Birthweight of children born into households enrolled in conditional cash transfer programs: The case of Mexico's PROGRESA-Oportunidades.

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<u>Abstract</u>

This paper uses conditional and unconditional quantile regressions to investigate whether the conditional cash transfer program *PROGRESA-Oportunidades* had an effect on the birthweight of babies born into enrolled households in rural Mexico. The paper finds that the program effect across the conditional birthweight distribution varies from 135 grams on birthweights at the 20^{th} percentile to 207 grams on birthweights at the 80^{th} percentile. The estimated program impacts on the respective unconditional birthweight quantiles are very similar. Program impacts on birthweight may thus be distributionally regressive, although positive, within the treated population. The paper also uncovers the large deleterious effect of maternal smoking during pregnancy on birthweights at lower quantiles. Specifically, maternal smoking decreases birthweights at the 20^{th} percentile of the conditional distribution by almost half a kilogram. This effect is not picked up by least squares regression estimates.

Keywords: birthweight, quantile regression, unconditional quantiles, conditional cash transfer programs, maternal smoking, Mexico, PROGRESA, Oportunidades.

JEL Classifications: C21, J18, J38

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I. Introduction

Each year, some 11.7 million babies in developing countries -16% of all newborns- are born with low-birthweight (LBW) (≤ 2500 grams) (UN SCN 2004). Compared to babies who were born with normal weight, LBW babies face several disadvantages. They have a much higher risk of neonatal mortality[1], which is especially true among babies from disadvantaged socioeconomic status (McCormick 1985). LBW babies also experience higher mortality and morbidity rates in early childhood (McCormick 1985, Boardman et al 2002), as well as lower adult productivity (Behrman and Rosenzweig 2004). Moreover, the latter paper shows that increased birthweight increases adult height and adult educational attainment at most birthweight levels.[2] Consistent with the latter finding, Breslau and Bohnert (2008) find that LBW children assessed at ages 6, 11, and 17 in urban communities face an increased risk of attention problems, and this risk is greater for babies weighing less than 1500 grams at birth. In this sense, the economic benefits of reducing the incidence of LBW in low-income countries (through lower mortality rates and medical costs, and increased learning and productivity), have been estimated to be at about $510 per infant (Alderman and Behrman 2006). Because of this, physicians and economists have for a while been interested in analysing the determinants of birthweight and on interventions that might prove effective in ameliorating bad outcomes associated with low birthweight.

Prematurity (being born before the 37th week of gestation) and intra-uterine growth retardation / restriction (IUGR) are the two main causes of LBW. While the former is relatively more important in developed countries (Villar and Belizán 1982), a

[1] Neonatal mortality is defined as dying within the first 28 days after birth.
[2] The same paper finds no effect on adult body mass.

combination of both factors, or just IUGR on its own, is more prevalent in developing countries (Villar and Belizán 1982, De Onis et.al 2008). Given that much of the baby's weight is gained during the last weeks of pregnancy, being born prematurely means less time in the maternal uterus to grow and gain weight. On the other hand, IUGR is defined as birth weight below the 10th percentile of the birthweight-for-gestational-age reference curve. It may occur due to factors that prevent normal circulation across the placenta, thus causing poor nutrient and oxygen supplies to the foetus.

The factors that have been found to directly or indirectly (i.e. through increasing the probability of preterm delivery or IUGR) contribute to giving birth to a LBW baby in developing countries are: Low maternal pre-pregnancy weight (Kramer 1987), poor maternal nutrition during pregnancy (Kramer 1987; Martorell and González-Cossío 1987), anaemia (Mavalankar et. al 1992; Feresu et. al 2004), infectious diseases such as malaria (Kramer 1987; Verhoeff et al. 2001; Feresu et. al 2004), maternal smoking during pregnancy (Kramer 1987; Ferraz et. al 1990; Reanne et. al 2004), and inadequate prenatal care (Ferraz et. al 1990; Goldani et. al. 2004) or the lack of it (Mavalankar et. al 1992; Coria-Soto et al 1996; Goldani et. al. 2004; Reanne et.al 2004).

Oportunidades (opportunities), formerly *PROGRESA*, is the most important government-run anti-poverty program in Mexico. It aims at cutting the intergenerational transmission of poverty by providing poor families with current funds to invest in their children's human capital: education, health, and nutrition. In this context, *Oportunidades* may affect the birthweight of newborns in beneficiary households as one of its goals is to improve maternal nutrition during pregnancy and provide pregnant women with access to adequate prenatal care. More specifically, beneficiary households receive cash transfers conditional upon their children's regular attendance to school. Members of beneficiary households are also required to visit health clinics regularly to obtain preventative health care, and the female head of households must attend bi-monthly talks where health and hygiene issues are discussed. If all household members attend their appointments at the health centres, the household receives a cash transfer intended for buying food for its members. Moreover, pregnant women are required to attend at least five prenatal care visits, the first of which has to take place within the first three months of pregnancy (SEDESOL 2009). They are also offered nutritional supplements which constitute 20 per cent of their daily calorie requirements and 100 percent of all necessary micronutrients (Hoddinott and Skoufias 2004). Newborns and malnourished children are also offered similar nutritional supplements.

Using generalized least squares, Barber and Gertler (2008) analysed the effect of *Oportunidades* on birthweight. They found that mean birthweight is 127 grams higher for babies who were born into households which had already received its first cash-transfer from *Oportunidades*. Mean regression estimates may however not be representative of the size and nature of the impact of a given variable on the extreme tails of the conditional distribution. In the case of infant birthweight, we may be particularly interested in the effects of the covariates at the lower tail of the distribution. In this regard, Abrevaya (2001) used quantile regressions in a study of birthweights in the United States. He found that the effect of several factors, including race, education, and prenatal care vary throughout the conditional birthweight distribution. Using a different sample, Hallock and Koenker (2001) corroborated those findings. More recently, Firpo et al. (2009) estimated the impact of covariates on the unconditional quantiles of a given variable of interest using *recentered influence function (RIF) regressions*. In one of their applications, they re-estimated Hallock and Koenker (2001)'s birthweight model using both conditional and unconditional quantile regressions. They found that the conditional quantile regression estimates vary substantially along the birthweight distribution (consistent with Hallock and Koenker 2001), and that such estimates are generally very similar to the ones obtained through unconditional quantile regressions.

This paper extends the analysis of Barber and Gertler (2008) using conditional and unconditional quantile regressions to assess the effect of *Oportunidades* on the birthweight of babies in enrolled households.

The remaining of the paper is divided as follows. The next section provides a deeper description of *Oportunidades* as well as the data at hand. The regression results from the conditional quantile regressions are shown in section III. Section IV deals with the unconditional quantile estimation. Section V concludes.

II. Description of the conditional cash transfer (CCT) program and the database

Oportunidades was introduced in August 1997 under the name *Programa de Educación, Salud y Alimentación* (Health, Education and Nutrition Program - *PROGRESA*)[3] to benefit 140,544 households in rural Mexico. Since then it has expanded, such that there are currently five million beneficiary families (about 20% of all families in Mexico) nationwide (INSP 2005). *Oportunidades* is a targeted CCT program whose main components are education, health, and nutrition. In order to avoid child labour and promote school enrolment, beneficiary households receive cash transfers conditional on their children's regular attendance at school between the grades 3 to 12 (usually, ages 8 to 18[4]). The subsidy increases as children progress through school and from the 7th grade on are higher for girls than for boys. Payments are made on a bimonthly basis directly to mothers as it is thought that they will spend the money on goods for their children and are less likely to spend it on cigarettes and alcohol. Table 1 shows the monthly educational grants in US dollars[5] (in 2008 prices) to which beneficiaries were entitled during the second semester of 2008 (SEDESOL 2008).[6]

The second component of the program emphasizes preventive health care through regular compulsory health centre visits. Table 2 outlines the visits required for different members of beneficiary households. Pregnant women are required to attend at least five prenatal care visits, the first of which has to take place within the first three months of

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[3] Its name was changed in 2002.
[4] The maximum age to enrol at school as a beneficiary is 23 years old.
[5] Although the payments are made in Mexican pesos, the average exchange rate of 11.7 pesos per dollar for the second semester of 2008 (www.banxico.org.mx) was used to convert the amounts to dollars to make the figures more accessible to a broader public.
[6] Monthly scholarships were however capped at a maximum of 1010 Mexican pesos for households with children enrolled in basic education, and at 1745 for those with children in high school (SEDESOL 2008).

pregnancy. Other household members must regularly visit the health clinics to obtain, free of charge, services such as: routine growth monitoring in children; vaccinations; anti-parasitic treatments; prevention and control of high blood pressure and diabetes; detection and control of cervical cancer, as well as family planning.

Finally, under the third component of the program nutritional supplements are given to children who are aged four months to two years, malnourished[7] children aged two to five years old and pregnant and lactating women (SEDESOL 2008). If all family members make their appointments, the household receives a monthly fixed[8] cash transfer -intended for buying food for the household- which in the second semester of 2008[9] was equal to 195 Mexican pesos (about 16.5 USD). The female head of household is also required to attend bi-monthly talks at the clinics, where health, hygiene, nutrition and best practices are discussed; other adult household members have to attend similar talks once a year (SEDESOL 2009). In 2006 an additional cash transfer was introduced for each elderly person living in the household, and one year later another one to cover bills.

In order to get the transfers, beneficiaries must fulfil their requirements (school attendance, health centre visits, and health talks) over a two-month period. Schools and health centres are then given about one month deadline to submit the beneficiaries' attendance sheets to the program administrators, who use them to calculate the transfers to which each household is entitled. After this, the cash is finally handed over the female household head (SEDESOL 2009).

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[7] This is defined as weighing less than the recommended weight given the child´s age. In order for the child not to be considered as malnourished anymore she has to reach the normal recommended weight given her age and maintain it during the six subsequent months. Source: http://www.salud.gob.mx/pagina_principal/manual_cont_sum_sup_alim/manual_progr_nec.htm
[8] That is, it does not depend on the household size.
[9]http://www.oportunidades.gob.mx/Wn_Reglas_Operacion/archivos/Reglas_de_Operacion_Oportunidades_2009_CON%20ANEXOS.doc.

At the rural level, the selection of beneficiaries into the program was carried out in three stages: (i) the localities to be targeted were identified, (ii) beneficiary households within those localities were selected, (iii) the list of beneficiaries in each locality was presented in a community assembly to be corrected and approved (INSP 2005).

Localities were identified using a marginalization index which was constructed for each locality in Mexico for which socio-demographic census data existed.[10] Localities were then categorized as having a: i) very high, ii) high, iii) medium, iv) low or v) very low marginalization index. Highly and very highly marginalized localities were given priority for inclusion in the program at its inception. In the first stages of the program, target localities were also required to have between 50 and 2500 inhabitants (INSP 2005).

To ensure that potential beneficiaries were able to satisfy the program's conditions, the localities to be targeted were then evaluated in terms of their availability of educational and health centre infrastructure. If such infrastructure existed in the locality or nearby[11], a socioeconomic and demographic survey was applied to each household (SEDESOL 2008), and a poverty index score was generated for each of them. Linear discriminant analysis was then used to identify the eligible households. In general, households having poverty index scores above the median were deemed to be eligible. Nonetheless, the fact that the final list of beneficiaries had to be approved by a community assembly meant that some ineligible households were enrolled in the program.

Due to logistical and financial constraints, the program was introduced in phases. Initially, 506 poor localities in seven Mexican states (Guerrero, Michoacan, Hidalgo,

[10] The censuses from which the data was collected are the 1990 National Census, the 1995 Population and Household Count (short census) and more recently, the 2000 National Census.
[11] Starting in 2000, service capacity of schools and health clinics was also verified.

