) refrain from providing certain subsidies, 4) abort state trading, and 5) eliminate price controls. A recent report by the U.S. Government Accountability Office commented that "China also has made a substantial number of important, specific commitments [in WTO accession negotiations] in the rule of law-related areas of transparency, judicial review, uniform enforcement of legal measures, and nondiscrimination in its commercial policy" (GAO-05-53, 2004). These commitments are explicitly incorporated in the Protocol as an integral part of the formal accession agreement enforceable through WTO's dispute settlement mechanism – unlike other statements made in a Working Party Report not reproduced in the Protocol, the stated commitments are legally binding. Drabek (1996) discusses how the commitments required for accessions might improve productivity and efficiency generally, rather than just in the trade area, in transition economies.

4.1 Measuring Policy Commitments and Governance Quality

The standardized format of the Working Party Reports in the WTO era ensures that every commitment item is clearly stated in the documents across the various acceding countries. Each single commitment pinpoints one particular area of policy. We adopt a simple and transparent approach by counting the total number of commitments mentioned in the Working Party Report as our proxy for the degree of a country's external commitment. Although it is by no means ideal, this measure is likely to embed a considerable amount of information about the *order* of the countries in terms of how stringent the policy requirements they were subject to.[10] By this metric, there is substantial variability in the degree of commitment. There is not a single, one-size-fits-all set of commitments applied to every country. The first column of Table 8 presents the number of commitments made by the 15 countries in our treatment sample. The median number of commitments is 27. China is a clear outlier with 147 commitments.

We proxy for a country's governance quality with the earliest edition (1996-97 edition) of World Bank's Governance Matters indices (Kaufmann, Kraay, and Mastruzzi, 2005), which in turn are based on 32 data sources compiled by 30 different organizations. For our purpose, we pick two of the indices' six dimensions that appear to be the most relevant to investment decisions and most likely to be areas that the accession negotiations focus on. They are "Regulatory Burden" – measuring incidence of market-unfriendly policies, and "Rule of Law" – measuring the quality of contract enforcement. We will refer to 3 plus the sum of a country's indices in the two dimensions as the country's governance index. The higher the score, the better the governance quality is. The second column of Table 8 lists the governance index for our treatment sample.

[10] No ex post evaluation of how closely each country observes its accession commitments is available on a systematic basis. The documents that resemble the most such evaluation – Trade Policy Reviews published by the WTO Secretariat – primarily focus on clarifying rather than evaluating the countries' trade policies.

4.2 Differential Effects of Policy Commitments

We can check whether having to make more commitments to policy changes boots an acceding country's growth and investment on average, by including the number of commitments as an additional regressor. It turns out there is indeed some statistical support for this hypothesis (reported in Table 12 of the working paper version).

To test the idea that external commitments may be a partial substitute for quality of governance, we check how the effects of policy commitments on a country's growth vary with the country's governance quality. We multiply the time profiles of accessions with the interaction terms of our measure of policy commitments and the governance index. The results, presented in the first and second columns of Tables 9, give support to the "substitute" hypothesis. The coefficient estimates on the interaction terms are significantly negative in both regressions, suggesting the positive effects of policy commitments are stronger among poor-governance countries.[11]

For further robustness check, we impose a binary structure on our commitment and governance variables. The small sample renders complete (2-by-2) categorization by these two variables infeasible; instead we separate the countries into three different groups. Countries with governance index above 3, which corresponds to the mean of all countries, are called "Good Governance" and the rest are called "Poor Governance" countries. Within the "Poor Governance" group, any countries that have more than 27 commitments (median of the sample) are called "Many Commitments," and the rest "Few Commitments." By this categorization, we have five countries in the (Poor Governance, Many Commitments), four in the (Poor Governance, Few Commitments) and six in the (Good Governance) groups.

The last two columns of Table 9 report the results. The (Poor Governance, Many Commitments) group is the benchmark group. Collaborating with the previous results, those with fewer commitments generally did not have as strong a pickup in economic performance as those with more commitments. Confirming the differential impacts of policy commitments, we find that among the good-governance countries, those with most commitments (Jordan and Lithuania) showed *smaller* improvement in growth and investment to GDP ratio than those with fewest commitments (Latvia, Panama and Estonia).

Overall, these results on the differential effects of policy commitments not only suggest their positive causal consequences, but also lend support to the view that the policy changes imposed by a third party particularly benefit countries with poor governance – they appear to be partial substitute for good governance.

[11] Our results also hold when we use other measures of governance, namely Doing Business index of legal rights and Heritage Foundation index of overall economic freedom.

5. Conclusions

Using WTO accessions as a case study, this paper investigates the value of making policy reform commitments externally. Some developing countries were eligible to obtain membership without serious reforms; most others would have to undertake wide-ranging policy changes that go beyond narrowly defined trade areas, including competition policy, price controls, investment policy, privatization plans, and transparency requirement.

