ABSTRACT

Title of Dissertation: ESSAYS ON REGIONAL INTEGRATION AND DEVELOPMENT ECONOMICS

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The first chapter of this dissertation studies the growth effects of regional integration agreements. During the 1990's the world experienced a new wave of regional integration agreements (RIAs) that reached unprecedented proportions. In the presence of economies of scale or extent-of-the market effects RIAs may have positive growth effects. I introduce a new measure of regional integration by interacting country membership to an RIA and the partners' share of world GDP, which allows capturing differentiated effects depending on the size of the partners. Results indicate that RIAs have exerted positive effects on growth. In addition, I find that North-North agreements have significant growth effects; South-South agreements have ambiguous effects

depending on the size of the countries joining them, and that there is no clear answer for North-South agreements.

The second chapter studies the impacts of Social Infrastructure Investments in Education focusing in the case of Nicaragua during the 1990's. This chapter assesses the impact of investments in primary school infrastructure carried by Nicaragua's Social Investment Fund (FISE) during the 1993-1997 period on several education outcomes in the primary school population, such as: enrollment, grade repetition, absenteeism, age at which students enroll in first grade, and education gap. I propose to use two different approaches that assume selection on observables to deal with possible selection bias in order to estimate the impact of FISE investments: a regression based estimation and a propensity score matching technique. With these two approaches I do not find any effects of FISE's investments on the selected educational outcomes. One possible explanation of the lack of significance might be that FISE investments actually had no impact on the outcomes because infrastructure is not a relevant determinant of those used here. If this is the case, then investing in setting up and maintaining Social Funds cannot be justified by appealing to the alleged positive impact of their investments on education. Since the impacts are simply not present in the Nicaraguan case, the reasons to keep Social Funds must be revisited.

ESSAYS ON REGIONAL INTEGRATION

AND DEVELOPMENT ECONOMICS

by

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For Diana and Bruno

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TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

CHAPTER 1. INTRODUCTION

The present dissertation is composed of two separate and different empirical essays. The first one investigates the impact of regional integration that takes the form of trade agreements on economic growth. The second evaluates the impact of a program of public investment in infrastructure in primary schools in Nicaragua on a selected group of educational outcomes.

During the 1990's the world experienced a new wave of regional integration agreements (RIAs) that reached unprecedented proportions. Even though there is a large literature measuring the effect of RIAs on trade, research on the growth effects of RIAs is not as extensive. On the one hand the theoretical literature provides no clear answers to what should be the expected impact on growth. On the other the empirical literature suffers from important drawbacks.

Previous empirical studies have measured growth effects by introducing dummy variables that account for countries membership in specific agreements. One problem with this approach is that it gives the same effect across countries—regardless of the characteristics of the countries joining the agreement—although we might expect that particular characteristics of the partners that each country is facing in the agreement, such as the size and the purchasing power of the market, would drive the effects on growth.

The first essay introduces a new measure of regional integration by interacting country membership in an RIA with the partners' share of world GDP, which captures a differentiated effect of integration depending on the extent of the market that is incorporated when joining an agreement. I also use a measure of the relative size of the market—the partner's share of world GDP over each member's share of world GDP.

These two variables measure the extent of each country's own integration agreements and they can suggest whether growth effects through economies of scale are present.[1]

Furthermore, I study the impact of exclusion from regional integration agreements by constructing a variable that measures the extent of the market incorporated in other countries' integration agreements. With this variable I capture the size of common markets being created by other countries.

Another merits of this essay is that it incorporates the largest span of time and the largest set of RIAs so far in the literature. I use data over a period of forty years starting in 1960 and ending in 1999, and I include seventy trade agreements for both cross-section and panel data regression analysis.

The results indicate that RIAs have exerted positive effects on growth, mainly through the absolute size of the market that a trade agreement incorporates into the domestic market. Thus, the larger the partners (economically) the higher the impact on growth for the joining countries. This result indicates that economies of scale are a driving force of growth. In addition, I estimate the effects for different type of agreements: those including only developed countries (North-North), those including both developed and developing members (North-South), and those between developing countries (South-South). I find that North-North agreements have significant growth effects; South-South agreements have ambiguous effect depending on the size of the countries joining them, and that there is no clear answer for North-South agreements.

[1] Throughout the dissertation I call the former measure of the extent of the market the "absolute size of the market", and the latter the "relative size of the market". Although the first variable is not measured in terms on actual GDP but in terms of shares of world GDP I use the name to differentiate this variable from the one measuring the relative size of the market.

In terms of the impact of other countries' integration agreements, I find that no evidence that being excluded from other countries' regional agreements affects growth in the excluded countries. Finally, in addition to direct growth effects of RIAs, I find that regional integration positively affects growth by increasing the rate of investment and the proportion of manufactures in total exports.

In the second essay, I study the impact of Nicaragua's Social Investment Fund (FISE) investments in primary schools on various educational outcomes. FISE is the most important channel through which social investment has been created in Nicaragua since 1990, and it has been particularly important in contributing to the reconstruction and improvement of primary school infrastructure.

As the most important provider of public infrastructure FISE has been subject to impact evaluations to assess the effectiveness of its investments. My second essay reviews the existing evaluations, and proposes and carries out an alternative method to produce unbiased estimates of its impacts.

The period under consideration is 1993-1997, and the outcomes studied are primary school enrollment, education gap, children in correct grade for age, absenteeism, age in first grade, grade repetition, and highest grade completed.

Because FISE investments are not randomly assigned to schools, it is necessary to use a methodology that accounts for the possibility of selection bias. An existing evaluation of the impact of FISE investments by the World Bank[2] does not account for this possibility, as it only compares the means of each outcome between a group of children in schools that received FISE funds with a selected comparison group. They proceed in this way because they assume that the comparison group was selected in such

[2] See Pradhan and Rawlings (2002) and World Bank (2000).

a way so that it completely eliminated the selection bias problem. With this procedure they find important positive effects of FISE investments on some of the outcomes mentioned above.

In this essay I first find that the existing comparison group does not completely eliminate the sample selection bias, therefore rendering the previous estimates inconclusive. Next, I propose the use of two different approaches that assume selection on observables to deal with the selection bias in order to estimate the impacts of FISE: a regression based estimation and a propensity score matching technique.

Applying these two methods, I find that the point estimates of both the regressions approach and propensity score matching point towards no statistically significant effects of FISE investments in primary schools on educational outcomes. One possible explanation of the lack of significance might be the result of insufficient observations, as the sample size might be too small to capture the expected effects. In fact power calculations indicate that for the observed impacts it would be necessary to have a sample size at least three times the available sample. Another explanation is that FISE investments actually had no impact on the outcomes because infrastructure is not a relevant determinant of those used here. If this is the case, then investing in setting up and maintaining Social Funds cannot be justified by appealing to the alleged positive impact of their investments on education. Since the impacts are simply not present in the Nicaraguan case, the reasons to keep Social Funds must be revisited.

The rest of the dissertation is distributed as follows: chapter 2 presents the study on the growth effects of regional integration, and chapter 3 presents the essay concerning

the impact of investments in primary school infrastructure on educational outcomes in Nicaragua.

CHAPTER 2. GROWTH EFFECTS OF REGIONAL INTEGRATION

2.1. Introduction

The increasing importance of regional integration agreements (RIAs) and in particular their extraordinary expansion during the 1990's are among the most salient developments of the international trading system. The evolution of the European Community into the European Union with a deeper level of integration, the creation of NAFTA and MERCOSUR, the revival of old arrangements such as the Andean Pact and ASEAN, and a new wave of bilateral agreements involving Latin American countries are examples of this phenomenon.

A first assessment of the extent of the expansion of RIAs can be obtained by examining Table 1, which shows the number of notifications to GATT/WTO of regional integration agreements. An important first wave of agreements was observed in the 1970's, and a second larger one—the revival of regional integration—during the 1990's. This last wave of agreements accounts for 52% of the notified RIAs as of 1998.[3] At present most countries in the world are members of at least one RIA, and at least one third of world trade is covered by RIA provisions (World Bank, 2000).

These developments have attracted academic attention, generating a large literature that seeks to explain the welfare properties of RIAs and their effects on trade.[4] Relatively less attention, at both the theoretical and empirical levels, has been devoted to the growth effects of RIAs, even though it has been recognized that the dynamics effects of regional integration are potentially larger than the static ones (Walz, 1999).

[3] A word of caution regarding these numbers is necessary. Not all agreements will result in a net increase of the number of agreements in force since some of them will replace existing agreements.
[4] See De Melo and Panagariya (1993), Kym and Blackhurst (1993), Baldwin and Venables (1995), and Baldwin (1997), among others, for discussions on these and related issues.

The way in which the empirical literature has tried to assess whether regional integration affects economic growth has been through the use of dummy variables for a country's participation in an RIA. They have been used in both cross section and panel data growth regressions, with findings that mostly point towards a lack of growth effects of regional integration. An important disadvantage of this approach is that a categorical variable will not capture countries' features that may have growth effects once an integration agreement is in place. In other words a dummy variable measures the impact or regional integration with error, which in turn may explain why empirical studies have generally found that regional integration has not affected growth significantly either positive or negatively.

This paper contributes to the literature along several lines: first and mainly, I introduce a new way to measure regional integration by creating an RIA variable that not only considers whether a group of countries has an RIA, but also captures the extent of the world market that is integrated into domestic markets once the agreement takes place. This is a clear improvement to the existing literature, for it uses additional and relevant information about the countries joining the RIA—at the same time that reduces its measurement error—in order to identify possible growth effects coming from the expansion of the market size.

In addition, I improve the literature by addressing endogeneity problems that might arise with regional integration, as it might be the case that growth and the decision to join certain RIAs are driven by common underlying factors that would bias current estimates from their true values. This problem has not been addressed before, and I deal with it by proposing a set of instruments comprised of regional dummies.

By analyzing the growth effects of size of the market, I find strong evidence that extending the domestic market through RIAs fosters growth, irrespective of the estimation method. In turn, this finding supports theoretical arguments that suggest positive effects of RIAs through the extent of the market. My estimates indicate that signing an agreement with a country whose GDP is 1 percent of world GDP increases growth by 0.026 to 0.1 percentage points, with the lower bound being the most appropriate one.[5] To put these results in context, an agreement with a country the size of Canada—whose share of world GDP in 2000 was approximately 2.1%—might increase growth by 0.05 to 0.021 percentage points.

With these point estimates we can, for instance, estimate the impact of the expected enlargement of the European Union, which would increase the growth rate of existing members by 0.02 percentage points. For incoming members such as Poland and Hungary, the increase in their growth rates might reach up to 0.6 percentage points. Considering that the worldwide annual average GDP growth rate during the 1990-2000 period was 2.6 percent,[6] the gains from regional integration agreements are not trivial.

A second contribution of this paper is the finding that when countries are classified as North or South—depending on their development level—those agreements between North countries have unambiguous positive growth effects. In turn, the growth effect of an agreement between South or developing countries depends on the size of its partners. For sufficiently large countries the effect is positive but for very small countries the effect is negative. For North-South agreements the evidence is mixed and does not allow me to conclude either in favor or against growth effects of regional integration.

[5] The magnitude of the growth effect varies depending on the estimation procedure.
[6] The World Bank. World Development Report 2002.

Third, I test the effect of others countries' integration on a country's own growth, with a variable that captures the size of the other economies that are integrating in the world, finding that exclusion has not reduced growth in left out countries, which provides some evidence that regional integration has not hindered growth on a global scale.

Lastly, I explore the possibility that growth effects might not come directly from RIAs to growth, but indirectly through its effects on other determinants of growth. In order to test this, I assessed the effects of regional integration on two different sources of growth, namely, investment and manufactured exports. I find evidence that regional integration tends to increase the investment rate of countries joining agreements and the proportion of manufactured exports of participant countries.

The rest of the chapter is organized as follows: section 2.2 reviews the theoretical and empirical literature on regional integration and growth. Section 2.3 discusses the measures of regional integration that I constructed, the data, and estimation strategies. Section 2.4 presents the results of introducing the new measure of regional integration, addresses some endogeneity concerns regarding regional integration, discusses the implications for different types of agreements, studies the effect of other countries' integration, and discusses additional channels for growth effects. Section 2.5 concludes.

2.2. Growth and Regional Integration

Relatively little attention has been devoted to study the growth effects of regional integration. Notable exceptions are the contributions of Walz (1995, 1997a, 1997b, 1999). Using dynamic general equilibrium growth models that account for regional aspects, he finds that the growth rate after integration depends on several factors, such as

whether the integration agreement causes trade creation or trade diversion, the comparative advantage of each member, and initial trade barriers. Given this simultaneous dependence of the growth rate on several factors, the endogenous growth literature that analyzes regional integration does not have a clear answer to the question of what are its effects on growth.

However, it has been long thought that the potential dynamic effects of regional integration might be important (Baldwin, 1989). One of the possible sources of dynamic effects are economies of scale. Walz (1997) points out that assuming increasing returns to scale in the R&D sector, scale effects would come from increases in productivity of this sector due to the expansion in the size of the market after integration. In this line, openness has been found to reduce the effect of the size of the domestic market on growth (Ades and Glaeser 1999), yet, no attempt has been made to test the impact on growth of the enlargement of foreign markets through regional integration agreements.

In a related literature, although not concerned with growth but with income convergence/divergence, Puga and Venables (1998) have reached conclusions regarding the effects of different types of agreements on income and welfare. Specifically, they investigate the effects of trade arrangements between developed and developing countries, i.e., North-South and South-South agreements, on industrial development. They conclude that unilateral liberalization is beneficial, but also that the gains from an integration agreement are likely to be larger. Moreover, they find that North-South agreements are more likely to bring greater gains to developing countries—if not to North countries as well—than South-South agreements.

Following a different approach related to traditional trade theories and using two different models, Venables (1999) shows that comparative advantages, coming from endowments of human capital, can provide similar conclusions to those of Puga and Venables (1998). His argument is simple: countries with comparative advantage closer to the world average have a lower risk of trade diversion—the source of losses of income and welfare—once a free trade area is formed, and countries with the comparative advantage most different from the world average have a higher risk of trade diversion. His conclusions lead to the following implications: on the one hand, a free trade area formed by developing countries—with lower relative endowments of human capital than the world average but different among them—might lead to divergence of income levels, with the richer partner—the one with endowments closer to world average—benefiting at the expense of the poorer. On the other hand, an agreement containing a high income partner—one with relative endowment of human capital above the world average—is more likely to lead to income convergence.

Although it is not possible to find an unambiguous prediction in the theory regarding the growth effects of regional integration, it is possible to extract some guidelines regarding what type of agreements might have positive growth effects: Puga and Venables (1998) and Venables (1999) suggest agreements that include at least one developed partner increase growth. In the present paper I contribute to the literature by developing a variable that tests this hypothesis empirically. The methodology used in this test and its results are reported in sections 3 and 4 respectively.

The empirical literature[7] has addressed regional integration by including dummy variables as the measure of regional integration. Using cross-section growth regressions, De Melo *et al*. (1992) and Vamvakidis (1998) found that for several integration agreements a dummy variable reflecting membership does not have a significant effect on growth. In a related paper, Vamvakidis (1999) addresses the question of whether countries engaging in unilateral liberalizations grow faster than countries that engage in regional integration agreements. Results show that economic growth is greater in countries that accomplish broad liberalization than in countries that engage in RIAs, specifically he finds that becoming a member of at least one RIA does not foster growth.

Recently, Henrekson *et al*. (1997) test the growth effects of European integration, namely the growth effects of both the European Community and the European Free Trade Agreement, finding that the EC/EFTA dummy variable is positive and significant at the 5% level, suggesting that the growth rate is increased in a range of 0.6-0.8 percentage points by EC/EFTA membership.

The use of a dummy variable is prevalent not only at the country level, but also in studies using industry data. Madani (2001a and 2001b) looks at the effect of the revival of the Andean Pact and ASEAN on industrial growth,[8] finding that for both groups the dummy variable used to estimate growth effects is not significant.

Using a dummy variable to capture the growth effects of an RIA is akin to assuming that the expected effect would come simply from signing the agreement. Instead, we should expect that characteristics of the agreement and its partners, such as

[7] I will discuss only studies involving econometric techniques because of the similarities to this paper. There is also a literature using computable general equilibrium methods. For a basic survey on computable general equilibrium evaluations see Baldwin and Venables (1995).

[8] The Andean Pact countries analyzed are Bolivia, Colombia, and Ecuador. The ASEAN countries are Singapore, Philippines, and Malaysia.

the level of integration, economic development or market size, would be forces driving the gains in growth. For instance, if we expect agreements involving countries of different sizes to have differentiated effects depending on the size of the partners joining the agreement, using a dummy variable will not give the meaningful estimates because a dummy variable in this context introduces a large measurement error in the integration variable.[9] Variables that reduce measurement error by incorporating the dimensions that might generate growth effects, such as the size of the market, would generate more consistent and economically meaningful estimates.

In summary, theory does not provide indications about the expected impact of regional integration on growth, although it is possible to indirectly extract some guidelines regarding the type of agreements that might have positive effects: those that include at least one developed partner. The empirical literature, by using a dummy variable to measure the impact of RIA membership, has ignored the fact that impacts are likely to depend on market size.

2.3. Measuring Regional Integration Agreements, Estimation Strategy, and Data

I measure the growth effects of regional integration—presumably through economies of scale—by creating a variable (*RIA*) that captures the extent of the world market that is potentially incorporated into the domestic market with the set of regional integration agreements that a country has joined. I define the variable as follows:

$$\text{Absolute RIA}_{jt} = \sum_{\substack{i=1 \\ j \neq i}}^{N} \left(D_{it}^{j} \times \text{SWGDP}_{i,1960} \right), \qquad (1)$$

[9] Henceforth, I will use 'size' to refer to a country's GDP.

where N is the number of countries in the world, D_{it}^{j} is a dummy variable that takes the value of 1 when country j has an integration agreement with country i in period t, and $SWGDP_i$ is the share of world GDP of country i.[10] The variable, therefore, is the sum of the shares of world GDP of the countries with which country j has signed regional integration agreements. The size of the partners' market that is being incorporated to their domestic market is potentially different for each country signing the agreement. As a result, countries entering an agreement are going to enlarge their domestic markets differently, thus facing different prospects for growth improvements. This measure of integration is a clear improvement in the direction of reducing the measurement problem posed by the use of dummy variables, and it will permit different growth estimates for each of the countries entering an agreement.

The RIA variable presented in equation 1 measures the absolute the size of the market, however, it is possible that economies of scale could appear not only from increasing the market in absolute terms but also in relative terms. In order to capture that effect I constructed a measure of the size of partners' market relative to the size of the country entering the agreement, to test whether the relative size of the incorporated market and not the absolute size is what determines the presence of economies of scale.[11] Thus, the relative RIA variable is constructed as:

[10] I use countries' share of world GDP in 1960 to avoid problems of reverse causation. However, this choice might introduce measurement error into the RIA measure as not all countries grew at the same rate over the period. The results reported here are robust to a measure of RIA that uses the observed shares of world GDP at every point in time instead of fixed ones.

[11] I thank Nuno Limão for a suggestion that led to the construction of this variable.

$$\text{Relative RIA}_{jt} = \frac{\sum_{\substack{i=1 \\ j \neq i}}^{N} \left(D_{it}^{j} \times \text{SWGDP}_{i,1960} \right)}{\text{SWGDP}_{j}}, \tag{2}$$

were the numerator of equation (2) is equation (1), and $SWGDP_j$ is the share of world GDP of country *j*. Therefore, the relative RIA variable conveys information regarding the size of the partners' market relative to the domestic market. These two RIA variables will be contrasted with a variable in line with the existing literature, a dummy variable reflecting whether a country belongs to at least one RIA.

Another contribution of this paper is to test whether different types of agreements, in terms of the level of development of the partners they involve—namely, developed or developing countries—have different growth effects. This will be achieved by classifying countries according to their level of development and recalculating the RIA variables for different types of agreements: North-North, North-South, and South-South.

Appendix 1 contains the list of regional integration agreements incorporated in the RIA variables. They include a wide variety of trade agreements—in total 70 of them—covering all continents, countries with a diverse degree of development, and a long period of time.[12] The main criterion used to include agreements in the data set is that they have to be deemed as active as of January 1, 2000 by the World Trade Organization (WTO). In addition, I also include in the data set some agreements, particularly in Latin America, that have not been notified to the WTO but were considered as active by other sources.

[12] To my knowledge, this study incorporates the largest number of trade agreements, and has the longest time coverage so far in the literature.

Given that the RIA variable measures the partners' share of world GDP, an important underlying assumption is that in the long run agreements will reduce trade barriers for a substantial share of trade among partners, allowing domestic producers to consider the partners' market as part of his 'domestic' market. It is possible that in practice this is not always the case for all agreements, especially for agricultural production (Panagariya, 2000). However, quantifying the extent of the market that is actually freed would be an extremely complex task, and it is beyond the scope of this paper. Given that the GATT Article XXIV requires tariffs and trade restrictions to be removed on 'substantially all' intra-regional trade, I decided to keep this assumption, and interpret results as an upper bound effect for the impact of agreements that remove all trade restrictions among members.

The basic estimation strategy is through cross-country and panel data growth regressions that incorporate the RIA variables I constructed. To capture possible economies of scale or extent-of-the-market effects of regional integration I introduce a set of control variables that intend to capture other sources of growth—factor accumulation, allocative efficiency, technological transitions, government policies and institutions—so that the integration variable I created reflects the effect of economies of scale or extent-of-the-market on growth.[13]

Growth is measured by the growth rate of real GDP per capita at international prices, i.e., purchasing power parity adjusted. The choice of the control variables is based

[13] Given that it is not possible to control perfectly for all previous sources of growth, and that technological transmissions and knowledge spillovers are admittedly difficult to control for, it is likely that the RIA variable, to some extent, is also capturing technological transmissions and knowledge spillover effects. See Dollar (1992) for a discussion on the impact of allocative efficiency in growth. North (1990), Fischer (1993), Easterly (1993), and Rodrik (1998), discuss the role of government policies and institutions in creating economy-wide conditions under which countries can experience long-term growth.

directly on both the theoretical and empirical growth literature. In the first case, endogenous growth theory recognizes that capital accumulation, skilled labor, and knowledge transmission and accumulation are major driving forces of growth (Rivera-Batiz and Romer, 1990a and 1991b; Grossman and Helpman, 1991). To proxy for capital accumulation I included the investment rate, and for skilled labor I use human capital measured as the average secondary and tertiary years of schooling in total population. Capturing knowledge transmission is more controversial and several variables have been proposed in the literature. Following Wacziarg (2001), I use foreign direct investment and manufactured exports as a share of total exports.

Given that the decision of joining regional integration agreements is ultimately a political one, an important econometric concern can be the possible correlation between government policies that might have growth effects and the political decision of entering integration agreements. I address this concern by including a measure of government size—the ratio of government consumption over GDP. I also incorporate a measure of the political system to control for the type of institutions countries have. Following Tavares and Wacziarg (2001) I use a variable that assesses the level of democracy.

Recognizing that trade restrictions affect tradable goods prices and that price distortions have been found to deter growth (Easterly, 1989 and 1993), I include the black market premium as proxy for distortionary policies. Next, I introduce terms of trade with the aim of avoiding possible correlations between international price movements and the decision of joining integration agreements. Additionally, I use the ratio of total trade over GDP to control for the country's degree of openness.

Recent literature has provided evidence on the possibility of growth spillovers across countries (Chua 1993, Moreno and Trehan 1997, Easterly and Levine 1998, Easterly 2001), thus, I introduced the growth rate of trade partners to capture part of such spillovers.[14] It tries to capture the possibility that countries trading with fast-growing economies experience faster growth themselves, pulled by their partners. The trading partners' growth rate is a weighted average of the growth rate of other countries, where weights are given by share of the bilateral trade between a given pair of countries.

As Moreno and Trehan (1997), and Easterly and Levine (1998) have pointed out, the use of this variable introduces a problem of simultaneity in the regressions—the right hand side of the regression includes the dependent variable, growth.[15] In order to solve the simultaneity problem I use instrumental variables that endogenize trading partners' growth. The instrument I propose is the trading partners' investment rate. The trading partners' investment rate is a weighted average of the investment rate existing in other countries, with weights being the shares of bilateral trade. The use of this instrument is justified by the fact that the empirical literature has found that the rate of investment is an important predictor of economic growth (Levine and Renelt, 1992), thus, ex-ante, trading partners' investment would appear to be a suitable instrument for trading partners' growth.[16,17] In turn, conditional on trading partner's growth we should not expect trading

[14] Here trade partners refer to all trading partners regardless of the existence of an integration agreement. See Appendix 2 for details about the construction of this variable.

[15] The baseline regression in this paper—in matrix notation—is the following: $G = \theta WG + X\beta + \varepsilon$, where $\varepsilon \sim N(0,\sigma^2 I)$, $X\beta$ is a matrix including all control variables and the RIA variables, W is the weighting matrix formed by shares of bilateral trade, and G is the growth vector. The product of W and G is the trading partners' growth variable. Given that growth is on both sides of the equation the simultaneity problem arises.

[16] In all my estimations, I also find that a country's own investment rate is a significant determinant of growth.

[17] In addition, I constructed trading partners' growth using a different growth rate than the one used for the dependent variable. For the latter, I use the growth rate of real GDP per capita—purchasing power parity

partner investments to be a determinant of growth as the growth spillovers should come directly from other growth paths and not from their investment rate evolution.

The last control variable is the share of world GDP of bordering countries. This variable is introduced to control for the possibility that the RIA variables might be capturing the growth effect coming from the market size of neighboring countries and not from regional integration since regional integration agreements are frequently formed by neighboring countries.

Regressions that include all these control variables will be used when testing the effects of the RIA variables above presented. In order to analyze whether RIAs affect other sources of growth, such as the investment rate, I will include additional control variables to improve the identification of these effects. These additional control variables will be discussed later in the paper.

Table 2 provides basic statistics about the set of variables used in subsequent estimations. From the absolute RIA variable one can notice that on average, for the 1960-1999 period, trade agreements potentially add to a domestic market a market of the size of about 6.3% of the world's GDP. It ranges from zero for countries such as China, Japan or Panama to 30.4% for Israel.[18]

Table 3 presents descriptive statistics of decade averages for economic growth and the RIA variables since part of the estimation is conducted using decade averages. Noticeable is the sustained increase in the averages and maximums of the absolute RIA

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adjusted—because it better captures changes in the standard of living. For the trading partners' growth variable, the growth rate is constructed from GDP per capita at constant dollars. This rate of growth measures the ability of countries to engage in international transactions of goods, services, capital and/or knowledge—the channels through which growth spillovers might operate—and therefore, the use of GDP per capita in constant dollars assures a better proxy for such ability. The correlation between these two growth rates is 0.43, which helps to lessen the simultaneity problem.

[18] China, Japan and Panama have no reported integration agreements.

variable. The former has increased from 1.74% in the sixties up to 10.8% in the 1990's, and the latter has moved from 14.3% in the 1960s to 68.8% in the same period.[19] The same table also reports descriptive statistics for the relative RIA variable, which on average also has experienced a substantial increase. Given that there is a large variance in the distribution of economic size in the world, this variance is also transmitted to the relative size variable, which has a relatively large standard deviation of about four times the mean in each decade.

Table 4 displays the unconditional correlations between growth and the absolute and relative RIA variables. As can be seen, there seems to exist a moderate positive correlation between growth and these two variables, although for the absolute RIA variables it has changed over time—specifically, it decreases during the 1970's, recovers during the next decade, only to decrease again in the 1990's.[20] A plausible explanation for the decrease during the 1970's could be that the oil price shock may have weakened the relationship. In order to control for this possibility, estimations control for shocks in the terms of trade and incorporate decade dummy variables in the panel data estimations.

2.4. Results

In this section I report the results using the RIA variables created in section 3 in both cross-section and panel data growth regressions. I also address concerns related to the endogeneity of regional integration agreements, and I study the role of the level of development in generating growth effects. Next I discuss the impact of exclusion and finally I test for additional channel effects of regional integration.

[19] The share of the world GDP is kept constant at 1960 values in all RIA calculations.
[20] The unconditional correlation for 1960-1999 averages is 0.40.

2.4.1. Growth Effects of Regional Integration: Is the Size of the Market Important?

The first estimation exercise involves cross-section growth regressions using 40-year averages for the 1960-1999 period, with least squares and instrumental variables. The instrumental variables regressions controls for the simultaneity problem already discussed in section 3, and they have trading partners' investment as the instrument for trading partner's growth.

Regressions incorporate all controls variables discussed above, namely, the logarithm of initial GDP per capita, the ratio of government consumption over GDP, the black market premium, the investment rate, foreign direct investment as a percentage of GDP, the share of manufactured exports in total exports, the terms of trade, the ratio of total trade over GDP, trading partner's growth, and bordering countries share of world GDP. In terms of the sample size, the regressions include 81 countries.[21]

Results are shown in Table 5. In columns 1 and 2, I first report results using a dummy variable that captures whether the country belongs to at least one RIA. [1] In this set of regressions belonging to an RIA means that a country joined at least one agreement before 1980. I constructed this variable to contrast results using the RIA variables that I introduce in this paper, with variables similar to the ones used so far the literature, in particular the one proposed by Vamvakidis (1999). According to Vamvakidis, and most of the empirical literature, I should expect this variable to be insignificant, and in fact, as in his paper, becoming a member of a RIA does not have any significant effect on growth.

