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# *Another Brick in the Wall: Subsidizing School Enrollment through a Breakfast Program*

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**Abstract**

Breakfast Programs can be implemented to subsidize school enrollment when there are barriers to access education. In this paper, I exploit a quasi experimental setting in Bolivian municipalities to assess the impact of a Breakfast Program over school enrollment. I supplement the analysis assessing if there were improvements in performance and family nutritional status. I find that the program had a positive impact on primary enrollment, but find no evidence of positive effects over performance or family nutritional outcomes. Results suggest that enrollment increased through a removal of economic barriers, that allowed to enroll children who were out of school system.

*Keywords:* School Breakfast Program, Subsidies, Education
*JEL Code:* I2, H20, O22

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# 1 Introduction

Reductions in education costs and the provision of subsidies can boost school participation (Kremer and Holla, 2009). School Breakfast Programs are provisions commonly implemented to promote adequate food intake for students. However, some countries have implemented similar programs to promote school participation or keep children at school, such as the implementation of the program in provinces of Peru (Cueto and Chinen, 2000) or in rural schools of Chile (McEwan, 2013). A extra food provision for children could positively affect education and health outcomes, particularly in constrained settings where good nutrition conditions and adequate food consumption might not be widespread among children.

School Breakfast Programs are very popular in several countries (Jomaa, McDonnell and Probart, 2011). Nonetheless, there is little consensus on the evidence of these programs. For instance, Bhattacharya, Currie and Haider (2006) find that the School Breakfast Program implemented in United States schools had no impact over the amount of calories that students consumed, but improved nutritional quality of the diet by reducing calories from fat consumption and reducing the probability of low fiber intake. Frisvold (2015) assesses the impact of the same program over cognitive achievement and finds that states where school breakfast was compulsory, increased math and reading scores. Moreover, Mahoney et al. (2005) use a experimental setting to assess the impact of breakfast composition on cognitive achievement and find that breakfast macronutrient composition could enhance spatial memory or short-term memory. On the other hand, recent evidence casts different results. Schanzenbach and Zaki (2014) find that breakfast programs in the United States only increased breakfast take up, but had no positive effects for test scores, attendance or health. In developing countries, where children might face different constraints, breakfast programs and similar programs are expected to have larger impacts (Currie and Vogl, 2013). For instance, Cueto and Chinen (2000) find positive results in short term memory and school attendance, but no results for test scores in Peru. Similar settings, such as meal programs have contradictory results, while McEwan (2013) finds no impact over enrollment, attendance, repetition nor test scores in Chilean rural schools, Afridi (2010) finds that a school meal program in India was successful by reducing protein, calorie and iron deficiencies. These differences can arrive due to differences in country background and program characteristics, but they can also reflect a not so straightforward path from food intake to educational outcomes.

This paper contributes with new evidence on the impacts of a breakfast program over enrollment by exploiting a quasi experimental setting in Bolivia, a developing country that has several features that make this evaluation unique. Bolivia is the poorest country of South America, as 2015 Bolivia had a Us$ 2,886 annual per capita GDP, the lowest of the region (International Monetary Fund, 2018). Accordingly, nutrition is a major issue in the country, according to FAO,

in 1998, 7.6 percent of Bolivian children below five years old suffered undernourishment and prevalence of undernourishment for the whole population was 33.4 percent in 2000. Furthermore, Roncal (2003) reports that 1 of 3 students do not have breakfast at home. Given these poor nutritional characteristics, we can expect a breakfast program to be a proficient intervention to generate improvements in educational outcomes. On the other hand, in 2000 net enrollment in Bolivia was 94.8 percent (World Bank, 2018). In spite of poor nutritional indicators, program was implemented on a high pre-program enrollment for primary.

The breakfast program is implemented by Bolivian municipalities, that provided a solid and a liquid ration of food for students in primary public schools. The aim of the program is to improve nutritional outcomes and to reduce dropout rates of primary students. Municipalities implemented the program in different years, therefore we have variability in time and among units, that allows us to estimate the impact of the program over enrollment through a difference in differences approach. I conducted a telephonic survey to municipality offices in order to gather data on the precise year of program implementation. Since developing countries are characterized by having barriers in the access to educational system, in this paper I explore the impact of the breakfast program over enrollment, where a food provision could be understood as a subsidy. I find that the program increased the enrollment by 3.4 percent. Results are robust to alternative specifications and placebo falsification tests. Moreover, I analyze if the increase in enrollment was due to changes in the intensive margin. That is, if students that were already at school drop less due to hunger relief and increased attention. To this aim, I observe changes in promotion and dropout rates. Furthermore, I explore a family effect on nutritional outcomes as supplemental evidence of intensive margin. Still, I do not find any evidence that supports intensive margin hypothesis. Therefore, I hypothesize on the extensive margin effect, where the increase in enrollment arrives from the effectiveness of the program by bringing children outside of educational system to school.

Different from the literature that focused the study of Breakfast Programs over nutritional outcomes, specially in developed countries, this paper contributes with new evidence on how families respond to subsidies for schooling. That is, studying breakfast programs as a tool to reduce access barriers. Different to other studies (Adelman et al., 2008; Kazianga, de Walque and Alderman, 2009), this paper finds robust positive effects over enrollment. Although this is not the first study to explore family effects, since Bhattacharya, Currie and Haider (2006) studied the impact of a Breakfast Program over family diet quality, to the best of my knowledge this is the first paper that assess family effects of Breakfast Programs over family anthropometric measures. The setting of this paper not only analyze the Breakfast Program during a growth period where Bolivia had a 1.9% per capita GDP growth rate (World Bank, 2018), but also after a large decentralization process, that allowed municipalities to set this unique policy.