Our empirical results show that WTO/GATT accessions are often associated with significant increases in growth and investment that last for about five years, but the effects work only for those countries that have to undertake substantial reforms (i.e., not eligible for Article XXVI 5(c). While the pickup in the growth rates is only temporary (five years after accession), the economy is permanently larger (by 20%) as a result. We also find that the beneficial effects of policy commitments seem more pronounced among countries with poorer governance. This suggests that binding policy changes enforced by a credible third party (WTO) serve as a (partial) substitute for good governance in promoting economic development. By utilizing the gap between the dates of application and actual accession as well as implementing a statistical procedure to correct for a selection bias, we conclude that the WTO/GATT benefits are unlikely to be caused by an endogenous selection bias.

In contrast to Barro and Lee (2005), who find no pro-growth effect of IMF supported programs, we have identified beneficial effects of reforms induced by WTO accessions. One conjecture is that policy commitments under WTO accessions are longer-lasting. Future research could examine this formally in order to understand why external commitment works in some context but not in others.

For lack of good measures of individual reforms, this paper focuses on the overall effects of the package of policy changes, instead of attempting to isolate individual reforms that seem most important. Also, due to time-series limitation on the data, our analyses can only focus on a timeframe around the accessions in recent years. We are not able to distinguish a level effect from a growth effect. It would be interesting for future research to measure the longer-term effects of policy commitments on economic development for a larger sample of countries.

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Table 1. List of Countries in the Samples and their Accession Years

2000	Albania	1965	Gambia, The	1997	Panama
#1994	Angola	2000	Georgia	#1994	Papua New Guinea
1987	Antigua and Barbuda	1957	Ghana	1994	Paraguay
1967	Argentina	#1994	Grenada	1951	Peru
@	Azerbaijan, Rep. of	1991	Guatemala	1979	Philippines
@	Bahamas, The	#1994	Guinea	1967	Poland
#1993	Bahrain, Kingdom of	#1994	Guinea-Bissau	1971	Romania
1972	Bangladesh	1966	Guyana	@	Russia
1967	Barbados	1950	Haiti	1966	Rwanda
@	Belarus	1994	Honduras	@	Samoa
1983	Belize	1973	Hungary	@	São Tomé & Príncipe
@	Bhutan	1948	India	1963	Senegal
1990	Bolivia	1962	Israel	@	Seychelles
1987	Botswana	1963	Jamaica	1961	Sierra Leone
1948	Brazil	2000	Jordan	1973	Singapore
1996	Bulgaria	@	Kazakhstan	1993	Slovak Republic
1965	Burundi	1964	Kenya	1994	Slovenia
@	Cambodia	@	Kiribati	#1994	Solomon Islands
1963	Cameroon	1967	Korea	1948	South Africa
@	Cape Verde	1998	Kyrgyz Republic	1948	Sri Lanka
1963	Central African Rep.	@	Lao People's Dem.Rep	#1994	St. Kitts and Nevis
1963	Chad	1999	Latvia	#1993	St. Lucia
1949	Chile	1988	Lesotho	#1993	St. Vincent & Grens.
2001	China,P.R.: Mainland	@	Liberia	@	Sudan
1986	China,P.R.:Hong Kong	2001	Lithuania	1978	Suriname
1981	Colombia	@	Macedonia, FYR	#1993	Swaziland
1971	Congo, Dem. Rep. of	1963	Madagascar	@	Syrian Arab Republic
1963	Congo, Republic of	1964	Malawi	@	Tajikistan
1990	Costa Rica	1957	Malaysia	1961	Tanzania
1963	Côte d'Ivoire	1983	Maldives	1982	Thailand
2000	Croatia	#1993	Mali	1964	Togo
1948	Cuba	1964	Malta	@	Tonga
1963	Cyprus	1963	Mauritania	1962	Trinidad and Tobago
1993	Czech Republic	1970	Mauritius	1990	Tunisia
#1994	Djibouti	1986	Mexico	1951	Turkey
#1993	Dominica	2001	Moldova	@	Turkmenistan
1950	Dominican Republic	1997	Mongolia	1962	Uganda
1996	Ecuador	1987	Morocco	@	Ukraine
1970	Egypt	#1992	Mozambique	1953	Uruguay
1991	El Salvador	#1992	Namibia	@	Uzbekistan
@	Equatorial Guinea	@	Nepal	@	Vanuatu
1999	Estonia	1950	Nicaragua	@	Vietnam
@	Ethiopia	1963	Niger	@	Yemen Arab Rep.
#1993	Fiji	2000	Oman	1982	Zambia
1963	Gabon	1948	Pakistan	1948	Zimbabwe

Note: # Denotes countries acceding to the GATT by Article XXVI 5(c) between 1990 and 1994
@ Denotes countries which never joined GATT/WTO before 2001