[21] Appendix 3 contains the list of the 81 countries included in regressions using 40-year averages.

In terms of the control variables, results indicate that the log of initial GDP per capita, the investment rate, manufactured exports, and bordering countries' share of world GDP have the expected signs and are statistically significant with OLS. Similar results are obtained with instrumental variables (column 2).[22] In terms of the quality of the proposed instrument for trading partners' growth, and following Bound *et al*. (1995), I report in the lower part of the table—in column 2—the *F*-statistics and the partial *R*-squared of the excluded instrument in the first-stage regression.[23] The *F*-statistic reveals that trading partners' investment is significant at any standard level confidence, and the partial *R*-squared indicates that it explains 49% of the trading partners' growth variation left after 'partialling-out' the included covariates. Thus, trading partners' investment is a suitable instrument for trading partners' growth. However, a Hausman specification test of the null hypothesis that no systematic differences exists in the instrumental variables and least squares coefficients fails to reject the null hypothesis, indicating that least squares should be preferred to instrumental variables.[24]

Column 3 reports the least squares regression but now introducing the absolute RIA variable presented in section 3 (equation 1), namely, the extent of the market

[22] The main purpose of this paper is to discuss the effect of regional integration agreements on growth; therefore, I will focus the analysis mostly on the RIA variables, without deeply discussing results on control variables, since they have been already discussed in the literature.

[23] Excluded instrument refers to that used in the first-stage regression but not included in the second-stage regression. Included instruments refers to all other exogenous variables used to identify the endogenous variable. For Table 5, the excluded instrument is trading partners' investment. Thus, the *F*-statistic is that for trading partners' investment on the regression of the RIA variable on all control variables and trading partners' investment (first-stage). The partial *R*-squared is the R-squared with the included instruments 'partialled-out' (Green, 2000).

[24] The Hausman specification test is based on the chi-squared statistic: $H = (\hat{\beta}_{IV} - \hat{\beta}_{OLS})'(Var[\hat{\beta}_{IV}] - Var[\hat{\beta}_{OLS}])^{-1}(\hat{\beta}_{IV} - \hat{\beta}_{OLS})$, where $Var[\hat{\beta}_i]$ is a covariance matrix. If $Var[\hat{\beta}_{IV}] < Var[\hat{\beta}_{OLS}]$ the chi-squared statistic would take a negative value, which is inconsistent with the definition of a chi-squared statistic. In that case, following Green (2000), the term containing the difference in the covariance matrices is assumed to be a zero matrix, thus, the chi-squared statistic is zero. Below the 'Hausman' statistic I report the corresponding p-value.

included in all agreements to which each country belongs. The OLS estimates of the coefficient on the absolute RIA variable indicate that the extent of the market has a positive and significant effect on growth, a result that contrasts with outcomes obtained using traditional dummies that measure integration. Column 4 displays instrumental variables estimates. Again, as in the OLS estimates, the instrumental variables estimate for the RIA variable is positive and statistically significant.

The instrument for trading partners' growth, i.e. trading partners' investment, performs well, both in terms of its significance in the first-stage regression—the *F*-statistics is 67.1, meaning that the instrument is significant at any conventional significance level—and in terms of its explanatory power on the absolute RIA variable—the partial *R*-squared of trading partners' growth is 0.5. Although, trading partners' investment appears to be a suitable instrument for trading partners' growth on account of these two measures, again the Hausman specification test indicates that least squares is the appropriate estimation procedure.

In terms of the control variables, in both the least squares and instrumental variables regressions the log of initial GDP per capita, government consumption, the investment rate, and bordering countries share of world GDP have the expected signs and are statistically significant, all results that are in line with those in the literature.[25]

One of the contributions of the absolute RIA variable I use in this paper is that it allows us to find differentiated growth effects varying with the size of RIA partners. The estimates I find suggest that joining an agreement with countries with a share of the world GDP of 1% might increas the growth rate, in the long-run, by 0.055 percentage

[25] See among others Levine (1992), Brunetti (1997), Easterly and Levine (1998), and Rodriguez and Rodrik (2000).

points. To put this result in perspective the estimated coefficient suggests that signing an agreement with countries such as Canada or France—that accounted for 2.1% and 5.4% of world GDP in 2000, respectively—would increase a country's growth rate by 0.12% and 0.3% percentage points, respectively. In contrast, signing an agreement with countries such as Egypt or Colombia—with a share of world GDP of 0.24% and 0.3% percentage points, respectively—would increas growth by 0.013 and 0.016 percentage points, respectively. The implication of this result is that countries would benefit more by signing agreements with larger partners.

In addition to the effect coming from the absolute size of the market, I have created a variable that measures the relative size of the integration partners, i.e., the relative RIA variable, and columns 5 and 6 report results using this variable. With both least squares and instrumental variables, the coefficient is positive and statistically significant, which indicates that the relative size of the market also matters. Again the Hausman specification test indicates that least squares are the most appropriate estimates for this model.

In order to obtain an indication of the economic importance of this effect, I will use the distribution of relative sizes of all pairs of countries in the world, which can be described as follows: the 75^{th} percentile has a value of 14.3, meaning that in 25% of all possible pairs of countries in the world, and therefore of possible bilateral integration agreements, one of the partners is at least 14 times the size of the other.[26] With this

[26] The inverse is also true: in 25% of all possible pairs of countries, the total GDP of one of the partners is 0.07 times the other.

difference in sizes, an integration agreement would benefit the smaller country of the pair, by increasing its growth rate by 0.009 percentage points.[27]

The 90th percentile of the distribution of relative size has a value of 110.5, giving a growth effect of 0.07 percentage points; the mean is 136.5, which gives a growth effect of 0.086 percentage points, and a 95th percentile value equals 358, giving a growth effect of 0.23 percentage points. The point estimates reported in Table 5 imply that the growth effect of the relative size of the market becomes economically significant only when extremely small countries engage in agreements with large countries.

Columns 7 and 8 introduce both RIA variables—absolute and relative—at the same time, finding that both variables remain significant, and the Hausman specification test again rejects the instrumental variables estimates. The decrease in the significance level of the RIA variables is due to collinearity between them, since the relative RIA variable is constructed using the absolute RIA variable. In addition, a test of the joint significance of both RIA variables—reported in the last row of the table—indicates that they are jointly significant.

With the estimates of the RIA variables presented en Table 5, column 7, I can estimate the possible growth effects of potential integration agreements. For instance an agreement between The Unites States and Brazil, within the framework of the Free Trade Area of the Americas (FTAA), would increase the growth rate of the Unites States by 0.12 percentage points and that of the Brazil by 1.4 percentage points, with almost all the effect coming from the absolute size of the market—given that the US economy is about 11.5 times the size of Brazil's economy, the effect from the relative RIA variable is almost negligible for both countries. In contrast, an agreement between Canada and

[27] The effect on the growth rate of the larger country would be 0.00004 percentage points.

Bolivia would increase their growth by 0.001 and by 0.14 percentage points, respectively. For Bolivia 25% of that increase would come from the effect of the relative RIA variable, since Canada is about 87 times larger, and for Canada all the effect would be generated by the absolute size of the market.

In order to exploit the time series dimension of the data, I estimated both country fixed and random effect models. A panel approach has several advantages vis-à-vis a cross-section one. First, by using decade-averages of the variables it reduces measurement error problems. In this in particular, it helps to decrease the measurement error in the independent variables of interest, the RIA variables. For instance, for the case of the absolute RIA, which in practice increases over time for all countries, decade averages will capture better that evolution, in contrast with the cross-section approach in which—obviously—only one value of the variable is used.[28]Another advantage of the panel approach is that allows incorporating the impact of time-specific and country-specific effects on growth. The former can control for worldwide changes in the growth rate due to worldwide business cycle, and the later control for time invariant country-specific determinant of growth such as the geography, climate, culture, or history. Panel data also allow for more variation in the data and therefore for reduction in the degree of collinearity in the covariates and for possible gains in efficiency (Baltagi, 2001).

The results are reported in Table 6a and they in line with those obtained with cross-section regressions. First, I introduce a variable for whether a country belongs to at least one RIA. A country is considered to belong to a RIA if it joined the agreement

[28] The choice of the way in which that value is measured becomes even more important in cross-section estimates, as different approaches can give very different values for a time-variant variable. For instance one could choose using the average of the whole period (as implemented here). Another possibility is to use the value of the variable in the time mid-point of the sample. Although these are still present when using decade average the differences should be considerably reduced in the panel framework.

during the first half of the decade. The point estimate indicates that belonging to at least one agreement does not have any significant growth effect, which again points out the lack of explanatory power of a dummy variable as a measure of regional integration (columns 1 and 2). Second, even after controlling for country fixed effects, which leaves only the within country variation as a source to explain changes in the growth rate, the absolute RIA variable appears to have a significant positive growth effect (columns 3 and 4). The Hausman specification test indicates that there is significant difference in the coefficients of the two models, thus rendering the country-fixed effects as the appropriate ones.

Third, the estimated coefficients on the relative RIA variables are also positive and significant in both models, with country fixed effects being more suitable according to the Hausman specification test (columns 5 and 6). However, when both RIA variables are introduced at the same time, results indicate that both the absolute and the relative RIA variables are insignificant. As before, one explanation for the lack of significance of the RIA variables is collinearity between the two RIA variables, caused by their high correlation, especially during the first part of the sample period. The unconditional correlation between the RIA variables was 0.84 in the 1960's; it decreased to about 0.4 in the next two decades, and fell to 0.36 in the 1990's. Even though the RIA variables are not significant individually, an *F*-test (reported in the last row of the table) suggests that are they jointly significant in both models.

Table 6b presents the results for the instrumental variables fixed effects and random effects models. These models instrument the trading partners' growth variable, thus correcting for the simultaneity problem caused by the use of this variable. The

results indicate that the RIA variable is only significant with random effects models, and that fixed effects estimates should be preferred. However, an additional Hausman test (not reported in the table), comparing the fixed and random effects models, with the instrumental variables fixed and random effects models, i.e. Table 6a with Table 6b models, indicate that in all four cases, the fixed and random effects should be preferred over the instrumental variables models, which suggests that simultaneity in not a problem in term of generating a bias in the estimates of the RIA variables.[29]

Using the estimates reported in Table 6a, column 7, and taking the coefficients of the RIA variables as jointly significant, the expected growth effect of regional integration of the hypothetical agreements used before would be as follows: an agreement between USA and Brazil would increase their growth rates by 0.05 and 0.6 percentage points respectively, with more that 99% of the effect coming from the absolute RIA variable. For an agreement between Canada and Bolivia, the increases in their growth rates would be of 0.0005 and 0.1 percentage points respectively. For Canada, 99% of the effect comes from the absolute size of the market, although the size of the effects is still very small given the size of Bolivia's economy, and for Bolivia, 49% of the effect would come from the absolute size and 51% would come from the relative size of the market.

[29] The results reported on Tables 5, 6a, and 6b are robust to the exclusion of the human capital and democracy variables, which increases the sample size to 105 countries. The results are also robust to the use of alternative measures of openness and regional spillovers. For openness the alternative measure was the trade tariff revenues as a percentage of imports plus exports. Following Moreno and Trehan (1997) alternative measures of regional spillovers were: weighted distance to the rest of the world (weight: bilateral trade share), normalized weighted distance to the rest of the world, and bordering countries log of GDP. Results are also robust to the exclusion of trading partners' growth.

2.4.2. Addressing Endogeneity of Regional Integration Agreements

A recurrent problem of cross-country regressions is the possibility of endogeneity in one or more of the regressors. Specifically, it might be that fast growing countries, because of some observable or unobservable characteristics, are more prone to engage in RIA agreements, implying that the relationship between RIAs and growth is not causal.[30] To address the possible endogeneity of RIAs I propose the use of geographical regions as instruments for the decision to engage in such agreements. It is not hard to see that neighboring countries within geographical regions tend to join the same RIAs, and therefore, a set of geographical categorical variables would have strong correlation with RIAs and could be used as appropriate instruments.[31] The regions I propose as instruments are Oceania, Asia, Africa, Europe and the Americas.[32]

Table 7 presents the estimates obtained when I introduce geographical regions as instruments for the RIA variables in the cross-section regressions (with 40-year averages) and in the panel data regressions (with decade averages). Because the instruments are geographical dummy variables, an instrumental variables fixed effects models cannot be estimated, leaving random effects as the only estimation procedure available for panel-data models. Columns 1 and 5 present the results of the regressions with the dummy for participation in RIAs. As in the previous results, entering at least one RIA does not affect growth. Columns 2 and 6 display estimates including the absolute RIA variable in the cross-section and panel data estimates, respectively. The results indicate that the absolute

[30] An additional concern might be a causality running directly from growth to RIA. However, Granger causality tests indicate that the RIA variable granger-causes growth but growth does not granger-causes the RIA variable.
[31] See Appendix 1 for the list of countries belonging to each agreement.
[32] Given that trading partners' growth would remain as an endogenous variable in the instrumental variables regressions, the full set of excluded instruments is composed by the regions defined above and trading partners' investment rate.

size of the market of countries joining an RIA has a strong positive and statistically significant growth effect, with point estimates similar on both the cross-section and panel data models.

The table also reports the *F*-statistic and the partial *R*-squared of the excluded instruments for the cross-section regressions following Bound *et al*. (1995). For the absolute RIA variable (column 2) they indicate that the instruments are jointly significant at any standard level of confidence, and that they explain a large portion (about 56%) of the left out variation of the absolute RIA variable, after netting out the effect of all exogenous covariates. However, the Hansen's 'J' statistic test of overidentifying restrictions rejects the null hypothesis that the proposed instruments are valid. Also, the Hausman specification test—under the null hypothesis that the coefficients of instrumental variables and least squares are equal—shows that we cannot reject the equality of coefficients and therefore we should keep the estimates obtained with least squares (Table 5).

For the panel data estimate (column 6), the Hausman specification test performed on the whole model—under the null hypothesis that the coefficients of instrumental variables random effects and random effects are equal—suggests that a random effect model is more appropriate, i.e. the appropriate results are those reported in Table 6a.

Columns 3 and 7 display the estimates with the relative RIA variable. As in the previous results, the relative RIA variable is positive and statistically significant. The instruments do not perform as well as with the RIA variable in terms of the *F*-statistic, and the partial *R*-squared, nevertheless, the overidentification test cannot reject their

validity. In addition, the Hausman specification test points out again that I should use least squares instead of instrumental variables.

Finally, columns 4 and 8 report results when both RIA variables are included. For both models only the absolute RIA variable is statistically significant, however, the *F*-test indicates that they are jointly significant.

In summary, this section has attempted to deal with a possible problem commonly reported in country growth regressions, namely, the potential endogeneity of one of the covariates. Specifically, here I have attempted to control for the probable endogeneity of the RIA variables. The instruments that I have proposed are geographical regions, and, even recognizing that they might be imperfect instruments, they are the best available choice I have to generate exogenous variation in the RIA variables, especially in the absolute RIA variable.

The general picture that arises from the estimates is that the results are in line with those of the literature concerning the lack of any growth effect when measuring regional integration as simple dummy variables. At the same time they are in line with results reported in the previous section regarding the role of the market size, suggesting that even after controlling for the potential endogeneity of RIAs, regional integration agreements have positive effects on growth.[33]

[33] The results reported in sections 4.1 and 4.2 are highly robust to some modifications in the data set of agreements. Starting from the 'Original Date' reported in Appendix 1, which is the date in which the agreement became active for the first time, I determined for some agreements a second date, called the alternative date, which is intended to capture the fact that some agreements, particularly some early agreements between developing countries, did not became fully active, or the planned liberalization schedules were not deepened enough over time, and were re-launched or revived later on. Thus, the alternative date is set at the time of the revival of the agreement. With the 'original' date I calculated the RIA variables used in all reported results. I use the 'alternative' date to re-calculate the RIA variables as a robustness test, and the exercise indicates no significant differences in the reported results. In an additional robustness test I classified all agreements as active/functioning or non-active/non-functioning either at the original or the alternative date. This categorization was made because for some agreements there is a lack

2.4.3. Is the Development Level of the Partners Important in Generating Growth Effects?

In this section I further explore whether regional integration has growth effects by looking at the impact of agreements depending on the type of countries joining them. In particular, I study whether the level of development of the members plays a role in determining growth effects. Even though this question might have been addressed indirectly before in the literature, by including dummy variables for different RIA agreements, which might be formed by partners of the same development level, here I introduce a new approach by classifying agreements in three categories, namely, North-North, North-South, and South-South, and testing if these types of agreements have generated different growth effects.

In order to implement this strategy, I first classified countries into North and South. Following Pritchett (2000) I defined North countries as those belonging to the OECD (except for Turkey, Mexico, Hungary, Korea, and Poland), Malta and Cyprus. All other countries were classified as South. With this criterion, 15 agreements are classified as North-North, 18 as North-South, and 37 as South-South. Agreements containing at least one South country were classified as North-South. Then, I re-calculated the absolute and relative RIA variables (equations 1 and 2) presented in section 3, for each 'type' of agreement.[34]

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of information regarding the actual effectiveness in the liberalization process followed by the members, which prevents me from concluding with absolute certainty that the agreement is active. In addition, different sources point out similar concerns regarding specific agreements (World Bank, 2003). With these two new categories, I re-calculated the RIA variables, first using agreements that were classified as active/functioning at their 'original' date, and second, using the agreements classified as active/functioning at their 'alternative' date. These two exercises gave highly similar results to the ones here reported.

[34] See Appendix 1 for the list of agreements, countries, and the category in which each agreement is classified.

Table 8 summarizes the results of cross-country regressions with forty-year averages.[35] First, I report the least squares estimates; second, the instrumental variables estimates in which only trading partners' growth is an endogenous variable, and third, the instrumental variables estimates in which trading partners' growth and the RIA variables are endogenous variables. For each model the RIA variables for North-North, North-South, and South-South agreements are included separately.

The estimated coefficients displayed in the columns 1 through 3 suggest that North-North agreements have positive and significant growth effects. North-South agreements seem to have a positive effect with OLS but the estimated coefficient is not significant (column 1). Once I instrument to correct for the simultaneity problem the coefficient on North-South agreements becomes significant (column 2). When instrumenting to correct for simultaneity and endogeneity of RIAs, only North-North agreements remain significant (column 3). South-South agreements have a positive but statistically insignificant coefficient in all three regressions.

In the instrumental variables regressions (columns 2 and 3), the instruments are jointly significant in the first stage regression, and explain a large portion of the unexplained variation in the RIA variables.[36] However, the Hausman specification test reported in the table rejects both instrumental variables models in favor of the OLS estimates.

An *F*-test of the joint significance on the three RIA variables indicates that they should be considered as jointly significant in all three models. This is the result of the combination of a high significance level for the North-North variable, which is close to

[35] For ease of exposition all other control variables are not reported.
[36] These statistics are not reported in the table but are available upon request.

1% in the three models, and the almost significant coefficient for the North-South variable in models 1 and 3.

In turn, the lack of significance of the North-South variable in the regressions reported in columns 1 and 3 could be explained first by the relatively small number of South countries engaged in North-South agreements. The reason is the following: if I expect the size—both absolute and relative—to have an effect on growth, from the standpoint of North countries both RIA variables take low values, since South countries are in general small countries (measured by their share in world GDP). Thus, I would expect a relatively small or negligible effect for North countries. On the contrary, I should expect a larger effect for South countries, given the larger size of North economies. However, existing North-South agreements involve relatively few South countries (10) compared to the number of North countries (17), and given the larger number of the latter, the results might be influenced by the impact on North countries—supposedly negligible—rendering the average effect of North-South agreements statistically insignificant.

Additionally, 12 out of 18 of the North-South agreements were signed in the second half of the estimation period, with 11 of them signed during the 1990's. Therefore, if the growth effects of regional integration take time to appear it might be that these North-South agreements are too recent to show significant growth effects. This should be reflected in the cross-section regressions for which I use forty-year averages, but still might be present in the panel-data estimations.

Columns 4, 5 and 6 show the estimates using the relative RIA variable. Again, only North-North agreements appear as having growth effects. Both North-South and

South-South agreements have statistically insignificant estimates; however, it is interesting to notice that the relative size of the partners in South-South agreements might have a negative growth effect. This result, although not statistically significant, is in line with what was suggested by Venables (1999) and Puga and Venables (1998) in that relatively small developing countries might be negatively affected by joining an agreement with other relatively large developing countries, i.e., the smaller partners in a South-South agreement have more to lose than the larger partners.

Columns 7, 8, and 9, display the estimates including both the RIA variables and the relative RIA variables. Taken as a whole they indicate that if any effect is present it might be the one coming from North-North agreements. Even though the Hausman specification test indicates that I should prefer the least squares estimates, the lack of significance of the RIA variables in column 7 might be explained again by the lesser variability of the RIA variables due to the reduced number of agreements contained in each of them, and the large correlation between them—specially between the absolute and the relative RIA variables of each type—that might be generating multicollineary problems.

Moving to the panel-data estimates, Table 9 displays the estimates with country fixed effects, instrumental variables estimates with country fixed effects that control for the simultaneity problem, and instrumental variables estimates with random effects that control for both simultaneity and endogeneity of the RIA variables. The use of instrumental variables combined with random effects to control for endogeneity is justified by the impossibility of estimating an instrumental variables fixed effect models

with regional dummy variables as instruments. This is due to the time invariant nature of the regional dummies that makes the estimation of a fixed effects model infeasible.

The first three columns indicate again that North-North agreements have generated positive and significant growth effects. The Hausman specification tests for the instrumental variable regressions indicate that I should use the fixed effects estimates. The primary difference with the cross-section estimates is that South-South agreements have a positive and significant effect on growth. In addition, the North-South variable has a negative point estimate, although it is not significant.

Columns 4, 5 and 6 include the relative RIA variable. Again North-North agreements have a positive and significant effect on growth and, as in the cross-section estimates, North-South agreements have a negative, but now significant growth effect. When both the absolute and relative RIA variables are included together—columns 7, 8, and 9—the above mentioned results remain unchanged: both North-North and South-South agreements have significant growth effect with either RIA variable. Furthermore, *F*-tests indicate that the North-North and South-South RIA variables are jointly significant. The Hausman specification test indicates in all cases that the country fixed effects models are most appropriate.

With these last estimates we can conclude that North-North agreements have had a positive effect on growth, and that the effect of South-South agreements is ambiguous, since the absolute size of the market seems to have a positive effect on growth, but the relative size might have a negative one. For instance an agreement between Mexico and Panama would increase the growth rate of the former by 0.018 percentage points, but would decrease the growth rate of the latter by 0.66 percentage points. However, an

agreement between Egypt and Algeria might increase both countries' growth rates by 0.1 and 0.08 percentage points, respectively. In general, obtaining a positive effect from a South-South agreement would depend on the size of each country. However I can estimate the minimum size that a country should have to experience positive growth effects. This critical size is obtained as follows:

$$SWGDP_i^* = -\frac{\beta_{rel}}{\beta_{abs}},$$

were β_{rel} is the point estimate on the relative RIA variable, and β_{abs} is the point estimate on the absolute RIA variable. If a countries' share of world GDP is larger than ratio of the RIA coefficients, then the expected growth effect of the agreement is positive. Specifically, using the point estimates reported in Table 9 column 7, any South country with a share of world GDP larger than 0.054 percent would experience an increase in its growth rate.

The increase in the significance of South-South agreements in the panel-data models is additional evidence that given the time structure of the agreements—i.e., a large number of North-South and South-South agreements were signed in the second half the period—a cross model using averages of a long period might not be the best option as it would tend to underestimate the impact of agreements formed towards the end of the estimation period. At the same time, the lack of significance of North-South agreements still might be explained by the small number of South countries participating in them, and by the fact that they were signed relatively late in time.

Overall, results suggest the existence of positive growth effects from North-North agreements and ambiguous effects for North-South agreements. Results for North-South agreements are mixed, with some evidence from cross-section regression pointing out

that the effects might be positive, but with results from panel data estimates indicating ambiguous effect.

It is interesting to notice that results on South-South agreements are closely related to predictions in Venables (1999) that free trade agreements between developing countries would bring larger benefit to those countries with comparative advantages closer to the world average. In his model, richer South countries, which are those with larger endowments of capital, would benefit the most. In my case, although not necessarily the richer country, larger countries would gain more provided that they are sufficiently large. In terms of North-South agreements I cannot provide any string evidence in regard to Puga and Venables (1998) hypothesis that North-South agreements would generate greater income gains than South-South agreements. Given that North-South agreements are both relatively few in number and a recent phenomenon, it is likely that the lack of any significant impact result from lack of data; as such, we will have to wait for some time to study their growth effects.

2.4.4. Growth Effects of Other Countries Integration

So far, the literature on the growth effects of regional integration agreements, including the present study, has concentrated solely on the direct growth effects of RIAs for countries when they join such agreements. This is an important part of the possible growth effects of RIAs, but might not be the only way in which regional integration could affect growth performance.

It might be the case that the integration of other countries could be affecting the growth performance of countries not participating in an agreement. Static trade theory clearly describes the costs for countries left out of an integration agreement; however, to

my knowledge, no empirical work has been done to estimate the long-term consequences on growth of this discrimination. Research along this line could help shed some light on the question of whether countries left out of agreements should respond by strategically integrating themselves into other agreements in order to compensate for possible negative growth effects of others countries' integration.

2.4.4.1. Measuring Exclusion

In order to study the effect that integration has on countries that are not included in the process, it is necessary first to generate a measure of such exclusion. The one that I propose here is the extent of the world market that is under regional integration agreements between other countries. Starting from this general definition of a measure of exclusion, in this section I will construct an index of other countries' integration and I will include it in my econometric model along with the RIA variable already introduced in previous sections.[37]

I propose as a measure of the extent of the market from which countries are excluded, an index the joint share of world GDP of the countries joining an agreement. This means that for each country the index would add the joint share of world GDP of all agreements in which it does not participate. I name this index the excluded RIA variable, and it can be represented as follows:

$$Excluded\ RIA_{jt} = \sum_{\substack{i=1 \\ i \neq j}}^{N} \sum_{\substack{k=1 \\ k \neq j}}^{N} \left[D_{kt}^{i} \times \left(SWGDP_i \times SWGDP_k \right) \right]. \qquad (3)$$

[37] Appendix 4 discuss in greater detail the way in which this and other alternative measures of exclusion are constructed, along with their characteristics, differences and their implications.

As before, N is the number of countries in the world, D_{kt}^{i} is a dummy variable for an integration agreement between country i and k in period t, $SWGDP_i$ is the share of world GDP of country i in 1960. Given that $\sum_{i=1}^{N} SWGDP_i = 1$, the excluded RIA variable is bounded between 0 and 1. The logic behind this variable is that whenever two countries engage in an integration agreement, countries that are left out of the treaty face a competitive disadvantage that might have negative consequences for growth, and, the larger the size of the market from which they are excluded, the larger is the negative effect that we might expect.[38]

2.4.4.2. Estimating the Growth Effects of Exclusion

In order to estimate the growth effects of being excluded from regional integration agreements, I will use both the multiplicative excluded RIA variable described by equation (3), and the RIA variable used in previous sections and described in equation (1).

Previous to the estimation, it can be useful to look at the unconditional correlations—by decade—between the excluded RIA variable and growth. Table 10 displays them, and, as can be seen, the correlation changes from negative to positive twice, which suggests that it may be difficult to find significant growth effects with this variable. Given that I will be introducing the excluded RIA variable and the absolute and relative RIA variables jointly in the estimations it is also interesting to see the

[38] Appendix 4 discusses why I multiplied instead of added the shares of world GDP of countries belonging to an agreement. In addition, this definition of exclusion is consistent with agreements that have enforceable rules of origin in place. When this is the case, for a given country, an agreement signed by third countries will generate some level of competitive disadvantage to the left out country that cannot be eliminated unless the excluded country joins the agreement, or the rules of origin are nonexistent or not enforced.

unconditional correlations between these two variables. Table 11 presents them, and it indicates that the correlation between the excluded RIA and the absolute RIA variable is large, although it has decreased over time. The correlation between the excluded RIA variable and the relative RIA variable is somewhat more complex, being negative during the 1960's and positive and increasing in later decades—although the correlation is still relatively low.

Table 12a reports cross-section regression results incorporating the excluded RIA variable proposed in this section. Column 1 shows that being excluded has not affected growth significantly, and if anything it might have a positive effect. A similar result is obtained when instrumenting trading partner's growth (column 2).

Column 3 introduces both the excluded RIA variable and the absolute size of the market variable in the same regression. I find that the direct growth effect of a country's own integration processes remains positive and significant—as reported in previous sections—and at the same time, the excluded RIA variable indicates that being excluded from other countries' integration does not significantly affect growth, although the estimated coefficient is positive. Similar results are reported in column 4 when I instrument the trading partner's growth variable. The Hausman specification test indicates that the least squares estimator should be preferred to the instrumental variables estimates. In terms of the possible joint significance of the RIA variables, an *F*-test indicates that the excluded and the absolute RIA variables are jointly significant. This result is likely generated solely by the high level of significance of the RIA variable, which has a p-value of 0.02.

Finally, columns 5 and 6, that include the excluded RIA variable, and the absolute and relative RIA variables, results are similar to previous ones, in that only the absolute RIA variable appears to have a significant (and positive) effect on growth.

