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 in 1997, and covered, by 2005, 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.

Almost all the existing evidence on this program's impact 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[2]. In contrast to previous studies, this is the first time that population-level data is used to assess the impact of Oportunidades on young children's nutritional status. By using this new source of information, and by combining the longitudinal nature of the survey together with insights from the biology of child growth, this project contributes to the existing literature in two important ways.

In the first place, the use of population-level data allows us to perform, for the first time, an impact analysis at the national level. The Oportunidades evaluation data only represents a very selective group of Mexican communities. The data includes a subsample of the rural and urban communities first introduced to the program in 1998 and 2002 respectively, which were specifically chosen among the poorest communities in Mexico or among those with the highest concentration of poor households. However, Oportunidades has dramatically expanded over the years, and is now widespread throughout the country. Given the great degree of heterogeneity across communities, current evidence doesn't necessarily apply to places incorporated later in time, and new evidence is imperative to assess the overall performance of the program.

Secondly, the use of longitudinal data, combined with the timing in the geographic expansion of the program and evidence drawn from the nutrition literature, allow us implementing a strong identification

[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.

strategy in the absence of experimental data. This strategy constitutes a big improvement over traditional methods found in the Oportunidades literature that use non-experimental data. Furthermore, the current evidence presents mixed results even among studies that exploit the experimental design of the Oportunidades rural evaluation sample during the first year of the program. This is due to the fact that only recently issues such as deviations from perfect randomization, selective access to program components, or selective attrition have been considered, altering in many cases the conclusions of the analyses. Therefore, this study offers a great opportunity to complement existing evidence.

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 trained personnel from the National Institute of Public Health (INSP).

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 have only modest effects on 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 some descriptive statistics. Section7 presents the results, Section 8 show robustness checks, and Section 9 concludes.

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

2.1 Description of the program

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[3].

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 evaluates whether transfers made to women have a higher impact on children than transfers made to men, but the evidence is 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.

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

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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.

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 eligible areas. However, due to the high degree of heterogeneity in socioeconomic status within urban localities, there are two important differences relative to the rural component of the program. With respect to the geographic targeting of the intervention, the geographic unit of interest was the Primary Sampling Unit (AGEB in Mexico) rather than the locality as a whole[7]. Only localities with at least one PSU with high concentration of poor households were selected, and the program was implemented in those places. Additionally, a different household selection process was implemented[8]. Instead of collecting information on every household, the program established registration offices in eligible areas and advertised the program through campaigns. Households 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[9]. 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[10].

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[11].

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[7] In urban areas, an AGEB consists of a group of 1 to 50 blocks.

[8] 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).

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

[10] 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.

[11] 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[12].

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-life nutritional investments[13]. Given the big differences between the rural and urban components 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 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

[12] Better access to preventive and curative health care services may be a fourth channel through which the program improves nutritional status.

[13] Other health outcomes evaluated in the literature include: obesity, anemia, weight-for-height, BMI-for-age, birthweight, probability of illness.

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 at baseline that live in the poorest households who are on average 1 cm taller than children the same age with only 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 receiving 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 younger 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 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 for by including 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)[14]. 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.

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.

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

[14] 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).

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[15]. 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.

Similar to 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 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 in 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[16], 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

[15] 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.

[16] Children's mothers were also measured, and children 2 to 4 years old at baseline were measured in 2002 but not in 2004.

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

The main data source of this paper is the Mexican Family Life Survey (MxFLS). MxFLS 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 is representative at the national, rural-urban and regional level. 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 across 16 different states[17]. 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 now in the final stages of the field work. This analysis only uses the first two rounds of the survey, but we plan on incorporating the third round in future work.

The main variable of interest in this paper is height, as it constitutes a marker of early life nutritional investments. MxFLS records anthropometric measures (weight and height) for every household member, and these measures were taken by trained personnel from the National Institute of Public Health (INSP). In order to control for age-gender specific differences, height-for-age z-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 implemented in this work follows an intent-to-treat approach. By drawing on evidence from the nutritional literature, program exposure will only depend on the age of the child at the time Oportunidades arrived to the place of residence. In order to identify the year in which each MXFLS community was incorporated to the Oportunidades program this paper combines MxFLS data with Oportunidades administrative records[18].

The main source of information is the complete list of Oportunidades beneficiaries (Oportunidades' padron) as of December 2009. These records have individual information on locality of residence and date of enrollment in the program. 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. We will refer to this source of information as *Mode Year* data. An alternative source of

[17] Mexico is divided into 31 states and the Federal District.

[18] The MxFLS community questionnaire has information on whether the community has or not the program at the time of the interview, but no information on the year of incorporation.

information is a locality-level data with the number of families enrolled each year. We will refer to this source of information as *Expansion* data. Using both data sets, together with the self-reported participation rates recorded in MxFLS, each locality is assigned a year of incorporation[20]. Figure 1 illustrates the pattern of expansion of the program for the rural and urban areas of the country separately[21].

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

Robustness checks will make use of a second definition of exposure that follows the eligibility criteria that Oportunidades implements to select beneficiary households. This confidential information was made available through contact with Oportunidades administrators. Fortunately, the household-level score is not a function of variables that are likely to vary substantially across different data sources (such as income or consumption), and most of the variables are relatively stable over time. Both of these attributes make the score less sensitive to the data used to compute it as well as to the time at which is computed, which adds some validity to the implementation of the Oportunidades criteria to MxFLS data.

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 on height 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 it should follow that, if Oportunidades improved child nutrition, that effect should be reflected in height-for-age measures but only on young children. In order to allow for the possibility of some effects on 4-year-old children, we will define the cut-off age at 5. This is what constitutes the basis of the treatment definition used in the analysis: treated children are defined to be those that were younger than 5 at the

[20] 74% of the localities are assigned the same year regardless of the data source (86% in the rural sector and 62% in the urban sector).

[21] 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 urban localities that are assigned a year of incorporation before 2001 are consistent with this evidence, except for two localities, that have around 10,000 inhabitants.

time Oportunidades arrived to the locality of residence. Children 5 years or older constitute the control group[23].