Table 2. Summary Statistics of Accession Countries

	I All Accession Countries 1990-2001	II Article XXVI 5(c) Countries	III Non-Article XXVI 5(c) Countries	*(T-stat. of Difference b/w II and III)*	IV Accession Countries 1995-2001
Growth					
Pre-accession: avg over 8 yrs a/	-0.2%	1.5%	-0.7%	*-1.84*	-0.4%
Post-accession: avg over 3 yrs	2.5%	1.3%	3.4%	*2.46**	4.1%
Average change in growth	2.7%	-0.2%	4.1%	*3.08**	4.4%
# Countries in sample	42	17	25		15
Private investment/GDP					
Pre-accession: avg over 8 yrs a/	14.4%	14.5%	14.4%	*-0.06*	13.7%
Post-accession: avg over 3 yrs	15.1%	14.0%	16.0%	*0.87*	15.9%
Average %change in ratio	14.5%	6.7%	20.0%	*0.83*	22.0%
# Countries in sample	38	16	22		13
Total investment/GDP					
Pre-accession: avg over 8 yrs a/	21.5%	23.7%	20.0%	*-1.66*	19.5%
Post-accession: avg over 3 yrs	22.0%	22.5%	21.6%	*-0.38*	21.8%
Average %change in ratio	6.0%	-3.7%	12.1%	*2.03**	14.1%
# Countries in sample	42	17	25		15
Total trade/GDP					
Pre-accession: avg over 8 yrs a/	94.7%	108.9%	85.0%	*-1.77*	86.0%
Post-accession: avg over 3 yrs	98.2%	110.2%	90.0%	*-1.83*	95.7%
Average %change in ratio	11.1%	8.6%	12.9%	*0.42*	17.0%
# Countries in sample	42	17	25		15
				** 5% significance*	

Note: a/ For countries whose data are not available for earlier years, the average is over a smaller number of years before accession

Table 3A. Changes in Growth and Investment around Accessions: Post-Uruguay Round Accessions

		1		2		3		4	
		Annual Growth Rate		Log(Pri Inv/GDP)		Annual Growth Rate		Log(Pri Inv/GDP)	
		Coef est.	*t-stat.*	*Coef est.*	*t-stat.*	*Coef est.*	*t-stat.*	*Coef est.*	*t-stat.*
Lagged log(GDP per capita)		-0.111	-4.58			-0.082	-4.70		
s =	-2	0.009	0.90	0.046	0.66	0.011	0.91	0.031	0.33
	-1	0.025	2.51	0.044	0.69	0.022	2.53	0.034	0.56
	0	0.019	2.59	0.026	0.39	0.024	2.66	0.012	0.16
	1	0.025	3.66	0.051	0.81	0.025	3.79	0.031	0.48
	2	0.031	4.79	0.075	1.12	0.033	4.55	0.057	0.91
	3	0.016	1.96	0.107	1.40	0.014	1.64	0.096	1.21
	4	-0.001	-0.14	0.058	0.75	-0.005	-0.34	0.044	0.55
	5	0.014	1.85	-0.011	-0.18	0.012	1.62	-0.018	-0.25
	beyond	-0.002	-0.35	-0.077	-1.11	-0.005	-0.86	-0.076	-1.10
Country fixed effects		Y		Y		Y		Y	
Year fixed effects		Y		Y		Y		Y	
# Observations		2375		1930		2552		2101	
Adjusted R-sq.		0.19		0.54		0.19		0.54	

"Treatment" group: Countries acceding between 1990 and 2001
"Control" group: All developing countries
Beginning period: 8 years before accessions
t-statistcs are based on robust standard errors clustered by country

Note: Regressions 1 and 2 exclude 10 outliers from the control group;
regressions 3 and 4 do not exclude any outliers from the control group.

Table 3B. Changes in Growth and Investment around Accessions: Pre-Uruguay Round Accessions

		1		2	
		Annual Growth Rate		Log(Pri Inv/GDP)	
		Coef est.	*t-stat.*	*Coef est.*	*t-stat.*
Lagged log(GDP per capita)		-0.031	-5.42		
s =	-2	0.022	1.55	0.048	0.46
	-1	0.016	1.61	-0.045	-0.41
	0	0.013	1.18	-0.163	-1.35
	1	0.026	1.57	-0.034	-0.33
	2	0.023	1.59	0.045	0.47
	3	0.010	1.23	0.103	1.08
	4	0.002	0.24	0.151	1.57
	5	0.007	0.72	0.079	0.81
	beyond	0.008	1.10	0.016	0.31
Country fixed effects		Y		Y	
Year fixed effects		Y		Y	
# Observations		3665		2385	
Adjusted R-sq.		0.12		0.56	

"Treatment" group: Countries acceding between 1955 and 1989 (44 countries acceded during the period);
"Control" group: All other developing countries
Beginning period: 8 years before accessions
t-statistcs are based on robust standard errors clustered by country

Note: Regressions exclude 10 outliers from the control group

Table 4. Article XXVI 5(c) and Non-Article XXVI 5(c) Countries with their Accession Years