Moving to panel data estimates, displayed in Table 12b, I first find that the excluded RIA variable does not have a significant growth effect for any of the models—fixed and random effect, and instrumental variables with fixed and random effects. Columns 1 though 4 indicate that the estimated coefficient on the excluded RIA variable is not significant, however, in contrast to the cross-section estimates, the point estimates are negative. In columns 6 through 9 I include both the excluded and the absolute RIA variables, finding again that only the absolute RIA variable has a positive and significant effect on growth. The Hausman specification tests indicate that fixed effects are the most appropriate, and that instrumental variables models do not produce significantly different estimates from those of fixed and random effects, when instrumenting for trading partner's growth.[39]

In the last four columns I include the excluded, absolute and relative RIA variables, finding that only for the instrumental variables random effect model is the absolute RIA variable statistically significant. As discussed in previous sections, this might be the result of multicollinearity between the RIA variables.

In sum, this section attempted to investigate empirically the consequences that regional integration has on countries that are excluded from the integration process. As a measure of exclusion I have proposed the use of a variable that captures the extent of the

[39] These last Hausman specification tests are not reported in the table.

market from which left-out countries are excluded when third parties engage in integration.

The estimates indicate that being excluded does not have significant growth effects. A plausible interpretation of these results might be the following: if regional integration is good for countries that participate in the process—which seems to be the case based on the evidence provided in previous sections—then, the growth experienced by those countries integrating might be pulling growth in other countries, for instance through an increase in trade, or foreign investment with and to excluded countries. Thus even though other countries are excluded from an agreement they might be benefiting from the positive effects of the agreement on its members. The results reported here suggest that positive growth effects are offsetting negative ones, rendering the final effect of being excluded insignificant.

2.4.5. Channel Effects of Regional Integration.

Previous sections have provided evidence that, overall, regional integration agreements over the last four decades have positively affected the long-term rate of growth. In addition to the direct effect of enlarging the domestic market via RIAs, it might also be that regional integration affects growth indirectly, by influencing other sources of growth. Henrekson *et al*. (1997), using OECD country data, have found that the European Community (EC) and the European Free Trace Area (EFTA) have helped to reduce inflation. They also tested whether the investment rate increased when countries joined the EC and EFTA, finding no significant change in this variable. However, their study cannot be regarded as conclusive evidence because it has limited

time coverage, and its conclusions cannot be generalized as it is focused only on the EC/EFTA.

The growth regressions reported in this paper point towards to two channels that might be interesting to explore as significant determinants of growth performance. These two channels are the investment rate and manufactured exports as share of total exports. I selected these variables because they appear to have significant growth effects in most regressions reported in this study.

If regional integration agreements have a significant effect on these channels, then the positive growth effects of regional integration might come not only directly but also indirectly from these three variables.[40] In this section I will explore this possibility by studying the effects of RIAs on the rate of investment and the share of manufactured exports over total exports, using four different estimation procedures: least squares, instrumental variables, seemingly unrelated regressions and three-stage least squares.

Table 13 reports the results of cross-section regressions using 40-year averages for the 1960-1999 period. Columns 1 and 2 contain the least squares regressions for the investment and manufactured exports equations respectively. In addition to the RIA variable, they control for the logarithm of initial GDP per capita, the ratio of government consumption over GDP, black market premium, foreign direct investment, human capital, terms of trade, the ratio of total trade over GDP, a democracy index, trading partner's growth, and bordering countries share of world GDP. In order to improve the identification of the investment channel, following Wacziarg (2001), I include in the

[40] From the growth regressions reported in Tables 5, 6, 7, and 12, I find that the size of bordering economies might also be an important determinant of growth. However, I have selected investment and manufactured exports as channels to study because, in addition to emerging as significant determinants of growth, they could be affected directly by domestic policies. In the same line, I did not include the size of bordering economies because it cannot be affected directly by domestic policy decisions.

investment equation the percentage of the population aged over 65, and an ethnolinguistic fractionalization index.[41] In the manufactured exports model I also include population density and a dummy variable for East Asian countries. Finally, the investment rate is incorporated as a control variable in the manufactured exports equation, and the share of manufactured exports on total exports is included in the investment equation.

The results indicate that in the long run the RIA variable—that measures the absolute size of the market—has a positive and significant effect on both channels. The effect is stronger, in terms of its statistical significance, for the manufactured exports channel. Additionally, in the investment equation I find that human capital, exports plus imports as percentage of GDP (trade share of GDP), and trading partners' growth have positive and significant effects, indicating that more educated and 'open' countries have a higher investment rate. Ethnoliguistic fractionalization has, as expected, a negative and significant effect on growth, suggesting that more ethnically divided countries might be subject to greater social and political instability that hampers capital accumulation.

In the manufactured exports equation, having a more democratic government, larger bordering countries, and being more densely populated are factors with positive and significant effects. Surprisingly, terms of trade, defined as the growth rate of export prices minus growth rate of import prices, and the trade share of GDP have negative effects on the share of manufactured exports.

Columns 3 and 4 display the same regressions but now endogenizing the absolute RIA variable in order to control for the simultaneous determination of the dependent variable in each equation and the RIA variable. The instruments for the absolute RIA

[41] It measures the probability that two randomly selected persons from a given country will not belong to the same ethnolinguistic group (Mauro, 1995).

variable are regional dummy variables.[42] Here again the RIA variable is positive and significant in both channel equations.

In order to assess the performance of the instruments, I report the *F*-statistic of the instruments in the first-stage regressions and the partial *R*-squared of the instruments on the RIA variable. The former indicates that in both channels the instruments are jointly significant, and the latter indicates that they explain a large part of the variation in the RIA variable—52 percent for investment and 56 percent for manufactured exports. These two statistics provide initial evidence that the regional dummies are suitable instruments. In addition, the Hansen test of overidentification fails to reject the null hypothesis of the validity of these instruments. Although in both regressions a Hausman specification test on the whole regression indicates that the OLS estimates should be preferred to the instrumental variables estimates, the same test performed only on the coefficients of the RIA variable suggests the opposite, that I should keep estimates obtained with instrumental variables.

An additional concern is the possibility of correlation across channel equations. In order to address this possibility I estimated both channel equations simultaneously using seemingly unrelated regressions (SUR). The results are reported in columns 5 and 6. As with previous models, the RIA variable has a positive and significant effect on investment and manufactured exports. The Breusch and Pagan (1980) test of independence—with a null hypothesis of zero correlation between the errors of both equations—is not rejected at any conventional level of significance, suggesting that the

[42] The regions are Asia, Africa, Europe, and the Americas. The left out region is Oceania.

system can be estimated separately and therefore, that the least squared estimates are more appropriate than the SUR ones.[43]

The last model, reported in columns 7 and 8, addresses both the possible endogeneity of the absolute RIA variable and the correlation in the error terms of both equations. Thus, I use three-stage least squares to endogenize the RIA variable and simultaneously estimate both channel equations. In doing so, I still find that the effect of regional integration on the investment and manufactured exports channels is strongly significant. A Hausman specification test performed to compare SUR with the three-stage least squares estimates suggests that the latter are the more appropriate ones.

The results reported in Table 13 suggest that regional integration has positively affected the investment rate and the share in total exports of manufactures. Using point estimates from columns 7 and 8, an agreement with a country that has a share of world GDP of 1 percent might increase the investment rate between 0.22 and 0.44 percentage points, and the share of manufactured exports by 1.1 to 1.9 percentage points.

Given that the data span four decades I was able to exploit the time series dimension of the data by reestimating previous regressions using decade averages. The results appear in Table 14, with columns 1 and 2 presenting the least squares estimates. They indicate that regional integration had no effect on investment but positively and significantly increased manufactured exports. When instrumenting the RIA variable with regional dummies—columns 3 and 4—the estimates suggest a positive and significant RIA effects on both investment and manufactured exports. The *F*-statistic and the partial *R*-squared on the excluded instruments indicate that the instruments are highly significant

[43] The Breusch and Pagan (1980) test of independence is distributed Chi-squared with one degree of freedom.

in the first-stage regression and that they explain a large part of the RIA variable variation, although the overidentication test rejects them as appropriate instruments. However, the Hausman specification tests for the investment equation points toward the use of instrumental variables instead of least squares. In the manufactured exports equation the Hausman test rejects the null hypothesis of equality of the coefficients, suggesting the use of the OLS estimates.

The next two columns deal with the possibility of simultaneity in the determination of channel effects using seemingly unrelated regressions. The effect of RIAs on manufactures remains positive and significant, although RIAs do not seem to have an effect on investment. The independence test cannot reject the null hypothesis of independence in the errors, which suggests the use of OLS estimates instead of SUR estimates.

Lastly, columns 7 and 8 report three-stage least squares results. Here again RIAs increase the investment rate and as well as the proportion of manufactured exports. The Hausman specification tests comparing the three-stage least squares estimates with SUR estimates—under the null of equality of coefficients—indicates that I should keep the former. These last estimates suggest that an agreement with a country of the size of 1 percent of the world GDP would increase the investment rate and the share of manufactured exports by 0.19 and 1.1 percentage points respectively.

Overall, this section provides evidences that regional integration might be positively affecting other sources of growth, in this case the investment rate and the share of manufactured exports in total exports. The finding regarding the investment rate contrasts with that reported by Henrekson *et al*. (1997), which suggests that the lack of an

investment effect of the EC/EFTA in their research, might be a result specific to those agreements during their particular period of study (1976-1985), or that they have misspecified their estimation models.

2.5. Conclusions

The empirical literature has commonly addressed the question of growth effects of regional integration agreements by introducing a dummy variable in growth regressions. The use of a dummy throws away interesting information regarding important characteristics of the countries in the agreement that can influence the growth effects of regional integration agreements.[44] This paper departs from the use of simple dummy variables and contributes to the literature by focusing on one of those characteristics: countries' economic size. If increasing returns are present, the size of market matters, and in the context of regional integration it is always the case that countries of different size will face a market of different size—the market composed by all other countries joining the agreement—and therefore, different growth prospects.

First, I show that using a dummy variable to measure regional integration agreements will not produce significant effects on long-run growth. This result is in line with the existing empirical literature—among others De Melo *et al*. (1992) and Vamvakidis (1998 and 1999).

Introducing a new way of measuring RIAs, namely by capturing the partners' share of world GDP, I determine the size of the market incorporated into the domestic market when an integration agreement is created. Measuring RIAs in this way, and

[44] Deardorff and Stern (2002) point out that a dummy variable might be a "too crude" measure.

controlling for other sources of growth, allows me to test whether extent-of-the-market effects are present.

Different estimation procedures—least squares and instrumental variables—provide evidence that the partners' size of the market is a relevant source of economic growth, with results varying in magnitude depending on the estimation procedure, but at the same time robust to them. Moreover, I find that both the absolute and the relative size of the market matter, although the latter is less economically significant.

In addition, by including a measure of others countries' integration, I provide empirical evidence that exclusion from integration agreements has not hindered growth in left out countries.

The point estimates that I obtain imply important growth effects. For example, Chile, which recently signed a Free Trade agreement with the EU, might expect to increase their growth rate by about 0.6 percentage points. The expected gain in growth for the EU would be of 0.005 percentage points given that the gains are tied to size of the market and Chile is a small economy. Here, it is necessary to point out that the estimated gains in growth may not be realized by particular countries, however, they are suggestive that in the long-run the dynamic effects might tend to outweigh the possible static losses of regional integration.

The paper also studies another characteristic of RIAs that could affect growth, namely, the partners' development stage. When agreements are classified as North-North, North-South and South-South, with North being developed countries, I find evidence that North-North integration has fostered economic growth. At the same time the effects of South-South integration are ambiguous, with negative effects for extremely small

countries, but with the effect changing to positive for relatively larger South countries. This result is in line with Venables (1999) in the sense that in some South-South agreements larger partners would benefit at the expense of the small ones. For North-South integration the results are not conclusive, but there is some evidence of positive effects. A possible explanation for the lack of a significant results is that North-South integration is a relatively recent trend, with a reduced number of South countries participating in it, which might render as inconclusive an econometric estimation for this type of agreements.

Lastly, I have provided evidence that in addition to the extent-of-the-market effects, regional integration might be positively affecting growth through an increase in the investment rate and the proportion of manufactures in total exports. The finding that regional integration is increasing manufactured exports might be explained by the fact that when countries integrate, the negotiated liberalizations, reductions in tariffs or trade restrictions, do not usually include agricultural products and services (Panagariya, 2000), as opposed to the common practice of opening industrial sectors. If regional integration has increased the participation of manufactured exports in the exporting mix of integrating countries, and if manufactured exports are a significant source of growth, as indicated by the growth regressions here reported, regional integration might have not only direct effects on growth but also indirect effects operating through these channels.

Even though it is difficult not to associate the size of the market with economies of scale, this might not necessarily be the only explanation for the growth effects found here. Knowledge spillovers or technological transmissions might be captured by the RIA variables used here, making it difficult to separate their effects. While the findings

reported in this paper do not allow me to conclude with certainty that extent-of-the-market effects are the only force behind the growth effects of regional integration, they certainly suggest that economies of scale are playing a substantial role in generating these effects.

CHAPTER 3. REVISITING THE EVIDENCE ON THE IMPACT OF SOCIAL INFRASTRUCTURE INVESTMENTS IN EDUCATION: THE CASE OF NICARAGUA

3.1. Introduction

During the last fifteen years social funds have grown from being merely experimental institutions to being thought of as effective tools for meeting the needs of communities in developing countries. Backed by the financial and technical support of multilateral organizations, mainly The World Bank, and international donors, they have spread across the world to reach more than fifty countries.

Social funds have broad goals. In general, they are designed to incorporate local preferences in decision processes related to the provision of social infrastructure or services in poor communities. They are also used as a means of providing employment and transferring income during economic transition periods. Given the rapid increase in the amount of resources channeled through social funds, a closer analysis and rigorous evaluations of their impacts are necessary.

This paper reviews the experience of the Nicaraguan social fund (FISE) during the 1993-1997 period. As FISE is the primary source of social investments in general and especially in the education sector in Nicaragua, the present study assesses whether FISE investments in primary schools have affected relevant educational outcomes. In doing so, this paper contributes to the empirical literature on project evaluation, in particular to the still small but growing body of literature assessing the impacts of social funds around the world on education.

The literature on the impact of social funds on education is mainly comprised of four assessments carried out by the World Bank in Bolivia, Peru, Armenia and

Nicaragua.[45] All four case studies find modest impacts of school infrastructure investments undertaken by social funds on education outcomes. The Bolivian experience deserves special attention because it is the only social fund among these four that uses some level of randomization in the assignment of funds to schools. Since randomization eliminates the bias in the estimates due to differences in observable characteristics between the treated and non-treated groups, estimates from the Bolivian study should provide a guideline for the expected impacts in the Nicaraguan case. The results from Bolivia show a significant negative effect of school infrastructure investments on dropout rates, but no effect on enrollment, attendance or academic achievement. Even though each social fund has its own particular structure, procedures, and goals, the results from the Bolivian, Peruvian and Armenian experiences suggest that not much improvement in educational outcomes should be expected from the operation of social funds.

Of particular interest for this paper are the findings reported by Pradhan and Rawling (2002), who study the social fund operating in Nicaragua. They find that investments in primary schools have positive impacts on enrollment, in reducing the education gap[46], and in lowering the age at which children enter school for the first time. However, the methodology they use to estimate the impacts of FISE investments does not deal appropriately with a problem every project evaluation faces: the possibility of selection bias. This problem arises in non-experimental evaluations when individuals can self-select into the program being evaluated, rendering direct comparison between the outcomes of program participants and non-participants inappropriate. Pradhan and

[45] See Newman et al., (2002) for the Bolivian case, Paxson and Schady (2002) for the Peruvian social fund (FONCODES), Chase (2002) for the Armenian case, and Pradhan and Rawling (2002) for Nicaragua.
[46] In general, the education gap is defined as the difference between the expected educational achievement for a child of a given age and the actual achievement of the child. See Appendix 5 for detail on the specific definition of this variable that I use in this study.

Rawlings attempt to deal with selection bias by relying on the sample design. However, the sample design in their case does not completely eliminate the problem, and other methodologies are required to obtain adequate estimates of FISE impacts.[47]

Following an approach that controls for selection on observables into FISE projects, I find that no significant improvements on a selected group of educational outcomes can be attributed to FISE investments in primary schools. These outcomes include enrollment, absenteeism, highest grade completed and grade repetition, among others, which contradicts the results reported by Pradhan and Rawlings (2002). The difference in the results is due to the explicit consideration in my estimates of the possibility of selection bias driven by observable characteristics, which I address through the use of a regression approach and propensity score matching techniques.

The analysis of the impacts of social funds has greatly benefited from the simultaneous development of project evaluation techniques.[48] At the present time, the project evaluation literature offers a variety of tools to evaluate programs, and the choice of the appropriate estimation technique varies with the characteristics of the program, the availability of data and the question researcher wants to answer. In the present study I based the choice of estimation technique—regressions and propensity score matching—mainly on the availability of data to evaluate the program and the characteristics of the selection process followed by FISE in the school funding process.

By studying the impact of school investments on education outcomes, the paper also contributes to the literature on the relationship between inputs and educational outcomes in developing countries. There is also a growing literature that points toward

[47] See section 3 for a deeper discussion of the evaluation problem and the methodology used in this paper.
[48] For reviews of the latest developments in the program evaluation literature see Heckman, LaLonde and Smith (1999), Smith (2000), and Smith and Todd (2003).

positive effects of investments in school facilities on school attainment and the quality of cognitive skills.[49] However, the estimates in this paper indicate that in the Nicaraguan case, improvements in school conditions have not translated into improvements in outcomes.

The remainder of the chapter is organized as follows: section 3.2 provides background on social funds, Nicaragua's economy, its social profile, and the operation of its social fund FISE. Section 3.3 discusses the evaluation problem, and how the approaches used in this paper attempt to deal with it. It also describes the data used in the assessment carried in sections 3.4 and 3.5. Section 3.6 concludes.

3.2. Background

3.2.1. Emergency Social Funds

Social funds were conceived as tools to channel public investment more efficiently than the traditional structures and organizations of public sectors in developing countries. As generally defined, social funds are "agencies that finance small projects in several sectors, targeted to benefit a country's poor and vulnerable groups based on a participatory manner of demand, generated by local groups and screened against a set of eligibility criteria" (Jorgensen and Van Domelen, 1999). In order to achieve these goals social funds are usually structured as public institutions that operate as private firms or autonomous agencies (Rawlings et al., 2002).

Social funds were first introduced in Bolivia in 1986. Since then, they have been adopted in most countries in Latin America, and they have also been established in

[49] See, among others, Duflo (2001), Glewwe (1999), Glewwe and Jacoby (1999), and Hanusek (1995).

Africa, the Middle East, Eastern Europe, and Asia. By 2002 there were 58 countries with operational social funds established as tools to cope with the negative consequences of economic crises and stabilization programs, despite the lack of a strong evaluation track record that might justify their functioning.

In some countries, with Nicaragua being one of them, these funds provide public infrastructure and social programs. The sectors and type of projects they fund vary from country to country, and they include schools, clinics, water supply, sanitation, local roads, drainage, school feeding programs, nutrition interventions, training programs, counseling programs, and micro credit, among others.

Total investments in social funds have reached 8.0 billion U.S. dollars, with the World Bank being the major provider of resources, as it has invested more than 3.5 billon U.S. dollars worldwide. Another 4.5 billion dollars have been invested by other institutions and governments. At the country level, investment levels vary greatly, although they generally do not account for more that 1 percent of a country's GDP.[50]

3.2.2. Nicaragua's Background

Nicaragua is the second poorest country in the Latin America and Caribbean region. With a Gross National Income (GNI) of about $2.1 billion in 2000, and a population of about 5 million, the GNI per capita that year was $420. In addition to this low income level, growth has been elusive, with the average growth rate during the 1990's being 2.8 percent. As a result, Nicaragua has been unable to reach the per-capita output level that it had during the mid 1970's. The low income level is reflected in high

[50] See World Bank (2003).

national poverty and extreme poverty rates of 48 and 17 percent, respectively. In rural areas, the incidence of poverty reached 68 percent in 1998.[51]

In terms of its education profile, Nicaragua's outcomes are typical for countries in its income range. Some basic indicators are sufficient to give a clear picture of Nicaragua's educational needs: the illiteracy rate in the population older than 10 years is about 19 percent, reaching 30 percent in rural areas and 38 percent in the extremely poor segments of the population. The percentage of children aged 7 to 12 not attending school is 14 percent nationwide, and 20 and 32 percent in rural areas and in extremely poor households, respectively. Finally, the mean years of schooling in the population is 4.9 years. Once again great differences are observed between urban and rural areas, with the former reaching 6.2 years and the latter only 3.2. In extremely poor households the numbers are even lower, with an average 2.3 years of schooling completed.

3.2.3. Nicaragua's Emergency Social Investment Fund

After the implementation of a stabilization program led by the IMF in 1990, the government of Nicaragua decided to implement an Emergency Social Fund (Fondo de Inversion Social de Emergencia, FISE) that would mitigate the negative impacts of the structural adjustment and modernization programs. Initially, FISE started mostly as a temporary employment program and, through time, its role shifted to financing basic social and economic infrastructure.

FISE investments in social infrastructure include projects in areas such as social assistance, environment, municipal infrastructure, education infrastructure, health infrastructure, and water and sewerage infrastructure. As of the end of 1998, FISE had

[51] World Bank (2001).

invested U.S.$191.1 million in 6,017 projects, with 67% of total investments devoted to education infrastructure.

Although in absolute terms FISE investments might seem small, given the size of Nicaragua's economy, FISE is the largest Social Fund in relative terms in Latin America—and one of the largest in world—with a volume of resources of more than 1 percent of the country's GDP on average for the 1991-1998 period. In addition, during the same period FISE investments accounted for almost 11 percent of total public investment, and 40 percent of total investments in social infrastructure. In the education sector—the main focus of this study—FISE accounted for almost half of public investment, with an average investment per year of $11.2 million compared to the $11.7 invested by the Ministry of Education (The World Bank, 2000).

For the purpose of assessing FISE's performance it is important to understand the process through which projects to be funded were prepared and selected during the 1993-1997 period. The way in which the project selection process was carried out—and therefore to some extent the participants' selection process—determined the scope and possibility of selection bias into the program.

The process of project selection begins with FISE's assignment of a fraction of resources to be invested in each municipality, based on the national poverty map that contains municipal poverty rates.[52] Given that its goal is to improve the quality and sustainability of priority social infrastructure in poor areas in accordance with community needs, in theory, FISE should have assigned more resources to poor communities. According to World Bank (2000) and Pradhan and Rawlings (2002) FISE investments are

[52] I have been unable to find more detailed information on how resources were assigned. There is no written document explaining the process,, and officials in different branches on the Nicaraguan government that I have contacted have not been able to provide that information.

progressive since the share of resources received by 'extremely poor' and 'poor' municipalities is slightly larger than their share of the population. However, FISE investments are not solely concentrated on extremely poor and poor municipalities, and most municipalities in the country have been assigned at least one project.

The process continues with FISE inviting communities to submit projects to obtain funds for their primary schools. Different types of institutions are allowed to participate, and most projects have been submitted by municipalities, the ministry of education and NGOs. Finally FISE chooses a number of these projects per municipality, which receive the resources previously assigned to each municipality.[53]

3.3. Methodology and Data

As commonly described, the project evaluation problem arises as a result of the inability to observe outcomes for individuals that participated in the assessed program had they not taken part in the program. In the present case, the question is what would have been the state of the education outcomes in the areas that received FISE funding had FISE not invested in those primary schools.

In order to answer that question, data on the outcomes for the treated population must be compared with appropriately constructed comparison groups whose outcomes estimate the unobserved counterfactual. In this section I will formalize the evaluation problem and describe the choice of estimation methods to be used in subsequent sections.

[53]Here again I have been unable to obtain information on how projects within a municipality were ranked and selected.

3.3.1. Methodology

Let 1 and 0 denote the states associated with belonging to the group affected by the program being evaluated (treatment) and to the group left out of the program (comparison), respectively.[54] In the particular case of FISE investments in primary schools, let D=1 for children in schools that received FISE funds and D=0 for children in non-FISE schools. Y_1 and Y_0 are the outcomes conditional on participation and non-participation in FISE, respectively. Thus, for any child, the observed outcomes can be represented by

$$Y = DY_1 + (1-D)Y_0 . \tag{3.1}$$

As part of the evaluation of FISE investments, we could study the impact of living in an area of influence of a FISE school on educational outcomes by estimating

$$\alpha = Y_1 - Y_0 . \tag{3.2}$$

For each child, only Y_1 or Y_0 is observed. Heckman, Ichimura and Todd (1997) and others have described this as a missing data problem, and therein lays the core of the evaluation problem, as we need to construct a group of non-participants that can be compared to participants to estimate the impact of the program.

There are different parameters that can be estimated when evaluating a program. In this paper I will concentrate on the estimation of the mean impact of treatment on the treated (α^{TT}), which can be generally described as:

$$\alpha^{TT} = E(\Delta \mid D=1) = E(Y_1 \mid D=1) - E(Y_0 \mid D=1), \tag{3.3}$$

[54] This section follows Heckman, LaLonde and Smith (1999), Heckman, Ichimura and Todd (1997) and Smith and Todd (2003).

where $E(Y_1 \mid D = 1)$ is the mean of the outcome under study for participants, and $E(Y_0 \mid D = 1)$ is the mean of the outcome of participants had they not participated in the program. The impossibility of observing both terms of (3.3) simultaneously is central in the evaluation problem.

In addition, given that the selection process of projects funded by FISE is not random, correct estimates of the impact of FISE investments cannot be obtained by direct comparison of the average outcome between communities that benefited by FISE projects with others that did not (Ravallion, 2001).[55] Instead, when comparing means of treatment and comparison samples the estimated impact is not (3.3) but rather:

$$\tilde{\alpha} = E(Y_1 \mid D = 1) - E(Y_0 \mid D = 0) . \quad (3.4)$$

The second term may be a poor approximation to what the outcomes of those in the program would have been had they not participated in it. Equations (3.3) and (3.4) are different, with the difference being the selection bias,

$$\text{Selection Bias} = \alpha^{TT} - \tilde{\alpha} = E(Y_0 \mid D = 0) - E(Y_0 \mid D = 1). \quad (3.5)$$

The key to solving the evaluation problem in the context of this paper is to construct an appropriate counterfactual that takes into account the possibility of non-random selection in the program. In order to generate this counterfactual for the treatment group I use two different methodologies based on selection on observables to estimate the average impact on the treated. The choice of estimators based on selection on observables is determined completely by the availability of data. Because data in the pre-estimation period are not available, difference-in-difference methods that control for time invariant unobservables cannot be estimated. Although the use of estimates based on observables

[55] This was one of the approaches followed by Pradhan and Rawlings (2002).

limits the robustness of the estimates it is the only choice available, which highlights the need for baseline data for program evaluation.

Write the outcomes as functions of observables (X) and unobservables (U_0, U_1):

$$Y_j = \phi(X_j) + U_j \,, \tag{3.6}$$

where j takes the value 1 for the treatment group and 0 for the comparison group, ϕ takes a linear form such that $\phi(X_j) = X\beta_j$ with K regressors and one intercept. Thus, the effect of treatment on the treated can be described as:

$$\alpha^{TT} = E(\alpha \mid X, D = 1) = X(\beta_1 - \beta_0) + E(U_1 - U_0 \mid X, D = 1) \,. \tag{3.7}$$

Assume in addition that $\beta_{1k} = \beta_{0k}$ for all regressors except for the intercept. Under these assumptions α^{TT} can be estimated with a regression that can be written as:

$$Y = X\beta + \alpha D + U \,, \tag{3.8}$$

where $E(U_0 \mid X, D)=0$. The estimated parameter for α is the average effect of treatment on the treated. The advantage of estimating (3.8) in the context of this paper comes mainly from the availability of data, as only cross-section estimators can be used.

The second estimation procedure used in this paper is a propensity score matching technique, which also provides a method to estimate the counterfactual in cases were an experimental control group is not available, as in the FISE intervention case. Propensity score matching estimates (3.3) by assuming that there exists a set of observable conditioning variables Z,[56] such that Y_0 and D are independent conditioning on Z. In notation,

$$Y_0 \perp\!\!\!\perp D|Z, \tag{3.9}$$

[56] Z can be a subset or a superset of X.

where "$\perp\!\!\!\perp$" denotes independence. This is called the conditional independence assumption (CIA) in the literature. If (3.9) holds, data on non-participants can be used to estimate the outcomes of participants had they not participated in the program. For the empirical implementation of matching, it is also necessary to assume that for all Z on which we are conditioning, there are non-participants who can be compared to participants. This assumption can be described as

$$\Pr(D=1|\, Z) < 1, \tag{3.10}$$

and is called the "support condition" in the literature. With (3.9) and (3.10), matching generates a comparison group for which, conditional on Z, the distribution of the counterfactual outcome Y_0 for participants is the same as the observed distribution of Y_0 for the comparison group.

Conditional on Z, the estimated parameter of interest—treatment on the treated—can be expressed as

$$\alpha^{TT} = E(\alpha \mid Z, D=1) = E(Y_1 \mid Z, D=1) - E(Y_0 \mid Z, D=1)\ . \tag{3.11}$$

Under (3.9) and (3.10), and as long as means exist, we have that $E(Y_0 \mid Z, D=1) = E(Y_0 \mid Z, D=0)$. Thus, conditional on Z, the bias is zero. As in the regression model, assumption (3.9) implies that selection into the program is based on observables, but as pointed out by Smith (2000) it has the advantage when compared to the regression approach of not imposing a linear functional form. An additional advantage is that matching highlights what is called the "support" problem. This problem arises when the distributions of Z are significantly different between the treatment and the comparison groups. In that case, the linear regression approach achieves comparability by extrapolating into regions were data are sparse or non-existent.

