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Evidence from Rural Guatemala

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Abstract

Evidence on the impacts of a large-scale expansion in public preprimary education is limited and mostly circumscribed to high and middle-income countries. This paper estimates the effects of such an expansion on progression in primary school in rural communities of Guatemala. Combining administrative and population census data in a difference-in-difference framework, the paper examines a large-scale construction program that increased the number of preprimaries from around 5,300 to 11,500 between 1998 and 2005. The results indicate that the program increased by 2.1 percentage points the fraction of students that progress adequately and attend sixth grade by age 12. These positive effects are heavily concentrated among girls.

JEL classifications: I21, I28
Key words: Early childhood development, Preprimary education, Rural areas

1. Introduction

Many low and middle-income countries have recently devoted (or are considering devoting) significant amounts of public resources to universalizing access to preprimary education. Besides the potential positive impacts on child development, the provision of public preprimary education is attractive because it can be implemented by expanding public primary schools "downwards," which requires limited infrastructure investments and implies that it can be easily accommodated within existing government structures. Therefore, investments in early childhood development have often been directed to preprimary education (UNESCO, 2006).

Yet empirical evidence on the effects of a large-scale expansion in preprimary education is limited and mostly circumscribed to high and middle-income countries. Cascio (2009), using data from four decennial censuses and exploiting the state-by-state expansion of Kindergarten in the United States to estimate long-term impacts, finds that whites affected by the expansion are less likely to drop out from high school and to be incarcerated but no effects are found for blacks. Exploiting comparisons across siblings in Uruguay, Berlinski, Galiani and Mancorda (2008) find that preschool attendance generates large effects on the probability of attending school by age 15. Focusing on a large preschool construction program in Argentina, Berlinski, Galiani and Gertler (2009) find that attending a preprimary induces sizable increases in third-grade test scores and improvements in child behavior in class.

There are several reasons to recommend caution in extrapolating this evidence to poorer countries. Shifting a child from a poor home environment to attend a preprimary may have a lower opportunity cost in a setting where her mother's education is low. On the other hand, the quality of preprimary education may be considerably lower in less developed countries. The effects of preprimary enrollment on child development can potentially be negative because they are highly dependent on the quality of the center attended and the quality of maternal time (Baker, Gruber and Milligan, 2008; Almond and Currie, 2010). Yet it is for policymakers in poorer countries that empirical evidence on the effects of expanding access to preprimary education is likely to be especially relevant: in 2010, average gross enrollment in preprimary education in low-income countries was 15 percent, compared with 52 and 82 percent for middle- and high-income countries, respectively (World Bank, 2012).

We contribute to filling this gap in the literature by examining a large-scale preprimary expansion in rural Guatemala, where large segments of the indigenous population live in poverty. Because of the signing of the Peace Accords that ended 36 years of civil strife in 1996, the government embarked on an ambitious construction program that more than doubled the number of preprimary schools between 1998 and 2005 from approximately 5,300 to 11,500.

We combine longitudinal school-level administrative data from 1995 to 2008 with Population Census data from 1994 and 2002 matched at the community level to estimate the impact of opening a preprimary in a rural community on primary school enrollment and progression. Precise geographic information allows us to construct a panel of isolated rural communities that had one primary school during the period analyzed and no preprimary by the start of the construction program.

We estimate the impacts by exploiting the variation across these communities over time regarding the construction of preprimaries as well as by taking advantage of the fact that some cohorts within a community had access to a preprimary while others did not. By focusing on rural communities with only one primary school and studying various cohorts over time, we tackle the potential problem that the opening of a preprimary annexed to a primary school may affect the student composition attending the primary school. The research design we adopt is similar to the one used by Duflo (2001) and Berlinski, Galiani and Gertler (2009), but we can exploit sharper variation in the variable of interest because of the availability of disaggregated geographic information.

The results show no statistically significant effects on average enrollment rates. However, negative effects of about 4 percentage points are documented for children age 7. This negative effect disappears at later ages, suggesting that the opening of preprimaries in beneficiary communities produces a later entrance to primary education to a small fraction of children. The results also point to average positive effects on adequate progression (expected grade given age) of about 1.5 percentage points. These positive effects arise at age 9 and become stronger as children age. By age 12, the program seems to have increased the fraction that attend sixth grade by 2.1 percentage points (from a mean of 13.6 percent). Though these positive effects are important, they suggest that complementary actions need to be implemented to generate sizeable improvements in adequate progression.

The results are robust to controlling for differential trends at various geographic aggregation levels, introducing time-varying controls, and applying trimming and propensity score weighting techniques. Moreover, we provide additional evidence on the robustness of the results documenting similar pretreatment trends between communities that that had a preprimary constructed and those that did not.

We also explore whether the preprimary expansion had heterogeneous impacts across subgroups. We find that the positive effects on adequate progression in primary school are heavily concentrated among girls. Greater impacts for girls are of significance given the documented large impacts of mothers' education on fertility, children's health and the larger intergenerational transmission of education between mothers and children (Martín and Juárez, 1995; Glewwe, 1999; Black, Devereux and Salvanes, 2005).

The paper proceeds as follows. Section 2 provides institutional background and Section 3 describes the data employed in the empirical analysis. Section 4 outlines the research design, Section 5 presents results and Section 6 provides robustness checks. Section 7 concludes the paper.

2. Background

2.1. Preprimary and Primary Education in Guatemala

With a GDP per capita of $2,623 in 2009, Guatemala is considered a low middle-income country. Around half its 14 million inhabitants live in rural areas and a similar fraction is indigenous. There is significant inequality in the country in a context of high poverty and extreme poverty rates (51 and 15 percent, respectively) concentrated in the indigenous rural populations. Life expectancy at birth reached 70 years in 2009, compared with 73.3 years for the rest of the Central American countries. Similarly, the under-5 mortality rate is significantly higher in Guatemala compared with the rest of Latin America and the Caribbean (39.8 and 22.5 deaths per 1,000 live births, respectively).[1]

Illiteracy is high: 23.7 percent of individuals aged 18 to 49 have no formal education (Living Standards Measurement Survey, 2006). Primary school coverage is practically universal, though the quality of education is considered low. Alvarez and Schieflbein (2007) find that Guatemalan teachers use inadequate teaching methods given the cultural barriers and socio-demographic context. Primary school repetition rates are high, especially during the initial grades

[1] Statistics obtained from http://data.worldbank.org/country/guatemala.

