

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

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July 16, 2024

## Abstract

We evaluate the long-term effects of receiving the Uruguayan *Plan de Atención Nacional a la Emergencia Social* (PANES), a large unconditional cash transfer program, on outcomes for 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 exposed to the program during 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 in welfare programs aimed at improving conditions in early life, especially in developing countries. While it is particularly interesting for policymakers to know how effective these policies are, we are still at the beginning stages of understanding 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 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 the long-term effects of interventions during early childhood poses 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 based on 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 the 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 to later-life outcomes. We make use of a rich dataset that we constructed for this project, which 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 (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 who were exposed to the program between the ages of 0 and 5 (those in group (i)) and children who were exposed to the program while in-utero and up to a maximum of two years and eight months (those in group (ii)).

Separating the analysis into children who were exposed to cash transfers since the in-utero period and children who were exposed later in life (but still in early childhood) enables us to examine 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 who received transfers while in-utero and after birth would benefit more from the program than those who received transfers only after birth. On the other hand, those who were exposed to the transfer later in life were born during an economic crisis and were at higher risk: the likelihood of being born with low birth weight was 0.083 in the pre-program period, while it was 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 who 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 who 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 slightly higher educational attainment and a lower incidence of delay than ineligible children. Additionally, 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 retention and not through dropout.

Following Heckman’s model of dynamic complementarity, one would expect that children who received transfers since the in-utero period should have stronger effects on education than those who 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 for children who are born with more risk. We further explore this issue by estimating heterogeneous effects

by low birth weight status among children born in the pre-program period. We find that the effects of PANES on educational attainment are stronger among children born with low birth weight.

We find no effects of PANES on the long-term educational results of children who were in their mothers' wombs 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](#))<sup>1</sup> and the importance of health at birth for later educational outcomes,<sup>2</sup> 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.<sup>3</sup>

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 Mexico's well-known 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 school 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). The positive impacts during early adulthood of childhood exposure to Progresa are mostly concentrated among women ([Parker and Vogl, 2023](#)).

Unconditional cash transfers are not tied to any particular behavior and thus provide

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<sup>1</sup>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](#)).

<sup>2</sup>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 the 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](#)).

<sup>3</sup>[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.

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 it is growing. Unconditional cash transfers have been shown 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 outcomes associated with conditions is inferior relative to conditional cash transfers (Baird et al., 2011).<sup>4</sup> Moreover, there is evidence that while unconditional cash transfers improve immediate welfare, additional interventions are necessary for lasting poverty reductions (Handa et al., 2019).

Longer-term analyses are especially important for cash transfers since these programs aim to reduce future poverty by augmenting the human-capital levels of children and youth from poor families. However, while there is sufficient evidence of the impact of cash transfer 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.

Overall, the sparse evidence on the long-term effects of unconditional cash transfers shows no or little effect on education. Two examples from 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 nine 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 for 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 two 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.<sup>5</sup> Moreover, we study the case of Uruguay,

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<sup>4</sup>Benhassine et al. (2015) show that transfers can serve as an effective policy tool to promote education if they are labeled as explicitly intended to be used for educational purposes. Labeled cash transfers are a middle ground between unconditional cash transfers and conditional cash transfers, leveraging the power of labeling to influence behavior without imposing rigid requirements.

<sup>5</sup>For example, a recent study for Nicaragua shows that cash transfers to families with children aged 7–13 led to higher market participation and earnings (for men) 10 years after its implementation (Barham et al.,

a middle-income country in Latin America whose population and economic setting are very different from those in Africa. Second, a novel angle of the paper rests on the age of the children considered. 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). 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 from an even earlier stage: the in-utero period. In this way, we provide evidence to complement the findings in Araujo and Macours (2021), the first long-term evaluation of a cash transfer implemented at scale during the first 1000 days of life.

Beyond the cash transfer literature, this paper relates to the literature that links 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 from April 2005 to December 2007 in Uruguay, a middle-income country in Latin America.<sup>6</sup> The program targeted the poorest households in the country. PANES was designed as an emergency plan to alleviate material hardship caused by 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 oversee the implementation of the program.

Program eligibility was based on families' scores on a poverty index. All applicant households were visited by personnel from the Ministry of Social Development and completed a detailed baseline survey, which allowed program officials to compute the score. The score

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2024).

<sup>6</sup>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 significant challenge for the Uruguayan government.

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.<sup>7</sup> The variables considered, the weights attached to the observed covariates, and the eligibility thresholds varied slightly across different geographic regions. Applicants were not aware of the variables that entered the score, the weights attached to them, or the eligibility criteria, easing concerns about manipulation of the score.

Rather than using actual reported income, the score was estimated using a wide range of socioeconomic variables. This approach was adopted because 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 approximately 700,000 individuals) submitted applications. After the interviewing process, households were ranked according to their level of deprivation based on their predicted poverty score. Households whose score was above a predetermined threshold were assigned to the program. About 54% of applicant households became beneficiaries, representing nearly 10% of households in the country. Regardless of their characteristics, eligible households received a monthly cash transfer that initially 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.<sup>8 9</sup>

The condition for continuing to receive the payment was that household income (from all sources) remained below a specific per capita level. In practice, only verifiable sources of income were taken into account. Successive checks were carried out by the social security ad-

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<sup>7</sup>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.

<sup>8</sup>This figure was calculated using the Uruguayan Continuous Household Survey of 2004. Similar results are obtained when using the 2005 wave. 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 represents slightly more than 50% of monthly average household income among the poorest 10 percent of households that do not live in Montevideo and slightly less than 40% of monthly average household income among the poorest 10 percent of households that live in Montevideo. With respect to the whole income distribution of the country, the transfer represents 9% of monthly household average income.