The rest of the paper is organized as it follows. Section 2 describes the setting of the Breakfast Program, section 3 describes the data, section 4 presents identification strategy, main estimates and explore paths through which a breakfast can affect enrollment. Finally, section 5 concludes.

# 2 The setting

In 1994 Bolivian Congress enacted the Popular Participation Law (Law 1551) that started a political decentralization process. Law 1551 transferred infrastructure, extended powers and raised budgets to municipalities. The law gave municipalities freedom to implement programs to promote education within its jurisdiction. Before Law 1551 it was unlikely to have policies and programs at the local level since municipalities didn't have the resources and approval from National Congress was mandatory. However, decentralization process was slow, since municipalities didn't received resources from national government until a couple of years later than the enacted law (World Food Programme, 2007).

School Breakfast implementation is in charge of municipalities since 1999 (World Food Programme, 2007).[1] In 1999 the municipality of El Alto successfully implemented the School Breakfast Program (Gobierno Municipal de La Paz, 2003).[2] The aims of the program were to improve nutritional status and improve school attendance for all child in public schools. In the following years other municipalities launched the program within their jurisdiction since it started to be a highly demanded program. The decision to implement the Program depended exclusively of municipal governments. By 2007, neither Ministry of Education nor Ministry of Health participated on the implementation or control of the program (World Food Programme, 2007). Educational Reform Law stated that each municipality should attend local demands regarding education. Program was implemented in many cases with support of parents, committees and local communities (World Food Programme, 2007). Some municipalities decided to make it compulsory for primary school only and others for primary and secondary.

The School Breakfast Program provides a solid ration and a liquid ration that are similar among municipalities. Both are determined at the municipal level. Most municipalities provide carbohydrates in the solid ration (some variation of bread or cookies) and flavored milk (or similar) in the liquid ration. Roncal (2003) documents different provisions given by municipalities different from the standard setting that include a fruit instead of bread, juice instead of milk, and some combinations including products available only at the local level. Different

[1] School Breakfast Programs were considered in largest cities before 2000, but they were unstructured attempts with little political and financial aid (Roncal, 2003).

[2] The municipality of El Alto is the second largest municipality in Bolivia in terms of population. It is located in the metropolitan area of La Paz city, the Bolivian seat of government, which makes it an influential municipality.

to other settings, consumption of SBP is not assigned to a specific cafeteria but provided to children directly during the first hours of the morning, at most until the mid-morning recess.

# 3 Data

Data is provided by the Educational Information System (SIE, by its acronym in Spanish) which is the organization within Ministry of Education that compiles official educational statistics. SIE provided information of students enrolled, promoted and dropouts by grade for public and private schools in all municipalities from 2000 to 2008. To characterize the starting point of this study I provide information on how students were divided between public and private education and the total number of students in primary that were enrolled, that promoted and dropout in 2000. There were 1,461,816 enrolled students in primary, of which 91 percent were enrolled in public schools.

Table 1: Students of primary schools in 2000

| | Total | Public | Private |
|---|---|---|---|
| Enrolled | 1,461,816 | 1,335,819 | 125,997 |
| Promoted | 1,326,213 | 1,206,147 | 120,066 |
| Dropouts | 80,815 | 76,663 | 4,152 |
| Repeating | 54,788 | 53,009 | 1,779 |

Source: Own elaboration with information of Ministry of Education.

Although SIE compiles educational statistics, by the time this research was carried, they did not have available information regarding the year of Breakfast Program implementation in each municipality. Since the implementation year is key to the identification strategy, I decided to collect data on year of implementation through a telephonic survey.[3] Survey was conducted with assistance of CEGIE-UPB (Center of Statistical Information Generation at the Bolivian Private University). Survey consisted of phone calls to municipality town halls. We contacted people in charge of education programs or budget offices. We asked for the precise first year of the implementation of the Breakfast Program. To check the reliance of information provided by town halls, during the pilot survey we asked if the program was implemented in all schools, since we had information by Ministry of Education on the percentage of implementation for municipalities for 2013 and 2014. Results were satisfactory, there was consistency between Ministry of Education information and telephonic survey.

We were not able to reach information on the implementation year for all municipalities due to timing and budget restrictions. The telephonic list of municipalities was randomly arranged to avoid bias in the contacted municipalities.

[3]Municipalities also provide information of expenses to National Treasury, however Roncal (2003) and World Food Programme (2007) report that not all municipalities detailed the provision of the program in their accounts even when they were giving it.

The survey reached 63% of all the municipalities in Bolivia, that is, 212 out of the 339 municipalities. A potential concern is that municipalities reached by the telephonic have different characteristics from municipalities that were not reached. For instance, it could be the case that contacted municipalities have higher state capacity and given that capacity, they might be able to implement programs easily. If this holds true we would be facing selection bias. To address this concern, I use municipality indicators based on 2012 National Census (National Statistic Institute) to compare observable characteristics of municipalities that were surveyed and municipalities that weren't. I will assume the assumption that if municipalities are not different in observable characteristics they might not be different in unobservable characteristics as well. In Table 2 I compare surveyed to not surveyed municipalities. Specifically I compare percentage of rural areas, average years of education, percentage of household with access to electricity, percentage of household with access to water services, percentage of people with Identification Document (ID) and incidence of poverty. Differences between both groups are negligible. In Column (3) I present p-value of mean comparison. None of these variables reject hypothesis on statistically significant differences. Therefore, I can state that surveyed municipalities can be considered as a representative group of Bolivian municipalities.