(for example, in first grade they reach 30 percent). As a result, Guatemala has one of the lowest average accumulated years of education in Latin America (Calderón and Urquiola, 2006).

Several factors may contribute to such poor performance. Almost 40 percent of children do not speak Spanish natively and tend to have poor academic performance relative to native-Spanish speakers. In addition, parent's education is low in rural areas, and anecdotal evidence suggests that education is not greatly valued in this context because a child's aspiration is to work in agriculture-related activities or housework (Rodríguez, 2001). Low levels of income seem a factor when deciding whether to enroll or withdraw a child from school (Alvarez and Schieflbein, 2007). Finally, studies have shown that the high rates of malnutrition are associated with a 50 percent larger probability of dropping out, and double the chances of repeating the grade (Martínez and Fernández, 2007).

Primary education in Guatemala has been compulsory since 1985. However, parents have to pay a small fee (between $0.60 and $5 for tuition) to cover operational costs related to running the school (for example, electricity bills). Preprimary attendance is compulsory as well, though this not enforced because of low coverage. It covers children aged 4 to 6. First grade is typically started at age 7, though this is not strict.

2.2. Preprimary Construction

In 1996, the signing of the Peace Accords ended 36 years of harsh civil conflict. As part of the Accords, the national government agreed to expand basic education and health services in rural and indigenous areas with inadequate coverage. In education, the government aggressively expanded preschool coverage resulting in increasing the number of public preprimaries from approximately 5,300 in 1998 to 11,500 in 2005 (see Figure 1). Preprimaries were usually constructed as annexes to primary schools.

According to former government staff, the selection of beneficiary communities was a two-step process. In the first step, agents at the departmental offices of the Ministry of Education identified eligible communities as those that have enough number of preprimary school aged children that lacked adequate access.[2] As a result, lists of eligible schools were constructed at the regional level. In the second step, final decisions were made at the central level. The procedures were ad hoc and no strict guidelines were applied to make decisions on beneficiary schools.

[2] Guatemala is divided into 23 departments and 330 municipalities (analogous to states and counties in the US).

3. Data

We use school-level administrative data obtained from the Ministry of Education for all primary schools from 1995 until 2008. At the start of the school year, each operating educational establishment (for example, preprimary, primary, high school) has to send information on initial enrollment by grade, age, and sex. Because financing is tied to receiving this information, non-response is low (about 1 percent).

We also accessed 100 percent samples of the Population Census data for 1994 and 2002. The data contains basic socio-economic characteristics at the household and individual level. Important for our analysis, the geographic location of the household is identified at the community level and it is possible to match communities from the 1994 to the 2002 Population Census. [3] Additionally, the National Institute of Statistics provides geo-coordinates for communities in the 2002 Population Census.

4. Research Design

4.1. Sample Construction

The empirical strategy focuses on a sample of isolated rural communities that had a primary school during the analyzed period. It exploits variation across cohorts within communities on access to preprimaries to assess the impacts on enrollment and progression rates.

The starting point in the empirical exercise is the sample construction. We apply four requirements to select the communities for the empirical sample. First, we only include communities that have population between 300 and 2,500 in 2002. This ensures that we are focusing on rural communities large enough to be potentially selected to have a preprimary constructed. Second, the communities need to have one primary school operating during the 1995 to 2008 period to be able to measure impacts in enrollment and progression. Third, we apply the restriction that there are no other communities located within a 0.5 kilometers radius of the community center. Focusing on isolated communities is motivated to reduce the possibility that the opening of a preprimary in one community would induce individuals in neighboring

[3] We can match around 82 percent of communities that account for 91 percent of the population. Correlation between average community characteristics is high.

communities to start attending the opened preprimary school.[4] Fourth, we drop communities that had a preprimary opened by 1997 to have a homogeneous group of communities, in terms of no baseline access to preprimary services, and to check trends during the pretreatment period.[5]

We next proceed to define the key variables in the empirical exercise. We define treatment status at the community-cohort level. Children in a cohort are considered treated if there was a preprimary in their community when they were aged 6. We measure impacts of access to preprimaries in two outcomes: enrollment and adequate progression rates. We restrict our sample to include cohorts born between 1988 and 1996 to observe outcomes during the whole relevant age range (7-12) for each included cohort. This allows that differences in estimated effects across ages are not driven by differences in cohort composition across observed ages instead by average dynamic effects for the observed cohorts.

For both measures, the denominator corresponds to population counts by community-cohort-age. We construct these counts by using data on population by community and age from the 1994 and 2002 Population Census and assuming that cohort sizes remain constant over time. For example, the number of students age 6 in 2000 in a community is assumed to be equal to the number of students age 8 in 2002 in that community. We compute counts for cohorts 1988-1989 using the 1994 Population Census and for the 1990-1996 cohorts we use the 2002 Census. We emphasize the use of the 2002 Census because it is closer to the period of analysis (1995-2008). However, the assumption of constant cohort sizes by community over time becomes problematic when focusing on children aged 13 and older in the 2002 Population Census: in these rural communities students might migrate to attend secondary education. This is why we compute cohort sizes for 1988 and 1989 cohorts from the 1994 Population Census.[6]

The numerator of the enrollment rate is the count of primary school students by community-cohort-age. The numerator for the progression rate corresponds to the number of students progressing adequately given their age. For example, for those aged 8, this includes all

[4] This type of spillover effects would bias down the estimated coefficients. We consider that spillovers are minimized in this context because of the rugged geography and poor infrastructure that makes it difficult for children in non-beneficiary communities to attend preprimaries in treatment communities. Anecdotal evidence also suggests that cultural factors related to a strong sense of belonging to living in a specific community reduce the possibility of seeking services outside one's community. Finally, the fact that young children had to travel by themselves to attend preprimaries beyond their own community makes this possibility less likely.
[5] In Section 6 we explore the robustness of the results to relaxing these restrictions.
[6] Results are robust to using 2002 Population Census for these cohorts instead and also to dropping them from the sample.

students in second or higher grades. The unit of observation of the resulting data set is at the community-cohort-age level. This data structure allows analyzing impacts at ages 7 to 12.