Puebla, Queretaro, San Luis Potosi and Veracruz)[12] were randomly selected to participate in an *evaluation sample*. Of the 506 localities, 320 were randomly selected to start receiving benefits from May 1998, with the remaining 186 incorporated from September 1999.[13] A thorough assessment by the Consulting Group of the quantitative assessment of *Oportunidades*[14] revealed that the treated and control localities had statistically indistinguishable characteristics at the locality level, but not at the household and individual levels, where there were significant differences in some variables (INSP 2005).

The evaluation of the program consisted of periodically interviewing all households (a total of 24,077) in both the control and treatment localities between November 1997 and November 1999, see the timeline in figure 1.[15] The first survey (Survey of Household Socioeconomic Characteristics, *Encuesta de Características Socioeconómicas de los Hogares - ENCASEH*) was used to select households into the program. Among the localities in the *evaluation sample*, on average 78% of the households were classified as eligible to receive program benefits (Hoddinott and Skoufias 2004). After being informed about their eligible status, households were given a deadline to enrol. In order to prevent migration into *PROGRESA* localities to receive the benefits, new households within the selected localities were not allowed to enrol until three years later, when the next assessment to decide on the program eligibility took

[12] During its first year of operation, the program was running in eight states: Campeche, Coahuila, Guanajuato, Hidalgo, Puebla, Querétaro, San Luís Potosí, and Veracruz.
[13] Note that none of the households in the control group knew that they would receive benefits in a later date, so that no anticipation behaviour is expected.
[14] Since its inception, the Mexican government requested some independent organizations to manage the (ongoing) evaluation process.
[15] Nonetheless, by the third follow-up round in March 1999, 5.5 percent of the households (5.1 percent of the individuals) had dropped from the sample. There seems however to be no difference in attrition between the control and treatment areas (Boyce and Gertler 2001).

place. 97% of the eligible households took up the program.[16] Enrolled households were to receive the benefits for a three-year period conditional on meeting their program duties and only about 1% of households were denied the cash transfer for non-compliance (Boyce and Gertler 2001).

After enrolment, four rounds of an evaluation survey (Household Evaluation Survey, *Encuesta de Evaluación de Hogares - ENCEL*) were carried out. The first *ENCEL* took place in March 1998 before the initiation of payments in May 1998. Three other, post-intervention *ENCEL*s were carried out approximately every six months: in October-November 1998, June 1999, and November 1999. The localities that served as the control group began to enrol in the program in September 1999 (as recorded in the evaluation datasets), and to receive benefits soon after.[17]

In order to assess the medium-term effects of the program (after potentially six years of intervention), all 506 localities that were part of the original *evaluation sample* were resurveyed in the Autumn 2003. Given that by then all of the original control localities had already been incorporated into the program, a new control group, which had to be as similar as possible to the original evaluation group, was selected. For this, each locality in the original *evaluation sample* was matched to a locality from a pool of 14,000 potential matches that had not yet been incorporated.[18] This was done through propensity score matching with replacement.[19] Any single potential match was allowed to

[16] Note however that some lags existed in the initiation of payments due to administrative errors, delays in the final registration of beneficiary households, and delays in the processing of the forms needed for payment authorization (Hoddinott and Skoufias 2004).
[17]In the Transfers database, where all transfers made to beneficiaries since September 1999 have been recorded, there are even some households in control localities which received money already in September 1999.
[18] The new control group joined the Program in 2004 (INSP 2005).
[19] Among others, the variables on which the matching was based were: A dwelling characteristics index (the proportion of households with no electricity, those with no toilet, those with clay floor, etc.); an asset

match with at most four original localities and was generally constrained to come from the same state as the original locality[20] (Todd 2004).

Prior to 2003, each *ENCEL* consisted only of a socioeconomic survey. In 2003 however a special fertility questionnaire was also administered to 14,861 fertile-aged (15-49 years old) women. This survey is the main source of data for my analysis. The structure of the fertility database is as follows: First, all women were asked about their date of birth and fertility history. Next, those women who gave birth to a live baby between 1997 and 2003 (the period for which *Oportunidades* had been running) were asked about the characteristics (e.g. date of birth and gender) of each of their children born within that period. Finally, the same subsample of women was questioned about their last pregnancy and additional characteristics of the baby born, including birthweight. There is thus at most one (maternal-reported) birthweight observation per woman.[21]

It is also worth noting that the fertility survey included a representative group of women from each of the three types of localities (original treatment, original control, and new control) in each of the seven Mexican states under evaluation. This was attained through the following steps. First, the number of women (sample size) from each locality type needed to yield positive differences in mean outcomes between the groups was ascertained. The number of localities to be visited in each of the three locality groups was

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index (the proportion of households with no telephone, those with no refrigerator, those with no gas stove, etc.); several socio-demographic characteristics such as the proportion of children aged 12 to15 who work; the proportion of people younger than 15 not in school; some household head characteristics (gender, age and schooling;) the average number of households with no social security; and total population in each locality. This information was obtained from the 2000 National Census (Todd 2004).

[20] The exceptions were localities in Queretaro, where most localities had already been incorporated into the program. In those cases, the matches were thus chosen from neighbouring states (Todd 2004).

[21] Still in the case of multiple births, only one birthweight observation is reported (maybe the one of the baby who came out the latest).

then determined. This was done by taking into account the average number of fertile-aged women by locality in each state, and the size required in each of them (which in turn depended on the number of localities in each of the three groups). Next, the localities to be visited were selected by sampling with probability proportional to the number of fertile-aged women in the locality. The number of households to be interviewed in each locality in each of the three groups was then decided based on the average number of fertile-aged women per household in each locality. Households were then selected by sampling with probability proportional to the number of fertile-aged women in the household. All fertility-aged women in the selected households were then interviewed (CONAPO 2003). All this information together with the data is publicly available at the program's web site (www.oportunidades.gob.mx).

III. Model, Sample and Results

3.1 Baseline model

The effect of *Oportunidades* will first be captured through a dummy variable (*B* = *beneficiary*) indicating whether the recorded birthweight refers to a beneficiary (*B*=1) or a non-beneficiary birth (*B*=0). The *baseline* specification, which controls also for other factors affecting birthweight, is the following:

$$BW_i = \alpha + \gamma\, B_i^k + \beta_1 M_i + \beta_2 Z_i + \beta_3 Y_i + \varepsilon_i \qquad (1)$$

Where: *BW* = birthweight in grams.

$$B_i^k = \begin{cases} 1 & \text{if the birth took place at least } k \text{ months after the household enrolled in } \textit{Oportunidades} \\ & \text{and before it withdrew from the program, in case it did so before the end of 2003.} \\ 0 & \text{otherwise} \end{cases}$$

M = Maternal and infant characteristics. Infant characteristics: birth order, gender and the number of days after birth when the baby was weighed; maternal characteristics:[22] age, smoking during pregnancy, and a prenatal care quality index.[23]

Z = Household's socioeconomic and demographic characteristics before the program was available in the locality. This includes characteristics of the household head (age, education, and ability to speak and indigenous language (proxy for ethnicity)), the

[22] Note that no maternal characteristics before the program's inception were included as there was evidence to think that the household member identifier was not always respected across different survey's rounds. This was concluded after having merged the Fertility Survey with the *ENCASEH* using the individual identifier along with the household one and having found that the DOB -and sometimes even the gender- referring to the same person did not coincide. Although this may be due to the fact that those two surveys were not necessarily answered by the same person (so that there may be inaccuracies even if the individual identifier was respected), I decided to include baseline characteristics only at the household level as it was difficult to know the exact reason why the information allegedly referring to the same person would not coincide across surveys.

[23] Note that no mother in the sample stated not having received prenatal care during her last pregnancy. The prenatal care quality index thus includes information on whether the medical review was undertaken by a physician or a nurse (as opposed to a health promoter, community helper or a non-certified midwife), whether the mother was weighed, and whether her uterus was measured. The latter is very relevant as measuring the height of the fundus (the top of the uterus) allows estimating the baby's weight during pregnancy. This is because the fundus' measure in centimetres usually corresponds with the number of weeks of pregnancy after the 20^{th} week. Likewise, maternal weight gain during pregnancy has also been found to be relevant in explaining birthweight (Abrevaya 2001, Koenker and Hallock 2001, Reanne et al 2004).

family structure (total number of household members, proportion of members who are children younger than six years old, and those between six and 18 years old), the dwelling infrastructure (availability of water and electricity, fridge and stove, and floor covered), and the assets possession (agricultural land ownership). The four variables controlling for dwelling infrastructure and assets possession were aggregated with an equal weight and included as an asset index[24] (*econindex*) in the regressions.

Y = Characteristics of the locality: altitude.[25]

A more detailed description of all covariates is presented in table A1 in the appendix. All variables in vector *M* were obtained from the fertility survey, those in *Z* from the *ENCASEH*, and data on the locality's altitude, *Y*, was obtained from the 2005 short census. Furthermore, I made use of two other publicly available databases in order to identify households that withdrew from the program between September 1999 and December 2003. These datasets are the Transfers Database, which contains information on all transfers made to beneficiaries since September 1999, and the (Socioeconomic) *ENCEL* 2003.

The minimum number of months of program exposure (*k*) after which a birth is recorded as a beneficiary was chosen as to generate groups of beneficiary and non-beneficiary births statistically indistinguishable in terms of their observable characteristics prior to the intervention.[26] This had to be assured for the estimated coefficient of the beneficiary indicator to correctly pick-up the program's effect on

[24] This was decided because there may be some correlation between the four elements (e.g.: if one has a fridge inside the dwelling one has to have electricity available as well).

[25] High altitude has been found to be associated with low birthweight (see Giussani et. al 2001).

[26] Note that only k>2 were considered as k=0 could mean a zero program exposure for some households. That is, k=0 represents the enrolment date; but then households would not have had the time to fulfil the program's requirements (school and health centre attendance) yet, so recording them as "beneficiaries" may be misleading. Likewise, a minimum of one-month exposure (k=1) might be considered as a too short time for households to "digest" the program; and there might be some households for which this threshold is relevant (that is, household in which the baby was born one month after having enrolled in *Oportunidades*.)

birthweight. To ensure as homogeneous sample as possible, the sample was restricted to singleton births, which took place in households where only one member gave birth (perhaps more than once) between 1997 and 2003, designated as poor, randomly assigned to incorporation into the program before 2000, with fully completed interviews, and weighing less than seven kilograms. Given these restrictions and the covariates in equation (1)[27], a sample of 767 observations was obtained.

Mean-comparison tests between the beneficiary and the non-beneficiary births generated by a minimum program exposure ranging from two to nine months (k = 2,..., 9) were then carried out for each independent variable (other than *beneficiary*) in equation (1). Through this process it was concluded that a minimum program exposure of *three* months (k=3) was the one which generated the most statistically similar beneficiary and non-beneficiary births at a 5% significance level.

However, 23 households where the birth took place at least *three* months after enrolment (i.e. potentially beneficiary births) had to be dropped from the analysis as it was not possible to find out whether they withdrew from *Oportunidades* before or after the baby's birth. None of those households had information in the Transfers Database, 16 of them identified themselves as not receiving program cash benefits at the end of 2003 (when the Socioeconomic *ENCEL* was conducted), and the remaining seven did not have information on this last question (three of them because of not showing up in the *ENCEL* 2003 at all.) [28]

The final sample thus consists of 744 observations. The mean-comparison-test exercise described in the previous page was then repeated (see table A2 in the

[27] That is, people with missing information in any of these variables were dropped from the analysis.
[28] See section 3.2 and table A3 (in the appendix) for an explanation of how the results are robust to the inclusion of these observations.

appendix[29]) and it was concluded that, given this sample, only a minimum program exposure of *two* months (k=2)[30] generated groups of beneficiary (560 observations) and non-beneficiary births (184 observations) statistically indistinguishable from each other in terms of pre-treatment characteristics at a 5% significance level, see table 3. From here onwards, k in equation (1) is thus fixed at two and any future reference to that equation will take this into account.