Table 2: Characteristics of the sample

| | (1) | (2) | (3) |
|---|---|---|---|
| Variable | Not surveyed | Surveyed | p-value |
| Rural | 0.534 | 0.507 | 0.635 |
| Years of education | 7.062 | 6.985 | 0.676 |
| Electricity | 0.671 | 0.654 | 0.406 |
| Water | 0.494 | 0.494 | 0.991 |
| ID | 0.785 | 0.792 | 0.472 |
| Poverty | 0.697 | 0.707 | 0.614 |

Source: Own elaboration with information of 2012 National Census (INE Bolivia).

Trough the survey I gathered information on the precise first year of implementation of the Breakfast Program. Table 3 presents the time line of the program for the years that we have information on education outcomes.

Table 3: Time line of the Program

| | 2000 | 2001 | 2002 | 2003 | 2004 | 2005 | 2006 | 2007 | 2008 |
|---|---|---|---|---|---|---|---|---|---|
| Treated | 6 | 10 | 11 | 13 | 14 | 21 | 34 | 51 | 78 |
| Control | 206 | 202 | 201 | 199 | 198 | 191 | 198 | 161 | 134 |

Source: Own elaboration.

# 4 The impact of Breakfast Program on Enrollment

## 4.1 Identification strategy and results

The ideal setting to identify the causal impact of a Breakfast Program would be a randomized control trial, where a group of children is randomly assigned to receive breakfast. Then we would be able to compare the mean difference in enrollment of those that were assigned to receive breakfast to those that were not. However, this optimal design setting is not available. Therefore, I exploit a quasi experimental setting that mimic the optimal design under reasonable conditions.

It is crucial for identification that the program was implemented at the municipal level. That is, each municipality decided to provide breakfast in public schools of its jurisdiction. A large number of municipalities implemented the program, and they started in different years, providing variability among units and across time that allowed me to assess the causal impact of the program. The main estimation strategy follows the same logic as a standard difference-in-differences (DD) strategy. I compare the relative change in enrollment in the post-program period relative to the pre-program period between municipalities that implemented the program and those that did not. However, we are not in the basic DD specification, where the treatment is introduced only in one year for all treated units, we are in the Generalized DD case where units enter the program in different years. Equation (1) states the main specification.

$$y_{it} = \alpha BP_{it} + \mathbf{X}_{it} + \mu_t + \lambda_i + \epsilon_{it} \tag{1}$$

Where $y_{it}$ is the natural logarithm of enrollment, $BP_{it}$ is a indicator variable that takes the value of 1 if municipality $i$ is implementing the program in year $t$, $\mathbf{X}_{it}$ is a vector of two variables that vary in time and across municipalities, the logarithm of municipality population and government transfers to municipalities, $\mu_t$ is a time effect and $\lambda_i$ is a fixed effect for municipalities, $\epsilon_{it}$ is the municipality time-varying error. The errors $\epsilon_{it}$ could be correlated across time and space. That is, unobservables of municipalities belonging to the same province could be correlated while should not be correlated with municipalities that are separated by a large distance. For instance, climate and epidemiological factors presence in one municipality can affect neighboring municipalities. Therefore, I use two approaches for standard errors, in the first one, the most conservative, I cluster standard errors at the municipality level, the smaller administrative level. In the second approach I use standard errors at the province-year level. This standard errors allow for correlation within a higher administrative level (less restrictive) but also for correlation for every year. Under parallel pre-treatment trends assumption, the coefficient $\alpha$ that measures the impact of the breakfast program on enrollment can be interpreted as the Average Treatment Effect on the Treated (AToT).

The stated specification attempts to address all potential threats to identification. Year fixed effects control for shocks that affected all municipalities similarly, controlling for policies implemented at the national level that were also designed to promote educational attainment.[4] Another potential threat to identification is that municipalities that implemented the program did it because they received more transfers from central government because of political alignment or other political motivation. To address this potential concern, I control for government transfers to municipalities. I also control for the logarithm of population, hence any shock that affected municipal population differently is also taken into account.

I present results of Equation (1) in Table 4. The table reports results from different specifications. Column (1) excludes the vector of controls, Column (2) is the complete specification that uses central Government Transfers and the logarithm of population as a control, Column (3) reports the same specification as in the previous column but standard errors are clustered at the province-year level, Column (4) is the same specification as Column (2) but excluding from the sample the first municipalities that implemented the program within the three largest metropolitan areas.

Table 4: Impact of Breakfast on Primary Enrollment of Public Schools

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Breakfast | 0.033* | 0.034* | 0.034*** | 0.034* |
| | (0.020) | (0.020) | (0.013) | (0.020) |
| Controls | No | Yes | Yes | Yes |
| $R^2$ | 0.992 | 0.992 | 0.992 | 0.991 |
| N | 1,852 | 1,852 | 1,852 | 1,825 |

Coefficients are reported with standard errors, clustered at the municipality level, in parenthesis, with exception of Column (3) that present clustered standard errors at the province-year level. Column (1) excludes the vector of control variables. Column (4) excludes 3 municipalities from the sample, the first three neighboring municipalities that implemented the program within the largest metropolitan areas of La Paz, Cochabamba and Santa Cruz de la Sierra.

