Oportunidades and its Impact on Child Nutrition

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Using population-level data from the Mexican Family Life Survey (MxFLS), we examine the impact on child health of a large-scale conditional cash transfer program, Oportunidades. This innovative anti-poverty program puts additional resources in the hands of women and their families and encourages parents to invest in human capital of their children. Program income accounts for about 25% of total resources in beneficiary households. The causal impact of the program on child health is isolated by exploiting insights from the biology of child growth in combination with the timing of the roll-out of Oportunidades and the panel dimension of MxFLS. Height for age among children exposed during the first 3 years of life is contrasted with similar children who were not exposed.

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1. INTRODUCTION

Oportunidades (formerly PROGRESA) is arguably the most important conditional cash transfer (CCT) program in the world[1]. It is an on-going antipoverty program that was implemented in Mexico starting in 1997. It has been in operation for 12 years, and, by 2005, covered about one quarter of the Mexican population.

The program links cash transfers with investments on different dimensions of human capital with the idea that such integration is crucial for the intervention to have long-lasting effects. In particular, monetary transfers are conditioned on investments in education, health and nutrition. This innovative approach started a new trend in the design of poverty alleviation programs throughout the world. Slightly modified versions have been implemented in many other countries including Argentina, Brazil, Chile, Colombia, Guatemala, Nicaragua, El Salvador, Honduras, Bangladesh, and Turkey.

Given the link that exists between early life nutritional status and later life success, one of the most important channels through which the program is expected to improve the long-term well-being of beneficiary households is through its impact on child nutrition. Malnutrition in early childhood is associated with deficits in cognitive development, greater risk of infant and child mortality and morbidity, as well as lower earnings during adulthood (Martorell 1999; Martorell et al. 2005; Strauss and Thomas 1995). The objective of this paper is to provide with conclusive evidence of the impact of Oportunidades on child nutrition, which constitutes an important step towards understanding the long-term impact of the intervention on beneficiary households.

The Oportunidades program has been extensively studied, and its impact on a broad array of indicators of well-being and behavioral choices has been assessed, many within the context of a randomized evaluation[2]. Generally speaking, the program has been found to improve the well-being of participating households. There is evidence of a positive impact on several dimensions, including educational outcomes, health outcomes, and consumption.

Almost all the existing evidence on this program is based on the Oportunidades evaluation data, which consists of a rural evaluation sample selected in 1997 based on a randomized design and a non-experimental urban evaluation sample selected in 2001, both complemented with a series of follow-up surveys. While these data constitute a rich source of information to evaluate the impact of Oportunidades, they face a number of limitations that are not always seriously considered in the literature, particularly in the literature that evaluates the impact on nutritional status. Only recently, issues such as deviations from perfect randomization, selective access to program components, or attrition have been considered, altering in many cases the conclusions of the analyses.

As opposed to previous studies, this paper uses population-level data to assess the impact of Oportunidades on young children's nutritional status. This allows, for the first time, to perform an

[1] PROGRESA stands for *Programa Nacional de Educación, Salud y Alimentación* (Education, Health and Nutrition Program) and Oportunidades for *Programa de Desarrollo Humano Oportunidades* (Oportunidades Human Development Program).

[2] See Parker, Rubalcava, and Teruel 2008 for a summary of the literature.

impact analysis at the national level. The program has been dramatically expanding over the years, but the current evidence only corresponds to its impact on a subset of the rural and urban localities first introduced to the program. However, these localities are not representative of the rural and urban sectors of the country. On the contrary, they were specifically chosen for being among those with the highest concentration of poor households. As a result, there is no reason to believe that the current evidence would apply to localities introduced to the program later in time.

Additionally, as will be seen in the literature review presented below, the current evidence presents mixed results. Even studies that exploit the strongest element of the Oportunidades evaluation sample, i.e. the experimental design in the first year of the program in rural areas, do not reach the same conclusions. As a result, this study constitutes a great opportunity to complement existing evidence using a data set that does not share the same limitations the Oportunidades evaluation data faces.

The data used in this paper is the Mexican Family Life Survey (MxFLS). MxFLS is a nationally representative, longitudinal survey that started in 2002, and it collects an extensive set of information on individuals, households and communities. Anthropometric measures are taken by specialized personnel from the National Institute of Public Health (INSP). As a result, MxFLS constitutes one of the few surveys that have both well measured anthropometrics (as opposed to self reported measures) as well as a very detailed set of socioeconomic variables, including income and consumption.

The causal impact of the program on child nutrition is isolated by exploiting insights from the biology of child growth, in combination with the timing of the roll-out of Oportunidades and the panel dimension of MxFLS. The evidence suggests that nutritional interventions do not affect children's height after they reach a certain age. Height-for-age, being a marker of early life nutritional investments, constitutes an ideal indicator to estimate the long term effects of the program on nutrition. Based on this evidence, and the fact that Oportunidades expanded over time, program exposure is defined as a function of the age of the child at the time Oportunidades was introduced to the locality of residence. The strategy basically consists of identifying cohorts of children that were exposed to the program and cohorts of children that were not and then performing an impact analysis at the community level. The panel dimension of MxFLS is used to overcome the difficulty of comparing z-scores of older and younger cohorts at one point in time. This identification strategy is innovative within the Oportunidades literature and has been successfully implemented in the nutrition and economics literatures.

The remaining of the paper is organized as follows. Section 2 provides a detailed description of the program and explains the channels through which Oportunidades is likely to improve children's nutritional status. Section 3 presents a short literature review that stresses the main caveats the current literature faces. Section 4 presents the data used in the analysis. Section 5 describes the identification strategy adopted in this paper, and explains its strengths and limitations. Section 6 shows the results, and Section 7 concludes.

2. OPORTUNIDADES: the program and its impact on nutritional status

2.1 Description of the program[3]

Oportunidades started in 1997 in the poorest rural areas of Mexico and has gradually expanded to cover less marginal rural, and urban areas. By the end of 1999 the program covered approximately 2.6 million families in almost 50,000 localities, which represents about 40% of the rural population. By the end of 2002 the program was operating in 70,520 localities, in all 31 states of the country, reaching 4.24 million households. As of mid 2005, Oportunidades covered 5 million families - about one quarter of the Mexican population.

The program links cash transfers with investments on different dimensions of human capital. The rational of such integration is that cash transfers would help households improve their current poverty status while investments in human capital would produce long lasting effects that would help break the intergenerational transmission of poverty. Additionally, transfers are targeted at women (whenever possible). There is a literature that tries to evaluate whether transfers made to women have a higher impact on children than transfers made to men, but the evidence are not conclusive[4].

The intervention basically consists of three elements: a universal monetary transfer (food component), an educational component, and a health and nutritional component. The universal monetary transfer aims to improve the food consumption and nutritional state of poor families[5]. The educational component consists of a pre-specified amount households receive for each child enrolled in grades 3 to 12[6]. The health and nutritional component offers household members access to basic health care services, nutritional supplements and educational talks. The nutritional supplements are provided to pregnant and lactating women and children between 4 and 24 months. They are also provided to children between 2 and 4 years old if malnutrition symptoms are detected by clinic personnel. The educational talks are community meetings where trained nurses and physicians discuss topics related to health, hygiene, and nutrition issues and practices. All the benefits are conditioned on regular health check-ups for every household member, school attendance for school-age children, regular attendance to the educational meetings, growth monitoring of preschool children, and regular prenatal and post-pregnancy health care visits. The periodicity of health check-ups and attendance to educational meetings varies by household member.

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[3] Some references: Skoufias, Davis, and Behrman 1999a, 1999b; Behrman and Todd 1999; Skoufias 2005; Gutierrez, Bertozzi, and Gertler 2003; Rivera et al. 2000; Angelucci and Attanasio 2009.

