Oportunidades and its Impact on Child Nutrition

Gabriela Farfán[*]

María E. Genoni[*]

Luis Rubalcava[±]

Graciela Teruel[¥]

Duncan Thomas[*]

Preliminary version – October 2011

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.

1. INTRODUCTION

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

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

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

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

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

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

---

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

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

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

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

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). As a result, MxFLS constitutes one of the few surveys that have both well measured anthropometrics (as opposed to self reported measures) as well as a very detailed set of socioeconomic variables, including income and consumption.

The causal impact of the program on child nutrition is isolated by exploiting insights from the biology of child growth, in combination with the timing of the roll-out of Oportunidades and the panel dimension of MxFLS. The evidence suggests that nutritional interventions 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 the results, and Section 7 concludes.

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

2.1 Description of the program[3]

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

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

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

---

[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.

[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.

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 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, a different household selection process was implemented in this case due to the fact that the share of potential beneficiaries with respect to the total population was expected to be considerably lower in these places[7]. Instead of collecting information on every household, 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[8]. By the year 2000 both groups were already under treatment. As a result, a new control group was incorporated into the evaluation sample in 2003 in order to estimate medium term impact effects. This new sample of localities was selected based on matching locality-level characteristics. Finally, a follow up survey was implemented in 2007 to evaluate long term effects.[9]

---

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

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

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

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

2.2 Program's impact on nutritional status

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

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

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

The literature has evaluated the impact of the program on child nutrition by looking at the effects on height because height constitutes a marker of early-life nutritional investments[12]. 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

---

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

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

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

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

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

---

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

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 response to this, health personnel exercised some discretion in the distribution of supplements by especially targeting those children that presented the most severe malnutrition symptoms. As a result, access to this component of Oportunidades among beneficiaries was, not only not universal, but also selective[14]. This implies that short-term impact estimates and estimates of differential exposure between the original treatment and control groups estimate intent-to-treat effects and may explain the lack of significant impact in some cases. Behrman and Hoddinott 2005 provide some evidence of this.

3.2 Oportunidades in urban areas

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

---

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

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[15], which limits the possibility of evaluating the effect on different age groups or performing robustness checks - the last of which seem to be crucial given the limitations of the data just exposed.

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

4. DATA AND DESCRIPTIVE ANALYSIS

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

For every household member, MxFLS records anthropometric measures (weight and height) that were taken by trained personnel from the National Institute of Public Health (INSP). As a result, MxFLS constitutes one of the few surveys that have both well measured anthropometrics (as opposed to self-reported measures) as well as a very detailed set of socioeconomic variables, including income and

---

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

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

consumption. Height, being a marker of early-life nutritional investments, constitutes the main variable of interest in this paper. 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 follows an intent-to-treat approach and exposure is defined at the locality level. More specifically, the main analysis will only take into account children´s age and place of residence when defining program exposure[17]. Therefore, we need to identify the year in which each MXFLS community was incorporated to the Oportunidades program. In order to do this, this paper combines MxFLS data with Oportunidades administrative records. We have the complete list of Oportunidades beneficiaries (Oportunidades' padron) as of December 2009 with 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[19]. Figure 1 illustrates the pattern of expansion in rural and urban areas separately[20]

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

As will be clear in the following section, the analysis will be performed with children 1 to 3 and 5 to 7 years old in either 2002 or 2005. Table 1 characterizes the final sample. The original sample of children 1 to 3 and 5 to 7 years old in rural communities is 3,500 and that of urban communities is 4,849. Some cases are dropped from the analysis due to lack of measurement, and only a few additional cases are

---

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

[19] Program exposure, defined in detail in the following section, will assume no exposure to the program before the assigned arrival date to the community. As a result, this measure is subject to measurement error because some households may have been exposed to the program before the assigned date of entry. However, the alternative measure, which would be the minimum year in which any household was enrolled in the program, constitutes a much noisier measure of exposure and contradicts the documented pattern of expansion of the program (a possible reason for this is migration). Additionally, when we analyze the localities for which the minimum year is different than the mode year (which happens mostly in urban localities), we see that the number of beneficiaries enrolled in the minimum year is very low (mostly less than 4 households), and the difference between this number and the number of beneficiaries enrolled in the mode year is very big (both in absolute and relative terms).

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

lost due to measurement error or change of residence[21]. As a result, 86% of the rural sample and 79% of the urban sample are used in the analysis that follows[22].

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

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

5. IDENTIFICATION STRATEGY

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

The biology of child growth suggests that the critical years during which nutrition interventions have the highest effect are from 0 to 4 years. When children turn 4 years old, the influence of nutrition interventions is substantially reduced (Martorell and Habicht 1986). Based on this evidence, and the fact that children's height-for-age constitutes an indicator of early life nutritional investments, two conclusions can be made. First of all, if Oportunidades affected children's nutritional status, that effect should be reflected in height-for-age measures. Additionally, Oportunidades shouldn't have a significant impact on height in children 5 years or older. This is what constitutes the basis of the treatment definition used in this analysis: treated children are defined to be those that were younger than 5 at the time Oportunidades arrived to the locality of residence. Children 5 years or older constitute the control group[23].

---

[21] 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.

[22] Appendix 1 analyzes in detail lack of measurement in children's height.

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

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 taken when they are the same age.

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

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

Taking into account both the pattern of expansion over the years and the fact that height is measured in 2002 and 2005, four different groups of rural localities and three groups of urban localities are identified. In the rural sector the groups are the following: the first group consists of localities that were incorporated at the very beginning of the program, in 1997 or 1998 (referred to as type-α communities); the second group consist of localities incorporated right after type-α localities, between the years 1999 and 2002 (type-β communities); the third group includes the localities that received the program between 2003 and 2005 (or type-γ localities); and finally the fourth group includes the localities that either received the program after 2005 or never did (type-δ or baseline group). In the urban sector only

---

[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.

the last 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.

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

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

where i represents the individual, c the cohort, v the community and t time[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 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.

Table 2 shows the expected effects of the program as a function of the time height was measured, the cohort of the child, and the locality where he/she lives[28]. Panel A shows the impact in rural communities and Panel B in urban communities. By 2002, children in the older cohort were partially exposed to the program if born in type-α communities (they were between 1 and 4 years old when the program arrived in 1997-1998), but they were too old to be exposed to the program if born in type-β or type-γ

---

[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.

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

communities[29]. By 2005, the younger cohort was fully exposed to the program if born in either type-α or type-β communities, but only partially exposed if born in type-γ communities. The rest of the cells are filled following the same reasoning.

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

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

Type-α communities (received the program between 1997 and 1998): $\alpha_{m05} - \alpha_{o02}$ gives an estimate of the program effect under full exposure relative to partial exposure. Because both coefficients are measured relative to children in baseline communities, the difference between them controls for any locality-specific effect that is common to both cohorts, eliminating the bias due to program placement. Furthermore, we are comparing children 5 to 7 years old in both cases, so that the dynamics of the z-score does not affect the results. However, this estimate also includes any time effect that might have existed between the time period the older cohort was in its critical years and the time period the middle cohort was in its critical years. If these communities experienced growth between those two periods, the middle cohort may have been exposed to a better environment than the older cohort when they both were between 1 and 5 years old. Under those circumstances, the difference between α_{m05} and α_{o02} would include both the additional exposure to Oportunidades and the improvement over time that would have happened regardless of the program. However, Table 2 suggests a way to overcome this issue. By assuming that the time effect is homogeneous across cohorts, it can be controlled for using the difference in the estimated coefficients that correspond to the younger cohort in 2005 and the middle cohort in 2002, that is: $\alpha_{y05} - \alpha_{m02}$. Both parameters estimate the effect of full exposure to the program in type-α communities at a time both groups were the same age. As a result, that difference can only be a consequence of time effects. Therefore, the double difference $(\alpha_{m05} - \alpha_{o02}) - (\alpha_{y05} - \alpha_{m02})$ should give an unbiased estimate of the program effect under full exposure relative to partial exposure. Note that the identification assumption regarding time effects is very specific. First of all, we need that the difference in the environment between the time period the

---

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

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[30]. Secondly, we need that this homogeneous time effects across cohorts is also the same across communities.

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

Type-γ communities (received the program between 2003 and 2005): For this group of communities an unbiased estimate of partial program exposure can be produced under the same assumptions mentioned for type-α communities. $\gamma_{y05} - \gamma_{m02}$ estimates the combined effect of partial exposure and time, while $\gamma_{m05} - \gamma_{o02}$ provides an estimate of time effects[31]. 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 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[32]. Previous literature summarized above provides evidence of the importance to control for selective access and selective participation.

---

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

[31] 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.

[32] 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

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

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

In the analysis described so far, exposure is only determined by the age of the child, and no other socio-economic characteristic of the household is taken into account. However, the program is not designed to benefit every household in the locality but to reach households deemed poor. Better-off households are not eligible and therefore their children are not expected to benefit from the program. As a result, both treated and non-treated children are included in the treatment group[33].

To the extent that non-treated children cannot be made worse-off by the program, the estimated impact in this paper 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 applied before, which resulted in much lower take-up rates among eligible households after 2001.

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

An additional limitation is the sensitivity of the results to the existence of time trends or chocks. If trends are non-linear or they are location-specific, the diff-in-diff approach does not eliminate these

---

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.

[33] 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.

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.

