Do Cash Transfers Improve Birth Outcomes? Evidence from Matched Vital Statistics, Program and Social Security Data

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Abstract: There is limited empirical evidence on whether cash transfers to poor pregnant women improve children's birth outcomes and potentially help weaken the cycle of intergenerational poverty. Using a unique array of program and social security administrative micro-data matched to longitudinal vital statistics in Uruguay, we estimate that participation in a generous social assistance program led to a sizeable reduction in the incidence of low birthweight. The effect is due to faster intrauterine growth rather than longer gestational length. Our findings are consistent with improved maternal nutrition during pregnancy being a key driver of improved birthweight.

This paper estimates the impact of in utero exposure to a social assistance program – the Uruguayan *Plan de Atención Nacional a la Emergencia Social* (*PANES*) – on children's birth outcomes, and investigates some of the underlying behavioral mechanisms using a rich assortment of matched micro-data from vital statistics, social security and program administrative records. We exploit the fact that program assignment depended on a discontinuous function of a baseline predicted income score. We compare the incidence of low birthweight among infants of "barely eligible" and "barely ineligible" mothers before and after they entered the *PANES* program using a regression discontinuity approach that we combine with a difference-in-differences strategy to improve statistical precision.

Although there is growing evidence that improvements in mother's education lead to improvements in children's birth outcomes, possibly through an increase in mother's permanent income (Currie and Moretti 2003), there is less evidence that temporary interventions in the form of cash transfers to pregnant women significantly affect birth outcomes. Almond and Currie (2011a) conclude that "research has shown little evidence of positive effects of cash welfare on children". This is particularly relevant from the perspective of policymakers if, as many argue, poor birth outcomes have long-lasting adverse impacts on individuals and society.

Children of poor parents are at disproportionate risk of ending up in poverty themselves (Black and Devereux 2011). This is partly due to their poorer health, which both affects the acquisition of other dimensions of human capital (Miguel and Kremer 2004) and can directly impact economic outcomes later in life (Case, Fertig and Paxson 2005). Early interventions, and in particular those *in utero*, have the potential to be particularly cost-effective since their benefits extend over a longer time span, due to potential complementarities with other inputs, and the possibility that they permanently affect the path of individual physiological and cognitive development (Heckman 2000). Improvements in

household financial resources brought about by social assistance can, in principle, increase children's wellbeing through better nutrition, sanitation and health care (Case 2000, Case, Lubotsky and Paxson 2005).

However, there is evidence that offsetting behavioral responses might also be at work. In addition to negative parental labor supply responses to welfare transfers (Moffitt 2002, Hoynes 1996, Hoynes and Schanzenbach 2011), poor parents might favor current consumption over investments in their children's human capital due to myopia or self-control problems, imperfect altruism, intergenerational commitment problems, or limited information about the technology of, or returns to, investment in their children's human capital (Jensen 2010). Social assistance could even potentially increase the consumption of certain "bads" (such as cigarettes or alcohol) that negatively affect birth outcomes, could increase the fraction of children born in poor health by creating incentives for poor women to boost their fertility (Currie and Moretti 2008), or could perhaps lead to family break-up (Moffitt 1998), with potentially negative effects on children.

Ultimately, whether cash transfers to poor parents affect children's early health outcomes positively, negatively or at all remains an open empirical question. In this paper, we focus on the effect of cash social assistance during pregnancy on a measure of early life health: low birthweight. The World Health Organization defines low birth weight as weight under 2,500 g (roughly 5.5 pounds). This is a widely available measure, and medical research shows that it is a major predictor of both short-run child morbidity and mortality as well as adult health outcomes (Kramer 1987, 2003, Gluckman and Hanson 2004, 2005, Almond, Chay and Lee 2005), and even the birthweight of the next generation (Painter 2008). Research in economics shows that birthweight also affects life outcomes beyond health, such as IQ, education and earnings (Almond and Currie 2011a, 2011b, Behrman and Rosenzweig 2004, Black, Devereux and Salvanes

2007, Currie and Hyson 1999, Currie and Moretti 2007, Currie 2009), although others claim that the costs of low birthweight might be overstated.[1]

With a few notable exceptions, evidence on the effect of *in utero* exposure to *cash* welfare transfers on birth outcomes, and in particular on low birthweight, is limited. This paucity of credible evidence results from the lack of both adequate micro-data as well as convincing sources of exogenous variation in transfers. The main channels of impact are also poorly understood, again in part due to data limitations.

This paper contributes to filling these gaps. Beyond specifically focusing on a program whose major component was a cash transfer, one of the main contributions of this paper lies in the dataset that we have assembled and the opportunities that it offers for econometric identification of both program effects and channels. We link multiple sources of administrative micro-data to build a monthly longitudinal dataset spanning five years of individual women's pregnancy and birth outcomes, as well the circumstances surrounding these events. In particular, we have information on socio-demographic characteristics, labor market outcomes, and the receipt of program transfers and other public benefits for the universe of female program applicants of childbearing age, approximately 185,000 women and 70,000 births.[2]

To our knowledge, this paper represents the first effort to link the universe of vital statistics data to social assistance transfer program data at the level of individual beneficiaries. In contrast, most existing studies (reviewed below) use either survey data with self-reported birth outcomes, program receipt and income, or rely on geographically aggregated data. Because of the aggregate nature of the

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[1] Almond, Chay and Lee (2005) find modest excess hospitalization costs associated with low birthweight (as opposed to very low birthweight) children, and argue that these effects are overstated in OLS regressions, while Royer (2009) finds modest effects of birthweight on adult outcomes including education.

[2] Although we use the universe of births for the country, note that Uruguay has roughly the same population and annual births as Connecticut, a medium sized U.S. state.

data used in many related studies, identification of program effects also typically relies on differential variation in program eligibility across geographic areas or demographic groups. An obvious drawback of such approaches is the difficulty of ruling out unobserved trends in outcomes that are correlated with program eligibility rates, possibly inducing omitted variable bias. In contrast, we exploit individual-level variation in eligibility generated by exact rules given that program assignment was a discontinuous function of baseline characteristics. This allows us to recover estimates of program impact based on a comparison of *changes* in outcomes between "barely eligible" and "barely ineligible" mothers, i.e. mothers who were most likely to be nearly identical to each other except for their program participation.

To preview our results, we find that the substantial increase in household income generated by the program (approximately 25 percent on average) led to a drop in the incidence of low birthweight of 19 to 25 percent (i.e., 1.9 to 2.4 percentage points on baseline incidence of 10 percent). In two years, the program closed the pre-existing gap in low birthweight incidence between the (worse off) mothers eligible for the cash transfer program and the (slightly better off) mothers who went through the full application process but did not qualify.

We also explore the behavioral channels that might explain these effects, and provide suggestive evidence that improved maternal nutrition during pregnancy likely played a key role. First, although participation in the program was announced to be conditional on prenatal health check-ups, this condition was never widely advertised to the public nor were they enforced by the Uruguayan government. Consistent with this, we find no evidence of increased utilization of prenatal care or better quality care among program beneficiaries. This largely rules out the possibility that program conditions, or in fact any effect of the program operating through improved prenatal care, is driving the reduction in low birthweight.

We next show that a range of other behaviors that might help explain the reduction in low birthweight – including fertility, residential patterns, and receipt of other government transfers – were also not affected by program participation. We similarly rule out that an in-kind transfer in the form of a food card, which was a secondary component of *PANES* only introduced in the second year of the program, had a meaningful additional effect. Rather than contributing to family break-up, *PANES* transfers actually reduced out-of-wedlock births.

Several patterns in the data provide suggestive, although not entirely definitive, evidence that improved maternal nutrition during pregnancy played a role in improving newborns' weight and health. While we unfortunately do not have direct measures of maternal nutrition or food consumption in our sample, secondary data sources indicate that a large share (11.6 percent) of adult females in the *PANES* target population (the bottom income quintile) experienced substantial undernutrition, making this channel plausible. We also show that the birthweight effects are largely driven by a sharp reduction in intrauterine growth retardation (i.e., slower fetal growth at a given gestational age) with no effects on gestational length, i.e., there was no change in the likelihood of premature births. This pattern leans against the possibility that changes in maternal psychological stress or reduced smoking due to the program contributed substantially to the improved birthweight outcomes, as the existing biomedical evidence (reviewed in section 2 below) finds that stress and smoking in pregnancy both lead to reduced gestational length. We also show that the *PANES* program led to a moderate drop in maternal labor supply (although it is sufficiently small that household total income still rises in program households). This slight reduction in work hours might have also contributed to maternal weight gains during pregnancy via a reduction in the mother's energy use.