Note that, as there is no publicly available data regarding the amount of cash transfers received by beneficiary households before September 1999, the final sample was obtained assuming that none of the original treatment households (i.e.: those which began to enrol in March 1998) withdrew from the program before that date. This assumption is supported by a mean program exposure before withdrawal for dropouts in our sample of three and a half years. In any case, the main results shown in this paper (i.e. those regarding the effect of *Oportunidades* and maternal smoking during pregnancy on birthweight) are invariant to six beneficiary births, whose households may have withdrawn from the program before September 1999[31], being treated as non-beneficiary births instead.

Figure 1 plots the estimated coefficients for the beneficiary indicator (first panel) and those for the indicator of maternal smoking during pregnancy (second panel) obtained from estimating equation (1) (k=2) using two types of regressions:

[29]Note that as the number of non-beneficiary births increases with the minimum time of program exposure, less people are prone to be dropped from the analysis because of being missing in the Transfers Database and/or having withdrawn from *Oportunidades* at some point before the end of 2003. This explains why there are marginally more observations in table A2 as the minimum time of program exposure increases.

[30] Note that after two months of enrolment households may not yet have received their first cash-transfer, given the joint responsibility calendar described in section II, page 7. Nonetheless, as in our sample the mean time of program exposure for households where a beneficiary birth occurred is two years and nine months, the beneficiary indicator will be on average capturing the program's monetary effect. Note also that if a minimum program exposure of two months (k=2) was considered given the sample of 767 observations, no extra observations would have been deleted as no birth occurred between two and three months after enrolment in any of the households with missing information in the Transfers Database.

[31] I suspect this because although they appear in the Transfers Database, and are not recorded as drop-outs, they have mostly missing values.

conditional quantiles at $\theta = 0.1,\ldots,0.9$ on the one hand, and ordinary least squares on the other hand. The respective 95% confidence intervals for each type of regression are also shown and were obtained using robust standard errors for the OLS regression, and bootstrapped standard errors with 1000 replications for the conditional quantile regressions.

From the first panel in figure 1, it is apparent that the effect of *Oportunidades* on birthweight (*beneficiary*) varies along the conditional distribution. Nonetheless, given the large standard errors that program impacts exhibit, all estimated conditional quantile coefficients lie within the 95% confidence interval of the mean effect.

The second panel in figure 1 illustrates that the confidence intervals for the estimated coefficients on maternal smoking during pregnancy (*smoked*) are also large in both types of regressions. Nonetheless, the 0.2 quantile regression estimate, which is very large and negative, lies well outside the 95% confidence interval of the mean regression.

Table 4 reports full regression results for the baseline specification (equation (1) with k=2) for three different conditional quantile functions, at θ = 0.2, 0.5, 0.8[32], along with the respective OLS regression estimates. As it was illustrated in the first panel of figure 1, we see that the mean effect of *Oportunidades* on birthweight (*beneficiary*) is very similar to the effect at the median of the conditional birthweight distribution, at about 155 grams and statistically significant at a one percent, *ceteris paribus*. A smaller and less significant program effect is found at the lower tail of the conditional birthweight distribution. Specifically, *Oportunidades* is associated with a 135-gram increase on 20th-percentile-birthweights, which is significant at the 10%

[32] Quantiles 0.2 and 0.8 were chosen to represent the effect at the tails of the conditional birthweight distribution, instead of for instance 0.1 and 0.9, because the latter are clearly outliers. That is, a positively sloped line picks up many more points along the conditional distribution than one with a negative slope.

level. In contrast, the program has had a much higher impact on birthweights at the 80th percentile of the conditional distribution, being associated with a 207-gram increase at the 5% significance level. These heterogeneous program impacts are consistent with Chavez (2006), who shows that the effect of *PROGRESA* on consumption is distributionally regressive, although positive, within the treated population.

So why may babies at the top of the conditional birthweight distribution benefit more from the program? If in the absence of *Oportunidades* a newborn's weight is, *ceteris paribus*, a proxy for the general wellbeing of her household, then heavier babies belong to households with better off, healthier members, such that when *Oportunidades* is introduced and the cash transfer received, it can be spent mostly on the pregnant woman, generating a higher positive impact on already better off babies; that is, on those at the upper tail of the conditional birthweight distribution.

Table 4 also shows that, as it was noticed in figure 1, the standard errors associated with the program impacts are very large; and in fact, tests (not shown) for the equality of the estimated coefficients on *beneficiary* at the 0.2, 0.5, and 0.8 conditional quantiles yield that no significant difference between any two set of coefficients exist.

In table 4, the negative effect of maternal smoking during pregnancy (*smoked*) on birthweights at lower quantiles is striking. In particular, babies at the 20th percentile of the conditional birthweight distribution born to a mother who smoked during pregnancy weigh almost 460 grams less than otherwise similar infants born to non-smoking mothers. This effect, though significant only at a ten percent level, is not picked up by least-squares regression estimates.

The results in table 4 also suggest that birth order does not have a significant impact on birthweight at any point of the conditional distribution. Likewise, girls weigh at birth about 185 grams less than boys at the same point of the conditional distribution. Babies at the top of the conditional birthweight distribution born to mothers younger than 20 years old are lighter by 355 grams than otherwise similar babies born to middle-aged (20 to 34 years old) mothers. Babies at the bottom of the conditional birthweight distribution whose mother received prenatal care of higher quality (i.e. the medical review was undertaken by a physician or a nurse, the mother was weighed, and her uterus measured) are 308 grams heavier than babies whose mother received prenatal care, but which lacked all of the above characteristics. The effect of any of those characteristics on birthweights at the 20th percentile of the conditional distribution is thus an increase of 102.6 grams.

Babies at the median of the conditional birthweight distribution born in households whose head speaks an indigenous language (which is a proxy for ethnicity) are 150 grams lighter than otherwise similar babies. Surprisingly, babies at the bottom of the conditional birthweight distribution born in households whose head has between one and six years of formal education are180 grams lighter than similar babies born in households whose head has no formal education. This effect is statistically significant only at the 10% level and may be due to the labour experience that a primary-school-drop-out loses because of attending school for a few years. Recall also that the head of the household is not necessarily the father of the baby whose birthweight is being analysed. On the other hand, the household size, which could be measuring how crowded the dwelling is, as well as how tight a fixed budget is, has a small but statistically significant negative effect on birthweights at the upper tail of the conditional distribution. More precisely, for each additional household member, birthweights at the 80th percentile decrease by 37 grams. Finally, babies at the top of the conditional birthweight distribution born in households with a higher proportion of members between the ages of 6 and 17 years old (*prage6_17*) are heavier by almost 700 grams than otherwise similar babies. Given that in our sample this variable (*prage6_17*) has a standard deviation of 0.214, the effect of a one-standard deviation increase in the proportion of members between the ages of 6 and 17 years old on birthweights at the 80th percentile of the conditional distribution is an increase of 147 grams. Although this effect is statistically significant only at the 10% level, its point estimate is very high and may be due to the social costumes in rural Mexico. There, older children -especially girls- take care of their younger siblings and are used to help with the housework since early ages. Therefore, a higher proportion of children in the household who are old enough (6-17 years old) to take care of the younger children and to help with the housework would mean that a pregnant woman in such a household has more free time to take care of herself during her pregnancy and is less stressed. This would allow her -*ceteris paribus*- to give birth to healthier, heavier babies. The reason why such effect is significant only at upper quantiles may lie on the fact that, as it was already hypothesized, a newborn's weight is a proxy for her household's general wellbeing. Households that -even in the absence of *Oportunidades*- produce heavier newborns may also have older children who are healthy enough to actually take care of younger children and to help with the housework, yielding the effect described above.

3.2 Robustness checks for the baseline sample

Although not reported, note that all the main results (i.e.: those regarding the impact of *Oportunidades* and maternal smoking during pregnancy on birthweight) from the baseline model still hold when the minimum length of program exposure for a given birth to be labelled as beneficiary is three months (*k*=3.)[33]

Table A3 (in the appendix) shows that the results are also not sensitive to the inclusion of most of the 23 births which had been left out of the analysis. It is difficult to know why some people, including those 23 observations, do not show up in the Transfers database. It might be that they withdrew sometime before the transfers began to be systematically recorded (September 1999). If so, and if the reason for withdrawal was random (e.g. if those people were dropped out from the program because the program "*administrators failed to turn in paper work or instructions to beneficiaries in a timely manner*" (Álvarez et.al 2008, p. 646), or any other administrative reason), there would be no reason for concern. In contrast, we may worry if the withdrawal was due to some households' characteristics that also affect birthweight. I thus re-estimated the baseline model (*k*=2), including as non-beneficiaries 16 out of the 23 births which were mentioned above. In those 16 households, the relevant birth occurred from July 2000 onwards, such that even if the household withdrew short before September 1999, there would be no program exposure whatsoever during the pregnancy. This exercise is however subject to criticisms as there are 109 households which despite of not being registered in the Transfers database[34], identified themselves as recipients of *Oportunidades* cash benefits at the end of 2003 (as shown in that year's Socioeconomic *ENCEL*.) Still I further estimated the model with this later sample and allowed the six births which are suspected to be drop-outs (see footnote 31 for clarification), to switch their status

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[33] From table A2 in the appendix, note that in that case the resulting beneficiary and non-beneficiary births differ statistically prior to the intervention only in the proxy for ethnicity at a 5% significance level, and that only six observations switch from the beneficiary to the non-beneficiary group as a consequence.

[34] There are 135 households out of the original 767 that do not appear in the Transfers database.

[35] We know that if the birth is a beneficiary one, the mother has been exposed to the program for at least two months, but we do not know exactly for how long.

(from beneficiary to non-beneficiaries.) Finally, equation (1) was re-estimated after having defined the beneficiary status at birth exclusively on the basis of enrolment (N=767.) The estimation results from these three exercises are shown in table A3 under *sample* (2), (3) and (4) respectively. The results are not particularly different to those using the baseline sample of 744 observations (*sample* 1), and the general patterns always hold. Moreover, note that in *samples* (2) and (3), the null hypothesis of equality of means for the indigenous proxy is rejected with a p-value of 0.03 and 0.01 respectively. This is because 15 out of the 16 observations that were added in *sample 2*, (to the original baseline sample 1, N=744.) and five out of the six observations switching status in (3) are non-indigenous; such that the households that dropped out from *Oportunidades* (if that were the reason for not showing up in the transfers database), are the non-indigenous ones.

Finally, table A4 in the appendix shows that the pattern of the *beneficiary* coefficients still holds when households in which more than one member gave birth during the studied period are included in the sample. The fact that the *beneficiary* point estimate is generally smaller than in the baseline sample may be due to the fact that a new birth translates itself into scarcer monetary resources within the household as no extra governmental subsidy is received due to the birth. Given that in our sample households where a beneficiary birth occurred have on average been exposed to *Oportunidades* for two years and nine months, *beneficiary* is on average capturing the program's monetary effect, as by then those households must have been regularly receiving their cash transfers. Moreover, given the structure of the fertility database, including more than one infant per household in the analysis necessarily means that they are not siblings, so that the conflict over scarcer monetary resources may be exacerbated. The results in table A4, in which the interaction of the beneficiary status and the indicator for the additional households (*add_mem_benef*) is always negative, are consistent with these hypotheses. The analysis will thus continue using the baseline sample of 744 observations.