[4] See for example Lundberg, Pollank, and Wales 1997; Thomas 1990; Duflo 2000. Rubalcava, Teruel, and Thomas 2009 show evidence consistent with that hypothesis for the Oportunidades case.

[5] A new transfer called "Senior Adults" was added in 2006, which is given to each adult 70 years old or older. Starting in 2007, households also receive an "energy component", a cash transfer established to help them face expenses related to energy sources. Starting in 2008, the program added the component "Vivir Mejor" which constitutes a fixed lump-sum transfer to compensate for food-price increases. Finally, in 2010 the component "Infantil vivir major" was implemented. It constitutes a fixed transfer for each child 0 to 9 years old.

[6] In 2003 Oportunidades added the component "youth with opportunities". It consists of a savings account that can be cashed when students graduate from high-school (12 years of education) if they graduate before they turn 22 years old.

The program was first implemented in rural areas, defined as communities with fewer than 2,500 inhabitants. Eligible localities were selected based on a marginality index which was constructed with the information available in the 1990 Mexican Population Census and the 1995 population count (Conteo). In the localities deemed eligible, Oportunidades carried out a census to collect information on every household. This information was then used to calculate a poverty index and identify beneficiary households. Then, those households were informed about their eligibility status. As a result, 97% of eligible households were incorporated to the program.

In 2001, marginal urban areas were incorporated into Oportunidades and urban localities were incorporated from 2002 on. Similar to the case of rural communities, census data were used to identify eligibility areas. However, a different household selection process was implemented in this case due to the fact that the share of potential beneficiaries with respect to the total population was expected to be considerably lower in these places[7]. Instead of collecting information on every household, registration offices were established in eligibility areas and advertising campaigns were carried out. Households that were interested in the program had to go to the registration offices on specific dates and answer an inclusion questionnaire. With that information households were immediately classified as qualified for the program or not. If qualified, they had to answer a second questionnaire and were visited later in their dwellings to confirm their eligibility status. As a result, the program resulted in much lower take-up rates than in the rural areas: administrative data suggests that about 50% of eligible households registered for the program.

Apart from the change in the household selection process, another important difference between the rural and urban components of the program has to do with the evaluation design. Among a subgroup of the rural communities classified as eligible to receive the program, a group was randomly selected to receive the treatment right away while the other group was assigned to receive treatment 18 months later[8]. By the year 2000 both groups were already under treatment. As a result, a new control group was incorporated into the evaluation sample in 2003 in order to estimate medium term impact effects. This new sample of localities was selected based on matching locality-level characteristics. Finally, a follow up survey was implemented in 2007 to evaluate long term effects.[9]

On the contrary, the urban evaluation design is not experimental. A sample of poor blocks was selected in 2001 from the areas assigned to receive the program in 2002. The control group was selected based on a matching process from localities planned to be incorporated to the program in 2004[10].

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[7] Marginal urban areas incorporated in 2001 were still under the previous system and so the selection process was the same as that in rural areas. Similarly, the household selection process applied to rural areas incorporated to Oportunidades in 2002 or later was a variant of that applied in urban areas (Gutierrez, Bertossi and Gertler 2003).

[8] Treatment communities started receiving the transfers in May 1998 and control communities during late 1999 and early 2000.

[9] The complete list of surveys that are part of the rural evaluation sample is: ENCASEH survey in 1997 (data used to identify eligible households), ENCEL surveys every six months between 1997 and 2000, ENCEL follow-up in 2003, ENCEL follow-up in 2007.

[10] The complete list of surveys that are part of the urban evaluation sample is: ENCERLUB survey 2002 (baseline), ENCERLUB follow-ups in 2003 and 2004.

2.2 Program's impact on nutritional status

Clearly, one of the components of the intervention is specifically designed to improve children's nutritional status. Both pregnant or lactating women and young children are given nutritional supplements on a regular basis. Additionally, two other components of the program are likely to affect the nutrition of young children. First, there is the universal monetary transfer which is aimed to improve the quality of the diet families consume. Secondly, there are the educational talks where health and nutrition related issues and practices are discussed. Trained personnel on the subject lead these meetings and it is mostly women who attend them, two factors that are expected to maximize the impact on children's nutrition[11].

However, participation in the program does not necessarily result in better nutrition. To begin with, nutritional supplements need to be consumed. There is some evidence suggesting that in both rural and urban areas access and consumption was not universal. With respect to the former case, Behrman and Hoddinott 2005 report that, during the first years of the program, the percentage of children aged 4 to 48 months that had access to the supplements varies from 52% to 63%. Similarly, during the first years of the urban program, Neufeld et al. 2004b show that about half of the children aged 6-23 months took the nutritional supplements at least once a week, and only about a quarter of lactating women did. With respect to the other two channels, their influence depend on the degree to which money is actually used to improve the nutritional quality of the food consumed and the extent to which women implement what they learn in the educational sessions.

3. Current evidence of Oportunidades' impact on child nutrition. Findings and Limitations.

The literature has evaluated the impact of the program on child nutrition by looking at the effects on height because height constitutes a marker of early-child nutritional investments[12]. Given the big differences between the rural and urban parts of the program in terms of timing, selection process and evaluation design, each part is analyzed separately.

3.1 Oportunidades in rural areas

Two studies evaluate the impact of Oportunidades after one year of exposure in rural areas. By exploiting the experimental design of the survey, Gertler 2004 analyzes the impact on children aged 12 to 36 months in 1999. He finds that children in treatment villages are 1 cm taller than children in control villages, but also finds no significant effect on the probability of being stunted (more than two standard deviations below the reference median). On the contrary, Behrman and Hoddinott 2005 find no significant impact of Oportunidades on child nutrition when using an estimation strategy that relies on the random allocation of the program. However, based on the evidence that a shortage of supplements did not allow every eligible children in treatment areas to have access to them (and the fact that some children with severe malnutrition symptoms in control areas did receive them), they estimate next a

[11] Better access to preventive and curative health care services may be a fourth channel through which the program improves nutritional status.
[12] Other health outcomes evaluated in the literature include: obesity, anemia, weight-for-height, BMI-for-age, birthweight, probability of illness.

treatment-on-the-treated effect. They control for the fact that access to the supplements was not random among eligible children using child fixed effects estimators and find that the program did increase growth per year by over 1 cm on children 12-36 months. They also evaluate the effect on the probability of stunting and find that children who receive the supplements have a predicted probability of stunting of one-third that of control children.

Rivera et al. 2004 also estimate the impact of one additional year of exposure to the program. However, they compare children with two years of treatment relative to children with one year (instead of one year relative to no exposure as was the case of the previous two studies). They only find a positive impact on children 6 months or younger that live in the poorest households who are on average 1 cm taller than children the same age with one year of exposure.

Neufeld et al. 2004a incorporate the 2003 round into the analysis. Using matching estimates, the study compares children in both early and late intervention communities (those that started receiving treatment in 1998 and those that were incorporated to the program 18 months later) with children residing in the new control communities. The authors find that children 24 to 71 months old in 2003 in the former group grew 0.67 cm more on average than control children and the prevalence of stunting is 12.4% lower (both effects statistically significant). They also compare the effects of differential exposure using the original treatment and control groups. Children 48 to 71 months in 2003 were fully exposed to the program if born in early intervention communities, but only partially exposed if born in late intervention communities. An evaluation of this differential exposure reveals no significant difference in height-for-age or prevalence of stunting between these two groups.

Finally, Fernald, Gertler, and Neufeld 2009 evaluate the effect of additional 18 months of exposure almost 10 years after original treatment communities started to receive the benefits. They use height measured in 2007 and restrict the sample to those children born between March 1997 and October 1998. Children in early intervention communities were around 1 year or younger when they started receiving the supplements whereas children in late intervention communities were more than 1 year old. No effects were found on height-for-age z-scores for the whole group, but there was an effect of about 1.5 cm on height in older children whose mothers had no formal education.