Although our interpretation of the precise underlying mechanisms remains necessarily speculative, most concerns regarding adverse behavioral responses

appear to be absent in our data. Unrestricted cash transfers during pregnancy were converted into improved child weight and wellbeing at birth, with few negative unintended consequences that we can measure.

I. Determinants of low birthweight and the role of income assistance

A large body of biomedical and economic research identifies maternal nutrition and maternal physical and mental health during pregnancy as major determinants of birth outcomes in general, and low birthweight in particular. The biomedical literature emphasizes that birthweight mainly reflects intrauterine life conditions while increasingly acknowledging that genetic factors play a lesser role (Barker 1990, Gluckman and Hanson 2004, 2005, Painter 2005, 2008).

Mechanically, low birthweight can result from either reduced gestational length or intrauterine growth retardation, IUGR (Kramer 1987, 2003, Gluckman and Hanson 2004). While the medical literature recognizes the effect of poor maternal nutrition and health, cigarette smoking and genetic history on IUGR, somewhat less is known about the determinants of prematurity, with mother's pre-pregnancy weight, previous history of prematurity and cigarette smoking being well-recognized risk factors (Clausson, Lichtenstein and Cnattingius 2000, Kramer 1987, 2003, Murtaugh and Weingart 1995).[3]

A considerable amount of evidence on the determinants of low birthweight comes from the economics literature. Almond and Mazumder (2011) in particular show that maternal fasting during Ramadan has negative effects on birthweight (on the order of 40 grams). This is direct evidence that even moderate changes in maternal nutrition during pregnancy can affect birthweight, although other

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[3] The medical literature is less conclusive on the effect of prenatal care on both IUGR and prematurity (Alexander and Korenbrot 1995, Kramer 1987, 2001, McCormick and Siegel 2001). The role of maternal stress and anxiety and genital tract infections on prematurity is also less well established (Kramer 1987).

mechanisms might also be at play in the Ramadan case (i.e., sleep deprivation or changes in work patterns).[4]

The detrimental effect of smoking and environmental pollution on birthweight, and in particular on reduced gestational length, has also been highlighted in economics (Currie 2009, Currie 2011, Currie and Schmieder 2009, Currie and Walker 2010, Del Bono et al. 2012). Similarly, it appears that exposure to violence and maternal stress reduce gestational length and increase low birthweight incidence (Camacho 2008, Aizer, Stroud and Buka 2009, Aizer 2010).

More directly related to our analysis, a body of evidence comes from studies that analyze government welfare and transfer programs. Several studies, largely from the United States, focus on *restricted* programs (i.e., those specifically aimed at improving the nutritional and health status of pregnant women). Bitler and Currie (2004) and Hoynes, Page and Stevens (2011) study the Special Supplemental Nutrition Program for Women, Infants and Children (WIC), which provides food and nutritional advice to pregnant women, and both find that it reduces the incidence of low birthweight infants, with effects largely due to reduced IUGR rather than gestational length. One channel through which WIC appears to have an effect is via greater prenatal care utilization. A limitation of these studies is that in order to control for selection into treatment they use either a simple selection-on-observables strategy or exploit the variation in take-up generated by program roll-out across counties, rather than household treatment variation.

Additional evidence comes from the conditional cash transfers literature. Barber and Gertler (2008) evaluate the impact of the Mexican

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[4] The effects of maternal infections and disease during pregnancy on outcomes at birth and later life have been analyzed, among others, by Almond (2006), Case and Paxson (2009) and Barreca (2010). These studies though mainly focus on infant mortality rather than on birthweight.

Progresa/Oportunidades program on birthweight, exploiting the random initial assignment of the program across communities. In a sample of 840 women, they find a very large reduction in the incidence of low birthweight as self-reported in a survey (of 4.5 percentage points on a base of around 10 percent) which they attribute to better quality prenatal care and the adoption of better health behaviors. The interpretation of their results is admittedly complicated by the fact that the program also increased the local supply of health care, and that it featured health and education conditions.

Other studies exploit the roll-out of the Food Stamps program across U.S. counties, with mixed results. Almond, Hoynes and Schanzenbach (2011a, 2011b) find sizeable and precisely estimated effects of Food Stamps on low birthweight, as well as on health outcomes later in life. They estimate that exposure to the program in the last trimester of pregnancy reduces the incidence of low birthweight by 7 to 8 percent for whites and 5 to 12 percent for blacks, with the effect coming from reduced intrauterine growth retardation rather than longer gestation, consistent with a effects working through maternal nutritional gains. Currie and Moretti (2008) do not find this pattern for California, a fact they explain with increased endogenous fertility among the subset of mothers who were more likely to display worse pregnancy outcomes.

Direct evidence on the effects of *unrestricted* cash transfers (as in the program we study) is scant but remains important, as one cannot automatically presume that cash and in-kind transfers have the same impact. Currie and Cole (1993) focus on participation in the U.S. Aid to Families with Dependent Children (AFDC) program. Despite the fact that AFDC mothers were also more likely to receive Medicaid, Food Stamps and housing subsidies, all of which could improve birth outcomes (e.g., see Currie and Gruber 1996 on Medicaid), they find no significant effects on low birthweight. Again, the ability to draw strong

conclusions is partly hampered by an identification strategy that relies on differential AFDC eligibility criteria across U.S. states.

A relevant paper is Hoynes, Miller and Simon (2013), which focuses on the effect of the U.S. Earned Income Tax Credit (EITC) on birthweight. Exploiting the differential effects of subsequent EITC reforms on children born at different parities, as well as changes in state-level program generosity over time, they use a difference-in-differences approach with grouped data to show that EITC led to an average reduction of 7 percent in low birthweight incidence, with more pronounced effects among less educated and ethnic minority mothers. Interestingly, their paper shows effects on both low birthweight and gestational length, which they in turn attribute to reduced maternal smoking, reduced maternal stress and increased prenatal care utilization.

In sum, a body of evidence from both economics and medical research suggests that maternal nutrition is a key determinant of low birthweight, most likely through its effect on intrauterine growth. Equally important, there is growing evidence that exposure to pollution and mother's smoking during pregnancy, as well as maternal stress, also affect low birthweight, although these effects seems to be mediated at least in part through shorter gestational length.

II. The *PANES* Program

The Uruguayan *Plan de Atención Nacional a la Emergencia Social* (*PANES*) was a temporary social assistance program targeted to the poorest 10 percent of households in the country, implemented between April 2005 and December 2007.[5] The program was devised by the center-left government that took office in

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[5] The program was replaced in January 2008 by a new system of family allowances accompanied by a health care reform and an overhaul of the tax system, together called *Plan de Equidad.* The target population, eligibility rules and assistance levels changed in the follow-up program.

March 2005 following the severe economic crisis of the early 2000's, when per capita income fell by more than 10 percent, unemployment reached its highest level in twenty years, and the poverty rate doubled. The crisis laid bare the weakness of the existing social safety net, which was largely focused on old-age pensions, a fact reflected in marked differences in poverty incidence by age, with nearly 50 percent of children aged zero to five living in poverty compared to just 8 percent for the over sixty-five population (UNDP 2008). Despite a rapid deterioration in living standards during the crisis, Uruguay remained a good performer in terms of infant mortality, birthweight and health care utilization relative to other Latin American countries, with levels not too dissimilar to the U.S. (Appendix Table A1).[6]

A. Program Eligibility

Following an initial program application phase (which mainly occurred in April and May 2005), all applicant households were visited by Ministry of Social Development personnel and administered a detailed *baseline survey*. Because of the large volume of applications and the time needed to administer the survey, household visits took place throughout most of the second half of 2005, sometimes with considerable delay from the original application (Appendix Figure A1).