Finally, the fact that the analysis compares children from different cohorts at different times makes the identification strategy subject to potential sample composition effects. For instance, if there is selective migration, it could be that children in a given age group in 2005 are significantly different than 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.[34].

In order to address these caveats, the following section presents baseline results and discusses a set of robustness checks or complementary analyses that will help mitigate some of these concerns.

6. RESULTS

Table 3 shows the preliminary results[35]. The upper panel is a copy of Table 2, showing the expected impact of the program as a function of the cohort of the child, the year height was measured and the type of locality in which he/she resides. Panel B below shows the estimated results for the rural sector, and Panel C at the bottom shows the results for the urban sector. Both Panel B and C consist of three blocks. The upper block shows the estimated coefficients corresponding to equation 1 (standard errors below each coefficient). In Panel B, for example, the cell corresponding to the older cohort in type-α communities in 2002 (-0.597) shows the estimated value of α_{o02}, when equation 1 is restricted to the rural sample. The middle block shows, for each type of locality, the estimates of the first differences suggested in the Identification Strategy section. For example, the line named "Full relative to partial exposure, time effects included" is the estimate of $\alpha_{m05} - \alpha_{o02}$ in the case of type-α communities, and of $\beta_{y05} - \beta_{m02}$ in the case of type-β communities. As can be inferred from Panel A, it is not possible to get an estimate of this effect for type-γ communities. Finally, the bottom block shows the double difference estimates, or the estimates that provide unbiased impact effects under the assumptions made in the previous section. P-values are reported below impact estimates in the middle and bottom blocks of Panels B and C.

---

[34] 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.

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

6.1 Estimated impact in the rural sector

The first thing to notice in Table 3 is the suggested evidence of selection in program placement. As expected, rural communities incorporated to Oportunidades before 2005 are statistically different from communities incorporated later (or never incorporated) in terms of average children's height. β_{o02}, γ_{o02} and γ_{M02} are negative and significant at the 5% or 10% level; which confirms that Oportunidades was introduced in communities where children's nutrition status was worse off. The analysis cannot provide direct evidence of placement selection in type-α communities, but the literature provides substantial evidence that they are among the poorest communities in Mexico. As a result, simple comparison between children exposed and not exposed across communities will not provide unbiased estimates of program effects.

Results also suggest that Oportunidades had a positive effect on children's height in the group of localities first introduced to the program. According to these estimates, children fully exposed to the program are on average 0.75 standard deviations taller than children that were only partially exposed, and this impact is statistically significant at the 5% level. The magnitude of this impact represents, for example, 3 cm for a four-year-old boy. Additionally, there seems to be no time effect on these communities, although the estimated coefficient is quite large.

As explained in the previous section, an estimate of program exposure for type-β communities that is not confounded with time effects cannot be identified. However, something can still be learnt about these communities. Assuming constant time effects, Table 3 suggests that partial exposure is much more relevant than full relative to partial exposure. The estimated impact of partial exposure (together with time effects) is positive and statistically significant at the 5% level. On the contrary, full relative to partial exposure is both close to zero in magnitude and statistically insignificant. If children in type-β communities are sufficiently better-off than children in type-α communities it does not seem unreasonable to find that every additional year produces a significant impact on children's nutrition in the latter group but not in the former. However, results are too preliminary to reach such conclusion.

Results for children in type-γ communities suggest that the program did not improve their nutritional status. The estimated effect of partial exposure is not significant.

6.2 Estimated impact in the urban sector

In urban communities there is no evidence of selective program placement. The estimated coefficients of the older cohort at time zero are both close to zero and statistically insignificant, as it is the coefficient corresponding to the middle cohort at time zero. This is not necessarily inconsistent with the program targeting rules. The geographic targeting in the urban sector did not use locality-level indicators to introduce the program, but only identified localities that had geographically concentrated blocks of poor households. Additionally, the heterogeneity among households is much bigger than that

of rural places, and the percentage of beneficiary households is much smaller. As a result, it can be that, on average, localities look the same[36].

In this case, type-β communities show counterintuitive results. The estimated effect of full relative to partial exposure is negative and significant. Because the program is not expected to affect children's nutritional status in a negative way, this estimate is only consistent with significantly negative time trends or other selection effects. The estimated effect of partial exposure (including time trends) is close to zero.

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

6.3 Interpretation of the results

Preliminary results seem to suggest that the program did have a positive impact on rural children that live in the poorest communities (those incorporated before 2002). However, there is no evidence of improvement in nutritional status among rural or urban children that live in communities incorporated at last (between 2003 and 2005). Results for urban localities incorporated between 1999 and 2002 are less clear. This reveals an important degree of heterogeneity in impact both across time and across rural and urban communities. Given that localities introduced to the program later in time are relatively better-off, it is reasonable to find that the program may have had no impact on them.

We now discuss the main identification issues raised earlier, and evaluate alternative ways to test or analyze the importance of each of them as well as the possible impact they may have on program estimates.

One concern has to do with the possibility that time trends might not be appropriately controlled for. As mentioned earlier, the underlying assumption regarding time effects comprises two components: time trends constant over time and time trends similar across localities.

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. In particular, there is some evidence that some localities (or households) were incorporated into Oportunidades because they suffered a negative shock (in most cases, due to hurricanes). If this was the case, children in their critical years were subject to two interventions that affect nutrition in opposite directions and there is no way to disentangle those two effects. We were able to get a list of localities in which some households were enrolled in the program under a special process due to these kinds of reasons. Fortunately, only 3 MxFLS localities were in this list and results are robust to the exclusion of these localities. However, more work will be done to try to evaluate the existence of confounding factors. In particular, the MxFLS community questionnaire can be used to see whether the localities were exposed to negative shocks over the last years. Indirect evidence can also be provided applying the estimation strategy to an outcome that was not affected by the program. Recent history of

---

[36] This could also be the result of measurement error in our definition of locality groups.

household income or wages can also be analyzed to see if there is a correlation between the dynamics of income indicators and the time at which the different cohorts were in their critical growth years.

The second component has to do with the existence of location-specific time trends. To see whether results are sensitive to this assumption we applied the analysis to each group of localities separately. Preliminary results (not shown) reinforce the conclusions drawn for the rural sector, although some concerns still remain in the urban sector. More specifically, the estimated program impact remains positive and highly significant for the poorest group of rural communities - type-α communities, and positive although not significant for both partial and full relative to partial exposure estimates for the second poorest group -type-β. The estimated impact effect for children in communities incorporated at last is still insignificantly different from zero, but the estimate is now positive. With respect to the urban sector, the group incorporated first still shows a negative and significant estimate of full-relative to partial exposure (including time effects), but now there seems to be a positive and significant effect of partial exposure (again including time effects). This suggests that there might be something particular to the young cohort driving these results. The one big difference between baseline results and group-specific results is on the last urban group. Previous results show no time trend and not program effect. When the sample is restricted to the group of households residing in type-γ communities, the time trend becomes positive and this makes the estimated effect of partial exposure negative and significant.

A second issue relates to the fact that every child in the critical age group is recorded as exposed to the program. As mentioned earlier, we might not be able to capture a positive impact if the ratio of exposed relatively to non-exposed children is very low. This is expected to be particularly important in urban places due to the high degree of heterogeneity in socioeconomic status across households and the lower share of "program-eligible" households. In order to analyze whether this explains the lack of impact of the program on children's height in certain places we are currently working with the eligibility criteria that Oportunidades uses to select beneficiary households. Through contact with program administrators we were able to get the exact formula the program uses to compute the eligibility score. Fortunately, the household-level score used is not a function of variables that are likely to vary substantially between different data sources, so that eligibility would be very sensitive to the way such variables are measured[37]. Additionally, most of the variables that determine eligibility are relatively stable over time, so that the score a household is assigned does not vary substantially as a function of the year the score is computed. The analysis of this eligibility criterion is still in its preliminary stages, so it will be included in future work.

Finally, 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. In order to address these concerns, a careful analysis of the characteristics of

---

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

the households of the different cohorts will be performed. In particular, we can apply the analysis to variables such as parent's height or weight as placebo tests.

7. 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, this is the first time that population-level 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.

Preliminary results suggest that the program had a positive impact on young children that live in the rural communities incorporated during the first years of the program. This results match previous evidence that exploit the experimental design of the program. More specifically, experimental evidence corresponds to what we defined as type-α communities, and children measured in the evaluation data 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. Impact estimates that correspond to rural and urban localities incorporated later –after 2002- seem to suggest that Oportunidades did not improve children's nutritional status in those places. This evidence is new, as no evaluation has focused on this group.

In order to address some of the concerns mentioned in the paper we are currently working on different dimensions that include, for example, evaluating the possibility of sample selection/composition effects by analyzing selectivity in parent's characteristics, and identifying the group of children that the program is expected to benefit by working with the Oportunidades eligibility criteria.

REFERENCES

Adato, M., D. Coady, and M. Ruel (2000). "Final Report: An Operations Evaluation of PROGRESA from the Perspective of Beneficiaries, Promotoras, School Directors, and Health Staff". Report submitted to PROGRESA, International Food Policy Research Institute, Washington DC.

Angelucci, Manuela, and Orazio Attanasio (2009). "Oportunidades: Program Effect on Consumption, Low Participation and Methodological Issues". *Economic Development and Cultural Change*, April 2009, pp.479-506.