<sup>9</sup>Our calculations are in 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%).



ministration to enforce this condition, and because of this, some households stopped receiving the transfer before the end of the program.<sup>10</sup> 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 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 in addition to the cash transfer. The food card operated through an electronic debit card, and its value represented between 22% and 59% 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 cash transfer and health care reform (*Plan de Equidad*) was launched in January 2008. Households did not need to reapply for the new program. The government informed households about the end of PANES and the start of the new program via mail, and eligible households received a formal written communication. Eligibility for the *Plan de Equidad* was based on a new score 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 from PANES; it became less restrictive and expanded the beneficiary base. Members from both eligible and ineligible households in PANES became eligible for the new program. In 79% of PANES applicant households, at least one household member became eligible for the *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 the *Plan de Equidad*. In this sense, our analysis resembles previous studies on Progres a in Mexico, which evolved into Oportunidades and later into Prospera (Araujo and Macours, 2021).

### 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 to match these three sources. In this section, we describe the data sources used and provide

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<sup>10</sup>Households that became ineligible before the end of the program are still considered part of the treatment group.



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 retained information only from the first visit to ease concerns about strategic behaviors to gain eligibility. The key variables 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.

#### Birth Data

We combine information from PANES administrative records with all registered live births in Uruguay from birth certificates (*Certificado de Nacido Vivo*) for 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, which we used to match them with females in PANES applicant households. However, the identity numbers of children were not available in birth certificates. To obtain this information, we used additional records from MIDES that contain identification numbers of mothers and children who applied for or received any social program.<sup>11</sup> We matched this 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 determine which identification number corresponded to each child. Because this information was crucial for linking birth data with education data, we had to drop observations from multiple births (1% of the sample).<sup>12</sup> The vital statistics natality data includes information on health at birth, the mother’s reproductive history, parental characteristics, and prenatal health care utilization.

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<sup>11</sup>This does not necessarily mean that all households in the analysis received social benefits by definition. Almost all PANES applicants were visited by MIDES at another point in time due to other program applications.

<sup>12</sup>Within the program period, multiple births were equally likely for PANES recipients as for controls, easing concerns of selection bias. Infants born in multiple births have, on average, lower birth weights than those born in single-order births, so our results may be sensitive to the inclusion of twins, triplets, and higher-order births.

## Education Data

Finally, we use children’s identification numbers to obtain information on 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 data 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. The highest grade attained corresponds to the grade the child had reached by 2017, the last year for which we have information, and ranges from 1 to 10, with 1 being preschool and 10 the last year of middle school.<sup>13</sup> If the child’s record is missing in 2017, we take the highest grade attained by the child in the period we observe her. Delay is measured with an indicator that takes value 1 if the child’s highest grade attained in 2017 is lower than the one determined by her year and month of birth and a regular track.<sup>14</sup> Appendix Table A3 shows the corresponding grade that a child should have attained in 2017 according to their 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 we observe her.<sup>15</sup>

The three outcomes we consider capture different elements of students’ educational careers. The highest grade attained shows the overall educational attainment of the child. Delay adds to the latter by also considering information on the year and month of birth of the child. There are two possible explanations for why a child may be enrolled at a lower grade than expected based on her age and a regular track: (i) the child repeated a grade, or (ii) the child did not enroll in school for some years.<sup>16</sup> We explore the possibility of expla-

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<sup>13</sup>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. Children that had not reach Grade 7 in 2017 were excluded from the estimation. The results are qualitatively equivalent to the ones we show in our main tables (see Table A7).

<sup>14</sup>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 corresponding to the level during the school year and that 1/3 of the children do so the following year.

<sup>15</sup>One observation per child is used and we assess all children at a common reference point: the year 2017. For example, one observation corresponds to a child born in 2007 who attained 5 years of education in 2017, and another observation corresponds to a child born in 2003 who attained 9 years of education in 2017.

<sup>16</sup>A third explanation could be that the parents delayed the child’s initial enrollment in 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 do not know in which year they entered school. In our setting, having parents who enroll children in a higher cohort than they should is unlikely to 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 who

nation (ii) using a variable that indicates whether the child dropped out of school for two or more years during the period we observe her.

### 3.2 Descriptive Statistics

Overall, we have information on 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).<sup>17</sup> There is a difference in educational outcomes measured 8 to 12 years after exposure to the program between eligible and ineligible groups. For example, considering the 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 years. 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 years.

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

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enroll early are a minority, and it is more common to see children enrolled late instead.

<sup>17</sup>We are not assessing the balancing properties of the sample in this table. We do so in Table 3 further in the text.