This summary reveals a mixture of positive and null impacts which depend on the methodology used, the difference in the degree of exposure, the time at which nutritional indicators are measured and the subgroup of children evaluated. In order to understand these differences, and motivate the value of the current analysis, the most important caveats that the rural evaluation sample faces are presented next. Some of them are common to any study that uses the rural evaluation sample while others are specific to evaluations that use nutrition indicators.

Short-term impact evaluations exploit the randomized design of the program, which help control for unobserved factors that differ between treatment and control individuals. However, the randomization was done at the locality level whereas impact estimates are performed at the household or individual level. While treatment and control groups look alike at the locality level, Behrman and Todd 1999 evaluate the differences in pre-program characteristics between treatment and control households and find that there are small but significant differences between these two groups. Additionally, a recent study shows that attrition, which was mainly ignored in this part of the literature, could potentially affect the results. Teruel and Rubalcava 2007 show that treatment households are more likely to leave the sample by the year 2000 than control households. The authors re-estimate the impact on high-school enrollment presented in Schultz's 2004 and find that correcting for attrition results in higher impact estimates. As a result, short-term impact estimates could be biased due to deviation from perfect randomization and/or differential attrition rates between control and treatment groups.

Medium-term impact estimates face additional concerns. In the first place, they rely on matching estimators that assume that the relevant differences between control and treatment individuals can be controlled with observable characteristics. One immediate threat to this assumption results from the fact that the new control group in 2003 resides in localities that belong to different geographic areas than the original control and treatment groups. As a result, any region-specific factor that cannot be controlled for can bias the results. There is also evidence of significant differences in terms of 1997 characteristics between the original evaluation group (treatment and control) and new control group. Parker, Rubalcava, and Teruel 2008 show that such differences include demographic characteristics, dwelling characteristics, ownership of durable goods, and household head and spouse characteristics. This situation can be partially overcome using difference-in-differences matching estimators which help control for time-invariant unobserved characteristics. However, these estimators face additional problems. These problems have to do with the fact that the new sample was drawn in 2003, and pre-intervention information of the new control group is based on questions that ask this group about their situation in 1997. As a result, difference-in-differences matching estimators rely on retrospective information which means that recall bias should be taken into account. Additionally, the possibility of sample selection bias should be considered given that the sample of households in 2003 may not be representative of the group of households that were there in 1997. Finally, attrition rates are not low: 83% of the households are in both the 1997 and 2003 surveys, and only 60% report information in every survey between those two years (at the individual level, the rates are 78% and 47% respectively)[13]. To the extent that people that remained in the sample are different than people that left in dimensions that are correlated with the outcome of interest (and cannot be controlled for in the estimation), high attrition rates constitute another threat to the analysis.

Fernald, Gertler, and Neufeld 2009 is the only study that uses the 2007 round. As the authors mention, the main limitation of the study are the high attrition rates. They found no differences in characteristics measured at baseline between treatment and control groups for the sample found in 2007, but there were some differences between household characteristics of children used in the analysis and those lost.

In addition to the caveats already mentioned, there are two things that are specific to the nutritional data used to assess the program impact on height.

[13] The survey was not designed to follow households but to come back to original dwellings, which explains why more than 80% of the attrition between 1997 and 2003 can be attributed to changes of residence or migration (Teruel and Rubalcava 2007).

In the first place, indicators of nutritional status were not collected as part of the general evaluation survey. The data was collected at different times and by different teams, under the supervision of the National Institute of Public Health. This seems to have introduced some complications when trying to link nutritional indicators with the rest of the household and individual information. Furthermore, because of the difference in the timing, the first available indicators are measured at a time some households have already received some transfers, and so they do not correspond to pre-treatment data. Finally, the data was collected from a subsample of children and only a small number of them were measured both in 1998 and 1999 even though a larger number was measured in 1998 and 2000.

Secondly, there is evidence of shortage in the availability of supplements in the first years of the program. Adato, Coady, and Ruel 2000 report that the distribution and intake of nutritional supplements seem to have been the most serious operational problem of the health component of Oportunidades. In response to this, health personnel exercised some discretion in the distribution of supplements by especially targeting those children that presented the most severe malnutrition symptoms. As a result, access to this component of Oportunidades among beneficiaries was, not only not universal, but also selective[14]. This implies that short-term impact estimates and estimates of differential exposure between the original treatment and control groups estimate intent-to-treat effects and may explain the lack of significant impact in some cases. Behrman and Hoddinott 2005 provide some evidence of this.

3.2 Oportunidades in urban areas

The two main characteristics of the urban evaluation sample were already mentioned in section 2: it does not follow an experimental design, and take-up rates were very low (around 50%). The challenges of dealing with a non-experimental sample were exposed in the previous subsection when explaining the caveats of using the new 2003 control group to evaluate medium effects in the rural areas. The second element, however, introduces a new challenge to the estimations because eligible households that decided to enroll in the program are not expected to be a random sample of the set of eligible households in urban localities. Angelucci and Attanasio 2006 argue that traditional matching estimators, designed to control for non-random assignment to the program, may give biased estimators in the presence of non-random participation. The reason is that matching estimators rely on the assumption that variables that determine both participation and outcomes are observed. They propose an IV-type estimator that takes both nonrandom assignment and nonrandom participation into account and apply it to the case of food consumption. They find that the estimated impact changes significantly when they use a traditional matching estimator compared to their preferred estimator. Parker, Todd, and Wolpin 2005 also use a combination of matching, differences and IV to estimate the impact of Oportunidades on schooling, and show that impact estimates vary as a function of the comparison group chosen. There is, however, no evidence on how much this would affect impact estimates on health outcomes.

As in the rural case, the use of nutritional data faces additional concerns. Children with nutritional information were not randomly chosen from the urban evaluation sample, but explicitly chosen to minimize the number of geographic areas in order to save costs. As a result, control children are not

[14] Behrman and Hoddinott 2005 find evidence of selective access to the nutritional supplements, but no evidence of selective access to the other components of the program.

children who live in communities not yet incorporated to Oportunidades at that time, but children from eligible households that reside in the same communities as treated children but did not enroll for the program. Even though this eliminates any bias due to locality-specific effects, it significantly raises concerns related to self-selection bias, especially considering what was mentioned in the previous paragraph. Additionally, height was measured both in 2002 and 2004 only on children younger than 2 years old at baseline[15], which limits the possibility of evaluating the effect on different age groups or performing robustness checks - the last of which seem to be crucial given the limitations of the data just exposed.

Based on this data, Leroy et al. 2008 evaluate the impact of Oportunidades in urban areas on children younger than 24 months at baseline (2002). They use a two-year panel of 432 children and implement a difference-in-differences propensity score matching estimator. After two years of program exposure Oportunidades seems to have had no impact on growth in children 6 to 24 months but a positive impact on children less than 6 months old: the height-for-age z-score of the latter group is 0.41 higher than that of control children. They claim that selection bias is not likely to affect the results given that no significant differences were found at baseline between control and treatment groups in terms of height for children 2 to 4 years old. They also claim that loss of follow-up, which was 45% and 40% for control and treatment children respectively, is not a concern because there are no significant differences in baseline characteristics between children lost and children used in the analysis. However, their robustness checks, even though encouraging, do not seem conclusive.