Angelucci, Manuela, and Giacomo De Giorgi (2009). "Indirect Effects of an Aid Program: How Do Cash Transfers Affect Ineligibles' Consumption?". *American Economic Review*, Vol 99(1), p. 486-508.

Angelucci, Manuela, and Orazio Attanasio (2006). "Estimating ATT Effects with Non-Experimental Data and Low Compliance". IZA Discussion Paper No. 2368.

Behrman, Jere R., and John Hoddinott (2005). "Programme Evaluation with Unobserved Heterogeneity and Selective Implementation: The Mexican *PROGRESA* Impact on Child Nutrition". *Oxford Bulletin of Economics and Statistics*, Vol. 67(4): 547-569.

Behrman, Jere R., and Petra E. Todd (1999). "Randomness in the Experimental Samples of PROGRESA (Education, Health, and Nutrition Program)". International Food Policy Research Institute, March 1999.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan (2004). "How Much Should We Trust Differences-in-Differences Estimates?" *Quarterly Journal of Economics*, Vol. 119(1): 249-275.

Bobonis, Gustavo J., and Frederico Finan (2006). "Endogenous peer effects in school participation". University of Toronto, Ontario, Canada and UC-Berkeley, CA.

Coady, David, and Susan Parker (2009). "Targeting Performance under Self-selection and Administrative Targeting Methods". *Economic Development and Cultural Change*, April 2009, pp.559-587.

Donald, Stephen G., and Kevin Lang (2007). "Inference with Differences-in-Differences and Other Panel Data". *Review of Economics and Statistics*, Vol. 89(2): 221-33.

Duflo, Esther (2001). "Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence from an Unusual Policy Experiment". *The American Economic Review*, Vol. 91(4): 795-813.

Duflo, Esther (2000). "Child Health and Household Resources in South Africa: Evidence from the Old Age Pension Program". *American Economic Review Papers and Proceedings*, Vol. 90(2): 393-398.

Fernald, Gertler and Neufeld (2009). "10-year effect of Oportunidades, Mexico's conditional cash transfer programme, on child growth, cognition, language, and behaviour: a longitudinal follow-up study". LANCET, 2009.

Frankenberg, Elizabeth, Wayan Suriastini and Duncan Thomas (2005). "Can expanding access to basic healthcare improve children's health status? Lessons from Indonesia's 'midwife in the village' programme". *Population Studies*, Vol. 59(1): 5-19.

Gertler, Paul (2004). "Do Conditional Cash Transfers Improve Child Health? Evidence from PROGRESA's Control Randomized Experiment". *American Economic Review*, Vol 94(2).

Gutiérrez, Juan Pablo, Stefano Bertozzi, and Paul Gertler (2003). "Evaluación de la identificación de familias beneficiarias en el medio urbano". Evaluación de Resultados de Impacto del Programa de Desarrollo Humano Oportunidades, Instituto Nacional de Salud Pública.

Imbens, Guido W., and Jeffrey M. Wooldridge (2009). "Recent Developments in the Econometrics of Program Evaluation". *Journal of Economic Literature*, Vol. 47(1): 5-86.

Leroy, Jef, Armando García-Guerra, Raquel García, Clara Dominguez, Juan Rivera and Lynnette M. Neufeld (2008). "The Oportunidades Program Increases the Linear Growth of Children Enrolled at Young Ages in Urban Mexico". *The Journal of Nutrition*, Vol. 138(4): 793-798.

Lundberg, Shelly J., Robert A. Pollak, and T.J. Wales (1997). "Do Husbands and Wives Pool Their Resources? Evidence from the United Kingdom Child Benefit". *Journal of Human Resources*, Vol. 32(3): 463-480.

Martorell, Reynaldo, Jere R. Behrman, Rafael Flores, and Aryeh D. Stein (2005). "Rationale for a follow-up focusing on economic productivity". *Food and Nutrition Bulletin*, Vol. 26 (2 Suppl 1): 5-14.

Martorell, Reynaldo (1999). "The nature of child malnutrition and its long-term implications". *Food and Nutrition Bulletin*, Vol. 20: 288-292.

Martorell, Reynaldo, and Jean-Pierre Habicht (1986). "Growth in early childhood in developing countries". In Frank Falkner and J.M. Tanner (eds.), *Human Growth: A Comprehensive Treaise*, Vol. 3, New York: Plenum Press, pp. 241-262.

McKee, Douglas, and Petra E. Todd (2009). "The Longer-term Effects of Human Capital Enrichment Programs on Poverty and Inequality: *Oportunidades* in Mexico". *Estudios de Economia*, December.

Neufeld, Lynnete, Daniela Sotres Álvarez, Paul Gertler, Lizbeth Tolentino Mayo, Jorge Jiménez Ruiz, Lia Fernald, Salvador Villalpando, Teresa Shamah and Juan Rivera Dommarco (2004a). "Impact of Oportunidades on Child Growth and Nutritional Status in Rural Communities". External Evaluation of the Impact of the Human Development Program Oportunidades, Instituto Nacional de Salud Publica.

Neufeld, Lynnete, R. Sotres-Álvarez, A. García-Peregrino, L. García-Guerra, L.F. Tolentino-Mayo, J. Rivera-Dommarco (2004b). "Evaluación del estado nutricional y adquisición de lenguaje en niños de localidades urbanas con y sin el programa Oportunidades". Documento Técnico #7 en la Evaluación de Oportunidades 2004, Evaluación Externa de Impacto del Programa de Desarrollo Humano Oportunidades, Instituto Nacional de Salud Pública, México.

Parker, Susan, Luis Rubalcava, and Graciela Teruel (2008). "Evaluating Conditional Schooling and Health Programs". In Schultz T. and Strauss John (eds), *Handbook of Development Economics*, North-Holland, Amsterdam, Vol. 4, pp. 3963-4035.

Parker, Susan W., Petra E. Todd, and K.I. Wolpin (2005). "Within family treatment effect estimators: The impact of Oportunidades on schooling in Mexico". Mimeo, University of Pennsylvania.

Rivera, Juan, Daniela Sotres-Alvarez, Jean-Pierre Habicht, Teresa Shamah and Salvador Villapando (2004). "Impact of the Mexican Program for Education, Health, and Nutrition (Progresa) on Rates of Growth and Anemia in Infants and Young Children. A randomized effectiveness study". *American Medical Associasion*, JAMA, Vol. 291, No. 21

Rivera, Juan A., Guadalupe Rodríguez, Teresa Shamah, Jorge L. Rosado, Esther Casanueva, Irene Maulén, Georgina Toussaint, and Alberto García-Aranda (2000). "Implementation, monitoring and evaluation of the nutrition component of the Mexican social programme (PROGRESA)". *Food and Nutrition Bulletin*, Vol. 21: 35-42.

Rubalcava, Luis, Graciela Teruel, and Duncan Thomas (2009). "Investments, Time Preferences, and Public Transfers Paid to Women". *Economic Development and Cultural Change*, April 2009, pp.507-538.

Rubalca, Luis, and Graciela Teruel (2008). *User's Guide for the Mexican Family Life Survey: Second Wave*. http://www.ennvih-mxfls.org.

Rubalca, Luis, and Graciela Teruel (2006). *User's Guide for the Mexican Family Life Survey: First Wave*. http://www.ennvih-mxfls.org.

Schultz, T.P. (2004). "School subsidies for the poor: Evaluating a Mexican strategy for reducing poverty". *Journal of Development Economic*s, Vol. 74(1): 199-250.

Skoufias, Emmanuel (2005). "PROGRESA and Its Impacts on the Welfare of Rural Households in Mexico". IFPRI Research Report No 139.

Skoufias, Emmanuel, Benjamin Davis and Jere R. Behrman (1999a). "Final Report: An Evaluation of the Selection of Beneficiary Households in the Education, Health, and Nutrition Program (PROGRESA) of Mexico". International Food Policy Research Institute, June 1999.

Skoufias, Emmanuel, Benjamin Davis and Jere R. Behrman (1999b). "Targeting the Poor in Mexico: An Evaluation of the Selection of Households into Progresa". International Food Policy Research Institute, December 1999.

Strauss, John, and Duncan Thomas (1995). "Human resources: empirical modeling of households and family decisions". In Behrman J.R. and Srinivasan T.N. (eds), *Handbook of Development Economics*, North-Holland, Amsterdam, Vol. 3A: 1883-2024.

Teruel, Graciela, and Luis Rubalcava (2007). "Attrition in PROGRESA". Mimeo.

Thomas, Duncan (1990). "Intra-household Resource Allocation: An Inferential Approach". *Journal of Human Resources*, Vol. 25(4): 635-664.