Our empirical analysis is based on comparing changes in outcomes between later (exposed) and earlier cohorts in treatment communities versus changes in comparison communities. For both sets of communities, we have information for pretreatment (earlier) cohorts, that is, children born between 1988 and 1991. Table 1 presents summary statistics. The top panel documents that pretreatment enrollment and progression rates are significantly higher for schools in treatment compared with those in comparison communities. It also shows that enrollment rates are high and close to 1 whereas adequate progression rates are much lower and decreasing with age.[7] The bottom panel shows that there are some differences in observed socio-demographic variables between both groups but they tend to be small. These differences in pretreatment outcomes levels and baseline characteristics can be accommodated in the differences-in-differences framework that we adopt and that it is presented next.

4.2. Empirical Models

This subsection describes the empirical models used to estimate the effects of expanding access to preprimary services in rural communities on primary enrollment and progression rates. The unit of observation of the constructed data set corresponds to the community-cohort-age level. Outcome variables are defined at this level and the treatment dummy is defined at the community-cohort level. The baseline specification is a parsimonious differences-in-differences model that exploits the variation in access to preprimary services within communities across cohorts to identify impacts. The following regression model is estimated:

$$Y_{c,b,a} = \beta \cdot Access_{c,b} + \lambda_c + \delta_b + \varepsilon_{c,b,a} \quad (1)$$

where $Y_{c,b,a}$ is the outcome of interest for the community c, birth cohort b at age a. The coefficient of interest (β) estimates the "intention-to-treat" parameter measuring the average effect of having access to a preprimary school on the analyzed outcomes. Fixed-effects by

[7] About 12 percent of schools in the original sample present average enrollment higher than 1.5. We have dropped these observations because they suggest a problem with the matching of school and communities. Still, the table shows that average enrollment rates for the analyzed sample are higher than 1 in certain cases. This suggests some over-reporting of enrollment in schools or under-reporting in population counts. As long as this measurement error is not correlated with the introduction of preprimaries over time, it should not bias our estimates. We further explore this issue in Section 6.

community (λ_c) and birth cohort (δ_b) are included to account for time invariant heterogeneity in the treatment and comparison communities. Because it is expected that errors are correlated within community over cohorts, in all regressions we cluster standard errors at the community level.

The identification assumption of the baseline specification is that outcomes in communities where preprimaries were opened would have evolved similarly as in those without openings. If departments targeted in the preprimary expansion program benefitted from other public programs that could potentially affect the outcomes of interest, the estimated effects generated using specification (1) would be biased. Moreover, if shocks in outcomes at the department level were correlated with preprimary openings (for example, if departments with worse trends in outcomes were targeted for compensatory programs such as preprimaries expansions), estimates would also be biased. We can tackle these potential threats to identification by adding to the baseline specification interactions of department and year. These interactions will flexibly control for changes in outcomes in departments over time that may bias the original specification. In this second specification, the regression model estimated is the following:

$$Y_{c,b,a} = \beta \cdot Access_{c,b} + \lambda_c + \delta_b + \gamma_{d,t} + \varepsilon_{c,b,a} \qquad (2)$$

where $\gamma_{d,t}$ correspond to department-year interactions and *d* and *t* index departments and years.

Still, it is possible that some bias would affect the estimated effects if there were selection into the program within departments, at the municipal level, and there are differential trends over time for municipalities with varying program participation. To account for this possibility, we add to the baseline specification linear controls for municipality-specific trends. The resulting regression model is specified by the following equation:

$$Y_{c,b,a} = \beta \cdot Access_{c,b} + \lambda_c + \delta_b + t \cdot \phi_m + \varepsilon_{c,b,a} \qquad (3)$$

where *t* corresponds to years elapsed after 1995, ϕ_m represents municipality indicator variables and, thus, the interaction would capture municipality-specific trends over time.

We additionally exploit data on the prevalence of households with running water, concrete floor and whose heads have no education and are indigenous from the Population Census from 1994 to 2002 to generate community-year covariates by linear extrapolation. We add these covariates to the baseline specification to control for observable changes at the community-level over time. The estimated model is:

$$Y_{c,b,a} = \beta \cdot Access_{c,b} + \lambda_c + \delta_b + \kappa \cdot \text{Covariate}_{c,t} + \varepsilon_{c,b,a} \qquad (4)$$

where $\text{Covariate}_{c,t}$ is a vector of covariates defined at the community-year level and κ is the associated vector of coefficients to be estimated.

In the final specification (5), we apply trimming and propensity score reweighting techniques to increase the similarity in observable dimensions of communities in the treatment and comparison groups. We implement this empirical approach by first predicting the probability, using a logit regression, that a preprimary is opened in a community as a function of pre-intervention school and community characteristics. Then, we drop communities with probability of treatment above the 85^{th} percentile or below the 15^{th} percentile. Finally, we reweight observations in the comparison group applying a factor of *PropScore/(1-PropScore)* where *PropScore* refers to the estimated probability of treatment and estimate the basic specification in the trimmed and reweighted sample.

So far, we have been estimating intention-to-treat (ITT) parameters of the impact of opening preprimaries in rural communities on school progression. To estimate the effect of *attending* a preprimary, the treatment-on-the-treated (ToT) parameter, we calculate the preprimary take-up rate for each cohort at age six and use an instrumental variable approach to tackle the endogenous nature of this attendance rate. In the first stage, we regress the take-up rate on the preprimary access indicator defined at the community-cohort level. We find that constructing a preprimary increases preprimary average enrollment by about 59 percentage points for a given cohort at age 6. In the second stage, we estimate the model described above but specifying the fraction of children attending the preprimary at the cohort-community level as the main independent regressor and instrumenting this variable with the preprimary access dummy.