4. DATA AND DESCRIPTIVE ANALYSIS

The main data source of this paper is the Mexican Family Life Survey (MxFLS). This is an on-going longitudinal survey that collects a rich set of information on demographic and socioeconomic characteristics of individuals, households, and communities. The sample has national, rural-urban and regional representation. The first wave (MxFLS1) was conducted in 2002 and interviewed 35,677 individuals in 8,440 households. These households reside in a total of 150 communities located in 16 different states[16]. The second wave (MxFLS2) was conducted during 2005-2006 and achieved a 90% re-contact rate at the household level. This wave consists of 36,946 individuals and 8,434 households, who due to migration decisions are located across 247 localities in 21 states throughout Mexico. The third wave (MxFLS3) started in 2009 and is in the final stages of the field work.

For every household member, MxFLS records anthropometric measures (weight and height) that were taken by specialized personnel from the National Institute of Public Health (INSP). As a result, MxFLS constitutes one of the few surveys that have both well measured anthropometrics (as opposed to self-reported measures) as well as a very detailed set of socioeconomic variables, including income and consumption. Height, being a marker of early-child nutritional investments, constitutes the main variable of interest in this paper. In order to control for age-gender specific differences, height-for-age z-

[15] Children's mothers were also measured, and children 2 to 4 years old at baseline were measured in 2002 but not in 2004.
[16] Mexico is divided into 31 states and the Federal District.

scores are constructed using the 2000 CDC Growth Charts for the United States provided by the National Center for Health Statistics (NCHS).

The survey collects information on Oportunidades participation at the individual, household and community level. However, in order to control for self-selection issues, the identification strategy follows an intent-to-treat approach and exposure is defined at the locality level. More specifically, the baseline analysis will only take into account children´s age and place of residence when defining program exposure[17]. Therefore, we need to identify the year in which each MXFLS community was incorporated to the Oportunidades program. In order to do this, this paper combines MxFLS data with the information available on the Oportunidades webpage as of April 2010[18]. Here there is a complete list of Oportunidades beneficiaries (Oportunidades' padron), a list that consists of around 5 million records with the following information: locality of residence, complete name, date of enrollment in the program, date of exit (if relevant), and concept and amount of transfer received in the last two months of 2009. Based on the households' date of entry, each of the 246 MxFLS localities is associated with the year in which the largest number of households was enrolled in the program[19]. Figure 1 illustrates the pattern of expansion in rural and urban areas separately[20].

To classify communities as rural or urban the 2000 Mexican Population Census is used. Following the Oportunidades definition, rural communities are defined as those with 2,500 inhabitants or less.

As will be clear in the following section, the analysis will be performed with children 1 to 3 and 5 to 7 years old. Table 1 characterizes the final sample. The original sample of children 1 to 3 and 5 to 7 years old in rural communities is 3,500 and that of urban communities is 4,849. Some cases are dropped from the analysis due to lack of measurement, and only a few additional cases are lost due to measurement

[17] A second definition of exposure will make use of the eligibility criteria that Oportunidades uses to select beneficiary households. This confidential information was made available through contact with Oportunidades administrators. By following their eligibility criteria it will be possible to use a definition of exposed children that is closer to the actual one, without reintroducing self-selection issues. This analysis is in its preliminary stages so it's not reported in this version of the paper.

[18] http://www.oportunidades.gob.mx/

[19] In some localities there is a difference between the earliest year in which any household was enrolled in the program (minimum year) and the year in which most households were enrolled in the program (mode year). However, in most of these localities the number of households enrolled in the minimum year is really low (mostly less than 4 households), and in all of them the difference (both in absolute and relative terms) between the number of households enrolled in the minimum year and the number of households enrolled in the mode year is huge. Additionally, the pattern of expansion based on the minimum year contradicts the documented pattern of expansion of the program. Therefore, the analysis is based on the mode year.

[20] Officially, Oportunidades expanded to urban areas in 2001. The vast majority of households in urban areas that enrolled in the program before 2001 are either in semi-urban areas (2500-5000 inhabitants) or areas classified as rural before the 2000 Population Census (Parker, Ruvalcaba and Teruel 2008). The characteristics of the five urban localities that are assigned a year of incorporation before 2001, are consistent with this evidence.

error or change of residence[22]. As a result, 86% of the rural sample and 79% of the urban sample are used in the analysis that follows[23].

Summary statistics of the 2002 and 2005 z-scores show that children 1 to 8 years old in Mexico are, on average, 0.56 standard deviations below the reference median in 2002 and 0.42 below the reference median in 2005 (standard deviations are 1.26 and 1.47 respectively). The percentage of stunted children, that is, children that are more than two standard deviations below the reference median, is 12% in both years.

As expected, the nutritional status of children in the rural sector is worse than that of children in the urban sector. Rural children are on average 0.75 and 0.59 below the reference median in 2002 and 2005 respectively, whereas the corresponding numbers for urban children are 0.42 and 0.29. With respect to stunting, the overall incidence of 12% is a combination of an incidence of stunting among rural children of 16% and an incidence of stunting in urban children of 10%.

5. IDENTIFICATION STRATEGY

The identification strategy followed in this study exploits the combination of three elements: the evidence that nutritional interventions have only modest effects on children's height after they reach a certain age, the fact that Oportunidades was not introduced in every place at the same time but gradually expanded over the years, and the panel dimension of MxFLS. Based on the first two, program exposure is defined as a function of the age of the child at the time Oportunidades was introduced to the locality of residence. The idea basically consists of identifying cohorts of children that were exposed to the program and cohorts of children that were not, and then performing an impact analysis at the community level. Variations of this empirical methodology have been successfully implemented in the nutrition and economics literatures (see, for example, Martorell and Habicht 1986; Duflo 2001; Frankenberg, Suriastini and Thomas 2005).

The biology of child growth suggests that the critical years during which nutrition interventions have the highest effect are from 0 to 4 years. When children turn 4 years old, the influence of nutrition interventions is substantially reduced (Martorell and Habicht 1986). Based on this evidence, and the fact that children's height-for-age constitutes an indicator of early life nutritional investments, two conclusions can be made. First of all, if Oportunidades affected children's nutritional status, that effect should be reflected in height-for-age measures. Additionally, Oportunidades could have only affected height of children younger than 5 years old. This is what constitutes the basis of the treatment definition used in this analysis: treated children are defined to be those that were younger than 5 at the time

[22] Children that moved between 2002 and 2005 are eliminated from the 2005 sample because treatment status based on the place of residence does not correspond to actual treatment. See Section 5 for more details.

[23] A selection analysis will be performed to evaluate whether having missing height is correlated with observable characteristics that could affect the results.

Oportunidades arrived to the locality of residence. Children 5 years or older constitute the control group[25].

Given this locality-level definition of exposure, the identification of the impact of the program would be relatively straightforward if the geographic expansion of the program had been random: children from localities not exposed by the time height was measured could be used as the control group. However, the expansion of the program was far from random. As already mentioned, it started in rural areas, and the localities were specifically chosen based on a marginality index. Around the year 2001, it expanded to cover marginal urban areas, and then gradually expanded to cover virtually all the country. Controlling for observable locality characteristics might mitigate the problem, but it is highly unlikely that those covariates would capture all of the locality-specific effects on height. Therefore, program placement will be controlled for using differences within homogeneous groups of localities that will be defined below.

To identify the impact of the intervention by exploiting variation in the level of exposure within localities, we will need to compare older (not exposed) with younger (exposed) cohorts. However, the standardized height-for-age is nonlinear in age, so that it is not possible to compare older and younger children at a given point in time. To overcome this difficulty, a modified version of the older-younger cohort comparison is used by exploiting the fact that height is measured at two points in time: 2002 and 2005. This allows comparing cohorts of children exposed and not exposed to the program using height measures taken when they are the same age.

Figure 2 helps explain the main idea. Time in years is represented on the horizontal axis and age on the vertical axis. The vertical lines at 2002 and 2005 correspond to the years MxFLS measures height, and the diagonal lines identify different cohorts: older, middle and younger.