TABLES AND FIGURES

Figure 1: Expansion of Oportunidades over time at the locality level, in the rural and urban sectors
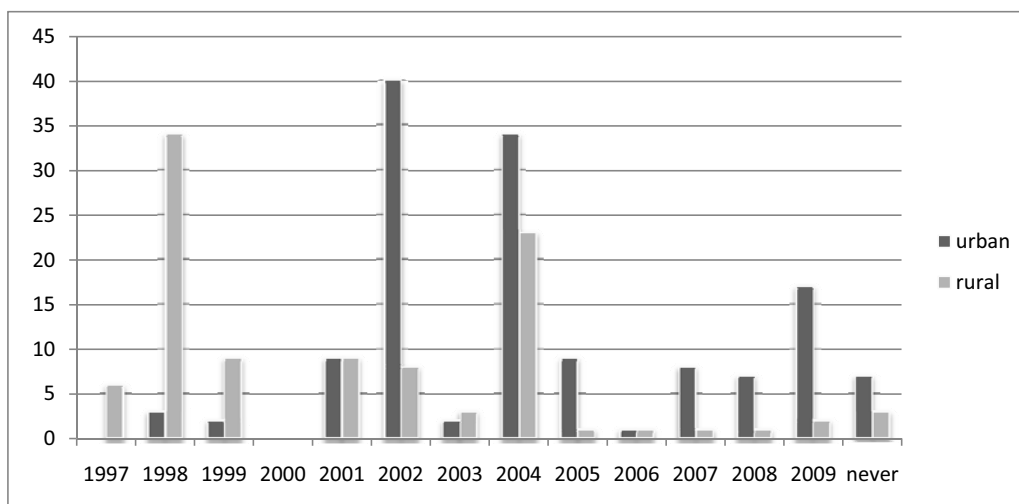
	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	never
RURAL	6	34	9	0	9	8	3	23	1	1	1	1	2	3
URBAN	0	3	2	0	9	40	2	34	9	1	8	7	17	7

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

Figure 2: Graphical exposition of the identification strategy. Definition of cohorts and groups of localities
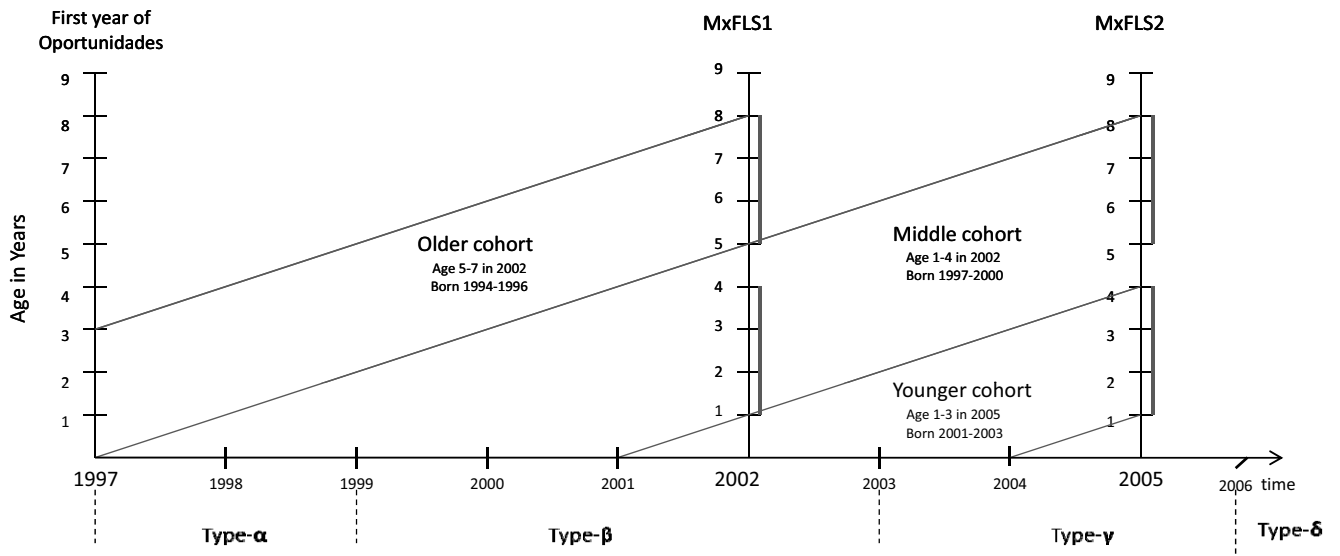
Table 1: Final sample used in the analysis, by rural-urban sector.
Children 1 to 3 and 5 to 7 years old in either 2002 or 2005

	2002		2005		TOTAL	
	#	%	#	%	#	%
TOTAL # OBS	4542		3807			
TOTAL RURAL	1931		1569		3500	
Obs lost due to:						
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 due to:						
missing height	496		492			
+ missing z-score	0		5			
+ moved			22			
FINAL URBAN SAMPLE	2115	81%	1719	77%	**3834**	79%

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

Source: MxFLS 2002 and 2005

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

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

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

Height measure in:	2002			2005		
Community type:	α	β	γ	α	β	γ
	≤ 98	99 ≤ y ≤ 02	03 ≤ y ≤ 05	≤ 98	99 ≤ y ≤ 02	03 ≤ y ≤ 05
Panel A: Expected impact						
Old Cohort	Partial	Zero	Zero	.	.	.
Middle Cohort	Full	Partial	Zero	Full	Partial	Zero
Young Cohort	.	.	.	Full	Full	Partial
Panel B: Rural Communities						
Old Cohort	-0.596	-0.613	-0.356	.	.	.
	[0.113]**	[0.146]**	[0.141]*			
Middle Cohort	-0.393	-0.568	-0.418	-0.170	-0.248	-0.105
	[0.152]*	[0.182]**	[0.176]*	[0.108]	[0.143]	[0.152]
Young Cohort	.	.	.	-0.718	-0.459	-0.533
				[0.237]**	[0.303]	[0.252]*
Full relative to partial exposure, time effects included				0.426	0.109	
				0.01	0.8	
Time effect				-0.325		0.251
Obs lost due to:				0.38		0.22
Partial exposure and time effect					0.365	-0.115
					0.05	0.76
Full relative to partial exposure				0.751		
				0.06		
Partial exposure						-0.366
Obs lost due to:						0.4
Panel C: Urban Communities						
Old Cohort	.	-0.049	0.004	.	.	.
		[0.089]	[0.113]			
Middle Cohort	.	0.156	0.265	.	0.043	-0.017
		[0.103]	[0.163]		[0.121]	[0.175]
Young Cohort	.	.	.	.	-0.23	-0.052
					[0.171]	[0.175]
Full relative to partial exposure, time effects included				-0.386		
				0.06		
Time effect						-0.021
						0.9
Partial exposure and time effect					0.092	-0.317
					0.5	0.13
Partial exposure						-0.296
						0.28

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

Source: MxFLS 2002 and 2005

Appendix 1: Analysis of Selectivity in the Measurement of Height

In this Appendix we analyze the prevalence and selectivity of missing data in children's height. Children's height is the main variable of interest in the paper, as it is used to measure the impact of Oportunidades on child nutrition. Therefore, it is important to evaluate whether children measured and not measured are significantly different in ways that can bias the estimated effects.

Table 1 in the paper shows that around 17% of children 1 to 3 and 5 to 7 years old were not measured in each wave. However, missing rates are not constant across ages. Figure A1 below shows the percentage of children with missing height by age and by year. The missing rate for children younger than 1 is very high (over 40% in both years), it decreases with age until children reach about 5 years old, it remains fairly constant around 11% for children 6 to 12 years old, and it starts going up again for children 12 years and older. The pattern is very similar across both years, although missing rates are somewhat larger in 2005 for children 2 to 5 years old. The figure shows that the average rate of 17% in the sample of children used in the analysis is a combination of a 22% on the younger group (1-3 years old) and a 13% on the older group.

Figure A1: Proportion of children with missing height, by age and year.
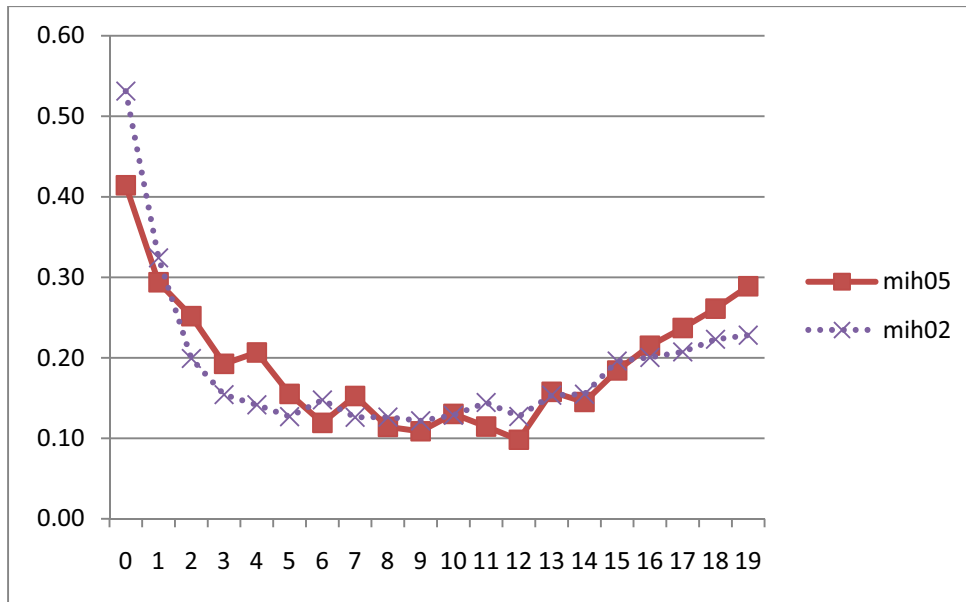
Because we are comparing children in a certain age group in one wave relative to children in the same age group in the second wave, it is somewhat encouraging that missing rates do not vary significantly by year. However, it is the difference in characteristics between children measured and not measured what can affect the results. Ideally, children with missing height are simply a random sample of all children, in which case missing data would not be a problem. However, that is very unlikely to be the case.