The geographic expansion of the program was far from random, and it is very unlikely that controlling for observable locality characteristics will capture all of the locality-specific effects on height. Therefore, program placement will be controlled for by 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, which complicates the comparison of 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 that are taken when children are the same age.

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

Four cohort-time groups are used to identify treatment effects: the older and middle cohorts in 2002 and the 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 consists of children between 1 and 3 years old in 2005 (born between 2001 and 2003). The middle cohort includes children 1 to 4 years old in 2002 and between 4 and 7 in 2005 (born between 1997 and 2000). Finally, the older cohort includes children between 5 and 7 years old in 2002, born between 1994 and 1996[24].

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 geographic-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

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

[24] 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.

three groups of localities exist[25][26]. 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.

At the time of measurement, the three cohorts of children will have experienced a different level of exposure to the program depending on the locality group they live in. To identify impact effects we are going to define three levels of exposure: Full Exposure refers to children exposed to the program since birth; Partial Exposure refers to children exposed to the program for some time before the age of 5 but not since birth; and Zero Exposure refers to children that were too old at the time Oportunidades arrived (5 or older). For instance, Figure 3 shows the expected level of exposure for each cohort of children if we focus on type-α communities. At the time Oportunidades arrived, the older cohort was between 1 and 4 years old. Therefore, we define this group of children to be partially exposed when we measure them in 2002. In contrast, both the middle and younger cohorts were born either at the time the program arrived or after Oportunidades was already in the community. As a result, these two groups of children are fully exposed to the program. The same information is presented in a table form in Table 1. Each cell represents the expected level of exposure to the program for a given cohort in a given locality-group at a given time. The first row in each of the 2002 and 2005 panels reproduce the information for type-α communities. The same reasoning we followed with this group applies to fill the rest of the cells.

Based on 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[27]. The specification allows for 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 in 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

[25] 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.

[26] Results are robust to reclassifying the urban localities introduced to Oportunidades before 2001 as rural (See footnote 20).

[27] The specification for the urban sector does not include the α_{ct} terms.

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.

In the absence of selective program placement, the parameters estimated in equation 1 would be the estimated effects of full or partial exposure identified in Table 1. However, program placement was not random, and type-α, β and γ communities are expected to be different than baseline communities in ways that might affect children's height. Therefore, impact effects will be identified from differences in exposure within locality groups. Additionally, in order to control for the dynamics of the z-score all estimates will be identified from the comparison between the middle cohort measured in 2005 and the older cohort measured in 2002 (5 to 7 years old at time of measurement), or the comparison between the younger cohort measured in 2005 and middle cohort measured in 2002 (1 to 3 years old at time of measurement).

Following this approach allows us identifying the following parameters of interest:

***Type-α communities (received the program between 1997 and 1998)*:** looking at Table 1 (or Figure 3) we see that $\alpha_{m05} - \alpha_{o02}$ gives an estimate of full relative to partial exposure. Because both coefficients are measured relative to children in baseline communities, the difference between them controls for 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, because we are comparing different cohorts, the estimate also reflects 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 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. If we are willing to assume that the time effect is homogeneous across cohorts, we can use the difference between the younger cohort in 2005 and the middle cohort in 2002 to control for time effects. Both α_{y05} and α_{m02} 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 be attributed to 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. 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 1 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 obtained 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[30]. 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.

Strengths and limitations of the analysis

The combined use of the biology of child growth and the geographic expansion of the program to identify children exposed to the program and children not exposed to it constitutes a powerful identification strategy to estimate 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[31]. 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 school enrollment rates among non-beneficiary children and Angelucci and De Giorgi 2009 find that Oportunidades increased food consumption among non-beneficiary households.

However, unless spillover effects are significantly widespread, the cost to the analysis of using this definition of treatment is that it may include a large proportion of children that did not benefit from the program. 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's target

[30] 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.

[31] 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.

is only to reach households deemed poor. Better-off households are not eligible and therefore their children are not expected to benefit from this intervention[32].

To the extent that non-treated children cannot be made worse-off by the program, the estimated impact 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 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 implemented before, which resulted in much lower take-up rates among eligible households after 2001. As a result, estimates obtained from the analysis as described so far will depend on the strength of the correlation between children exposed to the program and those actually affected by it. To address this concern, we show later robustness checks in which we restrict the sample, based on different criteria, to a group of households that most likely consists of program beneficiaries. Results barely change.

Also important to the identification of treatment effects are the assumptions regarding time trends or shocks. If trends are non-linear or if they are location-specific, the diff-in-diff approach does not eliminate these effects. Additionally, 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. We expand on this issue in the robustness checks section, where we show that results are not mainly driven by these factors by analyzing locality groups separately and by using information on past shocks, both at the locality and household level.

Finally, the fact that the analysis compares children from different cohorts at different times makes the identification strategy subject to potential sample composition effects. If there is, for instance, selective migration, children in a given age group in 2005 may be significantly different from children in the same age group in 2002, in dimensions other than the exposure to Oportunidades. If the migration patterns are the same across localities the analysis will still be informative. However, if they interact with program participation, selection effects will contaminate the results[33]. We are currently exploring the importance of these effects.

[32] 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.

[33] 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.

6. DESCRIPTIVE ANALYSIS

Table 2 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 error or change of residence[34]. As a result, 86% of the rural sample and 79% of the urban sample are used in the analysis that follows[35].

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%.

Two other sets of comparisons between the rural and urban sectors are worth looking at in order to better interpret the results presented in the following section.

In the first place, participation rates in the rural and urban sector are very different. Two factors make participation rates much lower in the urban sector: the percentage of eligible households is smaller, and the participation rate among eligible households is smaller. This is important to keep in mind because the main definition of exposure used in the analysis only takes into account birth date and place of residence. To document how participation rates look like, we combine the Expansion data with census data and calculate, for 2000 and 2005, the ratio of number of families in Oportunidades over the total number of households in the community. Table 3 shows the distribution of these ratios broken by locality groups. As expected, participation rates are much larger in the rural sector. On average, rural communities have between 30% and 70% of the households enrolled in the program, while the urban communities have at most 17%.

