Long-term impacts on education of a cash transfer during early-life

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July 11, 2023

Abstract

We evaluate long-term effects of receiving the Uruguayan *Plan de Atención Nacional a la Emergencia Social* (PANES), a large unconditional cash transfer program on outcomes of young and unborn children. We use a rich dataset that matches program administrative data to vital natality data and educational records 8 to 12 years after the beginning of the program. Overall, we find small and barely significant effects on educational attainment and delay. Among children that were exposed to the program during the early childhood (between ages zero to five), the results show significant beneficial effects for those with low birth weight.

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1 Introduction

A growing literature documents that prenatal and early-childhood experiences can have long-lasting impacts on later-life economic outcomes, human capital, health and well-being (Almond et al., 2018). In recent years, there has been a large increase of welfare programs that aim to improve conditions in early-life, especially in developing countries. While for policy-makers it is particularly interesting to know how effective these policies are, we are still at the beginning stages of learning what type of intervention matters for long-term outcomes.

In this paper, we evaluate whether being exposed to a poverty-alleviation program during early-life has an impact on long-term educational outcomes. We focus on the Uruguayan *Plan de Atención Nacional a la Emergencia Social* (PANES), a cash transfer program that was implemented between 2005 and 2007 and that targeted the poorest 10 percent of households in the country. The program was introduced after a severe economic crisis that hit Uruguay in 2002. One special feature is that the amount of the transfer represented approximately 45% of the average household income among its target population. Although participation was announced to be conditional on school attendance for all children under age 14 and on regular health checkups for pregnant women and all children, the conditions were never enforced, so the program was unconditional *de facto*.

Evaluating long-term effects of interventions during early childhood has two main challenges. The first challenge is to find a credible identification strategy to evaluate the intervention. In this paper, we exploit the way in which households were assigned to the PANES program. Program assignment was determined on the basis of a baseline predicted poverty score: households whose score was above a certain threshold were eligible to receive the transfer. This eligibility rule generates a discontinuity that we exploit using a regression discontinuity design. We compare educational outcomes of children belonging to households just above and just below the eligibility cutoff. We estimate impacts on three educational outcomes: highest grade attained, delay in educational attainment and dropout.

The second challenge in estimating long-term effects of early-childhood interventions is to find datasets that map early-life environments with later-life outcomes. We make use of a rich dataset that we constructed for this project that links long-term educational outcomes to early life experiences. Our dataset contains educational information (enrollment and grade) for the years 2013-2017 of eligible and ineligible children born between 2003 and 2007.

We separately estimate the impact of the PANES program for cohorts that were exposed at different stages of early childhood. We split our analysis according to the age of the child at the onset of the program. In particular, we focus on (i) children that were between 0 and 2 years of age when the program started (born between January 2003 and March 2005), and on (ii) children that were born during the program period. Therefore, given that the program ran between April 2005 and December 2007, our sample includes children that were exposed to the program between the ages 0 and 5 (those in group (i)) and children that were exposed to the program while in-utero and up to maximum two years and eight months (those in group (ii)).

Separating the analysis into children that were exposed to cash transfers since the in-utero period and children that were exposed later in life (but still in early childhood) enables us to look at differential effects among these subgroups. On the one hand, a growing literature suggests that investments that occur during the prenatal period may potentially be more cost-effective than postnatal interventions (Doyle et al., 2009). In this sense, we should expect that children that received transfers while in-utero and after birth benefit more from the program than those that received transfers only after birth. On the other hand, those that were exposed to the transfer later in life were born during an economic crisis and had more risk: the likelihood of being born with low birth weight was 0.083 in the pre-program period, while 0.075 in the program period. These adversities could result in worse early child development outcomes. In this sense, additional liquidity may lead to higher effects for these children than for those born in families that started receiving the transfer during a better economic landscape. Identifying which group of children benefited more from an intervention such as PANES might help focus cash transfer policies on those children that need them most.

Our results show that in the full sample the PANES program produced a small and barely significant improvement in educational attainment. The effect is entirely driven by children that were born before the onset of PANES and, hence, exposed to the program during early-childhood. Within this subsample, we find that children from eligible households have a slightly higher educational attainment and a lower incidence of delay than ineligible children. In addition, we find that PANES had no impact on educational dropout in the years of observation. Taken together, these findings suggest that the mild effect of PANES on educational attainment works through retainment and not through dropout.

Following Heckman's model of dynamic complementarity, one would expect that children that received transfers since the in-utero period should have stronger effects on education than those that received them only after birth. However, our findings show the opposite, the effect of the PANES program on education is driven by children exposed to the program during early childhood (and after birth). Because this group was born in a worse economic environment, our interpretation is that the transfers have a stronger effect on education on children that are born with more risk. We further explore this issue by estimating heterogeneous effects by low birth weight status among children that were born in the pre-program period. We find that the effects of PANES on educational attainment are stronger among children that are born with low birth weight.

We find no effects of PANES on long-term educational results of children that were in their mothers' womb during the program period. Given previous evidence showing that the PANES program improved health at birth as measured by birth weight (Amarante et al., 2016)[1] and the importance of health at birth for later educational outcomes,[2] this finding seems surprising. However, when we estimate the effect of PANES on health at birth, we find no significant effects. Our findings differ from those in Amarante et al. (2016) because we use a different identification strategy and a different dataset.[3]

Cash transfers were established with the aim of alleviating household financial restrictions. Some variations of these programs impose conditionalities on school attendance to promote human capital accumulation and break the intergenerational transmission of poverty. Such is the case of the well-known Mexican's Progresa, one of the earliest conditional cash transfer programs. Progresa began in 1997 and consisted of regular cash transfers to women conditional on human capital investments, including visits to healthcare providers for young children, and school enrollment and attendance for school-age children. The program increased schooling enrollment and attendance after its first 18-month randomized evaluation (Parker and Todd, 2017). Due in large part to Progresa's results, conditional cash transfers have become common in Latin America and have spread to other parts of the world. A large body of evidence has found that these interventions have positive effects on schooling while impacts on employment and earnings are mixed (see Millán et al. (2019) for a review).

Unconditional cash transfers are not tied to any particular behavior and thus provide cash payments to everyone in the eligible target population. The number of studies assessing unconditional cash transfers on schooling is substantially smaller than those analyzing transfers that impose conditionalities, but growing. Unconditional cash transfers have been shown

[1]The authors find that the PANES program led to a drop in the incidence of low birth weight that ranges between 19 and 25 percent and that fertility was not affected by program participation. The result could be considered a "first stage" effect for our long-term educational outcomes, although the program may affect long-term outcomes also through other mechanisms (Almond et al., 2018)

[2]Birth weight has emerged as the main focus of health policy, both in the United States and elsewhere, and has been used to evaluate the effectiveness of social policy (Almond et al., 2005). Research has shown that birth weight can affect neonatal outcomes and long-run health outcomes (Black et al., 2007; Oreopoulos et al., 2008), and even birth weight of the next generation (Royer, 2009; Black et al., 2007). Birth weight can also affect non-health outcomes such as schooling, wages, IQ and test scores (Behrman and Rosenzweig, 2004; Royer, 2009; Oreopoulos et al., 2008; Rosenzweig and Zhang, 2013; Black et al., 2007; Torche and Echevarría, 2011).

[3]Amarante et al. (2016) use a localized difference in differences strategy while we use a regression discontinuity design. When Amarante et al. (2016) use a regression discontinuity design, they do not find significant impacts of PANES on low birth weight. When we perform a localized difference in differences strategy, we do not find robust results showing that the program improved health at birth. We discuss this issue further in Section 5.3.

to increase enrollment in education in the short-run (Baird et al., 2013), as well as household consumption (Haushofer and Shapiro, 2016), but their effectiveness in improving the outcomes associated with conditions is inferior relative to conditional cash transfers (Baird et al., 2011).

Longer-term analyses are especially important for cash transfers since these programs aim to reduce future poverty by augmenting human-capital levels of children and youth from poor families. However, while there is sufficient evidence of the impact of cash transfers interventions in the short run (Fiszbein and Schady, 2009), the evidence on long-run effects is sparse (Millán et al., 2019). In the case of randomized evaluations, for example, high rates of migration make following-up samples expensive and complicated. Millán et al. (2019) claim that the measurement of long-term impacts using rigorous identification strategies should be high on the research agenda (Millán et al., 2019).

Overall, the sparse evidence on long-term effects of unconditional cash transfers shows no or little effects on education. Two examples for the African context are Haushofer and Shapiro (2018) and Blattman et al. (2020). The former evaluates an unconditional cash transfer in Kenya three years after the beginning of the program and the latter evaluates the effectiveness of cash grants in Uganda 9 years after the implementation of the program.

In Uruguay, the PANES program has been evaluated on a range of short-term outcomes such as school attendance, labor supply, political support and birth weight. Overall, studies find that the program had no impact on child labor or school attendance of children aged 14 to 17 (Amarante et al., 2013), decreased formal labor supply (Amarante et al., 2011), increased political support for the current government relative to the previous government (Manacorda et al., 2011) and improved health at birth outcomes (Amarante et al., 2016).

Our paper contributes to the literature in four ways. First, we contribute to a growing body of work on the medium to long-term effects of unconditional cash transfer programs in developing countries. We measure educational outcomes 8 to 12 years after exposure, a longer period than that in most other studies. Moreover, we study the case of Uruguay, a middle-income country in Latin America whose population and economic setting is very different than that in Africa. Second, we focus on the effects of the program since the in-utero period and up to 5 years of age. The evidence base for exposure in early childhood is more limited than for exposure during school-going ages (Millán et al., 2019). Third, a novel angle of the paper rests on the comparison between cohorts of children born before or during the program. Recent evidence shows positive effects of cash transfers following the birth of a child on earnings and education (Barr et al., 2022). We consider the long-term effect of cash transfers on education even from an earlier stage: the in-utero period. Fourth, beyond the cash transfer literature, we contribute to the literature that relates resources in-utero to educational outcomes later in life. While most other studies have focused on long-term effects of negative shocks such as famines, disease and radiation (see Almond et al. (2018) for a recent review), we focus on a policy that implies a positive treatment.

The remainder of this paper is structured as follows. Section 2 describes the PANES program, Sections 3 and 4 describe the data and empirical framework respectively. Section 5 reports results of the effect of the PANES program on educational outcomes and low birth weight. Finally, Section 6 provides a discussion of the findings.

2 The PANES program

The *Plan de Atención Nacional a la Emergencia Social* (PANES) was a temporary social assistance program that ran between April 2005 and December 2007, in Uruguay, a middle-income country in Latin America.[4] The program targeted the poorest households in the country. The PANES was designed as an emergency plan to alleviate material hardship from a severe economic crisis that hit Uruguay in 2002 and was among the flagship policies of the center-left government that took office in March 2005. The Ministry for Social Development (*Ministerio de Desarrollo Social*) was created to be in charge of the implementation of the program.