Fortunately, the fact that missing height is not random is not by itself enough to bias the results[1]. Given the identification strategy followed here, what we need is measurement not to be selective between cohorts and across localities, so that the differences in average height that we attribute to time or program exposure is actually due to selective measurement.

We present next two approaches that will help characterize the nature of missing height for the sample used in this paper. The first strategy uses the fact that MxFLS is a panel, so that we have children measured in both waves. The second approach is a standard regression analysis.

Descriptive analysis

The analysis presented here compares the distribution of height of children found in one wave but not in the other to have an idea of whether children with missing height are randomly selected from the height distribution. The objective is to provide suggested evidence but it is by no means conclusive.

Missing height in 2002

To see whether children measured in 2002 are significantly different from children not measured in 2002 in terms of height we can use the information collected in 2005. Assuming that missing height in 2005 is random (in the sense that children from every point of the 2005 height distribution are equally likely to be missing), we can compare the distribution of height in 2005 of those children measured in 2002 with the distribution of height in 2005 of those children not measured in 2002. Table A1 shows the results. We test whether different quartiles of the height distribution are different between children measured and not measured in 2002, and we also show the results of the Kolmogorov-Smirnov test[2]. As can be seen, we almost never reject that the distributions are the same, regardless of how we break the sample: all children in 2002, older or middle cohort in 2002, or children in each group of localities.

Missing height in 2005

Using a similar reasoning we can see whether those children measured and not measured in 2005 are significantly different from each other in terms of 2002 height. Again, the comparison using the 2002 information is informative if we assume that lack of measurement in 2002 is random. In this case, we can break the analysis into two parts: missing height due to attrition or due to lack of measurement.

To analyze the first part we compare the distribution of height in 2002 between the group of children that was found and the group of children that was not found in 2005. Table A2 Panel A suggests that the distributions of height in 2002 are not different between children in these two groups. When we break

---

[1] It can affect the interpretation of the results though. Let's say that we systematically miss children in the higher end of the height distribution across all cohorts and localities. In that case the diff-in-diff estimates will identify the impact on the average child of the subgroup of children measured, but it will not necessarily apply to taller children.

[2] This test is very sensitive to differences in the tails of the distribution, but we show it to complement the tests of differences in selected quartiles.

the analysis for each group of communities (results not shown), we find, for the rural sector, that the median in type-beta communities and the quartile 75th in type-gamma communities are different. In the urban sector, we find that the quartile 90th is different in type-gamma communities.

To evaluate selection in lack of measurement, we now restrict the analysis to those households that were tracked in 2005. The results are shown in Panel B. In this case, we do see significant differences when we analyze the older and middle cohorts together. However, when we break the analysis by cohort we see that all the differences are driven by the older cohort. If we analyze each locality group separately (results not shown), there are also some significant differences in three groups of localities, but again, all the differences are driven by the older cohort. There is no difference in any locality group if we restrict the analysis to the middle cohort. Therefore, even though it seems that the distribution of height in 2002 of children measured in 2005 is different than the distribution of height in 2002 of children not measured in 2005 (conditional on being tracked), this only holds for the older cohort. Fortunately, we do not use the older cohort measured in 2005.

To summarize the results, using the height distribution of children we do not find evidence of selective measurement in either wave. However, these tests are only suggestive, because to test selective measurement in one wave we are assuming random measurement in the other.

Regression analysis

Now we present more conventional regression analyses. Following the structure of the paper, we are going to study the rural and urban sectors separately.

Rural Sector

Table A3 presents the baseline specification of a logit regression where 1 represents having missing height. The omitted category is the older cohort in type-α communities. The first block of explanatory variables include the full cohort-community group interactions, as well as child's gender and age[3]. The second block includes parent characteristics: mother's education, height and age, father's height, presence of the father in the household, and dummies for missing parent's height. The third block includes household characteristics. The variable 'score' is the eligibility score computed following Oportunidades eligibility criteria. Other variables are: access to social security benefits, assets ownership (vehicle, refrigerator, and washing machine), dwelling characteristics (floor material, sanitary service, water inside the dwelling), household size, number of children, characteristics of the household head (gender, and education), a measure of crowding (number of household members per room), per-capita wealth, and log of per-capita expenditure. Finally the last block includes state dummies.

Statistically significant coefficients are highlighted in the table and they clearly suggest that lack of measurement is not random. As expected, the most influential variable is the measurement of the mother: if the mother is not measured the probability that the child is not measured increases a lot.

---

[3] Age enters in a non-parametric way using a linear spline.

Other important mother characteristics are height and age: children from taller and older mothers are more likely to be missing. The presence of the father in the household also increases the probability of having missing height. At the household level, the Oportunidades eligibility score is significant, suggesting that children in poorer households are more likely to be measured. Other variables such as access to social security benefits, number of people per room, and a couple of states are also significant, although a test of joint significance of all the variables at the household level cannot reject the null that the effect is zero.

Finally, children in two cohort-community groups are less likely to be missing. Although only marginally significant, the fact that these two groups are different is particularly important since we are using differences across cohorts and communities to identify impact effects. At the bottom of the table we present tests of joint significance for these subgroups of coefficients. The results suggest that the cohort interactions with the reference category (which represent cohorts in type-alpha communities) as well as the cohort interactions in type-beta and type-gamma communities are not jointly significant. However, children in type-delta communities do differ in their probability of being missing.

Because we are interested in differences across groups, we present next interactions of main explanatory variables with the groups of interest. In all the specifications we keep the explanatory variables of the baseline specification.

We only present the results on the variables that are going to be analyzed. However, no matter what interactions we include, the magnitude of the coefficients (and their significance level) of all the covariates other than the cohort-locality interactions remain fairly stable across specifications. We perform a test of joint significance of all the household-level variables and we can never reject the null that the joint effect is zero at standard confidence levels. With respect to the cohort-locality interactions shown in the first block, the conclusions are almost always the same, but magnitudes and individual significance do vary somewhat across specifications[4]. In a couple of cases none of the four groups of coefficients is jointly significant, but in most of the cases type-delta communities are significantly different.

Results are presented in Table A4. The first three columns show the estimated coefficients when we interact mother's height, mother's age and household score with the four cohort-time groups, first one at a time and all together in model 4. Mother's height seems to be important for younger children (middle cohort in 2002 and younger cohort in 2005). However, the effect does not differ across the cohorts that we compare in the analysis: the test of equal effects on the older cohort in 2002 and the middle cohort in 2005 is not rejected, and neither is the test that compares the middle cohort in 2002 with the younger cohort in 2005. Mother's age seems to be important for children measured in 2002, and the eligibility score is significant across all groups. However, the test across relevant cohorts is not rejected in these cases either.

---

[4] In particular, magnitudes and standard errors increase a lot in some specifications that add locality interactions (models 4 to 8 in Table A4).

The second panel of the table (models 5 to 8) shows interactions of the same explanatory variables by locality group. In this case we are testing whether each variable has a differential effect on the probability of having missing height depending on the locality of residence. Looking at mother´s height, we see that none of the interactions is individually significant, and they are also jointly insignificant. On the contrary, it seems that mother´s age plays a significantly different role on children that live in type-gamma communities. The test of joint significance of the three interactions is rejected, although p-values are not very high. Finally, the score interacted with type-delta communities is marginally significant in one of the specifications, but the effects are never jointly significant.

Finally, we analyze the full cohort-locality interaction for each of these variables. In two cases mother characteristics are significant for children in the young cohort in type-beta communities, and the eligibility score is individually significant for the middle cohort in type-gamma communities. As opposed to previous cases, now there are some groups for which the null hypothesis of no impact is rejected: the joint effect of mother's age in type-beta communities and the joint effect of the score on type-delta communities.

Urban sector

The baseline specification is the same as that used in the rural sector. Results are shown in Table A5. The fact that the mother is measured is again the main explanatory variable. Children from older mothers are still more likely to have missing height, and now also mother's education is significant. At the household level, the eligibility score is no longer individually significant. However, in the urban sector the joint effect of all the household-level explanatory variables is significant at the 5% level, and this effect remains in all the specifications presented next.

As in the rural sector, there are a couple of cohort-locality groups that have different probabilities of missing height. If we analyze the coefficients by locality groups, we see that cohorts of children in type-delta and type-gamma communities are not different from each other. The interactions are both individually and jointly insignificant. However, children in type-beta communities are different: the older cohort in 2002 and the middle cohort in 2005 are significantly more likely to have missing height, and the joint effect of the four cohort-time groups is significant at the 1% level.

As we did for the rural sector, we now interact some of the main explanatory variables with cohorts and locality groups. In this case, we are going to analyze the differential effect of mother's height, age and education[5].

Table A6 presents the results. Models 1 to 4 show interactions of mother characteristics with the four cohort-time groups. Although some individual interactions are statistically significant, none of the effects is significantly different between the older cohort in 2002 and the middle cohort in 2005, or the middle cohort in 2002 and the younger cohort in 2005. Models 5 to 8 show interactions with locality groups. Fortunately, only one variable in one model is statistically significant: mother's education has a differential effect on the probability of having missing height for children in type-gamma communities

---

[5] We also analyze the eligibility score, but that effect is never individually or jointly significantly different from zero.

relative to children in type-delta communities (although the joint effect of both interactions seems to be zero). Finally, we present the full cohort-locality interaction effects. Results suggest that there are significant differences across cohorts and localities in the effect that mother's height has on the probability of having missing height.