Four cohort-time groups are used to identify the treatment effect: older and middle cohorts in 2002 and middle and younger cohorts in 2005. Each of these groups will have experienced a different level of exposure to the program at the time height was measured, and this level of exposure depends on the community of residence. The younger cohort includes children born between 2001 and 2003, so that they are between 1 and 3 years old in 2005 when their height is measured. The middle cohort includes children born between 1997 and 2000 so that they are between 1 and 4 years old in 2002 and between 4 and 7 in 2005. Finally, the older cohort includes children born between 1994 and 1996, so that they are between 5 and 7 in 2002[26].

Next, localities are divided into different groups depending on the year they were incorporated into the program. The rationale behind this criterion is based on the fact that Oportunidades followed a specific

[25] Throughout this section exposure is always defined in terms of height. Older cohorts clearly benefit from the program in other dimensions.

[26] In the estimation only a subgroup of the middle cohort is used in order to compare groups of children as similar as possible. More specifically, since the younger cohort in 2005 will be compared to the middle cohort in 2002, only children between 1 and 3 years old in 2002 are included in the analysis. Similarly, the middle cohort in 2005 will be compared to the older cohort in 2002, so only children between 5 and 7 years old in 2005 are included in the analysis. These subgroups of the middle cohort are highlighted in Figure 1.

locality-targeting policy to incorporate new localities over time. Therefore, the use of date of incorporation is expected to define groups of localities that are relatively homogeneous.

Taking into account both the pattern of expansion over the years and the fact that height is measured in 2002 and 2005, four different groups of rural localities and three groups of urban localities are identified. In the rural sector the groups are the following: the first group consists of localities that were incorporated at the very beginning of the program, in 1997 or 1998 (referred to as type-α communities); the second group consist of localities incorporated right after type-α localities, between the years 1999 and 2002 (type-β communities); the third group includes the localities that received the program between 2003 and 2005 (or type-γ localities); and finally the fourth group includes the localities that either received the program after 2005 or never did (type-δ or baseline group). In the urban sector only the last three groups of localities exist[27][28]. Note that the selection of the groups is closely related to the two years in which height measures are taken. In other words, the three groups of urban localities correspond to those incorporated to the program up to the time the first measure of height was taken (2002), those incorporated between the two years measures were taken (2002-2005) and those without Oportunidades by 2005. Rural localities have an additional fourth group that corresponds to those localities incorporated to the program when Oportunidades had just begun (1997 and 1998). These groups and the time of incorporation are shown at the bottom of Figure 2.

With the given definition of cohorts and locality types, the following regression equation is estimated:

$$\theta_{icvt} = \delta_{ct} + \alpha_{ct} I_\alpha + \beta_{ct} I_\beta + \gamma_{ct} I_\gamma + \sigma'_{ct} X_{ivt} + \varepsilon_{ivt} , \qquad (1)$$

where i represents the individual, c the cohort, v the community and t time[29]. The specification imposes four different time-cohort intercepts: an intercept for the middle and older cohorts at time zero (year 2002), and an intercept for the middle and younger cohorts at time one (year 2005). They represent the time-cohort specific intercepts of children that live in baseline communities (δ_{ct} terms). Each of these four intercepts interacted with a dummy that represents type-α communities estimate, for the corresponding time-cohort group, the differences on height-for-age of children in communities that received the program between 1997 and 1998 relative to children in baseline communities (α_{ct} terms). For instance, α_{o02} measures the difference, in 2002, between the average z-score of children in the older cohort in type-α communities and the average z-score of children in the older cohort in baseline communities. β_{ct} and γ_{ct} are interpreted in a similar manner. The set of covariates X includes: gender, age in months, presence of mother and father in household, height of mother and father, education of the mother, and state of residence. Note that the effect of each of these covariates is allowed to change across the four cohort-time specific groups.

[27] Type-β communities are not exactly the same in the rural and urban sector, because in the urban sector this group includes three localities incorporated to Oportunidades in 1998 (See Figure 1). In terms of interpretation and exposition, however, they should be thought of as the same thing.
[28] Results are robust to reclassifying the urban localities introduced to Oportunidades before 2001 as rural (See footnote 20).
[29] The specification for the urban sector does not include the α_{ct} terms.

Table 2 shows the expected effects of the program as a function of the time height was measured, the cohort of the child, and the locality where he/she lives[30]. Panel A shows the impact in rural communities and Panel B in urban communities. By 2002, children in the older cohort were partially exposed to the program if born in type-α communities (they were between 1 and 4 years old when the program arrived in 1997-1998), but they were too old to be exposed to the program if born in type-β or type-γ communities[31]. By 2005, the younger cohort was fully exposed to the program if born in either type-α or type-β communities, but only partially exposed if born in type-γ communities. The rest of the cells are filled following the same reasoning.

These null, partial and full effects, however, cannot be directly identified from the parameters estimated in equation 1. The reason is, as mentioned before, that type-α, β and γ communities are potentially different than baseline communities in ways that might affect children's height. If that is the case, estimated coefficients confound the effect of exposure to Oportunidades and program placement. However, these coefficients do provide direct evidence of selective program placement. That would be the case, for example, if γ_{o02} is significantly different from zero. This implies that older children in type-γ communities are different from older children in baseline communities in terms of height in 2002, but this difference cannot be attributed to the program because Oportunidades was not present in any of these communities at the time height was measured.

Instead of using directly the estimated coefficients of equation 1, a differences-in-differences approach is followed, which allows identifying the following parameters of interest:

Type-α communities (received the program between 1997 and 1998): $\alpha_{m05} - \alpha_{o02}$ gives an estimate of the program effect under full exposure relative to partial exposure. Because both coefficients are measured relative to children in baseline communities, the difference between them controls for any locality-specific effect that is common to both cohorts, eliminating the bias due to program placement. Furthermore, we are comparing children 5 to 7 years old in both cases, so that the dynamics of the z-score does not affect the results. However, this estimate also includes any time effect that might have existed between the time period the older cohort was in its critical years and the time period the middle cohort was in its critical years. If these communities experienced growth between those two periods, the middle cohort may have been exposed to a better environment than the older cohort when they both were between 1 and 5 years old. Under those circumstances, the difference

[30] Throughout the analysis the terms "live" and "born" are used interchangeably. The reason is that the identification strategy is implicitly assuming that if a person lives in a given community in 2002 that person was born in that community (Technically, the assumption is somewhat weaker: what is needed is the child to be born in a community that belongs to the same group as the locality of residence). Only if that is true the year the community of residence was incorporated to Oportunidades can be used to determine level of exposure. In a future version of the paper the validity of this assumption can be assessed using the information households give on migration history.

[31] Figure 1 suggests that the assumption of null impact on children in the older cohort if born in type-β communities is more accurate in the case of urban localities than in rural localities. This is the case because over 90% of type-β urban communities got the program in 2001 and 2002 while only 65% of type-β rural communities did so. There are nine rural localities that got the program in 1999, so that some children in the older cohort (those between 2 and 4 years old in 1999) may have been affected by the program. In the case of type-γ communities the impact is definitely zero, given that the program arrived to the locality after height was measured.

between α_{m05} and α_{o02} would include both the additional exposure to Oportunidades and the improvement over time that would have happened regardless of the program. However, Table 2 suggests a way to overcome this issue. By assuming that the time effect is homogeneous across cohorts, it can be controlled for using the difference in the estimated coefficients that correspond to the younger cohort in 2005 and the middle cohort in 2002, that is: $\alpha_{y05} - \alpha_{m02}$. Both parameters estimate the effect of full exposure to the program in type-α communities at a time both groups were the same age. As a result, that difference can only be a consequence of time effects. Therefore, the double difference $(\alpha_{m05} - \alpha_{o02}) - (\alpha_{y05} - \alpha_{m02})$ should give an unbiased estimate of the program effect under full exposure relative to partial exposure. Note that the identification assumption regarding time effects is very specific. First of all, we need that the difference in the environment between the time period the older cohort was 1 to 5 years old and the time period the middle cohort was 1 to 5 years old, is exactly the same as the difference between the time period the younger cohort was 1 to 5 years old and the time period the middle cohort was 1 to 5 years old. The time effect between 2002 and 2005 is not the relevant concept because the time trend between those years affects the height of the three cohorts differently. For instance, if the time trend was not constant but these communities were constantly improving, the double difference would produce a lower bound of the real impact[32]. Secondly, we need that this homogeneous time effects across cohorts is also the same across communities.