3.3 An alternative measure for the program's impact

The beneficiary (B_i) indicator in equation (1) does not control for the length of time that the mother has been exposed to the program before giving birth.[35] However, it may be that a longer exposure to *Oportunidades* yields a larger positive effect on birthweight as the mother will presumably be fed more nutritiously for a longer time before giving birth (due to both the cash transfers and the nutritional supplements), positively affecting the weight of the newborn. Similarly, the larger the number of health talks a mother has attended before giving birth, the more likely she is to have internalized the recommendations she received there. Moreover, a woman who was already pregnant when her household enrolled in *Oportunidades* may not have had five prenatal visits as required, or at least the first visit may not have taken place within the first pregnancy trimester.

In order to assess the hypothesis that a longer exposure to *Oportunidades* yields a larger positive effect on birthweight, the beneficiary indicator (B_i) in equation (1) was replaced by *treatint* (treatment intensity): the number of months (for beneficiary births given k=2) between the date when the household took up the program and the baby's birth. Full results for this specification are shown in table A5 in the appendix. The effects of *Oportunidades* (*program impact*) and maternal smoking (*smoked*) on birthweights are however summarized in table 5 under the specification labelled "program months".

[35] We know that if the birth is a beneficiary one, the mother has been exposed to the program for at least two months, but we do not know exactly for how long.

In table 5, the *program impact* estimators under the "program months" specification were obtained by multiplying the estimated coefficients for *treatint* in table A5 by the average number of program months for beneficiary births. The same was done with their respective standard errors. Therefore, the estimated coefficient in the fifth row of table 5 at the upper quantile tells us that a baby at the 80th percentile of the conditional birthweight distribution born in a household that has been exposed to *Oportunidades* for two years and nine months (the average number of program months for beneficiary births) weighs, *ceteris paribus*, 121.2 grams more than a baby in a non-beneficiary household. Measured in this alternative way, *Oportunidades* has no statistically significant effect (at conventional levels) on median birthweights, and its effect on 20th-percentile-birthweights continues to be rather modest, being associated with only an 82-gram increase and significant at the 10% level.

The standard errors on the estimated program impact coefficients are large in this specification as well such that in the next sub-section, the group of non-beneficiary births will be increased using propensity score matching (PSM) at the individual level.

Finally in this specification, maternal smoking during pregnancy has a significant effect (at the ten percent level) on birthweights at the lower tail of the conditional distribution, being associated with a 465-gram decrease on 20th-percentile-birthweights.

3.4 Increasing the sample size

In order to increase the precision of the estimated program impact, the sample size was increased by exploiting information from the new control localities. In particular, each of the 560 beneficiary births in our sample was matched to its nearest-neighbour from a pool of 406 potential matches with complete data from the new control localities. As the total number of potential matches was just about 73% of the number of beneficiary births, and replacement was allowed, some observations had necessarily to serve as a match more than once.

Assuming that in the absence of *Oportunidades* birthweight is independent of whether the individual belongs to the group of beneficiary births or to the pool of potential matches given a set W of conditioning variables describing the average characteristics of the individuals in each group, the matching was performed on the propensity score P(W). Specifically, W was constructed using all variables in vectors Z and Y in equation (1). P(W) was then estimated through a logistic model. This procedure yielded only 184 matches due to the individuals from the new control localities having on average lower propensity scores. Moreover, it was not possible to re-estimate equation (1) using all 184 matches because doing so produced a zero probability of incorrectly rejecting the null hypothesis of equality of means between the resulting beneficiary and non-beneficiary births for several variables. Because of this, only 41 matches, which were the individuals having predicted probabilities larger than 0.7, were included in the final sample as non-beneficiary births. Table A7 shows the mean-comparison tests between the beneficiary and non-beneficiary births after matching for all variables in the vectors M, Z and Y in equation (1) (k=2).

Similar to the pre-matching situation, the beneficiary and non-beneficiary births are statistically equal (at a five percent level) in terms of all the individual, household and locality characteristics for which we are controlling for. Full results obtained from estimating equation (1) through conditional quantile regressions and OLS using the matched sample are shown in table A6. The main post-matching results are shown in table 5 under the specification labelled *matched* and are as follows.

The standard errors of the estimated beneficiary coefficients slightly decreased at each of the quantiles under investigation. However, the respective point estimates decreased as well, such that some statistical significance was lost. In particular, the program is now associated with a 120-gram increase on birthweights at the median of the conditional distribution at the 5% significance level. No program effect was found at the lower tail of the conditional birthweight distribution and, similar to the pre-matching situation, the largest effect is found on birthweights at the 80th percentile, at 184 grams.

The reason why the estimated beneficiary coefficients in the matched sample may be smaller than the ones in the original sample is because the baseline group has now changed and seems to be heavier at birth than the previous baseline group. In fact, while the original non-beneficiary group had a mean birthweight of 3154 grams, the respective post-matching group weighed on average 125 grams more at birth. Given that the constant term also generally increases in the post-matched sample (the exception is at the median, where the baseline group weighs 33 grams less at birth than the original baseline group), this means that *ceteris paribus*, beneficiary births are now generally being compared to heavier babies, such that the benefit of having enrolled in the program at least two months before birth is now lower than before.

So why may babies in the new control localities weigh more at birth than otherwise similar babies in the originally evaluated localities? First, recall that localities with very high or high marginalization index were incorporated into the program first, such that we may expect the new control localities not to be as poor as the ones in the original *evaluation sample*. Moreover, even when both types of localities were matched using propensity score matching, they may significantly differ in some variables at the household and/or individual levels.[36] This is why I used household characteristics to match the beneficiary births to births in the new control group (but differences at the individual level may still exist between them.) Furthermore, note that the new control localities were questioned about their 1997 socioeconomic and demographic characteristics retrospectively, in 2003. This may have led to some inaccuracy in the information given, which was then used to perform the matching.

As for the large negative impact of maternal smoking on birthweights at lower quantiles, this effect is still present after the matching. In fact, the relevant point estimate increased, such that babies at the 20th percentile of the conditional birthweight distribution born to a mother who smoked during pregnancy weigh 543 grams less than infants born to non-smoking mothers, *ceteris paribus*. This effect is again statistically significant at the 10% level.

So far, quantile regressions have helped us to uncover the differences in magnitudes of the effects of *Oportunidades* and maternal smoking during pregnancy at different points of the *conditional* birthweight distribution. However, *conditional* quantile regression estimates do not tell us what will happen to a particular baby when we change a covariate by a small amount since the baby will not necessarily be on the same quantile after the change. For this, *unconditional quantile regressions* are needed. Such estimation will allow us to learn whether what the program is doing is beneficial to mothers and babies so that the babies are born with higher weight. This is the subject of the next section.

[36] Recall that this was in fact the case between the original treatment and the original control localities (INSP 1005).

IV. Unconditional Quantile Estimation

In this section, the effect of the covariates on the marginal (unconditional) birthweight distribution is analysed by estimating equation (1) (*k*=2) using *unconditional quantile regressions* at $\theta = 0.2$, 0.5, and 0.8, and the pre-matching sample.

Unconditional quantile regressions, also known as *recentered influence function* (*RIF*) *regressions* (Firpo et.al 2009), build upon the concept of the influence function (IF), which represents the influence of an individual observation on a given distributional statistic. In our case, given a quantile q_θ, and an outcome variable *BW*, the influence function IF($BW; q_\theta$) is given by: $(\theta - 1\{BW \leq q_\theta\}) / f_{BW}(q_\theta)$. Where *BW* = birthweight, $1\{.\}$ is an indicator function, $f_{BW}(.)$ is the density of the unconditional birthweight distribution, and q_θ is the population θ-quantile of the marginal birthweight distribution.

Adding back the distributional statistic of interest to the IF (.) yields the RIF (Firpo et.al 2009). In our case, RIF ($BW; q_\theta$) is thus: q_θ + IF ($BW; q_\theta$). The *RIF-regression model* (Firpo et.al 2009) is then defined as E [RIF ($BW; q_\theta$) | X]. That is, as the conditional expectation of the RIF (.) modelled as a function of the explanatory variables *X*. In our case, *X* includes all variables in vectors *M*, *Z* and *Y* in equation (1).

Firpo et.al (2009) show that the partial effect of marginally changing the distribution of a covariate on an unconditional quantile is non-parametrically identified under sufficient assumptions guaranteeing that the conditional birthweight distribution does not change in response to a change in the distribution of covariates. The following algorithm suggested by Firpo et.al (2009) to compute *RIF-regressions* has been adapted for our case where the outcome variable is birthweight and regressions are estimated at the 0.2, 0.5 and 0.8 quantiles:

1. Estimate q_θ, θ = 0.2, 0.5, 0.8, by estimating the density of the unconditional birthweight distribution $f_{BW}(q_\theta)$, at θ= 0.2, 0.5, 0.8, through kernel or other methods.

2. Construct the function 1{BW≤ q_θ} indicating whether the value of birthweight is below q_θ.

3. Run an OLS, logit, or non-parametric regression of this new dependent variable on *X*. The estimated coefficients will then give us the average marginal effects, or *unconditional quantile partial effects* (Firpo et. al 2009), $E\left\{\frac{dE[RIF(BW, q_\theta) \mid X = x]}{dx}\right\}$

Figure 2 plots the estimated coefficients for the beneficiary indicator obtained by estimating equation (1) (*k*=2) using three different types of regressions: ordinary least squares (horizontal line with triangles), conditional quantile regressions at θ = 0.1,…,0.9 (curve with squares), and OLS-*RIF-regressions*[37] at θ = 0.1,…,0.9 (curve with diamond). In particular, as the unconditional birthweight distribution is unimodal and approximately Gaussian (see figure A1 in the appendix), the *RIF-regressions* were estimated using an Epanechnikov kernel function and the default "optimal" bandwidth (115) calculated in Stata. However, given that such "optimal" bandwidth may be too wide and may oversmooth the density if the distribution was multimodal, alternative unconditional-quantile regressions were estimated using a narrower bandwidth (95) and, as a sensitivity analysis, a wider bandwidth (135). Those regressions yielded quantitatively similar results to those reported in figure 2, as did the use of a Gaussian function, and are available upon request.

From figure 2 we see that the program effect on the unconditional quantiles of the birthweight distribution is very similar to the effect on the respective conditional

[37] Firpo et.al (2009) show that the choice of estimator is not crucial in large samples.

quantiles. Moreover, those effects lie all around the OLS estimate of the average program effect.

Table 6 shows full regression results from estimating equation (1) (k=2) using ordinary least squares, conditional, and unconditional quantile regressions at $\theta = 0.2$, 0.5, and 0.8. As it was noticed in figure 2, the unconditional-quantile program partial effects are very similar to the conditional effects. Specifically, *Oportunidades* seems to have had an effect on birthweights at the 20th percentile of the conditional distribution and on the respective percentile of the unconditional distribution at the 10 percent significance level and at about 140 grams. Likewise, the program point estimates on median birthweights from the standard quantile regression and the respective (OLS) *RIF- regression* are very similar, and both estimates increase at top birthweights. This similarity in program effects on the various quantiles of the conditional and unconditional birthweight distributions is due to the large standard errors that the estimated beneficiary coefficient displays and reflects the fact that, despite all our control variables, most of the birthweight variation remains unexplained. That is why marginalizing over the covariates does not change our findings.

As for maternal smoking during pregnancy (*smoked*), it is not statistically significant at any point of the marginal birthweight distribution. This is not too different from the case of the conditional distribution where an effect existed only on birthweights at the 20th percentile and only at the 10% significance level.