To summarize, the analysis suggests that missing height is not random for the group of children used in the analysis. Both in the rural and urban sectors, some parental and household characteristics affect the probability of measurement in a significant way. More importantly, the probability of having missing height is not the same across cohorts and localities, after controlling for a group of individual and household characteristics. Fortunately, interactions of main parental characteristics with relevant groups show that there doesn't seem to be differential effects across cohorts or across locality groups. Fully interacted models, however, do show some important differences.

Table A1: Missing height in 2002

Test of differences between the 2005 height distribution of children measured in 2002 and children not measured in 2002

| | OC-MC | OC | MC | Rural | | | Urban | | |
				type-alpha	type-beta	type-gamma	type-beta	type-gamma	type-delta
mean	-0.01	-0.35	-0.19	-0.04	-0.23	-0.19	0.01	0.39	0.20
	[0.06]	[0.09]	[0.09]	[0.14]	[0.17]	[0.22]	[0.12]	[0.22]	[0.14]
q1	-0.12	-0.23	-0.22	-0.42	-0.24	-0.12	0.14	0.24	-0.03
	[0.11]	[0.19]	[0.14]	[0.28]	[0.32]	[0.44]	[0.20]	[0.47]	[0.27]
q25	-0.79	-0.02	-0.19	-0.12	-0.01	0.00	-0.13	0.27	0.10
	[0.08]	[0.13]	[0.12]	[0.18]	[0.19]	[0.33]	[0.13]	[0.32]	[0.19]
q5	-0.02	-0.09	0.02	0.00	-0.24	-0.26	-0.03	0.35	0.17
	[0.08]	[0.11]	[0.12]	[0.18]	[0.23]	[0.26]	[0.16]	[0.35]	[0.20]
q75	0.14	-0.06	0.20	-0.23	-0.01	-0.13	0.19	0.61	0.27
	[0.09]	[0.11]	[0.13]	[0.14]	[0.22]	[0.26]	[0.16]	[0.38]	[0.18]
q9	**0.23**	0.14	0.30	0.23	-0.33	-0.03	0.29	0.55	**0.50**
	[0.10]**	[0.15]	[0.18]	[0.23]	[0.29]	[0.37]	[0.24]	[0.44]	[0.26]*
K-S test	**0.069**	0.088	0.074	0.066	0.091	0.086	0.099	0.149	0.099
	(0.03)	(0.113)	(0.11)	(0.76)	(0.585)	(0.93)	(0.20)	(0.428)	(0.355)
Obs w/heigth	2949	1670	1279	696	378	272	739	243	549
Obs w/o height	512	195	317	113	81	42	140	35	94

Type-delta in rural communities is not separately analyzed because the sample size is very smal

Standard errors in square brackets. P-values in brackets

*Significant at the 10% level, **Significant at the 5%level, ***Significant at the 1% leve

Source: MxFLS 2002 and 2005

Table A2: Missing height in 2005

Test of differences between the 2002 height distribution of children measured in 2005 and children not measured in 2005

	Panel A			Panel B		
	Attrition			Lack of measurement		
	OC-MC	OC	MC	OC-MC	OC	MC
mean	0.00	-0.01	0.02	**0.13**	**0.23**	0.05
	[0.06]	[0.09]	[0.10]	**[0.07]****	**[0.09]****	[0.10]
q1	0.01	-0.15	0.08	0.08	0.21	-0.05
	[0.12]	[0.15]	[0.19]	[0.13]	[0.16]	[0.21]
q25	-0.10	-0.06	-0.13	**0.21**	**0.30**	0.18
	[0.10]	[0.13]	[0.16]	**[0.10]****	**[0.14]****	[0.15]
q5	0.05	0.05	0.06	**0.20**	**0.32**	0.09
	[0.06]	[0.08]	[0.09]	**[0.08]*****	**[0.10]*****	[0.09]
q75	0.05	0.02	0.11	0.11	**0.20**	-0.01
	[0.09]	[0.11]	[0.12]	[0.09]	**[0.11]***	[0.12]
q9	-0.04	-0.01	-0.08	0.12	0.19	0.02
	[0.11]	[0.14]	[0.17]	[0.10]	[0.14]	[0.17]
K-S test	0.04	0.06	0.04	**0.09**	**0.15**	0.06
	(0.57)	(0.60)	(0.84)	**(0.01)**	**(0.00)**	(0.45)
Obs w/heigth	3331	1853	1478	2949	1670	1279
Obs w/o height	411	202	209	382	183	199

Standard errors in square brackets. P-values in brackets

*Significant at the 10% level, **Significant at the 5%level, ***Significant at the 1% level
Source: MxFLS 2002 and 2005

Table A3: Logit regression (=1 if missing height). Rural sector

mih	Odds Ratio	Robust Std. Err.	z	P>|z|	[95% Conf. Interval]	
mc0	.9186154	.4552545	-0.17	0.864	.3477672	2.426491
mc1	.4426214	.2314758	-1.56	0.119	.158812	1.23362
yc1	.516157	.2926973	-1.17	0.244	.1698598	1.568458
beta_oc0	1.472061	.7543998	0.75	0.451	.5391424	4.019281
beta_mc0	.8317196	.2838327	-0.54	0.589	.4260846	1.623521
beta_mc1	1.887587	.9559056	1.25	0.210	.6995909	5.092953
beta_yc1	1.077303	.3662688	0.22	0.827	.5532745	2.097661
gamma_oc0	**.4803388**	**.2011264**	**-1.75**	**0.080**	**.2114144**	**1.091342**
gamma_mc0	.7366634	.2133045	-1.06	0.291	.4176374	1.299388
gamma_mc1	1.247232	.7079965	0.39	0.697	.4099764	3.794332
gamma_yc1	1.205354	.4379699	0.51	0.607	.5913268	2.456982
delta_oc0	.7363592	.352545	-0.64	0.523	.2881124	1.881991
delta_mc0	**.4067469**	**.2188079**	**-1.67**	**0.094**	**.1417175**	**1.167414**
delta_mc1	.4956206	.3039838	-1.14	0.252	.1489614	1.649016
delta_yc1	1.129447	.5325977	0.26	0.796	.4482048	2.846133
bgender	.9758957	.0514806	-0.46	0.644	.8800366	1.082196
age1	**.3484773**	**.0763251**	**-4.81**	**0.000**	**.2268509**	**.5353139**
age2	.7163496	.1585279	-1.51	0.132	.4642527	1.105339
age3	.8879347	.1834147	-0.58	0.565	.5923168	1.331092
age4	.846824	.1622266	-0.87	0.385	.5817387	1.232703
moe	.9679595	.0296908	-1.06	0.288	.9114813	1.027937
moh	**1.021575**	**.0114767**	**1.90**	**0.057**	**.9993264**	**1.044318**
moage	**1.031775**	**.0112288**	**2.87**	**0.004**	**1.01**	**1.05402**
fah	1.002345	.0143353	0.16	0.870	.9746381	1.030839
fainhh	**2.696266**	**.6590299**	**4.06**	**0.000**	**1.669973**	**4.353276**
mimoe	**.0808053**	**.0326193**	**-6.23**	**0.000**	**.0366293**	**.1782587**
mimoh	**28.26825**	**7.674185**	**12.31**	**0.000**	**16.60417**	**48.12611**
mifah	**2.771081**	**.5516352**	**5.12**	**0.000**	**1.875866**	**4.093518**
score	**.5965285**	**.1324075**	**-2.33**	**0.020**	**.3860963**	**.9216515**
nososec_l	**1.640881**	**.4391789**	**1.85**	**0.064**	**.9710791**	**2.77268**
novehic	1.169318	.2536261	0.72	0.471	.7643749	1.788789
rewa	1.571586	.4434267	1.60	0.109	.9040026	2.732164
soil	.8499326	.1824353	-0.76	0.449	.5580547	1.294471
nobath	1.28459	.2803978	1.15	0.251	.8374635	1.97044
nowater	.8512723	.1709981	-0.80	0.423	.5742283	1.26198
nogas	1.609952	.4799949	1.60	0.110	.8974981	2.887966
hhsize	1.052275	.0473017	1.13	0.257	.9635326	1.149192
children	.9964957	.0840997	-0.04	0.967	.8445744	1.175744
male_hd	1.273972	.2857794	1.08	0.280	.8207621	1.977438
noedu_hd	1.152472	.2751845	0.59	0.552	.7217433	1.840256
iprim_hd	.9948072	.1612631	-0.03	0.974	.7240273	1.366856
hacina	**1.083786**	**.0499566**	**1.75**	**0.081**	**.9901657**	**1.186258**
pcw	.9999989	8.54e-07	-1.27	0.206	.9999972	1.000001
lpce	1.143879	.1249627	1.23	0.219	.9234019	1.416998
mi_pcw	3.970849	5.289286	1.04	0.301	.2917817	54.03917
mi_lpce	1.186828	1.873906	0.11	0.914	.0537538	26.20393
state2	1.149156	.4805915	0.33	0.740	.5062861	2.608328
state3	1.25433	.3397329	0.84	0.403	.7376757	2.132839
state4	.9670177	.3047641	-0.11	0.915	.5213997	1.793486
state5	.9024097	.2092853	-0.44	0.658	.5727889	1.421716
state6	1.133045	.3998372	0.35	0.723	.5673731	2.262692
state7	.5131743	.2372245	-1.44	0.149	.2073867	1.269839
state8	1.14307	.2599009	0.59	0.556	.7320395	1.78489
state9	.576667	.2163802	-1.47	0.142	.2763965	1.203144
state10	1.280682	.2246874	1.41	0.159	.9080366	1.806255
state11	**2.293599**	**1.085654**	**1.75**	**0.079**	**.9070039**	**5.799971**
state12	**.5221748**	**.1380404**	**-2.46**	**0.014**	**.3110248**	**.8766711**
state14	1.137176	.396254	0.37	0.712	.5744112	2.251296
state15	1.058414	.3797473	0.16	0.874	.5239062	2.138244
state16	.6416173	.1742496	-1.63	0.102	.3767974	1.092557
state17	1.453798	.6947938	0.78	0.434	.5697719	3.70943