Table 1: Descriptive statistics of outcome variables and selected covariates

	Eligible households			Non eligible households			Difference	
	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Coefficient	s.e.
<b>Panel A: Born in pre-program period</b>								
Child's highest grade attained	22157	7.758	1.016	6751	7.975	1.006	-0.217***	(0.014)
Delay	22157	0.602	0.490	6751	0.482	0.500	0.120***	(0.007)
Dropout	22157	0.025	0.156	6751	0.031	0.172	-0.006**	(0.002)
Mother's number of previous pregnancies	22157	3.381	2.310	6751	2.362	1.662	1.019***	(0.030)
Mother's age at birth	21778	25.432	6.727	6667	24.433	6.445	0.998***	(0.093)
Child's birth weight (BW) in grams	22000	3175.765	516.708	6696	3199.972	518.474	-24.206***	(7.217)
Child has low birth weight (=1 if BW<2500 grams)	22000	0.087	0.282	6696	0.079	0.269	0.008**	(0.004)
Gestational week of birth occurrence	21512	38.637	1.728	6581	38.647	1.783	-0.010	(0.025)
Child was born premature (=1 if gestational weeks<37)	21512	0.082	0.275	6581	0.081	0.273	0.001	(0.004)
Child's Apgar score 1 minute <sup>†</sup>	21923	8.535	1.035	6709	8.550	0.999	-0.015	(0.014)
Child's Apgar score 5 minutes <sup>†</sup>	21929	9.642	0.791	6708	9.649	0.742	-0.007	(0.011)
Mother's number of prenatal controls	21940	6.560	3.366	6678	7.560	3.166	-0.961***	(0.046)
<b>Panel B: Born during program period</b>								
Child's highest grade attained	22221	5.717	1.049	7999	5.823	1.009	-0.106***	(0.014)
Delay	22221	0.382	0.486	7999	0.284	0.451	0.097***	(0.006)
Dropout	22221	0.020	0.141	7999	0.032	0.176	-0.012***	(0.002)
Mother's number of previous pregnancies	22221	3.396	2.344	7999	2.422	1.590	0.974***	(0.028)
Mother's age at birth	21772	25.124	6.824	7873	24.675	6.386	0.449***	(0.088)
Child's birth weight (BW) in grams	22012	3214.934	522.616	7925	3225.874	525.796	-10.940	(6.857)
Child has low birth weight (=1 if BW<2500 grams)	22012	0.077	0.266	7925	0.074	0.262	0.003	(0.003)
Gestational week of birth occurrence	21353	38.648	1.694	7654	38.664	1.686	-0.015	(0.023)
Child was born premature (=1 if gestational weeks<37)	21353	0.079	0.269	7654	0.078	0.268	0.001	(0.004)
Child's Apgar score 1 minute <sup>†</sup>	22005	8.538	0.980	7925	8.516	0.999	0.021*	(0.013)
Child's Apgar score 5 minutes <sup>†</sup>	22004	9.651	0.713	7925	9.641	0.709	0.010	(0.009)
Mother's number of prenatal controls	21887	6.777	3.368	7869	7.728	3.132	-0.951***	(0.043)

Note: Standard errors (s.e.) are reported in parentheses. N corresponds to number of observations. Std. Dev. corresponds to standard deviation. <sup>†</sup>

The Apgar score is a widely accepted method for assessing a newborn's condition right after birth and gauging the effectiveness of any resuscitation efforts, maintaining its status as the benchmark for neonatal evaluation. It is assessed at two different times after birth: at 1 minute and at 5 minutes.

In summary, the 1-minute Apgar score focuses on the neonate's immediate response to birth, while the 5-minute score assesses how the baby is adapting to life outside the womb and the effectiveness of any resuscitative efforts. Higher values represent better health conditions. \* 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 present first-stage estimates of the effect of eligibility for 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 (RDD). This approach takes advantage of the fact that program assignment was determined by a predicted poverty score. Families with a score 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 at the threshold, which we exploit to identify the causal effects of the program. Given that eligibility enforcement is high but not perfect, we estimate program effects using a fuzzy regression discontinuity design.

We compare educational outcomes of children born to households that were just above and just below the threshold. The intuition behind this approach is that households near the cutoff are likely to be similar in all respects except for their treatment status, which is quasi-randomly assigned. Therefore, any significant differences in educational outcomes between the two groups can be attributed to the effect of the PANES program.

The equation we estimate is:

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

where  $Y$  represents the schooling outcome of interest for 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<sup>18</sup>.  $X_{imt}$  include the latter fixed effects and may also include other controls as we mention in the

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<sup>18</sup>The month-times-year-of-birth fixed effects control for shocks affecting both treated and control groups. Cohort and age effects cannot be disentangled since we observe data at a fixed point in time, and all children of the same age belong to the same cohort. The month-times-year-of-baseline-visit fixed effects control for common shocks affecting households when constructing the poverty score.

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,  $E_m$ .  $\alpha_1$  is the parameter of interest.

As in fully randomized experiments, it is not necessary to include covariates in regression discontinuity designs. However, it is common to include them to reduce variability in the estimation (Lee and Lemieux, 2010). In our estimations, we control for covariates,  $X_{imt}$ , at the levels of the child, mother, and household.<sup>19</sup> Controls 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 two subsamples: children exposed to the cash transfer while 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 conventional point estimates combined with robust standard errors, based on the bandwidth and polynomial selected following the approach of Calonico et al. (2014).<sup>20</sup> This approach involves a local polynomial nonparametric estimator with a data-driven bandwidth selector and bias-correction techniques. We refer to this method as "CCT." In most cases, the optimal bandwidth ranges between 0.05 and 0.1, corresponding to differences of 5 to 10 percentage points in the predicted poverty score.

To account for multiple hypotheses across outcomes and sub-populations, we also report the ‘sharpened q-values’ proposed by Benjamini and Hochberg (1995), which control for the false discovery rate. We group highest grade attained and delay together in one family of outcomes and dropout in another. For the sake of brevity, we report q-values only for statistically significant estimates that inform our main results.

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, such as a different probability of low birth weight. Given that the initial application period was concentrated in a relatively short timeframe (75% of applications occurred in the first nine months of the program), it seems unlikely that fertility patterns were influenced in such a period. A related issue is the possibility of fertility responses to the program to retain eligibility. To mitigate concerns about subsequent fertility choices, we use the predicted income score at the initial application as an instrument for program

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<sup>19</sup>We control for covariates that were not used to predict the eligibility score (see Tables A1 and A2), except for those that are unbalanced at baseline. Specifically, we control for the child’s gender, the mother’s educational level, and indicators for whether the household’s block has sewage and trash collection, as well as the number of bedrooms in the household.