Article XXVI 5(c) Countries		**Non-Article XXVI 5(c) Countries**				
	Accession Date		*Application Date*	*Working Party Report Date*	*Accession Date*	*Interval b/w Application and Accession (months)*
Angola	1994 Apr	Albania	1992 Nov	2000 Jul	2000 Sep	94
Bahrain	1993 Dec	Bolivia	1987 Oct	1989 Jul	1990 Aug	34
Djibouti	1994 Dec	Bulgaria	1990 Feb	1996 Sep	1996 Dec	82
Dominica	1993 Apr	Czech Republic*	1992 Dec	1993 Mar	1993 Apr	4
Fiji	1993 Nov	China	1987 Mar	2001 Oct	2001 Dec	177
Grenada	1994 Feb	Costa Rica	1987 Jun	1989 Oct	1990 Oct	40
Guinea	1994 Dec	Croatia	1993 Sep	2000 Jun	2000 Nov	86
Guinea-Bissau	1994 Mar	Ecuador	1992 Sep	1995 Jul	1996 Jan	40
Mali	1993 Jan	El Salvador	1988 Dec	1990 Nov	1991 Jan	13
Mozambique	1992 Aug	Estonia	1994 Mar	1999 Apr	1999 Nov	68
Namibia	1992 Sep	Georgia	1996 Jun	1999 Aug	2000 Jun	48
Papua New Guinea	1994 Dec	Guatemala	1990 Apr	1990 Nov	1991 Apr	12
Solomon Islands	1994 Dec	Honduras	1990 Oct	1993 Oct	1994 Apr	42
St. Kitts	1994 Mar	Jordan	1994 Jan	1999 Dec	2000 Apr	75
St. Lucia	1993 Apr	Kyrgyz	1996 Feb	1998 Jul	1998 Dec	34
St. Vincent	1993 May	Latvia	1993 Nov	1998 Sep	1999 Feb	63
Swaziland	1993 Feb	Lithuania	1994 Jan	2000 Nov	2001 May	88
United Arab Emirates#	1994 Mar	Moldova	1993 Nov	2001 Jan	2001 July	92
		Mongolia	1991 Oct	1996 Jun	1997 Jan	63
		Oman	1996 Apr	2000 Sep	2000 Nov	55
		Panama	1991 Oct	1996 Sep	1997 Sep	71
		Paraguay	1989 Mar	1993 Apr	1994 Jan	58
		Slovak Republic*	1992 Dec	1993 Mar	1993 Apr	4
		Slovenia	1992 Jul	1994 Jul	1994 Oct	27
		Tunisia	1981 Nov	1987 Dec	1990 Jul	104
		Venezuela#	1989 Jun	1990 Jun	1990 Aug	14

Note: * Czech Republic and Slovak Republic acceded to the GATT following the breakup of Czechoslovakia;
United Arab Emirates and Venezuela are not in our samples (all OPEC countries are excluded).

Table 5. Changes in Growth, Investment and Trade: Article XXVI 5(c) vs. Non-Article XXVI 5(c) Countries

		1		2		3		4	
		Annual Growth Rate		Log(Pri Inv/GDP)		Annual Growth Rate		Log(Pri Inv/GDP)	
		Coef est.	*t-stat.*	*Coef est.*	*t-stat.*	*Coef est.*	*t-stat.*	*Coef est.*	*t-stat.*
Lagged log(GDP per capita)		-0.104	-4.31			-0.088	-4.44		
s =	-2	0.007	0.54	0.011	0.17	0.008	0.59	0.009	0.14
	-1	0.022	3.14	0.062	1.02	0.026	3.23	0.071	1.12
	0	0.024	2.81	0.085	1.45	0.029	2.72	0.090	1.51
	1	0.039	4.54	0.094	1.46	0.049	4.95	0.091	1.41
	2	0.042	5.31	0.132	1.98	0.051	5.42	0.131	1.92
	3	0.033	3.35	0.184	2.35	0.035	3.31	0.179	2.26
	4	0.026	3.13	0.161	2.49	0.028	3.13	0.160	2.41
	5	0.022	2.49	0.102	1.50	0.020	2.23	0.103	1.46
	beyond	0.017	3.06	0.152	2.70	0.017	2.92	0.161	2.83
s*AXXVI5c Dummy:	-2 * AXXVI5c	0.006	0.31	0.083	0.62	0.007	0.37	0.053	0.44
	-1 * AXXVI5c	-0.008	-0.32	-0.035	-0.26	-0.007	-0.32	-0.070	-0.54
	-0 * AXXVI5c	-0.010	-0.81	-0.157	-1.11	-0.015	-0.87	-0.192	-1.39
	1 * AXXVI5c	-0.045	-3.31	-0.113	-0.93	-0.046	-3.39	-0.132	-1.08
	2 * AXXVI5c	-0.034	-3.11	-0.151	-1.14	-0.036	-3.07	-0.175	-1.41
	3 * AXXVI5c	-0.047	-3.57	-0.158	-1.21	-0.053	-3.78	-0.188	-1.41
	4 * AXXVI5c	-0.059	-3.24	-0.204	-1.52	-0.066	-3.35	-0.220	-1.66
	5 * AXXVI5c	-0.022	-1.72	-0.228	-1.81	-0.021	-1.70	-0.231	-1.83
	beyond * AXXVI5c	-0.044	-4.62	-0.419	-3.51	-0.040	-4.49	-0.417	-3.40
Country fixed effects		Y		Y		Y		Y	
Year fixed effects		Y		Y		Y		Y	
# Observations		2375		1930		2552		2101	
Ajusted R-sq.		0.21		0.54		0.20		0.54	

"Treatment" group: Countries acceding between 1990 and 2001
"Control" group: All developing countries
Beginning period: 8 years before accessions
t-statistcs are based on robust standard errors clustered by country

Note: Regressions 3 and 4 do not exclude 10 outliers from the control group.