This problem arises when there are values of Z for which there are observations in one group but not in the other. In that case, the linear regression approach achieves comparability by extrapolating into regions were data are sparse or non-existent.

When Z is high dimensional, comparison can be difficult. However, Rosenbaum and Rubin (1983) show that if (3.9) and (3.10) hold then

$$Y_0 \perp\!\!\!\perp D \mid P(Z), \tag{3.12}$$

which means that instead of conditioning on Z, we can condition on the probability of participation or "propensity score" P(Z) = Pr(D=1 | Z).

Propensity score matching attempts to find a comparable set of non-participants to each participant in the program by conditioning participation on Z. Then, a consistent estimate of program impact is obtained by taking differences between the outcomes of participants and their corresponding matches. The general formula for matching estimators is given by:

$$\hat{\alpha}^{M} = \frac{1}{n_1} \sum_{i \in I_1 \cap S_p} [Y_{1i} - \sum_{j \in I_0} W(i, j)\, Y_{oj}], \tag{3.13}$$

where I_1 and I_0 are the set of program participants and non-participants, respectively; S_p is the region of common support, n_1 is the number of children belonging to I_1 and S_p, and W(i,j) are weights given to non-participant children. Equation (3.13) answers the question of what would be the average of the individual gains from program participation had individuals not participated in the program. In order to obtain that measure, the estimator takes the average of differences between the outcome of participant children (Y_{1i}) and the average weighted outcome of a set of comparable non-

participant children ($\sum W(i, j)Y_{oj}$), where the weights depend on the particular matching estimation procedure utilized in the analysis.

The matching estimator reported in this paper corresponds to single nearest-neighbor matching with replacement. For this estimator, all the weights W(i,j) are equal to zero or one, with one being assigned to the individual in the comparison group with the closest estimated probability of participation P(Z) to each participant. Typically P(Z) is estimated either through a probit or logit model.[57]

3.3.2. Data

The data used in this paper were collected by the World Bank, which followed a school-level matched comparison design approach based on geographical proximity and other similarities between FISE and non-FISE schools.

To generate the school-level matching comparison group, they first selected randomly a number of FISE funded education projects, i.e., they selected a group of schools that had received funds during the 1994-1997 period. The number of FISE schools consisted of 24 out of a total of 724 primary (public) schools that benefited from FISE projects.[58] Next, they matched each primary school with its "nearest non-FISE facility, with the match restricted to facilities of similar size and type" (The World Bank, 2000).

Subsequently, a random sample of households in the area of influence of each school was selected. The area of influence of primary schools was determined by the

[57] In addition, I constructed matching estimates using multiple nearest neighbors and kernel matching. See Appendix 7 for details on these matching estimators and Smith and Todd (2003) for a more thorough discussion.
[58] In 1991, the total number of primary (public) schools in Nicaragua was 5,393.

World Bank in consultation with FISE engineers and staff from the Education Ministry, and it was defined as a radius of 500 meters (0.31 miles) around urban schools, and a radius of 3 kilometers (1.86 miles) for rural schools. For each school 10 households within the area of influence were selected randomly and surveyed using the Living Standard Measurement Survey (LSMS) questionnaire fielded in 1998 in Nicaragua.[59] The final sample was composed of 441 households, with 240 related to FISE schools.

The outcome measures that I study are primary school enrollment, education gap, children in correct grade for age, absenteeism, age in first grade, grade repetition, and highest grade completed.

The estimation methods used in this study differ from those used by Pradhan and Rawlings (2002). They claim that unbiased estimates of the average treatment effect of FISE interventions on the treated population can be obtained by comparing means between the treatment and comparison groups, as the sample design should have eliminated differences in observable and unobservable individual (and household) characteristics correlated with both the treatment status and the outcome under study. In other words, they assume that individuals in the treatment and comparison groups are not significantly different in their underlying characteristics, so that a simple comparison of group means consistently estimates the treatment effect.

However, the treatment and comparison groups may still have significant differences in their characteristics even if the school-level matching was done correctly. If the treatment and comparison groups are significantly different, and those differences are correlated with whether or not the school receives FISE funds and with the outcomes

[59] Households directly benefiting from the FISE and non-FISE schools—with benefiting meaning having at least one child of the household enrolled in the school—were oversampled to ensure a representative number of direct beneficiaries. Throughout the analysis weights are use to reflect the sampling procedure.

under study, then a selection bias may arise in the World Bank's estimates. In other words if the treatment and comparison groups are different, any differences in outcomes may result from differences in observables rather than from the effect of treatment.

In turn, a closer look to the comparison group school-level matching indicates that there are significant differences between the FISE schools and the matched comparison schools.[60] For instance, for only 3 pairs of schools out of 24 is the number of classrooms in the pre-treatment year (1993) the same (see Table A.6.2). Also, although FISE investments were mostly intended to improve existing infrastructure, available data indicate that in practice they increased the functional size of schools, as it is likely that schools that had not previously been used because of their poor conditions were utilized after completion of FISE projects. As a consequence, the estimation methodology must take into account the fact that the World Bank was not able to find exact matches for each school in the FISE sample.

In light of these considerations I propose the use of regression and propensity score matching to estimate the impact of FISE investments. These techniques are discussed in the following two sections.

The sample is composed of children aged 6 to 14. Its size varies with the availability of data and the type of outcome. For instance for enrollment the sample size is 927 and for age in first grade it is 224. Here it is important to point out that the sample I will use is different from the World Bank sample. They only include children aged 7 to 12 in their analysis. Even though 7 to 12 are the ages at which children are expected to be enrolled in primary schools in Nicaragua, many children aged 6, 13 and 14 attend

[60] See Appendix 6.

primary schools.[61] Including them increases the sample by 33% and by itself makes the two samples different for all outcome variables. There are other differences related to possible measurement error problems that they do not address in their sample.[62] I order to test for robustness I will also discuss the results I obtained using the World Bank sample.

3.4. A Regression Approach using the School-Level Matched Comparison Group

In this section I will follow a regression approach that incorporates covariates to control for differences in the underlying characteristics of the treatment and comparison groups, and the school-level matching process. The initial model to be estimated is as follows:

$$\text{Outcome}_i = \phi + \alpha \, \text{D}_i + \varepsilon_i, \tag{4.1}$$

where D_i is a dummy variable for treatment status that takes a value of one in the case of the treatment group and a value of zero for the comparison group. Because of the matching procedure followed to construct the comparison group I will also run a set of regressions that include school pair-specific dummies:

$$\text{Outcome}_i = \phi + \alpha \, \text{D}_i + \delta_j \sum_j \text{School Pair}_{ij} + \varepsilon_i, \tag{4.2}$$

where SchoolPair_{ij} is a dummy variable equal to one if the *i*th child is in the area of influence of one the schools in the *j*th school pair.

Owing to the availability of data on individual, household, and some school characteristics, it is possible to include in the regression equation variables that might affect the outcomes under analysis. I introduce a set of socio-economic and demographic

[61] 62% of children aged 6 years who do not attend pre-school are enrolled in primary school. The same proportion of children aged 13 and 14 who do not attend secondary school are enrolled in primary school.
[62] See Appendix 5 for details on the construction of the sample I use and a description of all variables discussed throughout the paper. See Appendix 8 for details on the World Bank sample.

characteristics of households and children. Following Haveman and Wolfe (1995) and Kruger (2003) I first selected a series of demographic variables. These variables are the number of children aged 0-5 living in the household, size of the family, and whether the child is female.

Next, I included two geographical variables: the time to reach the nearest primary school, and whether the child lives in a rural household. The household-level socio-economic variables used in the analysis are the following: the highest level of education among adults in the household; whether the mother lives in household, whether the mother works, and whether the head of household is female. I also include variables for total household income, a dummy variable for whether the household owns the house they are living in, and whether the household is poor or extremely poor.[63] To control for school characteristics I include the number of classrooms in 1993, the year before FISE started to fund primary schools. Finally, I include a set of age-specific dummies. The complete model can be expressed as follows:

$$\text{Outcome}_i = \phi + \alpha \text{D}_i + \text{X}_i\Phi + \lambda_k \sum_k \text{Age}_{ik} + \delta_j \sum_j \text{SchoolPair}_{ij} + \varepsilon_i, \qquad (4.3)$$

where X_i is a vector containing all the socio-economic, demographic and school covariates other than age and the school pair dummies, and Age_{ik} is a dummy variable indicating whether the *i*th child is in the *k*th age group.

Age is a particularly important variable in this context. As some education outcomes have improved over time in Nicaragua, there is a correlation between age and the outcome being analyzed, which needs to be taken into account if both groups—treatment and comparison—do not have the same age structure. For instance, Nicaragua

[63] Appendix 5 describes each of these variables in detail.

has experienced a general increase in the net enrollment rate (Arcia, 2001), which is higher in more recent cohorts of children. Thus, if the treatment group is on average younger than the comparison group, it might have higher enrollment rates because of its age structure rather than as a result of FISE's intervention.

Table 15 reports the summary statistics for the outcome and control variables used to estimate the model. The enrollment rate in the sample is 86 percent, however only 43 percent of the children are enrolled in the corresponding grade for their age. The average education gap is 1.5 years and the average age to enter primary school for the first time is 7.7 years. In terms of child-specific, household and school characteristics, 66 percent of children live in rural areas, 42 percent have mothers that work, and females head 26 percent of households. Poverty is prevalent: 61 percent of the households are considered poor and 26 percent fall in the extremely poor category.

Before presenting results from the regression analysis, Table 16 the reports results of tests of differences in means between the FISE (treatment) and Non-FISE (comparison) groups for these variables. The table reports the number of observations and the mean of the variable for each group, as well as the probability values of the tests.

The tests in Table 16 indicate that six of these variables are significantly different between the two groups. Specifically, the time to reach the nearest school and the proportion of rural households are significantly smaller in the treatment group. Of particular interest are the results coming from the economic variables, which indicate, on average, that the treatment group is economically better off than the comparison group. The proportion of mothers working, total income, and the proportion of households

owning the house in which they live are larger; and the proportion of extremely poor households is lower.

The general picture arising from this table is that children in the treatment group have easier access to schools (they live closer and in urban areas) and they are better off in terms of income and housing conditions. As some of these household characteristics are correlated with educational outcomes in Nicaragua (Kruger, 2003) it is necessary to include them to obtain a consistent estimate of the impact of FISE investments.

3.4.1. Regression Results

All of my regression results are obtained using a sample that is different from the one used by Pradhan and Rawlings (2002) and World Bank (2000) and I report them in Tables 17–20. Even though I consider my sample to be the most appropriate for the estimation of FISE's impact, as it includes a larger number of children that are attending primary school and as it deals in more detail with possible measurement error problems, as a robustness check I estimated the same regressions presented in Tables 17–20 using the World Bank sample; these results appear Appendix 8 (Tables A.8.1 through A.8.4).

Table 17 reports the results of the regressions for enrollment and education gap. For each outcome, the first column reports the estimates of a regression of the outcome on a treatment status dummy variable (equation 4.1). This is equivalent to estimating the difference in means between the treatment and comparison group with the difference being the estimated coefficient on the treatment variable (FISE school), as in Pradhan and Rawlings (2002) and World Bank (2000). The second column includes school-pair dummy variables to control for systematic differences across pairs of schools due to the

school-level matching procedure (equation 4.2). Finally, the third column adds the socio-economic, demographic and school variables described above (equation 4.3).[64]

For enrollment it is interesting to note that the estimated treatment effect decreases to about half of that obtained reported by Pradhan and Rawlings (2002) and World Bank (2000), making the estimated impact statistically insignificant (column 1). When school-pair dummies variables are included the point estimate falls almost to zero and remains statistically insignificant (column 2). Introducing socio-economic and demographic also lowers the estimated impact to less than half of the one estimated with the World Bank sample, even though it also reduces the standard errors (column 3). *F*-statistics for the joint significance of the school-pair and age dummies reported at the bottom of the table indicate that they are jointly significant, suggesting that both the school-level matching process and the age structure of the sample need to be taken into account in order to obtain consistent estimates of FISE impacts.

The socio-economic covariates are in line with what is expected in the empirical literature on education outcomes. Children living farther away from schools and in rural areas are less likely to be enrolled. The level of education of the adults in the household affects enrollment positively, as well as whether the mother works and whether the household owns their home. Also, children living in extremely poor households have a lower probability of being enrolled in school.

Columns 4 through 6 display results for the education gap.[65] The sample is smaller as the education gap is defined as zero for children aged 6 and 7 regardless of their enrollment status, so that these estimates use a sample of children aged 8-14. The

[64] These correspond to the same column numbering in Table A.8.1 in Appendix 8.
[65] Cf. footnote 64.

treatment status variable is not statistically significant for any of the models as the point estimates are reduced to about half of those obtained with the World Bank sample; its sign even changes from negative to positive and then back to negative, indicating that FISE investments have no consistent and significant impact on the education gap. In addition, time to school increases the education gap, while the maximum years of education among adults in the household and home ownership reduce it. The results also indicate that girls have a smaller education gap. This finding is consistent with the literature on educational attainment in Nicaragua which finds that girls tend to have better educational outcomes in primary schools than boys (Arcia 2000; Kruger 2003; Hill and King 1993).

The result for the education gap is consistent with that for enrollment because the education gap is indirectly related to enrollment. The education gap is obtained by taking the current age of the child minus seven years, and then subtracting his or her highest grade completed. Thus, a larger education gap is observed the later the child enrolls in school for the first time, and the slower the progress the child has made while enrolled. Therefore the null impact on the education gap is consistent with FISE investments not affecting enrollment.

The age dummies indicate an increasing education gap with age. The average increase in the education gap is 0.56 years per year of age. The increasing education gap could be explained by the fact that in each cohort there is a proportion of children that have never been enrolled in school. These children have, by definition, a larger education gap as they get older, and, even with a constant proportion of this type of child, older cohorts will have larger education gaps. Increasing education gaps are also explained by

grade repetition, because every time that a child repeats a year she increases her education gap. Thus, it is natural to observe larger education gaps as age increases.

Table 18 displays results for the percentage of children in the correct grade and absenteeism.[66] In the former the sample is restricted to children aged 8 through 12.[67] The results in column 1 indicate that even though the point estimate of the treatment effect is larger than the one reported in Appendix 8 (Table A.8.2), a higher standard error produces a statistically insignificant impact. When I control for school-pair dummies and socio-economic characteristics (columns 2 and 3), the impact of FISE investments is reduced and remains statistically insignificant.

Columns 4 through 6 report regression results for days not attending school, showing that they have not been significantly reduced by FISE interventions. The point estimate reported in column 6 is a third of the one estimated with the World Bank sample. The results also indicate that for the most part, with respect to this outcome, there are no significant differences by children's age. Similar results are obtained for grade repetition and highest grade completed, which are reported in Table 19; in both cases FISE interventions do not have a statistically significant impact, mainly because of noticeably smaller point estimates when compared with the results in Appendix 8 (Table A.8.3). The education level of adults in the household and wealth have positive effects on the highest grade completed, and it is also the case that girls have higher educational attainment than boys.

Finally, Table 20 presents regression results for the age at which children are enrolled in first grade. The point estimate of the effect of treatment status in column 1

[66] The estimates for these outcomes using the World Bank sample are found in Appendix 8, Table A.8.2.
[67] Children aged 6 and 7 are always in the correct grade for their age, while 13 and 14 year-olds never are.

indicates that FISE might have reduced the age at which children enroll in first grade. However, once school-pair dummies are included the impact disappears, and remains statistically insignificant when socio-economic and demographic variables are included. Again, this is the result of smaller point estimates, not of larger standard errors. In the last case, the only socio-economic determinant of the age at which children are enrolled in school seems to be the education of the adults in the household. A possible concern with the use of this variable is the possibility that even though no statistically significant effect is observed in reducing the age at which children are enrolled, FISE might be succeeding in increasing first time enrollment among children that would not have otherwise enrolled, even if they are older when they enroll for the first time. The fact that this variable uses information only on children actually enrolled allows for this possibility. However, given that no impact was observed in the enrollment variable, it is unlikely that this issue is substantively important in this particular data set.

The regression results using my sample and the one used by the World Bank (2000) are similar; however, two comments are worth mentioning: first, simply applying a method that is robust to the presence of heteroskedasticity, such as a regression with robust standard errors, eliminates the statistical significance of all but one of the significant results of the World Bank sample. Second, when other control variables are included, such as school-pair dummies and economic and demographic characteristics, the standard errors remain similar but the point estimates fall considerably, eliminating the statistical significance of all results for the World Bank sample.

The conclusions of the regression analysis are clear: once an appropriate sample is used, the variables are corrected for measurement problems, heteroskedasticity-consistent

standard errors are reported, and I control for the school-level matching procedure, and school, socio-economic, and demographic characteristics of the children and their households, no statistically significant impact of FISE is found on any of the seven outcomes studied here.

It is possible that the lack of significance in the estimates is the result of a small sample. Power calculations based on the existing impact data for enrollment indicate that robust estimates of FISE's treatment effect (at the 90% confidence level) would require a sample size at least double the size of the present one.[68]

3.5. A Propensity Score Matching Approach using the Matched Comparison Group

In this section I implement the propensity score matching method discussed in section 2. I estimate the probability of participation—in this case living in the area of influence of a school that received FISE funds—or propensity score P(Z)=Pr(D=1 | Z), using a probit model that includes the following as conditioning variables that affect both program participation and outcomes: age, a dummy for female children, the number of projects not funded by FISE that the household reports having benefited from, the number of projects not funded by FISE that the household reports having benefited from

[68] In order to estimate the impact of FISE one can calculate a t-statistic test of the following form:

$$T_v = \frac{(\bar{y}_t - \bar{y}_u) - (\mu_t - \mu_u)}{\sqrt{(S_t^2 / n_t) + (S_u^2 / n_u)}}$$

where $\bar{y}_i$ is the observed mean of the outcome, μ_i is the expected value of the outcome, S_i^2 is the sample variance, and n_i is the sample size, where i = treated (t) and untreated (u) groups, and v are the degrees of freedom. Using data from the sample to obtain $(\bar{y}_t - \bar{y}_u)$ and $(S_t^2 / n_t) + (S_u^2 / n_u)$, and different values for the true impact $(\mu_t - \mu_u)$ of FISE, it is possible to estimate different sample sizes that would generate significant estimates (at various significance levels).

and participated in, the number of organizations that a household belongs to, a dummy for whether the access to the home is through a dirt road, the time (in minutes) to reach the nearest primary school, the education and the income level of the household, a dummy variable for whether the household owns their home, dummies for whether the household is poor or extremely poor, the rate of poverty at the municipality level, the rate of extreme poverty at the municipality level, the number of classrooms available in the school in 1993, and municipality dummies. In addition, to provide more flexibility, interaction terms between age and the municipal poverty rate, and the other control variables were included.

These variables are the best available to simultaneously capture the selection process and the outcome levels. Variables such as municipal poverty rates and school conditions are included to control for the way in which funds are assigned at the municipality level, and how projects within them are selected. Participation variables, such as the number of non-FISE projects in which the household participated and benefited from, are included because they provide complementary information on the ability of communities to attract public infrastructure, thus they also help determine whether a community might receive funding for their schools from FISE. Communities in which people have benefited from other projects tend to receive fewer funds from FISE, as they might be better off in general. Finally, individual and household characteristics are included as determinants of the outcomes. The imperfect information that I was able to obtain about the project selection process limits my ability to estimate the probability of participation. In order for future evaluations to improve upon existing ones, FISE

should release precise information about the procedures used to assign funds at the municipality level, and on the mechanism to assign funds within each municipality.

Table 21 reports the estimates of the probit model of participation.[69] The results indicate that children living in households that participated in and were benefited from other community projects are less likely to live in the areas of influence of the FISE schools, while households with poorer access (dirt roads) are more likely to be within their area of influence. Unexpectedly, more educated households also have higher (conditional) probabilities of being in areas benefited by FISE. Finally, children in poorer communities, as reflected by their rate of poverty, are more likely to live near schools that received FISE funding. However, this result does not appear for the incidence of extreme poverty, indicating that FISE investments might have been well targeted to poor communities but not to extremely poor communities.

Following Rosenbaum and Rubin (1985) I performed a balancing test, which is used to determine whether or not there are differences between the treatment and matched comparison groups after conditioning on P(Z). The idea is that individuals with similar estimated propensity scores in the treatment and comparison groups should have similar values of the Z variables. If that is not the case the model is mis-specified. To operationalize the test, I performed a Hotelling T^2 test, where the joint null hypothesis is equality of means in a set of variables. I apply the test to the Z variables included in the estimation of the propensity score, within different strata defined by P(Z). The strata are defined as 13 n-tiles of estimated P(Z) in the treatment group (D=1).

[69] Table 21 does not report the municipality fixed effects and interaction terms, although they were included in the estimation.

The results indicate that in 12 out of 13 strata, the means of all of the Z variables are not significantly different between the D=1 and D=0 groups at any conventional level of significance. For the remaining strata, I cannot reject the null hypothesis that the means of Z are not different between those groups at the ninety-five percent level. These results indicate that the model used to estimated the probability of participation does relatively well in balancing the covariates.

The densities of the estimated probabilities of participation for the treatment and matched comparison groups obtained from the probit model are presented in Figure 1, which permits an easy evaluation of possible support problems. The figure shows that for all of the strata defined by the estimated probability of participation there exists at least one child in the comparison group with similar characteristics to those in the treatment group. As expected, the ratio of participants to non-participants decreases with the probability of participation, but the data still allowsfor the existence of comparable children at every level of participation.

The propensity score matching estimates of the impact of FISE investments on all seven educational outcomes are reported in Table 22. The first column reports the number of children in the treated group (as the matching procedures finds a matching child for each of them). The second and third columns present the average value of the outcome in the treated and matched comparison groups, respectively, with the fifth column displaying the difference in means. The next column reports bootstrapped standard errors based on 750 repetitions. The results indicate that, as in the case of the regression analysis, even though the estimates point towards the existence of positive impacts of FISE investment, they are not statistically significant for any of the outcomes.

In addition, I performed matching estimations using multiple nearest neighbors and kernel matching, as well as alternative specifications for the propensity score, and obtained similar results (see Appendix 7). The propensity score estimates reinforce those obtained with a regression approach in the previous section, in that they indicate that the education outcomes that can be measured in Nicaragua at the present time have not been affected by FISE investments.

3.6. Conclusions

This study contributes to and improves the existing literature in the area of program evaluation, and in particular of the evaluation of social funds, by taking a closer look at the impacts that the existing social fund operating in Nicaragua (FISE) has on educational outcomes. Previous estimates of the Nicaraguan case carried out by The World Bank (2000) and Pradhan and Rawlings (2002) point towards significantly positive effects on several key educational outcomes related to quantity of education: enrollment, the education gap, and the age at which children are enrolled in first grade.

These studies compared the average outcomes of a sample of children living under the area of influence of schools benefited by FISE projects with the average outcomes of a group of children living in the area of influence of non-FISE schools. The assumption behind that comparison is that the matched school sample design carried out in the process of collecting the data eliminated any sample selection bias associated with the non-random nature of the allocation process of FISE projects.

The present study shows that their sample, their definition of variables and the method they use to estimate the impact of FISE investments have serious shortcomings.

First, the sample they include in the analysis is not appropriate for the Nicaraguan reality. I generated a sample that is one third larger by including children aged 6 to 14; at the same time I defined some of the variables in ways that account for measurement problems, and finally and most importantly, I applied heteroskedastic-robust estimators that account for sample selection bias to estimate the impact of FISE investments on the educational outcomes. The methods that I use are regression analysis and propensity score matching. These two methods overcome the selection bias problem that is not accounted for in World Bank estimates that simply take differences in means between the treated and comparison groups.

The results I obtain in the regression analysis and propensity score matching indicate that none of the outcomes has improved significantly for children living in communities with schools that benefited from FISE investments compared to children living in communities that did not. One implication of these results is that the estimation method matters. Applying incorrect methods, such as simple differences in means in this case, is likely to generate biased estimates. The message here is that special attention should be paid to the selection of the methodology used to obtain estimates.

In addition, the power calculations I performed indicate that larger samples might be necessary to obtain statistically significant estimates, given the likely effects of sizes in the population. The choice of the sample size has important implications for the design of project evaluations in developing countries. Given the large amount of resources involved in project evaluation, agencies providing the resources—in this case the Nicaraguan government—would obtain better returns from their assessment projects if

they were to select more carefully the methodology for the analysis, and to make sure beforehand that they would have enough data to produce robust estimates.

Overall, the impact estimations that I conducted suggest that previous World Bank estimates are incorrect and irrelevant. In addition, they also suggest that the impact of primary school infrastructure improvements is less important than is thought to be on the selected outcomes. Although school infrastructure might be important for some educational outcomes, the evidence reported here on the Nicaraguan case indicates that relevant outcomes such as enrollment, attendance or grade repetition were not affected by existence and operation of FISE.

TABLES

Table 1. Notifications to GATT and WTO of of Regional Integration Agreements, 1949-1998

Period	Number
1949-1959	9
1960-1969	17
1970-1979	42
1980-1989	13
1990-1998	87
Total	168

Source: WTO, in World Bank (2000).

Table 2. Summary Statistics (1960-1999 averages)

	Mean	Std. Dev	Minimum	Maximum
Growth	1.96	1.55	-1.64	6.48
Belongs to an RIA	0.60	0.49	0.00	1.00
RIA variable	6.29	9.50	0.00	30.38
Relative RIA variable	83.44	326.69	0.00	2309.30
Excluded RIA variable	7.35	0.52	4.70	7.52
Log Initial GDP p.c.	7.50	0.85	5.91	9.20
Government Consumption	14.11	4.57	6.94	30.40
Black Market Premium	59.00	197.91	0.00	1619.21
Investment Rate	21.97	5.35	11.44	34.57
Foreign Direct Investment	1.27	1.13	0.04	7.54
Manufactured Exports	35.12	26.60	0.62	94.08
Human Capital	0.76	0.87	0.01	3.70
Terms of Trade	-0.09	1.40	-2.97	5.02
Trade Share of GDP	59.10	40.37	14.79	331.02
Democracy	1.74	6.40	-8.35	10.00
Trading Partners' Growth	2.20	0.48	1.34	3.84
Bordering countries Share of WGDP	3.22	6.42	0.00	36.00
Trading Partners' Investment Rate	22.17	1.38	20.24	26.70
Ethnolinguistic Fractionalization	38.81	29.64	0.00	90.00
Population over 65	5.27	2.85	1.87	12.04

Number of observations: 81

Table 3. Summary Statistics Growth and RIA Variables (decade averages)

	Decade	Num. Obsv.	Mean	Std. Dev	Minimum	Maximum
Growth[a]	(60-69)	63	2.85	2.12	-3.18	9.48
	(70-79)	85	2.85	2.16	-3.12	9.25
	(80-89)	81	0.74	2.22	-5.11	6.26
	(90-99)	78	1.59	1.78	-3.05	6.77
RIA Variable[b]	(60-69)	63	1.74	4.34	0.00	14.26
	(70-79)	85	4.52	8.46	0.00	26.17
	(80-89)	81	7.64	12.61	0.00	40.37
	(90-99)	78	10.80	15.12	0.00	68.84
Relative RIA Variable[c]	(60-69)	85	72.77	334.42	0.00	2493.88
	(70-79)	63	2.53	7.43	0.00	39.23
	(80-89)	81	114.47	461.23	0.00	3177.25
	(90-99)	78	141.51	520.44	0.00	3564.59

[a]: Percentage points. [b]: Percentage of World GDP. [c]: Number of Times.

Table 4. Correlations for Growth and RIA Variables (decade averages)

		Growth Rate			
	Decade	(60-69)	(70-79)	(80-89)	(90-99)
RIA Variable[b]	(60-69)	0.21			
	(70-79)		0.16		
	(80-89)			0.36	
	(90-99)				0.12
Relative RIA Variable[c]	(60-69)	0.15			
	(70-79)		0.31		
	(80-89)			0.23	
	(90-99)				0.12

Number of observations varies with each decade.