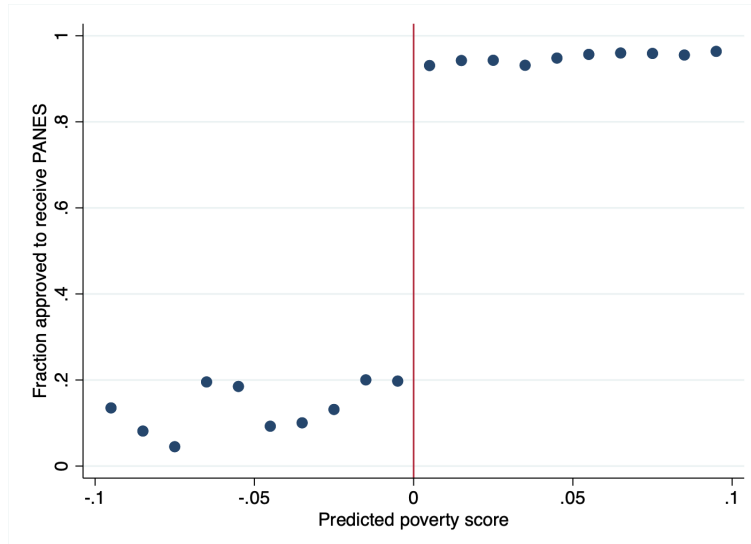
<sup>20</sup>Calonico et al. (2014) incorporates the latest advances in regression discontinuity methods and refines the estimator proposed by Imbens and Kalyanaraman (2012).

receipt, rather than the score at each reassessment of eligibility status, where household circumstances, including childbirth, may have changed.

## 4.2 First-stage effects of the PANES program

Figure 1 shows a clear jump in the fraction of individuals who actually received the PANES transfers.<sup>21</sup> 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.

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.

Table 2 presents first-stage estimates of the effect of eligibility for 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 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 substantial (between 0.70 and 0.76)

<sup>21</sup>Note that the normalized predicted poverty score ranges from -0.19 to 0.95 in our sample.



and remains consistent across specifications.<sup>22</sup> The first-stage estimates become larger when using observations that are further away from the cutoff.

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<sup>22</sup>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

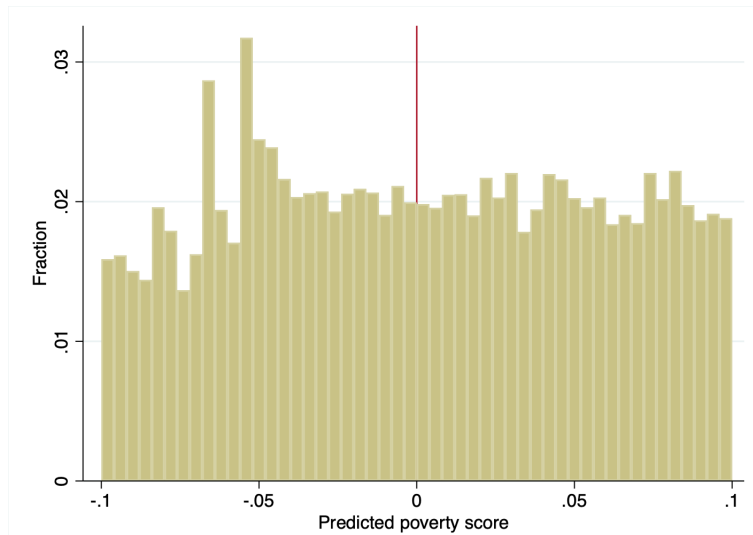
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<b>Panel A: All observations</b>								
Coefficient	0.747***	0.755***	0.741***	0.747***	0.701***	0.717***	0.702***	0.710***
s.e.	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)	(0.013)	(0.012)
Observations	25622	25622	19863	19863	13262	13262	9358	9592
Range	0.1	0.1	0.075	0.075	0.05	0.05	0.036	0.037
<b>Panel B: Exposed during early-childhood</b>								
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
<b>Panel C: Exposed while in-utero</b>								
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
<b>Controls</b>								
	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 for any 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 (Figures A1 and A2 in the Appendix). A visual inspection of the density graphs suggests that bunching does not occur. More formally, we test for bunching by conducting McCrary’s density test (McCrary, 2008) using observations near the threshold.<sup>23</sup> 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.<sup>24</sup> Table 3 reports results from estimating the effect of the PANES program on various covariates at baseline. Most coefficients are small and not significantly different from zero, which aligns with the assumption that assignment around the threshold is as good as random.<sup>25</sup> Moreover, a joint significance

<sup>23</sup>We use observations that have a value of the running variable in the range -0.1 to 0.1.

<sup>24</sup>We use pre-program data for those covariates that are not measured at baseline: birth weight, low birth weight, Apgar score at 1 minute, Apgar score at 5 minutes, age of the mother at birth, number of prenatal controls, gestational weeks, and number of previous pregnancies.

<sup>25</sup>Among the few variables that are unbalanced, the number of previous pregnancies is the most expected

test yields a p-value of 0.159.<sup>26</sup> Most covariates have a strong correlation with the highest grade attained, yet they are balanced between treated and control groups (see Column (5)).<sup>27</sup> Boys have lower educational attainment than girls; being born with low birth weight is negatively correlated with the highest grade attained; and children whose mothers have completed primary education attain higher grades than those with less-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 significance. This is consistent with the identification assumption that assignment around the threshold is as good as random. In the sample of children exposed to PANES during early childhood, coefficients are significant in a few more cases, but the signs 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.

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one. This is because families with more children have a lower income per capita and are therefore more likely to receive the PANES program.