The estimated coefficient of the Breakfast program $\alpha$ reveals the average change in enrollment. According to the estimates of Column (2) implementing the program increased enrollment by 3.4 percent on average. Changes in the coefficient from column (1) to Column (2) show that specification is robust to the inclusion of control variables. That the coefficient is unaltered after the inclusion of central governmental transfers provides supportive evidence that the year of entry into the program is not correlated to income shocks that could affect enrollment through channels different from the Breakfast Program. In Column (3) as stated above I use clustered standard errors at the province-year

[4]For instance, year fixed effects control for a large program of conditional cash transfers implemented at the national level in 2006.

level. Both clustered standard errors give significant estimates at the conventional levels. Even in the more restrictive case, it is possible to state that we are in the presence of a positive effect. In any case, for the rest of the paper I prefer to be conservative and present standard errors clustered at the municipality level. A potential concern of estimation is that children from a municipality could go to neighbor municipalities to receive the breakfast. For instance, if students from La Paz go to El Alto municipality (neighboring municipality) because they implemented the breakfast earlier, enrollment in La Paz would decrease while El Alto would have a larger number of enrolled students, making the coefficient of our estimation artificially positive, since the increase in enrollment of municipalities with the program would be a displacement effect instead of a increase in enrolled students within each municipality. However, this is an unlikely event, since transportation cost might be at least as great as the amount that a student receives as a food ration. In any case, to address this problem for Column (4) I exclude the first municipalities to implement the program within the three largest metropolitan areas, these are the municipality of El Alto in La Paz metropolitan area, the municipality of La Guardia in Santa Cruz metropolitan area and municipality of Cochabamba for the homonym metropolitan area. Since those are the municipalities that might be affected by this concern. On the other hand, rural municipalities might not be a concern regarding displacement effect, since in Bolivia rural municipalities are characterized for being widely dispersed (Andersen, 2002). That is, municipalities are small and transportation is not as well integrated as in largest municipalities. Reported coefficient of Column (4) is 0.034 and it is still significant at the conventional levels. This result suggests that the estimated coefficient is robust to a potential displacement effect.



## 4.2 Parallel trends assumption

The main assumption of our DD estimates is that municipalities that implemented the program in late years are a good counterfactual of municipalities that implemented the program in early years in absence of intervention. In our setting, authorities of each municipality decided the year of implementation in year $t$. This brings the concern that selection to the program could be endogenously determined. If this is the case, municipalities that implemented the program later are not a good counterfactual of municipalities that implemented the program in early years. While I cannot directly test the DD main assumption, I can provide evidence that supports the assumption. In order to provide evidence I use an alternative specification stated in Equation (2). This equation uses leads and lags as in Autor (2003) and Munyo and Rossi (2018).

$$y_{it} = \sum_{k=q^-}^{q+} \alpha^k BP_{it}^k + \mathbf{X}_{it} + \mu_t + \lambda_i + \epsilon_{it} \tag{2}$$

Where $BP_{it}^k$ is an indicator variable for three years before adoption, the year of adoption and four years after adoption of the Breakfast Program ,$q^-$ is the

pre-period furthest back, and $q+$ is the post-period furthest after the implementation. Consequently, $\alpha^k$ measures the effect $k$ periods after program took place. If $k$ is negative then $\alpha^k$ measures the effect $k$ periods before the treatment.

Table 5 reports estimates of Equation (2). The four columns are the same columns presented in table 4, where Column (1) excludes the vector of controls, Column (2) is the preferred estimation, Column (3) uses standard errors clustered at the province-year level and Column (4) excludes first municipalities that implemented the program within a metropolitan area. I set $q^- = 3$ and $q^+ = 4$. All the coefficients on the pre-adoption (leads) for all the specifications are close to zero and are not statistically significant at the conventional levels. This fact provides confidence on the DD parallel-trends assumption. It can be concluded that there is no early response to treatment by municipalities. Figure 1 plots the sequence of $\alpha^k$ corresponding to the specification in Column (2) with their correspondent confidence bandwidths at the 0.90 level against the event time line. It can be seen that the positive effect does not arrive in the very first year of the program implementation, but it is increasing in time and the bulk of the effect starts after two years of implementation.

Table 5: Pre-treatment trends and dynamic patterns

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| t-3 | 0.002 | 0.005 | 0.005 | 0.007 |
| | (0.017) | (0.017) | (0.013) | (0.017) |
| t-2 | -0.005 | -0.001 | -0.001 | 0.002 |
| | (0.021) | (0.022) | (0.013) | (0.022) |
| t-1 | 0.017 | 0.021 | 0.021 | 0.027 |
| | (0.024) | (0.024) | (0.013) | (0.024) |
| t | 0.011 | 0.016 | 0.016 | 0.022 |
| | (0.026) | (0.026) | (0.016) | (0.026) |
| t+1 | 0.024 | 0.029 | 0.029 | 0.036 |
| | (0.027) | (0.028) | (0.018) | (0.028) |
| t+2 | 0.052 | 0.058* | 0.058** | 0.067** |
| | (0.032) | (0.032) | (0.025) | (0.033) |
| t+3 | 0.023 | 0.026 | 0.026 | 0.032 |
| | (0.022) | (0.022) | (0.016) | (0.023) |
| t+4 | 0.044* | 0.046* | 0.046** | 0.052* |
| | (0.026) | (0.027) | (0.020) | (0.030) |
| Controls | No | Yes | Yes | Yes |
| $R^2$ | 0.992 | 0.992 | 0.992 | 0.991 |
| N | 1,852 | 1,852 | 1,852 | 1,825 |