Type-β communities (received the program between 1999 and 2002)**:** Table 2 suggests that $\beta_{y05} - \beta_{m02}$ gives an estimate of full relative to partial exposure confounded with time effects, and $\beta_{m05} - \beta_{o02}$ gives an estimate of both partial exposure and time. As a result, cleaning the estimate from the time effect is more complicated in this case. As opposed to the other two groups of communities, this group does not have two cohorts of children with the same level of exposure to the program so that the difference between them can be attributed to time effects.

Type-γ communities (received the program between 2003 and 2005)**:** For this group of communities an unbiased estimate of partial program exposure can be produced under the same assumptions mentioned for type-α communities. $\gamma_{y05} - \gamma_{m02}$ estimates the combined effect of partial exposure and time, while $\gamma_{m05} - \gamma_{o02}$ provides an estimate of time effects[33]. As a result, the double difference $(\gamma_{y05} - \gamma_{m02}) - (\gamma_{m05} - \gamma_{o02})$ gives an estimate of partial exposure to the program clean of placement and time effects. Note that the double difference in this case is equal to minus the double difference in type-δ communities: the difference that measured the main effect in the previous case now controls for time trends, and vice versa. This implies that the underlying assumption regarding time trends is exactly the same.

[32] In a future version of this paper some evidence of this assumption can be provided using the information available at the community level or looking at alternative data sources.

[33] See that now the estimate of time effects is identified with the difference between two groups that were not exposed to the program, rather than with two groups that were fully exposed to it as was the case in type- α communities.

Strengths and limitations of the analysis

The combined use of the biology of child growth and the expansion of the program to identify children exposed to the program and children not exposed to it constitutes a powerful identification strategy to estimate what is known as an intent-to-treat effect.

In the first place, the fact that the definition of exposure is exogenous to the household implies that the analysis is free from selection bias. Household-specific factors correlated with both actual treatment status and the outcome of interest do not affect the estimates here[34]. Previous literature summarized above provides evidence of the importance to control for selective access and selective participation.

Additionally, the definition of treatment at the locality level makes the analysis robust to the existence of spillover effects. There is no evaluation assessing the existence of these effects on health outcomes, but there is evidence on other dimensions. Bobonis and Finan 2006 find that the program significantly affected enrollment rates among non-beneficiary children and Angelucci and De Giorgi 2009 find that Oportunidades increased food consumption among non-beneficiary households.

However, as expected, the analysis also faces some limitations. Some have to do with the definition of exposure, whereas others have to do with the implicit assumptions needed for the differences-in-differences to identify the true impact effect.

In the analysis described so far, exposure is only determined by the age of the child, and no other socio-economic characteristic of the household is taken into account. However, the program is not designed to benefit every household in the locality, but to reach households deemed poor. Better-off households are not eligible and therefore their children are not expected to benefit from the program. As a result, both treated and non-treated children are included in the treatment group[36].

To the extent that non-treated children cannot be made worse-off by the program, the estimated impact in this way would provide a lower bound of the program effect on children's height. However, the analysis may end up being uninformative if there is not enough power to identify positive impact effects. This would be the case if the share of untreated children in the cohort exposed to the program is sufficiently large. The degree to which exposed cohorts were actually affected by the program will vary by locality. In places with a higher proportion of poor households the proportion of non-treated children should be lower than in places with a low proportion of poor households. Given that Oportunidades was first introduced in more marginal places, the identification strategy is expected to be weaker as we

[34] Implicit here is the assumption that the time Oportunidades arrived to the locality of residence is exogenous to the household, which holds if Oportunidades did not induce migration from places without the program to places with the program. Note additionally that the fact that children's height cannot be affected after they reach certain age rules out the possibility that parents compensate untreated children. If that was the case, behavioral responses induced by program participation would need to be considered even if treatment status was exogenous to the households.

[36] Every intent-to-treat effect estimator includes both treated and non-treated individuals. Note, however, that the share of non-treated individuals is expected to be higher in this case than in more traditional impact estimates. The reason is that this analysis not only classifies as treated those children that are eligible but decided not to participate or children that participate but have limited access, but also children not designed to participate.

evaluate communities incorporated later in time. This effect is reinforced by the fact that the household selection process in localities incorporated after 2001 was different from the one applied before, which resulted in much lower take-up rates among eligible households after 2001.

As a result, the estimates obtained from this analysis will depend on the strength of the correlation between children exposed to the program and those actually affected by it. In other words, a trade-off between power and bias is faced. The implemented strategy is robust to selection bias or spillover effects, but relies on the fact that the effect of the program on exposed children is big enough so that it can be identified after including non-treated children.

An additional limitation is the sensitivity of the results to the existence of time trends. If different cohorts were exposed to different shocks, the estimated impact will not be able to disentangle the effect of the program from the effect of these shocks.

Finally, the fact that the analysis compares children from different cohorts at different times makes the identification strategy subject to potential sample selection bias. For instance, if there is selective migration, it could be that children in a given age group in 2005 is significantly different than children in the same age group in 2002, in dimensions other than the exposure to Oportunidades. In that case, the difference in height between these two groups could not be interpreted as a program impact effect[37].

6. RESULTS

Table 3 shows the preliminary results[38]. The upper panel is a copy of Table 2, showing the expected impact of the program as a function of the cohort of the child, the year height was measured and the type of locality in which he/she resides. Panel B below shows the estimated results for the rural sector, and Panel C at the bottom shows the results for the urban sector. Both Panel B and C consist of three blocks. The upper block shows the estimated coefficients corresponding to equation 1 (standard errors below each coefficient). In Panel B, for example, the cell corresponding to the older cohort in type-α communities in 2002 (-0.663) shows the estimated value of α_{o02}, when equation 1 is restricted to the rural sample. The middle block shows, for each type of locality, the estimates of the first differences suggested in the Identification Strategy section. For example, the line named “Full relative to partial exposure, time effects included” is the estimate of $\alpha_{m05} - \alpha_{o02}$ in the case of type-α communities, and of $\beta_{y05} - \beta_{m02}$ in the case of type-β communities. As can be inferred from Panel A, it is not possible to get an estimate of this effect for type-γ communities. Finally, the bottom block shows the double difference estimates, or the estimates that provide unbiased impact effects under the assumptions made in the previous section. P-values are reported below impact estimates in the middle and bottom blocks of Panels B and C.

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[37] Note that this selection bias has to do with the sample of children surveyed/found in a locality at different points in time, and not with the selection into treatment. Selection into treatment is controlled for with the exogenous definition of exposure followed in this paper.

[38] Standard errors are clustered at the locality level.

3.1 Estimated impact in the rural sector

The first thing to notice in Table 3 is the suggested evidence of selection in program placement. As expected, rural communities incorporated to Oportunidades before 2005 are statistically different from communities incorporated later (or never incorporated) in terms of average children's height. β_{o02}, γ_{o02} and γ_{M02} are negative and significant at the 5% level; which confirms that Oportunidades was introduced in communities where children's nutrition status was worse off. The analysis cannot provide direct evidence of placement selection in type-α communities. However, the Oportunidades selection process specially targeted the poorest communities, so that they are also expected to be statistically different from baseline communities. As a result, simple comparison between children exposed and not exposed across communities will not provide unbiased estimates of program effects.