The main estimated coefficients regarding different specifications or samples have been summarized in tables A8 and A9 in the appendix.

V. Conclusions

Using quantile regressions this paper has shown that the effect of *Oportunidades* on birthweight varies across the conditional distribution. Specifically, while the estimated effect on median birthweights is 155 grams, a lower (135 grams) and statistically less significant program effect has been found on birthweights at the 20th percentile of the conditional distribution. In contrast, *Oportunidades* is associated with a 206-gram increase on birthweights at the 80th percentile of the conditional distribution. Nonetheless, given the huge variance that the program effect exhibits at any given point of the conditional birthweight distribution, it may not be possible to distinguish the program effect at different quantiles from the mean effect.

Because of this, propensity score matching at the individual level was used to reduce the variance but the standard errors of the estimated program impact coefficients continue to be quite large. Efforts to further increase the sample size proved fruitless as the resulting beneficiary and non-beneficiary birth groups did not seem to be comparable prior to treatment.

The use of conditional quantile regressions in this paper has also uncovered the fact that least-squares regression estimates hide the deleterious effect of maternal smoking on birthweights at lower quantiles. Although this effect is statistically significant only at the ten percent level, the point estimate means that maternal smoking decreases birthweights at the 20th percentile of the conditional distribution by almost half a kilogram.

Finally, *recentered influence function regressions* (Firpo et. al 2009) were used to estimate the effect of *Oportunidades* on various quantiles of the unconditional birthweight distribution. Such estimates are qualitatively and quantitatively very similar to the ones obtained through conditional quantile regressions.

In summary, the impacts of *Oportunidades* on birthweight in Mexico were found to be distributionally regressive, although positive, within the treated population.

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Figures and Tables

Table 1: Monthly educational cash transfer for July-December 2008 by school grades and gender (US dollars*, 2008 prices).

School Grade	Cash Transfer (US$)	
Primary School	Males & Females	
3rd	11	
4th	13	
5th	17	
6th	23	
Lower Secondary School	Males	Females
7th	33	35
8th	35	38
9th	37	42
Upper Secondary School	Males	Females
10th	55	63
11th	59	68
12th	63	72

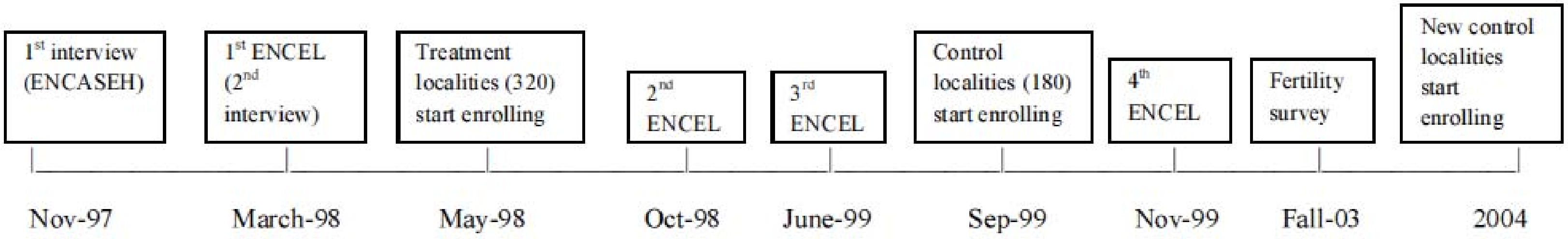
**As the transfers are made in Mexican pesos (www.oportunidades.gob.mx), the average exchange rate of 11.7 pesos per US dollar for the second semester of 2008(Bank of Mexico) was used to convert the amounts to dollars.*

Table 2: Required visits to health clinics by beneficiaries.

Who?	# of check-ups	When?
Children		
< 4 months	3	At 7, 28 days, and two months
4-24 months	8 + 20	At 4,6,9,12,15,18,21, and 24 months + 1 per month to check weight and size
2-4 years old	3 per year	One every 4 months
5-9 years old	2 per year	One every 6 months
10-19 years old	2 per year	One every 6 months
Women		
Pregnant	5	The first one must take place during the first pregnancy trimester
Lactating	2	-
Young people and adults		
20-49 years old	2 per year	One every 6 months
> 50 years old	1 per year	-

Source: Operation Rules 2008, SEDESOL.

Figure 1: Time line for the evaluation of *Oportunidades*



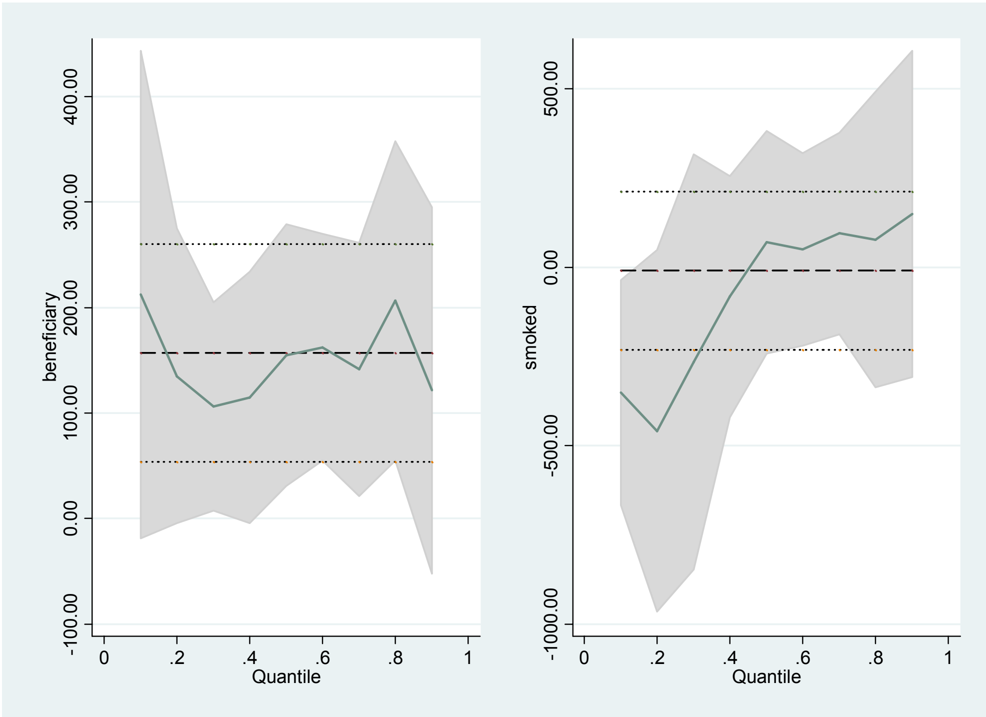
ENCASEH stands for the Survey of Household Socioeconomic Characteristics, ENCEL stands for the Household Evaluation Surveys. Source: INSP 2005.

Table 3: Means' comparison of individual & household characteristics for beneficiary and non-beneficiary births given at least two months of exposure to *Oportunidades*.

Two-month exposure	Non-beneficiaries		Beneficiaries		Means' Diff.	*P*-value*
	Mean	SD	Mean	SD		
Maternal and Infant Characteristics						
Total number of babies born alive	4.59	2.35	4.76	2.43	-0.16	0.42
Infant's gender (female=1)	0.42	0.49	0.47	0.50	-0.05	0.24
Number of days after birth when baby was weighted	4.15	10.00	2.94	8.21	1.20	0.14
Maternal age	29.79	6.10	30.20	6.23	-0.41	0.43
Mother smoked during pregnancy (Y/N)	0.04	0.20	0.04	0.20	0.00	0.89
Prenatal care's quality index (0-1)	0.91	0.24	0.94	0.17	-0.03	0.16
HH socieco. & demograp. Charact. at Baseline						
Head of HH speaks indigenous language (Y/N)	0.28	0.45	0.34	0.47	-0.06	0.10
Head of household's education (years)	3.84	2.73	3.51	2.56	0.33	0.15
Head of household's age	34.76	9.71	34.29	11.01	0.47	0.58
Household size	6.05	2.08	5.89	2.28	0.15	0.39
Children aged 0-5 in household (proportion)	0.31	0.17	0.30	0.17	0.01	0.45
Children aged 6-17 in household (proportion)	0.29	0.22	0.27	0.21	0.02	0.22
Economic index (0-1)	0.30	0.22	0.29	0.22	0.01	0.65
Locality Characteristics						
Altitude (meters)	1307	838	1293	814	13.95	0.84
N	184		560		Total N	744

**P*- value for the test: H_0: μ_0-μ_1 = 0 vs. H_a: μ_0-$\mu_1 \neq 0$. Where 0=non-beneficiary birth, 1=beneficiary birth. Unequal variances were assumed. *Source: Own computation using data from ENCASEH, fertility and socioeconomic rural surveys 2003, and transfers database.*

Figure 1: The effect of *Oportunidades* and maternal smoking on birthweight at different quantiles (minimum program exposure is two months.)



Plots of the estimated coefficients for the beneficiary indicator (first panel) and those for the indicator of maternal smoking during pregnancy (second panel) obtained from estimating equation (1) (k=2) using conditional quantiles (continued curve) on the one hand, and ordinary least squares (horizontal dashed line) on the other hand. The respective 95% confidence intervals are also shown and were obtained using robust standard errors in the case of the OLS regression, and bootstrapped standard errors (1000 replications) for the quantile regressions.

Table 4: Regression results for singleton birthweights after at least two months of exposure to *Oportunidades* (baseline specification*).

	Quantile Regressions			OLS
	20%	50%	80%	
beneficiary	135.2*	155.0***	206.5**	157.2***
	(77.00)	(54.52)	(83.01)	(50.61)
firstbir	-8.616	-142.3	-204.2	-157.4
	(146.7)	(126.0)	(129.4)	(103.8)
second	-125.9	-142.0	0.552	-114.0
	(114.0)	(115.1)	(138.0)	(87.25)
third	-81.69	-56.38	77.70	-44.33
	(101.3)	(80.51)	(110.5)	(74.87)
female	-182.4***	-191.3***	-190.9***	-168.3***
	(62.83)	(53.35)	(72.04)	(45.40)
daysafwe	5.886	5.358**	5.787	6.114**
	(3.660)	(2.315)	(3.916)	(2.674)
young	-88.65	-198.6*	-355.2**	-250.1**
	(218.1)	(111.8)	(139.6)	(115.6)
old	-34.65	11.13	-16.00	44.31
	(83.36)	(69.03)	(98.45)	(59.93)
smoked	-458.7*	69.35	77.59	-9.786
	(276.4)	(181.2)	(191.2)	(149.5)
qualindex	308.3**	124.9	36.96	80.98
	(154.8)	(146.2)	(221.5)	(133.5)
indig	-123.7	-149.7**	-113.3	-144.2**
	(94.95)	(67.16)	(83.83)	(62.19)
edu6head	-179.1*	-48.45	-51.44	-110.0*
	(92.19)	(62.88)	(91.43)	(60.53)
edplushead	-12.91	61.49	110.3	1.338
	(82.19)	(73.98)	(112.6)	(68.65)
agehead	2.898	3.438	3.467	3.551
	(4.142)	(3.712)	(3.844)	(2.668)
famsize	-6.507	-13.73	-36.90**	-23.29*
	(23.11)	(15.39)	(18.23)	(12.84)
propage5	209.4	22.31	156.3	109.1
	(351.8)	(329.2)	(400.0)	(293.9)
prage6_17	186.5	402.1	688.8*	326.3
	(317.4)	(287.7)	(351.6)	(247.6)
econindex	-58.61	-132.3	-263.6	-125.3
	(173.1)	(152.6)	(172.2)	(112.4)
altitude	0.000779	-0.0582	-0.0297	-0.0248
	(0.0453)	(0.0405)	(0.0435)	(0.0354)
Constant	2510***	3129***	3581***	3207***
	(295.8)	(269.2)	(413.5)	(258.3)
N	744	744	744	744