Coefficients are reported with standard errors, clustered at the municipality level, in parenthesis, with exception of Column (3) that present clustered standard errors at the province-year level. Column (1) excludes the vector of control variables. Column (4) excludes 3 municipalities from the sample, the first three neighboring municipalities that implemented the program within the largest metropolitan areas of La Paz, Cochabamba and Santa Cruz de la Sierra.

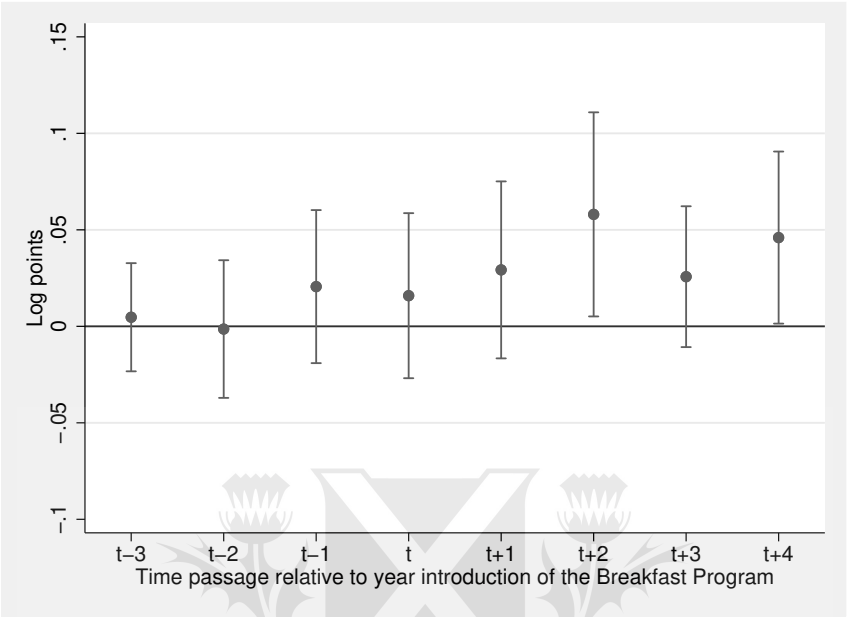


Figure 1: Sequence of $\alpha^k$ against the event time line

### 4.3 Addressing potential concerns

A major concern to identification, is the presence of any unobserved contemporary shock that could bias the estimates. For instance, results in enrollment might have been driven by other policies implemented to affect education or health in municipalities. To address this concerns I estimate Equation (1) for different outcomes that could imply contemporary shocks. We expect to find non significant coefficients when using these outcomes. Results of these estimations are presented in Table 6.

Table 6: Robustness and Falsification tests

| | (1) | (2) | (3) |
|---|---|---|---|
| | ln (Schools) | Private Schools | False Year |
| Breakfast | 0.026 | -0.008 | 0.023 |
| | (0.020) | (0.119) | (0.020) |
| Controls | Yes | Yes | Yes |
| $R^2$ | 0.983 | 0.972 | 0.992 |
| N | 1,852 | 469 | 1,852 |

Coefficients are reported with standard errors, clustered at the municipality level, in parenthesis. Outcomes of columns are detailed below. Column (1) is the natural logarithm of the number of schools. Column (2) outcome is the logarithm of enrolled students in primary private schools. Column (3) outcomes is the logarithm of enrolled students of primary public schools, as in Table 4, but randomly assigning a false year of treatment for treated municipalities.

In Column (1) of Table 6 I present the estimation using the logarithm of public schools as an outcome. It could be that municipalities not only implemented the Breakfast program, but also affected the supply side by constructing schools during the same period or affected other outcomes that resulted in more schools. I don't find a statistically significant coefficient for this specification. This result suggests that Breakfast provision is not associated to supply improvements along the same period. Moreover, I use two approaches as falsification tests. First, I use the logarithm of enrolled students in private primary schools as an outcome. The Breakfast Program was implemented only by public schools, thus we would expect to find no significant effect on private school outcomes. As expected, Column (2) shows a small negative coefficient not statistically significant, the program only affected beneficiaries. As a second falsification test I randomly assign a false year of implementation for municipalities that implemented the program. If treated municipalities are different of control municipalities we would expect the year of implementation to be irrelevant on the significance of coefficient, as well as if there are other policies affecting the outcome of treated municipalities. Column (3) reports the results of the false year test, as expected results are not statistically significant.

Evidence of Table 6 provides evidence supporting the idea that the very first year of entrance into the program is associated to changes in enrollment. Furthermore, it seems plausible to believe that there aren't other shocks that could be affecting enrollment during the same years, given that the number of schools is not contemporaneously correlated and that other shocks that could affect municipality outcomes do not affect private school enrollment. Not less important, the main specification is controlling for central government transfers to municipalities, that include transfers that were compulsorily destined for education and transfers from Hydrocarbon Taxes (IDH, for its acronym in Spanish). These taxes represent the greatest source of fiscal income for Bolivia (Chávez, 2013). As stated above, that the main specification is robust to the inclusion of a control variable that affects resources that could be used for other programs provides confidence regarding the exogeneity of the treatment.