Program eligibility was based on families' scores on a poverty index. All applicant households were visited by personnel of the Ministry of Social Development and completed a detailed baseline survey which allowed program officials to compute the score. The score depended on many household socioeconomic characteristics and was based on a probit model of the likelihood of being below a per capita income level using a highly saturated function of household variables (Amarante et al., 2005). The estimation of the underlying model was performed using the 2003 and 2004 National Household Survey (*Encuesta Continua de Hogares*) and the resulting coefficient estimates were used to predict a score value for each applicant using PANES baseline survey data. Appendix Tables A1 and A2 provide further information on the variables used to predict the poverty score.[5] The variables considered, the weights attached to the observed covariates and the eligibility thresholds were allowed to vary slightly across different geographic regions. Applicants were not aware of the variables that entered into the score, nor the weights attached to them, or the eligibility criterion,

[4] In 2003, Uruguay had a population of around 3.3 million people and per capita GDP was about 8000 USD. The country offers free public education from elementary school to university. There are 14 years of mandatory schooling: 2 in elementary school, 6 in primary school and 6 in secondary school. While primary education is universal, secondary school completion rates pose a big challenge for the Uruguayan government.

[5] One of the variables used to predict the poverty score was the household's value in a wealth index. The variables included in the latter measure are listed in Appendix Table A2.

easing concerns about manipulation of the score.

Rather than using actual reported income, the score was estimated using a wide range of socioeconomic variables. The reason for this is that the program's target population often worked in the informal sector making it difficult to verify self-reported income. By using indirect measures of income the possibility of strategic misreporting was minimized.

Around 188,671 households (with around 700,000 individuals) sent applications. After the interviewing process, households were ordered according to their level of deprivation based on their predicted poverty score. Those households whose score was above a predetermined level were assigned to the program. Around 54% of applicant households became beneficiaries, representing nearly 10% of households in the country. Independently of their characteristics, eligible households received a monthly cash transfer that originally amounted to $1360 Uruguayan pesos (US$102 adjusted by PPP). This amount was adjusted for inflation on a quarterly basis. The transfer corresponded to approximately 45 percent of the average household income among the poorest 10 percent of households in Uruguay.[6] [7]

The condition to keep receiving the payment was that household income (of all sources) remained below a specific level per capita. In practice, only verifiable sources of income were taken into account. Successive checks were carried out by the social security administration to enforce this condition and, because of this, some households stopped receiving the transfer before the end of the program.[8] There were no other formal conditionalities (such as health checks for children and pregnant women or school attendance for children) until mid 2007, and even then, conditionalities were never enforced.

The program included several components. The main element of the program was the monthly cash transfer (*ingreso ciudadano*, "citizen income"). Midway through the program, an electronic food card (*tarjeta alimentaria*) was introduced and households with children or pregnant women were entitled to receive it on top of the cash transfer. The food card operated through an electronic debit card and its value represented between 22% and 59%

[6]This number was calculated using the Uruguayan Continuous Household Survey of 2004. If we use the wave of 2005, we obtain very similar results. Income is substantially lower outside Montevideo, the capital city of Uruguay, which explains why 70% of applicants live outside the capital city. The fixed amount of $1360 Uruguayan pesos represent slightly more than 50% of monthly average household income among the poorest 10 percent households that do not live in Montevideo and slightly less than 40% of monthly average household income among the poorest 10 percent households that live in Montevideo. With respect to the whole income distribution of the country, the transfer represents a 9% of the monthly household average income.

[7]Our calculations are line with Amarante and Vigorito (2010) and Amarante et al. (2011) who state that the monthly amount of the transfer corresponded to half (50%) of the pre-program household self-reported income. In Amarante et al. (2016), the authors state that the amount of the transfer represented a quarter of self-reported income (25%).

[8]Households that became non-eligible before the end of the program are still considered within the treatment group.

of the value of the income transfer depending on household size and demographic structure.

On an annual basis, the program's cost was 0.41% of GDP. The program ended in December 2007 and the target population, eligibility rules and assistance levels changed when a new system of family allowances and a health care reform (*Plan de Equidad*) was launched in January 2008. Households did not need to reapply for the new program. The eligibility to the *Plan de Equidad* was based on a new score that was estimated for all original PANES applicant households using the same baseline characteristics registered in 2005 but with a new formula. The threshold for program eligibility changed with respect to PANES: it became less restrictive and expanded the beneficiaries' base.[9] The government informed households about the ending of the PANES and the start of the new program via mail and eligible households received a written formal communication.

3 Data

We use a rich dataset that links administrative records from three governmental sources. All sources contain information at the individual level and we use de-identified identity numbers for matching these three sources. In this section we describe the data sources used and the descriptive statistics.

3.1 Data sources

Data from the Ministry of Social Development

Our primary source is the administrative records of the initial baseline survey visit for both successful and unsuccessful female applicants in PANES. The Ministry of Social Development (*Ministerio de Desarrollo Social*, MIDES) shared with us the responses to the comprehensive questionnaire applied by MIDES agents during the visits. Some households submitted more than one application to the program but we keep information only from the first visit to ease concerns about strategic behaviors to gain eligibility. The key variables that we use from this source are the household's exact predicted poverty score and an indicator for approval in PANES. We also use information on the household's sociodemographic characteristics, housing conditions and durable asset ownership.

[9]Members from eligible and ineligible households in PANES became eligible for the new program. In 79% of applicant households to PANES, at least one household member became eligible in *Plan de Equidad*. Further in the paper we show that we do not find significant differences in the probability that at least one household member received the *Plan de Equidad* when considering households that are close to the PANES eligibility threshold (See Table 3). It is important to note, however, that in this paper we estimate the marginal effect of receiving the PANES program on top of receiving future cash transfers from *Plan de Equidad*.

Birth data

We combine information from PANES administrative records with all registered live births in Uruguay coming from birth certificates (*Certificado de Nacido Vivo*) in the period 2003-2007. The latter are registered by the Statistical Office of the Ministry of Health (*Ministerio de Salud Pública*). Birth certificates have unique identification numbers for mothers and we used these to match them with females in PANES applicant households. The identity numbers of children, however, were not available in birth certificates. To obtain this information, we used additional records of MIDES that contain identification numbers of mothers and children that receive any social program. We matched the latter dataset with PANES records using the mother's identification number and the date of birth. For multiple births of the same gender, it was not possible to disentangle which was the identification number that corresponded to each child. Because this information was key to link birth data with education data, we had to drop observations from multiple births (1% of the sample).[10] The vital statistics natality data has information of health at birth, the reproductive history of the mother, parental characteristics and prenatal health care utilization.

Education data

Finally, we use children's identification numbers to obtain information of enrollment by year and grade from administrative data registered by the Statistical Office of the National Administration of Public Education (*Administración Nacional de Educación Pública*). We have information for the years 2013 to 2017, corresponding to 8 to 12 years after the beginning of the PANES program. For each year, we know the grade in which the child was enrolled but not whether the grade was completed in that particular year. With this data, we constructed three outcome variables for our analysis: highest grade attained, delay and dropout in education. Highest grade attained corresponds to the grade attained by the child in 2017, the last year for which we have information, and it ranges from 1 to 10 being 1 preschool and 10 the last year of middle school. If the observation of the child is missing in 2017, we take the highest grade attained by the child in the period we observe her.[11] Delay is measured with an indicator that takes value 1 if the child's highest grade attained in 2017 is lower than

[10] Within the program period, multiple births are equally likely for PANES recipients as for controls, easing concerns of selection on an outcome. Infants born in multiple births have, on average, lower birth weights than those born in single order births, so our results may be sensible to the inclusion of twins, triplets and higher order births.

[11] We acknowledge that we do not measure completed education and that highest grade attained is a truncated variable. We have performed our estimations using an alternative outcome variable that measures the likelihood of enrolling in seventh grade (first year of secondary school) with no delay which excludes the possibility of truncation for younger students.[12] The results are qualitatively equivalent to the ones we show in our main tables (see Table A7).

the one determined by her year and month of birth and a regular track.[13] Appendix Table A3 shows the corresponding grade that a child should have attained in 2017 according to its year and month of birth. Dropout is an indicator that takes the value 1 if the child was not enrolled in education for two or more years during the period of observation.

The three outcomes we consider capture different elements of students' educational career. Highest grade attained shows overall educational attainment of the child. Delay adds to the latter by considering also information of the year and month of birth of the child. There are two possible explanations to why a child may be enrolled at a lower grade than the one we would expect her to be based on her age and a regular track: (i) the child repeated a grade, or (ii) the child did not enroll in school during some years.[14] We explore the possibility of explanation (ii) using a variable that indicates whether the child dropped out from school for two or more years in the period we observe her.

3.2 Descriptive statistics

Overall, we have information of 49,062 mothers and 59,128 children. Almost half of the children in our sample (49%) were born during the program period. Table 1 presents descriptive statistics of our outcome variables and selected covariates for children born in the pre-program period (January 2003-March 2005) and children born in the program period (April 2005-December 2007).[15] There is a difference in educational outcomes measured 8 to 12 years after exposure to the program between eligible and ineligible groups. For example, taking into account highest grade attained, children born in non-eligible households in the pre-program period attained 8 years of education while eligible children born in the same period attained 7.6. Children born during the program period attain lower levels of education than children born in the pre-program period because they are younger at the time we observe them. Non-eligible children born after the beginning of the program attain on average 5.8 years of education while eligible children attain 5.7.

There is also a difference in the incidence of low birth weight between eligible and ineligible

[13] In Uruguay, the requirement to enter the public education system is to have the age corresponding to the level before April 30 of the school year. That causes most children (2/3) to reach the age following the level during the school year and that 1/3 of the children do it the other year.

[14] A third explanation could be that the parents delayed the enrollment of the child at the first grade of education. We are not able to capture this as a separate outcome because we do not observe the full educational trajectory of the child and therefore we do not know in which year they entered school. In our setting, having parents that enroll children at a higher cohort than the one they should enter could be problematic in terms of our outcome measures because if these kids repeat a grade we would still consider them as non-delayed. Even though age cutoffs to enter preschool are not strictly enforced in Uruguay, the children that enroll early are minority, and it is more common to see children enrolled late instead.

[15] We are not assessing the balancing properties of the sample in this table. We do so in Table 3 further in the text.

households. In the pre-program period, 8.7% of eligible children were born with low birth weight while among ineligible children the incidence was 7.9%. During the program period, the gradient in low birth weight is less pronounced (7.7% and 7.4% for eligible and ineligible households respectively).