Table 7. Changes in Growth and Investment for Non-AXXVI 5(c) Countries around Application and Accession

		1		2	
		Annual Growth Rate		Log(Pri inv/GDP)	
		Coef est.	*t-stat.*	*Coef est.*	*t-stat.*
Lagged log(GDP per capita)		-0.100	-3.78		
Year from application	0	0.014	0.95	-0.004	-0.04
	1	0.024	1.45	-0.003	-0.03
	2	0.027	2.57	0.017	0.12
	3	0.025	2.29	0.109	1.40
	4	0.009	0.63	0.131	2.09
	5	0.005	0.53	0.074	1.26
	6	-0.001	-0.05	0.005	0.12
s =	-2	0.014	1.12	-0.030	-0.37
	-1	0.030	3.52	0.006	0.09
	0	0.032	3.07	0.034	0.53
	1	0.051	4.51	0.057	0.78
	2	0.053	5.34	0.122	1.53
	3	0.044	3.50	0.154	1.69
	4	0.043	3.59	0.169	1.99
	5	0.035	2.82	0.108	1.21
	beyond	0.028	2.85	0.168	2.13
Country fixed effects		Y		Y	
Year fixed effects		Y		Y	
# Observations		2009		1578	
Ajusted R-sq.		0.20		0.56	

"Treatment" group: Countries acceding by normal procedures between 1990 and 2001
"Control" group: All developing countries
Beginning period: 4 years before application
t-statistcs are based on robust standard errors clustered by country

Note: For countries that acceded to the WTO/GATT in fewer than 9 years since application,
for some years both application and accession time-profiles would simultaneously have non-zero dummies.

Table 11. Extent of Commitments and Governance Quality of Acceding Countries

	# Commitments in WPRs	Governance Index	# Words in WPRs
Bulgaria	27	2.83	24542
Albania	29	2.84	38829
China	147	2.49	78641
Croatia	27	2.39	38479
Ecuador	21	2.56	25835
Estonia	24	4.76	22920
Georgia	29	1.32	27139
Jordan	29	3.26	36608
Kyrgyz	29	2.15	32149
Latvia	22	3.72	25717
Lithuthania	28	3.23	43029
Moldova	28	2.87	43859
Mongolia	17	2.91	12055
Oman	26	4.73	24695
Panama	24	3.91	19558
Mean			
Incl. China	33.8	3.06	32937
Excl. China	25.7		29672
Median	27	2.87	27139
Standard dev.			
Incl. China	31.5	0.92	15537
Excl. China	3.7		9370

Table 13. Differential Effects of Policy Commitments

		1 a/		2 a/		3		4	
		Annual Growth Rate		Log(Inv/GDP)		Annual Growth Rate		Log(Inv/GDP)	
		Coef est.	*t-stat.*	*Coef est.*	*t-stat.*	*Coef est.*	*t-stat.*	*Coef est.*	*t-stat.*
Lagged log(GDP per capita)		-0.137	-8.51			-0.142	-8.63		
s*log(# Com.)*Gov Quality:	-2 * log(#Com.) * gov.	-1.552	-2.09	-4.920	-2.65				
	-1 * log(#Com.) * gov.	-1.340	-2.85	-7.972	-5.60				
	-0 * log(#Com.) * gov.	-0.971	-2.07	-6.821	-3.86				
	1 * log(#Com.) * gov.	-1.504	-3.47	-8.375	-4.25				
	2 * log(#Com.) * gov.	-1.479	-3.22	-8.440	-4.58				
	3 * log(#Com.) * gov.	-2.674	-5.95	-9.659	-5.27				
	4 * log(#Com.) * gov.	-1.488	-1.90	-9.357	-3.99				
	5 * log(#Com.) * gov.	-0.861	-1.50	-1.962	-0.88				
	beyond * log(#Com.) * gov.	-3.871	-4.09	16.750	3.82				
s =	-2					0.049	2.16	0.211	1.49
	-1					0.046	3.00	0.220	1.69
	0					0.054	3.04	0.298	1.98
	1					0.085	4.07	0.385	2.50
	2					0.085	3.97	0.455	3.20
	3					0.111	2.99	0.655	3.75
	4					0.032	1.50	0.469	3.92
	beyond					0.082	3.48	0.470	3.65
s*Good Gov:	-2 * Good Gov.					-0.026	-0.80	-0.034	-0.19
	-1 * Good Gov.					-0.041	-1.63	-0.091	-0.58
	-0 * Good Gov.					-0.030	-1.22	-0.210	-1.22
	1 * Good Gov.					-0.028	-0.89	-0.251	-1.34
	2 * Good Gov.					-0.033	-1.06	-0.280	-1.58
	3 * Good Gov.					-0.054	-1.20	-0.402	-2.01
	4 * Good Gov.					0.028	0.59	-0.233	-1.45
	beyond * Good Gov.					-0.074	-2.65	-0.287	-1.53
s*(Poor Gov, Few Com.):	-2 * (Poor, Few)					-0.089	-1.61	-0.258	-1.39
	-1 * (Poor, Few)					-0.081	-2.95	-0.241	-1.40
	-0 * (Poor, Few)					-0.082	-2.44	-0.367	-1.98
	1 * (Poor, Few)					-0.113	-3.15	-0.463	-2.24
	2 * (Poor, Few)					-0.080	-2.82	-0.391	-2.08
	3 * (Poor, Few)					-0.131	-2.88	-0.521	-2.56
	4 * (Poor, Few)					-0.035	-1.16	-0.400	-2.70
	beyond * (Poor, Few)					-0.065	-2.51	-0.388	-2.54
Country fixed effects		Y		Y		Y		Y	
Year fixed effects		Y		Y		Y		Y	
# Observations		1769		1655		1780		1572	
Adjusted R-sq.		0.30		0.48		0.29		0.49	