Chi2 (alpha) = 4.89 (p-value = 0.18)
Chi2 (beta) = 4.64 (p-value = 0.32)
Chi2 (gamma) = 3.01 (p-value = 0.55)
Chi2 (delta) = 14.3 (p-value = 0.006)

Standard errors clustered at the locality level. Reference category: old cohort in type-alpha localities. Sample size: 3436

Table A4: Missing children's height in the rural sector

Logit of missing height for the sample of children used in the analysis.

Odds Ratio and Chi2 tests of key variables reported.

		Cohort-time interactions					Locality interactions			
		Model 1	Model 2	Model 3	Model 4		Model 5	Model 6	Model 7	Model 8
	moh_oc0	1.00			1.00	**moh**	1.02			1.02
		[0.20]			[0.02]		[0.19]			[0.02]
	moh_mc0	1.03			1.04	**moh_b**	1.01			1.00
		[0.18]*			[0.02]**		[0.03]			[0.03]
	moh_mc1	1.00			1.00	**moh_g**	1.01			1.00
		[0.02]			[0.02]		[0.02]			[0.02]
	moh_yc1	1.04			1.03	**moh_d**	1.01			1.00
		[0.02]*			[0.02]		[0.04]			[0.05]
Chi2 tests	**(all=0)**	5.46			6.14	**(b=g=d=0)**	0.22			0.03
		(0.24)			(0.19)		(0.97)			(0.99)
	(oc0=mc1)	0.11			0.00					
		(0.74)			(0.95)					
	(mc0=yc1)	0.08			0.02					
		(0.78)			(0.89)					
	moage_oc0		1.05		1.04	**moage**		1.05		1.05
			[0.17]***		[0.02]***			[0.01]***		[0.02]***
	moage_mc0		1.04		1.04	**moage_b**		0.97		0.97
			[0.02]**		[0.02]**			[0.02]		[0.02]
	moage_mc1		1.03		1.03	**moage_g**		**0.94**		**0.95**
			[0.02]		[0.02]			**[0.03]****		**[0.03]****
	moage_yc1		1.01		1.01	**moage_d**		0.98		0.98
			[0.02]		[0.02]			[0.02]		[0.02]
Chi2 tests	**(all=0)**		**15.06**		**14.39**	**(b=g=d=0)**		5.54		4.88
			(0.00)		**(0.00)**			(0.14)		(0.18)
	(oc0=mc1)		0.4		0.45					
			(0.53)		(0.50)					
	(mc0=yc1)		2.11		1.17					
			(0.15)		(0.22)					
	score_oc0			0.60	0.60	**score**			0.65	0.65
				[0.13]**	[0.13]**				[0.16]*	[0.16]*
	score_mc0			0.66	0.70	**score_b**			0.9	0.93
				[0.15]*	[0.16]				[0.13]	[0.14]
	score_mc1			0.50	0.50	**score_g**			0.78	0.79
				[0.14]**	[0.14]**				[0.18]	[0.19]
	score_yc1			0.58	0.63	**score_d**			**0.67**	0.7
				[0.16]**	[0.18]*				**[0.14]***	[0.17]
Chi2 tests	**(all=0)**			**8.97**	**10.51**	**(b=g=d=0)**			3.89	2.51
				(0.06)	**(0.03)**				(0.27)	(0.47)
	(oc0=mc1)			0.57	0.63					
				(0.45)	(0.43)					
	(mc0=yc1)			0.94	0.50					
				(0.33)	(0.48)					

Regressions include all the explanatory variables shown in Table A3. Robust standard errors in square brackets below estimated coefficients. P-values in brackets below Chi2 tests.

In models 1 to 4 (all=0) referst to the null hypothesis that the joint effect of the four cohort-time interactions is zero, (oc0=mc1) refers to the null hypothesis that the interactions with the old cohort in 2002 and the middle cohort in 2005 are the same, (mc0=yc1) referts to the null hypothesis that the interactions with the middle cohort in 2002 and the young cohort in 2005 are the same.
In models 5 to 8 (b=g=d=0) refers to the null hypothesis that the joint effect of the three locality interactions is zero
*Significant at the 10% level, **Significant at the 5% level, ***Significant at the 1% level
Source: MxFLS 2002 and 2005

Table A4 continued

Locality-Cohort interactions

var =	Mother's height		Mother's age		Household Score	
	coeff	st. error	coeff	st. error	coeff	st. error
var	1.02	[0.03]	1.07	[0.02]	0.67	[0.18]
var_mc0	1.02	[0.04]	0.97	[0.03]	1.02	[0.14]
var_mc1	0.98	[0.06]	0.98	[0.04]	0.81	[0.30]
var_yc1	0.97	[0.04]	1.00	[0.04]	0.91	[0.25]
var_oc0_b	0.94	[0.04]	0.97	[0.04]	0.78	[0.19]
var_mc0_b	0.98	[0.04]	1.01	[0.03]	1.03	[0.18]
var_mc1_b	1.04	[0.06]	1.00	[0.04]	0.91	[0.32]
var_yc1_b	**1.09**	**[0.05]****	**0.91**	**[0.03]****	0.93	[0.28]
var_oc0_g	1.03	[0.06]	0.95	[0.03]	1.30	[0.62]
var_mc0_g	1.03	[0.04]	0.97	[0.04]	**0.54**	**[0.18]***
var_mc1_g	0.94	[0.06]	0.92	[0.06]	0.72	[0.44]
var_yc1_g	1.05	[0.06]	0.93	[0.05]	0.90	[0.37]
var_oc0_d	0.99	[0.1]	0.97	[0.05]	0.48	[0.32]
var_mc0_d	0.93	[0.07]	1.01	[0.04]	1.23	[0.38]
var_mc1_d	0.93	[007]	1.00	[0.09]	0.52	[0.53]
var_yc1_d	1.15	[0.1]	0.95	[0.05]	0.50	[0.22]
Chi2_alpha	1.55	(0.67)	1.23	(0.75)	0.68	(0.88)
Chi2_beta	6.80	(0.15)	**9.71**	**(0.05)**	2.48	(0.65)
Chi2_gamma	1.92	(0.75)	6.29	(0.18)	3.53	(0.47)
Chi2_delta	7.57	(0.11)	4.52	(0.34)	**24.46**	**(0.00)**

Regressions include all the explanatory variables shown in Table A3. Robust standard errors in square brackets next to estimated coefficients. P-values in brackets next to Chi2 tests. *Significant at the 10% level, **Significant at the 5% level, ***Significant at the 1% level
Source: MxFLS 2002 and 2005