   The baseline survey allowed program officials to compute a predicted income score based on a linear combination of many household socioeconomic characteristics, which in turn determined program eligibility.[7] Households with a

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[6] In 2005, Uruguay was a middle-income country with annual GDP per capita of US$13,189 (in 2006 PPP), and is home to 3.3 million individuals. This highly urbanized country experienced rapid economic growth in the early 20th century, and was among the first countries in the region with universal primary education and old-age pensions. Uruguay is still among the most developed Latin American countries according to the UNDP Human Development Index.

[7] The eligibility score, which was devised by researchers at the Universidad de la Republica in Montevideo (Amarante, Arim and Vigorito, 2005), including some of the authors of this paper, was based on a probit model of the likelihood of being below a critical per capita income level, using a highly saturated function of household variables, including: the

predicted income score below a predetermined level were assigned to the program. The program was not specifically targeted to pregnant women, nor was child-bearing an eligibility criterion. Neither the enumerators nor households were informed about the variables that entered into the score, the weights attached to them, or the eligibility threshold, easing concerns about score manipulation (also see section 6).

Of the 188,671 applicant households (with around 700,000 individuals), roughly 102,000 households eventually became program beneficiaries, or approximately 10 percent of all Uruguayan households (and 14 percent of the national population). The total cost of the program was approximately US$250 million, i.e. US$2,500 per beneficiary household, and on an annual basis, program spending was equivalent to 0.4 percent of GDP.

B. Program Components

PANES eligible households were entitled to a monthly cash transfer whose value was originally set at US$102 in PPP terms (UY$1,360, equivalent to US$56 in non-PPP terms using the 2005 exchange rate) independent of household size, and was later adjusted for inflation. This amounted to approximately 25 percent of average pre-program household self-reported income for recipient households according to the 2006 household survey data. Many households received the first cash transfer during 2005, although due to the delays in administering the baseline survey, there was considerable variation in the timing of first payments even

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presence of children from different age groups, public employees in the household, pensioners in the household, average years of education among individuals over age 18 and its square, indicators for age of the household head, residential overcrowding, whether the household was renting its residence, toilet facilities and an index of durables ownership. The model was estimated using the 2003 and 2004 National Household Survey (*Encuesta Continua de Hogares*). The resulting coefficient estimates were used to predict a poverty score for each applicant household using PANES baseline survey data. The eligibility thresholds were allowed to vary across five national regions. Although official government documents used a predicted "poverty score", we use a predicted income score, which is simply -1 times the poverty score. This simplifies presentation, as households with higher values of the score are better off, but it obviously makes no difference to the analysis.

among the earliest applicants (Appendix Figure A1). Successful applicants were entitled to the transfer for the duration of the program until December 2007, provided their income (from all sources) remained below a predetermined level (approximately PPP US$100 per month per capita).[8] Indeed, a sizeable share of beneficiaries eventually dropped out of the program as they re-entered the recovering Uruguayan labor market.

A second, smaller program component, only launched midway through the program in mid-2006, was an electronic food card, whose monthly value varied between approximately US$22 and US$60 in PPP terms (UY$300 to 800), or between one fourth and one half of the value of the income transfer, depending on household size and demographic structure (see Appendix B). We return to this feature of the program in the results section, where we show that program impacts were already apparent, and of similar magnitude, even before the food card was introduced. *PANES* did not create new health centers, and while there was some additional financing for existing public health centers, it was not specifically targeted to program beneficiaries.

Similar to other recent Latin American cash transfer programs such as *Progresa/Oportunidades*, *PANES* transfers were originally intended to be conditional on mandatory health checks for pregnant women (as well as health checks and school attendance for children), but these conditionalities were not formally laid out by the government until mid-2007, two years into the program and just months away from the end of *PANES* transfers. Even at that late stage, the conditionalities were *de facto* not enforced due to a lack of coordination among the multiple institutional actors involved. This was eventually

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[8] In practice, the income conditionality criteria only applied to verifiable sources of income, i.e., labor income from formal employment as recorded in social security records, retirement pensions or other government transfers and it explicitly excluded non-contributory old age-pensions. The social security administration performed periodic checks on *PANES* beneficiaries' records to enforce this condition. A non-trivial fraction of beneficiaries stopped receiving the transfer before the end of the program, typically because of their failure to satisfy this income conditionality.

acknowledged by the government and widely discussed in the local press (*El Pais* 2007, *El Espectador* 2007). Indeed, there is no record of any *PANES* household having lost eligibility due to failure to fulfill the conditionalities. Similarly, there is no evidence that program households perceived health checks as a condition for program receipt. In a small sample survey of around 2,000 beneficiary households (Manacorda, Miguel and Vigorito 2011), only 12 percent listed "prenatal visits" as a condition for program receipt. In sum, there were effectively no conditions associated with *PANES* transfers, and we thus treat them as unconditional cash transfers. We return to this issue below, and show that the program did not lead to greater utilization of prenatal care.

III. Data

The analysis brings together several individual-level data sets (Appendix Figure A2). *PANES* administrative records provide information from the initial baseline survey visit for both successful ("eligible") and unsuccessful ("ineligible") applicants on household demographic characteristics, housing conditions, income, labor market participation, schooling, durable asset ownership, and the household's exact predicted income score used to determine eligibility. The data also contain the unique national identification number (*cédula*) for all adult household members, and allow us to identify individuals belonging to the same household at the time of the baseline survey. For successful applicants, the data also provide monthly information on the amount of the cash transfer and, if applicable, the food card.

      PANES program data are matched to vital statistics natality micro-data that provide information on all registered live births in the country (*Instituto Nacional de Estadística* 2009). At 98 percent, the fraction of registered births in Uruguay is the highest in Latin America (UNICEF 2005, Duryea, Olgiati and Stone 2006).

Vital statistics come from certificates completed by physicians and they contain information on parental characteristics, the reproductive history of the mother, prenatal care utilization, and birth outcomes including weight and APGAR scores. Since the confidential version of the data used in this paper includes the mother's *cédula* only from 2003 onwards, we limit the use of vital statistics to 2003-2007. This includes a period before the start of *PANES* (which took place in April 2005).[9]

Finally, we link program and vital statistics data to Social Security records for all members of *PANES* applicant households, again using the unique *cédula* individual number (although we note that we do not have access to social security data for those who did not apply to the program). These data contain monthly information on all sources of formal income, including income from formal employment (for both the self-employed and employees, in both the private and the public sector), and all contributory and non-contributory government transfers, including pensions, unemployment and disability benefits. Social security data are available since March 2004, and thus are available for more than a year before the launch of *PANES* (but starting one year after the earliest observations in the natality files).

The data are summarized in Table 1. The top panel reports averages for the period January 2003 to March 2005 before the start of the program (pre-program period), while the bottom panel reports information for April 2005 to December 2007 (program period). We report pre-program outcomes for three groups of mothers that we define based on their program application status and predicted income score: those who applied and were eligible for *PANES* (column 1), those who went through the full program application process but were unsuccessful

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[9] We do not use post-2007 data, as 2008 saw the introduction of a different system of family allowances and a health care reform (*Plan de Equidad*). There is evidence of differential participation in the new programs by *PANES* eligible and ineligible households, which may confound estimates of the genuine long-term effect of *PANES*.

(column 2), and those who did not apply (column 3).[10,11] Roughly speaking, these three groups correspond to increasingly higher levels of income and socio-economic status.[12]

The data show a clear gradient in birthweight across groups (rows 1 and 2). While among *PANES* eligible mothers the fraction of births below 2,500 grams is 10.2 percent, among non-applicant mothers it is 8.4 percent, and for ineligible applicants it lies in between, at 9.3 percent. There is also a mild gradient in APGAR scores at 1 and 5 minutes after birth (rows 3 and 4). APGAR scores between 8 and 10 are normal, between 4 and 7 are considered low and below 4 are critically low.

There is no appreciable difference in either gestational length or the incidence of premature births (defined as pregnancies of less than 37 weeks) between eligible and ineligible applicant mothers, and there is only modest evidence of longer gestational length among non-applicant mothers (rows 5 and 6). Importantly, this suggests that the greater baseline incidence of low weight births among program eligible mothers is due largely to greater incidence of slow intra-uterine growth rather than prematurity. Using data on all applicants, the incidence of low birthweight is disproportionately concentrated among premature children (Appendix Figure A3), and the figure also shows that such children represent a small fraction of births.