"Treatment" group: Countries acceding between 1995 and 2001, China excluded from regressions 1 and 2
"Control" group: All developing countries
Beginning period: 8 years before accessions
t-statistcs are based on robust standard errors clustered by country

Note a/: Other regressors include (but not reported) in regressions 1 and 2: s's, s*Governance Quality, and s*log(#Commitments)

Figure 1. Change in Growth around Accession

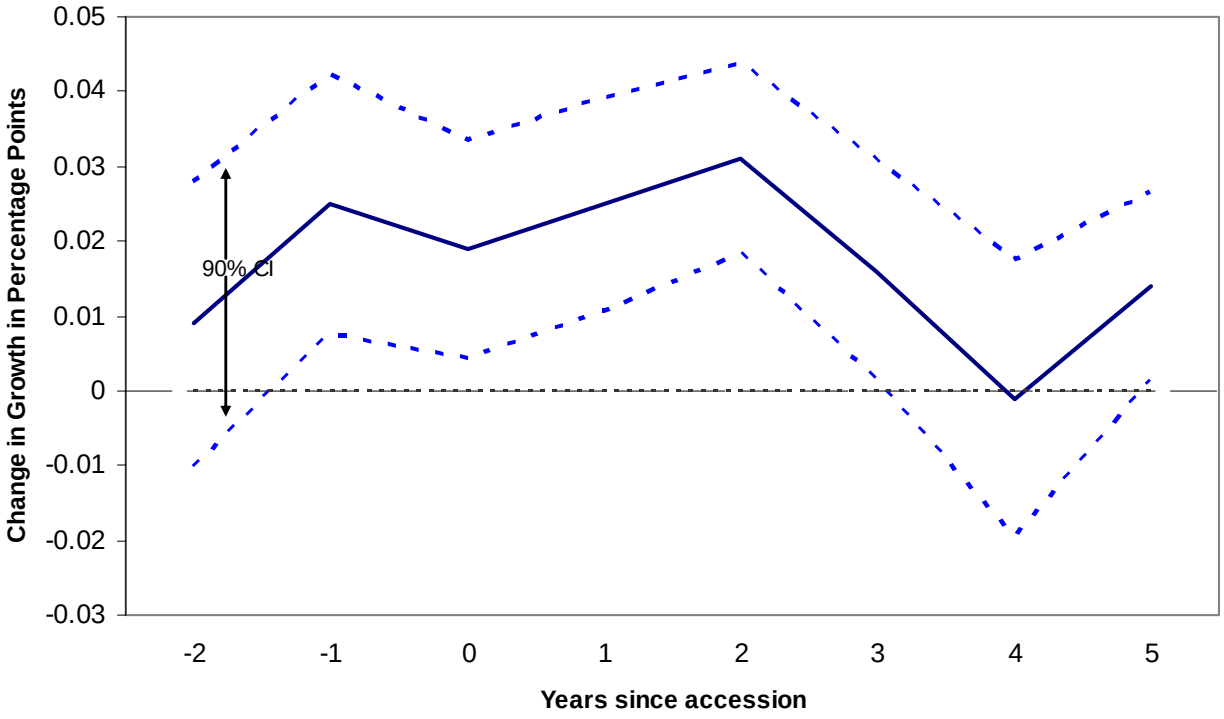

Figure 2. Change Private Investment-to-GDP Ratio around Accession

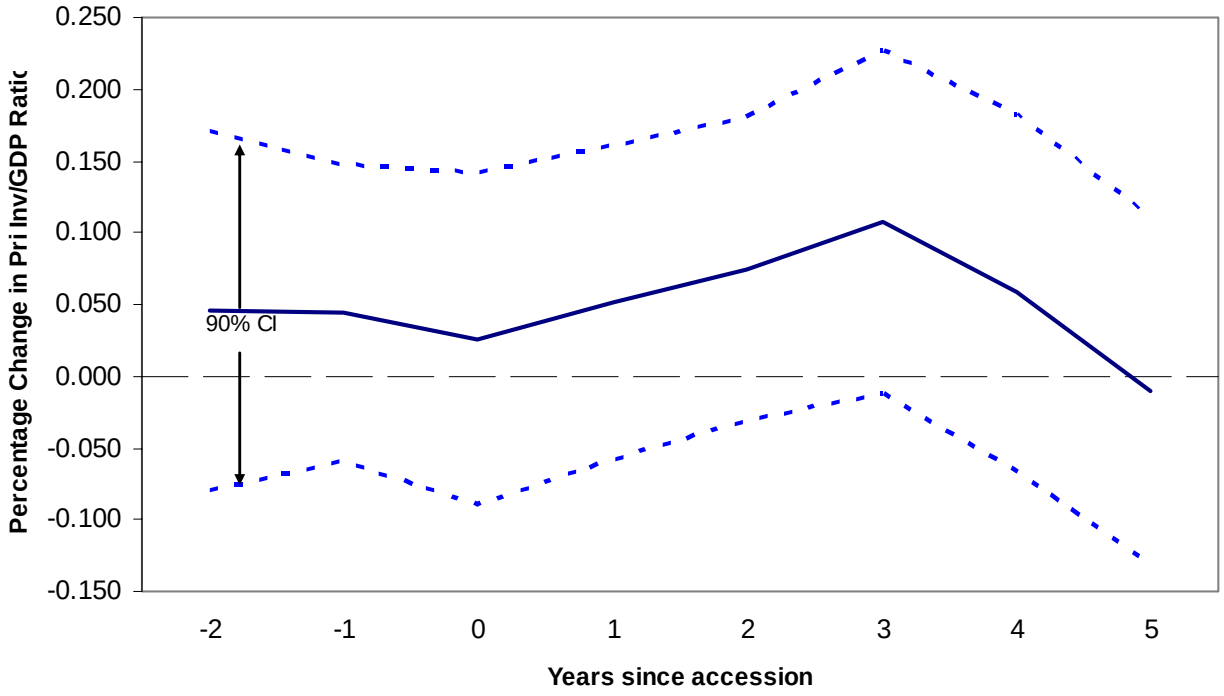

Figure 3. Change in Growth: Article XXVI5c vs. Other Countries

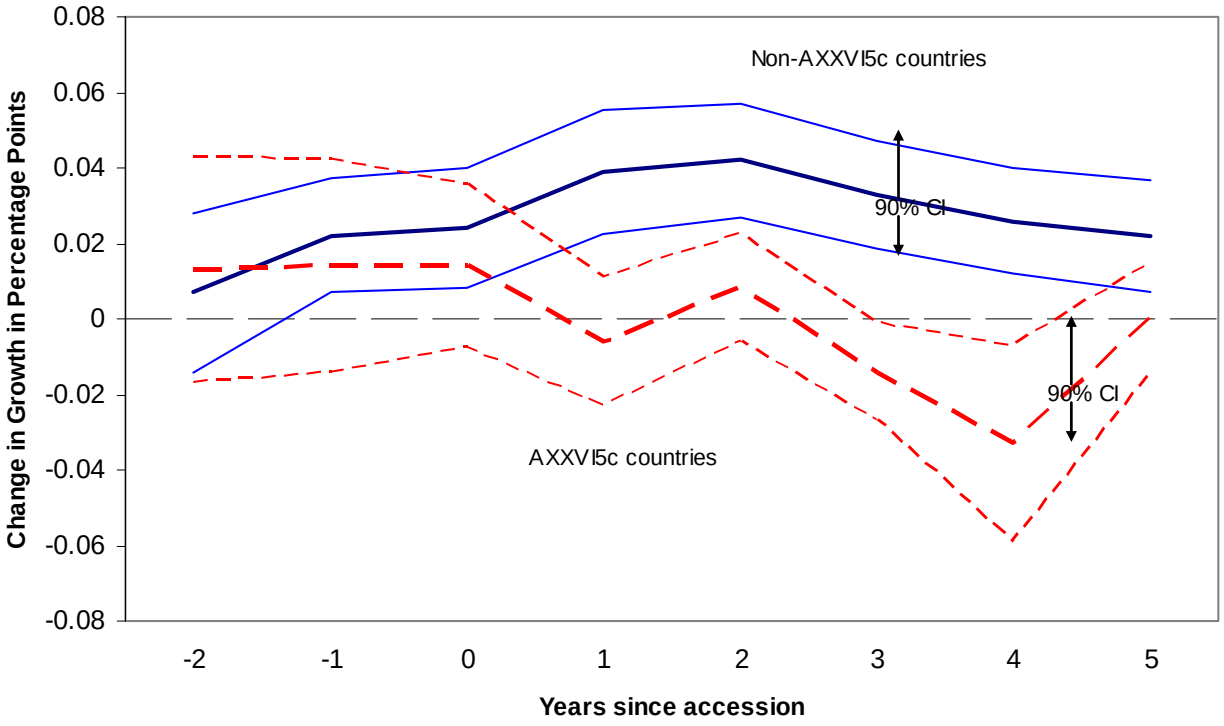

Figure 4. Change in Pri Investment-to-GDP Ratio: Article XXVI5c vs. Other Countries