Table A5: Logit regression (=1 if missing height). Urban sector

```
-----------------------------------------------------------------------------
            |              Robust
        mih | Odds Ratio   Std. Err.      z    P>|z|     [95% Conf. Interval]
------------+----------------------------------------------------------------
        mc0 |  .8532678    .3728364    -0.36   0.716     .3623697    2.00918
        mc1 |  1.148527    .2581065     0.62   0.538     .7393537    1.784146
        yc1 |  .955237     .3806134    -0.11   0.908     .4374723    2.085796
   betau_oc0 |  1.167328    .4544586     0.40   0.691     .5442605    2.503679
   betau_mc0 |  2.369994    .6453202     3.17   0.002     1.389874    4.04128
   betau_mc1 |  1.020012    .3254092     0.06   0.950     .5458218    1.906162
   betau_yc1 |  1.81621     .4418812     2.45   0.014     1.12738     2.925918
  gammau_oc0 |  .8980266    .2928308    -0.33   0.742     .4739442    1.701575
  gammau_mc0 |  .5887138    .2690619    -1.16   0.246     .240368     1.441889
  gammau_mc1 |  .6247153    .208681     -1.41   0.159     .3245976    1.202317
  gammau_yc1 |  1.707276    .6304399     1.45   0.147     .8279105    3.520659
    bgender |  .9286823    .047606     -1.44   0.149     .8399105    1.026837
       age1 |  .5092028    .1052584    -3.26   0.001     .339576     .7635623
       age2 |  .7284049    .1040886    -2.22   0.027     .5504739    .9638489
       age3 |  .9773267    .1684383    -0.13   0.894     .6971699    1.370064
       age4 |  1.237769    .1599192     1.65   0.099     .9608701    1.594463
------------+----------------------------------------------------------------
        moe |  .9685767    .0182797    -1.69   0.091     .9334038    1.005075
        moh |  1.014612    .0107258     1.37   0.170     .9938064    1.035854
      moage |  1.032943    .00789       4.24   0.000     1.017594    1.048523
        fah |  .9887395    .0106719    -1.05   0.294     .9680427    1.009879
     fainhh |  2.42317     .4870656     4.40   0.000     1.63414     3.593175
      mimoe |  2.25e+07    5704025     66.76   0.000     1.37e+07    3.70e+07
      mimoh |  35.09904    7.081446    17.64   0.000     23.63524    52.12311
    mimoage |  5.00e-09        .          .       .          .           .
      mifah |  3.258533    .4324264     8.90   0.000     2.512249    4.226507
------------+----------------------------------------------------------------
      score |  1.12222     .213493      0.61   0.544     .7729371    1.629341
   nososec_l |  1.13284     .1980418     0.71   0.476     .8041971    1.595786
    novehic |  .8082647    .0909746    -1.89   0.059     .6482555    1.007769
       rewa |  .853918     .1922171    -0.70   0.483     .5493012    1.327461
       soil |  1.273579    .2768922     1.11   0.266     .8316945    1.950239
     nobath |  1.045045    .3755553     0.12   0.902     .5167026    2.113631
    nowater |  .9790767    .1471043    -0.14   0.888     .7293321    1.314341
      nogas |  .6950125    .1819136    -1.39   0.165     .4161001    1.16088
     hhsize |  .9489571    .0311188    -1.60   0.110     .889884     1.011952
   children |  .986443     .0694983    -0.19   0.846     .8592151    1.13251
    male_hd |  1.146593    .2234876     0.70   0.483     .7825244    1.680045
   noedu_hd |  .8152594    .1830166    -0.91   0.363     .525062     1.265847
   iprim_hd |  1.014464    .173721      0.08   0.933     .725226     1.419057
     hacina |  .9787215    .052255     -0.40   0.687     .8814802    1.08669
        pcw |  1           4.18e-08     2.00   0.046     1           1
       lpce |  1.094273    .0713932     1.38   0.167     .9629219    1.243542
     mi_pcw |  1.75081     .6950256     1.41   0.158     .804145     3.811919
    mi_lpce |  1.095375    .394954      0.25   0.801     .5403174    2.220631
------------+----------------------------------------------------------------
     state2 |  1.447452    .479083      1.12   0.264     .756607     2.769096
     state3 |  2.224833    .8222572     2.16   0.030     1.078224    4.590773
     state4 |  1.162236    .4434783     0.39   0.694     .5501717    2.455218
     state5 |  1.456468    .537607      1.02   0.308     .7064931    3.002575
     state6 |  1.245975    .421465      0.65   0.516     .6420681    2.417897
     state7 |  1.524207    .5493881     1.17   0.242     .7520304    3.089247
     state8 |  1.216761    .4414842     0.54   0.589     .597529     2.477716
     state9 |  .9588021    .3720113    -0.11   0.914     .4481943    2.051123
    state10 |  2.581149    1.085416     2.25   0.024     1.132057    5.885151
    state11 |  1.495616    1.164366     0.52   0.605     .3251942    6.878555
    state12 |  .9584044    .4095163    -0.10   0.921     .4148004    2.214412
    state14 |  .7192904    .2753705    -0.86   0.389     .3396514    1.523264
    state15 |  .8386473    .3157679    -0.47   0.640     .4009448    1.75418
    state16 |  .8316062    .4010396    -0.38   0.702     .323168     2.139967
    state17 |  1.41247     .6426866     0.76   0.448     .578995     3.445749
-----------------------------------------------------------------------------
```

Chi2 (beta) = 17.9 (p-value = 0.001)
Chi2 (gamma) = 4.62 (p-value = 0.329)
Chi2 (delta) = 0.74 (p-value = 0.864)

```
-----------------------------------------------------------------------------
```
Standard errors clustered at the locality level. Reference category: old
cohort in type-delta localities. Sample size: 4566

Table A6: Missing children's height in the urban sector

Logit of missing height for the sample of children used in the analysis.

		Cohort-time interactions					Locality interactions			
		Model 1	Model 2	Model 3	Model 4		Model 5	Model 6	Model 7	Model 8
	moh_oc0	1.03			1.03	moh	1.02			1.02
		[0.02]			[0.02]		[0.02]			[0.02]
	moh_mc0	1.00			1.01	moh_b	0.98			0.99
		[0.02]			[0.02]		[0.02]			[0.02]
	moh_mc1	0.98			0.98	moh_g	1.00			1.01
		[0.02]			[0.02]		[0.02]			[0.03]
	moh_yc1	1.03			1.03					
		[0.02]**			[0.01]**					
Chi2 tests	(all=0)	**8.03**			**8.56**	(b=g=0)	0.73			0.62
		(0.09)			**(0.07)**		(0.69)			(0.74)
	(oc0=mc1)	2.36			2.81					
		(0.12)			(0.10)					
	(mc0=yc1)	2.17			1.80					
		(0.14)			(0.18)					
	moage_oc0		1.03		1.03	moage		1.03		1.03
			[0.02]		[0.02]*			[0.01]**		[0.01]**
	moage_mc0		1.05		1.05	moage_b		1.01		1.00
			[0.01]		[0.01]***			[0.02]		[0.02]
	moage_mc1		1.02		1.01	moage_g		1.02		1.02
			[0.02]		[0.02]			[0.02]		[0.02]
	moage_yc1		1.03		1.03					
			[0.01]**		[0.01]**					
Chi2 tests	(all=0)		**19.63**		**18.9**	(b=g=0)		0.66		0.51
			(0.00)		**(0.00)**			(0.72)		(0.78)
	(oc0=mc1)		0.34		0.62					
			(0.56)		(0.43)					
	(mc0=yc1)		0.78		0.44					
			(0.38)		(0.51)					
	moe_oc0			0.98	0.97	moe			1.00	1.01
				[0.03]	[0.03]				[0.03]	[0.03]
	moe_mc0			0.96	0.96	moe_b			0.94	0.95
				[0.03]	[0.03]				[0.03]	[0.04]
	moe_mc1			0.97	0.98	moe_g			**0.94**	0.93
				[0.03]	[0.03]				**[0.04]***	[0.04]
	moe_yc1			0.97	0.96					
				[0.03]	[0.02]					
Chi2 tests	(all=0)			3.22	**2.76**	(b=g=0)			3.46	3.27
				(0.52)	**(0.06)**				(0.18)	(0.19)
	(oc0=mc1)			0.01	0.12					
				(0.94)	(0.73)					
	(mc0=yc1)			0.20	0.00					
				(0.65)	(0.94)					

Regressions include all the explanatory variables shown in Table A5. Robust standard errors in square brackets below estimated coefficients. P-values in brackets below Chi2 tests.

In models 1 to 4 (all=0) referst to the null hypothesis that the joint effect of the four cohort-time interactions is zero, (oc0=mc1) refers to the null hypothesis that the interactions with the old cohort in 2002 and the middle cohort in 2005 are the same, (mc0=yc1) referts to the null hypothesis that the interactions with the middle cohort in 2002 and the young cohort in 2005 are the same.
In models 5 to 8 (b=g=0) refers to the null hypothesis that the joint effect of the tow locality interactions is zero
*Significant at the 10% level, **Significant at the 5% level, ***Significant at the 1% level
Source: MxFLS 2002 and 2005

var =	Mother's height		Mother's age		Mother's education	
	Locality-Cohort interactions					
	coeff	st. error	coeff	st. error	coeff	st. error
var	1.08	[0.05]*	1.05	[0.04]	1.05	[0.05]
var_mc0	0.96	[0.05]	0.96	[0.05]	0.97	[0.05]
var_mc1	**0.98**	**[0.04]***	0.97	[0.04]	0.96	[0.08]
var_yc1	0.96	[0.05]	0.97	[0.04]	0.93	[0.05]
var_oc0_b	0.96	[0.05]	0.98	[0.04]	**0.89**	**[0.05]***
var_mc0_b	**0.95**	**[0.02]****	1.06	[0.04]	0.92	[0.05]
var_mc1_b	**1.09**	**[0.04]****	0.98	[0.04]	0.96	[0.07]
var_yc1_b	0.99	[0.04]	0.99	[0.03]	1.01	[0.06]
var_oc0_g	**0.89**	**[0.05]****	0.94	[0.07]	0.91	[0.09]
var_mc0_g	1.03	[0.05]	1.01	[0.06]	0.93	[0.09]
var_mc1_g	1.04	[0.04]	1.03	[0.05]	0.95	[0.07]
var_yc1_g	1.02	[0.04]	1.06	[0.04]	0.97	[0.07]
Chi2_beta	**11.13**	**(0.03)**	3.12	(0.54)	6.00	(0.20)
Chi2_gamma	7.25	(0.12)	3.99	(0.41)	2.77	(0.60)
Chi2_delta	**13.49**	**(0.00)**	0.75	(0.86)	1.70	(0.64)

Regressions include all the explanatory variables shown in Table A5. Robust standard errors in square brackets next to estimated coefficients. P-values in brackets next to Chi2 tests. *Significant at the 10% level, **Significant at the 5% level, ***Significant at the 1% level

Source: MxFLS 2002 and 2005