Table 5. Cross-Country Growth Regressions, 1960-1999 [a] (40-year averages)

	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
Log Initial GDP p.c.	-0.541 *	-0.519 *	-0.693 ***	-0.704 ***	-0.472	-0.445	-0.640 **	-0.656 **
	(0.300)	(0.289)	(0.256)	(0.253)	(0.295)	(0.283)	(0.262)	(0.260)
Government Consumption	-0.013	-0.005	-0.068 *	-0.072 *	-0.026	-0.019	-0.069 *	-0.073 *
	(0.029)	(0.032)	(0.039)	(0.040)	(0.029)	(0.030)	(0.039)	(0.039)
Black Market Premium	-4.0e-04	-4.4e-04	-3.1e-04	-3.2e-04	-4.4e-04	-4.9e-04	-3.2e-04	-3.3e-04
	(5.5e-04)	(4.7e-04)	(5.2e-04)	(4.6e-04)	(5.4e-04)	(4.6e-04)	(5.2e-04)	(4.5e-04)
Investment Rate	0.148 ***	0.134 ***	0.133 ***	0.122 ***	0.144 ***	0.131 ***	0.132 ***	0.120 ***
	(0.031)	(0.033)	(0.033)	(0.034)	(0.032)	(0.034)	(0.033)	(0.034)
Foreign Direct Investment	0.038	-0.005	0.221	0.225	0.110	0.079	0.232	0.236
	(0.187)	(0.196)	(0.175)	(0.178)	(0.182)	(0.186)	(0.176)	(0.180)
Manufactured Exports	0.017 **	0.017 **	0.012 *	0.011	0.018 ***	0.018 **	0.013 *	0.012
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Human Capital	0.006	-0.035	0.154	0.154	0.080	0.052	0.170	0.168
	(0.168)	(0.164)	(0.181)	(0.172)	(0.154)	(0.147)	(0.177)	(0.168)
Terms of Trade	-0.026	-0.038	-0.061	-0.076	-0.027	-0.040	-0.057	-0.074
	(0.120)	(0.117)	(0.105)	(0.102)	(0.116)	(0.113)	(0.105)	(0.101)
Trade Share of GDP	5.1e-03	6.3e-03	7.6e-04	7.0e-04	1.7e-03	2.3e-03	-2.9e-04	-2.6e-04
	(5.4e-03)	(5.7e-03)	(5.1e-03)	(5.3e-03)	(5.6e-03)	(5.8e-03)	(5.6e-03)	(5.8e-03)
Democracy	0.056	0.072	0.046	0.054	0.037	0.049	0.038	0.048
	(0.041)	(0.044)	(0.034)	(0.035)	(0.037)	(0.038)	(0.035)	(0.036)
Trading Partners' Growth	0.185	0.684	0.389	0.769 *	0.175	0.662	0.373	0.783 *
	(0.265)	(0.468)	(0.270)	(0.426)	(0.269)	(0.445)	(0.271)	(0.425)
Bordering countries Share of WGDP	0.030 **	0.029 *	0.024 *	0.023 *	0.026 *	0.025 *	0.022 *	0.021
	(0.015)	(0.015)	(0.013)	(0.012)	(0.014)	(0.014)	(0.013)	(0.013)
Belongs to an RIA	**-0.186**	**-0.252**						
	(0.302)	(0.329)						
RIA variable			**0.055** **	**0.065** ***			**0.049** **	**0.060** **
			(0.024)	(0.024)			(0.024)	(0.024)
Relative RIA variable					**6.3e-04** **	**6.5e-04** ***	**4.0e-04** *	**3.7e-04**
					(3.1e-04)	(3.1e-04)	(2.4e-04)	(2.4e-04)
Constant	1.514	0.475	2.957 *	2.486	1.205	0.132	2.663	2.182
	(1.960)	(1.865)	(1.647)	(1.564)	(1.886)	(1.836)	(1.647)	(1.565)
Observations	81	81	81	81	81	81	81	81
R-squared	0.6	0.58	0.63	0.62	0.61	0.59	0.64	0.63
F-statistic	9.2	9.18	10.32	10.53	9.71	9.63	14.47	15.48
F-statistic (excluded instrument)[b]		64.25 ***		67.09 ***		66.35 ***		67.58 ***
Partial R-squared (excluded instrument)[b]		0.49		0.50		0.50		0.51
Hausman Specification Test[b]		0.00		0.07		1.39		0.00
P-Value (Hausman Test)		1.00		1.00		1.00		1.00
F-statistic RIA variables[c]							4.12 **	5.40 ***

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

[a] Endogenous variable: trading partners' growth. Instrument: trading partners' investment rate.

[b] See text for explanation.

[c] Test of joint significance of the RIA variables.

Table 6a. Country Fixed and Random Effects Growth Regressions, 1960-1999 [a] (10-year averages)

	(1) FE	(2) RE	(3) FE	(4) RE	(5) FE	(6) RE	(7) FE	(8) RE
Log Initial GDP p.c.	-3.529 ***	-0.937 ***	-3.746 ***	-1.058 ***	-0.480 **	-3.669 ***	-3.798 ***	-1.064 ***
	(0.486)	(0.253)	(0.495)	(0.257)	(0.215)	(0.491)	(0.498)	(0.256)
Government Consumption	-0.033	-0.024	-0.040	-0.046 *	-0.030	-0.030	-0.037	-0.044 *
	(0.040)	(0.026)	(0.040)	(0.027)	(0.024)	(0.040)	(0.040)	(0.027)
Black Market Premium	-3.6e-04	-3.4e-04	-3.5e-04	-3.0e-04	-3.6e-04 *	-3.6e-04	-3.6e-04	-3.0e-04
	(2.5e-04)	(2.4e-04)	(2.5e-04)	(2.4e-04)	(2.0e-04)	(2.5e-04)	(2.5e-04)	(2.4e-04)
Investment Rate	0.151 ***	0.136 ***	0.161 ***	0.143 ***	0.127 ***	0.154 ***	0.161 ***	0.143 ***
	(0.026)	(0.021)	(0.027)	(0.021)	(0.021)	(0.026)	(0.027)	(0.021)
Foreign Direct Investment	0.306 ***	0.267 ***	0.306 ***	0.274 ***	0.199 **	0.326 ***	0.319 ***	0.291 ***
	(0.088)	(0.081)	(0.087)	(0.080)	(0.083)	(0.088)	(0.088)	(0.080)
Manufactured Exports	0.010	0.017 ***	0.010	0.014 **	0.019 ***	0.011	0.011	0.014 ***
	(0.010)	(0.006)	(0.010)	(0.006)	(0.005)	(0.010)	(0.010)	(0.006)
Human Capital	0.738	0.461 ***	0.660 **	0.443 **	0.205 *	0.721 ***	0.665 **	0.471 ***
	(0.268)	(0.174)	(0.269)	(0.174)	(0.120)	(0.266)	(0.268)	(0.174)
Terms of Trade	0.052 *	0.049 *	0.046 *	0.046 *	0.040	0.049 *	0.046 *	0.048 *
	(0.026)	(0.026)	(0.026)	(0.025)	(0.031)	(0.026)	(0.026)	(0.025)
Trade Share of GDP	-6.0e-03	-1.1e-03	-7.6e-03	-1.6e-03	-1.3e-03	-7.2e-03	-7.9e-03	-3.3e-03
	(1.0e-02)	(4.0e-03)	(1.0e-02)	(3.9e-03)	(3.6e-03)	(1.0e-02)	(1.0e-02)	(4.1e-03)
Democracy	-0.047 *	0.027	-0.052 *	0.027	0.030	-0.047 *	-0.051	0.026
	(0.027)	(0.021)	(0.027)	(0.021)	(0.023)	(0.027)	(0.027)	(0.021)
Trading Partners' Growth	0.138	0.399 *	0.126	0.430 *	1.045 ***	0.157	0.139	0.425 *
	(0.307)	(0.225)	(0.304)	(0.222)	(0.369)	(0.304)	(0.304)	(0.222)
Bordering countries Share of WGDP	-0.018	0.042	0.024	0.045 *	0.031 **	0.005	0.032	0.044 *
	(0.116)	(0.026)	(0.117)	(0.026)	(0.015)	(0.116)	(0.118)	(0.026)
Belongs to an RIA	**-0.090**	**-0.179**						
	(0.292)	(0.251)						
RIA variable			**0.033** *	**0.030** **			**0.026**	**0.022**
			(0.017)	(0.013)			(0.018)	(0.014)
Relative RIA variable					**6.6e-04** ***	**1.1e-03** *	**7.3e-04**	**5.6e-04**
					(2.3e-04)	(6.5e-04)	(7.0e-04)	(3.5e-04)
Constant	25.605 ***	5.067 ***	27.073 ***	6.007 ***	26.391 ***	5.488 ***	27.325 ***	6.081 ***
	(3.740)	(1.893)	(3.786)	(1.926)	(3.746)	(1.890)	(3.793)	(1.923)
Observations	307	307	307	307	307	307	307	307
R-squared	0.58	0.46	0.59	0.47	0.59	0.46	0.59	0.47
F/Wald -statistic	17.76	246.69	18.3	255.62	18.16	255.82	17.3	259.52
Hausman Specification Test [b]		175.96		223.45		147.97		140.60
P-Value (Hausman Test)		0.00		0.00		0.00		0.00
F/Chi2 -statistic RIA variables [c]							2.40 *	7.74 **

Robust standard errors in parentheses. Decade dummies not reported. * significant at 10%; ** significant at 5%; *** significant at 1%

[a] Endogenous variable: trading partners' growth. Instrument: trading partners' investment rate.

[b] See text for explanation.

[c] Test of joint significance of the RIA variables.

Table 6b. Instrumental Variables Country Fixed and Random Effects Growth Regressions, 1960-1999 [a] (10-year averages)

	(1) IV-FE	(2) IV-RE	(3) IV-FE	(4) IV-RE	(5) IV-FE	(6) IV-RE	(7) IV-FE	(8) IV-RE
Log Initial GDP p.c.	-3.077 ***	-0.637 ***	-3.196 ***	-0.719 ***	-0.991 ***	-3.222 ***	-3.273 ***	-0.717 ***
	(0.632)	(0.234)	(0.682)	(0.234)	(0.253)	(0.650)	(0.681)	(0.234)
Government Consumption	-0.074	-0.022	-0.086	-0.047 *	-0.031	-0.073	-0.081	-0.045 *
	(0.053)	(0.024)	(0.055)	(0.025)	(0.025)	(0.054)	(0.055)	(0.025)
Black Market Premium	5.0e-05	-3.4e-04	1.2e-04	-2.9e-04	-3.2e-04	8.0e-05	1.2e-04	-2.9e-04
	(3.6e-04)	(2.5e-04)	(3.8e-04)	(2.5e-04)	(2.4e-04)	(3.7e-04)	(3.8e-04)	(2.5e-04)
Investment Rate	0.157 ***	0.131 ***	0.166 ***	0.135 ***	0.140 ***	0.161 ***	0.166 ***	0.135 ***
	(0.032)	(0.020)	(0.035)	(0.020)	(0.020)	(0.033)	(0.035)	(0.020)
Foreign Direct Investment	0.254 **	0.218 ***	0.245 **	0.225 ***	0.292 ***	0.272 **	0.264 **	0.240 ***
	(0.110)	(0.083)	(0.116)	(0.083)	(0.081)	(0.113)	(0.116)	(0.084)
Manufactured Exports	0.007	0.019 ***	0.007	0.016 ***	0.016 ***	0.008	0.008	0.016 ***
	(0.013)	(0.005)	(0.013)	(0.005)	(0.005)	(0.013)	(0.013)	(0.005)
Human Capital	0.315	0.274 *	0.183	0.243	0.495 ***	0.266	0.192	0.269 *
	(0.386)	(0.162)	(0.404)	(0.159)	(0.174)	(0.399)	(0.403)	(0.160)
Terms of Trade	0.051	0.043	0.047	0.039	0.050 *	0.048	0.046	0.041
	(0.032)	(0.027)	(0.034)	(0.027)	(0.025)	(0.033)	(0.034)	(0.027)
Trade Share of GDP	5.7e-03	-3.3e-04	6.7e-03	-7.0e-04	-3.7e-03	5.7e-03	6.2e-03	-2.2e-03
	(1.4e-02)	(3.6e-03)	(1.5e-02)	(3.5e-03)	(4.1e-03)	(1.4e-02)	(1.5e-02)	(3.6e-03)
Democracy	-0.036	0.033	-0.039	0.03428*	0.025	-0.036	-0.038	0.032
	(0.034)	(0.021)	(0.035)	(0.021)	(0.021)	(0.034)	(0.036)	(0.021)
Trading Partners' Growth	3.168 **	0.981 **	3.642 **	1.148 ***	0.415 *	3.399 **	3.650 **	1.167 ***
	(1.527)	(0.407)	(1.576)	(0.400)	(0.222)	(1.618)	(1.577)	(0.399)
Bordering countries Share of WGDP	0.401	0.037 *	0.507 *	0.038 *	0.042	0.462 *	0.517 **	0.036 *
	(0.249)	(0.022)	(0.259)	(0.021)	(0.026)	(0.265)	(0.260)	(0.021)
Belongs to an RIA	**0.057**	**-0.148**						
	(0.362)	(0.251)						
RIA variable			**0.027**	**0.033** ***			**0.018**	**0.026** *
			(0.022)	(0.013)			(0.024)	(0.014)
Relative RIA variable					**7.5e-04** **	**1.3e-03**	**1.1e-03**	**4.8e-04**
					(3.3e-04)	(8.2e-04)	(9.1e-04)	(3.1e-04)
Constant	11.560	1.189	10.592	1.534	11.573	1.215	11.002	1.481
	(8.237)	(2.181)	(8.665)	(2.146)	(8.577)	(2.158)	(8.626)	(2.142)
Observations	307	307	307	307	307	307	307	307
R-squared	0.38	0.46	0.32	0.47	0.36	0.47	0.32	0.47
F-statistic	12.23	15.2	11.33	15.76	11.89	15.74	10.7	15.07
Hausman Specification Test [b]		55.91		47.87		50.54		47.49
P-Value (Hausman Test)		0.00		0.00		0.00		0.00
F-statistic RIA variables [c]							1.45	4.75 ***

Robust standard errors in parentheses. Decade dummies not reported. * significant at 10%; ** significant at 5%; *** significant at 1%

[a] Endogenous variable: trading partners' growth. Instrument: trading partners' investment rate.

[b] See text for explanation.

[c] Test of joint significance of the RIA variables.

Table 7. Instrumental Variables Growth Regressions, 1960-1999[a]

	Cross-Section (40-year averages)				Random Effects (10-year averages)[b]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log Initial GDP p.c.	-0.542 *	-0.788 ***	-0.114	-0.749 **	-0.505 **	-0.941 ***	-0.672 **	-0.916 ***
	(0.282)	(0.278)	(0.432)	(0.291)	(0.239)	(0.261)	(0.299)	(0.259)
Government Consumption	-0.007	-0.099 **	-0.069	-0.097 **	-0.048 *	-0.093 ***	-0.055 *	-0.093 ***
	(0.029)	(0.042)	(0.052)	(0.042)	(0.029)	(0.030)	(0.031)	(0.030)
Black Market Premium	-3.8e-04	-2.5e-04	-4.8e-04	-2.6e-04	-3.8e-04	-1.7e-04	-2.3e-04	-1.7e-04
	(5.8e-04)	(4.4e-04)	(4.7e-04)	(4.4e-04)	(2.7e-04)	(2.7e-04)	(3.5e-04)	(2.7e-04)
Investment Rate	0.141 ***	0.113 ***	0.097 **	0.112 ***	0.138 ***	0.143 ***	0.126 ***	0.141 ***
	(0.033)	(0.036)	(0.045)	(0.036)	(0.022)	(0.021)	(0.026)	(0.021)
Foreign Direct Investment	-0.002	0.302	0.309	0.303	0.190 **	0.251 ***	0.373 ***	0.244 ***
	(0.252)	(0.210)	(0.299)	(0.210)	(0.089)	(0.090)	(0.132)	(0.091)
Manufactured Exports	0.017 **	0.009	0.021 *	0.009	0.016 ***	0.010 *	0.018 ***	0.010 *
	(0.007)	(0.008)	(0.012)	(0.008)	(0.005)	(0.006)	(0.006)	(0.006)
Human Capital	(0.035)	0.213	0.281	0.217	0.240	0.201	0.418 *	0.189
	(0.260)	(0.190)	(0.227)	(0.185)	(0.165)	(0.171)	(0.215)	(0.171)
Terms of Trade	-0.029	-0.095	-0.042	-0.091	0.039	0.031	0.048	0.031
	(0.119)	(0.100)	(0.109)	(0.100)	(0.030)	(0.029)	(0.038)	(0.030)
Trade Share of GDP	6.6e-03	-8.1e-04	-9.5e-03	-1.3e-03	-1.6e-03	-1.7e-03	-1.4e-02 **	-1.1e-03
	(9.3e-03)	(6.0e-03)	(1.2e-02)	(6.3e-03)	(3.8e-03)	(3.7e-03)	(6.6e-03)	(3.8e-03)
Democracy	0.069	0.054	-0.011	0.050	0.018	0.040 *	0.013	0.041 *
	(0.052)	(0.034)	(0.050)	(0.036)	(0.024)	(0.022)	(0.028)	(0.022)
Trading Partners' Growth	0.404	0.929 **	1.018 *	0.920 **	1.027 ***	1.653 ***	1.591 ***	1.630 ***
	(0.374)	(0.415)	(0.544)	(0.410)	(0.377)	(0.429)	(0.491)	(0.418)
Bordering countries Share of WGDP	0.029 *	0.020	-0.001	0.018	0.036	0.041 *	0.020	0.040 *
	(0.016)	(0.013)	(0.018)	(0.013)	(0.022)	(0.023)	(0.026)	(0.023)
Belongs to an RIA	**-0.355**				**1.344**			
	(1.062)				(0.963)			
RIA variable		**0.094** ***		**0.088** **		**0.101** ***		**0.102** ***
		(0.034)		(0.036)		(0.025)		(0.028)
Relative RIA variable			**4.3e-03** *	**2.5e-04**			**4.6e-03** ***	**-1.2e-04**
			(2.5e-03)	(2.6e-04)			(1.5e-03)	(3.7e-04)
Constant	1.175	3.199 *	-1.659	2.945	0.146	2.077	0.492	1.988
	(1.854)	(1.765)	(3.027)	(1.816)	(2.131)	(2.188)	(2.504)	(2.157)
Observations	81	81	81	81	307	307	307	307
R-squared	0.6	0.61	0.11	0.61	0.39	0.41	0.29	0.41
F-statistic	8.75	10.53	4.47	17.77	13.63	14.18	9.2	13.57
F-statistic (excluded instrument)[c]	1.23	16.32 ***	1.6	14.45 ***				
Partial R-squared (excluded instrument)[c]	0.09	0.56	0.11	0.53				
Hansen J-statistic (Overidentification Test)	11.19	7.49	3.93	7.65				
P-Value (Overidentification Test)	0.01	0.06	0.27	0.05				
Hausman Specification Test[c]	0.33	0.49	8.62	0.37	10.44	0.00	5.22	0.00
P-Value (Hausman Test)	1.00	1.00	0.80	1.00	0.79	1.00	0.99	1.00
F-statistic RIA variables[d]				6.02 ***				9.18 ***

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

[a] Endogenous variables: trading partners' growth and RIA variables. Instruments: trading partners' investment rate and regional dummies.

[b] Decade dummies not reported.

[c] See text for explanation.

[d] Test of joint significance of the RIA variables.

Table 8. Cross-Country Growth Regressions for RIA variables by Type of RIA, 1960-1999[a] (40-year averages)

	(1) OLS	(2) IV[b]	(3) IV-RIA[c]	(4) OLS	(5) IV[b]	(6) IV-RIA[c]	(7) OLS	(8) IV[b]	(9) IV-RIA[c]
RIA variable North-North	0.052 **	0.063 **	0.160 **				0.046	0.063 **	0.149 *
	(0.024)	(0.024)	(0.064)				(0.027)	(0.030)	(0.078)
RIA variable North-South	0.064	0.077 *	0.265				0.052	0.069	0.274
	(0.043)	(0.043)	(0.166)				(0.045)	(0.045)	(0.227)
RIA variable South-South	0.195	0.139	1.213				0.267	0.150	1.216
	(0.318)	(0.328)	(1.318)				(0.367)	(0.387)	(1.672)
Relative RIA variable North-North				5.8e-04 *	6.0e-04 *	4.5e-03	4.0e-04	3.0e-04	1.7e-04
				(3.2e-04)	(3.1e-04)	(2.9e-03)	(2.9e-04)	(2.7e-04)	(4.5e-04)
Relative RIA variable North-South				1.4e-03	1.5e-03	1.4e-02	1.2e-03	1.4e-03	-1.2e-03
				(1.6e-03)	(1.6e-03)	(9.5e-03)	(1.9e-03)	(1.8e-03)	(4.8e-03)
Relative RIA variable South-South				-1.4e-02	-9.6e-03	-9.7e-02	-1.2e-02	-3.0e-03	-7.3e-03
				(2.6e-02)	(2.4e-02)	(1.4e-01)	(3.2e-02)	(3.1e-02)	(7.2e-02)
Observations	81	81	81	81	81	81	81	81	81
R-squared	0.64	0.62	0.28	0.61	0.59		0.64	0.63	0.27
F-statistic	8.81	8.87	7.18	8.67	9.12	3.99	10.68	16.05	20.42
Hausman Specification Test[d]		0.07	0.00		2.22	6.03		0.54	0.00
P-Value (Hausman Test)		1.00	1.00		1.00	0.98		1.00	1.00
F-statistic All RIA variables[d]	2.16 *	2.76 **	2.29 *	1.45	1.62	1.57	1.56	2.06 *	1.89 *
F-statistic N-N RIA variables[d]							4.01 **	5.63 ***	5.08 ***
F-statistic N-S RIA variables[d]							1.05	1.64	1.03
F-statistic S-S RIA variables[d]							0.26	0.08	0.49

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

[a] Other control variables not reported include: Log initial GDP p.c., government consumption, black market premium, investment rate, foreign direct investment, manufactured exports, terms of trade, trade share of GDP, trading partners' growth, and bordering countries share of GDP.

[b] Endogenous variable: trading partners' growth. Instrument: trading partners' investment rate.

[c] Endogenous variables: trading partners' growth and RIA variable. Instruments: trading partners' investment rate and regional dummies.

[d] See text for explanation.

Table 9. Panel Data Growth Regressions for RIA variables by Type of RIA, 1960-1999[a] (10-year averages)

	(1) FE	(2) IV-FE[b]	(3) IV-RE[c]	(4) FE	(5) IV-FE[b]	(6) IV-RE[c]	(7) FE	(8) IV-FE[b]	(9) IV-RE[c]
RIA variable North-North	0.099 ***	0.081 **	0.122 ***				0.073 ***	0.056 *	0.128 ***
	(0.025)	(0.034)	(0.034)				(0.026)	(0.033)	(0.045)
RIA variable North-South	-0.011	-0.008	0.046				0.007	-0.007	-0.031
	(0.020)	(0.027)	(0.092)				(0.023)	(0.030)	(0.173)
RIA variable South-South	0.397 *	0.212	0.541				0.657 ***	0.592 **	0.623
	(0.209)	(0.288)	(1.020)				(0.223)	(0.281)	(1.456)
Relative RIA variable North-North				2.3e-03 ***	2.3e-03 ***	4.4e-03 ***	1.6e-03 **	1.8e-03 *	-2.0e-05
				(7.0e-04)	(8.6e-04)	(1.7e-03)	(7.4e-04)	(9.3e-04)	(5.8e-04)
Relative RIA variable North-South				-2.0e-03	-5.5e-04	7.1e-03	-2.1e-03	-6.0e-05	1.0e-03
				(1.4e-03)	(1.8e-03)	(5.1e-03)	(1.6e-03)	(2.2e-03)	(5.4e-03)
Relative RIA variable South-South				-2.5e-02 **	-3.6e-02 **	-2.2e-02	-3.5e-02 ***	-4.7e-02 ***	-2.5e-02
				(1.1e-02)	(1.5e-02)	(9.2e-02)	(1.2e-02)	(1.6e-02)	(3.7e-02)
Observations	307	307	307	307	307	307	307	307	307
R-squared	0.62	0.32	0.4	0.63	0.44	0.29	0.65	0.45	0.38
F-statistic	17.9	10.42	12.68	18.55	12.69	8.11	17.48	11.41	11.97
Hausman Specification Test[d]		5.49	8.94		3.92	0.00		5.18	11.60
P-Value (Hausman Test)		1.00	0.94		1.00	1.00		1.00	0.87
F-statistic All RIA variables[d]	5.70 ***	1.97	4.73 ***	7.32 ***	5.33 ***	3.10 **	6.25 ***	3.80 ***	3.58 ***
F-statistic N-N RIA variables[d]							9.96 ***	5.13 ***	6.12 ***
F-statistic N-S RIA variables[d]							0.96	0.05	0.02
F-statistic S-S RIA variables[d]							6.03 ***	4.96 ***	0.31

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

[a] Other control variables not reported include: Log initial GDP p.c., government consumption, black market premium, investment rate, foreign direct investment, manufactured exports, terms of trade, trade share of GDP, trading partners' growth, and bordering countries share of GDP.

[b] Endogenous variable: trading partners' growth. Instrument: trading partners' investment rate.

[c] Endogenous variables: trading partners' growth and RIA variable. Instruments: trading partners' investment rate and regional dummies.

[d] See text for explanation.

Table 10. Correlation Between Growth and Excluded RIA Variable (decade averages)

		Growth Rate			
	Decade	(60-69)	(70-79)	(80-89)	(90-99)
Excluded RIA Variable	(60-69)	-0.17			
	(80-89)			-0.17	
	(90-99)				0.04

Number of observations varies with each decade.

Table 11. Correlations Between RIA Variables (decade averages)

		Excluded RIA Variable			
	Decade	(60-69)	(70-79)	(80-89)	(90-99)
RIA Variable	(60-69)	-0.65			
	(70-79)		-0.56		
	(80-89)			-0.49	
	(90-99)				-0.43
Relative RIA Variable	(60-69)	-0.21			
	(70-79)		0.05		
	(80-89)			0.06	
	(90-99)				0.09

Number of observations varies with each decade.

Table 12a. Cross-Country Growth Regressions with Excluded and RIAs Variables, 1960-1999[a] (40-year averages)

	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV
Log Initial GDP p.c.	-0.499 *	-0.486 *	-0.656 **	-0.675 **	-0.621 **	-0.643 **
	(0.294)	(0.285)	(0.260)	(0.259)	(0.264)	(0.264)
Government Consumption	-0.011	-0.007	-0.062	-0.068	-0.064	-0.070 *
	(0.028)	(0.030)	(0.040)	(0.042)	(0.040)	(0.041)
Black Market Premium	-4.6e-04	-5.0e-04	-3.1e-04	-3.2e-04	-3.2e-04	-3.3e-04
	(5.5e-04)	(4.7e-04)	(5.2e-04)	(4.5e-04)	(5.2e-04)	(4.5e-04)
Investment Rate	0.146 ***	0.135 ***	0.129 ***	0.116 ***	0.129 ***	0.116 ***
	(0.033)	(0.035)	(0.034)	(0.036)	(0.035)	(0.036)
Foreign Direct Investment	0.072	0.037	0.233	0.236	0.240	0.242
	(0.195)	(0.197)	(0.178)	(0.182)	(0.179)	(0.184)
Manufactured Exports	0.019 **	0.018 **	0.014 *	0.012	0.014 *	0.013
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Human Capital	0.019	-0.003	0.131	0.135	0.151	0.153
	(0.170)	(0.165)	(0.190)	(0.181)	(0.188)	(0.180)
Terms of Trade	-0.021	-0.037	-0.051	-0.070	-0.050	-0.070
	(0.122)	(0.119)	(0.108)	(0.104)	(0.108)	(0.103)
Trade Share of GDP	3.6e-03	4.4e-03	3.3e-04	3.4e-04	-4.5e-04	-3.9e-04
	(5.3e-03)	(5.5e-03)	(5.3e-03)	(5.5e-03)	(5.7e-03)	(5.9e-03)
Democracy	0.048	0.061	0.045	0.055	0.039	0.049
	(0.036)	(0.037)	(0.034)	(0.035)	(0.035)	(0.036)
Trading Partners' Growth	0.145	0.640	0.370	0.824 *	0.362	0.820 *
	(0.274)	(0.428)	(0.275)	(0.416)	(0.276)	(0.416)
Bordering countries Share of WGDP	0.034 **	0.032 **	0.029 *	0.026 *	0.026 *	0.023 *
	(0.016)	(0.016)	(0.015)	(0.014)	(0.015)	(0.014)
Excluded RIA variable[b]	**0.156**	**0.089**	**0.235**	**0.194**	**0.170**	**0.134**
	(0.192)	(0.206)	(0.187)	(0.194)	(0.186)	(0.192)
RIA variable			**0.057** **	**0.068** ***	**0.051** **	**0.063** ***
			(0.024)	(0.023)	(0.024)	(0.023)
Relative RIA variable					**3.5e-04**	**3.2e-04**
					(2.4e-04)	(2.4e-04)
Constant	0.044	-0.432	0.956	0.742	1.257	1.020
	(2.237)	(2.148)	(2.139)	(2.121)	(2.084)	(2.065)
Observations	81.00	81.00	81.00	81	81.00	81
R-squared	0.60	0.58	0.64	0.62	0.64	0.63
F-statistic	7.93	8.10	10.26	10.64	14.12	14.98
F-statistic (excluded instrument)[b]		85.68		82.61		81.35
Partial R-squared (excluded instrument)[b]		0.56		0.56		0.56
Hausman Specification Test[b]		0.27		0.33		0.43
P-Value (Hausman Test)		1.00		1.00		1.00
F-statistic RIA variables[c]			4.10 **	5.48 ***	3.27 **	4.19 ***

Robust standard errors in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%

[a] Endogenous variable: trading partners' growth. Instrument: trading partners' investment rate.

[b] See text for explanation.

[c] Test of joint significance of the RIA variables.