Results also suggest that Oportunidades had a positive effect on children's height in type-α communities, which are the communities first introduced to the program. According to these estimates, children fully exposed to the program are on average 0.6 standard deviations taller than children that were only partially exposed, and this impact is statistically significant at the 10% level. For example, the magnitude of this impact represents, for a four-year-old boy, 2.8 cm. Additionally, there seems to be no time effect on these communities: the estimated coefficient of time trend is small in magnitude and not statistically different from zero. This suggests that younger children fully exposed to the program by 2005 have on average the same height than children in the middle cohort fully exposed to the program by 2002.

As explained in the previous section, an estimate of program exposure for type-β communities that is not confounded with time effects cannot be identified. However, something can still be learnt about these communities. Table 3 suggests that the joint effect of partial exposure and time is positive as well as the joint effect of full exposure and time relative to partial exposure, although neither is statistically significantly different from zero. However, partial exposure is slightly bigger than full-relative to partial exposure and it is in the limit to be marginally significant. Therefore, it could be that partial exposure has an impact on children's height and every additional year beyond that has almost no effect. If children in type-β communities are sufficiently better than children in type-α communities it does not seem unreasonable to find that every additional year produces a significant impact on children's nutrition in the latter group but not in the former. However, results are too preliminary to reach such conclusion.

In type-γ communities children do not seem to benefit from the program: the estimated effect of partial exposure is not significant. This estimate is of the result of a positive time trend (that is significant at the 10% level), and an insignificant joint impact of partial exposure and time. Therefore, even though the net impact is not significant, the comparison of these two effects, together with the prior that Oportunidades should not have a negative impact on height, raises concerns about the existence of effects that the analysis is currently not controlling for.

3.2 Estimated impact in the urban sector

In the case of urban communities, there is no evidence of selective program placement. The estimated coefficients of the older cohort at time zero are both close to zero and statistically insignificant, and the coefficient corresponding to the middle cohort at time zero (that should also be zero) is positive and marginally significant.

As in the rural sector, the joint effect of program exposure (partial or full) and time effects is not significant for children in type-β communities. Additionally, the comparison between these two effects suggests again that if one is going to be relevant, it would be partial exposure. In this case, however, only important negative trends would make the impact of the program significant for urban children.

Finally, there seems to be no impact on children in type-γ communities.

Interpretation of the results

Except for children in the rural localities introduced to Oportunidades at the beginning of the program (1997-1998), these preliminary results suggest that the program did not significantly affect children's nutritional status. There are different ways to interpret the results, which are closely related with the limitations mentioned in the previous section.

The first possibility is that time trends are not appropriately controlled for. If older and younger children were exposed to different environments beyond Oportunidades participation, then the double difference does not isolate the impact of the program from any other factor that differentially affects exposed and non-exposed children. In particular, there is some evidence that some localities (or households) were incorporated into Oportunidades because they suffered a negative shock (in most cases, due to hurricanes). If this was the case, children in their critical years were subject to two interventions that affect nutrition in opposite directions and there is no way to disentangle those two effects. We were able to get a list of localities in which some households were enrolled in the program under a special process due to these kinds of reasons. Fortunately, only 3 MxFLS localities were in this list and results are robust to the exclusion of these localities. However, more work will be done to try to evaluate the existence of confounding factors. In particular, the MxFLS community questionnaire can be used to see whether the localities were exposed to negative shocks over the last years. Indirect evidence can also be provided applying the estimation strategy to an outcome that was not affected by the program. Recent history of household income or wages can also be analyzed to see if there is a correlation between the dynamics of income indicators and the time at which the different cohorts were in their critical growth years.

The second possibility is that the estimated coefficients do not reflect the positive effect that the program had on the subgroup of exposed children because the ratio of exposed relatively to non-exposed children is very low. This is expected to be particularly important in urban communities due to the high degree of heterogeneity in socioeconomic status across households and the lower share of "program-eligible" households. In order to analyze whether this is the reason there seems to be no impact on children's height, we are currently working with the eligibility criteria that Oportunidades uses to select beneficiary households. Through contact with program administrators we were able to get the exact formula the program uses to compute the eligibility score. Fortunately, the household-level score used is not a function of variables that are likely to vary substantially between different data sources, so that eligibility would be very sensitive to the way such variables are measured[39]. Additionally, most of the variables that determine eligibility are relatively stable over time, so that the score a household is assigned does not vary substantially as a function of the year the score is computed. The analysis of this eligibility criterion is still in its preliminary stages, so it will be included in future work.

Alternatively, it could be that selectivity in the cohorts of children measured at different points in time is contaminating the results. If there are selective migration patterns, the sample of young children in one community in a given year may not be representative of the sample of young children in the same community at some point in the past. Furthermore, Oportunidades could be driving this selectivity if it changes the composition of households that decide to stay in a certain community, or alters the timing in the decision to leave. In order to address these concerns, a careful analysis of the characteristics of the households of the different cohorts will be performed. In particular, variables such as parent height or weight could work as placebo tests.

Finally, it could be that Oportunidades did not improve children's nutritional status throughout the country. Given that localities introduced to the program later in time are relatively better-off, the program may have had no impact on them. Although possible, such a conclusion can only be made after addressing the concerns mentioned above.

7. DISCUSSION

Oportunidades is an ambitious antipoverty program that has been operating in Mexico since 1997. There is extensive literature that evaluates its impact on beneficiary households in a wide range of dimensions. With respect to the impact on nutritional indicators, however, the literature is not conclusive. The evidence consists of a few impact evaluations that find mixed results and are subject to a series of limitations.

The analysis presented here contributes to the current literature by evaluating the impact of Oportunidades on height-for-age using different data that allows evaluating the impact of the program at the national level, and implementing an identification strategy not yet exploited in the context of this program.

Preliminary results suggest that the program had a positive impact on young children that live in the rural communities incorporated during the first years of the program. This is consistent with the purpose of the program that seeks to improve the nutritional status of beneficiary children. However, impact estimates that correspond to rural and urban localities incorporated later seem to suggest that Oportunidades did not improve children's nutritional status in those places.

It could be that Oportunidades did not have any impact on the nutritional status of children other than those in the poorest rural localities of Mexico. Pre-intervention nutritional status of children residing in communities incorporated later in time is expected to be better than nutritional status of children

[39] That would be the case if, for instance, concepts such as consumption or income determined eligibility.

residing in the localities first incorporated into Oportunidades. Therefore, it may be the case that the program did not generate a significant impact on the nutritional status of children belonging to the former group, but did have a significant impact on children belonging to the latter. However, there are still potential concerns that should be addressed before reaching such conclusion. In particular, more work will be done in three dimensions: identify a group of children that is closer to the group the program aims to reach to evaluate the impact on them; evaluate the existence of differential time trends that may be confounding the results; and assess the possibility of sample selection bias.