Another difference between the rural and urban sectors has to do with the differences across the locality groups that we use in the analysis. Due to the geographic targeting of the program, we would expect localities incorporated later in time to be relatively better-off, especially in the rural sector. To see how the locality groups look like, we put together information from the 1990, 2000 and 2010 censuses, and the 1995 and 2005 population counts. The information from 1990 to 2000 provide us with a picture of what these localities look like at the time Oportunidades determines which localities to introduce and at what time. Additionally, we added the 2005 and 2010 data to see whether there is any evidence of

[34] 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.

[35] Appendix 1 analyzes in detail lack of measurement in children's height. Missing data is clearly not random, but fortunately, interactions of main parental characteristics with the relevant groups show that there doesn't seem to be differential effects across the cohorts or locality groups used in the analysis. Nonetheless, concerns regarding selectivity of measurement cannot be ruled out completely.

differential growth rates across locality groups, which would make the results presented in the next section more difficult to interpret.

We analyzed a broad set of socioeconomic variables, including: dwelling characteristics (floor and roof materials, access to basic services such as running water and electricity), asset ownership, literacy rates, school attendance, and access to health insurance. As expected, for every locality group there are improvements over time, but the changes do not seem to change significantly across locality groups. However, there are big differences in the levels, although only in the rural sector. Both the mean and the median of each characteristic improve as we move from type-α communities to type-δ communities. According to every indicator, the localities incorporated earlier are worse-off relative to communities incorporated later. Contrary to the rural sector, we don't see such differences across locality groups in the urban sector. Even though the levels move in the expected direction, the differences are negligible. On average, the three groups of urban communities look the same[37].

One variable that deserves a little bit of explanation is access to health insurance. In 2003, the Mexican government introduced a program called Seguro Popular that provides with health insurance to households that do not have access to this service. The program expanded gradually over time, participation is voluntary, the only selection criteria is lack of health insurance, and the service is provided for free or at a very low cost. Fortunately, participation rates in our localities are not very high. In 2005 in the rural sector, they are between 5 and 20%. In the urban sector, they are between 2 and 7%. By 2010, participation rates are much higher. The fact that this program only offers an insurance product, when Oportunidades offers a wide set of benefits (including a sizable cash transfer) mitigates our concerns. However, results should be interpreted with the existence of this program in mind.

Finally, we quantify our measures of partial exposure and full exposure in terms of years to have a better idea of the degree of exposure we are dealing with when interpreting the estimates. As explained in the previous section, the estimated impact effects in this paper are identified from differences between groups of children that are fully exposed, partially exposed or not exposed to the program. In that section full exposure is defined as exposed to the intervention since birth and partial exposure is defined as some exposure during the critical growth years from some point after birth. To have a better picture of what that means, we construct two measures of potential exposure. The first measure computes, for each child, the number of months there are between the time Oportunidades arrives to the locality (or birth if born after Oportunidades arrives) and the time of measurement or the time the child turns 4 years old, whichever comes first. Because one year of exposure between the ages of 1 and 2 could be very different than one year of exposure between the age of 3 and 4, in the second measure we calculate the percentage of life each child was exposed (again measured in months)[38].

Table 4 shows the distributions of these two measures for each cohort-time group in each locality group, by rural/urban sector[39]. For instance, we see that when we estimate *"Full relative to Partial*

[37] Results are available upon request.

[38] These measures are only approximations since we don't have the exact month Oportunidades arrived to each place. If Mode Year data and Expansion data coincide in the year, we use the month of incorporation recorded in the Expansion data. Otherwise, we use 6.

[39] Differences in exposure between rural and urban children result from differences in the distribution of year of entry to Oportunidades within each locality group.

exposure" for type-α communities in the rural sector, we are comparing children exposed to the program for 4 years to children exposed on average for a year and a half. However, the same estimate for type-β communities actually estimates the effect of one additional year of exposure, as it compares children exposed on average for a little over 2 years versus children exposed for a little bit more than a year. Something similar happens when we talk about *"Partial exposure"* in the rural sector. In type-β communities it represents almost 2 additional years of exposure as opposed to a little over a year in type-γ communities. In the urban sector, *"Partial exposure"* represents about 15 months of exposure for both locality groups.

7. RESULTS

Table 5 shows estimated coefficients on a selective group of variables of equation 1[40]. It is divided in three panels. The upper panel is a copy of Table 1, and presents 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. Each cell presents the estimated coefficient corresponding to the α_{ct}, β_{ct}, or γ_{ct} terms in equation 1. In Panel B, for example, the cell corresponding to the older cohort in type-α communities in 2002 (-0.597) shows the estimated value of α_{o02}, when equation 1 is restricted to the rural sample.

As mentioned before, these coefficients cannot be directly interpreted as program effects, but their sign and significance provide some evidence on the existence, or lack of, selection in program placement. Looking at the rural sector, results are consistent with the fact that Oportunidades was first implemented in the poorest communities of Mexico, and reinforces the descriptive analysis done with census data on a broader set of socioeconomic variables. 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} and γ_{o02} are negative and significant at the 5% and 10% level, when they should be zero in the absence of selection in program placement. The analysis cannot provide direct evidence of placement selection in type-α communities but the literature provides substantial evidence that they are among the poorest communities in Mexico. As a result, we see that Oportunidades was introduced first to communities where children's nutritional status was worse off, and a simple comparison between children exposed and not exposed across communities will not provide unbiased estimates of program effects.

Contrary to the rural sector, in the urban sector there is no evidence of selection in program placement at the locality level. The estimated coefficients of the older cohort at time zero are both close to zero and statistically insignificant, as it is the coefficient corresponding to the middle cohort at time zero. Once again, this is consistent with the geographic targeting policy described previously, as well as with the descriptive evidence performed with census data. Since the objective in the urban sector was to target, within localities, areas that have high concentration of poor households, and the degree of heterogeneity

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

within localities is much bigger than in rural places, it is not surprising that, on average, localities look the same[41].

Following the identification strategy presented in the previous section, Table 6 presents the estimated impact effects of the intervention on child nutrition. After discussing the results of our preferred specification, we discuss a set of robustness checks in the following section.