Table 1: Descriptive statistics of outcome variables and selected covariates

	Eligible households		Non eligible households		Difference	
	N	Mean	N	Mean	Coefficient	s.e.
Panel A: Born in pre-program period						
Child's highest grade attained	22157	7.758	6751	7.975	−0.217***	(0.014)
Delay	22157	0.602	6,751	0.482	0.120***	(0.007)
Dropout	22157	0.025	6751	0.031	−0.006**	(0.002)
Mother's number of previous pregnancies	22157	3.381	6751	2.362	1.019***	(0.030)
Mother's age at birth	21778	25.432	6667	24.433	0.998***	(0.093)
Child's birth weight (BW) in grams	22000	3175.765	6696	3199.972	−24.206***	(7.217)
Child has low birth weight (=1 if BW<2500 grams)	22000	0.087	6696	0.079	0.008**	(0.004)
Gestational week of birth occurrence	21512	38.637	6581	38.647	−0.010	(0.025)
Child was born premature (=1 if gestational weeks<37)	21512	0.082	6,581	0.081	0.001	(0.004)
Child's APGAR score 1 minute	21923	8.535	6709	8.550	−0.015	(0.014)
Child's APGAR score 5 minute	21929	9.642	6708	9.649	−0.007	(0.011)
Mother's number of prenatal controls	21940	6.560	6678	7.560	−0.961***	(0.046)
Panel B: Born during program period						
Child's highest grade attained	22221	5.717	7999	5.823	−0.106***	(0.014)
Delay	22221	0.382	7999	0.284	0.097***	(0.006)
Dropout	22221	0.020	7999	0.032	−0.012***	(0.002)
Mother's number of previous pregnancies	22221	3.396	7999	2.422	0.974***	(0.028)
Mother's age at birth	21772	25.124	7873	24.675	0.449***	(0.088)
Child's birth weight (BW) in grams	22012	3214.934	7925	3225.874	−10.940	(6.857)
Child has low birth weight (=1 if BW<2500 grams)	22012	0.077	7925	0.074	0.003	(0.003)
Gestational week of birth occurrence	21353	38.648	7654	38.664	−0.015	(0.023)
Child was born premature (=1 if gestational weeks<37)	21353	0.079	7654	0.078	0.001	(0.004)
Child's APGAR score 1 minute	22005	8.538	7925	8.516	0.021*	(0.013)
Child's APGAR score 5 minute	22004	9.651	7925	9.641	0.010	(0.009)
Mother's number of prenatal controls	21887	6.777	7869	7.728	−0.951***	(0.043)

Note: Standard errors (s.e.) are reported in parentheses. N corresponds to number of observations. * p<.1, ** p<.05, *** p<.01.

4 Empirical Framework

In this section we explain the main identification strategy used to estimate the impacts of the PANES program on long-term educational results. We also show first-stage estimates of the effect of eligibility in the PANES transfer on actual treatment and discuss the validity of our main estimates.

4.1 Identifying long-run impacts of the PANES program

To examine the impact of the PANES program on educational attainment 8 to 12 years after exposure to the program, we use a regression discontinuity design. We exploit the fact that program assignment was determined by a predicted poverty score. Families that ranked above a certain threshold were eligible to receive the cash transfer while those below the threshold were not. This rule creates a discontinuity in the probability of receiving the transfer. Given that eligibility enforcement is high but not perfect, we estimate program effects using a fuzzy regression discontinuity design.

We compare outcomes of children that were born in households that were just above and just below the cutoff. The equation that we estimate is the following:

$$Y_{imt} = \alpha_0 + \alpha_1 T_m + f(N_m) + \alpha_2 X_{imt} + e_{imt} \tag{1}$$

where Y is the schooling outcome of interest of child i conceived by mother m and born in year t, T_m is a binary indicator variable that takes the value 1 if the mother m received the benefit or 0 otherwise, N_m denotes mother m's predicted poverty score (normalized relative to the eligibility threshold such that households with positive N_m are eligible for treatment), f is a function of the running variable that is continuous at the threshold (N_m=0) and that may have different slopes at each side of the cutoff. All regressions control for month times year of birth fixed effects, and month times year of baseline visit fixed effects. X_{imt} include the latter fixed effects and may also include other controls as we mention in the following paragraph. e_{imt} is a random error term. We instrument the PANES treatment variable T_m, with an indicator for the mother's program eligibility, E_m. α_1 is the parameter of interest.

As in fully randomized experiments, it is not necessary to include covariates in regression discontinuity designs. However, it is often the case that studies include them to reduce variability in the estimation (Lee and Lemieux, 2010). In our estimations we control for covariates, X_{imt}, at the level of the child, of the mother and of the household.[16] Controls

[16]We control for covariates that are not used to predict the eligibility score (see Tables A1 and A2) with the exception of those that are unbalanced at baseline. In particular, we control for: gender of the child, educational level of the mother, indicators for whether the household's block has sewage, trash collection and

are included as indicator variables and we use a separate category for missing observations in each control.

We estimate Equation 1 for the entire sample and on two subsamples: children exposed to the cash transfer while they were in-utero (i.e. those born between April 2005 and December 2007) and children exposed to the cash transfer after birth (i.e. those born between January 2003 and March 2005). We report results based on the bandwidth and polynomial selected following the approach of Calonico et al. (2014).[17] This approach consists of a local polynomial nonparametric estimator with data-driven bandwidth selector and biased-correction techniques. We refer to this approach as "CCT". In most cases, the optimal bandwidth ranges between 0.05 and 0.1 (meaning, respectively, differences of 5 to 10 percentage points in the predicted poverty score).

One concern is that pregnancy might be endogenous to gaining program eligibility. Having one more child would increase the probability of treatment since the score was estimated using the *per capita* income level of the household. This could bias the estimates of program impact if women who change their pregnancy patterns give birth to children with different characteristics, for example, with a different probability of low birth weight. Given that the initial application period was concentrated in a relatively short period of time (75% of applications took place in the first nine months of the program), it seems unlikely that in such period fertility patterns may have been influenced. A related issue is the possibility of any fertility responses to the program in order to retain eligibility. To ease concerns about later fertility choices, we use the predicted income score at the initial application as an instrument for program receipt, instead of the score at each reassessment of eligibility status (where circumstances in the household, including child birth, may have changed).

4.2 First-stage effects of the PANES program

Figure 1 shows a clear jump in the fraction of individuals that actually received the PANES transfers.[18] While 96% of poor households located to the right of the cutoff received the cash transfer, 13% of ineligible households managed to enter the program.

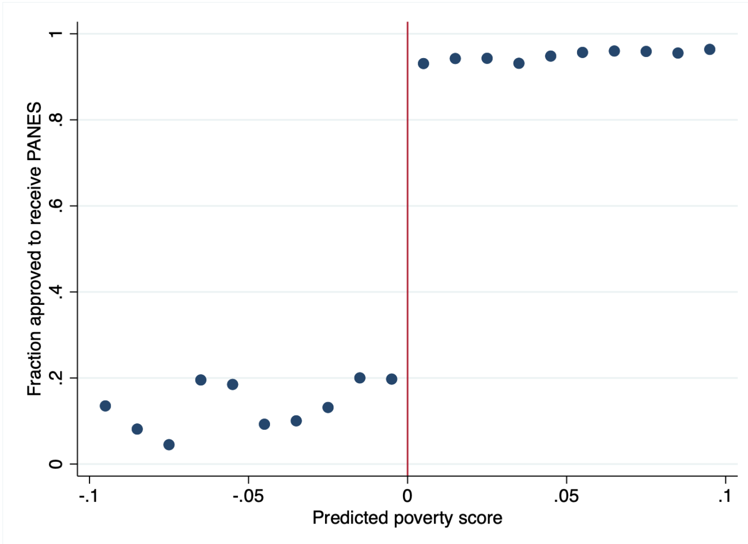
Table 2 presents first-stage estimates of the effect of eligibility in the PANES transfer on actual treatment. We report results using three different ranges around the eligibility threshold (Columns (1)-(8)). We also report results for the bandwidth defined according to Calonico et al. (2014) (Columns (13)-(14)). In Panel A we report estimates for the whole

for the number of bedrooms in the household.

[17] Calonico et al. (2014) incorporates the latests advances in regression discontinuity methods and refines the estimator proposed by Imbens and Kalyanaraman (2012)

[18] Note that the normalized predicted poverty score ranges from -0.19 to 0.95 in our sample.

Figure 1: Receipt of PANES



Note: The vertical line corresponds to the eligibility cutoff, above which households are eligible to the program and below which they are not eligible to the program. There are 10 bins at each side of the cutoff and the range is -0.1, 0.1. Each dot represents the fraction of households that received the PANES transfers in that bin.

sample, in Panel B we report estimates for children exposed during early childhood and in Panel C we report estimates for those exposed while in-utero. The estimated increase in the fraction of treated households at the threshold is large (between 0.70 and 0.76) and does not change much between specifications.[19] The first-stage estimates become larger when using observations that are further away from the cutoff.

[19]We obtain very similar results when using a second order polynomial function (see Table A4 in the Appendix).

Table 2: First stage estimates of the effect of the eligibility on the PANES cash transfer

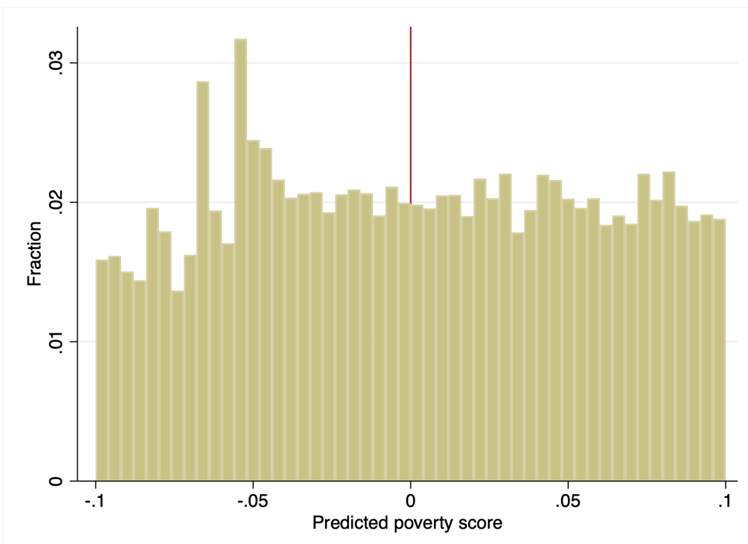
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: All observations								
Coefficient	0.747***	0.755***	0.741***	0.747***	0.701***	0.717***	0.702***	0.716***
s.e.	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)	(0.013)	(0.012)
Observations	25622	25622	19863	19863	13262	13262	9358	9592
Range	0.1	0.1	0.075	0.075	0.05	0.05	0.036	0.037
Panel B: Exposed during early-childhood								
Coefficient	0.742***	0.757***	0.736***	0.750***	0.697***	0.720***	0.693***	0.718***
s.e.	(0.010)	(0.010)	(0.012)	(0.011)	(0.014)	(0.014)	(0.019)	(0.018)
Observations	12198	12198	9435	9435	6277	6277	5119	5156
Range	0.1	0.1	0.075	0.075	0.05	0.05	0.042	0.042
Panel C: Exposed while in-utero								
Coefficient	0.751***	0.754***	0.744***	0.747***	0.703***	0.715***	0.707***	0.710***
s.e.	(0.009)	(0.009)	(0.011)	(0.011)	(0.014)	(0.013)	(0.019)	(0.018)
Observations	13424	13424	10428	10428	6985	6985	5066	5053
Range	0.1	0.1	0.075	0.075	0.05	0.05	0.037	0.037
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Note: Each cell corresponds to a different regression. In Columns (1)-(6) we estimate Equation 1 using as outcome variable an indicator that takes the value of one if the household received the PANES transfer. We report results for three different fixed ranges around the eligibility threshold and a first order polynomial. We also report the estimates obtained when using the bandwidth and polynomial defined according to Calonico et al. (2014) (Columns (7)-(8)). All estimations include month times year of birth fixed effects, and month times year of baseline visit fixed effects. Estimations in even columns we include the following additional controls: gender of the child, an indicator for whether the mother completed primary school, number of bedrooms in the household and indicators for whether the household's block has sewage and trash collection. All controls are included as indicator variables and include a category for missing observations. Standard errors (s.e.) are reported in parentheses and number of observations are reported below each coefficient. * p<.1, ** p<.05, *** p<.01.