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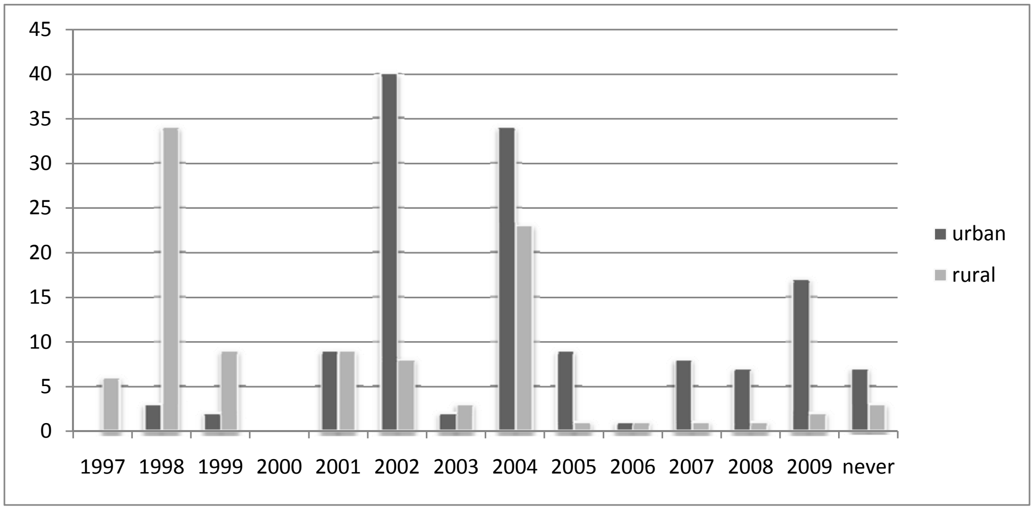
TABLES AND FIGURES

Figure 1: Expansion of Oportunidades over time at the locality level, in the rural and urban sectors



	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	never
RURAL	6	34	9	0	9	8	3	23	1	1	1	1	2	3
URBAN	0	3	2	0	9	40	2	34	9	1	8	7	17	7

Source: MxFLS and online list of Oportunidades beneficiaries as of the last two months of 2009

Figure 2: Graphical exposition of the identification strategy. Definition of cohorts and groups of localities

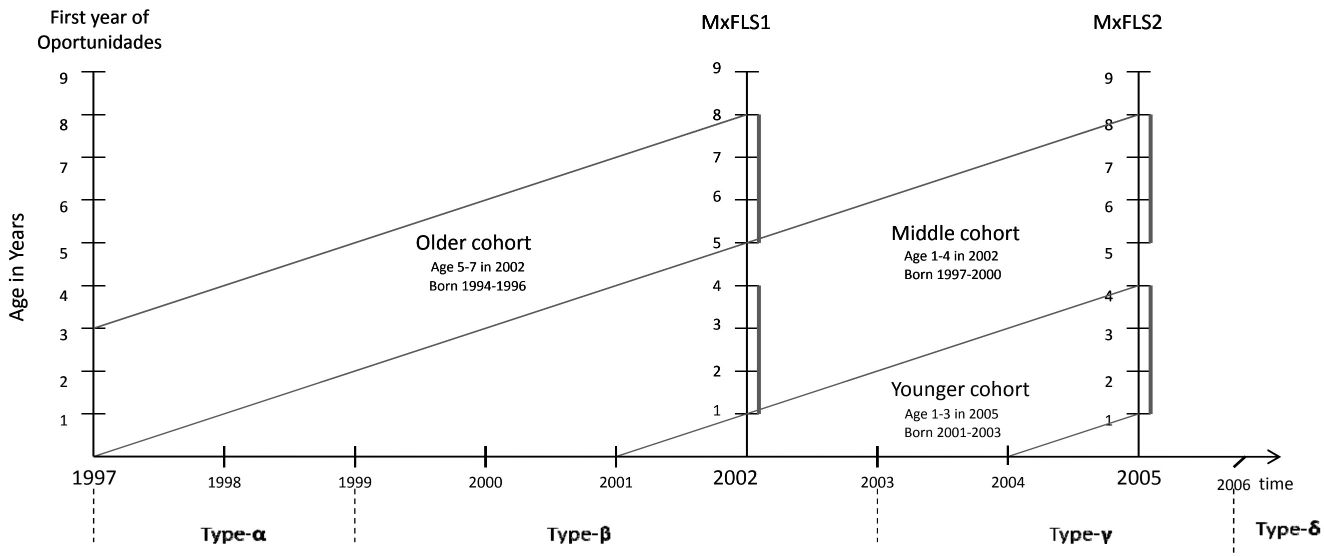


Table 1: Final sample used in the analysis, by rural-urban sector.
Children 1 to 3 and 5 to 7 years old

	2002		2005		TOTAL	
	#	%	#	%	#	%
TOTAL # OBS	4542		3807			
TOTAL RURAL	1931		1569		3500	
Obs lost because of:						
missing height	307		172			
+ missing z-score	0		3			
+ moved			19			
FINAL RURAL SAMPLE	1624	84%	1375	88%	**2999**	86%
TOTAL URBAN	2611		2238		4849	
Obs lost because of:						
missing height	496		492			
+ missing z-score	0		5			
+ moved			22			
FINAL URBAN SAMPLE	2115	81%	1719	77%	**3834**	79%

If children moved between 2002 and 2005 height is set to missing in 2005 (the observations are treated as if they were individuals not found in 2005)

Source: MxFLS 2002 and 2005

Table 2: Expected impact of Oportunidades on children´s height as a function of their cohort, locality of residence and year of measurement, by rural-urban sector

Height measure in:	2002			2005		
Community type:	α ≤ 98	β 99 ≤ y ≤ 02	γ 03 ≤ y ≤ 05	α ≤ 98	β 99 ≤ y ≤ 02	γ 03 ≤ y ≤ 05
Panel A: Rural						
Old Cohort	Partial	Zero	Zero	.	.	.
Middle Cohort	Full	Partial	Zero	Full	Partial	Zero
Young Cohort	.	.	.	Full	Full	Partial
Panel B: Urban						
Old Cohort	.	Zero	Zero	.	.	.
Middle Cohort	.	Partial	Zero	.	Partial	Zero
Young Cohort	.	.	.	.	Full	Partial

Table 3: Estimated impact of Oportunidades on children's height by type of locality and rural-urban sector

Height measure in:	2002			2005		
Community type:	α	β	γ	α	β	γ
	≤ 98	99 ≤ y ≤ 02	03 ≤ y ≤ 05	≤ 98	99 ≤ y ≤ 02	03 ≤ y ≤ 05
Panel A: Expected impact						
Old Cohort	Partial	Zero	Zero	.	.	.
Middle Cohort	Full	Partial	Zero	Full	Partial	Zero
Young Cohort	.	.	.	Full	Full	Partial
Panel B: Rural Communities						
Old Cohort	-0.663	-0.484	-0.464	.	.	.
	[0.126]**	[0.168]**	[0.150]**			
Middle Cohort	-0.39	-0.554	-0.395	-0.164	-0.142	-0.035
	[0.145]**	[0.180]**	[0.146]**	[0.122]	[0.161]	[0.176]
Young Cohort	.	.	.	-0.516	-0.339	-0.339
				[0.250]*	[0.320]	[0.258]
Full relative to partial exposure, time effects included				0.499	0.215	
				0.01	0.59	
Time effect				-0.126		0.429
				0.69		0.06
Partial exposure and time effect					0.342	0.056
					0.1	0.86
Full relative to partial exposure				0.625		
				0.09		
Partial exposure						-0.373
						0.36
Panel C: Urban Communities						
Old Cohort	.	-0.116	-0.052	.	.	.
		[0.112]	[0.139]			
Middle Cohort	.	0.258	0.37	.	0.024	-0.053
		[0.138]	[0.176]*		[0.133]	[0.179]
Young Cohort	.	.	.	.	-0.055	0.157
					[0.167]	[0.173]
Full relative to partial exposure, time effects included					-0.313	
					0.16	
Time effect						-0.001
						1
Partial exposure and time effect					0.14	-0.213
					0.38	0.35
Partial exposure						-0.212
						0.43

Robust standard errors in squared brackets; p-values below estimates of program effects. Reference category: communities that did not have Oportunidades by 2005. Regressions control for: gender of child, age in months, presence of mother and father in the household, mother's and father's height, mother's education, state of residence.

Source: MxFLS 2002 and 2005