<sup>26</sup>The estimation is performed using pre-program data and considers the optimal bandwidth obtained in Table 2.

<sup>27</sup>We checked these correlations for other educational outcomes, and the conclusion is the same.

## 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.<sup>28</sup> Overall, coefficients go in the expected direction: the effects on the highest grade attained are positive, the effects on educational delay are negative, and the effects on dropout rates are 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). The sharpened q-values cross the 10% threshold. When splitting the sample, we find that eligible children who were exposed to the program during early childhood (Panel B) have higher educational attainment (the p-value in Columns (1) and (2) is 0.08). Additionally, we find that the effect on educational attainment is due to a lower incidence of educational delay (the p-value is 0.06 and 0.08 in Columns (3) and (4), respectively). These effects are robust when accounting for multiple hypotheses testing using sharpened q-values. We find no significant effects on dropout rates. This is unsurprising, given that we are considering children primarily in primary school, where dropout is less likely than in secondary school. Educational attainment of children exposed to the program while in utero (Panel C) is not significantly different between eligible and ineligible households for any of the outcomes considered.

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<sup>28</sup>Note that the number of observations in each regression changes according to the bandwidth. For the same sample, the number of observations changes depending on whether we use controls or not. These changes do 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 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 (1)	Coefficient (2)	s.e. (3)	N (4)	Correlation with outcome (5)
<b><i>Child's indicators</i></b>					
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 score 1 minute <sup>†</sup>	8.575	-0.037	(0.043)	9368	0.026 *** (0.009)
Apgar score 5 minutes <sup>†</sup>	9.675	0.002	(0.031)	9366	0.044 *** (0.013)
Age in months in Dec 2007	31.099	-0.006	(0.008)	19863	0.029(0.019)
<b><i>Mother's indicators</i></b>					
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)
<b><i>Household's indicators</i></b>					
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 <i>Plan de Equidad</i>	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. <sup>†</sup> The Apgar score is a widely accepted method for assessing a newborn's condition right after birth and gauging the effectiveness of any resuscitation efforts, maintaining its status as the benchmark for neonatal evaluation. It is assessed at two different times after birth: at 1 minute and at 5 minutes. In summary, the 1-minute Apgar score focuses on the neonate's immediate response to birth, while the 5-minute score assesses how the baby is adapting to life outside the womb and the effectiveness of any resuscitative efforts. Higher values represent better health conditions. \* p<.1, \*\* p<.05, \*\*\* p<.01.

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

	Highest grade attained		Delay		Dropout	
	(1)	(2)	(3)	(4)	(5)	(6)
<b>Panel A: All observations</b>						
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)
q-value			[0.192]	[0.185]		
Observations	11613	13586	16597	16744	18224	18297
CCT bandwidth	0.045	0.051	0.062	0.063	0.068	0.068
<b>Panel B: Exposed during early-childhood</b>						
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)
q-value	[0.083]	[0.092]	[0.083]	[0.092]		
Observations	6553	7376	7608	7264	8350	9111
CCT bandwidth	0.052	0.058	0.060	0.057	0.066	0.072
<b>Panel C: Exposed while in-utero</b>						
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). Sharpened q-values are reported in squared brackets only for significant coefficients. 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 for children born with a higher risk of weighing 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 thus, who were exposed to the program during early childhood. The first two columns correspond to children born with low birth weight (defined as weighing less than 2500 grams), and the last two columns correspond to children born with normal birth weight. Barely eligible children born with a weight less than 2500 grams have higher educational attainment and a lower incidence of delay compared to barely ineligible children born with the same condition. Despite having 800 observations, the effect is significant at least at the 5% level, indicating that its magnitude is especially large. The effect is also significant when considering sharpened q-values. There is also an effect on dropout, but it goes in the opposite direction than expected. In particular, the likelihood of dropout is higher among barely eligible children born with low birth weight compared to barely ineligible children born with low birth weight. This effect is significant at the 10% level when considering unadjusted p-values, but becomes non-significant or barely significant when considering sharpened q-values in the specifications without and with controls, respectively. Overall, the findings from this analysis suggest that the program had stronger effects on children born when the economic context was more unfavorable and with a higher risk of low birth weight.

## 5.3 Exploring short run impacts of PANES on low birth weight

We found no evidence supporting the idea that the PANES program improved educational attainment for those exposed while in utero. Given the extensive 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 who were born during the program period. Table A10 in the Appendix shows that the subsample of low birth weight children is balanced. A visual inspection of the incidence of low birth weight on 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 they are small in magnitude and not significant.

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

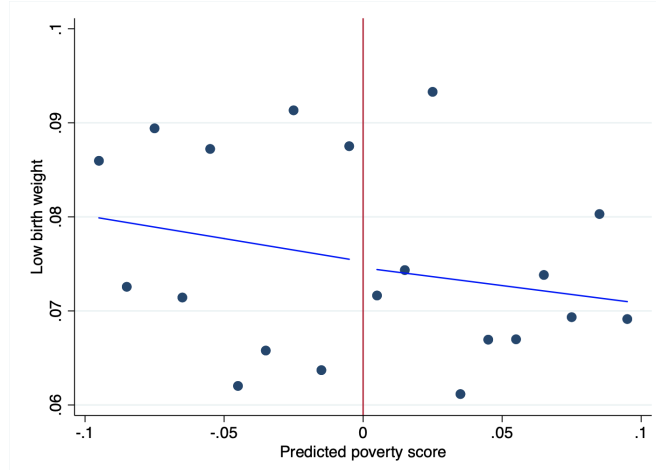
	Born with low birth weight		Born with normal birth weight	
	(1)	(2)	(3)	(4)
<b>Panel A: Highest grade attained</b>				
Coefficient	0.500**	0.529***	0.078	0.070
s.e.	(0.199)	(0.198)	(0.067)	(0.058)
q-values	[0.016]	[0.010]		
Observations	783	761	6936	8434
CCT bandwidth	0.075	0.072	0.060	0.074
<b>Panel B: Delay</b>				
Coefficient	-0.241**	-0.255***	-0.050	-0.040
s.e.	(0.099)	(0.099)	(0.035)	(0.035)
q-values	[0.016]	[0.010]		
Observations	865	848	7923	7657
CCT bandwidth	0.084	0.082	0.068	0.067
<b>Panel C: Dropout</b>				
Coefficient	0.065*	0.069*	0.004	0.003
s.e.	(0.039)	(0.041)	(0.011)	(0.010)
q-values	[0.108]	[0.098]		
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. \(2014\)](#). Sharpened q-values are reported in squared brackets only for significant coefficients. 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. \*  $p < .1$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