5. Results

5.1. Main Impacts

In this subsection, we examine how opening preprimaries in rural communities affect primary school progression. Table 2 present Intention-to-Treat (ITT) estimates of the average effect of providing access to a preprimary in a rural community. The baseline specification described above is presented in column 1. We find no statistically significant effects associated with having access to a preprimary on enrollment rates (Panel A). However, we do find statistically significant positive effects on adequate school progression (Panel B). Results indicate that opening a preprimary in these rural communities produced an increase in the fraction of children in the expected grade given their age of approximately 1.5 percentage points.

In columns 2 to 5, we test the robustness of these results using alternative specifications. Column 2 presents results when controlling for interactions at the department-year level and column 3 when controlling for differential municipality linear time trends. We exploit the matched Census 1994 and 2002 data and control for time-varying controls in column 4. Finally, results in column 5 are obtained after applying trimming and propensity score reweighting techniques to increase the similarity of treatment and comparison communities. Baseline estimates are robust to these alternative specifications except for the positive effects on adequate progression that are no longer statistically significant (though point estimates are similar).

We next examine the Treatment-on-the-Treated (ToT) average effects of attending a preprimary on primary school progression. Table 3 presents the results. Because the average take-up rate is about 59 percent, the estimated effects are roughly 1.66 times the ITT estimates previously presented. Results indicate no significant effects on enrollment rates but a positive statistically significant effect on adequate progression of about 2.5 percentage points. As before, the results are robust to changes in the empirical specification used.

Expanding access to preprimary education seems to improve adequate progression but not changing primary enrollment rates, which suggests that repetition rates are reduced by this intervention. This is important in Guatemala, where repetition rates in early grades are high (about 30 percent in first grade and 10-15 percent in later grades). However, the magnitude of the effect on the adequate progression rate of about 1.5 percentage points seems modest compared with the overall mean (22 percent). On the other hand, lack of effects on primary enrollment could have been expected given that the overall baseline coverage is high. Still, if improvements in adequate progression are signaling that children are better prepared to the challenges faced in primary education, the intervention may produce reductions in drop-out in higher grades and hence the effects on years of completed education might be larger, an issue that we further explore below when analyzing impacts by students' ages.

The positive results in adequate progression are in line with existing evidence from Argentina that documented sizeable effects of preprimary attendance on test scores in primary school (Berlinski, Galiani and Gertler, 2009). In Uruguay, attending preschool was associated with a large increase in the probability of attending school at age 15 of 27 percentage points (Berlinski, Galiani and Manacorda, 2008). However, as mentioned above, extrapolating these positive findings to rural contexts in developing countries such as Guatemala is not straightforward. On one hand, expected household stimulation in skills conducive to high academic achievement might be significantly worse in Guatemala than in Argentina and Uruguay. For example, average mothers' education in the Uruguay study amounted to 9.8 years, whereas in the communities included in our study average education for women aged 20 to 40 is only 3.2 years. On the other hand, several reports suggest that the quality of preprimary education in Guatemala is low, and this could attenuate any positive benefits related to attending a preprimary (UNICEF, 1996; Rubio, Fernando and Chávez, 2001).

5.2. Heterogeneity of Impacts

In this subsection, we explore heterogeneous impacts by age and across groups defined by gender, school size, prevalence of indigenous population and adult education levels in the community.

Table 4 presents effects by age. Panel A shows evidence of interesting dynamic effects on enrollment. Negative significant effects, of about 4 percentage points, are estimated at age 7. These negative effects decrease in magnitude and become not significant by age 8 and completely dissipate for ages 9 to 11. By age 12 coefficients become positive though not significant. These patterns could be explained by an average null effect across ages but with negative effects arising at early ages because of parents and teachers deciding to enroll a few seven-year-old children, potentially deemed not ready to start primary school, in preprimary in treatment communities. These children in comparison communities could only be enrolled in primary and this could explain the negative effects on primary school enrollment by age seven.

Panel B also shows evidence of more positive effects at older ages for adequate progression, with estimated effects close to 0 at age 8. Effects rise for older ages and become significant by age 10. Results indicate that by age 12, the expansion of preprimaries produced an increase in about 2.1 percentage points in the fraction of children progressing adequately (significant at the 1 percent level). This suggests that reduced repetition produced by attending preprimary can more than compensate for the potential effect of later entrance to primary schools generated by the openings and generate, by age 12, a positive effect on adequate progression. These dynamic effects on school progression suggest that focusing on short-term measures at early ages, at least for enrollment and adequate progression indicators, may provide an incomplete picture of the full impacts of providing preprimary access.[8]

Do effects differ across subgroups? Identifying heterogeneous impacts across subpopulations can provide useful guidance on targeting preschool-expansion programs to maximize expected effects. For clarity of exposition, Table 5 presents only estimated effects for the baseline specification and controlling for shocks at the department-year level (columns 1 and 2). Panel A presents average effects (presented previously in Table 2). Panel B shows results from regression where the relevant explanatory variables are a treatment indicator and the interaction of this variable with a dummy for girls. Similarly, in Panel C the treatment indicator is interacted with a dummy that signals that the school has a higher enrollment than the median school. Finally, Panels D and E present results from analogous regressions when splitting the sample by prevalence of indigenous population and by the share of adults with no education.

There are two noteworthy findings. First, there is not much evidence that effects on enrollment rates vary by the studied dimensions. Second, our results suggest that the positive effects on adequate progression are concentrated on girls. Estimated effects for boys point to a (not significant) effect of about 0.6 percentage points whereas the interaction term of treatment and female dummy is 1.7 percentage points and statistically significant. We cannot provide a definite explanation for this pattern; anecdotal evidence, however, suggests that girls in these contexts enjoy less enriching stimulation at home than boys. This could explain larger effects for girls who may suffer a lower opportunity cost, in terms of forgone opportunities from stimulation

[8] Moreover, because we are mostly interested in ultimate educational attainment, the most relevant indicator may be the share of students who have progressed adequately and attend sixth grade by age 12.

from home, when they start attending a preprimary.[9] These results match previous evidence from the United States that document larger effects of preschool programs for girls (Oden et al., 2000; Anderson, 2008; Cascio, 2009). However, evidence on stronger effects on girls is not present in the referred studies for Argentina and Uruguay (Berlinski, Galiani and Manacorda, 2008; Berlinski, Galiani and Gertler, 2009).