Table 7 presents heterogeneous response to Breakfast program by grade. Each column represents each grade of primary school, from 1st grade to 8th grade. For the Breakfast coefficient I present standard errors clustered at the municipality level, in parenthesis, and standard errors clustered at the province-year level, in brackets. Both estimations show significant coefficients for 3rd and 5th grade. With less restrictive standard errors, results are also significant for 2nd, 4th and 6th grade at the conventional levels. The bulk of the effect seems to be in 3rd grade (9 year old children), where coefficient indicates that the program increased enrollment by 4.4 percent. This result is consistent with the significant effect two years after the implementation of the program reported in Table 5 and Figure 1. Barriers might not necessarily be associated to entry but to the possibility to stay at school, since Breakfast Program increases the opportunity cost to dropout school.

Table 7: Impact of Breakfast on Primary Enrollment of Public Schools by Grade

| | (1) 1st grade | (2) 2nd grade | (3) 3rd grade | (4) 4th grade | (5) 5th grade | (6) 6th grade | (7) 7th grade | (8) 8th grade |
|---|---|---|---|---|---|---|---|---|
| Breakfast | 0.026 | 0.029 | 0.044 | 0.036 | 0.043 | 0.041 | 0.017 | -0.002 |
| | (0.027) | (0.020) | (0.023)* | (0.022) | (0.026)* | (0.031) | (0.034) | (0.033) |
| | [0.020] | [0.015]* | [0.017]*** | [0.017]** | [0.020]** | [0.022]* | [0.025] | [0.025] |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| $R^2$ | 0.986 | 0.987 | 0.986 | 0.984 | 0.983 | 0.979 | 0.973 | 0.970 |
| N | 1,849 | 1,847 | 1,845 | 1,847 | 1,846 | 1,840 | 1,825 | 1,821 |

Notes: Coefficients are reported with standard errors, clustered at the municipality level, in parenthesis, for the coefficient of interest, standard errors, clustered at the province-year level are reported in brackets. Different from the rest of the tables, stars of significance in this table are not placed next to the coefficient, they are placed next to the standard errors.

## 4.4 Paths from Breakfast to Enrollment

Overall, I find that the Breakfast is associated to an increase of 3.4 percent in primary enrollment of public schools. The bulk of the effect is among third and fifth grades. Several paths can cause breakfast to positively affect enrollment. Breakfast can affect enrollment by two margins: extensive and intensive. The extensive margin is straightforward, breakfast reduces parents costs of feeding their children and opportunity cost of not going to school increases. Then, parents of children that were not in school decide to send them to school. In other words, breakfast acts as a subsidy that removes a barrier in the access to education. Intensive margin is not so straightforward. Incumbent students that receive the breakfast can improve attention (Cueto, 2001) and breakfast can positively affect learning skills (Grantham-McGregor, 2005).[5] If incumbent students are affected positively by breakfast intake due to calorie consumption or relief of hunger, that works immediately after breakfast intake, we would say that the short run channel is operating. If the short run margin works, we could expect a better performance, that in the margin can reduce dropouts and improve the percentage of promoted students. Therefore if I observe changes in promotion or dropout rates I will account them as changes in the short run channel. On the other hand, we can define a long run channel as the accumulation of human capital in time, in other words, when cognition and nutrition are affected. For instance, the long run channel could be affected due to improved iron deficiencies and can affect cognitive abilities or nutrition indicators.

First I will explore the short run intensive margin. Table 8 present results for promotion and dropout. These results can be interpreted as the short term impact of receiving breakfast at the aggregate level. If breakfast works, on average we would expect to find better aggregated results regarding promotion or a reduction of the average dropouts. Column (1) presents the impact of breakfast over number of promoted students in logarithm. Results suggest a increase in promotion of 3.3%. However, the increasing number in promoted students is linked to the higher number of enrolled students. To explore if intensive margin is working we would expect a higher promotion rate. Column (2) reports the results for promotion rate (promoted students divided enrolled students). Coefficient is positive, but results are not statistically significant. Columns (3) and (4) present the estimations for dropout in logarithm and dropout rate, respectively. Dropout coefficients are close to zero. Promotion and Dropout rates seem to be unaffected by the provision of a Breakfast. This results suggest that short run intensive margin was not affected. Even when average attention during classes could have been improved, the improvement for students in the margin was not high enough to let the average promotion to improve or the average dropout to be negatively affected.

[5] Pollitt, Cueto and Jacoby (1998) find that overnight and morning fasting had an adverse effect on the accuracy of responses in problem solving and slower recall in short-term memory on children of 9 to 11 years old.

Table 8: Impact of Breakfast on Promotion and Dropout of Public Schools

| | (1) ln(Promotion) | (2) Promotion rate | (3) ln(Dropout) | (4) Dropout rate |
|---|---|---|---|---|
| Breakfast | 0.033* | 0.080 | -0.001 | 0.002 |
| | (0.019) | (0.069) | (0.005) | (0.003) |
| Controls | Yes | Yes | Yes | Yes |
| $R^2$ | 0.992 | 0.901 | 0.557 | 0.616 |
| N | 1,852 | 1,829 | 1,852 | 1,852 |

Coefficients are reported with standard errors, clustered at the municipality level, in parenthesis. Promotion rate, reported in Column (2) is defined as promoted students divided enrolled students. Dropouts rate, reported in Column (4) is defined as dropout students divided enrolled students.