The first column in Table 6 presents the estimated results for the rural sector. Looking at the poorest group of rural communities, the evidence suggests that Oportunidades did have a positive effect on children's height. The first row shows the difference in height between the middle cohort measured in 2005 and the older cohort measured in 2002, which would reflect the impact of being fully exposed to the program relative to being partially exposed to it, in the absence of time effects. If we look at the sample statistics discussed previously in Table 4, we see that the middle cohort was, by 2005, not only exposed to the intervention since birth, but it was also, on average, exposed for over two years more than the older cohort. Given the size of the difference in the degree of exposure, and the fact that these are very poor communities, it is not surprising that the estimated effect is positive and highly significant. When we use the difference between the young and middle cohorts to account for time trends, we see that the net estimate of "Full relative to Partial" exposure remains positive and significant. According to these estimates, children fully exposed to the program are on average 0.88 standard deviations taller than children only partially exposed to it. This magnitude represents, for example, 4 cm for a four-year-old boy.

As explained in the previous section, an estimate of program exposure that is not confounded with time effects cannot be identified for type-β communities. Taking this into account, we don't see evidence of a positive impact effect on these children. Finally, we don't see any positive effect on children living in the group of rural communities incorporated at last. The estimated effect of partial exposure, with or without accounting for time trends, is negative and statistically insignificant.

When we move to the urban sector, we see that there is a positive effect (confounded with time effects) on children partially exposed to the program relative to children never exposed, in type-β communities. The two other estimated effects, however, are negative and statistically significant. Because the program is not expected to affect children's nutritional status in a negative way, these estimates might be the consequence of differential time trends, shocks or other selection effects. Given that the Intent-to-treat approach that we implement includes a large set of ineligible households in the estimation, robustness checks for the urban sector are particularly important. As we will see in the following section, these estimates either remain negative and loose significance or turn positive across different specifications.

[41] Even though by using some socioeconomic indicators urban localities do not look, on average, better-off as they are incorporated later in time, it is still hard to believe that the expansion was random. Most probably localities introduced first have bigger geographically concentrated blocks of poor households, or they differ in other dimensions, such as the political process. Another possibility is that they were introduced following a transitory shock instead of due to structural differences. In any case, we still think it is reasonable to keep working with the locality classification introduced in the previous section which uses year of entry to identify locality groups.

8. ROBUSTNESS CHECKS

We now discuss the main identification issues raised earlier, and present alternative ways to test or analyze the importance of each of them to check the robustness of our main results.

8.1 Eligible households

One concern with the main estimation strategy used in the previous section has to do with the fact that every child in the critical age group is considered to be exposed to the program, when in reality only eligible households can participate in the program. As mentioned earlier, this can result in null impact effects if the ratio of exposed relatively to non-exposed children is very low. To address this concern is particularly important in urban places due to the high degree of heterogeneity in socioeconomic status across households and the very low share of "program-eligible" households. Even by 2009 participation rates in our urban sample are on average around 10 percent while participation rates in the rural sample are above 50 percent in the first two groups of communities and 30 percent in type-γ communities.

In order to see whether including every child in the estimation has important implications for the analysis we present next a series of results that restrict the sample in a couple of different ways. The first stratification of the sample considers the education level of the mother of the child. We see how impact estimates vary when we restrict the analysis to the sample of children whose mothers have at most high-school incomplete (less than 12 years of education), and to the sample of children whose mothers have at most primary education. An alternative stratification uses the household eligibility score mentioned previously. By applying the Oportunidades eligibility criteria to MxFLS households, we can restrict the analysis to those children that live in households that would be deemed eligible for the program. After computing the score, we implement two sample restrictions. In the first one, we keep the sample of children that belong to households with a score equal or above the official cut-off. The second sample only keeps children that are above the 75th percentile of the distribution, to see whether by focusing on the poorest children we are able to identify some effects.

As expected, restricting the sample in any of these ways reduces substantially the urban sample size. 80% of the children have mothers with less than 12 years of education and this number goes down to 35% if we consider only those with mothers that have at most primary education. When looking at the household eligibility score, only 50% would be program-eligible, and by definition, only the poorest 25% of the sample remains if the cut-off is the 75th percentile of the score distribution. Columns 5 to 8 in Table 7 present the results for the urban sector. We see that regardless of the sample considered, the positive partial effect found earlier disappears, and none of the negative effects remain statistically significant.

Columns 2 and 3 in Table 7 present the results for the rural sector. In this case, we only show results that restrict the sample based on mother's education, because virtually every household is above the official Oportunidades cut-off[42]. We still see evidence of positive effects in the poorest communities, although estimates net of time trends loose significance. In the second group of communities, estimates of

[42] The formula Oportunidades uses to select eligible households has changed over time. It used to vary by region, but from 2002 on a unified formula is used. This might be one reason almost every household satisfies the criteria in our rural sample. When we restrict the sample to be above the median of the rural score distribution results are exactly the same as the ones using the entire sample. Sample size restrictions prevent us from analyzing the poorest 25%.

partial exposure are now bigger and significant. Results remain unchanged when we focus on the richest group of communities.

8.2. Time effects

A second issue raised earlier relates to the possibility that time trends might not be appropriately controlled for. The underlying assumption regarding time effects comprises two components: time trends constant over time and time trends similar across locality groups.

With respect to the first component, 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. To explore whether this could be the case we use three sources of information: Oportunidades administrative records and MxFLS household and community questionnaires on past shocks.

Regarding the first one, we were informed that some localities (or households) were incorporated into Oportunidades because they suffered a negative shock (in most cases, due to hurricanes). We were able to get a list of the 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.