4.3 Testing the Identifying Assumptions

The regression discontinuity design assumes that assignment to either side of the threshold is as good as random. To check whether there is bunching just above or just below the threshold, we plot a density graph of the running variable (predicted poverty score) for the whole sample (Figure 2) and for each of the two subsamples (Figure A1 and Figure A2 in the Appendix). A visual inspection of the density graphs suggests that bunching does not occur. More formally, we test bunching by conducting a McCrary's density test (McCrary, 2008) using observations near the threshold.[20] The log difference in height is 0.022 (s.e. 0.047) in the full sample, 0.019 (s.e. 0.060) in the sample of children exposed during early-childhood and 0.041 (s.e. 0.066) in the sample of children exposed while in-utero.

Figure 2: Density



Note: The figure shows the distribution in the range of -0.1 and 0.1 of the running variable. Each bar represents the fraction of households in specific values of the predicted poverty score. The vertical line corresponds to the eligibility cutoff.

To check whether covariates are balanced at baseline, we run Equation 1 using a wide range of baseline household, mother and child characteristics.[21] Table 3 reports results from estimating the effect of the PANES program on the different covariates at baseline. Most coefficients are small and not significantly different from zero which is in line with assignment around the threshold being as good as random.[22] Moreover, a joint significance test gives

[20] We use observations that have a value of the running variable in the range -0.1 and 0.1.

[21] We use pre-program data for those covariates that are not measured at baseline: birth weight, low birth weight, apgar 1 minute, apgar 5 minutes, age of the mother at birth, number of prenatal controls, gestational weeks and number of previous pregnancies.

[22] From the few variables that are unbalanced, number of previous pregnancies is the most expected one.

a p-value of 0.159.[23] Most covariates have a strong correlation with highest grade attained, yet they are balanced between treated and controls (see Column (5)).[24] Boys have a lower educational attainment than girls, being born with low birth weight has a negative correlation with highest grade attained and children whose mothers have completed primary education attain higher grades than those with lower educated mothers.

We include estimates of the effect of the PANES transfer on different covariates for children exposed to the program during early-childhood and for children exposed to the program while in-utero separately in Tables A5 and A6 in the Appendix. Balancing in the sample of children exposed while in-utero is similar to that in the full sample, with only three coefficients showing-up significant. This is consistent with the identification assumption that assignment around the threshold is as good as random. In the case of the sample of children exposed to PANES during early-childhood, coefficients are significant in a few more cases but the sign of these coefficients go in the opposite direction of the correlation of the covariate with the outcome highest grade attained. In any case, we control for all pre-treatment covariates

This is due to the fact that families with more kids have a lower income per capita and are therefore more likely to receive the PANES program.

[23]The estimation is performed using pre-program data and considers the optimal bandwidth obtained in Table 2.

[24]We checked these correlations for the other educational outcomes and the conclusion is the same.

Table 3: Estimates of the effect of the PANES transfer on different covariates using baseline data and correlation of covariates with main outcome

	Non-eligible mean	Coefficient	s.e.	N	Correlation with outcome
	(1)	(2)	(3)	(4)	(5)
Child's indicators					
Child is a boy	0.510	0.008	(0.014)	19863	$-0.191***(0.011)$
Birth weight	3199	18.840	(22.140)	9362	$0.000***(0.000)$
Low birth weight	0.080	-0.007	(0.012)	9362	$-0.180***(0.033)$
Apgar 1 minute	8.575	-0.037	(0.043)	9368	$0.026***(0.009)$
Apgar 5 minutes	9.675	0.002	(0.031)	9366	$0.044***(0.013)$
Age in months in Dec 2007	29.930	-0.307	(0.490)	19863	$0.021***(0.001)$
Mother's indicators					
Age	24.627	0.005	(0.278)	9290	$-0.003***(0.001)$
Complete primary education	0.925	0.014*	(0.008)	19826	$0.341***(0.020)$
Complete secondary education	0.031	0.001	(0.005)	19826	$0.130***(0.033)$
Number of prenatal controls	7.463	0.218	(0.135)	9349	$0.026***(0.003)$
Gestational weeks	38.640	0.073	(0.077)	9169	$0.026***(0.005)$
Number of previous pregnancies	2.564	-0.219***	(0.076)	9435	$-0.046***(0.005)$
Household's indicators					
Hot water	0.294	0.019	(0.012)	19859	$0.081***(0.013)$
Heater	0.192	0.001	(0.011)	19845	$0.090***(0.014)$
Kitchen	0.684	0.012	(0.014)	19862	$0.113***(0.012)$
Heating	0.007	-0.003	(0.002)	19833	$-0.023(0.071)$
Concrete floor	0.555	-0.012	(0.014)	19653	$-0.050***(0.011)$
Mud wall	0.920	0.010	(0.008)	19551	$0.109***(0.019)$
Block has electricity	0.978	0.000	(0.005)	19858	$0.067*(0.035)$
Block has piped water	0.940	0.010	(0.007)	19851	$0.060***(0.023)$
Block has sewage	0.409	0.050***	(0.014)	19793	$0.058***(0.011)$
Block has trash collection	0.900	0.017*	(0.009)	19835	$0.092***(0.018)$
Block has paved streets	0.666	0.006	(0.014)	19797	$0.032***(0.012)$
Block has sidewalk	0.701	0.009	(0.013)	19808	$0.077***(0.012)$
House	0.879	-0.015	(0.010)	19533	$0.068***(0.016)$
Microwave	0.045	0.002	(0.005)	19863	$0.111***(0.029)$
Refrigerator	0.662	0.018	(0.014)	19848	$0.065***(0.011)$
Freezer	0.092	0.008	(0.008)	19824	$0.035*(0.020)$
Washing machine	0.186	0.000	(0.011)	19863	$0.021(0.015)$
Dishwasher	0.002	0.001	(0.001)	19849	$0.065(0.128)$
TV	0.791	0.005	(0.012)	19859	$0.104***(0.013)$
VCR	0.040	0.008	(0.005)	19857	$-0.018(0.030)$
Cable TV	0.134	0.010	(0.009)	19863	$0.122***(0.018)$
Computer	0.010	0.001	(0.003)	19855	$0.143**(0.063)$
Car	0.031	0.003	(0.005)	19863	$0.080**(0.035)$
Home owned	0.498	-0.006	(0.014)	19831	$-0.041***(0.011)$
Number of rooms	2.407	0.059	(0.055)	19861	$0.016***(0.003)$
Number of bedrooms	1.721	0.047**	(0.024)	19861	$0.022***(0.007)$
Receipt of *Plan de Equidad*	0.804	-0.006	(0.011)	19392	$0.065***(0.014)$

Note: In Column (2) we report estimates of Equation 1 using different covariates at baseline as outcome variables. We use pre-program data for those covariates that are not measured at baseline: birth weight, low birth weight, apgar 1 minute, apgar 5 minutes, age of the mother at birth, number of prenatal controls, gestational weeks and number of previous pregnancies. Estimates are obtained using a bandwidth of 0.075 around the threshold and a first order polynomial. In Column (5) we report the correlation of each covariate with the outcome highest grade attained. We obtain these correlations by regressing highest grade attained on each covariate and conditioning on month times year of birth fixed effects, and month times year of baseline visit fixed effects. Standard errors (s.e.) are reported in parentheses. N corresponds to number of observations. * p<.1, ** p<.05, *** p<.01.

5 Empirical Results

5.1 Main Results

In Table 4 we report estimates of the effect of receiving the PANES transfer during early childhood and while in-utero on educational attainment 8 to 12 years later. In Panel A we report estimates for the whole sample, in Panel B we report estimates for children exposed during early childhood and in Panel C we report estimates for those exposed while in-utero. For each outcome, we use two specifications: one with controls and one without controls.[25] By and large, coefficients go in the expected direction: the effects on highest grade attained should be positive, the effects of delay in educational attainment should be negative and the effects on dropout should be negative. For the entire sample (Panel A), there is a negative and significant effect on the probability of being delayed (the p-value is 0.07 and 0.08 in Columns (3) and (4) respectively). When splitting the sample, we find that eligible children that were exposed to the program during early-childhood (Panel B) have a higher educational attainment (the p-value in Columns (1) and (2) is 0.08). In addition, we find that the effect on educational attainment is due to a lower incidence of delay in education (the p-value is 0.06 and 0.08 in Columns (3) and (4) respectively). We find no significant effects on dropout. The latter comes at no surprise given that we are considering children that are mainly in primary school and dropout is more likely to occur in secondary. Educational attainment of children exposed to the program while in-utero (Panel C) is not significantly different between eligible and ineligible households in any of the outcomes considered.

[25] Note that the number of observations in each regression changes according to the bandwidth. For the same sample, the number of observations changes whether we use or not use controls. These changes not always go in the same direction. In Tables A8 and A9 in the Appendix we report results using specific bandwidths with a fixed number of observations.