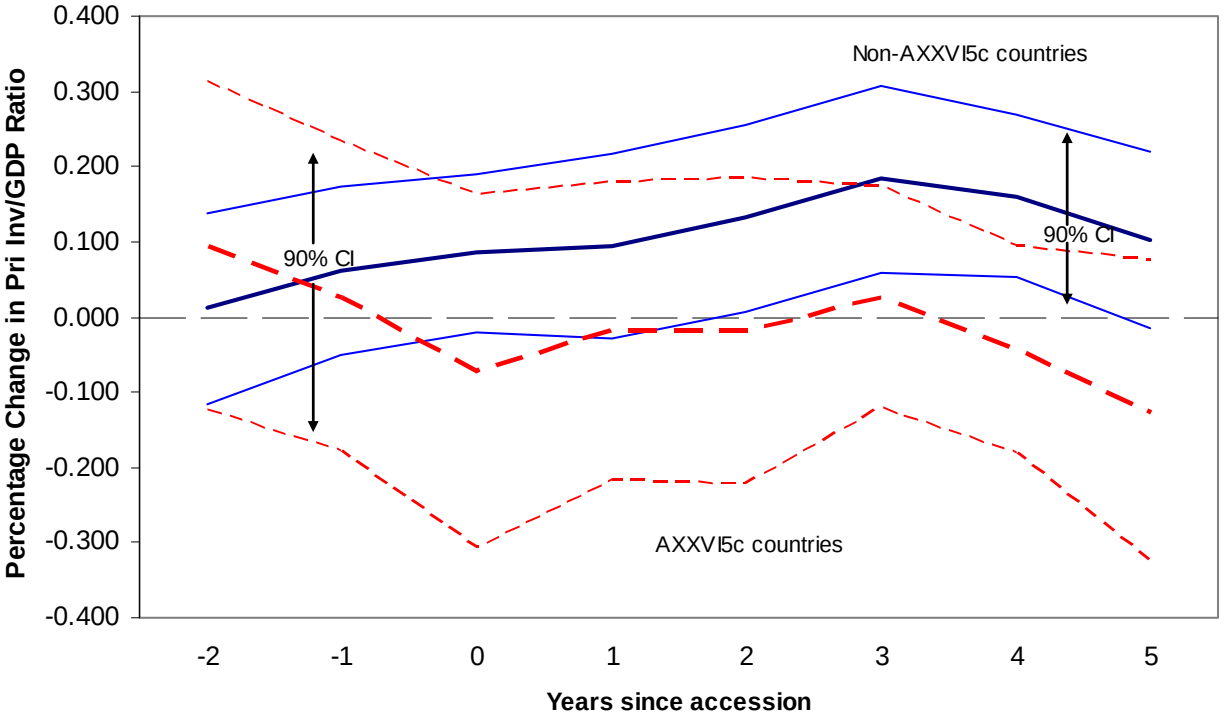

Figure 5. Change in Growth Following Application and around Accession

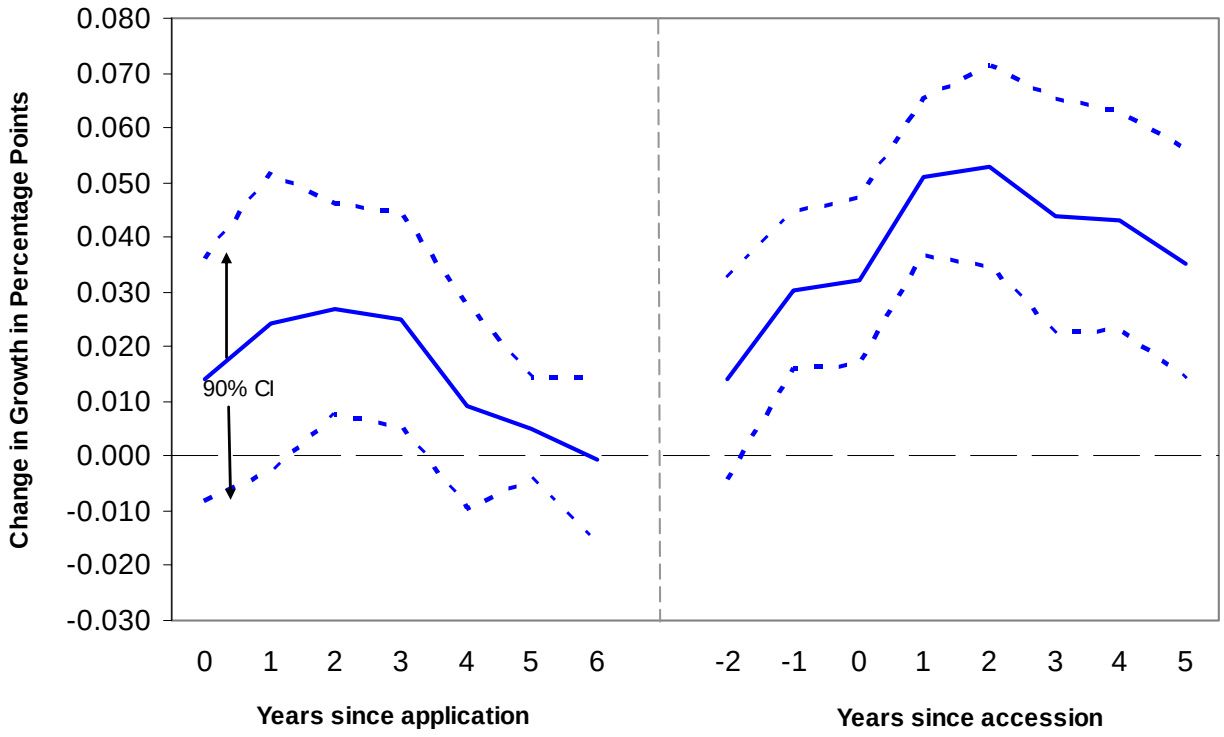

Figure 6. Change in Pri Inv/GDP Following Application and around Accession