Additionally, both the community and the household MxFLS questionnaires have a section that asks about specific shocks experienced by the community (household) during the 3 years (5 years) previous to the interview. At the locality level the shocks considered include: flood, earthquake, landslide, fire, hurricane, drought, plague, frost, and hailstorm. At the household level, they ask about the death or illness of household members, unemployment, and crop, production animals or property losses. A simple descriptive analysis that looks at the differences across years within locality groups does not provide clear evidence that any cohort of children is particularly worse-off or better-off. What we did notice using the household questionnaire is that, for some shocks, a few localities were placed far away in the right tail of the distribution. For instance, in a few localities the percentage of households that reported having lost their house, business or total crop production was as high as 30 to 40%. To confirm that our results are not driven by any of these particular places we run the regressions without these localities and all results remain unchanged. We also run the models using the entire sample but adding a dummy variable that equals one if the community questionnaire indicates that the locality suffered any shock in the previous three years, and that dummy interacted with each locality group. Once again, none of the results change either in the rural or urban sectors[43].

The second component has to do with the existence of location-specific time trends. If control communities experienced different growth rates relative to any of the treatment locality groups, impact estimates would be biased. The descriptive analysis performed with census data over the 20-year period between 1990 and 2010 did not suggest that the rate of improvement on various socio-demographic indicators was significantly different among locality groups. However, in order to see more formally whether results are sensitive to this assumption we implement an alternative estimation strategy that consists of applying the analysis to each group of localities separately. As a result, impact estimates for

[43] Results are available upon request.

each locality group are identified from the differences across the four cohort-time specific intercepts when the sample is restricted to children that live in the corresponding locality group[44]. Results following this specification are shown in columns 1 and 3 of Table 8, for the rural and urban sectors respectively.

Looking at the rural sector, we see that the estimate of full relative to partial exposure in the poorest communities continues to be positive, but it looses significance when estimated net of time effects. In type-beta communities the estimate of partial exposure continues to be positive and significant, and now the estimate of full relative to partial exposure is also positive and statistically significant. Finally, there is still no evidence of a positive impact on children living in the group of rural communities incorporated at last. For the urban sector, results remain the same. The program didn't have a significant impact on urban children in localities incorporated after 2002, but estimates of partial exposure (including time effects) in type-β communities are positive and significant.

Analyzing each locality group separately, allows us to impose an alternative sample selection criteria, which is to restrict the analysis to those households that report being in the program[45]. Columns 2 and 4 show that conclusions do not change when using this specification.

8.3 Sample composition effects

To conclude the set of robustness checks, it is worth evaluating the possible selectivity in the cohorts of children measured at different points in time. To explore this issue is particularly important to understand the negative estimated effects. If, for example, 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.

Work in progress…

9. CONCLUSION

Oportunidades is an ambitious antipoverty program that has been operating in Mexico since 1997. The existing literature has analyzed the impact of the intervention on child nutrition using the Oportunidades evaluation data. However, this data only represents a very selective group of Mexican communities (in general, the poorest), and the current evidence is not conclusive.

In contrast to previous evidence, this is the first time that population-representative data is used to answer this question. As a result, we are able to perform an impact analysis at the national level, and provide with new evidence on the impact of this intervention on localities incorporated later in time. In order to isolate impact effects using non-experimental data, we implement an identification strategy that combines insights from the biology of child growth, the timing in the roll-out of the program, and the panel dimension of MxFLS.

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[44] Due to sample size constraints, the only difference with equation 1 is that the covariates are no longer cohort-time specific.

[45] Recall that MxFLS asks about participation in Oportunidades at the household and individual levels. This analysis can only be implemented for the first two groups of localities, because there are no Oportunidades beneficiaries in the control group, and there are no beneficiaries in the third group of communities in 2002.

The results presented here suggest that the program did have a positive impact on young children that live in the poorest communities in rural Mexico. These results match existing evidence that exploit the experimental design of the program during the first couple of years of operation. More specifically, experimental evidence corresponds to what we defined as type-α communities, and children measured in the rural evaluation sample correspond to what we defined as the older cohort. As a result, our results extend previous evidence suggesting that the program continued to have a positive impact on younger cohorts.

However, our results also reveal an important degree of heterogeneity in impact both across the rural and urban sectors as well as across community groups incorporated at different points in time within the rural or urban sectors.

With respect to the remaining groups in the rural sector, estimated effects suggest that the program seem to have had a positive impact in our second group of communities, those incorporated between 1999 and 2002. Although these results do not account for time trends, our estimates of partial exposure are robust across several specifications and are in the same order of magnitude as those estimated for the first group of communities, and the estimates of full relative to partial exposure are positive in some specifications. In contrast, there is no evidence of a positive impact of the intervention on children that live in the locality group that was incorporated much later, between 2003 and 2005, more than 6 years after Oportunidades started. Across all specifications, impact estimates are statistically insignificant (and negative).

Finally, there is very limited evidence of an improvement in children's nutritional status in urban areas. Estimated effects are never significant for those localities incorporated after 2002, even when we restrict the sample to the poorest 25% of the households. The only case in which estimates are positive and significant is for the group of communities incorporated before 2003 if we use the whole sample.

To conclude, we find that the program significantly improved the nutritional status of beneficiary children, but this positive effect is confined to the poorest localities in rural Mexico. We are not aware of any study that evaluated this intervention at the national level, so we think that this constitutes a valuable contribution

Next steps…

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TABLES AND FIGURES

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

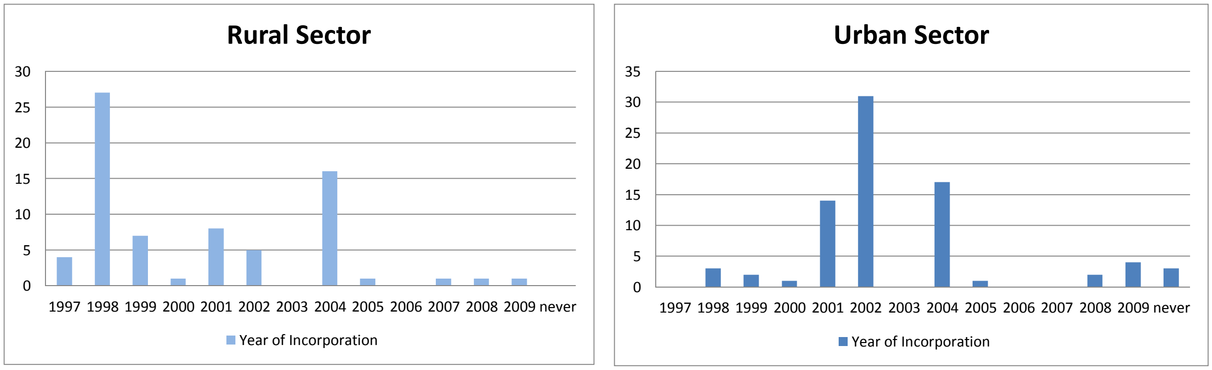

Figure 2: Graphical exposition of the identification strategy. Definition of cohorts and locality-groups