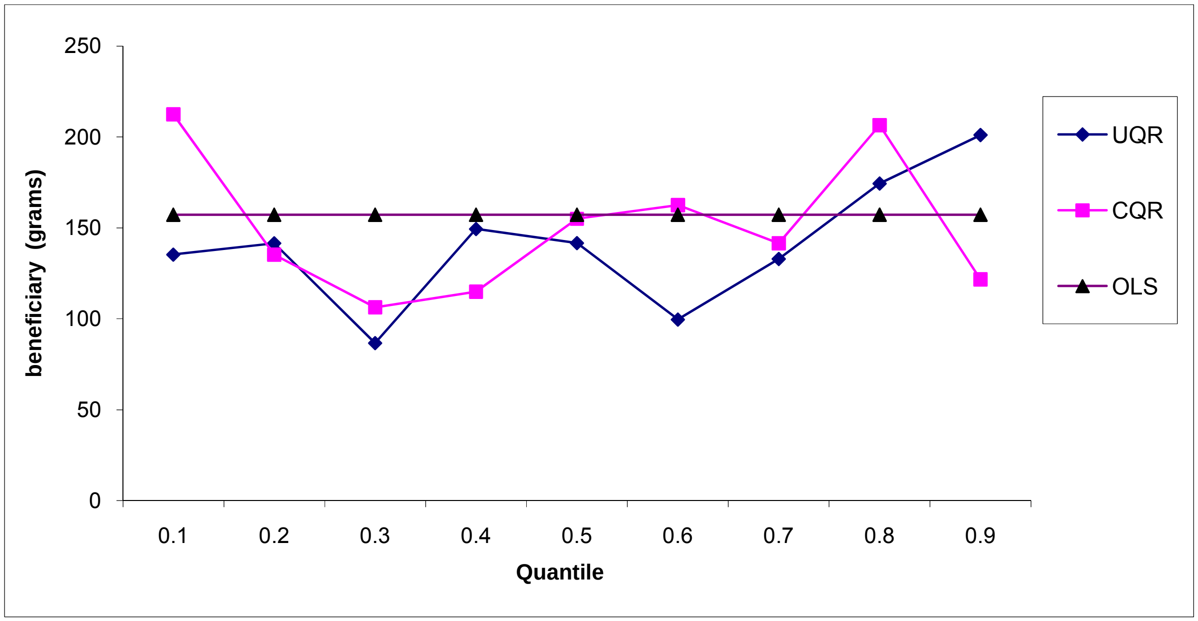
*This specification refers to equation (1) (*k*=2) in page 13. Robust standard errors (OLS) and bootstrapped standard errors (1000 replications) for the quantile regressions in parentheses. Asterisks denote the significance level (double sided) *: 10%, **: 5%, ***: 1%

Table 5: Main regression results for singleton birthweights for three different specifications / samples* (minimum program exposure is two months.)

Variable	Quantile Regressions			OLS
Specification	20%	50%	80%	
Program impact (*beneficiary* or *treatint*)				
Baseline	135.2*	155.0***	206.5**	157.2***
	(77.00)	(54.52)	(83.01)	(50.61)
Program months	81.99*	57.04	121.19**	85.56**
	(45.81)	(42.90)	(51.19)	(33.81)
Matched	109.8	120.2**	184.3**	120.4**
	(71.38)	(47.92)	(71.97)	(46.85)
Smoked				
Baseline	-458.7*	69.35	77.59	-9.786
	(276.4)	(181.2)	(191.2)	(149.5)
Program months	-464.7*	74.33	-15.18	-12.88
	(281.3)	(174.4)	(188.7)	(149.4)
Matched	-543.4*	95.56	124.4	-6.182
	(289.7)	(164.1)	(164.9)	(144.5)
Constant				
Baseline	2510***	3129***	3581***	3207***
	(295.8)	(269.2)	(413.5)	(258.3)
Program months	2466***	3269***	3728***	3257***
	(300.7)	(299.3)	(392.3)	(264.8)
Matched	2610***	3096***	3847***	3326***
	(297.8)	(224.4)	(402.0)	(229.1)

*"Baseline" refers to the model in equation (1) (*k*=2). "Program months" substitutes the number of months between program enrolment and the baby's date of birth for the indicator variable *beneficiary* in the baseline model. In this latter case, the estimated coefficients for program exposure (*treatint*) in table A5 (in the appendix), and their respective standard errors, have been multiplied by the average number of program months (two years and nine months) for beneficiary births in order to gain intuition. "Matched" stands for the results after matching the beneficiary births to births from non-control localities. Full results for this latter specification are shown in table A7 (in the appendix.)The total number of observations for the original model is 744; for the model after matching, it is 785. Robust standard errors (OLS) and bootstrapped standard errors (1000 replications) for the quantile regressions in parentheses. Asterisks denote the significance level (double sided) *: 10%, **: 5%, ***: 1%

Figure 2: Conditional and unconditional quantile regressions (CQR and UQR respectively) estimates for the effect of *Oportunidades* (*beneficiary*) on birthweight in grams given a minimum program exposure of two months.



Estimated coefficients for the beneficiary indicator obtained by estimating equation (1) (*k*=2) using three different types of regressions: ordinary least squares (horizontal line with triangles), conditional quantile regressions (curve with squares), and unconditional quantile regressions (curve with rhombus). The latter were estimated using an Epanechnikov kernel function and the default "optimal" bandwidth (115) calculated in Stata. Alternative UQR were carried out using a narrower bandwidth (95), a wider bandwidth (135), as well as a Gaussian kernel function, yielding quantitatively similar results to those reported in this table.

Table 6: OLS, conditional quantile regressions (CQR) and unconditional quantile regressions (UQR) for singleton births given a program exposure of at least two months.

	OLS	20th percentile		50th percentile		80th percentile	
		CQR	UQR	CQR	UQR	CQR	UQR
beneficiary	157.2***	135.2*	141.5*	155.0***	141.6**	206.5**	174.4**
	(50.61)	(77.00)	(74.22)	(54.52)	(62.87)	(83.01)	(77.27)
firstbir	-157.4	-8.616	-136.2	-142.3	-95.68	-204.2	-220.8
	(103.8)	(146.7)	(168.6)	(126.0)	(135.0)	(129.4)	(143.6)
second	-114.0	-125.9	-81.92	-142.0	-92.06	0.552	-32.01
	(87.25)	(114.0)	(117.8)	(115.1)	(101.9)	(138.0)	(114.5)
third	-44.33	-81.69	-0.704	-56.38	-15.02	77.70	7.989
	(74.87)	(101.3)	(87.82)	(80.51)	(88.67)	(110.5)	(102.8)
female	-168.3***	-182.4***	-198.1***	-191.3***	-195.6***	-190.9***	-180.3***
	(45.40)	(62.83)	(64.40)	(53.35)	(59.15)	(72.04)	(63.46)
daysafwe	6.114**	5.886	4.185	5.358**	6.979**	5.787	8.918**
	(2.674)	(3.660)	(2.586)	(2.315)	(2.952)	(3.916)	(4.284)
young	-250.1**	-88.65	-55.48	-198.6*	-378.7***	-355.2**	-225.4*
	(115.6)	(218.1)	(200.0)	(111.8)	(132.7)	(139.6)	(124.8)
old	44.31	-34.65	-47.31	11.13	16.36	-16.00	60.64
	(59.93)	(83.36)	(78.96)	(69.03)	(73.84)	(98.45)	(82.40)
smoked	-9.786	-458.7*	-275.5	69.35	49.93	77.59	183.9
	(149.5)	(276.4)	(193.4)	(181.2)	(141.8)	(191.2)	(183.8)
qualindex	80.98	308.3**	231.5	124.9	182.9	36.96	20.73
	(133.5)	(154.8)	(167.4)	(146.2)	(139.4)	(221.5)	(166.7)
indig	-144.2**	-123.7	-87.00	-149.7**	-145.7**	-113.3	-109.0
	(62.19)	(94.95)	(75.88)	(67.16)	(72.59)	(83.83)	(76.44)
edu6head	-110.0*	-179.1*	-151.1*	-48.45	-54.01	-51.44	-111.8
	(60.53)	(92.19)	(81.23)	(62.88)	(80.55)	(91.43)	(91.16)
edplushead	1.338	-12.91	6.536	61.49	2.928	110.3	52.39
	(68.65)	(82.19)	(82.65)	(73.98)	(90.67)	(112.6)	(105.3)
agehead	3.551	2.898	4.725	3.438	1.631	3.467	0.991
	(2.668)	(4.142)	(3.472)	(3.712)	(3.561)	(3.844)	(3.743)

famsize	-23.29*	-6.507	-2.256	-13.73	-8.344	-36.90**	-30.36
	(12.84)	(23.11)	(18.21)	(15.39)	(19.78)	(18.23)	(18.45)
propage5	109.1	209.4	626.6*	22.31	6.502	156.3	-91.75
	(293.9)	(351.8)	(361.4)	(329.2)	(295.3)	(400.0)	(337.2)
prage6_17	326.3	186.5	330.0	402.1	396.2	688.8*	432.1
	(247.6)	(317.4)	(324.6)	(287.7)	(284.5)	(351.6)	(291.0)
econindex	-125.3	-58.61	-89.59	-132.3	-139.2	-263.6	-171.1
	(112.4)	(173.1)	(143.8)	(152.6)	(140.0)	(172.2)	(156.4)
altitude	-0.0248	0.000779	-0.00662	-0.0582	-0.0122	-0.0297	-0.0180
	(0.0354)	(0.0453)	(0.0428)	(0.0405)	(0.0421)	(0.0435)	(0.0469)
Constant	3207***	2510***	2365***	3129***	3090***	3581***	3827***
	(258.3)	(295.8)	(317.6)	(269.2)	(259.5)	(413.5)	(340.6)

The total number of observations in each case is 744. Robust standard errors (OLS) and bootstrapped standard errors for the quantile regressions (1000 repetitions for the conditional quantiles and 800 for the unconditional ones) in parentheses. For the RIF regressions, the “optimal” bandwidth of 115 calculated by the program has been used. Alternative bandwidths (at 95 and 135) were tried yielding no particularly different results. Asterisks denote the significance level (double sided) *: 10%, **: 5%, ***: 1%

Appendix

Table A1: Description of Variables

Variable's Name	Description
Dependent Variables	
birthwe	Birth-weight in grams.
treatint	Number of months between enrolment in Oportunidades and date of birth for beneficiary births (ie: *beneficiary* =1).
Main Independent Variable	
beneficiary	Binary variable. 1=The baby was born at least two months after his/her household registered in the program and before it withdrew from it.
Maternal and Infant's Characteristics	
firstbir, second, third	Birth order: firstbir=first birth; second=second birth; third=third birth; highord=omitted category (fourth and beyond births).
female	Infant's gender: female=baby girl; male=default case.
daysafwe	Number of days after birth when baby was weighted.
young, old	Maternal age: young=less than 20 years old; old=more than 34 years old; middle=default category (20-34 years old)
smoked	Binary variable: 1=Mother smoked during pregnancy
qualindex	Prenatal care's quality index (0-1): The revision was undertaken by a physician or a nurse, the mother was weighted, her uterus was measured.
Household's Socioeconomic and Demographic Characteristics at Baseline (i.e.: before the program's inception)	
indig	Binary variable: 1=Household head speaks an indigenous language
edu6head, edplushead	Household head's years of education: edu6head=1-6 years; edplushead=more than 6 years; noeduc=no formal education (default category)
agehead	Household head's age
famsize	Number of members in the household
propage5	Proportion of household members who are children aged 0-5 years old
prage6_17	Proportion of household members who are children aged 6-17 years old
econindex	Asset index (0-1): The dwelling has water and electricity, a fridge and a gas stove available, its floor is covered, and the household owns agric. land.
Locality Characteristics	
altitude	Locality's altitude in meters.
Interactions	
add_mem_benef	Interaction between *beneficiary* and *add_mem_benef* (indicator for households in which more than one member gave birth between 1997-2003.)