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[10] Note that we define these three groups on program application status, independent of the time when the child was born. The eligible group, for example, includes all children born between January 2003 and December 2007 to mothers who applied and were deemed eligible for *PANES* between March 2005 and December 2007. Note, too, that average differences between *PANES* eligible and ineligible households are often statistically significant in the pre-treatment period. For this reason, in the analysis we focus on households with predicted income scores in a neighborhood of the eligibility threshold, and show that average differences in baseline characteristics between eligible and ineligible households are small and typically not significant in this neighborhood (appendix Table A6).

[11] In the remainder of the analysis, we exclude households for whom the income score was computed before September 24, 2005 because a different formula was used to compute the predicted income before that date. These early households only account for 8.3 percent of all applicant households.

[12] Note that while around 10 percent of households qualify for the program, they contain more than 20 percent of infants due to their higher fertility rates. Consistent with this, in 2004 the official poverty rate among households with children was 53.9 percent, compared to 17.3 percent among childless households.

There is also evidence that *PANES* eligible mothers had the fewest prenatal visits at baseline (6.5 versus 7.5 for ineligible applicants and 8.3 for non-applicant mothers, Table 1, Panel A, row 7, although the average number of visits is still considerable) and that they had their first prenatal visit later in the pregnancy (in week 17, compared to week 16 for ineligible applicants and week 14 for non-applicants, row 8). *PANES* eligible mothers were also more likely to live in areas with lower average birthweight (row 12), more likely to give birth in public health centers (row 14) and less likely to be privately insured (row 15).[13]

The natality files also report additional information on mothers' reproductive history and parents' socio-demographic characteristics, and, as expected, *PANES* eligibility status is negatively correlated with mother's education (row 16) and positively correlated with the number of previous pregnancies (row 17). *PANES* eligible mothers were less likely to be married to the father's child (row 18).[14] *PANES* fathers also display lower levels of education (row 20).

Unsurprisingly, data from the social security records show that *PANES* eligible mothers were also less likely to report being employed during pregnancy (row 21), had lower formal sector earnings during pregnancy (row 22) and belong to households with lower labor and non-labor income (rows 23 to 25). Total household monthly income (including earnings and benefits) from social security records in the first two trimesters of pregnancy is UY$1,164 for *PANES* mothers, and around twice as much for ineligible applicant mothers.[15] Although this figure is likely to underestimate true income levels, as it excludes earnings from

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[13] A universal, *de facto* free, health system of relatively poor quality coexists in Uruguay with mandated employer-provided private insurance. In practice, nearly all formal workers have access to private insurance and medical care.

[14] This fraction is quite high in Uruguay as a whole, with nearly 60 percent of children born out-of-wedlock.

[15] Note that pre-treatment income and other outcomes often differ considerably between the eligible and ineligible households, and this makes any naïve comparison between them after treatment suspect. We thus focus on households in the neighborhood of the eligibility threshold ("barely" eligible vs. ineligible), and show below that pre-treatment characteristics are typically similar and not significantly different in this neighborhood.

informal employment and any non-governmental transfers, it remains very low at approximately US$90 in PPP terms.

The bottom panel of Table 1, Panel B reports data for the program period. Note that the low birthweight gap between eligible and ineligible applicant mothers completely closes during the program period, with the two applicant groups of mothers (columns 1 and 2) showing a lower birthweight incidence of 9.1 percent (row 1). The gap in the APGAR scores also appears to shift slightly in favor of eligible mothers (rows 2 and 3). Neither gestational length nor prematurity change considerably across periods, and, as in the pre-program period, there is no appreciable gradient in these variables across the three groups of mothers (rows 4-5).

Around 97 percent of *PANES* eligible mothers received the program at some point during the period (row 6), although only around 63 percent received it during pregnancy (row 7). This gap is due both to the staggered incorporation of households into the program as well as to some beneficiaries losing eligibility due to their eventual failure to meet the income means test (as discussed above). Although a small share of ineligible mothers also eventually received transfers, initial eligibility remains a very strong predictor of program receipt.[16] *PANES* eligible households do not receive more cash benefits through other government programs (row 15), and in fact receive a somewhat lower level of non-*PANES* transfers, as they also did pre-program (row 24 of Panel A). In all, the gap in total household income between eligible and ineligible households closes substantially although not entirely (row 16), largely due to the *PANES* transfer (row 8).

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[16] In a related paper (Manacorda, Miguel and Vigorito 2011) we present evidence of nearly perfect compliance with the initial eligibility rules. The program enrollment data used in that paper, though, only refer to the period through March 2006. In the data used in the present paper, we find evidence of somewhat laxer enforcement of the eligibility rules in the final six months of the program (namely, the second semester of 2007).

IV. Econometric analysis

We exploit the discontinuous assignment to the *PANES* program as a function of the baseline predicted income score, and compare outcomes of "barely eligible" and "barely ineligible" children in the neighborhood of the eligibility threshold using a fuzzy Regression Discontinuity (RD) estimator. As long as assignment near the eligibility threshold is "as good as random", this approach will yield estimates of the causal impact of program participation on outcomes. This approach accounts for many potential omitted variables, including time-varying concerns such as changing earnings over time, which might lead some households to lose eligibility (this is dealt with by using baseline program eligibility as the key explanatory variable), as well as any mean reversion (which should be equally relevant on both sides of the program eligibility threshold). To identify the effect of interest, we exploit the longitudinal dimension of the data and mainly focus on changes in outcomes among eligible versus ineligible mothers across the pre-program, using a localized difference-in-differences estimator within a close neighborhood of the eligibility threshold. This analytical approach generates more precise estimates than a traditional regression discontinuity (RD) approach that only examines outcomes during the program period. Below we also report such cross-sectional RD estimates using data only during the program period and we show that they deliver very similar though less precise estimates.

Let Y_{imt} denote a birth outcome (e.g., low birthweight) of child i conceived by mother m in month t; T_{imt} denotes an indicator for "treated" births that takes on a value of one if the mother received at least one program transfer during the pregnancy; N_m denotes mother m's predicted income score (normalized relative to the eligibility threshold such that households with negative N_m are eligible for

treatment); and D_{imt} is an indicator for births during the program period. Ignoring other covariates (for presentational parsimony), the regression model is:

(1) $\qquad Y_{imt} = \beta_0 + \beta_1 \, T_{imt} + f(N_m) + \beta_2 E_m + \beta_3 D_{imt} + e_{imt}$

where $f(N_m)$ is function of the predicted income score that is continuous at the threshold ($N_m=0$) and E_m is an indicator for the mother's *PANES* eligibility, namely, $E_m = 1(N_m < 0)$.

We instrument the *PANES* treatment variable T_{imt} in eqn. 1 with an indicator for the mother's program eligibility, E_m, during the program period, i.e., $Z_{imt} = E_m * D_{imt}$. Formally:

(2) $\qquad T_{imt} = \gamma_0 + \gamma_1 \, Z_{imt} + g(N_m) + \gamma_3 \, E_m + \gamma_4 \, D_{imt} + u_{imt}$

As noted, the instrument is based on the baseline household income score regardless of whether the household later stopped receiving transfers, easing concerns about any systematic differences (say, in low birthweight risk) for those who remain in versus leave the program.[17]

There are several challenges associated with implementing this approach. For one, different beneficiary households entered the program in different months even if they completed the application survey at the same time (see Appendix Figure A1, panel C), and hence had different effective exposure to the program ($D_{imt}=1$) in particular periods. One obvious concern is that the month of program entry might be systematically correlated with important household characteristics. Second, the program entry month is only defined for actual program beneficiaries; for ineligible applicants, we only have the month they filled in the application

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[17] A related issue is the possibility of strategic fertility to gain or retain eligibility, however, this seems unlikely. Most program applications were collected in a concentrated time period (see appendix figure A1), leaving little time for any fertility response. Although ineligible households could apply for a reassessment of their eligibility status as their circumstances changed (including child birth), throughout this paper we use the predicted income score at the time of the initial application as the instrument for program receipt, easing concerns about later fertility choices. Note, however, that if those who maintained program eligibility throughout were not a random sample of the initially eligible, this might affect interpretation of the IV estimates. To avoid potential bias that might arise from strategic and endogenous household formation choices, we also drop all observations for individuals who joined applicant households in the second or subsequent baseline survey rounds.

survey. To address both concerns, we assign each household the same program "entry month" for both eligible and ineligible households, namely the first month in which any payment was made to a treatment household with a baseline application survey completed in a particular month. This removes any endogenous component of entry into the program and defines the program entry month analogously for eligible and ineligible households. We thus also include indicators for mother m's month of baseline survey (d_{bm}) as regression controls throughout (see Appendix Figure A1, Panel B for more detail on the survey timing).