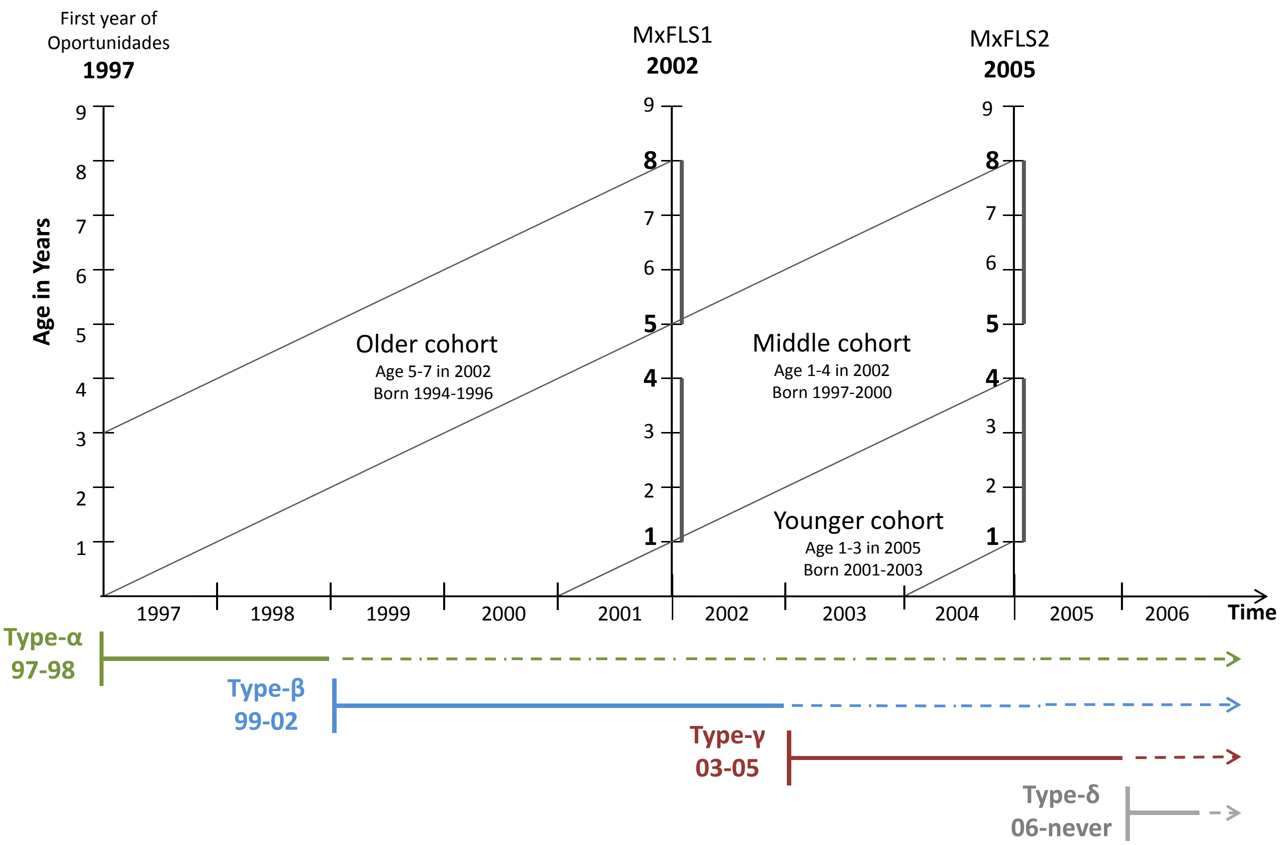

Figure 3: Expected impact of Oportunidades on children's height in type-α communities.

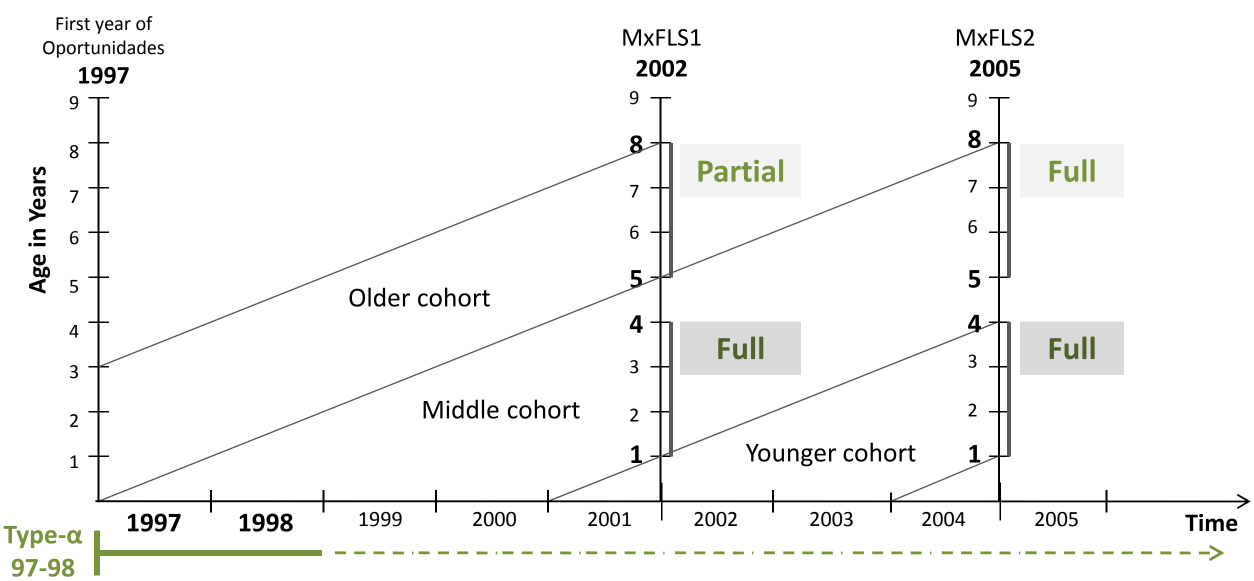

Table 1: Expected impact of Oportunidades on children´s height as a function of their cohort, locality of residence and year of measurement, by rural-urban sector