Our findings align with [Buser et al. \(2017\)](#), who found no effect on weight and height from receiving a cash transfer in Ecuador. Our conclusion is that low birth weight cannot be considered a first-stage effect for our long-term impacts on education for 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

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.

strategy).<sup>29</sup> Table 7 reports results from estimating Equation 2 (see Appendix B) using low birth weight as the outcome variable.<sup>30</sup> 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 cutoff.<sup>31</sup> These results differ from the findings of [Amarante et al. \(2016\)](#),<sup>32</sup> and we attribute this to the fact that our sample is different. Specifically, our database does not include multiple births, which on average are those born with lower weights.<sup>33</sup> 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. Therefore, our regression discontinuity estimates are our preferred specification.

<sup>29</sup>This method was first formalized by [Grembi et al. \(2016\)](#), but others have used similar empirical strategies in prior literature. [Grembi et al. \(2016\)](#) propose and verify a set of diagnostic tests for this design, referring to this method as a "difference-in-discontinuity design." Identification rests on the difference between two cross-sectional estimators rather than within-unit variation in treatment assignment.

<sup>30</sup>We include an equivalent set of control variables as those used in [Amarante et al. \(2016\)](#).

<sup>31</sup>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.

<sup>32</sup>Note, however, that [Amarante et al. \(2016\)](#) do not find an effect of PANES on low birth weight when using a regression discontinuity strategy.

<sup>33</sup>[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 (0.012)	-0.006 (0.019)	0.000 (0.014)	-0.008 (0.027)	-0.009 (0.018)	-0.008 (0.027)	-0.003 (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 (0.012)	-0.005 (0.019)	0.001 (0.014)	-0.006 (0.027)	-0.009 (0.018)	-0.006 (0.027)	-0.003 (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. Specifically, we examine the effect of being exposed to the Uruguayan PANES during the prenatal period and early childhood on educational outcomes 8 to 12 years later. We use a rich dataset that matches administrative data from three sources, allowing us to distinguish effects for children who were exposed since they were in their mother’s womb and children who were exposed to the program in their first years of life.

Our results show that children from eligible households that started receiving the program after they were born exhibit mild improvements in educational attainment and a reduction in the likelihood of educational delay. These results are significant at the 10% level. We find an increase in educational attainment of 0.1 years of education and a decrease in the likelihood of delay by 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 the target population, the magnitude of the effects of PANES on education is relatively small.

One potential explanation for why we find results for the subsample of relatively older children but not for the relatively younger children is that the former 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 periods. Our interpretation is that, rather than arguing against Heckman’s theory of dynamic complementarities, our findings suggest that the program impacts children born in families close to the eligibility cutoff when children are born in a worse economic situation and with a higher risk of low birth weight.

Another possible explanation for the lack of effects for the group exposed while in utero is that older cohorts are observed for more years in the education system, allowing a higher margin of response. This is specifically relevant for dropout rates, given that in Uruguay these begin to rise significantly during ages 12-14. By the conclusion of this stage, a significant percentage of students, particularly from lower socioeconomic backgrounds, disengage from the education system. In future research, we plan to incorporate more years of education data to observe all students through the end of secondary school, where dropout rates reach 40-50%.

While we are considering a cash transfer in the Latin American context, our results align with studies on the longer-term effects of unconditional cash transfers in Africa ([Haushofer and Shapiro, 2018](#); [Blattman et al., 2020](#)). However, the evidence in this paper contrasts

with findings from other Latin American countries regarding the effect of conditional cash transfers on educational attainment in the longer run ([Millán et al., 2019](#)).



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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
Year of birth	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
	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

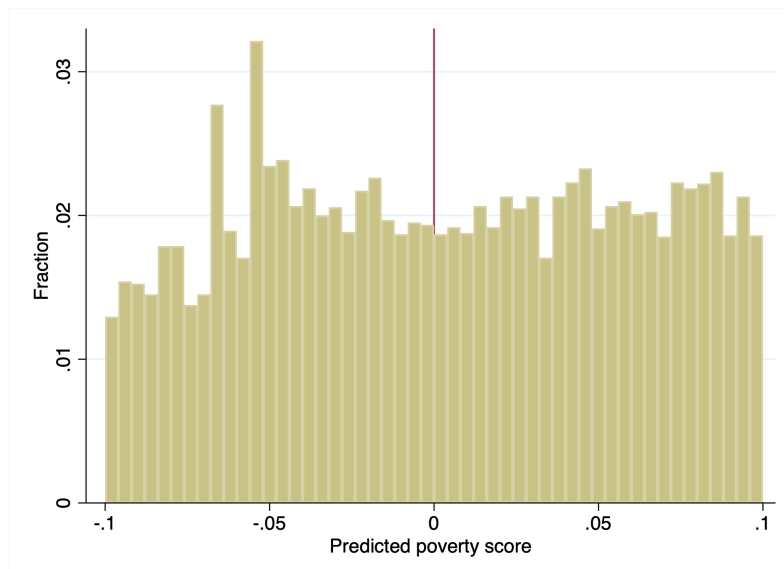


Table A4: First stage estimates of the effect of the eligibility on the PANES cash transfer using a second order polynomial function