In the empirical model, we also condition on month of conception (d_t) indicators, which we define as the date of birth minus the gestational length, both of which are available in the birth records. We prefer to condition on the month of conception rather than month of birth since gestational length could potentially be affected by the program. These month of conception terms also account for any trends in the incidence of low birthweight due to secular improvements in health care quality, in living standards, or in any other factors.

To assess robustness, we report estimates for the entire sample as well as for subsamples within 0.10 and 0.075 of the discontinuity threshold (meaning, respectively, differences of 10 and 7.5 percentage points in the estimated probability of falling below the poverty line relative to the cutoff). We employed different polynomials in the predicted income score (from degree one to degree three), where slopes are allowed to vary on either side of the cutoff, and we also present results with no controls (in row 1 of Table 2) and with the inclusion of a large array of additional controls (in row 2).[18] Following Lee and Card (2008), we

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[18] These include: indicators for 19 geographic *departamentos*, mother's age and education (incomplete primary, complete primary, complete secondary), sex of the newborn, number of previous pregnancies, number of newborns (twins or more), whether the dwelling is a house and whether it is owned, whether the household has centralized hot water, a toilet, a stove, micro-wave, refrigerator, freezer, washing machine, dishwasher, heater, central heating, TV, VCR, cable TV, computer,

also cluster standard errors by the values of the running variable, the predicted income score.[19] Since the income score is the same for all household members, this allows for unrestricted correlation in outcomes among children in the same household, whether from the same or different mothers.

V. Empirical results

A. Main results

Table 2 presents instrumental variable estimates of eqn. 1 where the dependent variable is an indicator for birthweight below 2.5 kg. There is a robust negative effect of participation in the *PANES* program on low birthweight. This result is nearly unchanged for different order polynomials, different ranges of analysis near the eligibility threshold, and whether or not additional covariates are included. Estimated effects range between -0.019 and -0.025 and are robustly statistically significant at over 95 percent confidence.[20, 21]

Appendix Figure A4 plots differences in low birthweight outcomes between eligible and ineligible mothers at different leads and lags from program

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car, or phone, indicators for material of the floor and walls, number of rooms and bedrooms, whether the block has electricity, piped water, sewage, trash collection, paved streets, and public lighting.

[19] There are 37,256, 19,278 and 14,193 groups defined based on the predicted income score, depending on whether one uses the entire sample, restricts to the range -0.1/+0.1, or the range -0.075/+0.075, respectively.

[20] Point estimates remain virtually unchanged if we restrict to the even narrower range (-0.05, 0.05) but estimates are much less precisely estimated (not shown).

[21] The interpretation of our regression estimates is that eligible households in the neighborhood of the program threshold saw an improvement in terms of both income (1,000 pesos) and birth outcomes relative to ineligible households. A question is how this finding squares with the evidence in Table 1, Panel A that among all sample households, and not just those with predicted income scores near the threshold, eligible and ineligible households have a difference in income of around 1,300 pesos (row 25) and a difference in birth outcomes of around 1 percentage point (row 1). During the treatment period (Panel B), differences in income are roughly 700 pesos (row 16) while birth outcomes are roughly equalized (row 1), and thus birth outcomes were effectively equalized across these two groups despite their average incomes not being equalized. There are several potential explanations. One is that *PANES* income might have larger impacts than non-*PANES* income. A second is a non-linear birthweight response to income gains. A third is that total income is measured with error. Such measurement error might arise because we use information on income from official social security records, and this misses income from informal employment. Informal employment is likely to be higher among eligible households due to program eligibility incentives, in which higher reported income could lead to a loss of program transfers. It is thus likely that actual household income differences are somewhat less pronounced between eligible and ineligible households post-treatment than appears to be the case in Table 1. We have no way to definitively disentangle these different explanations with the data at hand.

entry (defined as in the regressions) in a range of +/- 2 years, providing a visual representation of the variation that we exploit. The left panel presents the entire universe of births, and the right panel contains just observations in the range -0.1/+0.1, and we superimpose the localized difference-in-differences estimates (the fitted lines). Despite considerable variability in the data due to the relatively limited number of births occurring at each lead and lag, there is a clear "jump" (i.e., a reduction in low birthweight among eligible mothers) precisely for births occurring after program entry.

Mechanically, the reduction in low birthweight as a result of the program can be accounted for by either reduced intrauterine growth retardation and/or greater gestational length. The biomedical literature suggests that the drivers of these two phenomena might be different, so an investigation of these two margins can potentially help us understand the leading channels of impact. The estimated effect of program exposure on gestational length is just 0.1 weeks, or roughly 0.7 of a day (Table 3, row 1). This results from a specification with second order polynomial controls and a range of -0.1 to 0.1 around the threshold (analogous to Table 2, column 5, row 2), a specification we continue to focus on below. This increase in gestational is quite modest and not statistically significant at conventional levels, and there is similarly no impact on premature births (less than 37 weeks, row 2). This implies that the reduction in low birthweight is primarily due to improved in intra-uterine growth.

There is a modest increase in average birthweight of 31 grams (row 3), which is roughly the baseline difference between eligible and ineligible newborns (Table 1, Panel A).

To investigate effects at different birthweights, we next report the implied proportional change in the fraction of newborns below a given weight level, together with the associated 95 percent confidence interval (Figure 1). These coefficients are estimated based on a series of regressions similar to those in

Table 2 (column 5, row 2). Program exposure significantly reduces the incidence of a range of birthweights below 3,000 grams, with effects between 10 to 30 percent that grow at lower birthweights. Estimates become less precise for weights below approximately 2,000 grams due to the smaller number of observations in this range.

We also report impacts on APGAR scores at both 1 and 5 minutes after birth (Table 3, rows 4 and 5). Estimated impacts are positive and significant at 95 percent confidence, though modest in magnitude (an effect of 0.09 for the score at 1 minute after birth and 0.06 at 5 minutes).

We next explore whether the effects of cash transfers on low birthweight are more pronounced among particular subgroups (Appendix Table A2). In addition to being prominent in public policy discourse, several of these subgroups (i.e., premature children and children of unmarried mothers), are at greater risk of having low birthweight (column 2). Despite prematurity being unaffected by the program, the reduction in low birthweight was particularly pronounced among premature children, with a sizeable reduction of -0.116 (on a base incidence of 60 percent), and a large increase in average birthweight of 165 g (not shown). The low birthweight effect among non-premature children is small (at -0.007) and not statistically significant. We also find more pronounced effects among single (unmarried) mothers, with a drop of 2.0 percentage points (versus 1.4 points for married mothers) and among teen mothers with a drop of 2.9 percentage points (versus 2.2 points among non-teen mothers).

The evidence in this section indicates that *PANES* cash transfers significantly improved child birth outcomes, reducing the incidence of low birthweight by 20 percent. Impacts are largely concentrated among premature children and those in the lower tail of the birthweight distribution, suggesting that children with the worst birth outcomes gained the most.

B. Alternative estimates and robustness checks

As a robustness check, we next estimate the model by assuming a fixed program entry month that is the same for all households (regardless of the month of their baseline survey visit), namely, April 2005, the month the first *PANES* payment was made. The estimated effect is nearly unchanged at -0.024 (s.e. 0.013, Table 4 row 1), and statistical precision falls somewhat, as expected since this approach effectively introduces measurement error into the treatment term.

We also assess robustness to using data from the program period only ($D_{imt}=1$) in a RD model. The regression model is:

$$(3) \qquad Y_{imt} = \beta_0 + \beta_1 T_{imt} + f(N_m) + e_{imt}$$

The first stage specification is:

$$(4) \qquad T_{imt} = \gamma_0 + \gamma_1 E_m + g(N_m) + u_{imt}$$

where $g(N_m)$ is again a continuous function of its argument at zero and we instrument *PANES* treatment status T_{imt} with an indicator for the mother's *PANES* eligibility ($E_m = 1(N_m < 0)$).