6. Robustness Checks

This section explores the robustness of the empirical findings. We first tackle specific issues with the empirical strategy followed. We then test whether there are pretreatment differential trends between treatment and comparison schools to gauge the basic identification assumption in the difference-in-difference framework used.

6.1. Challenges to the Identification Strategy

In this subsection, we check the robustness of the main results to three potential issues about the empirical strategy followed. First, spillover effects from treatment to comparison communities may bias the estimated effects. As mentioned, we consider that this possibility is minimized because of the high real and psychological costs for children of one community to attend a preprimary in another community. We tackle this issue in the main empirical strategy by focusing on communities with primary schools that did not have a neighboring school within a radius of 0.5 kilometers. Restricting the sample to communities that are more isolated may reduce even more the possibility of these biases but at the cost of a reduction in the sample size and hence of the estimates' precision. We explore this issue by checking the robustness of the presented results to focusing on these more isolated communities. In columns 3-4 and 5-6 of Table 6, we replicate the main results presented earlier but now focusing on communities that do not have a neighboring school within 0.75 kilometers and 1 kilometer, respectively (in odd columns, we use the baseline specification and in even ones we control for interactions between departments and years). As expected, results are less precise and they are not significant when using the larger radius but point estimates still point in the same direction as in the main results.

A second issue is whether the results are robust to including all communities in the sample that did have a preprimary by 1997. Focusing on communities without a preprimary by

[9] Average take-up rates are similar across boys and girls suggesting that differential attendance may not explain the documented heterogeneity in impacts.

that time allowed having a more homogeneous sample and the possibility of checking differential pretreatment trends for beneficiary communities. We now check whether the results are robust to focusing on all communities without imposing the mentioned restriction. Results are presented in columns 7 and 8. They again indicate a positive statistically significant effect of preprimary opening on adequate progression, though the results on average enrollment suggest a small negative (statistically significant for one specification) effect.

A final challenge relates to the measurement error that seems to be present in the outcome variables, especially for enrollment rates. About half of the communities in the sample present average enrollment rates for the pretreatment period above 1. A substantial fraction of schools with average enrollment rate above one could arise if mean coverage is close to one, and there is some noise in this measure because the ratio is constructed from two data sets. However, the fact that the average rate across schools is above one at certain ages does suggest that there is some over-reporting of students enrolled or under-reporting of the number of children in the communities in the sample. If measurement error in these variables is classical and uncorrelated with the introduction of preprimaries, it will only induce larger standard errors. However, we cannot check this assumption. Hence, to gauge this issue we check if we can replicate the main results by focusing on those schools that in the pretreatment period had average enrollment below 1 (columns 9-10). Results indicate, again, no effects in enrollment rates but positive statistically significant impacts on adequate progression rates similar to baseline estimates. Taken together the results presented in this subsection suggest that the main findings are robust to the issues analyzed.

6.2. Testing for Differential Pre-Intervention Trends

Identification of the parameter of interest relies on the assumption that, in absence of the treatment, outcomes in the intervened primary schools would have evolved similarly to those from the comparison group. Although this assumption cannot be tested directly, we can still provide some evidence on its validity by studying pre-intervention trends (Heckman and Hotz, 1989). To do so, we perform a falsification test: we drop all cohorts that had access to a preprimary. We create a placebo indicator that takes the value of 1 for cohorts born one or two years before the first exposed cohort in treatment communities. For example, if the 1995 cohort was the first to be exposed to a preprimary in certain community, then the placebo indicator would take the value of 0 for cohorts 1990 to 1992, 1 for the 1993 and 1994 cohorts and cohorts 1995 and 1996 will be dropped.

In Table 7 we reproduce baseline estimates in columns 1 and 2. In columns 3-4, we use the placebo indicator when turning the indicator on two years prior and in columns 5-6, we repeat the exercise focusing on schools with average pretreatment enrollment below one. Results indicate that pretreatment trends are similar between the treatment and comparison communities providing additional evidence to the basic identification assumption used.

7. Conclusion

Many developing countries have expanded (or are considering expanding) preprimary coverage as an instrument to stimulate human capital accumulation. Evidence from the United States, Uruguay and Argentina suggests that these investments yield large returns (Cascio, 2009; Berlinski, Galiani and Manacorda, 2008; Berlinski, Galiani and Gertler, 2009). But this evidence cannot be easily extrapolated to poorer countries, where the potential for policy interventions in this domain is much greater.

This paper has contributed to filling this gap in the literature by exploiting a large-scale expansion in preprimary coverage in rural Guatemala from 1998 to 2005. Combining administrative and population census data in a difference-in-difference framework, we have estimated the impacts of opening a preprimary on enrollment and adequate progression in primary school. We find no effects on enrollment, though we do find positive statistically significant effects on the fraction of students attaining the expected grade given their age. At later ages (namely, 12 years), the positive results on adequate progression are larger and some positive effects on enrollment arise, suggesting that benefits in these measures accumulate over time. We find that positive effects in adequate progression are more pronounced for girls and also document some positive effects on enrollment for communities inhabited by indigenous populations and where average adult education is lower.

The average positive effects in adequate progression amount to about 2 percentage points. These effects can be seen as modest compared with the average adequate progression rate (22 percent). Hence, they suggest that tackling the problem of high repetition rates will require significant complementary actions and a significant improvement in the quality of primary education. We document that take-up rates are about 59 percent, suggesting that programs that can boost attendance may be a valuable policy option. For example, adding to existing cash transfer programs a condition on preprimary attendance might be an inexpensive way to improve the impacts generated by both social programs. And if additional payments may be necessary, they may be structured to focus on bolstering female attendance given the evidence found in this study that suggests that effects might be larger for girls.