	(1)	(2)	(3)	(4)	(5)	(6)
<b>Panel A: All observations</b>						
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
<b>Panel B: Exposed during early-childhood</b>						
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
<b>Panel C: Exposed while in-utero</b>						
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
<b>Controls</b>						
	No	Yes	No	Yes	No	Yes

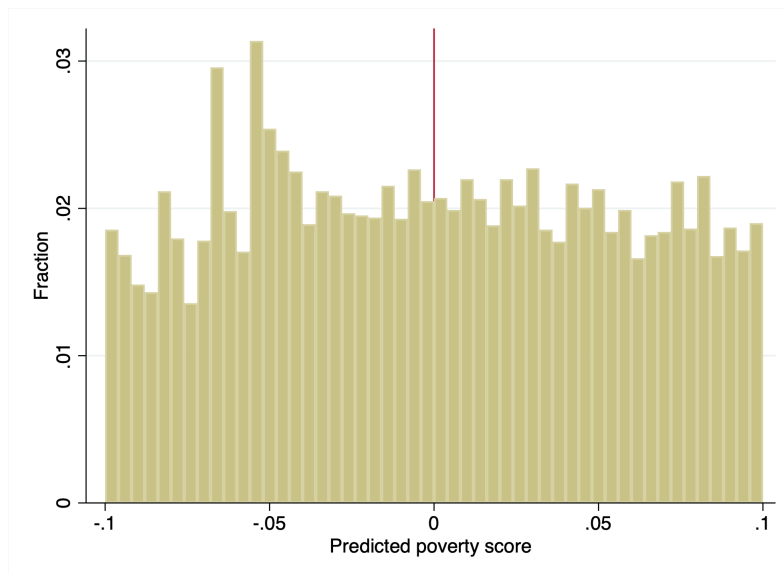
Note: Each cell corresponds to a different regression. In Columns (1)-(6) we estimate Equation 1 using as outcome variable an indicator that takes the value of one if the household received the PANES transfer. We report results for three different fixed ranges around the eligibility threshold and a second 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 (1)	Coefficient (2)	s.e. (3)	N (4)
<b>Child's indicators</b>				
Child is a boy	0.511	0.010	(0.021)	9435
Age in months in Dec 2007	47.018	-0.023	(0.012)	9435
<b>Mother's indicators</b>				
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
<b>Household's indicators</b>				
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 <i>Plan de Equidad</i>	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)
<b>Child's indicators</b>				
Child is a boy	0.509	0.007	(0.020)	10428
Age in months in Dec 2007	16.993	0.010	(0.012)	10428
<b>Mother's indicators</b>				
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
<b>Household's indicators</b>				
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 <i>Plan de Equidad</i>	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 without delay

	Likelihood of enrolling in seventh grade with no delay	
	(1)	(2)
<b>Panel A: All observations</b>		
Coefficient	0.058*	0.057**
s.e.	(0.033)	(0.029)
Observations	7552	9720
CCT bandwidth	0.043	0.054
<b>Panel B: Exposed during early childhood</b>		
Coefficient	0.083**	0.072**
s.e.	(0.039)	(0.035)
Observations	5518	6640
CCT bandwidth	0.045	0.053
<b>Panel C: Exposed while in-utero</b>		
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 with no delay as outcome variable. Children that had not reach Grade 7 in 2017 were excluded from the estimation. 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)
<b>Panel A: Highest grade attained</b>						
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
<b>Panel B: Delay in educational attainment</b>						
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
<b>Panel C: Dropout</b>						
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)
<b>Panel A: Highest grade attained</b>						
1. No controls	-0.039 (0.029)	-0.039 (0.029)	-0.001 (0.033)	-0.001 (0.033)	-0.006 (0.042)	-0.007 (0.042)
Observations	13424	13424	10428	10428	6985	6985
2. Controls	-0.039 (0.029)	0.007 (0.034)	0.007 (0.034)	-0.012 (0.043)	-0.014 (0.043)	0.007 (0.009)
Observations	13424	13424	10428	10428	6985	6985
<b>Panel B: Delay in educational attainment</b>						
1. No controls	0.021 (0.020)	0.021 (0.020)	-0.010 (0.023)	-0.010 (0.023)	-0.012 (0.030)	-0.011 (0.030)
Observations	13424	13424	10428	10428	6985	6985
2. Controls	0.020 (0.020)	-0.016 (0.024)	-0.016 (0.024)	-0.008 (0.030)	-0.007 (0.030)	-0.009*** (0.003)
Observations	13424	13424	10428	10428	6985	6985
<b>Panel C: Dropout</b>						
1. No controls	-0.008 (0.007)	-0.008 (0.007)	-0.010 (0.008)	-0.010 (0.008)	-0.008 (0.010)	-0.008 (0.010)
Observations	13424	13424	10428	10428	6985	6985
2. Controls	-0.008 (0.007)	-0.010 (0.008)	-0.010 (0.008)	-0.009 (0.010)	-0.009 (0.010)	0.012 (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. \*  $p < .1$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .



Table A10: Estimates of the effect of the PANES transfer on different covariates using baseline data for subsample of children born with low birth weight

	Non-eligible mean (1)	Coefficient (2)	s.e. (3)	N (4)
<b><i>Child's indicators</i></b>				
Child is a boy	0.468	0.031	(0.052)	1552
Birth weight	2125.805	34.430	(57.380)	784
Apgar score 1 minute <sup>†</sup>	8.027	0.127	(0.235)	773
Apgar score 5 minutes <sup>†</sup>	9.254	0.173	(0.157)	773
Age in months in Dec 2007	31.682	-0.022	0.031	1552
<b><i>Mother's indicators</i></b>				
Age	24.372	-0.945	(1.012)	768
Complete primary education	0.920	0.016	(0.031)	1549
Complete secondary education	0.033	0.024	(0.017)	1549
Number of prenatal controls	5.512	0.873*	(0.461)	768
Gestational weeks	35.681	0.363	(0.469)	764
Number of previous pregnancies	2.331	-0.240	(0.264)	784
<b><i>Household's indicators</i></b>				
Hot water	0.273	-0.0113	(0.044)	1,552
Heater	0.201	0.056	(0.042)	1552
Kitchen	0.659	0.041	(0.050)	1552
Heating	0.006	-0.001	(0.010)	1552
Concrete floor	0.593	-0.078	(0.052)	1531
Mud wall	0.898	-0.030	(0.032)	1519
Block has electricity	0.977	0.030**	(0.015)	1552
Block has piped water	0.938	0.012	(0.025)	1552
Block has sewage	0.390	0.047	(0.051)	1545
Block has trash collection	0.886	0.017	(0.034)	1547
Block has paved streets	0.628	0.039	(0.050)	1545
Block has sidewalk	0.673	0.052	(0.049)	1546
House	0.855	-0.029	(0.038)	1526
Microwave	0.044	-0.005	(0.019)	1552
Refrigerator	0.631	0.024	(0.051)	1552
Freezer	0.088	0.005	(0.026)	1551
Washing machine	0.181	-0.003	(0.038)	1552
Dishwasher	0.006	-0.010	(0.008)	1551
TV	0.794	-0.064	(0.045)	1552
VCR	0.046	0.002	(0.020)	1552
Cable TV	0.133	0.031	(0.032)	1552
Computer	0.001	0.005	(0.005)	1552
Car	0.025	0.009	(0.016)	1552
Home owned	0.536	-0.087*	(0.052)	1548
Number of rooms	2.330	0.120	(0.123)	1552
Number of bedrooms	1.714	-0.018	(0.079)	1552
Receipt of <i>Plan de Equidad</i>	0.758	0.047	(0.046)	1512

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, 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. Standard errors (s.e.) are reported in parentheses. N corresponds to number of observations. <sup>†</sup> The Apgar score is a widely accepted method for assessing a newborn's condition right after birth and gauging the effectiveness of any resuscitation efforts, maintaining its status as the benchmark for neonatal evaluation. Higher values represent better health conditions. It is assessed at two different times after birth: at 1 minute and at 5 minutes. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## 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:

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

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  $\beta_3$  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) 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 A11 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 A11: 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)	-0.008 (0.013)	-0.000 (0.014)	-0.025 (0.021)	-0.008 (0.016)	-0.040 (0.025)
Observations	27835	27835	12102	12102	9362	9362
2. Controls	-0.002 (0.009)	-0.005 (0.013)	-0.004 (0.013)	-0.022 (0.021)	-0.009 (0.015)	-0.035 (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

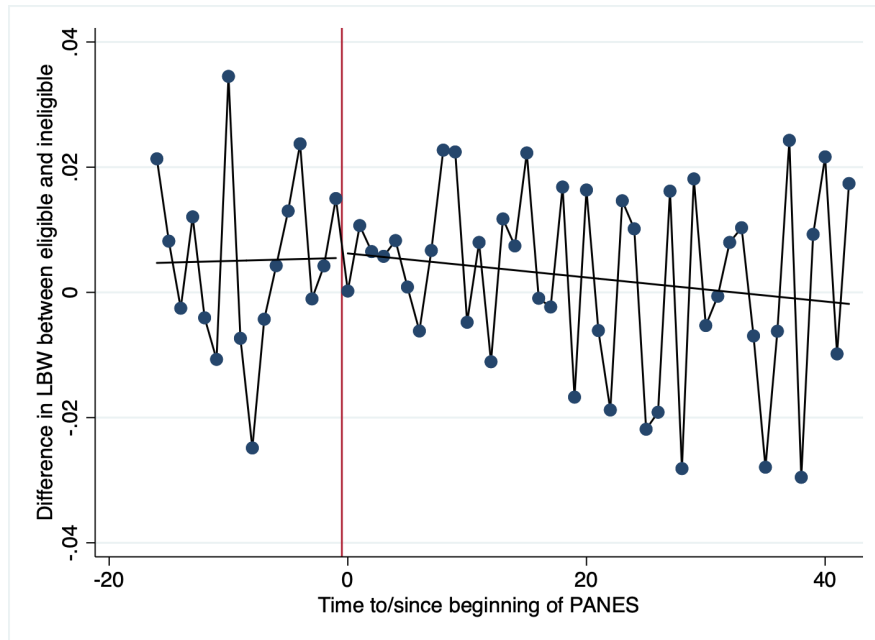
Note: Each cell corresponds to a different regression. Sample includes pre-program data only. In Columns (1)-(6) we estimate Equation 2 for three different ranges around the eligibility threshold and two different orders of polynomial. All estimations include month times year of birth fixed effects, 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.<sup>34</sup> The x-axis corresponds to the months to and since the beginning of the PANES program (April 2005). Each dot represents the coefficient of the interaction between treatment status and

<sup>34</sup>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.

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  $\beta_3$  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 pe-

riod, 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.