Height measured 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 (OC)	Partial	Zero	Zero	.	.	.
Middle Cohort (MC)	Full	Partial	Zero	Full	Partial	Zero
Young Cohort (YC)	.	.	.	Full	Full	Partial
Panel B: Urban						
Old Cohort (OC)	.	Zero	Zero	.	.	.
Middle Cohort (MC)	.	Partial	Zero	.	Partial	Zero
Young Cohort (YC)	.	.	.	.	Full	Partial

Table 2: 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 3: Oportunidades participation rates by locality groups

	RURAL SECTOR				URBAN SECTOR			
	p25	**p50**	**p75**	**mean**	**p25**	**p50**	**p75**	**mean**
ALPHA COMMUNITITES								
Incorporated between 1997 and 1998								
2000	45	57	70	58				
2005	45	58	78	64				
BETA COMMUNITITES								
Incorporated between 1999 and 2002								
2000	0	0	39	21	0	0	0	6
2005	40	50	73	53	4	7	17	15
GAMMA COMMUNITITES								
Incorporated between 2003 and 2005								
2000	0	0	0	0	0	0	0	0
2005	19	25	40	30	2	4	9	7

Source:

Table 4: Potential exposure to Oportunidades, by cohort and locality group.

A, Rural Sector

	Months of exposure				Percentage of life			
	p25	**p50**	**p75**	**mean**	**p25**	**p50**	**p75**	**mean**
ALPHA								
5-7 years old								
FULL - Middle Cohort 05	48	48	48	47.3	1.00	1.00	1.00	0.99
PARTIAL - Old Cohort 02	10	18.5	28	18.72	0.20	0.38	0.58	0.39
1-3 years old								
FULL - Young Cohort 05	20	28	38	28.96	1.00	1.00	1.00	1.00
FULL - Middle Cohort 2002	21	30	39.5	30.1	1.00	1.00	1.00	1.00
BETA								
5-7 years old								
PARTIAL - Middle Cohort 05	13	27	41	26.14	0.27	0.56	0.85	0.54
ZERO - Old Cohort 02	0	0	3	2.95	0.00	0.00	0.06	0.06
1-3 years old								
FULL - Young Cohort 05	18	28	38	28.3	1.00	1.00	1.00	0.99
PARTIAL - Middle Cohort 2002	8	12	23	15.13	0.24	0.46	0.95	0.53
GAMMA								
5-7 years old								
ZERO - Middle Cohort 05	0	0	0	0.6	0.00	0.00	0.00	0.01
ZERO - Old Cohort 02	0	0	0	0	0.00	0.00	0.00	0.00
1-3 years old								
PARTIAL - Young Cohort 05	13	15	18	15.23	0.38	0.56	0.92	0.60
ZERO - Middle Cohort 2002	0	0	0	0	0.00	0.00	0.00	0.00

B, Urban Sector

	Months of exposure				Percentage of life			
	p25	**p50**	**p75**	**mean**	**p25**	**p50**	**p75**	**mean**
ALPHA								
5-7 years old								
FULL - Middle Cohort 05								
PARTIAL - Old Cohort 02								
1-3 years old								
FULL - Young Cohort 05								
FULL - Middle Cohort 2002								
BETA								
5-7 years old								
PARTIAL - Middle Cohort 05	2	14	24	15.82	0.04	0.29	0.50	0.33
ZERO - Old Cohort 02	0	0	0	1.15	0.00	0.00	0.00	0.02
1-3 years old								
FULL - Young Cohort 05	20	28	35	27.81	1.00	1.00	1.00	0.97
PARTIAL - Middle Cohort 2002	0	0	8	4.56	0.00	0.00	0.23	0.17
GAMMA								
5-7 years old								
ZERO - Middle Cohort 05	0	0	0	0.87	0.00	0.00	0.00	0.02
ZERO - Old Cohort 02	0	0	0	0	0.00	0.00	0.00	0.00
1-3 years old								
PARTIAL - Young Cohort 05	12	13	20	15.48	0.38	0.57	0.85	0.61
ZERO - Middle Cohort 2002	0	0	0	0	0.00	0.00	0.00	0.00

Table 5: Estimated impact of Oportunidades on children's height by type of locality and rural-urban sector

Height measured 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: Expected impact						
Old Cohort (OC)	Partial	Zero	Zero	.	.	.
Middle Cohort (MC)	Full	Partial	Zero	Full	Partial	Zero
Young Cohort (YC)	.	.	.	Full	Full	Partial
Panel B: Rural Communities						
Old Cohort (OC)	-0.52	-0.46	-0.28	.	.	.
	[0.092]**	[0.134]**	[0.128]*			
Middle Cohort (MC)	-0.31	-0.41	-0.27	-0.19	-0.24	-0.04
	[0.129]*	[0.162]*	[0.165]	[0.135]	[0.159]	[0.175]
Young Cohort (YC)	.	.	.	-0.860	-0.470	-0.690
				[0.216]**	[0.278]	[0.232]**
Panel C: Urban Communities						
Old Cohort (OC)	.	-0.08	0.03	.	.	.
		[0.145]	[0.146]			
Middle Cohort (MC)	.	0.35	0.29	.	0.31	0.21
		[0.119]**	[0.171]		[0.136]*	[0.175]
Young Cohort (YC)	.	.	.	.	-0.23	-0.11
					[0.229]	[0.264]

Robust standard errors in squared brackets. 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

Table 6: Estimated impact of Oportunidades on children's height by type of locality and rural-urban sector