Table 12b. Panel Data Growth Regressions with Excluded RIA Variable
1960-1999[a] (10-year averages)

	(1) FE	(2) RE	(3) IV-FE	(4) IV-RE
Log Initial GDP p.c.	-3.543 ***	-0.962 ***	-3.107 ***	-0.649 ***
	(0.488)	(0.257)	(0.637)	(0.238)
Government Consumption	-0.034	-0.029	-0.075	-0.026
	(0.040)	(0.026)	(0.053)	(0.024)
Black Market Premium	-3.6e-04	-3.3e-04	6.0e-05	-3.4e-04
	(2.5e-04)	(2.4e-04)	(3.7e-04)	(2.5e-04)
Investment Rate	0.152 ***	0.139 ***	0.159 ***	0.133 ***
	(0.027)	(0.021)	(0.033)	(0.020)
Foreign Direct Investment	0.306 ***	0.265 ***	0.252 **	0.217 ***
	(0.088)	(0.081)	(0.111)	(0.083)
Manufactured Exports	0.010	0.016 ***	0.008	0.018 ***
	(0.010)	(0.006)	(0.013)	(0.005)
Human Capital	0.716 ***	0.467 ***	0.271	0.282 *
	(0.272)	(0.175)	(0.401)	(0.163)
Terms of Trade	0.051 *	0.048	0.051	0.043
	(0.026)	(0.026)	(0.032)	(0.027)
Trade Share of GDP	-6.6e-03	-1.3e-03	5.5e-03	-4.5e-04
	(1.0e-02)	(4.0e-03)	(1.4e-02)	(3.6e-03)
Democracy	-0.046 *	0.026	-0.036	0.032
	(0.027)	(0.021)	(0.034)	(0.021)
Trading Partners' Growth	0.147	0.419 *	3.237 **	0.966 **
	(0.306)	(0.224)	(1.595)	(0.410)
Bordering countries Share of WGDP	-0.006	0.041	0.435	0.036
	(0.120)	(0.027)	(0.266)	(0.023)
Excluded RIA variable[b]	**-0.176**	**-0.112**	**-0.294**	**-0.054**
	(0.382)	(0.251)	(0.472)	(0.231)
Constant	26.175 ***	5.527 **	12.337	1.487
	(3.973)	(2.160)	(8.480)	(2.360)
Observations	307	307	307	307
R-squared	17.78	246.43	12.07	15.09
F -statistic	0.58	0.45	0.38	0.46
Hausman Specification Test (FE-RE)[b]		179.95		53.82
P-Value (Hausman Test)		0.00		0.00

Robust standard errors in parentheses. Decade dummies not reported.

* significant at 10%; ** significant at 5%; *** significant at 1%

[a] Endogenous variable: trading partners' growth. Instrument: trading partners' investment rate.

[b] See text for explanation.

[c] Test of joint significance of the RIA variables.

Table 12c. Panel Data Growth Regressions with Excluded and RIAs Variables, 1960-1999 [a] (10-year averages)

	(1) FE	(2) RE	(3) IV-FE	(4) IV-RE	(5) FE	(6) RE	(7) IV-FE	(8) IV-RE
Log Initial GDP p.c.	-3.746 ***	-1.050 ***	-3.207 ***	-0.705 ***	-3.812 ***	-1.071 ***	-3.312 ***	-0.721 ***
	(0.497)	(0.258)	(0.682)	(0.237)	(0.500)	(0.258)	(0.680)	(0.237)
Government Consumption	-0.040	-0.045 *	-0.086	-0.046 *	-0.036	-0.045 *	-0.080	-0.045 *
	(0.040)	(0.027)	(0.055)	(0.025)	(0.040)	(0.027)	(0.055)	(0.025)
Black Market Premium	-3.5e-04	-3.0e-04	1.2e-04	-2.9e-04	-3.6e-04	-3.0e-04	1.1e-04	-2.9e-04
	(2.5e-04)	(2.4e-04)	(3.8e-04)	(2.5e-04)	(2.5e-04)	(2.4e-04)	(3.8e-04)	(2.5e-04)
Investment Rate	0.161 ***	0.143 ***	0.167 ***	0.134 ***	0.162 ***	0.145 ***	0.168 ***	0.135 ***
	(0.027)	(0.021)	(0.035)	(0.020)	(0.027)	(0.021)	(0.035)	(0.020)
Foreign Direct Investment	0.306 ***	0.275 ***	0.245 **	0.227 ***	0.321 ***	0.290 ***	0.268 **	0.241 ***
	(0.088)	(0.080)	(0.116)	(0.084)	(0.089)	(0.081)	(0.116)	(0.084)
Manufactured Exports	0.010	0.014 **	0.008	0.016 ***	0.012	0.014 **	0.009	0.016 ***
	(0.010)	(0.006)	(0.013)	(0.005)	(0.010)	(0.006)	(0.013)	(0.005)
Human Capital	0.659 **	0.441 **	0.167	0.237	0.651 **	0.473 ***	0.159	0.271 *
	(0.272)	(0.174)	(0.410)	(0.160)	(0.272)	(0.175)	(0.411)	(0.161)
Terms of Trade	0.046 *	0.046 *	0.047	0.040	0.046 *	0.047 *	0.046	0.041
	(0.026)	(0.026)	(0.034)	(0.027)	(0.026)	(0.025)	(0.034)	(0.027)
Trade Share of GDP	-7.6e-03	-1.7e-03	6.4e-03	-8.7e-04	-8.2e-03	-3.2e-03	5.4e-03	-2.2e-03
	(1.0e-02)	(4.0e-03)	(1.5e-02)	(3.5e-03)	(1.0e-02)	(4.1e-03)	(1.5e-02)	(3.6e-03)
Democracy	-0.052 *	0.027	-0.039	0.034	-0.050 *	0.026	-0.037	0.032
	(0.027)	(0.021)	(0.036)	(0.021)	(0.027)	(0.021)	(0.036)	(0.021)
Trading Partners' Growth	0.127	0.430 *	3.637 **	1.164 ***	0.145	0.426 *	3.641 **	1.167 ***
	(0.305)	(0.223)	(1.577)	(0.397)	(0.305)	(0.222)	(1.575)	(0.398)
Bordering countries Share of WGDP	0.025	0.046 *	0.518 *	0.040 *	0.043	0.042	0.543 **	0.036
	(0.121)	(0.027)	(0.265)	(0.022)	(0.122)	(0.027)	(0.268)	(0.022)
Excluded RIA variable [b]	**-0.016**	**0.039**	**-0.185**	**0.097**	**-0.149**	**-0.076**	**-0.390**	**-0.005**
	(0.389)	(0.257)	(0.507)	(0.233)	(0.407)	(0.266)	(0.535)	(0.242)
RIA variable	**0.033 ***	**0.031 ****	**0.026**	**0.035 *****	**0.024**	**0.020**	**0.012**	**0.026 ***
	(0.018)	(0.014)	(0.023)	(0.013)	(0.019)	(0.015)	(0.025)	(0.014)
Relative RIA variable					**8.0e-04**	**5.8e-04**	**1.3e-03**	**4.9e-04**
					(7.3e-04)	(3.6e-04)	(9.6e-04)	(3.2e-04)
Constant	27.123 ***	5.840 ***	11.213	1.098	27.811 ***	6.362 ***	12.393	1.524
	(3.981)	(2.148)	(8.616)	(2.296)	(4.028)	(2.166)	(8.493)	(2.318)
Observations	307	307	307	307	307	307	307	307
R-squared	17.14	254.71	10.63	14.81	16.27	258.78	10.11	14.18
F-statistic	0.59	0.47	0.32	0.47	0.59	0.47	0.33	0.47
Hausman Specification Test (FE-RE) [b]		218.51		48.67		136.38		46.19
P-Value (Hausman Test)		0.00		0.00		0.00		0.00
F-statistic RIA variables [c]	1.85	5.22 *	0.84	3.54 **	1.64	7.80 *	1.14	3.15 **

Robust standard errors in parentheses. Decade dummies not reported.

* significant at 10%; ** significant at 5%; *** significant at 1%

[a] Endogenous variable: trading partners' growth. Instrument: trading partners' investment rate.

[b] See text for explanation.

[c] Test of joint significance of the RIA variables.

Table 13. Investment and Manufactured Exports Channel Regressions, 1960-1999 (40-year averages)

	OLS		IV[a]		SUR		3SLS[b]	
	(1) Investment	(2) Manufactured Exports	(3) Investment	(4) Manufactured Exports	(5) Investment	(6) Manufactured Exports	(7) Investment	(8) Manufactured Exports
RIA variable	**0.220** **	**1.127** ***	**0.390** ***	**2.082** ***	**0.223** **	**1.120** ***	**0.443** ***	**1.905** ***
	(0.106)	(0.339)	(0.123)	(0.571)	(0.089)	(0.322)	(0.129)	(0.477)
Log Initial GDP p.c.	1.059	-4.144	0.863	-7.034	1.049	-5.216	0.821	-6.768
	(1.198)	(4.738)	(1.266)	(4.800)	(1.155)	(4.787)	(1.239)	(5.590)
Government Consumption	-0.061	0.168	-0.195	-0.770	-0.063	0.475	-0.236	-0.181
	(0.172)	(0.794)	(0.218)	(1.061)	(0.142)	(0.636)	(0.164)	(0.715)
Black Market Premium	-7e-04	-3e-03	-5e-04	-3e-04	-7e-04	-4e-03	-5e-04	-2e-03
	(1.3e-03)	(4.0e-03)	(1.3e-03)	(5.1e-03)	(2.3e-03)	(9.4e-03)	(2.4e-03)	(9.9e-03)
Foreign Direct Investment	0.423	-3.051	0.841	0.327	0.411	-3.248	0.811	-0.401
	(0.673)	(3.244)	(0.794)	(3.699)	(0.759)	(3.120)	(0.821)	(3.470)
Human Capital	1.202 *	2.462	1.681 **	4.390	1.215	2.604	1.830 **	4.325
	(0.717)	(3.001)	(0.815)	(3.281)	(0.808)	(3.151)	(0.872)	(3.338)
Terms of Trade	-0.112	-3.976 ***	-0.177	-4.238 **	-0.118	-3.360 **	-0.243	-3.549 **
	(0.371)	(1.429)	(0.391)	(1.672)	(0.343)	(1.401)	(0.370)	(1.503)
Trade Share of GDP	0.032 *	-0.276 *	0.020	-0.339 **	0.032	-0.275 **	0.022	-0.294 *
	(0.018)	(0.158)	(0.020)	(0.161)	(0.022)	(0.131)	(0.023)	(0.153)
Democracy	0.023	1.354 **	0.013	1.164 *	0.027	1.644 ***	0.044	1.580 ***
	(0.147)	(0.583)	(0.151)	(0.673)	(0.124)	(0.493)	(0.141)	(0.526)
Trading Partners' Growth	3.509 ***	0.017	3.840 ***	4.370	3.521 ***	1.975	3.880 ***	8.343
	(1.276)	(6.520)	(1.297)	(5.980)	(1.149)	(5.464)	(1.237)	(5.781)
Bordering countries Share of WGDP	0.017	0.633 **	0.000	0.440	0.018	0.613 *	0.007	0.495
	(0.055)	(0.299)	(0.058)	(0.311)	(0.079)	(0.314)	(0.086)	(0.334)
Population Proportion over 65	-0.654 *		-0.894 ***		-0.656 **		-0.940 ***	
	(0.350)		(0.332)		(0.308)		(0.334)	
Ethnolinguistic Fractionalization	-0.046 **		-0.040 *		-0.046 **		-0.033 *	
	(0.022)		(0.023)		(0.018)		(0.019)	
Population Density		0.047 ***		0.046 ***		0.049 ***		0.050 ***
		(0.012)		(0.013)		(0.012)		(0.013)
East Asia Dummy		18.081 *		16.367		16.851 *		14.164
		(9.730)		(10.211)		(9.396)		(12.588)
Investment Rate		0.350		0.029		-0.056		-1.125
		(0.611)		(0.626)		(0.472)		(1.251)
Manufactured Exports	0.003		-0.006		0.001		-0.025	
	(0.030)		(0.032)		(0.024)		(0.045)	
Constant	7.272	55.991 *	10.000	81.262 **	7.391	63.062 **	10.875	85.084 **
	(7.995)	(28.566)	(8.860)	(31.871)	(7.822)	(31.112)	(8.638)	(33.311)
Observations	79	81	79	81	79	79	79	79
R-squared	0.48	0.62	0.46	0.58	0.48	0.65	0.43	0.6
F-statistic	7.55	15.08	8.27	14.51	72.95	146.59	73.34	138.83
F-statistic (excluded instruments)[c]			16.76	19.88				
Partial R-squared (excluded instrument)[c]			0.52	0.56				
Hansen J-statistic (Overidentification Test)[c]			3.60	2.57				
P-Value (Chi-squared)			0.31	0.46				
Hausman Specification Test[c]			0.00	3.17			76.85 ***	
Hausman Specification Test RIA variable[c]			7.67 ***	4.32 **			5.67 **	4.96 **
Independence Test[c]					0.01			

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

[a] Endogenous Variable: RIA variable. Instruments: regional dummies.

[b] Endogenous Variables: Investment, Manufactured Exports, and RIA variable. Instruments: regional dummies.

[c] See text for explanation.

Table 14. Investment and Manufactured Exports Channel Regressions, 1960-1999 (10-year averages)

	OLS		IV[a]		SUR		3SLS[b]	
	(1) Investment	(2) Manufactured Exports	(3) Investment	(4) Manufactured Exports	(5) Investment	(6) Manufactured Exports	(7) Investment	(8) Manufactured Exports
RIA variable	**0.013**	**0.679** ***	**0.252** ***	**1.677** ***	**-2e-04**	**0.678** ***	**0.194** **	**1.114** ***
	(0.045)	(0.148)	(0.089)	(0.237)	(0.040)	(0.139)	(0.084)	(0.295)
Log Initial GDP p.c.	3.733 ***	2.226	3.730 ***	-2.502	3.615 ***	0.156	3.745 ***	-5.325
	(0.730)	(2.754)	(0.739)	(2.952)	(0.690)	(2.655)	(0.711)	(4.090)
Government Consumption	-0.016	0.026	-0.161	-0.701 *	-0.005	0.225	-0.125	-0.482
	(0.071)	(0.312)	(0.106)	(0.406)	(0.070)	(0.302)	(0.088)	(0.357)
Black Market Premium	0.000	-0.004 ***	0.000	-0.002	0.000	-0.004	0.000	-0.003
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.003)	(0.001)	(0.003)
Foreign Direct Investment	0.160	-2.638 ***	0.363	-1.430	0.227	-2.450 **	0.312	-9.556 **
	(0.304)	(0.933)	(0.316)	(1.018)	(0.248)	(0.979)	(0.270)	(4.019)
Human Capital	-0.535	2.140	-0.146	2.174	-0.586	2.433	-0.239	3.677 *
	(0.429)	(1.675)	(0.442)	(1.973)	(0.449)	(1.713)	(0.477)	(2.031)
Terms of Trade	0.071	-0.782 **	0.052	-0.783 **	0.086	-0.717 **	0.056	-1.000 ***
	(0.111)	(0.307)	(0.108)	(0.334)	(0.084)	(0.332)	(0.089)	(0.378)
Trade Share of GDP	0.045 ***	-0.133 **	0.037 ***	-0.160 **	0.042 ***	-0.151 ***	0.039 ***	-0.042
	(0.012)	(0.062)	(0.012)	(0.066)	(0.009)	(0.056)	(0.010)	(0.129)
Democracy	-0.009	0.835 ***	0.009	0.819 ***	-0.027	0.970 ***	0.005	1.082 ***
	(0.060)	(0.252)	(0.062)	(0.273)	(0.060)	(0.234)	(0.066)	(0.260)
Trading Partners' Growth	0.578	-4.653	0.556	-3.421	0.567	-4.200	0.557	-0.758
	(0.596)	(3.289)	(0.639)	(3.163)	(0.611)	(2.756)	(0.633)	(3.117)
Bordering countries Share of WGDP	0.001	0.676 ***	-0.016	0.628 ***	-0.014	0.732 ***	-0.011	0.682 ***
	(0.043)	(0.203)	(0.052)	(0.197)	(0.056)	(0.216)	(0.062)	(0.236)
Population Proportion over 65	-0.558 ***		-0.998 ***		-0.545 ***		-0.894 ***	
	(0.176)		(0.205)		(0.167)		(0.212)	
Ethnolinguistic Fractionalization	-0.027 **		-0.016		-0.026 **		-0.019	
	(0.013)		(0.014)		(0.012)		(0.013)	
Population Density		0.023 ***		0.023 ***		0.022 ***		0.021 ***
		(0.005)		(0.005)		(0.005)		(0.006)
East Asia Dummy		12.885 **		14.423 **		13.902 **		4.247
		(6.319)		(6.826)		(5.411)		(6.965)
Investment Rate		0.502 *		0.519 *		0.773 ***		2.159 ***
		(0.275)		(0.309)		(0.239)		(0.636)
Manufactured Exports	0.032 **		0.026		0.052 ***		0.027	
	(0.015)		(0.018)		(0.014)		(0.030)	
Constant	-8.541	10.677	-5.225	48.940 **	-8.125	16.989	-6.122	30.399
	(5.525)	(20.403)	(5.822)	(22.197)	(5.176)	(19.968)	(5.437)	(27.578)
Observations	299	307	299	307	299	299	299	299
R-squared	0.4	0.54	0.33	0.46	0.39	0.54	0.36	0.36
F / Chi2 -statistic	12.63	26.31	11.71	24.81	202.98	368.53	188.95	353.42
F -statistic (excluded instruments)[c]			22.70	53.07				
Partial R-squared (excluded instrument)[c]			0.25	0.43				
Hansen J-statistic (Overidentification Test)[c]			18.21	10.78				
P-Value (Chi-squared)			0.00	0.01				
Hausman Specification Test[c]			39.21 ***	0.00			64.28 ***	
Hausman Specification Test RIA variable[c]			9.80 ***	28.97 ***			6.91 ***	2.80 *
Independence Test[c]					2.43			

Robust standard errors in parentheses. Decade dummies not reported. * significant at 10%; ** significant at 5%; *** significant at 1%

[a] Endogenous Variable: RIA variable. Instruments: regional dummies.

[b] Endogenous Variables: Investment, Manufactured Exports, and RIA variable. Instruments: regional dummies.

[c] See text for explanation.

Table 15. Summary Statistics Socio-Economic Variables

Variable[†]	Obs.	Mean	Std. Dev.	Min	Max
Outcomes					
Enrollment	927	0.86	0.34	0.00	1.00
Education Gap	712	1.54	1.62	0.00	7.00
Child in Correct Grade	550	0.43	0.50	0.00	1.00
Absenteeism	774	0.41	2.13	0.00	23.00
Repeating Grade	774	0.09	0.29	0.00	1.00
Highest Grade Completed	927	1.75	1.57	0.00	5.00
Age in First Grade	224	7.74	1.70	6.00	13.00
Socio-Economic Variables[‡]					
Age	927	9.61	2.40	6.00	14.00
Number of children aged 0-5	927	1.09	0.98	0.00	6.00
Family size	927	7.41	2.71	0.00	18.00
Female	927	0.47	0.50	0.00	1.00
Time to school	924	14.65	16.67	0.00	120.00
Rural household	927	0.66	0.47	0.00	1.00
Maximum years of education	927	6.44	3.46	0.00	17.00
Mother works	927	0.42	0.49	0.00	1.00
Head of household is female	927	0.26	0.44	0.00	1.00
Total household income (000C$)	924	26.93	36.44	0.00	384.69
Owns house	927	0.85	0.35	0.00	1.00
Household is poor	927	0.61	0.49	0.00	1.00
Household is extremely poor	927	0.26	0.44	0.00	1.00
School classrooms in 1993	927	5.37	5.07	1.00	17.00

Source: Author calculations using FISE Survey.

[†] See Appendix 1 for detailed description of the variables.

[‡] Obtained using the largest possible sample.

Table 16. Differences in Socio-Economic Variables by Treatment Status

	Treatment group		FISE comparison group		*t* -test on means	
Variable[†]	Obs.	Mean	Obs.	Mean	Obs.	(p-value)
Age	466	9.51	461	9.66	927	0.344
Number of children aged 0-5	466	1.04	461	1.11	927	0.274
Family size	466	7.52	461	7.36	927	0.375
Female	466	0.44	461	0.48	927	0.201
Time to school	466	12.21	458	15.69	924	0.001 ***
Rural household	466	0.58	461	0.70	927	0.000 ***
Maximum years of education	466	6.66	461	6.35	927	0.181
Mother works	466	0.54	461	0.37	927	0.000 ***
Head of household is female	466	0.28	461	0.25	927	0.292
Total household income (000C$)	465	30.16	459	25.56	924	0.049 **
Owns house	466	0.88	461	0.84	927	0.061 *
Household is poor	466	0.59	461	0.62	927	0.449
Household is extremely poor	466	0.21	461	0.28	900	0.024 **
School classrooms in 1993	466	5.36	461	5.38	900	0.945

Source: Author calculations using FISE Survey.

* significant at 10%; ** significant at 5%; *** significant at 1%.

[†]See Appendix 1 for detailed description of the variables. Obtained using the largest possible sample.

Table 17. Outcome Regressions

	Enrollment			Education Gap		
	(1)	(2)	(3)	(4)	(5)	(6)
FISE School	0.023	0.002	0.016	-0.155	0.079	-0.123
	(0.032)	(0.030)	(0.028)	(0.182)	(0.151)	(0.113)
Number of children aged 0-5			0.011			-0.126 *
			(0.020)			(0.068)
Family size			-0.004			0.085 ***
			(0.006)			(0.028)
Female			0.017			-0.390 ***
			(0.029)			(0.112)
Time to school			-0.002			0.010 **
			(0.001)			(0.004)
Rural household			-0.123 *			0.339
			(0.070)			(0.311)
Maximum years of education			0.014 ***			-0.069 ***
			(0.005)			(0.022)
Mother works			0.073 **			0.119
			(0.030)			(0.126)
Head of household is female			-0.097 ***			0.220
			(0.035)			(0.123)
Total household income			-0.003			-0.007
			(0.004)			(0.018)
Owns house			0.066			-0.445 ***
			(0.043)			(0.149)
Household is poor			-0.003			0.121
			(0.037)			(0.147)
Household is extremely poor			-0.146 ***			0.214
			(0.039)			(0.192)
School classrooms in 1993			0.002			-0.060 **
			(0.006)			(0.027)
Age 7			0.243 ***			
			(0.061)			
Age 8			0.261 ***			
			(0.061)			
Age 9			0.257 ***			0.145
			(0.068)			(0.111)
Age 10			0.273 ***			0.685 ***
			(0.057)			(0.135)
Age 11			0.227 ***			1.076 ***
			(0.071)			(0.189)
Age 12			0.190 ***			1.752 ***
			(0.080)			(0.243)
Age 13			0.048			2.317 ***
			(0.084)			(0.189)
Age 14			-0.084			2.957 ***
			(0.109)			(0.267)
Constant	0.856 ***	0.777 ***	0.606 ***	1.580 ***	1.632 ***	0.790
	(0.025)	(0.081)	(0.125)	(0.126)	(0.441)	(0.494)
Observations	927	927	921	712	712	706
R-squared	0.001	0.13	0.32	0.002	0.23	0.65
F-statistic	0.55	4.70	4.23	0.73	17.88	21.11
P-value School-Pair		0.00	0.00		0.00	0.00
P-value Age			0.00			0.00

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 18. Outcome Regressions

	Child in Correct Grade			Absenteeism		
	(1)	(2)	(3)	(4)	(5)	(6)
FISE School	0.109	0.019	0.031	-0.062	-0.178	-0.238
	(0.066)	(0.052)	(0.051)	(0.218)	(0.196)	(0.195)
Number of children aged 0-5			0.081 ***			0.135
			(0.031)			(0.087)
Family size			-0.047 ***			-0.047
			(0.014)			(0.037)
Female			0.098 *			-0.212
			(0.052)			(0.199)
Time to school			0.000			0.010
			(0.002)			(0.008)
Rural household			-0.111			0.165
			(0.132)			(0.359)
Maximum years of education			0.024 **			-0.067 **
			(0.010)			(0.033)
Mother works			-0.063			-0.133
			(0.054)			(0.217)
Head of household is female			-0.098 *			0.083
			(0.059)			(0.227)
Total household income			-0.003			0.039
			(0.007)			(0.026)
Owns house			0.188 **			-0.105
			(0.075)			(0.422)
Household is poor			-0.009			0.392
			(0.075)			(0.360)
Household is extremely poor			-0.066			-0.302
			(0.073)			(0.215)
School classrooms in 1993			0.008			0.063
			(0.013)			(0.039)
Age 7						0.645
						(0.508)
Age 8						0.495
						(0.440)
Age 9			-0.061			-0.220
			(0.081)			(0.273)
Age 10			-0.198 ***			-0.129
			(0.075)			(0.241)
Age 11			-0.365 ***			0.708
			(0.082)			(0.481)
Age 12			-0.378 ***			0.216
			(0.080)			(0.479)
Age 13						0.212
						(0.347)
Age 14						0.222
						(0.393)
Constant	0.402 ***	0.485 ***	0.697 ***	0.432 ***	0.071	0.055
	(0.045)	(0.118)	(0.200)	(0.151)	(0.080)	(0.420)
Observations	550	550	546	774	774	768
R-squared	0.01	0.31	0.45	0.0002	0.09	0.15
F-statistic	2.70	118.45	16.73	0.08	0.75	0.67
P-value School-Pair		0.00	0.00		0.76	0.11
P-value Age			0.00			0.11

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 19. Outcome Regressions

	Repeating Grade			Highest Grade Completed		
	(1)	(2)	(3)	(4)	(5)	(6)
FISE School	-0.030	0.001	0.020	-0.006	-0.094	0.010
	(0.028)	(0.028)	(0.026)	(0.159)	(0.154)	(0.105)
Number of children aged 0-5			0.012			0.038
			(0.019)			(0.060)
Family size			0.003			-0.050 **
			(0.007)			(0.025)
Female			-0.051			0.314 ***
			(0.031)			(0.107)
Time to school			0.000			-0.010 **
			(0.001)			(0.004)
Rural household			-0.125 *			-0.044
			(0.070)			(0.308)
Maximum years of education			-0.004			0.049 ***
			(0.004)			(0.019)
Mother works			0.006			-0.014
			(0.038)			(0.115)
Head of household is female			0.014			-0.143
			(0.045)			(0.110)
Total household income			-0.004			0.010
			(0.003)			(0.020)
Owns house			0.004			0.315 **
			(0.034)			(0.156)
Household is poor			-0.083 **			-0.222
			(0.038)			(0.141)
Household is extremely poor			0.041			-0.173
			(0.049)			(0.176)
School classrooms in 1993			-0.007			0.037
			(0.006)			(0.026)
Age 7			0.094			0.540 ***
			(0.063)			(0.157)
Age 8			0.141 **			0.905 ***
			(0.068)			(0.161)
Age 9			0.038			1.701 ***
			(0.041)			(0.172)
Age 10			0.083 *			2.038 ***
			(0.049)			(0.180)
Age 11			0.061			2.502 ***
			(0.056)			(0.234)
Age 12			0.026			2.725 ***
			(0.050)			(0.253)
Age 13			0.018			3.203 ***
			(0.050)			(0.207)
Age 14			0.043			3.494 ***
			(0.042)			(0.319)
Constant	0.099 ***	0.142 **	0.203 *	1.756 ***	1.763 ***	-0.227
	(0.022)	(0.069)	(0.106)	(0.112)	(0.325)	(0.429)
Observations	774	774	768	927	927	921
R-squared	0.002	0.09	0.15	0.00	0.09	0.57
F-statistic	1.15	2.63	1.20	0.00	2.97	18.02
P-value School-Pair		0.00	0.57		0.00	0.00
P-value Age			0.57			0.00

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 20. Outcome Regressions

	Age in First Grade		
	(1)	(2)	(3)
FISE School	-0.515 *	-0.359	-0.251
	(0.308)	(0.250)	(0.257)
Number of children aged 0-5			-0.181
			(0.136)
Family Size			0.107 *
			(0.057)
Female			-0.182
			(0.236)
Time to school			0.006
			(0.009)
Rural household			0.187
			(0.826)
Maximum years of education			-0.120 ***
			(0.040)
Mother works			-0.304
			(0.300)
Head of household is female			-0.364
			(0.350)
Total household income			0.006
			(0.028)
Owns house			-0.062
			(0.461)
Household is poor			0.392
			(0.338)
Household is extremely poor			-0.077
			(0.565)
School classrooms in 1993			-0.117
			(0.075)
Constant	7.891 ***	9.141 ***	10.112
	(0.256)	(1.348)	(1.714)
Observations	224	224	222
R-squared	0.02	0.38	0.39
F-statistic	2.80		5.23
P-value School-Pair		0.00	0.07

Robust standard errors in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 21. Probit Estimates (Marginal Effects) of Participation Equation

Variable	Coefficient
Age	0.011
	(0.219)
Female	-0.189
	(0.140)
Non-FISE projects HH benefited from	0.275
	(0.189)
Non-FISE projects HH benefited/participated	-1.701 ***
	(0.530)
Number of organizations HH belongs to	0.24
	(0.174)
House has dirt access road	0.559 ***
	(0.177)
Rural household	-0.434 ***
	(0.147)
Time to school	-0.009
	(0.010)
Maximum years of education	0.071 **
	(0.034)
Total household Income	-0.119
	(0.369)
Owns house	-0.096
	(0.286)
Household is poor	0.006
	(0.203)
Household is extremely poor	0.634 *
	(0.344)
Municipality poverty rate	0.24 ***
	(0.067)
Municipality extreme poverty rate	-0.158 **
	(0.066)
School classrooms in 1993	0.016
	(0.033)
Observations	830
Wald (Chi2)	209.11
Pseudo R2	0.23
P-value Municipalities	0.00

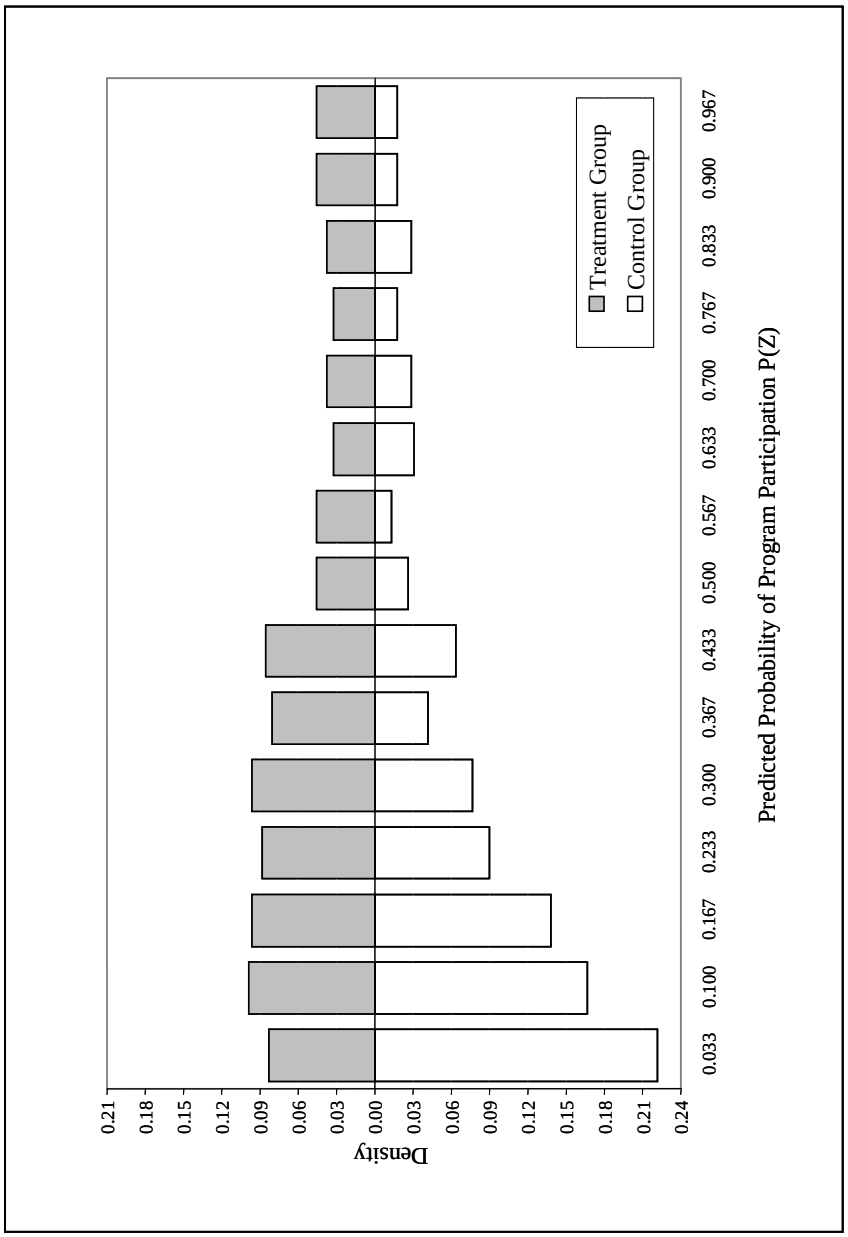
Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Covariates not reported: interactions between age and all other variables except for municipalities and rural dummy, and interactions between municipality poverty rate and all other variables except for municipalities, rural dummy, age and female.