Source: Fertility survey (maternal and infant variables), ENCASEH (household variables), 2005 short census (altitude), transfers database and socioeconomic ENCEL 2003 (beneficiary.)

Table A2: P-values for the differences of means between beneficiary and non-beneficiary births given different minimum lengths of program exposure.

Minimum program exposure (months) = *k*	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**
Total number of babies born alive	0.42	0.50	0.66	0.81	0.58	0.36	0.57	0.50
Infant's gender (female=1)	0.24	0.20	0.19	0.11	**0.04**	0.07	**0.02**	**0.04**
Number of days after birth weighing took place	0.14	0.10	0.14	0.07	0.11	0.16	0.13	0.17
Maternal age	0.43	0.47	0.58	0.69	0.98	0.94	1.00	0.94
Mother smoked during pregnancy (Y/N)	0.89	0.97	0.93	0.76	0.63	0.53	0.42	0.36
Prenatal care's quality index (0-1)	0.16	0.17	0.18	0.25	0.17	0.13	0.21	0.26
Head of HH speaks indigenous language (Y/N)	0.10	**0.05**	**0.04**	**0.03**	**0.03**	**0.02**	**0.01**	**0.01**
Head of HH's education (years)	0.15	0.17	0.15	0.39	0.44	0.28	0.29	0.42
Head of HH's age	0.58	0.51	0.53	0.23	0.14	0.20	0.17	0.09
Household size	0.39	0.27	0.22	0.06	**0.02**	**0.02**	0.06	0.11
Children aged 0-5 in HH (proportion)	0.45	0.71	0.88	0.95	0.83	0.93	0.74	0.55
Children aged 6-17 in HH (proportion)	0.22	0.10	0.06	**0.03**	**0.01**	**0.02**	**0.03**	**0.02**
Economic index (0-1)	0.65	0.31	0.28	0.26	0.21	0.42	0.49	0.50
Altitude (meters)	0.84	0.75	0.75	0.70	0.67	0.64	0.53	0.46
Total N	744	744	745	745	745	745	747	747
No. of non-beneficiary births + No. of beneficiary births	184+560	190+554	197+548	210+535	220+525	229+516	240+507	246+501

**P*- value for the test: H_0: $\mu_0-\mu_1 = 0$ vs. H_a: $\mu_0-\mu_1 \neq 0$. Where 0=non-beneficiary birth, 1=beneficiary birth. P-values are shown in bold for 5% significance.
Source: Own computation using data from ENCASEH, fertility and socioeconomic rural surveys 2003, and transfers database.

Table A3: Robustness check: Regression results for singleton births using k=2 and different samples*

Variable	Sample	Quantile Regressions 20%	50%	80%	OLS
Beneficiary					
	(1)	135.2*	155.0***	206.5**	157.2***
		(77.00)	(54.52)	(83.01)	(50.61)
	(2)	144.3**	137.4***	201.0***	153.3***
		(73.44)	(51.41)	(72.16)	(48.27)
	(3)	141.0**	136.2**	201.1***	155.6***
		(70.19)	(54.23)	(71.68)	(48.00)
	(4)	79.49	114.1**	151.3*	109.0**
		(76.80)	(51.71)	(83.21)	(51.16)
Smoked					
	(1)	-458.7*	69.35	77.59	-9.786
		(276.4)	(181.2)	(191.2)	(149.5)
	(2)	-484.4*	67.06	78.47	-11.56
		(278.8)	(163.8)	(191.6)	(144.6)
	(3)	-477.1*	101.7	79.97	-13.25
		(286.0)	(161.0)	(167.9)	(144.6)
	(4)	-555.8*	22.19	70.77	-13.19
		(297.3)	(156.8)	(181.7)	(144.9)
Constant					
	(1)	2510***	3129***	3581***	3207***
		(295.8)	(269.2)	(413.5)	(258.3)
	(2)	2620***	3031***	3582***	3190***
		(284.2)	(262.1)	(394.1)	(249.6)
	(3)	2627***	3031***	3597***	3191***
		(301.9)	(262.3)	(385.7)	(249.6)
	(4)	2438***	3016***	3658***	3207***
		(300.7)	(251.1)	(410.3)	(248.7)

The total number of births (non-beneficiaries + beneficiaries) in each case is as follows: (1) 744 (184 + 560), (2) 760 (200 + 560), (3) 760 (206 + 554), and (4) 767 (176 + 591). The results in (1) were extracted from table 4. In (2) 16 out of the 24 observations that had been left out of the analysis in (1) (because of not showing up in the transfers database) have been added as non-beneficiary births. In (3) six observations which had been considered as beneficiary births in the two previous samples are treated as non-beneficiaries instead, as they have mostly missing values in the transfers database. In (4) the beneficiary status at birth is defined exclusively on the basis of enrolment. In this case, k=3 was chosen (see page 15 for clarification.) Robust standard errors (OLS) and bootstrapped standard errors (1000 replications) for the quantile regressions in parentheses. Asterisks denote the significance level (double sided) *: 10%, **: 5%, ***: 1%.

Table A4: Regression results for singleton birth-weights (*k*=2) including households with birth-weight information on more than one member*.

	Quantile Regressions			OLS
	20%	50%	80%	
beneficiary	102.6	163.7***	184.0**	161.9***
	(73.26)	(50.00)	(81.07)	(50.41)
add_mem_benef	-252.4	-293.9	-617.9*	-432.8*
	(420.1)	(275.4)	(315.2)	(231.9)
add_member	145.0	186.4	451.1	243.2
	(392.5)	(255.7)	(306.5)	(214.7)
firstbir	-117.8	-114.9	-144.4	-176.8*
	(165.2)	(106.5)	(107.3)	(94.21)
second	-80.18	-99.36	-42.56	-98.34
	(105.0)	(105.9)	(123.9)	(82.74)
third	-61.85	-30.01	99.83	-20.11
	(94.28)	(76.77)	(111.4)	(71.67)
female	-173.1***	-173.2***	-188.5***	-170.7***
	(62.87)	(50.39)	(64.08)	(43.75)
daysafwe	5.104	6.277***	6.978*	5.949**
	(3.329)	(2.415)	(3.759)	(2.545)
young	-58.91	-191.7*	-414.8***	-239.1**
	(185.9)	(99.87)	(107.0)	(99.02)
old	-73.37	-31.43	-30.26	-0.0535
	(87.87)	(66.96)	(88.84)	(58.17)
smoked	-556.4**	140.1	34.31	-46.35
	(277.5)	(168.3)	(148.3)	(143.3)
qualindex	301.9*	66.60	31.70	51.56
	(165.2)	(147.1)	(191.0)	(126.5)
indig	-154.3*	-164.0**	-106.2	-171.9***
	(90.39)	(65.96)	(81.94)	(62.00)
edu6head	-140.8*	-16.66	-42.88	-94.39
	(84.38)	(61.47)	(80.99)	(59.88)
edplushead	21.69	64.67	89.83	9.395
	(82.90)	(72.98)	(101.5)	(68.07)
agehead	3.580	3.465	3.075	4.441*
	(4.258)	(3.188)	(3.401)	(2.549)
famsize	19.04	-2.562	-24.56*	-14.58
	(21.00)	(13.17)	(14.74)	(11.81)
propage5	184.5	77.99	118.7	147.0
	(345.5)	(299.7)	(371.9)	(278.2)
prage6_17	76.10	428.0	627.6*	361.4
	(305.5)	(265.0)	(322.0)	(229.5)
econindex	-179.4	-197.2	-267.9*	-184.7
	(164.5)	(140.5)	(161.2)	(112.7)
altitude	0.00518	-0.0566	-0.0273	-0.0251
	(0.0449)	(0.0401)	(0.0427)	(0.0345)
Constant	2406***	3076***	3564***	3154***
	(306.2)	(258.0)	(368.3)	(246.0)
N	805	805	805	805

add_member* is a binary variable which takes the value of 1 for households where more than a member gave birth between 1997 and 2003; its interaction with the beneficiary variable generates *add_mem_benef*. Robust standard errors (OLS) and bootstrapped standard errors (1000 replications) for the quantile regressions in parentheses. Asterisks denote the significance level (double sided) *: 10%, **:5%, *: 1%

Table A5: Regression results for singleton birthweights using the number of months in the program at birth* (minimum program exposure is two months).

	Quantile Regressions			OLS
	20%	50%	80%	
treatint	2.481*	1.726	3.667**	2.589**
	(1.386)	(1.298)	(1.549)	(1.023)
firstbir	35.13	-213.0	-263.4**	-187.9*
	(159.6)	(130.8)	(131.7)	(103.7)
second	-155.9	-147.9	-71.88	-120.5
	(112.4)	(113.2)	(125.9)	(87.79)
third	-81.87	-68.11	58.85	-39.85
	(93.78)	(81.37)	(105.4)	(75.64)
female	-185.2***	-187.6***	-178.4**	-170.6***
	(65.09)	(53.34)	(69.47)	(45.58)
daysafwe	5.389	7.013***	4.850	6.003**
	(3.916)	(2.715)	(3.657)	(2.695)
young	-120.9	-132.1	-357.2**	-243.2**
	(215.0)	(114.7)	(148.0)	(115.8)
old	-21.70	23.07	1.104	42.46
	(86.59)	(75.02)	(93.33)	(60.11)
smoked	-464.7*	74.33	-15.18	-12.88
	(281.3)	(174.4)	(188.7)	(149.4)
qualindex	335.7**	106.1	52.66	90.52
	(167.7)	(159.4)	(213.9)	(133.5)
indig	-156.5	-114.0	-125.7	-143.9**
	(95.63)	(70.88)	(78.53)	(62.17)
edu6head	-189.3**	-1.536	-70.21	-107.9*
	(88.62)	(72.58)	(92.84)	(60.82)
edplushead	-15.77	76.40	60.27	0.814
	(81.38)	(86.55)	(112.7)	(69.13)
agehead	4.353	1.420	3.652	3.931
	(4.471)	(3.651)	(3.852)	(2.726)
famsize	-2.997	3.891	-25.39	-23.97*
	(22.37)	(14.87)	(17.74)	(12.90)
propage5	191.0	-200.6	-75.32	86.44
	(341.1)	(331.0)	(401.0)	(298.6)
prage6_17	115.6	175.4	413.8	313.2
	(304.4)	(293.0)	(353.0)	(250.9)
econindex	-63.85	-206.6	-206.7	-124.7
	(166.3)	(152.2)	(162.4)	(112.0)
altitude	0.0135	-0.0436	-0.0341	-0.0253
	(0.0430)	(0.0425)	(0.0453)	(0.0355)
Constant	2466***	3269***	3728***	3257***
	(300.7)	(299.3)	(392.3)	(264.8)
N	744	744	744	744

*This specification has been labelled "program months" in table 5. Robust standard errors (OLS) and bootstrapped standard errors (1000 replications) for the quantile regressions in parentheses. Asterisks denote the significance level (double sided) *: 10%, **: 5%, ***: 1%

Table A6: Means' comparison of individual & household characteristics for non-beneficiary and beneficiary births after matching (minimum program exposure is two months.)