	RURAL SECTOR	URBAN SECTOR
ALPHA COMMUNITIES		
FULL relative to PARTIAL exposure (MC05-OC02)	0.34	
(including time effecs)	[0.18]	
	(0.06)	
TIME effects (YC05-MC02)	-0.55	
	[0.32]	
	(0.09)	
FULL relative to PARTIAL exposure	**0.88**	
	[0.41]	
	(0.04)	
BETA COMMUNITIES		
FULL relative to PARTIAL exposure (YC05-MC02)	-0.06	-0.58
(including time effecs)	[0.37]	[0.24]
	(0.87)	(0.02)
PARTIAL exposure (M05-O02)	0.22	0.39
(including time effecs)	[0.19]	[0.19]
	(0.24)	(0.05)
GAMMA COMMUNITIES		
TIME effects (M05-O02)	0.24	0.18
(including time effecs)	[0.20]	[0.22]
	(0.24)	(0.40)
PARTIAL exposure (YC05-MC02)	-0.43	-0.40
(including time effecs)	[0.32]	[0.27]
	(0.19)	(0.15)
PARTIAL exposure	**-0.67**	**-0.58**
	[0.44]	[0.33]
	(0.13)	(0.08)

Source:

Standard errors in square brackets, p-values in parenthesis

Table 7: Estimated impact of Oportunidades on children's height by locality group. Robustness checks using subsamples of the population.

	RURAL SECTOR			URBAN SECTOR				
Sample of children	ALL SAMPLE	MOTHER'S EDU < 12	MOTHER'S EDU <=6	ALL SAMPLE	MOTHER'S EDU < 12	ELIGIBLE HOUSEHOLDS	MOTHER'S EDU <=6	POOREST HOUSEHOLDS (p75 of score dist)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ALPHA COMMUNITIES								
FULL relative to PARTIAL exposure (MC05-OC02)	0.34	0.41	0.49					
(including time effecs)	[0.18]	[0.19]	[0.26]					
	(0.06)	(0.03)	(0.07)					
TIME effects (YC05-MC02)	-0.55	-0.46	-0.05					
	[0.32]	[0.49]	[0.35]					
	(0.09)	(0.35)	(0.88)					
FULL relative to PARTIAL exposure	**0.88**	**0.88**	**0.54**					
	[0.41]	[0.59]	[0.52]					
	(0.04)	(0.14)	(0.30)					
BETA COMMUNITIES								
FULL relative to PARTIAL exposure (YC05-MC02)	-0.06	0.07	0.33	-0.58	-0.22	-0.15	0.09	0.30
(including time effecs)	[0.37]	[0.47]	[0.34]	[0.24]	[0.22]	[0.24]	[0.38]	[0.35]
	(0.87)	(0.89)	(0.35)	(0.02)	(0.32)	(0.55)	(0.81)	(0.39)
PARTIAL exposure (M05-O02)	0.22	0.47	0.52	0.39	0.06	-0.03	0.22	-0.06
(including time effecs)	[0.19]	[0.22]	[0.28]	[0.19]	[0.20]	[0.24]	[0.26]	[0.26]
	(0.24)	(0.04)	(0.07)	(0.05)	(0.78)	(0.89)	(0.40)	(0.81)
GAMMA COMMUNITIES	0			0				
TIME effects (M05-O02)	0.24	0.44	0.53	0.18	-0.01	0.15	0.15	0.12
(including time effecs)	[0.20]	[0.24]	[0.31]	[0.22]	#DIV/0!	[0.28]	[0.28]	[0.33]
	(0.24)	(0.07)	(0.09)	(0.40)	(0.96)	(0.58)	(0.58)	(0.72)
PARTIAL exposure (YC05-MC02)	-0.43	-0.27	0.21	-0.40	-0.18	-0.28	-0.20	0.34
(including time effecs)	[0.32]	[0.50]	[0.36]	[0.27]	[0.25]	[0.26]	[0.40]	[0.38]
	(0.19)	(0.59)	(0.57)	(0.15)	(0.47)	(0.29)	(0.62)	(0.37)
PARTIAL exposure	**-0.67**	**-0.71**	**-0.32**	**-0.58**	**-0.17**	**-0.43**	**-0.35**	**0.22**
	[0.44]	[0.64]	[0.58]	[0.33]	[0.30]	[0.37]	[0.49]	[0.50]
	(0.13)	(0.27)	(0.58)	(0.08)	(0.58)	(0.24)	(0.48)	(0.65)

Table 8: Estimated impact effect of Oportunidades in children's height. Estimations performed on each locality group separately

	RURAL SECTOR		URBAN SECTOR	
Sample of children	**All**	**In Oport**	**All**	**In Oport**
	(1)	**(2)**	**(3)**	**(4)**
ALPHA COMMUNITIES				
FULL rel to PARTIAL exp (MC05-OC02)	0.17	0.25		
(including time effecs)	[0.08]	[0.10]		
	(0.05)	(0.02)		
TIME effects (YC05-MC02)	-0.07	0.07		
	[0.16]	[0.20]		
	(0.64)	(0.75)		
FULL relative to PARTIAL exposure	**0.24**	**0.18**		
	[0.16]	[0.20]		
	(0.13)	(0.38)		
BETA COMMUNITIES				
FULL rel to PARTIAL exp (YC05-MC02)	0.37	0.31	-0.08	0.08
(including time effecs)	[0.16]	[0.15]	[0.10]	[0.36]
	(0.03)	(0.05)	(0.44)	(0.83)
PARTIAL exposure (M05-O02)	0.24	0.16	0.26	0.32
(including time effecs)	[0.12]	[0.14]	[0.07]	[0.11]
	(0.06)	(0.24)	(0.00)	(0.01)
GAMMA COMMUNITIES				
TIME effects (M05-O02)	0.19		0.22	
(including time effecs)	[0.18]		[0.12]	
	(0.30)		(0.07)	
PARTIAL exposure (YC05-MC02)	-0.06		-0.03	
(including time effecs)	[0.17]		[0.17]	
	(0.72)		(0.85)	
PARTIAL exposure	**-0.25**		**-0.26**	
	[0.30]		[0.24]	
	(0.41)		(0.29)	
Observations				