FIGURES

Figure 1. Density of Estimated Probability of Program Participation



APPENDIX 1: REGIONAL INTEGRATION AGREEMENTS

Agreements notified under GATT Article XXIV*	Original Date	Alternative Date	Type of Agreement[a]
European Communities (EC)	1958[1]		North-North
Belgium, France, Germany, Italy, Luxembourg and Netherlands			
Accession of Denmark, Ireland, and United Kingdom	1973[1]		North-North
Accession of Greece	1981[1]		North-North
Accession of Spain and Portugal	1986[1]		North-North
Accession of Austria, Finland and Sweden	1995[1]		North-North
EC/Algeria	1977[1]	2001	North-South
EC/Cyprus	1973[1]		North-North
EC/Egypt	1977[1]	2002	North-South
EC/Hungary	1994[1]		North-South
EC/Iceland	1973[1]	1994	North-North
EC/Israel	1975[1]	2000	North-South
EC/Jordan	1977[1]	2001	North-South
EC/Malta	1971[1]	2001	North-North
EC/Morocco	1977[1]	2000	North-South
EC/Norway	1973[1]		North-North
EC/Poland	1992[3]		North-South
EC/Switzerland	1973[1]		North-North
EC/Syria	1977[1]		North-South
EC/Tunisia	1998[1]		North-South
EC/Turkey	1996[1]		North-South
European Free-Trade Area (EFTA)	1960[1]		North-North
Austria, Denmark, Liechtenstein, Norway, Portugal, Sweden, Switzerland, and United Kingdom			
EFTA-Finland (full membership in 1986)	1961[1]		North-North
Denmark and United Kingdom leave	1972		
Accession of Iceland	1970[1]		North-North
Portugal leaves	1985		
Austria, Finland and Sweden leave	1995		
EC/EFTA	1974[1]	1994	North-North
EFTA/Hungary	1994[1]		North-South
EFTA/Israel	1993[1]		North-South
EFTA/Poland	1994[1]		North-South
EFTA/Turkey	1992[1]		North-South
Hungary/Israel	1998[1]		South-South
Israel/Poland	1998[1]		South-South
Turkey/Hungary	1998[1]		South-South
Turkey/Israel	1997[1]		South-South

Agreements notified under GATT Article XXIV*	Original Date	Alternative Date	Type of Agreement[a]
United States/Israel	1986[1]		North-South
Canada/Chile	1997[1]		North-South
Canada/Israel	1997[1]		North-South
North American Free Trade Agreement (NAFTA) Canada, Mexico, and United States	1994[1]		North-South
Central American Common Market (CACM) Costa Rica (1962), El Salvador, Guatemala, Honduras, and Nicaragua	1962[1]	1993	South-South
Caribbean Community and Common Market (CARICOM) Bahamas (1983), Barbados, Guyana, Jamaica, Trinidad and Tobago, Antigua and Barbuda, Belize, Dominica, Grenada, Haiti (1997), Montserrat, St. Kitts and Nevis, St. Lucia, St. Vincent and the Grenadines, and Suriname (1995)	1974[3]		South-South
Closer Economic Relations Trade Agreement (ANZCERTA) Australia and New Zealand	1983[1]		North-North

* Regional integration agreements notified to the GATT/WTO and in force in May 2000 (excluding RIAs deemed to be inactive as of 1 May 2000).

Agreements notified under the Enabling Clause**	Original Date	Revival Date	Type of Agreementa
Andean Pact relabeled Andean Group Bolivia, Colombia, Ecuador, Peru, and Venezuela	1969[2]	1993	South-South
Southern Common Market (MERCOSUR) Argentina, Brazil, Paraguay, and Uruguay	1992[1]		South-South
MERCOSUR/Chile	1997[1]		South-South
MERCOSUR/Bolivia	1997[1]		South-South
Chile/Mexico	1992[1]		South-South
Argentina/Colombia	1991[3]		South-South
Argentina/Venezuela	1992[3]		South-South
Argentina/Ecuador	1993[3]		South-South
Brazil/Peru	1993[3]		South-South
Brazil/Venezuela	1994[3]		South-South
Bolivia/Chile	1993[3]		South-South
Chile/Colombia	1994[1]		South-South
Chile/Venezuela	1994[1]		South-South
Group of Three Colombia, Mexico and Venezuela	1995[1]		South-South
Mexico/Bolivia	1995[1]		South-South
Chile/Ecuador	1995[1]		South-South

APPENDIX 1: REGIONAL INTEGRATION AGREEMENTS (CONTINUED)

Agreements notified under the Enabling Clause**	Original Date	Revival Date	Type of Agreementa
Mexico/CARICOM	1993[3]		South-South
Colombia/CARICOM	1994[3]		South-South
Costa Rica/CARICOM	1994[3]		South-South
Venezuela/CARICOM	1994[3]		South-South
Mexico/Costa Rica	1995[1]		South-South
Mexico/Nicaragua	1998[1]		South-South
Chile/Peru	1998[1]		South-South
Tripartite Agreement Egypt, India, and Yugoslavia	1968[3]		South-South
Central African Customs and Economic Union (UDEAC) relabeled Comunaute Economique et Monetaire d'Afrique Centrale (CEMAC) Cameroon, Central African Republic (CAR), Chad, Congo, Gabon, (Equatorial Guinea (since 1985))	1966[3]	1994	South-South
Common Market for Eastern and Southern Africa (COMESA) Angola, Burundi, Comoros, Democratic Republic of Congo, Djibouti, Eritrea, Ethiopia, Kenya, Madagascar, Malawi, Mauritius, Namibia, Rwanda, Seychelles, Sudan, Swaziland, Tanzania, Uganda, Zambia, and Zimbabwe	1995[3]		South-South
Economic Community of Western African States (ECOWAS) Benin, Burkina Faso, Cape Verde, Côte d'Ivoire, Gambia, Ghana, Guinea, Guinea Bissau, Liberia, Mali, Mauritania, Niger, Nigeria, Senegal, Sierra Leon, and Togo	1975[3]		South-South
Economic Cooperation Organization (ECO) Islamic Republic of Iran, Pakistan, and Turkey	1992[3]		South-South
South Asian Preferential Trade Arrangement (SAPTA) Bangladesh, Bhutan, India, Maldives, Nepal, Pakistan, and Sri Lanka	1996[3]		South-South
Bangkok Agreement the Republic of Korea, Sri Lanka, and Thailand	1976[3]	1990	South-South
ASEAN Preferential Trade Arrangements (AFTA) Indonesia, Laos (1997), Malaysia, Myanmar (1997), The Philippines, Singapore, and Thailand	1977[2]	1992	South-South

** Regional integration agreements notified to the GATT/WTO and in force on 1 January 2000 (excluding RIAs deemed to be inactive as of 1 January 2000).

[1] : Considered as active/functioning at original date. See text for explanation.

[2] : Considered as active/functioning at alternative date. See text for explanation.

[3] : Not considered as active/functioning either at original or alternative date. See text for explanation.

[a] Following Pritchett (2000) North Countries are all OECD Countries (except for Turkey, Mexico, Hungary, Korea, and Poland), Cyprus and Malta. South Country = Otherwise. Source: Pritchett (2000)

Source: WTO, The World Bank, Inter-American Develepment Bank, the European Union, and Soloaga and Winters (1999).

APPENDIX 2: VARIABLES, DESCRIPTION, AND DATA SOURCES

Growth: Growth rate of Real GDP Per Capita in constant dollars (international prices, base year 1985). Source: Penn World Table 5.6 and World Bank. Missing data calculated from 1985 GDP per capita and GDP per capita growth rates (Global Development Finance and World Development Indicators).

Initial Income: Log of Real GDP Per Capita in constant dollars (international prices, base year 1985). Source: Penn World Table 5.6 and World Bank. Missing data calculated from 1985 GDP per capita and GDP per capita growth rates (Global Development Finance and World Development Indicators).

Government Consumption: General government final consumption expenditure (% of GDP).

Source: Global Development Finance & World Development Indicators.

Black Market Premium: Black Market premium on the official exchange rate.

$$BMP = \left[\left(Parallel\ Rate / Official\ Rate - 1\right) * 100\right]$$

Source: Levine and Renelt; World's Currency Yearbook (for 1985, 1990-93); Adrian Wood, Global trends in real exchange rates: 1960-84, WB Discussion paper no. 35. 1988 (filling in missing observations); Global Development Finance and World Development Indicators (for 1996-1997, calculated as); values for industrial countries are added as 0).

Investment: Gross domestic investment as % of GDP. Source: World Development Indicators 2001.

Foreign direct investment: Net inflows as % of GDP. Source: World Development Indicators 2001 and World Bank.

Manufactures exports: Ratio of manufactured exports to merchandise exports (%). Source: World Development Indicators 2001

Terms of Trade Shocks: Growth rate of export prices minus growth rate of import prices (%). Source: LDB central database.

Trade Share: Exports and Imports as % of GDP. Source: World Development Indicators and Global Development Finance

Trading Partners' Growth: Weighted GDP Per Capita growth of Trading Partners.

$TPG_j = \sum_i \left(ShareT_{ij} * g_i \right)$, where $ShareT_{ij} = \frac{X_{ij} + M_{ij}}{\sum_j (X_{ij} + M_{ij})}$, X_{ij} are exports from country i to j, M_{ij} are country's i imports from j, and g_i is the growth rate of country i. Source: IMF: Directions of Trade (for trade data); Global Development Finance and World Development Indicators (for growth of Real GDP Per Capita in constant dollars at international prices, base year 1985) in Easterly (2001).

Trading Partners' Investment: Weighted Investment rate of Trading Partners.

$TPG_j = \sum_i \left(ShareT_{ij} * I_i \right)$, where $ShareT_{ij} = \frac{X_{ij} + M_{ij}}{\sum_j (X_{ij} + M_{ij})}$, X_{ij} are exports from country i to j, M_{ij} are country's i imports from j, and I_i is gross domestic investment as % of GDP. Source: IMF: Directions of Trade (for trade data), and World Development Indicators 2001 (for investment rate).

Neighbors: Share of World GDP of all neighboring countries.

$Neighbors_i = \sum_j \left(N_{ij} * SWGDP_j \right)$, where N_{ij} is a dummy variable that takes the value of 1 when country j a neighbor of country i, and $SWGDP_j$ is the share of world GDP of country j. Source: World Development Indicators (for share of World Real GDP).

Human Capital: Average years of secondary schooling in the total population plus average years of higher schooling in the total population. Source: Barro R. and J.W. Lee.

Democracy: general openness or closedness of political institutions. The operational indicator is derived from authority characteristics according to the following criteria: Regulation of Executive Recruitment: institutionalized procedures regarding the transfer of executive power. Competitiveness of Executive Recruitment: extent to which executives are chosen through competitive elections. Openness of Executive Recruitment: opportunity for non-elites to attain executive office. Executive Constraints: operational (de facto) independence of chief executive. Regulation of Participation: development of institutional structures for political expression. Competitiveness of Participation: extent to which non-elites are able to access institutional structures for political expression. Range = -10 to 10 (-10 = high autocracy; 10 = high democracy). Source: Polity IV Project, Political Regime Characteristics and Transitions, 1800-1999. Center for International Development and Conflict Management at the University of Maryland, College Park.

Ethnolinguistic Fractionalization: Probability that two randomly selected persons from a given country will not belong to the same ethnolinguistic group. Source: Mauro (1995).

Population over 65: Share of population aged over 65 in the total population. Source: Barro R. and J.W. Lee.

Regional Integration Agreements: Interaction between dummy variable for participation on regional integration agreements and partners' share of World Real GDP (%). Source: Berthelon data set (for RIAs) and Penn World Table 5.6, Global Development Finance, and World Development Indicators (for share of World Real GDP).

APPENDIX 3: LIST OF COUNTRIES

Europe	Americas	Asia	Africa	Oceania
Austria	Argentina	China	Algeria	Australia
Cyprus	Barbados	India	Benin	New Zealand
Denmark	Bolivia	Indonesia	Cameroon	
Finland	Brazil	Iran	Central African Republic	
France	Canada	Israel	Congo	
Greece	Chile	Japan	Egypt	
Hungary	Colombia	Jordan	Ghana	
Iceland	Costa Rica	Korea	Kenya	
Ireland	Dominican Republic	Malaysia	Malawi	
Italy	Ecuador	Nepal	Mali	
Malta	El Salvador	Pakistan	Niger	
Netherlands	Guatemala	Philippines	Rwanda	
Norway	Haiti	Singapore	Senegal	
Portugal	Honduras	Sri Lanka	Sierra Leone	
Spain	Jamaica	Syria	South Africa	
Sweden	Mexico	Thailand	Sudan	
Switzerland	Nicaragua	Turkey	Togo	
United Kingdom	Panama		Tunisia	
	Paraguay		Uganda	
	Peru		Zambia	
	Trinidad and Tobago			
	U.S.A			
	Uruguay			
	Venezuela			
(18)	(24)	(17)	(20)	(2)

APPENDIX 4: EXCLUDED RIA VARIABLES

The purpose of this appendix is to explore in greater detail the construction of the excluded RIA variable; it discusses alternatives RIA variables, alternatives measures of exclusion, and their appropriateness for the econometric analysis.

For expositional purposes I choose to create an example that will serve to explain some characteristics of the measures of exclusion that I am creating. This will illustrate their implications more easily and it will become clear why I use only one of them in the econometric estimation.

Let's assume first that we have a world with 10 countries, some of them with different sizes in terms of their shares of world GDP. The distribution of sizes for the countries of this world is presented in Table A.4.1. I have assigned a large share of world GDP to country 1 for the purpose of illustrating a crucial property of the variables that I will discuss below.

Next, let's assume that some of the countries in this world have joined some regional integration agreements. The network of agreements is displayed in matrix form—as pairs of agreements—in Table A.4.2.[70] The same information is also presented in Figure 1, in which each box represents a country, and the size of the box is an approximation to the size of each country in terms of its share of world GDP reported in Table A.4.1. As can be seen from Table A.4.1 and Figure A.4.1, there are two major

[70] Commonly, a regional integration agreement is referred to as an association between two or more countries. However, every RIA between more than two countries can be broken down to a set of bilateral agreements. In this section I will refer to an RIA as those bilateral agreements that may or may not be part of a larger agreement. Thus, a block such as the European Union can be seen as formed by a group of RIAs.

trading blocks, one formed by countries 2, 3, and 4, and the other formed by countries 7, 8, and 9.

Starting with this information, I want to construct the best indicator, for each country, of the extent of integration in the rest of the world, measured in terms of the size of world GDP that it is involved in the integration processes. An indicator as such requires two pieces of information, one regarding the matrix of agreements in the world, and the other is a measure of the extent of the world market that belongs to each agreement. In the present example, I have defined the former in Table A.4.1. The latter is not as straightforward as it may seem, and requires further discussion.

A first step in this direction consists in measuring the share the world GDP that is integrated whenever two countries join in an agreement. For instance, if countries 1 and 5 have an agreement I will sum the shares of world GDP of country 5 (0.05) and country 1 (0.45). With this data we could construct an indicator of the size of the world market that a country has been excluded from due to not participating in the integration process developed by other countries, by adding the size of all agreements to which each particular country does not belong.

Figure A.4.2 represents the agreements that would be considered in constructing this variable from the standpoint of country 5. The full lines joining two countries symbolize agreements that would be included in this variable. As can be seen, it would include all agreements signed in the world in which country 5 is not participating. Generalizing, such a variable would be expressed as follows:

$$Additive\,Excluded\,RIA_{jt} = \frac{1}{2}\sum_{\substack{i=1 \\ i\neq j}}^{N}\sum_{\substack{k=1 \\ k\neq j}}^{N}\left[D_{kt}^{i}\times\left(SWGDP_{i}+SWGDP_{k}\right)\right], \qquad (A.4.1)$$

where N is the number of countries in the world, D_{kt}^{i} is a dummy variable for an integration agreement between country i and k in period t, $SWGDP_i$ is the share of world GDP of country i, and the numerator is divided by two in order to eliminate double counting of all pairs of countries. With this variable a country would be more excluded with fewer agreements and/or smaller partners.

The next question to answer is: how sensitive is this measure of exclusion to an increase in the number of agreements in the world? Let's suppose that we want to see the effects of a new agreement between countries 1 and countries 2, 3, and 4. Given that countries 2, 3, and 4 already have an agreement, we could think of them as if they were acting as a block, such as the European Union, and given that country 1 is the largest country in the example, an agreement of this type might be akin to an agreement between the EU and the USA. Let's call this agreement *A*. In contrast, let's compare this agreement with a new one between countries 1 and countries 6, 7, 8, and 9. Again, countries 7, 8, and 9 already have agreements in place and are relatively smaller than countries 2, 3, and 4, a situation that could be paralleled, for instance, by the Southern Cone Common Market or MERCOSUR. Consequently this might be analogous to an agreement between MERCOSUR and the USA. Let's call this other agreement *B*.

I want to obtain a measure of the change in the additive excluded RIA when agreements *A* and *B* take place, and see if this measure reflects such agreements reasonably well. Table A.4.3 summarizes such an exercise, showing that agreement *A* would increase the additive excluded RIA variable for countries not in the agreement by 1.70. Agreement *B*, would increase the variable by 1.92.

Are such differences reasonable, given the size of the countries involved? If we look at the size of the economies involved in both agreements, we have that agreement *A* incorporates 80% of world GDP, and agreement *B* includes only 57% of world GDP. Thus, countries belonging to agreement *A* are 40% larger than agreement *B*, a difference that is not reflected in the numbers reported in Table A.4.3.

This occurs because in order to create the additive excluded RIA variable of equation 1, I am adding the shares of world GDP of pairs of countries, and if one country is signing more than one agreement—as country 1 is doing in the example above—its share would be counted a number of times equal to the number of agreements that it is joining, regardless of the size of the partner. In the case of agreement *A*, country 1 is counted 3 times, and for agreement *B* it is counted 4 times. Results in Table A.4.3 are a reflection of this multiple counting, where the total added share of world GDP due to agreement *B* is mostly driven by number of times that country 1 is added.

The main consequence of the multiple counting is that an excluded RIA variable as the one proposed in equation 1, which might be reasonable in theory, does not provide with realistic indices of the extent of the market from which countries are excluded when they do not participate in new RIAs.

A better alternative to equation one is to multiply the shares of world GDP of countries that are joining an agreement. In this way, each country's share of world GDP is weighted by its partner's size, ensuring that multiple counting is avoided. The new excluded RIA variable I propose is the following:

$$Multiplicative\ Excluded\ RIA_{jt} = \sum_{\substack{i=1 \\ i \neq j}}^{N} \sum_{\substack{k=1 \\ k \neq j}}^{N} \left[D_{kt}^{i} \times \left(SWGDP_i \times SWGDP_k \right) \right], \quad \text{(A.4.2)}$$

Table A.4.4 reports changes in the variable due to agreements *A* and *B* described above. As before, we should expect that agreement *A* increases the excluded variable more than agreement *B* does, and we can see that the multiplicative variable obeys this property: agreement *A* increases the excluded variable by 0.315, compared to an increase of 0.108 due to agreement B. In addition to generating a correct ranking in the effect, the size of the change in the variable seems to be reasonable, since the ratio of the two values is exactly equal to the ratio of the size of the partners of country 1 in those agreements. Thus, the multiplicative excluded RIA variable described by equation 2 is an appropriate measure of the extent of the market from which countries are excluded when third parties engage in regional integration agreements.

Even though I have already advanced the concept I want to capture with the excluded variable, there is an additional definition that it is useful to discuss with greater detail before proceeding, namely the definition of exclusion. The way in which I have defined exclusion is that it occurs for a given country whenever two or more other countries join a regional integration agreement.

However, there is also possible to create a more restrictive definition of exclusion that would eliminate those agreements signed by countries that also have agreements with country 5. Let's call this new definition the excluded RIA variable 2. Figure A.4.2 characterizes the agreements that would be included in this variable by a dashed line. As can be seen, excluded variable 2 does not include agreements between country 10 and any other countries, since country 5 has an agreement with country 10. The reason to create a variable of this type is that when countries are excluded from agreements joined by two or more other countries, the excluded country might reduce or eliminate such

exclusion indirectly through an agreement with at least one of the countries that signed the agreement. In the example, even though country 5 does not have an agreement with country 4, it might use country 10—with which does have one—to access country 4's market. In practice this might only happen if rules of origin do not exist, are loosely defined or unenforceable. Therefore, if we think that rules of origin do work in practice, this new definition of exclusion would not be necessary.

In general, excluded variable 2 is defined as follows:

$$Multiplicative\,Excluded\,RIA_{jt}^{2} = \sum_{\substack{i=1 \\ i\neq j}}^{N}\sum_{\substack{k=1 \\ k\neq j}}^{N}\left[D_{kt}^{i}\times\left(SWGDP_{i}\times SWGDP_{k}\right)\times(1-D_{jt}^{i})\times(1-D_{jt}^{k})\right],$$

(A.4.3)

where D_{jt}^{i} and D_{jt}^{k} are dummy variables for agreements between country *j* and *i*, and country *j* and *k*, respectively. If country *j* has an agreement either with country *i* or *k* the term in squared bracket is equal to zero, thus an eventual agreement between *i* and *k* is not counted.

Regression results including the first excluded RIA variable are reported in section 2.4.4.2. Results obtained introducing the RIA variable and the second excluded RIA variable point toward collinearity problems between these two variables. In these regressions, both RIA variables retain the sign they had when used alone: the RIA variable has a positive growth effect and the excluded RIA variable 2 has a negative impact. The difference is that both are not significant at any conventional level of significance. A possible explanation for lack of statistical significance of these variables when included simultaneously might be found in the high correlation they have. The decade correlations between the RIA variable and the excluded RIA variable 2 is above

0.95 in all decades, reaching -0.99 in two decades. The large correlation between these two variables generates collinearity problems that render the correct estimation of both coefficients impossible.[71]

[71] The correlation for the first excluded RIA variable is also high but smaller than the one for the second excluded RIA variable, and this difference seems to be enough to obtain significant estimates for the RIA variable.