Table 4: Effect of receiving the PANES transfer on educational attainment

	Highest grade attained		Delay		Dropout	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All observations						
Coefficient	0.043	0.047	-0.040*	-0.038*	-0.001	-0.002
s.e.	(0.044)	(0.040)	(0.022)	(0.022)	(0.007)	(0.007)
Observations	11613	13586	16597	16744	18224	18297
CCT bandwidth	0.045	0.051	0.062	0.063	0.068	0.068
Panel B: Exposed during early-childhood						
Coefficient	0.125*	0.112*	-0.069*	-0.064*	0.008	0.007
s.e.	(0.071)	(0.065)	(0.036)	(0.037)	(0.011)	(0.010)
Observations	6553	7376	7608	7264	8350	9111
CCT bandwidth	0.052	0.058	0.060	0.057	0.066	0.072
Panel C: Exposed while in-utero						
Coefficient	-0.001	-0.002	-0.015	-0.012	-0.009	-0.009
s.e.	(0.034)	(0.034)	(0.024)	(0.023)	(0.008)	(0.008)
Observations	11917	11809	12065	12453	11973	11667
CCT bandwidth	0.087	0.086	0.088	0.092	0.088	0.084
Controls	No	Yes	No	Yes	No	Yes

Note: Each cell corresponds to a different regression. In Panel A we use the sample of children whose family received the PANES transfer during early-childhood. In Panel B we use the sample of children whose family received the PANES transfer while the child was in-utero. In Panel C we use all observations. We estimate Equation 1 using different outcome variables. In Columns (1)-(2) we report results using as outcome variable highest grade attained in education. In Columns (3)-(4) we report results using as outcome variable an indicator for delay in educational attainment that takes value 1 if the child is enrolled at a lower grade than the one determined by her year and month of birth and a regular track. In Columns (5)-(6) we report results using as outcome variable an indicator for whether the child dropped out from education, where dropout is measured as being two or more years not enrolled in any program. We report the estimates obtained when using the CCT bandwidth and polynomial defined according to Calonico et al. (2014). CCT bandwidths are reported below each coefficient and CCT polynomial is 1 in all cases. All estimations include month times year of birth fixed effects, and month times year of baseline visit fixed effects. In even columns we present results of estimating Equation 1 using the following additional controls: gender of the child, an indicator for whether the mother completed primary school, number of bedrooms in the household and indicators for whether the household's block has sewage and trash collection. All controls are included as indicator variables and include a category for missing observations. Standard errors (s.e.) are reported in parentheses and number of observations are reported below each coefficient. * p<.1, ** p<.05, *** p<.01.

5.2 Heterogeneous effects by low birth weight

Children who were first exposed to PANES during early childhood were born at the time of a severe economic crisis. In this subsection we explore whether transfers have a stronger effect on education on children that are born with more risk of weighting less than 2500 grams. Table 5 shows heterogeneous effects of receiving the PANES transfer on education by low birth weight. Estimations only consider observations of children born in the pre-program period, and hence, that were exposed to the program during early childhood. The first two columns correspond to children that were born with low birth weight (=1 if <2500 grams) and the last two columns correspond to children that were born with normal birth weight. Barely eligible children that are born with a weight less than 2500 grams have a higher educational attainment and a lower incidence of delay than barely ineligible children born with the same condition. In spite of having 800 observations, the effect is significant at least at the 5% level, indicating that its magnitude is especially large. There is also an effect on dropout but goes in the opposite direction than expected. In particular, the likelihood of dropout is higher among barely eligible children born with low birth weight than among barely ineligible children born with low birth weight. The latter effect is significant at the 10 percent level. Overall, the findings from this analysis suggest that the program had stronger effects on children who were born when the economic context was more unfavorable and with more risk of low birth weight.

5.3 Exploring short run impacts of PANES on low birth weight

We found no evidence supporting that the PANES program improved educational attainment for those exposed while in-utero. At a first glance, these results are surprising given the large literature on the effects of low birth weight on educational attainment (Figlio et al., 2014) and previous evidence showing that PANES improved health at birth. In this subsection we explore whether low birth weight is a potential mechanism behind long-term educational outcomes.

We use a regression discontinuity approach and compare health at birth outcomes between eligible and ineligible children that were born during the program period. A visual inspection of the incidence of low birth weight at both sides of the PANES eligibility cutoff (Figure 3) suggests that the program had no impact on health at birth. Table 6 shows results from estimating Equation 1 using low birth weight as the outcome variable. We find that the relevant coefficients are negative but are small in magnitude and not significant. Our findings are in line with Buser et al. (2017) which finds no effect on weight and height of gaining a cash transfer in Ecuador. Our conclusion is that low birth weight cannot be considered a

Table 5: Heterogeneous effects of receiving the PANES transfer on education by low birth weight

	Born with low birth weight		Born with normal birth weight	
	(1)	(2)	(3)	(4)
Panel A: Highest grade attained				
Coefficient	0.500**	0.529***	0.078	0.070
s.e.	(0.199)	(0.198)	(0.067)	(0.058)
Observations	783	761	6936	8434
CCT bandwidth	0.075	0.072	0.060	0.074
Panel B: Delay				
Coefficient	-0.241**	-0.255***	-0.050	-0.040
s.e.	(0.099)	(0.099)	(0.035)	(0.035)
Observations	865	848	7923	7657
CCT bandwidth	0.084	0.082	0.068	0.067
Panel C: Dropout				
Coefficient	0.065*	0.069*	0.004	0.003
s.e.	(0.039)	(0.041)	(0.011)	(0.010)
Observations	844	789	8591	9259
CCT bandwidth	0.081	0.076	0.075	0.082
Controls	No	Yes	No	Yes

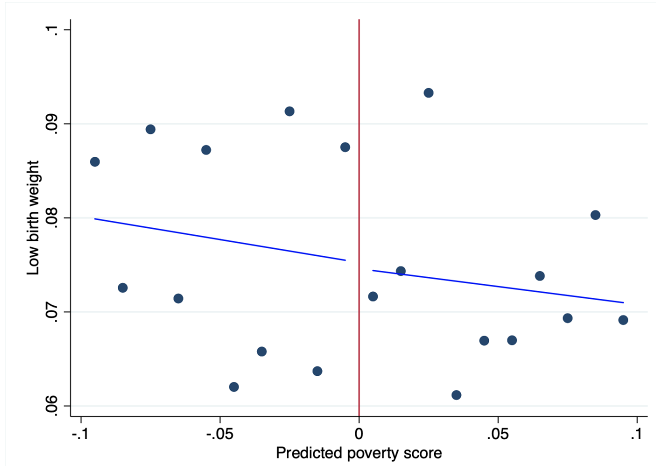
Note: Each cell corresponds to a different regression. Estimations consider the sample of children whose family received the PANES transfer during early-childhood. We estimate Equation 1 using different outcome variables and for two different subsamples. Each panel corresponds to a different outcome. In Columns (1)-(2) we report results for children with low birth weight. In Columns (3)-(4) we report results for children without low birth weight. We report the estimates obtained when using the CCT bandwidth and polynomial defined according to Calonico et al. (2014). CCT bandwidths are reported below each coefficient and CCT polynomial is 1 in all cases. All estimations include month times year of birth fixed effects, and month times year of baseline visit fixed effects. In even columns we present results of estimating Equation 1 using the following additional controls: gender of the child, an indicator for whether the mother completed primary school, number of bedrooms in the household and indicators for whether the household's block has sewage and trash collection. All controls are included as indicator variables and include a category for missing observations. Standard errors (s.e.) are reported in parentheses and number of observations are reported below each coefficient. * p<.1, ** p<.05, *** p<.01.

first stage effect for our long-term impacts on education of children exposed to the program while in-utero.

Following Amarante et al. (2016), we also report results on health at birth using a localized difference in differences estimator (See Appendix B for details of this identification strategy).[26] Table 7 reports results from estimating Equation 2 (see Appendix B) using

[26]This method was first formalized by Grembi et al. (2016) but others have executed similar empirical strategies in prior literature. Grembi et al. (2016) propose and verify a set of diagnostic tests for this design. They refer to this method as "difference in discontinuity design". Identification rests on the difference between two cross-sectional estimators instead of within unit variation in treatment assignment.

Figure 3: Low birth weight around the PANES cutoff



Note: The vertical line corresponds to the eligibility cutoff, above which households are eligible to the program and below which they are not eligible to the program. There are 10 bins at each side of the cutoff and the range is -0.1, 0.1. Each dot represents the average low birth weight in that bin. The two solid lines represent the best fit from a linear regression from each side of the cut-off.

low birth weight as outcome variable.[27] From our estimations, we cannot reject the null hypothesis of no effect of the PANES transfer on the incidence of low birth weight. The estimated coefficients are negative and mostly non-significant. Standard errors increase as we get closer to the cut-off.[28] These results differ with the findings from Amarante et al. (2016)[29] and we attribute this to the fact that the sample that we use is different. Specifically, our database does not include multiple births which on average are those that are born with lower weights.[30] In any case, as we discuss further in Appendix B, we cannot validate all of the assumptions of the localized difference in differences in the setting of this paper, hence, our regression discontinuity estimates are our preferred specification.

[27]We include an equivalent set of control variables as those used in Amarante et al. (2016).

[28]We do not use a CCT bandwidth for these estimations given that the equation we estimate does not correspond to a traditional regression discontinuity design.

[29]Note, however, that Amarante et al. (2016) do not find an effect of PANES on low birth weight when using a regression discontinuity strategy.

[30]Amarante et al. (2016) find that exposure to PANES reduces the incidence of birth weights below 3000 grams and that effects grow at lower birth weights.

Table 6: 2SLS estimates of the effect of the PANES transfer on low birth weight (<2.500 kg) children born during program period

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1. No controls	-0.001	-0.006	0.000	-0.008	-0.009	-0.008	-0.003
	(0.012)	(0.019)	(0.014)	(0.027)	(0.018)	(0.027)	(0.013)
Observations	13283	13283	10309	6905	6905	6905	14243
Bandwidth	0.1	0.1	0.075	0.075	0.05	0.05	0.114
Order of polynomial	1	2	1	2	1	2	1
2. Controls	-0.001	-0.005	0.001	-0.006	-0.009	-0.006	-0.003
	(0.012)	(0.019)	(0.014)	(0.027)	(0.018)	(0.027)	(0.013)
Observations	13283	13283	10309	6905	6905	6905	13384
Bandwidth	0.1	0.1	0.075	0.075	0.05	0.05	0.101
Order of polynomial	1	2	1	2	1	2	1

Note: Each cell corresponds to a different regression. Sample includes children that were born during the program period. In Columns (1)-(6) we estimate Equation 1 for three different bandwidths around the eligibility threshold and two different orders of polynomial. In Column (7) we report the estimates obtained when using the bandwidth and polynomial defined according to Calonico et al. (2014). All estimations include month times year of birth fixed effects, and month times year of baseline visit fixed effects. Row 1 presents regressions with no additional controls while row 2 reports results with the following additional controls: gender of the child, an indicator for whether the mother completed primary school, number of bedrooms in the household and indicators for whether the household's block has sewage and trash collection. Controls are included as indicator variables and include a category for missing observations. Standard errors are reported in parentheses and number of observations are reported below each coefficient. * p<.1, ** p<.05, *** p<.01.