One limitation of the paper is that it does not analyze impacts on test scores, because of data limitations. Nonetheless, examining impacts on enrollment and adequate progression is interesting for several reasons. First, under the assumption that students below a specific threshold are assigned to repeat the grade, increases in adequate progression signal improvements in learning, at least for students in the margin of failing. Second, the accumulated evidence from preschool evaluations in the United States suggests that impacts on test scores tend to fade out rapidly, whereas children's average positive effects on school progress and other non-cognitive outcomes remain or even arise only in adolescence and later (Almond and Currie, 2010). Hence, impacts on promotion can indicate effects on accumulated years of schooling in the absence of changes in age at school exit. Third, in many developing countries high rates of repetition and school dropout and re-entry causes increased expenses and problems associated with heterogeneity in the classroom. Hence, evaluating programs that can alleviate these issues is also desirable.

Future studies using individual-level data and experimental designs may provide more definitive answers about the impacts of expanding preprimary coverage. Still, evidence from large-scale expansions most surely will be generated using non-experimental approaches that exploit significant policy shifts like the one examined here. Both approaches together will be informative on effective ways to increase human capital in less developed countries by optimally investing in early childhood development.

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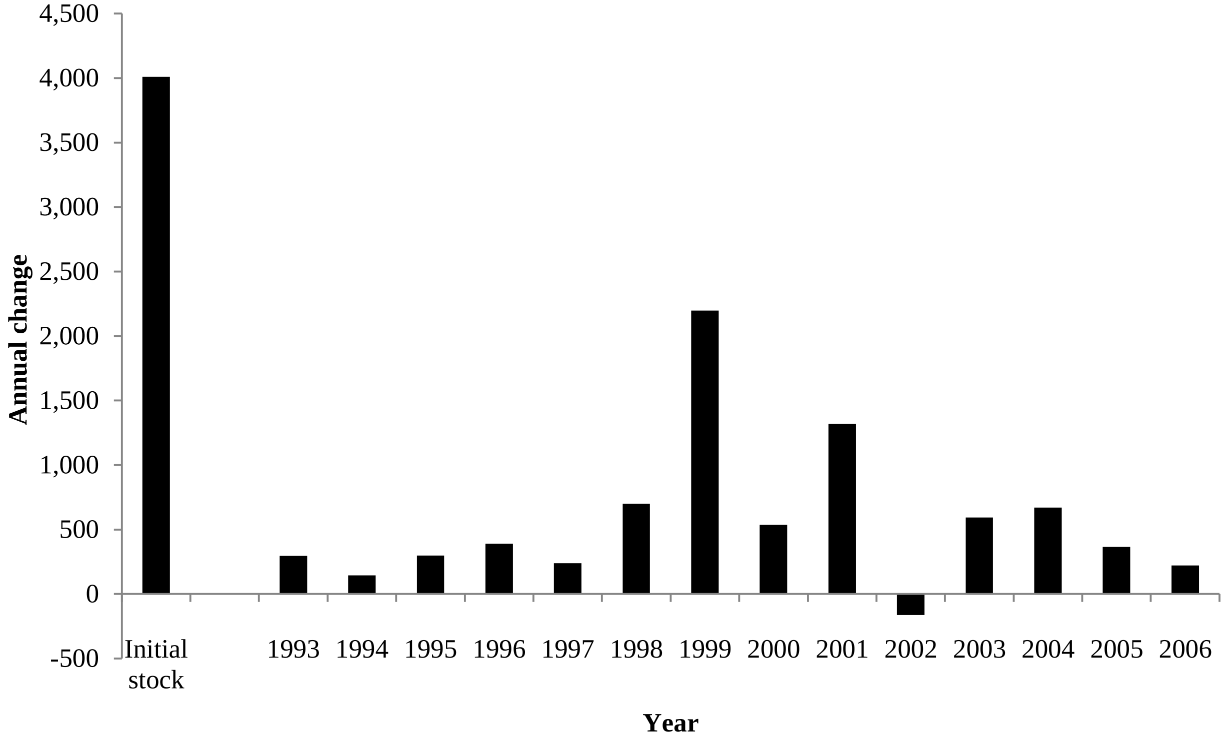
Figure 1. Annual Change in the Stock of Preprimaries



Notes: This figure presents the annual change in the stock of preprimaries in Guatemala. For comparison purposes, the leftmost bar plots the stock of preprimaries in 1992.

Table 1. Sample Descriptive Statistics: Pre-Treatment Cohorts

	Treatment (1)	Comparison (2)	Difference (3)
Panel A - Primary school outcomes			
% Enrolled at ages 7-12	1.006	0.954	0.052
			[0.018]***
% Progressing adequately at ages 8-12	0.222	0.198	0.023
			[0.010]**
% Enrolled at age 8	1.110	1.024	0.085
			[0.025]***
% Enrolled at age 10	1.120	1.063	0.057
			[0.021]***
% Enrolled at age 12	0.886	0.847	0.039
			[0.018]**
% Progressing adequately at age 8	0.350	0.318	0.032
			[0.015]**
% Progressing adequately at age 10	0.216	0.188	0.029
			[0.011]***
% Progressing adequately at age 12	0.121	0.108	0.014
			[0.006]**
Panel B - Community characteristics			
% Household head is indigenous	0.363	0.361	0.002
			[0.023]
% Household head has no education	0.473	0.536	-0.063
			[0.010]***
Household head's age	46.423	46.603	-0.181
			[0.130]
% Household has running water	0.615	0.589	0.026
			[0.021]
% Household has concrete floor	0.363	0.289	0.074
			[0.014]***
Community size	606.373	501.913	104.460
			[20.966]***
Number of communities	452	885	

Notes: This table presents statistics and estimated differences between communities that eventually had access to a preprimary and those that did not. Panel A reports statistics generated from administrative records for pre-treatment cohorts (those born between 1988 and 1991). Panel B presents statistics constructed from the 2002 Population Census for the same cohorts. Columns (1) and (2) present means, column (3) presents results from OLS regressions. Standard errors, reported in brackets, are clustered at the community level. Significance at the one, five and ten percent levels is indicated by ***, ** and *, respectively.