Table A.4.1. Countries Share of World GDP

Country	Share of World GDP
1	0.45
2	0.15
3	0.10
4	0.10
5	0.05
6	0.02
7	0.04
8	0.05
9	0.01
10	0.03
Total	1.00

Table A.4.2. Matrix of Agreements

Country	1	2	3	4	5	6	7	8	9	10
1					Yes					
2			Yes	Yes						Yes
3		Yes		Yes						Yes
4		Yes	Yes							Yes
5	Yes									Yes
6										
7								Yes	Yes	Yes
8							Yes		Yes	Yes
9							Yes	Yes		Yes
10		Yes	Yes	Yes	Yes		Yes	Yes	Yes	

Table A.4.3. Changes in the Additive Excluded RIA Variable

Size of the change in the Additive Excluded RIA variable due to:

Agreement *A* : Countries 1, 2, 3, and 4.	Share	Agreement *B* : Countries 1, 6, 7, 8, and 9.	Share
Countries 1 and 2	0.60	Countries 1and 6	0.47
Countries 1 and 3	0.55	Countries 1and 7	0.49
Countries 1 and 4	0.55	Countries 1and 8	0.50
		Countries 1and 9	0.46
Total	1.70	Total	1.92

Table A.4.4. Changes in the Multiplicative Excluded RIA Variable

Size of the change in the Multiplicative Excluded RIA variable due to:

Agreement *A* : Countries 1, 2, 3, and 4.	Share	Agreement *B* : Countries 1, 7, 8, and 9.	Share
Countries 1 and 2	0.14	Countries 1and 6	0.02
Countries 1 and 3	0.09	Countries 1and 7	0.04
Countries 1 and 4	0.09	Countries 1and 8	0.05
		Countries 1and 9	0.01
Total	0.32	Total	0.11

Figure A.4.1. Network of Regional Integration Agreements

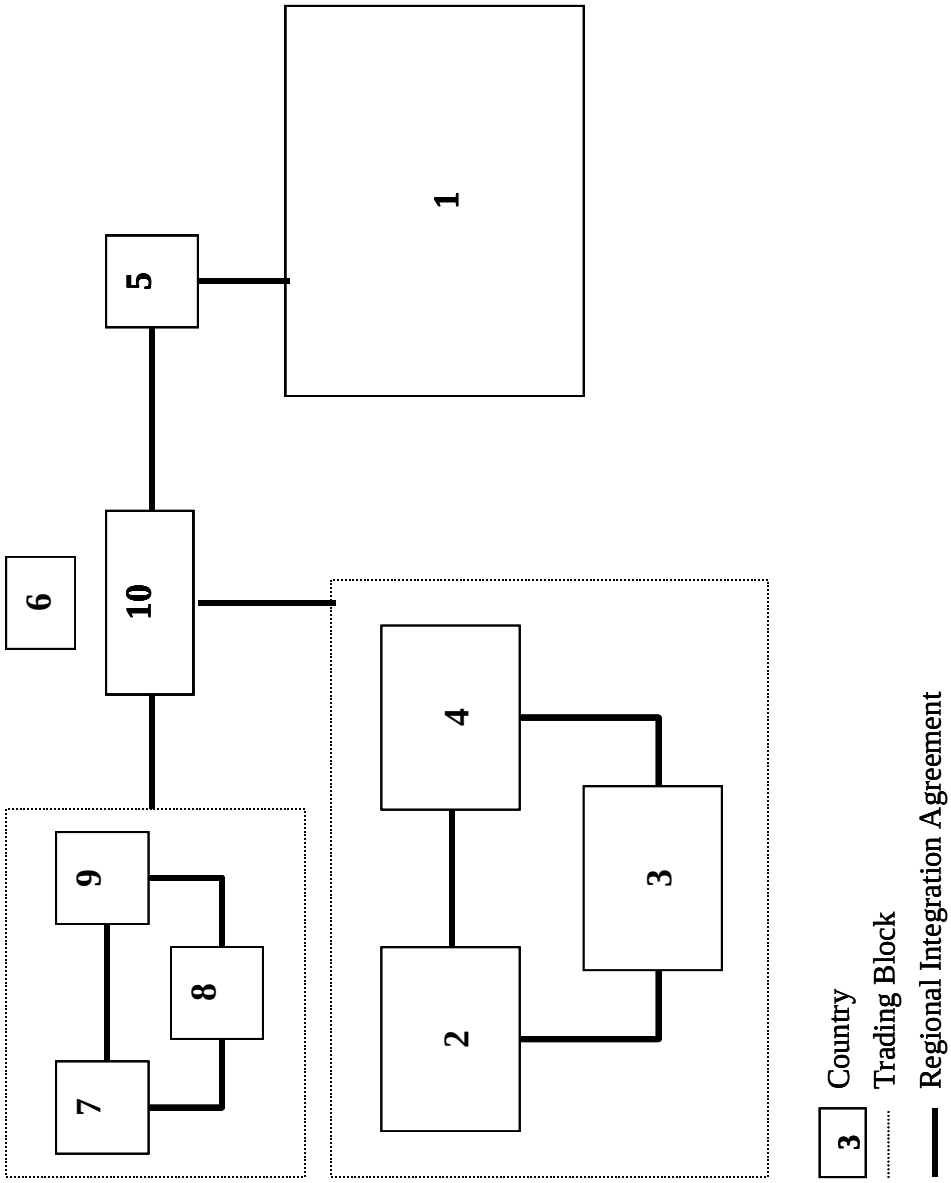
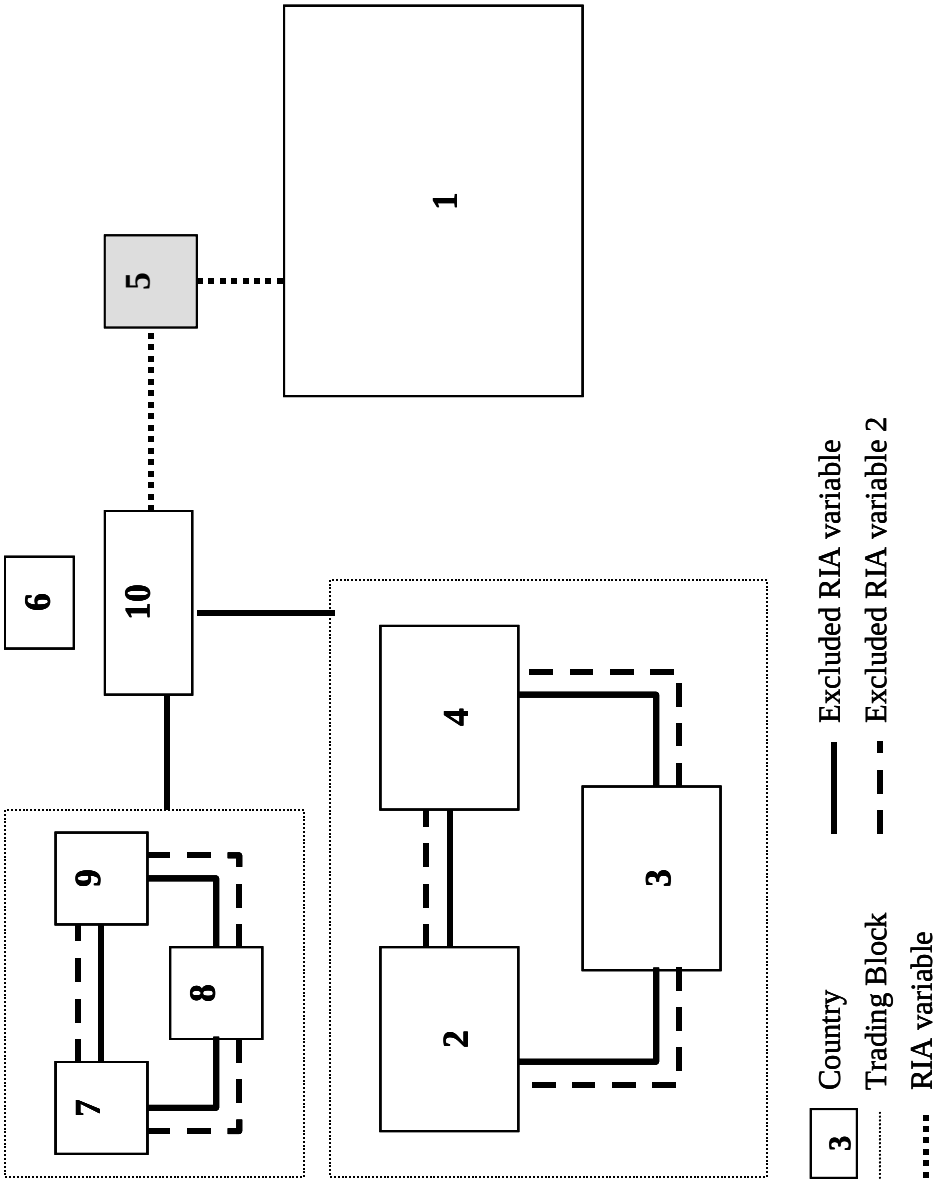


Figure A.4.2. RIA and Excluded RIA variables for Country 5



APPENDIX 5: VARIABLES AND SAMPLE CONSTRUCTION

A.5.1. OUTCOME VARIABLES

The sample is composed of all children aged 6 to 14. To reduce the measurement error in the sample I keep only children who have not completed primary education. This is done because there are some inconsistencies in the information regarding school attainment. For instance, some children report being enrolled in a primary school and at the same time they report having passed 6^{th} grade (the highest grade in Nicaragua's primary education system). Thus, I restrict the sample to children that report 5^{th} grade as the highest grade that they have completed. Starting from the sample generated with these two criteria, each particular outcome variable is defined and constructed as follows:

Enrollment: dummy variable equal to 1 when a child was enrolled in a primary school.

Education Gap: (Age – LMAFG) – HGC. LMAFG is the legal minimum age to be enrolled in first grade (7 years in Nicaragua) and HGC is the highest grade completed by the child. In other words, the education gap is the difference between the expected educational achievement for a child of a given age—the term in brackets—and the actual achievement of the child. In addition, the education gap is bottom-coded in order to eliminate negative values:

$$\text{Education Gap} = \begin{cases} (\text{Age} - 7) - \text{HGC} & \text{if } (\text{Age} - 7) - \text{HGC} \geq 0 \\ 0 & \text{if } (\text{Age} - 7) - \text{HGC} < 0 \end{cases}.$$

Child in correct grade for age: dummy variable that takes the value of 1 if the education gap is equal to zero, and 0 otherwise. Thus, as in the case of education gap, it is a measure of education advancement but unlike the education gap, this variable is binary.

Absenteeism: number of days not attended during last month for the following reasons: bad school teachers, lack of school teachers, school is in bad condition, school is too far away, child is not interested in going to school, and child dropped out of school. As some families report an implausible large of number of days not attending school in the last month, I restricted the maximum number of days to be 23 by recoding observed values above that mark to take the value of 23.[72]

Age in first grade: age of the child when enrolled in first grade.

Repeating Grade: binary variable for whether the child is repeating the grade in which he or she is currently enrolled.

Highest Grade Completed: number of years of completed education

A.5.2. Control Variables in the Regressions

Number of children aged 0-5: Number of children aged 0 to 5 living in the household.

Family size: Total number of family members living in the household, excluding domestic servants.

Female: Dummy variable equal to 1 if the child is female.

Time to the nearest primary school: Time in minutes to reach the nearest primary school.

[72] This change was done only to one child.

Rural household: Dummy variable equal to 1 if the household is located in a rural area.

Maximum years of education: Number of years of education of the adult with the highest level of education completed.

Mother works: Dummy variable equal to 1 if the mother of the child works.

Head of household is female: Dummy variable equal to 1 if the head of the household is female.

Total household Income: Total income of the household during the past year (in thousands of *córdobas*).

Owns house: Dummy variable equal to 1 if the household own the house in which it lives, with or without property title.

Household is poor: Dummy variable equal to 1 if the household is below the poverty line. The poverty line is defined by The World Bank at the level of consumption per-capita of 4,259 *córdobas* per year, which is approximately US$403 per year.

Household is extremely poor: Dummy variable equal to 1 if household is below the extreme poverty line. The extreme poverty line is defined by The World Bank at the level of consumption per-capita of 2,246 *córdobas* per year, which is approximately US$213 per year.

School Classrooms in 1993: Number of classrooms available in the school in 1993.

A.5.3. VARIABLES IN PROBIT ESTIMATE OF THE PROPENSITY SCORE MATCHING.

Non-FISE projects HH benefited from: Number of projects not funded by FISE that the household reports having benefited from.

Non-FISE projects HH benefited/participated: Number of projects not funded by FISE that the household reports having benefited from and participated in.

Number of Organizations HH belongs to: Number of organizations that household belongs to.

House has dirt access road: Access to household is through a dirt road.

Time to school: Time in minutes to reach the nearest primary school.

Municipality Poverty Rate: Rate of poverty at the municipality level.

Municipality Extreme Poverty Rate: Rate of extreme poverty at the municipality level.

APPENDIX 6: SCHOOL-LEVEL MATCHING COMPARISON GROUP

The school-level matching was done with the objective of generating a comparison group for the random sample of 24 FISE schools that were included in the treatment group. The idea is that the comparison group was to be composed of schools similar to those in the treatment group. According to The World Bank (2000), the matches were selected by choosing the closest non-FISE school (outside the influence area) of similar size and type.

Specifically, size was defined by the number of classrooms, and type as being in the same municipality, area (urban or rural), and municipal poverty category (categories for poverty at the municipality level are medium, high and extreme).

Table A.6.1 presents the information for each final pair of schools chosen in the construction of the comparison group. It indicates that of the 24 school pairs, 19 are located in the same municipality, 22 are located in municipalities with the same poverty level, and 20 are in the same area.

In terms of the number of classrooms in 1993, the average in FISE and non-FISE schools is 4.4 and 3.9, respectively. The difference of 0.5 classrooms is statistically insignificant. However, a closer look at the disaggregated data indicates that only 3 pairs of schools had the same number of classrooms in 1993, and only another 5 had a difference of one classroom (one classroom was the equivalent of 25% of the size of the average school).

The table also shows that between 1993 and 1998 there was a statistically significant increase in the average size of the FISE schools, but not of the non-FISE schools. The increase in the size of the FISE schools might be attributed to FISE

investment in these schools. Table A.6.2 reports the type of investments done by FISE projects on the 24 schools that comprise the treatment group. Even though in all but one project the main objective was school replacement and/or repair, and not an increase in school size, in practice the FISE projects might have had an impact on the available size of the school, as previously unused classrooms were repaired or replaced, thus expanding the functional space in the school.

Table A.6.1. School Matching

Matching by the same			Matching by School Number of Classrooms								
Municipality	Municipality Poverty[a]	Area[b]	FISE Schools			Non-FISE Schools			FISE - Non Fise		Differences
			93	98	Diff	93	98	Diff	93	98	Diff-Diff
No			3	10	7	11	12	1	-8	-2	6
			4	6	2	7	9	2	-3	-3	0
			14	15	1	17	17	0	-3	-2	1
No	No		3	4	1	5	6	1	-2	-2	0
No		No	1	4	3	3	3	0	-2	1	3
No			7	7	0	9	9	0	-2	-2	0
			3	4	1	5	8	3	-2	-4	-2
			1	3	2	3	3	0	-2	0	2
			1	2	1	2	2	0	-1	0	1
		No	2	2	0	3	3	0	-1	-1	0
			2	3	1	3	3	0	-1	0	1
		No	1	1	0	2	2	0	-1	-1	0
			1	1	0	1	1	0	0	0	0
			2	4	2	2	2	0	0	2	2
			1	3	2	1	1	0	0	2	2
			2	3	1	1	1	0	1	2	1
			3	4	1	1	1	0	2	3	1
			4	6	2	1	1	0	3	5	2
			6	9	3	3	3	0	3	6	3
No	No		8	8	0	3	2	-1	5	6	1
		No	7	10	3	2	1	-1	5	9	4
			7	7	0	1	1	0	6	6	0
			14	14	0	7	13	6	7	1	-6
			9	8	-1	1	2	1	8	6	-2
Average			4.42	5.75	1.33 ***	3.92	4.42	0.50	0.50	1.33 *	0.83 *
Std. Dev.			3.86	3.80	1.63	3.89	4.51	1.44	3.74	3.45	2.28

Source: Author calculations, INEC-Nicaragua and FISE.

* significant at 10%; ** significant at 5%; *** significant at 1%.

[a] Municipality poverty category are medium, high and extreme.

[b] Areas are urban and rural

Table A.6.2. Projects Funded by FISE in the Treatment Group ($)

	Investments ($)					
Type of Project	**Total**	**Repair**	**Enlargement**	**Replacement**	**Exteriors**	**Furniture**
Replacement	214,519	0	0	163,749	38,689	12,081
Replacement	100,695	0	0	87,950	4,206	8,540
Replacement	84,954	0	0	62,547	16,885	5,522
Replacement	62,744	0	0	44,323	13,273	5,147
Replacement	59,496	0	0	47,872	11,624	0
Replacement	47,050	0	0	39,665	4,066	3,320
Replacement	41,824	0	0	30,234	9,404	2,185
Replacement	40,287	0	0	33,178	4,660	2,448
Replacement	37,101	0	0	33,517	2,516	1,067
Replacement	35,857	0	0	24,928	8,248	2,681
Replacement	34,095	0	0	24,118	8,905	1,072
Replacement	29,189	0	0	21,957	5,806	1,426
Replacement	22,892	0	0	18,304	3,736	851
Replacement	19,377	0	0	16,404	2,448	524
Replacement	18,635	0	0	13,648	3,654	1,333
Replacement	15,625	0	0	11,600	3,080	945
Repair and Replacements	79,523	21,055	0	37,377	13,944	7,147
Repair and Replacements	75,415	26,648	0	31,028	12,388	5,351
Repair and Replacements	68,484	22,428	0	34,211	11,845	0
Repair and Replacements	43,958	13,304	0	18,179	8,766	3,709
Repair	76,643	59,114	0	0	9,810	7,718
Repair	17,151	9,182	0	0	5,505	2,463
Repair	8,814	4,596	0	0	2,030	2,189
Enlargement	152,400	0	149,308	0	3,093	0
	1,386,728	156,328	149,308	794,790	208,583	77,719
	100.0%	11.3%	10.8%	57.3%	15.0%	5.6%

Source: INEC-Nicaragua and FISE.

APPENDIX 7: MATCHING ESTIMATORS

Following Smith and Todd (2003), matching estimators can be represented in the following general form:

$$\hat{\alpha}^{M} = \frac{1}{n_1} \sum_{i \in I_1 \cap S_p} [Y_{1i} - \sum_{j \in I_0} W(i, j)\, Y_{oj}], \qquad \text{(A.7.1)}$$

where I_1 and I_0 are the set of program participants and non-participants, respectively, S_p is the region of common support, n_1 is the number of children belonging simultaneously to I_1 and S_p, and W(i,j) are the weights given to the non-participants. Equation (A.7.1) operates by estimating the average of the differences between the outcome of participant children (Y_{1i}) and the average weighted outcome of a set of comparable non-participant children ($\sum W(i, j)Y_{oj}$), where the weights depend on the matching estimation procedure.

Furthermore, define a neighborhood $C(P_i)$ for each person in the treatment group. Neighbors belonging to $C(P_i)$ are individuals in the non-treatment group for whom $P_j \in C(P_i)$. Thus, the set of neighbors of treated person *i* are those non-treated persons belonging to the set $A_i = \{j \in I_0 \mid P_j \in C(P_i)\}$. The matching estimators used in this paper—nearest neighbor, multiple near neighbors and kernel matching—differ in the way in which both the set A_i and the weights are defined.

Nearest neighbor matching uses for each treatment individual the non-treatment one with the closet probability of participation (P_i). Thus the neighborhood for person *i* is defined as:

$$C(P_i) = \min_j \left\| P_i - P_j \right\|, j \in I_0 \ . \qquad \text{(A.7.2)}$$

In the case of multiple nearest neighbors, I used three nearest neighbors, with each one receiving the same weight. Multiple nearest neighbors decreases the variance because it uses more information to generate the estimates, but increases the bias, as more distant neighbors and therefore poorer matches are included. Both nearest neighbor and multiple near neighbors matching were implemented with replacement. This means that observations in the comparison group can be used more that one time to obtain a match for each treated individual.

The kernel matching estimators generate an estimated counterfactual for each treated person by means of a kernel function that generates a weighted average over individuals in the non-treatment group. The weights in equation (A.7.1) are defined as:

$$W(i, j) = \frac{G\left(\frac{P_j - P_i}{a_n}\right)}{\sum_{k \in I_0} G\left(\frac{P_k - P_i}{a_n}\right)} \ , \qquad \text{(A.7.3)}$$

where G(.) is a kernel function and a_n is a bandwidth parameter. In my estimates I used an Epanechnikov kernel function and tried different values for the bandwidth parameter: beginning with a minimum of 0.005, I re-estimated the kernel matching estimator by increasing the bandwidth parameter by 0.005 each time, up to a maximum of 0.06. In this paper, I report results when the bandwidth parameter is set at 0.01, although there are no substantive differences with the results I obtained with the other values.

Results for the multiple nearest neighbor and kernel matching estimators are reported in Tables A.7.1 and A.7.2. They yield similar results to those obtained with nearest neighbor matching: no significant impacts of FISE can be found on educational

outcomes. An exception is highest grade completed, where the effect of FISE is marginally significant using multiple nearest neighbors matching.

Table A.7.1. Propensity Score Matching Impact Estimates
of FISE Investments on Educational Outcomes
Multiple Nearest Neighbors Matching

Outcome	Treatment Group Mean	Matched Comparison Group Mean	Impact	Standard[a] Error	*t* -statistic
Enrollment	86.81	85.43	1.39	4.464	0.31
Education Gap	1.19	1.33	-0.14	0.220	-0.63
Child in Correct Grade	46.15	42.21	3.95	6.826	0.58
Absenteeism	0.45	0.47	-0.02	0.425	-0.04
Repeating Grade	7.55	8.76	-1.21	3.589	-0.34
Highest Grade Completed	1.78	1.44	0.34	0.208	1.65
Age in First Grade	7.47	7.50	-0.03	0.395	-0.07

Source: Author calculations using FISE Survey.
[a] Bootstrapped standard errors based on 750 replications.

Table A.7.2. Propensity Score Matching Impact Estimates
of FISE Investments on Educational Outcomes
KernelMatching

Outcome	Treatment Group Mean	Matched Comparison Group Mean	Impact	Standard[a] Error	*t* -statistic
Enrollment	86.64	85.02	1.62	3.96	0.41
Education Gap	1.19	1.40	-0.21	0.22	-0.96
Child in Correct Grade	46.54	39.08	7.47	6.90	1.08
Absenteeism	0.46	0.28	0.18	0.32	0.57
Repeating Grade	7.76	9.88	-2.13	4.03	-0.53
Highest Grade Completed	1.76	1.48	0.28	0.20	1.42
Age in First Grade	7.61	7.53	0.08	0.51	0.16

Source: Author calculations using FISE Survey.
Bandwith equal to 0.01
[a] Bootstrapped standard errors based on 200 replications.

APPENDIX 8: WORLD BANK SAMPLE AND RESULTS

The World Bank (2000) and Pradhan and Rawlings (2002) estimated the impact of FISE by comparing the means of the treatment and comparison groups. This appendix applies the regression estimators of the present study to the sample they used.

There are some differences between the samples utilized in these two studies and the one I use (as described in Appendix 5). The main difference is that World Bank (2000) and Pradhan and Rawlings (2002) restrict their sample to children ages 7 to 12. The World Bank also defines some outcomes in ways that they do not justify: the education gap variable is defined as [Age – LMAFG – Highest grade completed] and bottom coded to eliminate negative values (as in my sample), where LMAFG is the legal minimum age to be enrolled in first grade; they used 6 years as the measure of LMAFG even though the legal age to enter first grade in Nicaragua is 7 years. Given that the education gap is bottom coded, the use of a different LMAFG affects the point estimates. It also affects the variable of children being in the correct grade-for-age, because the education gap is used as the base to define whether or not a child is in the correct grade.

For the absenteeism variable, World Bank (2000) and Pradhan and Rawlings (2002) included children who did not attend school for reasons that should not be included in order to estimate this variable, such as: vacations, weather conditions, illness, economic problems, and whether the child is working. These reasons are not related to school conditions and should not be used to measure the impact of FISE investments. In addition, some families report that their children did not attend school during the last month for an implausibly large number of days—some children reported being absent form school more days than the maximum number of school days that there are in any

month. World Bank (2000) and Pradhan and Rawlings (2002) did recode the variable to account for this measurement problem.

In what follows, I report results using the sample from World Bank (2000) and Pradhan and Rawlings (2002) in Tables A8.1 through A8.4, and the first column for each outcome is equivalent to the estimated differences in means that they reported. Although the point estimates are equivalent to their differences in means, the significance levels are different, as I estimate them using a procedure that is robust to heteroskedasticity. The results indicate that once heteroskedasticity is taken into account, no significant effects are observed in enrollment and in the education gap. There is still a statistically significant effect on the age at which children are enrolled in first grade.

The results reported remain unchanged when I include school-pair fixed effects and socio-economic and demographic covariates, and in general smaller point estimates are obtained when those covariates are included. The only exception is absenteeism, but since absenteeism includes reasons that should not be included in the context of this evaluation, the estimates for this variable are meaningless when their sample is used.[73]

The conclusions obtained using the World Bank sample are similar to the ones obtained with the sample I use in this paper. There are no significant and robust effects of FISE investments in primary schools on educational outcomes.

[73] In particular they included vacations, which comprise most (about 85 percent) of the absent days.

Table A.8.1. Outcome Regressions Using World Bank Sample

	Enrollment			Education Gap		
	(1)	(2)	(3)	(4)	(5)	(6)
FISE School	0.044	0.044	0.034	-0.226	-0.009	-0.002
	(0.032)	(0.036)	(0.032)	(0.164)	(0.147)	(0.119)
Number of children aged 0-5			-0.013			-0.124 *
			(0.028)			(0.074)
Family size			0.016			0.085 ***
			(0.010)			(0.031)
Female			-0.003			-0.409 ***
			(0.032)			(0.113)
Time to school			-0.003 *			0.005
			(0.002)			(0.004)
Rural household			-0.022			0.135
			(0.101)			(0.299)
Maximum years of education			-0.001			-0.070 ***
			(0.007)			(0.022)
Mother works			0.030			-0.106
			(0.041)			(0.130)
Head of household is female			-0.091 **			0.151
			(0.045)			(0.133)
Total household income			-0.007			-0.015
			(0.005)			(0.010)
Owns house			0.071			-0.363 **
			(0.059)			(0.172)
Household is poor			0.013			0.239
			(0.042)			(0.160)
Household is extremely poor			-0.132 ***			-0.227
			(0.050)			(0.195)
School classrooms in 1993			-0.014 *			-0.063 ***
			(0.008)			(0.030)
Age 8			0.017			0.598 ***
			(0.038)			(0.131)
Age 9			0.006			0.726 ***
			(0.046)			(0.155)
Age 10			0.063 **			1.494 ***
			(0.031)			(0.157)
Age 11			-0.073			1.830 ***
			(0.063)			(0.224)
Age 12			-0.188 ***			2.302 ***
			(0.069)			(0.229)
Constant	0.872 ***	0.793 ***	0.869 ***	1.706 ***	1.475 ***	0.892 *
	(0.027)	(0.093)	(0.130)	(0.117)	(0.285)	(0.456)
Observations	699	699	688	695	695	686
R-squared	0.004	0.04	0.18	0.005	0.23	0.55
F-statistic	1.89	1.27	1.34	1.90	7.05	14.34
P-value School-Pair		0.25	0.19		0.00	0.00
P-value Age			0.00			0.00

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.8.2. Outcome Regressions Using World Bank Sample

	Child in Correct Grade			Absenteeism		
	(1)	(2)	(3)	(4)	(5)	(6)
FISE School	0.005	-0.049	-0.064	-0.706	0.977 *	0.919 *
	(0.052)	(0.049)	(0.046)	(0.784)	(0.531)	(0.480)
Number of children aged 0-5			-0.032			0.273
			(0.029)			(0.243)
Family size			0.004			0.248 *
			(0.012)			(0.129)
Female			0.045			-1.515 ***
			(0.049)			(0.501)
Time to school			-0.001			-0.030
			(0.001)			(0.020)
Rural household			0.109			1.887
			(0.137)			(1.314)
Maximum years of education			0.012			0.081
			(0.008)			(0.084)
Mother works			0.096 *			-0.371
			(0.058)			(0.581)
Head of household is female			-0.068			-0.879
			(0.061)			(0.617)
Total household income			0.003			0.095
			(0.005)			(0.090)
Owns house			0.095			0.375
			(0.081)			(0.753)
Household is poor			-0.038			-0.427
			(0.073)			(0.597)
Household is extremely poor			0.033			-0.876
			(0.066)			(0.638)
School classrooms in 1993			0.016			-0.485 ***
			(0.014)			(0.132)
Age 8			-0.257 ***			0.782
			(0.080)			(0.690)
Age 9			-0.223 ***			-0.587
			(0.083)			(0.692)
Age 10			-0.406 ***			-0.085
			(0.080)			(0.751)
Age 11			-0.413 ***			-0.285
			(0.088)			(0.965)
Age 12			-0.403 ***			-0.526
			(0.079)			(0.728)
Constant	0.255 ***	0.277 ***	0.270	7.483 ***	5.569 ***	6.194 ***
	(0.034)	(0.091)	(0.177)	(0.556)	(1.453)	(1.971)
Observations	699	699	688	615	615	608
R-squared	0.00	0.19	0.30	0.002	0.62	0.67
F-statistic	0.01	6.52	5.24	0.81	322.22	79.12
P-value School-Pair		0.00	0.00		0.00	0.00
P-value Age			0.00			0.25

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.8.3. Outcome Regressions Using World Bank Sample

	Repeating Grade			Highest Grade Completed		
	(1)	(2)	(3)	(4)	(5)	(6)
FISE School	-0.039	-0.004	0.014	-0.186	-0.299 *	-0.064
	(0.033)	(0.035)	(0.032)	(0.194)	(0.178)	(0.131)
Number of children aged 0-5			0.006			0.163 *
			(0.022)			(0.088)
Family size			0.005			-0.124 ***
			(0.009)			(0.046)
Female			-0.061			0.463 ***
			(0.038)			(0.138)
Time to school			-0.001			-0.003
			(0.001)			(0.005)
Rural household			-0.093			-0.209
			(0.083)			(0.353)
Maximum years of education			-0.003			0.086 ***
			(0.005)			(0.025)
Mother works			0.019			0.198
			(0.046)			(0.171)
Head of household is female			0.033			-0.117
			(0.055)			(0.173)
Total household income			-0.003			0.010
			(0.003)			(0.012)
Owns house			-0.006			0.440 **
			(0.043)			(0.215)
Household is poor			-0.085 *			-0.297 *
			(0.046)			(0.178)
Household is extremely poor			0.039			0.308
			(0.059)			(0.216)
School classrooms in 1993			-0.006			0.072 **
			(0.007)			(0.034)
Age 8			0.043			0.388 **
			(0.070)			(0.151)
Age 9			-0.065			1.249 ***
			(0.056)			(0.178)
Age 10			-0.016			1.423 ***
			(0.062)			(0.179)
Age 11			-0.039			2.299 ***
			(0.067)			(0.327)
Age 12			-0.075			2.696 ***
			(0.064)			(0.258)
Constant	0.113 ***	0.133 *	0.252 **	1.896 ***	2.029 ***	0.188
	(0.026)	(0.074)	(0.108)	(0.138)	(0.404)	(0.504)
Observations	615	615	608	695	695	686
R-squared	0.004	0.10	0.17	0.003	0.17	0.56
F-statistic	1.38	2.33	1.16	0.92	4.80	12.10
P-value School-Pair		0.00	0.01		0.00	0.00
P-value Age			0.51			0.00

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.8.4. Outcome Regressions Using World Bank Sample

	Age in First Grade		
	(1)	(2)	(3)
FISE School	-0.662 **	-0.346	-0.301
	(0.300)	(0.240)	(0.248)
Number of children aged 0-5			-0.273 **
			(0.132)
Family size			0.146 **
			(0.064)
Female			-0.313
			(0.306)
Time to school			0.006
			(0.008)
Rural household			2.019 *
			(1.089)
Maximum years of education			-0.101 **
			(0.042)
Mother works			0.063
			(0.305)
Head of household is female			-0.245
			(0.455)
Total household income			-0.067
			(0.055)
Owns house			-0.127
			(0.526)
Household is poor			0.313
			(0.557)
Household is extremely poor			-0.381
			(0.550)
School classrooms in 1993			0.025
			(0.097)
Constant	8.604 ***	8.128 ***	7.491 ***
	(0.235)	(0.884)	(0.943)
Observations	161	161	159
R-squared	0.05	0.35	0.36
F-statistic	4.87		
P-value School-Pair		0.00	0.09

Robust standard errors in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%.

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