Table 7: 2SLS estimates of the effect of the PANES transfer on low birth weight (<2.500 kg) using a difference in discontinuity design

	(1)	(2)	(3)	(4)	(5)	(6)
1. No controls	-0.011*	-0.011*	-0.013	-0.013	-0.014	-0.014
	(0.006)	(0.006)	(0.008)	(0.008)	(0.010)	(0.010)
Observations	56856	56856	25384	25384	19670	19670
2. Controls	-0.013**	-0.013**	-0.013	-0.013	-0.013	-0.013
	(0.006)	(0.006)	(0.008)	(0.008)	(0.010)	(0.010)
Observations	56856	56856	25384	25384	19670	19670
Range	All	All	0.1	0.1	0.075	0.075
Order of polynomial	1	2	1	2	1	2

Note: Each cell corresponds to a different regression. Sample contains pooled pre-program and program period data, corresponding to children born between the years 2003 and 2007. In Columns (1)-(6) we estimate Equation 2 for three different ranges around the eligibility threshold and three different orders of polynomial. All estimations include month times year of birth fixed effects, and month times year of baseline visit fixed effects. Row 1 presents regressions with no additional controls while row 2 reports results with the following additional controls: gender of the child, number of previous pregnancies of the mother, an indicator for whether the mother completed primary school, indicators for geographic department of the household at baseline, for whether the household has centralized hot water, heater, kitchen, microwave, refrigerator, freezer, washing machine, dishwasher, TV, VCR, cable TV, computer, car, whether the block has electricity, piped water, sewage, trash collection, paved streets, sidewalk, whether the home is a house, is owned, and indicators for material of the floor and walls. Controls are included as indicator variables and include a category for missing observations. Standard errors are reported in parentheses and number of observations are reported below each coefficient. * p<.1, ** p<.05, *** p<.01.

6 Discussion

In this paper we explore whether expanding economic resources during early-life in the form of an unconditional cash transfer improves later outcomes. In particular, we explore the effect of being exposed to the Uruguayan PANES in the prenatal period and during early childhood on educational outcomes 8 to 12 years later. We use a rich dataset that matches administrative data from three sources and enables us to distinguish effects for children that were exposed since they were in their mother's womb and children that were exposed to the program in the first years of life.

Our results show that children from eligible households that started receiving the program after they were born, have mild effects on educational attainment and the likelihood of educational delay. Results are significant at the 10% level. We find an increase on educational attainment of 0.1 years of education and a decrease in the likelihood of delay of 6.9 percentage points around the eligibility cutoff. These results correspond to local average treatment effects around the cutoff point. Considering that the amount of the transfer represented almost half of the average household income among its population, the magnitude of the effects of PANES on education is rather small.

One potential explanation to why we find results for the subsample of relatively older children and not on the relatively younger children is that the former sample were born and started receiving the program when the Uruguayan context was more unfavorable and poverty rates were higher. Note that total income in PANES applicant families doubled between the pre-program and program period. Our interpretation is that, more than arguing against Heckman's theory of dynamic complementarities, our findings suggest that the program has an impact on children born in families that are close to the eligibility cutoff when children are born in a worse economic situation, and with more risk of low birth weight.

While we are considering a cash transfer in the Latin American context, our results are in line with studies on the longer-term effects of unconditional cash transfers in Africa (Haushofer and Shapiro, 2018; Blattman et al., 2020). The evidence in this paper, however, contrasts the one found in other Latin American countries for the effect of conditional cash transfers on educational attainment in the longer run (Millán et al., 2019). We conclude that while unconditional transfers may be sufficient to fight present poverty, including conditionalities and requiring investment in children's human capital might be necessary to improve educational outcomes when children grow up.

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Appendices

A Appendix Tables and Figures

This section includes several tables and figures to supplement the information in the main text. Tables and figures show: (i) the variables that enter the poverty score, (ii) the corresponding grade of children in the sample according to their birth date, (iii) first stage estimates using a second order polynomial, (iv) the bunching and balancing properties of each subsample, (v) the effect of the PANES program on the likelihood of enrolling in seventh grade without delay, and (vi) 2SLS estimates of the effect of the PANES program for fixed bandwidths.

Table A1: Variables included in the poverty score

	Urban areas		Rural areas
	Capital city	Other regions	
Public employees in the household	✓	✓	
Retirees in the household	✓	✓	✓
Pensioners in the household	✓	✓	
Logarithm of the number of household members	✓	✓	✓
Presence of children aged 0-5	✓	✓	
Presence of adolescents aged 12-17	✓	✓	
Presence of children aged 0-4			✓
Presence of children aged 5-10			✓
Presence of adolescents aged 11-17			✓
Wealth index (See Table A2)	✓	✓	✓
Average years of education of adults	✓	✓	
Household's head completed primary education			✓
Residential overcrowding	✓	✓	✓
Toilet facilities: no toilet	✓		
Toilet facilities: flush toilet	✓		
Toilet facilities: pit latrine	✓		
Toilet facilities: other	✓		
Toilet facilities: no toilet		✓	
Toilet facilities: flush toilet or pit latrine		✓	
Toilet facilities: other		✓	
Toilet facilities: no cistern			✓
Masonry			✓
Concrete floor			✓
Dirt floor			✓
House is owned	✓		
House is leased	✓		
House is occupied	✓		
Household type: head only			✓
Household type: head and spouse			✓
Household type: head and children			✓
Household type: head, spouse and children only			✓
Household type: head, spouse, children and other relatives			✓
Household type: head, spouse, children and other non-relatives			✓
At least one of the household's member has mutual insurance			
Household's head has mutual insurance			✓
Year	✓	✓	
Constant	✓	✓	

Note: Own elaboration based on Amarante et al. (2005). The model used to predict the poverty score was estimated using the Continuous Household Survey of 2003 and 2004.

Table A2: Variables used to construct the wealth index

	Urban areas	Rural areas
Ownership of water heater	✓	✓
Ownership of boiler	✓	
Ownership of fridge	✓	✓
Ownership of color television	✓	✓
Access to cable television	✓	
Ownership of videocassette recorder	✓	✓
Ownership of washing machine	✓	✓
Ownership of dishwasher	✓	
Ownership of microwave	✓	
Ownership of laptop computer	✓	
Ownership of car	✓	✓
Ownership of telephone	✓	✓

Note: Own elaboration based on Amarante et al. (2005).

Table A3: Corresponding grade in 2017 according to child's year and month of birth

		Month of birth											
		January	February	March	April	May	June	July	August	September	October	November	December
	2003	10	10	10	10	9	9	9	9	9	9	9	9
	2004	9	9	9	9	8	8	8	8	8	8	8	8
Year of birth	2005	8	8	8	8	7	7	7	7	7	7	7	7
	2006	7	7	7	7	6	6	6	6	6	6	6	6
	2007	6	6	6	6	5	5	5	5	5	5	5	5

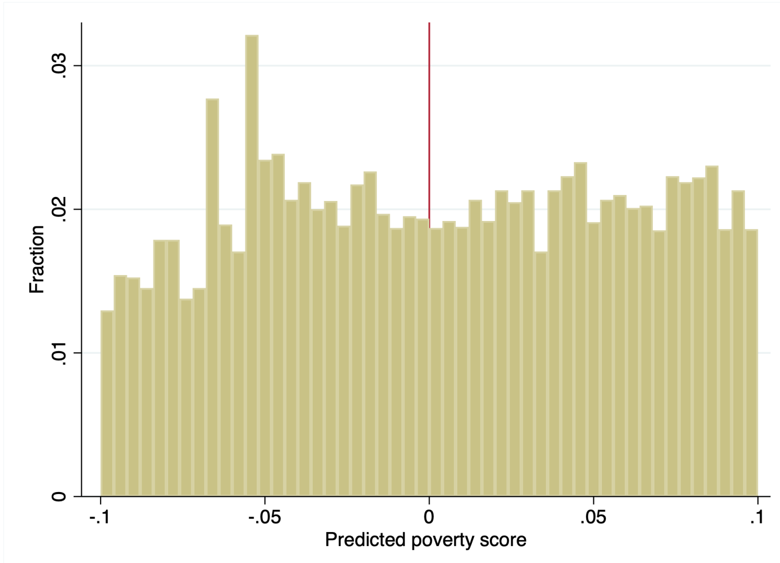
Note: Table shows corresponding grade that a children should attain according to its year and month of birth in Uruguay. Grade 1 corresponds to the last year of preschool education and grade 10 corresponds to the third year of secondary school. The requirement to enter the Uruguayan public education system is to have the age corresponding to the level before April 30 of the school year. That causes most children (2/3) to reach the age following the level during the school year and that 1/3 of the children do it the other year

Table A4: First stage estimates of the effect of the eligibility on the PANES cash transfer using a second order polynomial function

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All observations						
Coefficient	0.747***	0.755***	0.741***	0.747***	0.702***	0.718***
s.e.	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
Observations	25622	25622	19863	19863	13262	13262
Range	0.1	0.1	0.075	0.075	0.05	0.05
Panel B: Exposed during early-childhood						
Coefficient	0.741***	0.756***	0.736***	0.749***	0.698***	0.720***
s.e.	(0.010)	(0.010)	(0.012)	(0.011)	(0.014)	(0.014)
Observations	12198	12198	9435	9435	6277	6277
Range	0.1	0.1	0.075	0.075	0.05	0.05
Panel C: Exposed while in-utero						
Coefficient	0.751***	0.754***	0.744***	0.747***	0.704***	0.716***
s.e.	(0.009)	(0.009)	(0.011)	(0.011)	(0.014)	(0.013)
Observations	13424	13424	10428	10428	6985	6985
Range	0.1	0.1	0.075	0.075	0.05	0.05
Controls	No	Yes	No	Yes	No	Yes

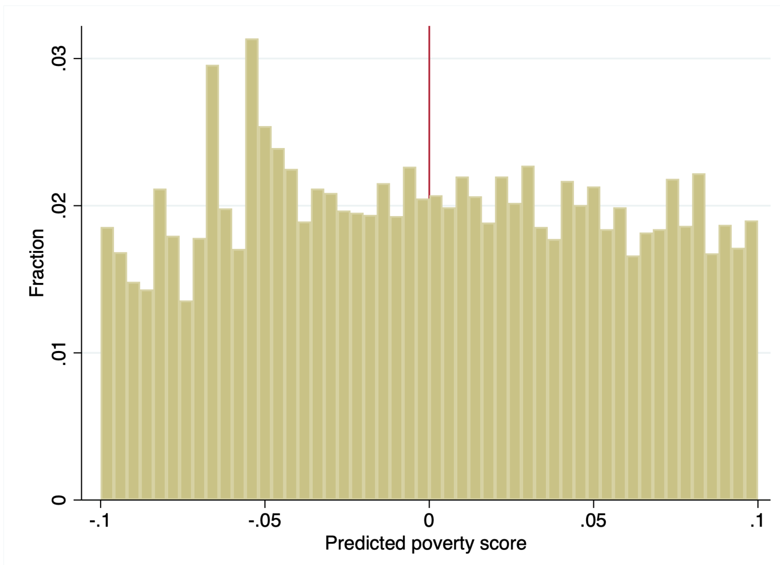
Note: Each cell corresponds to a different regression. In Columns (1)-(6) we estimate Equation 1 using as outcome variable an indicator that takes the value of one if the household received the PANES transfer. We report results for three different fixed ranges around the eligibility threshold and a second order polynomial. All estimations include month times year of birth fixed effects, and month times year of baseline visit fixed effects. Estimations in even columns we include the following additional controls: gender of the child, an indicator for whether the mother completed primary school, number of bedrooms in the household and indicators for whether the household's block has sewage and trash collection. All controls are included as indicator variables and include a category for missing observations. Standard errors (s.e.) are reported in parentheses and number of observations are reported below each coefficient. * $p<.1$, ** $p<.05$, *** $p<.01$.