Table 2. Impacts of Preprimary Construction: ITT Estimates

	(1)	(2)	(3)	(4)	(5)
Panel A - Dependent variable: % Enrolled in primary					
Preprimary access	-0.009	-0.021	-0.009	-0.013	-0.010
	[0.013]	[0.014]	[0.014]	[0.014]	[0.017]
R-Squared	0.013	0.031	0.042	0.020	0.015
Number of observations	72,623	72,623	72,623	72,623	50,473
Panel B - Dependent variable: % Progressing adequately					
Preprimary access	0.015	0.014	0.015	0.015	0.011
	[0.006]**	[0.006]**	[0.006]**	[0.006]**	[0.008]
R-Squared	0.015	0.036	0.178	0.192	0.148
Number of observations	60,951	60,951	60,951	60,951	42,370
Number of schools	1,392	1,392	1,392	1,392	960
Department-year interactions		Yes			
Municipality linear time trends			Yes		
Time-varying controls				Yes	
Trimming and reweighting					Yes

Notes: This table presents the estimated effects of preprimary access on primary school progression. The unit of observation is community-cohort-age. Each cell corresponds to one OLS regression. All regressions are estimated with cohort and school fixed effects. Specification (2) includes interactions of department-year, specification (3) linear-time trends at the municipality level and specification (4) time-varying controls. For trimming and reweighting (specification 5), we compute the propensity score using average time-varying controls and community size. Standard errors, reported in brackets, are clustered at the community level. Significance at the one, five and ten percent levels is indicated by ***, ** and *, respectively.

Table 3. Impacts of Preprimary Construction: ToT Estimates

	(1)	(2)	(3)	(4)	(5)
Panel A - Dependent variable: % Enrolled in primary					
% Attended preprimary	-0.022	-0.041	-0.022	-0.027	-0.023
	[0.023]	[0.023]*	[0.023]	[0.023]	[0.027]
R-Squared	0.013	0.030	0.042	0.020	0.015
Number of observations	71,658	71,658	71,658	71,658	49,861
Panel B - Dependent variable: % Progressing adequately					
% Attended preprimary	0.025	0.025	0.024	0.026	0.019
	[0.011]**	[0.010]**	[0.011]**	[0.011]**	[0.013]
R-Squared	0.037	0.179	0.192	0.149	0.041
Number of observations	60,143	60,143	60,143	60,143	41,857
Number of schools	1,392	1,392	1,392	1,392	960
Department-year interactions		Yes			
Municipality linear time trends			Yes		
Time-varying controls				Yes	
Trimming and reweighting					Yes

Notes: This table presents the estimated effects of preprimary attendance on primary school progression. The endogenous variable (percent attended preprimary) is instrumented by a dummy indicating that the cohort had access to a preprimary in the community. The unit of observation is community-cohort-age. Each cell corresponds to one IV regression. All regressions are estimated with cohort and school fixed effects. Specification (2) includes interactions of department-year, specification (3) linear-time trends at the municipality level and specification (4) time-varying controls. For trimming and reweighting (specification 5), we compute the propensity score using average time-varying controls and community size. Standard errors, reported in brackets, are clustered at the community level. Significance at the one, five and ten percent levels is indicated by ***, ** and *, respectively.

Table 4. Heterogeneous Impacts of Preprimary Access by Age

	(1)	(2)	(3)	(4)	(5)
Panel A - Dependent variable: % Enrolled in primary at specific age					
Age 7	-0.044	-0.044	-0.037	-0.048	-0.052
	[0.021]**	[0.021]**	[0.023]	[0.021]**	[0.026]**
Age 8	-0.033	-0.044	-0.029	-0.036	-0.026
	[0.021]	[0.021]**	[0.023]	[0.021]*	[0.026]
Age 9	0.006	-0.012	-0.007	0.001	-0.003
	[0.019]	[0.020]	[0.020]	[0.019]	[0.024]
Age 10	-0.001	-0.005	-0.005	-0.004	0.005
	[0.018]	[0.018]	[0.019]	[0.018]	[0.022]
Age 11	-0.004	-0.010	-0.028	-0.007	-0.014
	[0.017]	[0.017]	[0.018]	[0.017]	[0.021]
Age 12	0.018	0.022	0.005	0.017	0.030
	[0.016]	[0.016]	[0.017]	[0.016]	[0.021]
Number of observations	71,658	71,658	71,658	71,658	49,861
Panel B - Dependent variable: % Progressing adequately at specific age					
Age 8	0.003	0.001	-0.006	0.000	-0.001
	[0.011]	[0.011]	[0.012]	[0.011]	[0.013]
Age 9	0.016	0.013	0.010	0.013	0.008
	[0.009]*	[0.009]	[0.010]	[0.009]	[0.011]
Age 10	0.018	0.017	0.014	0.015	0.013
	[0.008]**	[0.008]**	[0.008]*	[0.008]*	[0.009]
Age 11	0.015	0.015	0.009	0.014	0.007
	[0.007]**	[0.007]**	[0.008]	[0.007]*	[0.009]
Age 12	0.021	0.022	0.016	0.021	0.024
	[0.005]***	[0.005]***	[0.006]***	[0.005]***	[0.006]***
Number of observations	60,143	60,143	60,143	60,143	41,857
Number of schools	1,392	1,392	1,392	1,392	960
Department-year interactions		Yes			
Municipality linear time trends			Yes		
Time-varying controls				Yes	
Trimming and reweighting					Yes

Notes: This table presents the estimated effects of preprimary access on primary school progression by age. The unit of observation is community-cohort. Each cell corresponds to one OLS regression. All regressions are estimated with cohort and school fixed effects. Specification (2) includes interactions of department-year, specification (3) linear-time trends at the municipality level and specification (4) time-varying controls. For trimming and reweighting (specification 5), we compute the propensity score using average time-varying controls and community size. Standard errors, reported in brackets, are clustered at the community level. Significance at the one, five and ten percent levels is indicated by ***, ** and *, respectively.