The purely cross-sectional RD estimates are robustly negative and similar (or larger) in magnitude to the earlier results, which is reassuring, but they are less precisely estimated (Appendix Table A3).[22] The use of panel data simply leads to much more precise, and thus informative, estimates and we focus on the panel approach for the remainder of the analysis.

This pattern is also apparent in Figure 2, panels A and B, which present reduced form estimates, at baseline and at follow-up respectively, in the range (-0.1, 0.1). Here we plot residuals from regressions of an indicator for low

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[22] In column 10 of Appendix Table A3, we report RD estimates based on local linear regressions over the entire range of support of the predicted income score (with no additional controls), using the optimal bandwidth for a sharp design suggested by Imbens and Kalyanaraman (2012). We regard the similarity between estimates in columns 1-9 and in column 10 as reassuring. We also report point estimates and the associated confidence interval for different levels of the bandwidth (from half to twice the optimal bandwidth) in Appendix Figure A5.

birthweight on month of conception, month of baseline visit and all controls (as in Table 2, row 2). We present linear regression fits where the intercept and slope are allowed to vary on each side of the threshold. Consistent with the underlying RD identification assumption, there is no visually apparent discontinuity in low birthweight at baseline.[23] However, we do find some evidence for a discontinuity at the follow-up although estimates are rather imprecise due to sampling variability. A clear jump is evident in the first stage estimates (Figure 2 panel C and Appendix Table A4). Despite imperfect eligibility enforcement and some program dropouts, the estimated increase in the fraction of treated households at the threshold for program transfers is large, at 0.66, and very precisely estimated.[24]

Following McCrary (2008), the left hand-side panel of Appendix Figure A7 reports the density of the normalized income score. We find no discontinuity in the density of the running variable, suggesting a lack of data manipulation, consistent with the identification assumption that assignment around the discontinuity was nearly "as good as" random. Appendix Table A6, rows 1 to 36, also reports reduced form estimates of the discontinuity for each of the baseline covariates at the threshold (we adopt a quadratic specification in the range -0.1, 0.1, as in column 5, row 1 of Table 2). By and large, most coefficients are small

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[23] A reader might be surprised by the lack of gradient in the outcome variable with respect to predicted income. This is due to the fact that the figure contains a "close neighborhood" around the eligibility threshold. When one considers a wider range of predicted income score values, there is clearly an income gradient with the predicted slope. To illustrate, Appendix Figure A6 reports the fraction of low birthweight pre-program over the entire range of predicted income scores. We also present a quadratic fit to the data while the two vertical lines denote the restricted range of values in the preferred regression specifications. Unlike in Figure 2, we simply present means here with no other controls for transparency. There is a global negative income gradient, as expected, and it is particularly steep among the poorest households. Consistent with Figure 2, there is a smaller slope in the analysis range of -0.1/+0.1.

[24] We have computed additional estimates including mother fixed effects (in the even-numbered columns of Appendix Table A6). Although this approach allows us, in principle, to control for unobserved time invariant mother characteristics, it relies on the subsample of mothers giving birth during both periods, considerably restricting the sample size. These estimates are similar to those in Table 2 but they are imprecisely estimated. Estimates are again similar but less precisely estimated in models restricted to the same population of mothers with births in both the pre-program and program periods, but which do not include mother fixed effects (in the odd-numbered columns). This suggests that the sample of panel mothers is simply too small to deliver precise estimates of program impact.

and not significant, again consistent with assignment around the thresholds being as good as random.[25] We also report regressions for outcomes during the entire pre-program period. We find no evidence of significant differences in the incidence of low birthweight, average birthweight, mother labor force participation and mother and household income from different sources during pre-program pregnancies. This evidence argues against systematic sorting around the discontinuity, consistent with the finding that the inclusion of covariates does not affect estimated program impacts.

As an additional test of the robustness of our findings, we present an estimate of program impacts on low birthweight obtained based on a different econometric identification strategy. In particular, we restrict the analysis to households that were *treated at some point* (i.e., households that received at least one *PANES* payment over the course of the program) and we exploit the gradual roll-out of the program across these households, in the spirit of an event-study design. In this analysis, we use actual (as opposed to imputed) household entry into the program. Formally, we restrict to program households that received the program for at least one month (whether eligible or not) and regress outcomes on a treatment indicator (T_{imt}). To account simultaneously for take-up among the ineligible and program drop-out among the treated, both of which could be endogenous, we instrument the treatment indicator with a program period indicator F_{it} – which, differently from D_{imt}, is based on each household's own program entry date – interacted with an indicator for program eligible households, E_m. The main and reduced form equations are:

(5) $\qquad\qquad Y_{imt} = \beta_0 + \beta_1\, T_{imt} + e_{imt}$

(6) $\qquad\qquad T_{imt} = \gamma_0 + \gamma_1\, E_m{*}F_{imt} + u_{imt}$

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[25] Only 4 of 36 coefficients are significant at 90 percent, roughly what one would expect given random sampling variation. Appendix Figure A8 also shows no evidence of a discontinuity at the threshold for most of the covariates.

where again we also include indicators for mother m's month of baseline survey (d_{bm}), month of conception indicators (d_t), an eligibility indicator (E_m) and all other covariates (not shown here).

Despite the entirely different source of variation in cash transfers – in particular, variation over time in household transfer receipt, in contrast to the model in Table 4, row 1, which uses the same entry month for the entire sample – and the use of a somewhat different subsample, this approach delivers remarkably similar program impact estimates, with an estimated reduction in low birthweight of 0.015 (s.e. 0.008, Table 4 row 2), which is significant at 90 percent confidence.

VI. Channels of Impact

The richness of the dataset allows us to investigate several behavioral channels that could be driving the findings. We also assess the role of the program's in-kind food card component.

A. Income, Labor Supply and In-kind Transfers

We start by examining the effect of the program on household income. The effect on the value of the monthly *PANES* cash transfer during pregnancy is UY$1,028, or approximately US$75 in PPP terms (Table 3, row 6). This figure is roughly 30 percent lower than the transfer (UY$1,360), consistent with the imperfect compliance to program assignment discussed above. Program eligibility is associated with higher in-kind food card transfers during pregnancy of UY$193 per month, or approximately US$15 in PPP (row 7). As with income transfers, this is substantially below what in-kind transfers would have been with full take-up, given both imperfect compliance and the mid-program food card roll-out (Appendix Figure A1, panel D).

This finding raises the question of what role if any the in-kind transfers are playing in driving the results above. A differential effect of cash versus the food card would not be predicted if households are infra-marginal in terms of food consumption. However, if the food card amount is greater than baseline household food expenditure, they might have larger effects on birthweight (working through maternal nutrition) than equivalent cash transfers. We investigate this issue directly in row 3 of Table 4 by exploiting the fact that the food card was only rolled out starting in the second half of 2006 (Appendix Figure A1).[26] Here we report regression results in which we include an indicator for receipt of the food card during pregnancy as an additional explanatory variable. Since only households in receipt of the income transfer could receive food card payments, the coefficient estimate on the food card variable here is the incremental effect of receiving a food card over and above the effect of the income transfer.

The effect of the income transfer remains negative and similar to the one in the main regressions (-0.033, as opposed to -0.024 in column 5 row 2 of Table 2, and we cannot reject that this coefficient is equal to -0.024). This implies that the low birthweight results are not driven mainly by the in-kind (food card) component of the *PANES* program. The coefficient on the food card is positive (as opposed to negative as one would expect) but small and not statistically significant (0.015, s.e. 0.023). Indeed, we cannot reject the hypothesis that the effect of the food card is the same as the income transfer (p-value=0.14), despite the fact that the value of the food card is just 20 percent of the cash transfer; moreover, due to limited statistical precision, we cannot reject the hypothesis that

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[26] As with the date of the first program cash payment, we determine the first month of food card payments for each month of baseline application survey visits and assign this month as the entry date into food cards for all households surveyed in that month, whether they received a food card or not at that time. We use our preferred specification and instrument receipt of the food card with an indicator for program eligibility interacted with the birth occurring at least three months after the first food card payment.

the effect of the food card is 20 percent of the effect of the income transfer.[27] Several implications follow. Cash transfers per se have an effect. We also cannot reject the hypothesis that cash and in-kind transfers have the same effect, which is what one would expect for households that are infra-marginal with respect to food consumption.