Figure A1: Density for subsample of children exposed during early-childhood



Note: The figure shows the distribution in the range of -0.1 and 0.1 of the running variable for the subsample of children exposed during early childhood. Each bar represents the fraction of households in specific values of the predicted poverty score. The vertical line corresponds to the eligibility cutoff.

Figure A2: Density for subsample of children exposed while in-utero



Note: The figure shows the distribution in the range of -0.1 and 0.1 of the running variable for the subsample of children exposed while in-utero. Each bar represents the fraction of households in specific values of the predicted poverty score. The vertical line corresponds to the eligibility cutoff.

Table A5: Estimates of the effect of the PANES transfer on different covariates using baseline data for subsample of children exposed during early-childhood

	Non-eligible mean	Coefficient	s.e.	N
	(1)	(2)	(3)	(4)
Child's indicators				
Child is a boy	0.511	0.010	(0.021)	9435
Age in months in Dec 2007	33.368	-1.014	(0.730)	9435
Mother's indicators				
Age	24.627	0.005	(0.278)	9290
Complete primary education	0.932	0.008	(0.011)	9422
Complete secondary education	0.036	0.003	(0.008)	9422
Household's indicators				
Hot water	0.313	0.043**	(0.019)	9434
Heater	0.196	-0.007	(0.017)	9428
Kitchen	0.696	0.024	(0.020)	9435
Heating	0.007	-0.003	(0.003)	9421
Concrete floor	0.928	0.018	(0.012)	9287
Mud wall	0.537	-0.010	(0.021)	9330
Block has electricity	0.978	0.004	(0.007)	9433
Block has piped water	0.937	0.009	(0.010)	9431
Block has sewage	0.417	0.044**	(0.021)	9402
Block has trash collection	0.894	0.015	(0.013)	9422
Block has paved streets	0.664	0.037*	(0.020)	9403
Block has sidewalk	0.703	0.023	(0.019)	9411
House	0.884	-0.006	(0.014)	9273
Microwave	0.046	0.007	(0.008)	9435
Refrigerator	0.679	0.012	(0.020)	9426
Freezer	0.093	0.015	(0.012)	9412
Washing machine	0.197	0.029*	(0.016)	9435
Dishwasher	0.002	0.004**	(0.002)	9429
TV	0.797	0.019	(0.017)	9434
VCR	0.040	0.009	(0.008)	9434
Cable TV	0.134	0.021	(0.013)	9435
Computer	0.010	0.004	(0.004)	9433
Car	0.032	0.004	(0.007)	9435
Home owned	0.500	-0.007	(0.021)	9421
Number of rooms	2.391	0.159**	(0.073)	9434
Number of bedrooms	1.715	0.093***	(0.035)	9434
Receipt of *Plan de Equidad*	0.871	-0.023	(0.015)	9287

Note: In Column (2) we report estimates of Equation 1 using different covariates at baseline as outcome variables for the subsample of children exposed during early-childhood. Estimates are obtained using a bandwidth of 0.075 around the threshold and a first order polynomial. Standard errors (s.e.) are reported in parentheses. N corresponds to number of observations. * p<.1, ** p<.05, *** p<.01.

Table A6: Estimates of the effect of the PANES transfer on different covariates using baseline data for subsample of children exposed while in-utero

	Non-eligible mean (1)	Coefficient (2)	s.e. (3)	N (4)
Child's indicators				
Child is a boy	0.509	0.007	(0.020)	10428
Age in months in Dec 2007	26.885	0.553	(0.638)	10428
Mother's indicators				
Age	24.765	0.012	(0.258)	10242
Complete primary education	0.919	0.018	(0.011)	10404
Complete secondary education	0.027	-0.001	(0.006)	10404
Household's indicators				
Hot water	0.278	-0.001	(0.017)	10425
Heater	0.188	0.008	(0.015)	10417
Kitchen	0.674	0.001	(0.019)	10427
Heating	0.007	-0.002	(0.003)	10412
Concrete floor	0.913	0.003	(0.012)	10264
Mud wall	0.571	-0.013	(0.020)	10323
Block has electricity	0.978	-0.003	(0.006)	10425
Block has piped water	0.943	0.011	(0.009)	10420
Block has sewage	0.401	0.055***	(0.019)	10391
Block has trash collection	0.905	0.019	(0.012)	10413
Block has paved streets	0.668	-0.021	(0.019)	10394
Block has sidewalk	0.700	-0.003	(0.018)	10397
House	0.875	-0.023*	(0.014)	10260
Microwave	0.045	-0.002	(0.007)	10428
Refrigerator	0.646	0.023	(0.019)	10422
Freezer	0.092	0.002	(0.011)	10412
Washing machine	0.177	-0.026*	(0.014)	10428
Dishwasher	0.002	-0.001	(0.002)	10420
TV	0.785	-0.006	(0.017)	10425
VCR	0.040	0.008	(0.007)	10423
Cable TV	0.134	0.000	(0.012)	10428
Computer	0.009	-0.002	(0.003)	10422
Car	0.029	0.003	(0.006)	10428
Home owned	0.496	-0.006	(0.020)	10410
Number of rooms	2.420	-0.038	(0.080)	10427
Number of bedrooms	1.730	0.004	(0.032)	10427
Receipt of *Plan de Equidad*	0.744	0.010	(0.176)	10105

Note: In Column (2) we report estimates of Equation 1 using different covariates at baseline as outcome variables for the subsample of children exposed while in-utero. Estimates are obtained using a bandwidth of 0.075 around the threshold and a first order polynomial. Standard errors (s.e.) are reported in parentheses. N corresponds to number of observations. * p<.1, ** p<.05, *** p<.01.

Table A7: Effect of receiving the PANES transfer on the likelihood of enrolling in seventh grade of primary school without delay

	Likelihood of enrolling in sixth grade with no delay	
	(1)	(2)
Panel A: All observations		
Coefficient	0.058*	0.057**
s.e.	(0.033)	(0.029)
Observations	7552	9720
CCT bandwidth	0.043	0.054
Panel B: Exposed during early childhood		
Coefficient	0.083**	0.072**
s.e.	(0.039)	(0.035)
Observations	5518	6640
CCT bandwidth	0.045	0.053
Panel C: Exposed while in-utero		
Coefficient	0.011	0.030
s.e.	(0.056)	(0.054)
Observations	2454	2635
CCT bandwidth	0.047	0.050
Controls	No	Yes

Note: Each cell corresponds to a different regression. In Panel A we use the sample of children whose family received the PANES transfer during early-childhood. In Panel B we use the sample of children whose family received the PANES transfer while the child was in-utero. In Panel C we use all observations. We estimate Equation 1 using the likelihood of enrolling in seventh grade of primary school with no delay as outcome variable. We report the estimates obtained when using the CCT bandwidth and polynomial defined according to Calonico et al. (2014). CCT bandwidths are reported below each coefficient and CCT polynomial is 1 in all cases. All estimations include month times year of birth fixed effects, and month times year of baseline visit fixed effects. In Column (2) we present results of estimating Equation 1 using the following additional controls: gender of the child, an indicator for whether the mother completed primary school, number of bedrooms in the household and indicators for whether the household's block has sewage and trash collection. All controls are included as indicator variables and include a category for missing observations. Standard errors (s.e.) are reported in parentheses and number of observations are reported below each coefficient. * p<.1, ** p<.05, *** p<.01.

Table A8: 2SLS estimates of the effect of the PANES transfer during early-childhood on educational outcomes for fixed bandwidths

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Highest grade attained						
1. No controls	0.043	0.045	0.117**	0.117**	0.118*	0.117*
	(0.043)	(0.043)	(0.050)	(0.050)	(0.064)	(0.064)
Observations	12198	12198	9435	9435	6277	6277
2. Controls	0.053	0.128**	0.128**	0.137**	0.135**	0.024**
	(0.044)	(0.051)	(0.051)	(0.065)	(0.065)	(0.010)
Observations	12198	12198	9435	9435	6277	6277
Panel B: Delay in educational attainment						
1. No controls	-0.034	-0.035	-0.067**	-0.067**	-0.056	-0.056
	(0.024)	(0.024)	(0.028)	(0.028)	(0.036)	(0.036)
Observations	12198	12198	9435	9435	6277	6277
2. Controls	-0.040	-0.073***	-0.073***	-0.066*	-0.065*	-0.006*
	(0.024)	(0.028)	(0.028)	(0.036)	(0.036)	(0.003)
Observations	12198	12198	9435	9435	6277	6277
Panel C: Dropout						
1. No controls	0.014*	0.014*	0.003	0.003	0.007	0.007
	(0.008)	(0.008)	(0.009)	(0.009)	(0.012)	(0.012)
Observations	12198	12198	9435	9435	6277	6277
2. Controls	0.014*	0.003	0.003	0.008	0.008	-0.005
	(0.008)	(0.009)	(0.009)	(0.012)	(0.012)	(0.010)
Observations	12198	12198	9435	9435	6277	6277
Range	0.1	0.1	0.075	0.075	0.05	0.05
Order of polynomial	1	2	1	2	1	2

Note: Each cell corresponds to a different regression. Sample includes children born before the program period. Each Panel corresponds to a different outcome. In Columns (1)-(6) we estimate Equation 1 using Ordinary Least Squares. We use three different ranges around the eligibility threshold and two different orders of polynomial. All estimations include month times year of birth fixed effects, and month times year of baseline visit fixed effects. Row 1 presents regressions with no additional controls while row 2 reports results with the following additional controls: gender of the child, an indicator for whether the mother completed primary school, number of bedrooms in the household and indicators for whether the household's block has sewage and trash collection. Controls are included as indicator variables and include a category for missing observations. Standard errors are reported in parentheses and number of observations are reported below each coefficient. * p<.1, ** p<.05, *** p<.01.