To explore the long run intensive margin I use 2003 and 2008 DHS survey data, that provides weight and length measures of children between 0 and 5 years old. Since there is no available information of anthropometric measures for beneficiaries, I explore if breakfast affected anthropometric measures of siblings. Siblings are not direct beneficiaries of the program, but indirectly, we can define family as the beneficiary unit. Furthermore, there is evidence that families compensate for child endowments within the household acting as a net equalizer in response to early health shocks for children (Yi et al., 2015). Accordingly, a beneficiary family would be able to re-allocate resources within the household. That is, given a positive shock to beneficiaries, family could level endowments by providing siblings a better nutrition. If this holds true, nutrition indicators of siblings could be positively affected.

There is an advantage in analyzing nutritional measures of siblings. Young children are especially vulnerable to health shocks in developing countries (Yi et al., 2015). For instance, Currie et al. (2010) find that health in early childhood have larger effects on educational attainment and use of welfare than health in early adolescence. In the same line, Cunha and Heckman (2008) find that parental inputs have larger impacts at early ages. Currie and Vogl (2013) argue that weight is typically a proxy for short-term nutritional status, and height, a marker of health and nutrition during the critical periods of growth in early life, especially from conception to the age of three years old. Accordingly, I use as outcome variables anthropometric z-scores of four measures: Body Mass Index, Weight-for-Age, Length-for-Age and Weight-for-Length (World Health Organization, 2006). Particularly, I explore changes in the incidence of malnutrition, that is, the proportion of child below two standard deviations from the expected average measure according to WHO standards.

I use a triple difference strategy (DDD) and observe the average change in malnutrition incidence of treated municipalities before and after having implemented the breakfast between those that have siblings in primary and those that don't. Equation (3) presents the DDD specification.

$$z_{ity} = \beta_0 + \beta_1 After_{iy} + \beta_2 Treat_{ty} + \beta_3 Sibling_{it} + \beta_4 Treat_{ty} \times Sibling_{it} + \beta_5 Treat_{ty} \times After_{iy} + \beta_6 After_{iy} \times Sibling_{it} + \beta_7 After_{iy} \times Treat_{ty} \times Sibling_{it} + \mathbf{X_{it}} + \epsilon_{ity} \quad (3)$$

Where $z_{ity}$ is the average incidence of malnutrition (BMI, Weight for age, Length for age and Weight for length) for municipality $i$ in year $t$ and for group $y$ ($y = 1$ if child have siblings in primary). The coefficient of interest $\beta_7$ represents the difference in average incidence of malnutrition for those that are in a treated municipality with respect to a non-treated municipality, accounting for potential different trends between families with siblings in primary and families not affected by the policy. I cluster standard errors at the household level in order to account for correlation among families that have more than one child between 0 and 5 years old. A caveat to this identification is that through DHS it is not possible to identify if beneficiary goes to public or private school. However, according to Ministry of Education data 91% of enrolled students in 2000 belonged to public schools. Vera-Cossio (2017) also reports that enrollment of public schools in Bolivia represents approximately 90 % of total enrollment. Therefore I do not consider this caveat as a major treat to identification.

Table 9: Impact of Breakfast on malnutrition measures of siblings

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | BMI | Weight | Length | W for L |
| 2008 X Treat X Sibling | 0.011 | -0.004 | 0.024 | 0.018 |
| | (0.013) | (0.020) | (0.039) | (0.012) |
| Controls | Yes | Yes | Yes | Yes |
| $R^2$ | 0.030 | 0.034 | 0.110 | 0.029 |
| N | 9,370 | 9,370 | 9,370 | 9,370 |

Coefficients are reported with standard errors, clustered at the province level, in parenthesis. Outcome is the proportion of child below two standard deviations from WHO standards: for Column (1) Body Mass Index, for Column (2) weigth for age, for Column (3) length for age and for Column (4) weight for length. All observations that reported missing data in one of these dimensions were excluded.

Table 9 reports the results of the estimated coefficients of Equation (3). The coefficient reports the effect of Breakfast Program over malnutrition measures of siblings of beneficiary children. Column (1) presents results for Body Mass Index, Column (2) Weight for age, Column (3) Length for age and Column (4) Weight for Length. I find no evidence of changes in the incidence of undernourishment measures of siblings. A negative coefficient in Column (2) indicates a reduction in the incidence of siblings malnutrition measured by weight, which is the proxy for short-term nutritional status. However, result is not statistically significant. The rest of the coefficients are positive and not statistically significant. Therefore, it is not possible to conclude that there was a negative impact on the incidence of nutritional status of siblings. In other words, I don't find a

family effect. Although, we can not rule out the possibility that families do not reallocate endowments within families.

Most of the literature that studied the impact of breakfast programs over nutritional status has not found conclusive evidence of positive effects over nutritional characteristics of beneficiaries. Accordingly, it is not surprising that families result unaffected as well. Furthermore, our findings cannot support the hypothesis of the intensive margin channel.