Regarding access to other government programs beyond the food card, we also find that *PANES* did not act as a passport into other programs: if anything the total transfers received from other government programs fell modestly among eligible households (Table 3, row 8).

Standard theory in labor economics predicts that welfare programs will tend to reduce labor supply due to an income effect. In addition, means tested programs like *PANES* have the potential to affect work hours and participation due to a substitution effect (Moffitt 2002). While such labor supply responses would dampen the increase in expenditures generated by welfare transfers, allowing pregnant women to withdraw from the labor market might also reduce maternal energy consumption and possibly psychological stress, potentially contributing to improved child birth outcomes. Among female program beneficiaries who were employed at baseline, nearly 60 percent worked in domestic service, a physically strenuous occupation.

We next specifically examine maternal labor supply and earnings from the social security records. There is evidence that *PANES* mothers reduced their labor supply by roughly 4 percentage points (Table 3, row 9) and had somewhat lower earnings (of about UY58 or approximately US$4 in PPP terms, row 10). Despite these negative labor supply effects and a slight displacement in terms of other household benefits, program eligible households still experienced a large and

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[27] The limited statistical precision presumably stems from the fact that the food card benefit was relatively small in magnitude and was only in place for a relatively short period of time. We also cannot reject the hypothesis that there are non-linearities in the relationship between income and birthweight, or that program impacts fell over time.

significant increase in total household income of UY\$1,000 per month (row 11), where this figure includes all sources of formal income and public transfers (including *PANES* transfers) during pregnancy. Thus it appears that labor supply responses to the *PANES* program were far too modest in magnitude to offset the direct transfer amount.

To put this in context, we use a conservative counterfactual, namely, total income among ineligible program applicants between July 2006 and December 2007 (when the program had completely rolled out), which was roughly UY\$3,650 (or just over US\$270 per month in PPP). This implies that the program increased total household income relative to this counterfactual by more than 25 percent. This in turn implies an elasticity of low birthweight with respect to household income (as recorded in official government records) during pregnancy of between -0.8 and -1.0.

For comparison, Hoynes et al.'s (2013) estimates imply an elasticity of low birthweight with respect to income of between -0.25 and -0.86.[28] Our estimates are slightly larger, and one possibility is that this is due to greater proportional effects at lower income levels (and higher incidence of low birthweight). One potential implication is that the "local" effects we estimate at the eligibility discontinuity would be a lower bound on the average effect of the *PANES* program on outcomes among the entire population of treated households, who tend to be poorer. [29]

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[28] Estimates in Hoynes et al (2013) suggest that an extra 1,000 US\$ in the annual EITC refund is associated with a 0.17 to 0.31 percentage point decrease in the likelihood of low birthweight status. Taking Hoynes et al.'s estimates and assuming an average monthly household income of 1,250 US\$ per EITC recipient household (i.e., an average income of 15,000 US\$ per year), and an incidence of low birthweight of 10 percent, yields the elasticities.

[29] We might alternatively be overestimating the elasticity due to mis-measured total income among the eligible (as discussed above). Although Uruguay has relatively low levels of informality for Latin America (Loyaza, Servén and Sugawara 2009), current work by some of the authors suggests that program incentives affected behavior.

B. Prenatal care

Evidence from the medical literature suggests that the week of the first prenatal care visit and the total number of visits in the first trimester are crucial for the detection and treatment of pregnancy risk factors and hence for birth outcomes, although their effects on low birthweight are not clearly established in existing research. This raises the question of whether and to what extent program effects are driven by improvements in the quantity and quality of prenatal care.

At first glance, such improvements seem unlikely. As noted above, the *PANES* program was *de facto* unconditional, and survey evidence indicates that the vast majority of beneficiary households were unaware of any formal prenatal check condition for pregnant women. Yet it remains possible that beneficiaries did obtain some additional access to health care (or felt pressure to make additional visits), or that the increased financial resources due to the program affected prenatal care utilization, and we next explore this possibility.

We report the effect of *PANES* on the number of prenatal visits overall and by trimester, as well as the week of first visit. We do not find any evidence of increased prenatal care utilization (Table 3, rows 12-16). If anything the number of births assisted by a doctor (row 17) is slightly negatively affected by program participation, the fraction of births in public hospitals – which are typically considered of lower quality – rises (row 18) and the fraction of deliveries paid by private insurance falls (row 19). A likely explanation is that some program beneficiaries lost access to private medical insurance, which in Uruguay is typically obtained through formal employment, due to the program's impact on formal labor supply. Yet we do not find any evidence of significant changes in the average quality of the health center where the delivery occurred as proxied by the center's (pre-program) average birthweight (row 20).

Finally, we investigate whether residential mobility increased, and specifically whether the *PANES* transfer led households to move into neighborhoods with better average health outcomes (which might contribute to better health outcomes through several channels), but find no evidence that average child health in the households' residential area (as proxied by the pre-*PANES* average birthweight in the area) improved among program households (Table 3, row 21).

In sum, we find no evidence of increased prenatal care, and no evidence of improvements in the quality of medical care at birth. If anything, the reverse may be true to some extent as the drop in formal labor supply among participants slightly reduced private insurance coverage.

C. Family structure and endogenous fertility

PANES led to a large and statistically significant reduction in the proportion of children born to unmarried parents. Thus contrary to what is sometimes presumed, welfare transfers led to a moderate increase in marital stability, with a drop of 3.0 percentage points (standard 1.1, significant at 95 percent confidence – Table 3, row 22) in out-of-wedlock births, on a base of 80 percent. To the extent that intact marriages and the presence of the father have positive impacts on the mother and child, this could partly explain the program effects on low birthweight.[30] However, the link between married parents and child health

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[30] While one concern is that the observed drop in out-of-wedlock births might depend on households' behavioral response to the transfer, this seems highly unlikely. First, marital status did not enter into the computation of the income score used to determine program eligibility. Similarly, household size (other than the number of children) did not matter for program eligibility (although residential overcrowding did). The program eligibility score formula was unknown to households, although we cannot rule out that some households incorrectly assumed that marital status did in fact matter for eligibility and adjusted marital status accordingly. Second, eligible mothers may have had incentives to conceal a partner and hence not to marry in order to retain program eligibility. This is because a male partner's formal income would count towards the income conditionality, so if anything, one might expect out-of-wedlock births to increase among eligible mothers compared to the ineligible. The reduction in out-of-wedlock births in eligible households is thus arguably even more striking than it initially appears, given these incentives.

outcomes remains highly speculative, and we do not take a stand on whether marriage has benefits for child health here. The increase in marriage rates could be due to multiple factors, including the possibility that income transfers led to greater relationship stability or that it made the mother a more attractive partner (in those cases where she was the household's program recipient, which is the majority of cases). In any case, there is no evidence of the hypothesized adverse behavioral responses with respect to marriage.

Another concern, and an issue of interest in its own right, is that the level or timing of fertility might be endogenous to program eligibility and cash transfer receipt. Standard economic theory yields ambiguous predictions on the effect of household income on fertility: if children are a normal good, the cash transfer could increase fertility (Becker 1960), although it might also lead to increased investment in child quality while reducing overall fertility.[31] Indeed, the existing empirical evidence is mixed (Moffitt 1998). The program might also have affected fertility via changing access to contraception, abortion or evolving fertility preferences, or affected the probability of successfully completing a pregnancy through improved fetal survival.[32] Endogenous fertility choices could lead to biased estimates of program impact if the mothers whose fertility is affected have a different risk of low birthweight children.[33]

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[31] See Becker and Lewis (1973). An additional effect might arise if program generosity is conditioned on the number of children (e.g., Stecklov et al. 2007). However, PANES consisted of a fixed transfer per household.

[32] Although abortion was illegal in Uruguay until 2013 (other than when the life of the mother was at risk), it was widely practiced. The *Centro Internacional de Investigación e Información para la Paz* (CIIIP) estimates a rate of voluntary abortion of 38.5 percent (for the year 2000). The comparable rate in the U.S. is on the order of 20 percent.