Table A9: 2SLS estimates of the effect of the PANES transfer while in-utero on educational outcomes for fixed bandwidths

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Highest grade attained						
1. No controls	-0.039	-0.039	-0.001	-0.001	-0.006	-0.007
	(0.029)	(0.029)	(0.033)	(0.033)	(0.042)	(0.042)
Observations	13424	13424	10428	10428	6985	6985
2. Controls	-0.039	0.007	0.007	-0.012	-0.014	0.007
	(0.029)	(0.034)	(0.034)	(0.043)	(0.043)	(0.009)
Observations	13424	13424	10428	10428	6985	6985
Panel B: Delay in educational attainment						
1. No controls	0.021	0.021	-0.010	-0.010	-0.012	-0.011
	(0.020)	(0.020)	(0.023)	(0.023)	(0.030)	(0.030)
Observations	13424	13424	10428	10428	6985	6985
2. Controls	0.020	-0.016	-0.016	-0.008	-0.007	-0.009***
	(0.020)	(0.024)	(0.024)	(0.030)	(0.030)	(0.003)
Observations	13424	13424	10428	10428	6985	6985
Panel C: Dropout						
1. No controls	-0.008	-0.008	-0.010	-0.010	-0.008	-0.008
	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
Observations	13424	13424	10428	10428	6985	6985
2. Controls	-0.008	-0.010	-0.010	-0.009	-0.009	0.012
	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)	(0.015)
Observations	13424	13424	10428	10428	6985	6985
Range	0.1	0.1	0.075	0.075	0.05	0.05
Order of polynomial	1	2	1	2	1	2

Note: Each cell corresponds to a different regression. Sample includes births that occurred in the program period. Each Panel corresponds to a different outcome. In Columns (1)-(6) we estimate Equation 1 using Ordinary Least Squares. We use three different ranges around the eligibility threshold and two different orders of polynomial. All estimations include month times year of birth fixed effects, and month times year of baseline visit fixed effects. Row 1 presents regressions with no additional controls while row 2 reports results with the following additional controls: gender of the child, an indicator for whether the mother completed primary school, number of bedrooms in the household and indicators for whether the household's block has sewage and trash collection. Controls are included as indicator variables and include a category for missing observations. Standard errors are reported in parentheses and number of observations are reported below each coefficient. * p<.1, ** p<.05, *** p<.01.

B Results on low birth weight using a difference in discontinuity design

Following Amarante et al. (2016), in this paper we report results of estimating the effect of the PANES program on low birth weight using a localized difference in differences estimator. Amarante et al. (2016) implement this methodology to add more observations to the estimation and improve precision. Since we can observe health at birth outcomes of children born during the pre-program period (before May 2005) and during the program period (between May 2005 and December 2007) in our data, we can take the difference of the local average treatment effect between pre and post treatment discontinuities. More specifically, we focus on changes in outcomes among eligible versus ineligible mothers/children across the pre-program and program period within a close neighborhood of the eligibility threshold. The estimator is then:

$$\begin{aligned}(E[Y|T_m = 1, D_{imt} = 1] - E[Y|T_m = 0, D_{imt} = 1])\\ - (E[Y|T_m = 1, D_{imt} = 0] - E[Y|T_m = 0, D_{imt} = 0])\end{aligned}$$

where D_{imt} is an indicator for births that took place during the program period and $E[Y|T_m = 1, D_{imt} = 1]$ is the average outcome for children born during the program period in a treated household, $E[Y|T_m = 0, D_{imt} = 1]$ is the average outcome for children born during the program period in a control household, $E[Y|T_m = 1, D_{imt} = 0]$ is the average outcome for children born before the program period in a treated household, and $E[Y|T_m = 0, D_{imt} = 0]$ is the average outcome for children born before the program period in a control household.

To implement this, we estimate the following regression with instrumental variables:

$$Y_{imt} = \beta_0 + \beta_1 D_{imt} + \beta_2 T_m + \beta_3 T_m \cdot D_{imt} + f(N_m) + f(N_m \cdot T_m) + e_{imt} \quad (2)$$

We instrument T_m and $T_m \cdot D_{imt}$ with E_m and $E_m \cdot D_{imt}$, where E_m is an indicator for the mother's PANES eligibility, that is, $E_m = 1(N_m{>}0)$. Our parameter of interest is β_3 and it measures the average difference in outcomes among children born in eligible and ineligible households across the pre-program. We comment on the validity of this strategy below.

The localized difference in differences approach is valid if: (i) the regression discontinuity identifying assumptions are satisfied, (ii) the difference in differences identifying assumptions are satisfied and (iii) in expectation, the Local Average Treatment Effect (LATE) of treatment effect is the same in the pre-program and program period (Jackson, 2019). In Section 4 we showed evidence that (i) is likely satisfied. In addition, in Appendix Table A10 we report regressions for outcomes during the entire pre-program period. We find no evidence of significant differences in the incidence of low birth weight during pre-program pregnancies. This evidence argues against systematic sorting around the discontinuity. Below we discuss (ii) and (iii).

Table A10: 2SLS Estimates of the effect of the PANES transfer on low birth weight (<2.500 kg) pre-program data

	(1)	(2)	(3)	(4)	(5)	(6)
1. No Controls	0.003	-0.008	-0.000	-0.025	-0.008	-0.040
	(0.008)	(0.013)	(0.014)	(0.021)	(0.016)	(0.025)
Observations	27835	27835	12102	12102	9362	9362
2. Controls	-0.002	-0.005	-0.004	-0.022	-0.009	-0.035
	(0.009)	(0.013)	(0.013)	(0.021)	(0.015)	(0.025)
Observations	27835	27835	12102	12102	9362	9362
Range	All	All	0.1	0.1	0.075	0.075
Order of polynomial	1	2	1	2	1	2

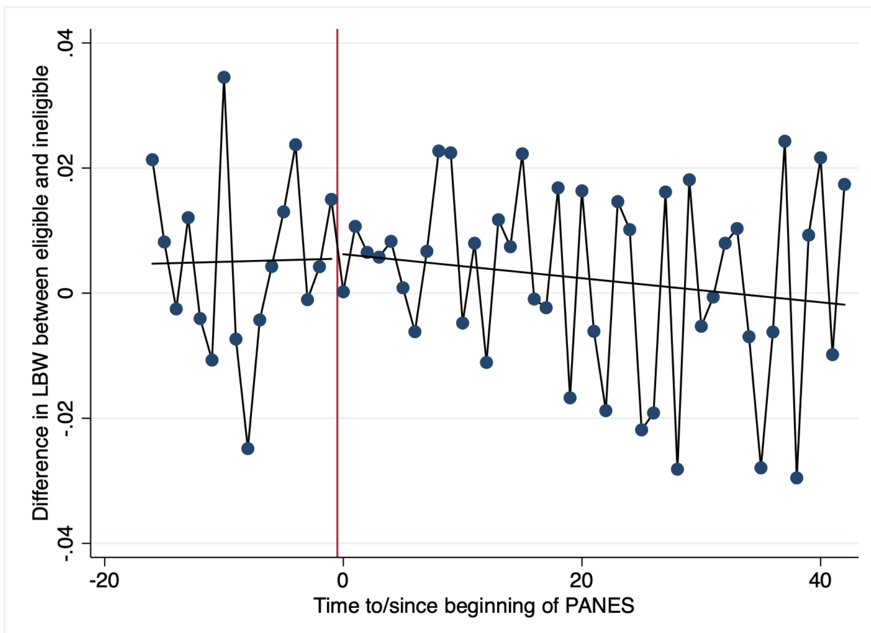
Note: Each cell corresponds to a different regression. Sample includes pre-program data only. In Columns (1)-(6) we estimate Equation 2 for three different ranges around the eligibility threshold and two different orders of polynomial. All estimations include month times year of birth fixed effects, and month times year of baseline visit fixed effects. Row 1 presents regressions with no additional controls while row 2 reports results with the following additional controls: gender of the child, an indicator for whether the mother completed primary school, indicators for geographic department of the household at baseline, for whether the household has centralized hot water, heater, kitchen, microwave, refrigerator, freezer, washing machine, dishwasher, TV, VCR, cable TV, computer, car, whether the block has electricity, piped water, sewage, trash collection, paved streets, sidewalk, whether the home is a house, is owned, and indicators for material of the floor and walls. Controls are included as indicator variables and include a category for missing observations. Standard errors are reported in parentheses and number of observations are reported below each coefficient. * p<.1, ** p<.05, *** p<.01.

The localized difference in differences is valid if there were no changes in eligible households that coincided with eligibility to the program. One may worry that health at birth (incidence of low birth weight) was already improving (decreasing) among eligible households prior to the program. To assess this, Figure A3 plots differences in low birth weight (LBW) outcomes between eligible and ineligible mothers giving birth at different months.[31] The x-axis corresponds to the months to and since the beginning of the PANES program (April

[31] We consider the entire range of the wealth index because, in a given month, the number of observations is significantly reduced when considering smaller bandwidths.

2005). Each dot represents the coefficient of the interaction between treatment status and month of birth. For example, the first dot indicates that in February 2003 the incidence of low birth weight among children born from eligible mothers was 2.1 percentage points higher than among those born from ineligible mothers. The solid blue line shows the trend for the difference in low birth weight between eligible and ineligible children. The trend remains constant in approximately 0.05 and there is no indication of a decreasing trend in the pre-program period. This result supports the claim that there were no other changes in health at birth among eligible households in the pre-program period.

Figure A3: Difference in the incidence of low birth weight between eligible and ineligible households



Note: The horizontal axis represents time to/since the beginning of the PANES program in months. The vertical line corresponds to the beginning of the PANES program. Each dot represents the difference in the incidence of low birth weight between children born in eligible and ineligible households in each month. The two solid lines represent the best fit from a linear regression from each side of the cut-off.

Also, the localized difference in differences estimates represent a causal effect of the PANES program if the effect of β_3 is homogeneous. This means that the effect of receiving the transfer while in-utero should be the same for children born in the pre-program period and for children born in the program period. In our setting, our treatment groups and our control groups contain the same households, easing concerns of mothers differing systematically across periods. However, the localized difference in differences strategy compares children's outcomes among eligible and ineligible households across the pre-program and program period, two very different periods for Uruguay. In 2002, Uruguay was hit by a severe economic crisis and between 2003 and 2005 the economic situation of the country was very adverse. In the period previous to the program, Uruguay started recovering and households' economic situation improved in general. For example, while GDP per capita was on average 8500 USD between 2003 and 2005, it averaged 9500 USD between 2005 and 2007. The unemployment rate also improved as it decreased from 16.7% in 2003 to 9.4% in 2007. The impact of receiving a cash transfer in a context of a severe economic crisis could be different than the one of receiving a transfer when the country is in a better economic situation. Therefore, it is possible that the homogeneity assumption is not valid in this setting.