Table 5. Heterogeneous Impacts of Preprimary Access by Selected Characteristics

	% Enrolled in primary		% Progressing adequately	
	(1)	(2)	(3)	(4)
Panel A - Baseline estimates				
Preprimary access	-0.009	-0.021	0.015	0.014
	[0.013]	[0.014]	[0.006]**	[0.006]**
Panel B - By gender				
Preprimary access	-0.008	-0.023	0.006	0.006
	[0.018]	[0.019]	[0.008]	[0.008]
Preprimary access * I(female)	-0.002	-0.002	0.017	0.016
	[0.018]	[0.018]	[0.008]**	[0.008]**
Panel C - By school size				
Preprimary access	-0.035	-0.045	0.012	0.013
	[0.020]*	[0.021]**	[0.011]	[0.010]
Preprimary access * I(large school)	0.043	0.040	0.004	0.002
	[0.024]*	[0.024]*	[0.012]	[0.012]
Panel D - By indigenous population				
Preprimary access	-0.032	-0.032	0.007	0.013
	[0.018]*	[0.018]*	[0.009]	[0.009]
Preprimary access * I(high indigenous)	0.047	0.022	0.016	0.003
	[0.023]**	[0.023]	[0.011]	[0.011]
Panel E - By adult population with no education				
Preprimary access	-0.026	-0.035	0.011	0.013
	[0.016]	[0.016]**	[0.008]	[0.008]
Preprimary access * I(high no education)	0.041	0.033	0.008	0.002
	[0.024]*	[0.024]	[0.011]	[0.011]
Number of schools	1,392	1,392	1,392	1,392
Department-year interactions		Yes		Yes

Notes: This table presents the estimated effects of preprimary access on primary school progression by selected characteristics. Each column in a panel corresponds to a separate regression. In each OLS regression, the relevant explanatory variables are a treatment indicator and the interaction of this variable with a dummy for the analyzed characteristic (e.g. in Panel B, girls). In Panels C, D and E, communities are divided by the median value of the analyzed dimension. All regressions are estimated with cohort and school fixed effects. Specifications (2) and (4) include interactions of department-year. Standard errors, reported in brackets, are clustered at the community level. Significance at the one, five and ten percent levels is indicated by ***, ** and *, respectively.

Table 6. Robustness Checks

	Baseline estimates		Nearest town > 0.75 km		Nearest town > 1 km		All openings		Schools with average enrollment below 1	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**
Panel A - Dependent variable: % Enrolled in primary										
Preprimary access	-0.009	-0.021	-0.012	-0.018	-0.024	-0.019	-0.029	-0.014	0.014	0.001
	[0.013]	[0.014]	[0.017]	[0.018]	[0.022]	[0.022]	[0.012]**	[0.012]	[0.016]	[0.016]
R-Squared	0.013	0.031	0.013	0.033	0.012	0.038	0.014	0.039	0.015	0.041
Number of observations	72,623	72,623	45,278	45,278	23,323	23,323	105,825	105,825	32,860	32,860
Panel B - Dependent variable: % Progressing adequately										
Preprimary access	0.015	0.014	0.013	0.012	0.01	0.014	0.011	0.014	0.021	0.021
	[0.006]**	[0.006]**	[0.008]	[0.008]	[0.011]	[0.011]	[0.005]**	[0.005]***	[0.008]***	[0.008]***
R-Squared	0.015	0.036	0.034	0.181	0.027	0.176	0.039	0.175	0.043	0.172
Number of observations	60,951	60,951	37,985	37,985	19,588	19,588	89,033	89,033	27,684	27,684
Number of schools	1,392	1,392	867	867	448	448	2,025	2,025	646	646
Department-year interactions		Yes		Yes		Yes		Yes		Yes

Notes: This table explores the robustness of the estimated effects of preprimary access on primary school progression. Each cell corresponds to one OLS regression. Baseline estimates correspond to those presented in columns 1 and 2 in Table 2. Columns 3 and 4 restrict the sample to communities with no neighbouring ones in a radius of 0.75 km. Columns 5 and 6 restrict the sample to communities with no neighbouring ones in a radius of 1 km. Columns 7 and 8 include all preschool openings during the period 1993 to 2005. Columns 9 and 10 restrict to schools with pre-treatment average enrollment below 1. All regressions control for cohort and school fixed effects. Even-numbered columns include the interaction of dummies for department and year. Standard errors, reported in brackets, are clustered at the community level. Significance at the one, five and ten percent levels is indicated by ***, ** and *, respectively.

Table 7. Falsification Test: Impacts of Preprimary Access before Construction

	Baseline estimates		Construction 2-years prior		Construction 2-years prior - School with enrollment below 1	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Panel A - Dependent variable: % Enrolled in primary						
Preprimary access	-0.009	-0.021	0.012	0.002	0.033	0.025
	[0.013]	[0.014]	[0.017]	[0.017]	[0.018]*	[0.018]
R-Squared	0.013	0.031	0.013	0.031	0.015	0.04
Number of observations	72,623	72,623	63,269	63,269	29,421	29,421
Panel B - Dependent variable: % Progressing adequately						
Preprimary access	0.015	0.014	0.010	0.008	0.010	0.010
	[0.006]**	[0.006]**	[0.007]	[0.007]	[0.007]	[0.007]
R-Squared	0.015	0.036	0.028	0.168	0.033	0.161
Number of observations	60,951	60,951	53,141	53,141	24,809	24,809
Number of schools	1,392	1,392	1,392	1,392	646	646
Department-year interactions		Yes		Yes		Yes

Notes: This table presents placebo tests to check whether there were pre-intervention differential trends in outcomes between treatment and comparison communities. Each cell corresponds to one OLS regression. Baseline estimates correspond to those presented in column 1-2 in Table 2. In columns 3-4, treated cohorts are dropped from the sample and the treatment dummy is replaced by a placebo indicator that takes the value of 1 for cohorts born one or two years before the first exposed cohort in treatment communities (as the preprimary was constructed two years prior). We repeat this exercise, but restricting to schools with average enrollment below 1, and present the results in columns 5 and 6. All regressions control for cohort and school fixed effects. Even-numbered columns include the interaction of dummies for department and year. Standard errors, reported in brackets, are clustered at the community level. Significance at the one, five and ten percent levels is indicated by ***, ** and *, respectively.