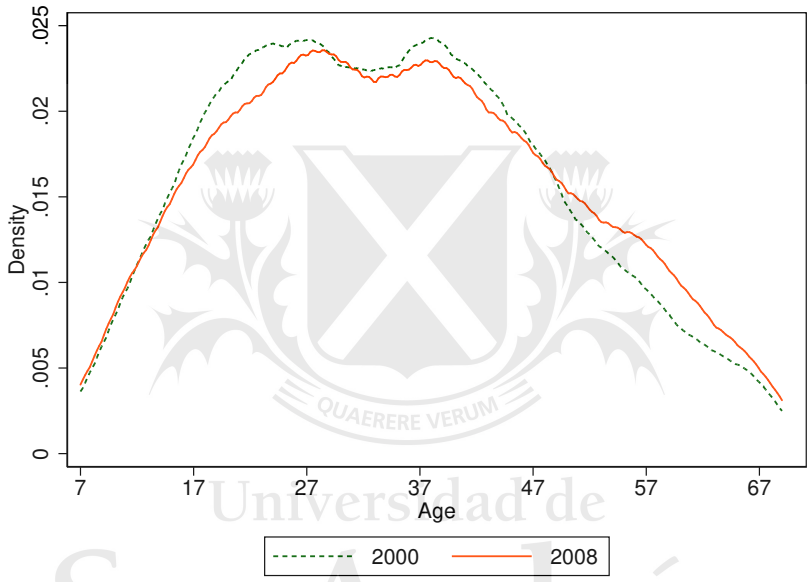


Figure 2: Distribution of age of working population

Given that I find robust evidence of a increase in enrollment of 3.4% for public schools and no evidence of intensive margin effects, I hypothesize that the effect over enrollment arrives by the extensive margin. Therefore, I explore statistics at the national level from Household Surveys (INE). I compare the proportion of enrolled children in the ages of primary school, from 7 years old to 12 years old. In 2000, 95.5% of children between 7 years old and 12 years old were enrolled. While, in 2008, 98.8% of children in the same age ranges were enrolled. That is a 3.3 percentage points increase in primary enrollment. Even when this statistics should be interpreted cautiously, they support the results found in the previous subsection as well as the extensive margin hypothesis. Furthermore, given the increase in enrollment, we would expect a substitution of labor for education in children. However, when I compare Household Surveys statistics I do not find changes in this direction. In 2000, 19.8 percent of children between 7 and 18 worked in the reference week, while in 2008 this percentage

slightly increased to 21.6 percent. Figure 2 presents the comparison of the distribution of working people by age for those two reference years. There are no shifts in the distribution for scholar ages (7 to 18 years old). If child substituted work for school, we would have expected a right shift in the 2008 distribution, but we find that both distributions are overlapped for school age. Thus, if the program bring more children to school, it does not take them out of the labor force.

# 5 Conclusions

Breakfast Programs are widely spread programs, specially in developing countries (Jomaa, McDonnell and Probart, 2011). Although there is evidence on their impact on both nutritional and educational outcomes, there still no consensus on their effectiveness. Lack of consensus arrives from different backgrounds of implementation, different program characteristics, but also because the path from food intake to educational outcomes is not straightforward. Moreover, evidence of the effectiveness of Breakfast Programs over enrollment is scarce. Kazianga, de Walque and Alderman (2009) find enrollment increased by 5 percentage points only for girl sub sample in Burkina Faso. While McEwan (2013) and Adelman et al. (2008) do not find evidence of food programs on enrollment in Chile and Uganda, respectively. Enrollment is a key outcome of food programs, since it might reflect family preferences towards education in the presence of access barriers.

This paper exploits a quasi experimental setting in Bolivia to study the role of a Breakfast Program over enrollment. I perform a survey to municipality town halls to identify the year of program implementation by each municipality and assess the impact of the program over primary public school enrollment. The difference in difference estimation shows that the program increased enrollment by 3.4 percent on average for public primary schools. Evidence suggests that breakfast program was successful liberating a constraint, that is, bringing child outside educational system to schools. I arrive this conclusion given that I don't finding any effects on promotion rates and dropout rates. According to Cueto (2001) the effect of breakfast consumption on education outcomes depends on the interaction of the program, student nutritional characteristics and effective provision. Unless the setting guarantees a minimum quality standard, the benefits of breakfast consumption will not be evident in performance. In the case of Bolivian municipalities, there is evidence that even when the program was widely accepted there was a concern related to quality (World Food Programme, 2007). Consequently, the lack of evidence on promotion rates could be attributable to insufficient intakes or inadequate breakfast composition.

I also studied if the program affected siblings undernourishment status, but results suggest that siblings were not affected. Bhattacharya, Currie and Haider (2006) also studied the impact of a breakfast program over family members but

they use adults, where it is harder to have impacts. Since most of the literature do not find positive results on nutritional characteristics of beneficiaries, it is not surprising that family members are not affected.

Overall, this paper finds positive evidence on the impacts of a Breakfast Program over enrollment. However, there is no evidence of positive impacts over performance or family nutrition outcomes. Program seems to be effective bringing children from outside the system, but all other potential outcomes seem to be unaltered. A question regarding the efficiency of the program, given that the only affected outcome is enrollment, is whether is it better to provide the breakfast or to provide a cash transfer. Akresh et al. (2013) find that cash transfers are effective boosting enrollment. Both programs can act as subsidies to affect family decisions regarding child education, by removing economic barriers for the access to educational services.

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