Two-month exposure	Non-beneficiaries		Beneficiaries		Means' Diff.	*P*-value*
	Mean	SD	Mean	SD		
Maternal and Infant Characteristics						
Total number of babies born alive	4.53	2.33	4.76	2.43	-0.22	0.23
Infant's gender (female=1)	0.45	0.50	0.47	0.50	-0.01	0.71
Number of days after birth when baby was weighted	3.93	9.95	2.94	8.21	0.99	0.19
Maternal age	29.63	6.35	30.20	6.23	-0.57	0.25
Mother smoked during pregnancy (Y/N)	0.04	0.20	0.04	0.20	0.00	0.95
Prenatal care's quality index (0-1)	0.91	0.24	0.94	0.17	-0.03	0.06
HH socieco. & demograp. Charact. at Baseline						
Head of HH speaks indigenous language (Y/N)	0.28	0.45	0.34	0.47	-0.06	0.12
Head of household's education (years)	3.89	2.75	3.51	2.56	0.38	0.08
Head of household's age	34.29	9.92	34.29	11.01	0.01	0.99
Household size	6.06	2.07	5.89	2.28	0.16	0.33
Children aged 0-5 in household (proportion)	0.29	0.17	0.30	0.17	0.00	0.82
Children aged 6-17 in household (proportion)	0.30	0.21	0.27	0.21	0.03	0.12
Economic index (0-1)	0.30	0.23	0.29	0.22	0.01	0.52
Locality Characteristics						
Altitude (meters)	1222	842	1293	814	-70.98	0.28
N	225		560		Total N	785

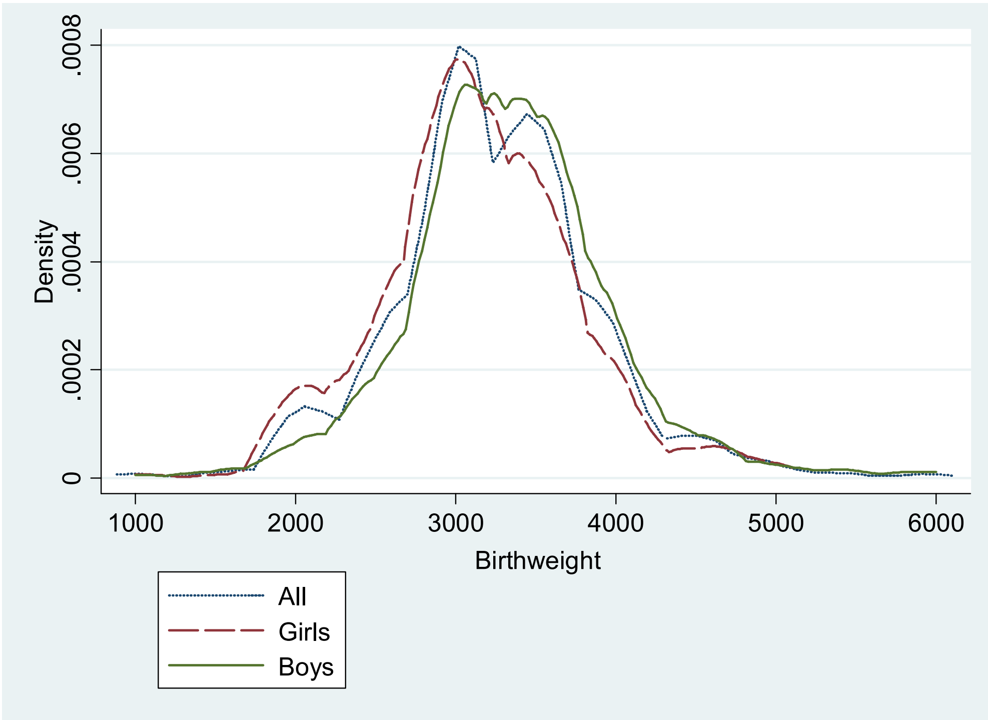
**P*- value for the test: H_0: μ_0-μ_1 = 0 vs. H_a: μ_0-$\mu_1 \neq 0$. Where 0=non-beneficiary birth, 1=beneficiary birth. Unequal variances were assumed. *Source: Own computation using data from ENCASEH, fertility and socioeconomic rural surveys 2003 (including the 1997retrospective module), and transfers database.*

Table A7: Regression results for birthweight after matching (minimum program exposure is two months).

	Quantile Regressions			OLS
	20%	50%	80%	
beneficiary	109.8	120.2**	184.3**	120.4**
	(71.38)	(47.92)	(71.97)	(46.85)
firstbir	-8.673	-126.7	-126.6	-158.2
	(149.0)	(115.2)	(120.6)	(99.94)
second	-79.03	-124.8	5.239	-112.8
	(105.7)	(114.3)	(127.1)	(81.78)
third	-121.6	-59.38	-15.97	-69.32
	(96.52)	(70.67)	(100.8)	(69.14)
female	-170.3***	-148.1***	-112.9*	-144.9***
	(64.05)	(50.00)	(66.89)	(43.75)
daysafwe	6.644*	5.229**	4.183	5.738**
	(3.577)	(2.306)	(3.732)	(2.566)
young	-55.44	-131.7	-308.6**	-192.3*
	(211.4)	(114.6)	(151.9)	(114.3)
old	9.003	25.01	26.26	62.00
	(88.59)	(69.33)	(95.62)	(57.58)
smoked	-543.4*	95.56	124.4	-6.182
	(289.7)	(164.1)	(164.9)	(144.5)
qualindex	275.1	237.0**	-113.4	65.59
	(168.4)	(119.4)	(252.5)	(128.4)
indig	-91.41	-185.6***	-128.1*	-143.1**
	(88.97)	(61.85)	(75.98)	(57.93)
edu6head	-148.9*	-45.57	-56.08	-107.5*
	(90.17)	(64.58)	(86.07)	(59.17)
edplushead	17.94	46.39	110.1	12.36
	(84.56)	(78.39)	(104.0)	(67.09)
agehead	1.314	1.825	0.470	2.397
	(4.242)	(3.298)	(3.695)	(2.542)
famsize	-0.289	-10.14	-42.36**	-20.96*
	(22.53)	(12.95)	(16.41)	(12.00)
propage5	71.71	-53.22	7.125	-22.82
	(337.5)	(244.2)	(383.8)	(252.1)
prage6_17	22.88	387.6*	605.9*	220.7
	(293.2)	(229.5)	(322.0)	(211.5)
econindex	-35.16	-83.03	-97.09	-98.62
	(164.8)	(134.1)	(153.5)	(106.8)
altitude	0.00441	-0.0773*	-0.0214	-0.0249
	(0.0452)	(0.0422)	(0.0414)	(0.0331)
Constant	2610***	3096***	3847***	3326***
	(297.8)	(224.4)	(402.0)	(229.1)
N	785	785	785	785

Robust standard errors (OLS) and bootstrapped standard errors (1000 replications) for the quantile regressions in parentheses. Asterisks denote the significance level (double sided) *: 10%, **: 5%, ***: 1%

Figure A1: Probability density functions for birthweight (in grams) for different groups.



All densities use an Epanechnikov kernel function and the default "optimal" width (115) calculated in STATA.

Table A8: Summary regression results for the main coefficients for all specifications (minimum exposure to *Oportunidades* is two months).

Variable Model	Quantile Regressions			OLS
	20%	50%	80%	
Program impact				
Baseline	135.2*	155.0***	206.5**	157.2***
	(77.00)	(54.52)	(83.01)	(50.61)
Baseline (large N)	102.6	163.7***	184.0**	161.9***
	(73.26)	(50.00)	(81.07)	(50.41)
Program months	81.99*	57.04	121.19**	85.56**
	(45.81)	(42.90)	(51.19)	(33.81)
Baseline (matched)	109.8	120.2**	184.3**	120.4**
	(71.38)	(47.92)	(71.97)	(46.85)
Baseline (UCR)	141.5*	141.6**	174.4**	157.2***
	(74.22)	(62.87)	(77.27)	(50.61)
Smoked				
Baseline	-458.7*	69.35	77.59	-9.786
	(276.4)	(181.2)	(191.2)	(149.5)
Baseline (large N)	-556.4**	140.1	34.31	-46.35
	(277.5)	(168.3)	(148.3)	(143.3)
Program months	-464.7*	74.33	-15.18	-12.88
	(281.3)	(174.4)	(188.7)	(149.4)
Baseline (matched)	-543.4*	95.56	124.4	-6.182
	(289.7)	(164.1)	(164.9)	(144.5)
Baseline (UCR)	-275.5	49.93	183.9	-9.786
	(193.4)	(141.8)	(183.8)	(149.5)
Constant				
Baseline	2510***	3129***	3581***	3207***
	(295.8)	(269.2)	(413.5)	(258.3)
Baseline (large N)	2406***	3076***	3564***	3154***
	(306.2)	(258.0)	(368.3)	(246.0)
Program months	2466***	3269***	3728***	3257***
	(300.7)	(299.3)	(392.3)	(264.8)
Baseline (matched)	2610***	3096***	3847***	3326***
	(297.8)	(224.4)	(402.0)	(229.1)
Baseline (UQR)	2365***	3090***	3827***	3207***
	(317.6)	(259.5)	(340.6)	(258.3)

The sample size in each specification is 744, except in "large N" and "matched", where it is respectively 805 and 785. Robust standard errors (OLS) and bootstrapped standard errors for the quantile regressions (1000 repetitions for the conditional quantiles and 800 for the unconditional ones) in parentheses. In the UQR, the "optimal" bandwidth calculated by the program has been used. Alternative bandwidths (at 95 and 135) were used yielding no particularly different results. Asterisks denote the significance level (double sided) *: 10%, **: 5%, ***: 1%

Table A9: Summary regression results for the program impact (minimum exposure to *Oportunidades* is two months).

Specification		(1)	(2)	(3)	(4)	(5)
Quantile Regressions	20%	135.2*	102.6	81.99*	109.8	141.5*
		(77.00)	(73.26)	(45.81)	(71.38)	(74.22)
	50%	155.0***	163.7***	57.04	120.2**	141.6**
		(54.52)	(50)	(42.9)	(47.92)	(62.87)
	80%	206.5**	184.0**	121.19**	184.3**	174.4**
		(83.01)	(81.07)	(51.19)	(71.97)	(77.27)
OLS		157.2***	161.9***	85.56**	120.4**	157.2***
		(50.61)	(50.41)	(33.81)	(46.85)	(50.61)
N		744	805	744	785	744

All regressions control for the maternal and infant characteristics, household socioeconomic and demographic characteristics at baseline, and locality characteristics outlined in table A1 in the appendix. (1) Stands for the baseline specification (i.e.: that in equation (1), with *k*=2). (2) Stands for the baseline specification including households in which more than one member gave birth between 1997 and 2003. (3) Substitutes the number of months between program enrolment and the baby's date of birth for the binary variable *beneficiary* in the baseline specification. (4) Stands for the baseline model after matching. (5) Stands for the baseline model estimated using *unconditional quantile regressions* (UCR*)*. Robust standard errors for the OLS regressions and bootstrapped standard errors for the quantile regressions (1000 repetitions for the conditional quantiles and 800 for the unconditional ones) in parentheses. In the UQR, the "optimal" bandwidth calculated by the program has been used. Alternative bandwidths were tried yielding no particularly different results. Asterisks denote the significance level (double sided) *: 10%, **: 5%, ***: 1%