[33] Our data do not provide information on miscarriages (other than as a retrospective question to women who subsequently gave birth). We also have no information on infant mortality. The latter is recorded in the official death records but it is not possible to link these death records to birth records or to *PANES* program data, unfortunately. This is because the infant mortality data report the national identification number of the child (but not of the mother), while the opposite is true of the birth data, and while program data contain information on children's national ID number, crucially, this is only for those children already present in the household at the time of the baseline survey, and thus not for children who were not yet born (or those who died before the survey). Some children in the survey are also not assigned a national ID number until school enrollment age, especially among poor households, so there is some missing data on child ID, further complicating our ability to match across datasets.

To investigate fertility patterns, we restrict the analysis to *PANES* applicant females of roughly child-bearing age (12 to 49 years old) and create a monthly panel that spans the period January 2003 to December 2007. For each woman in each month, we construct an indicator variable equal to one if the woman gave birth and zero otherwise. Overall, we have information on approximately 185,000 women over up to 60 months, with more than 10 million individual-month observations. The average monthly fertility rate in this sample is 0.66 percent.

A challenge in estimating the fertility response to *PANES* is the fact that program eligibility depended in part on the number of children in the household at the time of the baseline survey, implying that mothers who gave birth just before the survey were considerably more likely to be eligible than otherwise identical mothers giving birth just afterwards. This is apparent in Panel A of Appendix Figure A9, which plots the probability of a child's household being a recipient of *PANES* as a function of the child's date of birth relative to the date of the baseline survey. The figure shows a clear discontinuity in the probability of *PANES* treatment as a function of the child's date of birth. This eligibility rule coupled with negative state dependence in childbearing (i.e., the probability of giving birth to a child today conditional on a child having been born in the previous nine months is basically zero), mechanically leads to a fall in observed fertility among eligible mothers after the launch of the program, which might erroneously be considered a causal program effect. Some of this is simply mechanical regression to the mean, which arises in many other contexts, for example in job training programs (Ashenfelter 1978, Card and Sullivan 1988). Consistent with this mechanism, the difference in fertility between *PANES* eligible and ineligible applicants displays a sharp drop between the pre-program and program period, i.e., before and after the date of first program payment (panel B).

A first piece of evidence that compositional differences between eligible and ineligible households are driving this pattern is found in panels C to E of Appendix Figure A9, which successively plot monthly fertility rates for applicant mothers as a function of the time to and since the first program payment, separately by the number of children born between January 2003 and the baseline survey (namely, 0, 1, or 2 or more children). Conditional on the mother's past fertility, patterns during the program are nearly identical for the eligible and ineligible.

A simple way to correct for the bias induced by this compositional effect in the regressions is to use a matching estimator that re-weights observations for *PANES* ineligible mothers so that the pre-program fertility is the same as for eligible mothers. We use the number of pregnancies at different lags before the date of the baseline survey (1 to 3 months, 4 to 6 months, etc., as well as indicators for any missing values of these variables) from the natality files, plus the mother's age, mother's education (in five groups) and the month of the baseline survey to fit a probit model in which the dependent variable is an indicator variable for program eligibility. We use the predicted values from this probit regression to assign ineligible mothers a weight that is equal to the predicted probability of being eligible (relative to being ineligible), while we assign a weight equal to one to all eligible mothers. In practice, this procedure re-weights observations for ineligible mothers so that their distribution of observable characteristics at baseline mimics that of eligible mothers, as in a matching estimator.

Estimates are reported in Table 5, where the dependent variable is an indicator for fertility (multiplied by 100). We use eqn. 1 and 2, as in Table 2, and for brevity only report specifications with a quadratic polynomial in the baseline predicted income score (although results are not sensitive to this choice). Columns 1 to 3, respectively, report unweighted estimates for the entire range of variation

of the income score and then in neighbourhoods of the eligibility threshold (-0.1, 0.1 in column 2; -0.075, 0.075 in column 3). Columns 4 to 6 report re-weighted estimates that correct for compositional differences. Row 1 presents results without additional controls while row 2 presents results with the entire set of baseline controls.

Naïve estimates imply that fertility falls as a result of program participation (columns 1 to 3) which is consistent with Panel B of Appendix Figure A9. However, the re-weighted estimates (in columns 4 to 6) show an effect of program participation on fertility that is positive and, when one restricts to a neighbourhood around the eligibility threshold, the fertility estimates are small and not statistically significant. The inclusion of controls makes virtually no difference.

We turn to investigating the effects of the mechanical fall in fertility among treated households on our estimates of the program's impact on low birthweight. A leading concern is that birth outcomes might be correlated with the mother's fertility history, which in turn affects program eligibility. We note at the onset that in Table 2 the standard controls also included the number of previous births, meaning that any bias arising from any spurious correlation between prior birth outcomes and program eligibility should be accounted for. For further reassurance, we report estimates of program impact on low birthweight reweighting by the fertility weights, and obtain an estimate of -0.025 (s.e. 0.010, Table 4 row 4), which is very similar to the main unweighted estimates in Table 2, again suggesting that selection based on mother's fertility history plays little to no role in explaining the drop in low birthweight for *PANES* beneficiaries.

VII. Discussion and Conclusion

This paper studies the impact of a social assistance program on the incidence of low birthweight in Uruguay. The program mainly consisted of a cash transfer that was targeted to households in the bottom decile of the income distribution, a population with an incidence of low birthweight of around 10 percent. Using a unique matched micro-dataset with vital statistics, program, hospital and social security records, our estimates imply a drop in the incidence of low birthweight on the order of 20 percent, allowing beneficiaries to close the baseline gap in low birthweight incidence with the slightly better-off households who applied for the program but were ineligible for assistance. The results suggest that cash assistance may potentially help to break the cycle of intergenerational poverty by improving child health. This result is derived using a localized difference-in-differences estimator within a close neighborhood of the program eligibility threshold, an approach that generates more precise estimates than a traditional RD approach. We also obtain very similar results using a different econometric identification strategy which, in the spirit of an event-study, exploits variation in the month of program entry.

In terms of the biomedical channels, the effect of the program on low birthweight mainly operates through a reduction in intrauterine growth retardation, and in particular improved birthweight among premature children. The unusually rich dataset also allows us to explore a large number of behavioral mechanisms. Although we find some evidence of minor offsetting household labor supply responses, most likely induced in part by the means-tested nature of the program, we show that *PANES* receipt increased eligible mothers' total net household income by at least 25 percent, which may have improved maternal nutrition. The drop in labor market participation among beneficiaries contributed to slightly lower private health insurance coverage. We also show that the in-kind

food card component cannot account for the birthweight impacts. The *PANES* program did not increase family break-up and in fact reduced out-of-wedlock births.

Increases in pre-natal health care utilization and improved health quality care also do not explain our results. Although we are unable to say anything conclusive about how outcomes would have differed in the presence of strictly enforced prenatal visit conditions, this is a noteworthy result in itself given the current policy debate in development economics regarding the role of conditionality in social programs (Baird, McIntosh and Özler 2011).

Equally important, we do not find evidence of substantial fertility effects among welfare eligible women, and we show that there are no meaningful program impacts on either residential migration or take-up of other government programs, implying that program effects cannot be attributed to these alternative channels. We have no direct measures of the mother's levels of mental stress, nor of smoking during pregnancy, unfortunately, but these channels appear less likely to have played a major role in our case, since recent biomedical evidence indicates that these factors are most likely reduce gestational length and increase prematurity, but no reduction in gestational length is found in our data.

Taken together, the evidence points unequivocally to the cash transfer component itself, and the improved living standards that it provided, as the key driver behind improved birth outcomes. The maternal nutrition channel is a natural candidate as a main driver of the low birth weight effect. We lack data on mothers' nutritional status, weight, food expenditure and caloric intake during pregnancy, and thus the precise role of this channel remains somewhat speculative, and we cannot rule out other explanations, such as greater female empowerment due to program participation. Yet this interpretation would be in line with several studies evaluating the impact of U.S. social programs that also emphasize the key role of improved maternal nutrition.

Finally, the findings in this paper appear relevant not just for Latin American countries but also potentially for wealthier societies given Uruguay's reasonably good average infant health outcomes, which are similar to those observed in poor populations in the U.S., and the nearly universal access to prenatal care in the country.

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FIGURE 1— ESTIMATED PROPORTIONAL PROGRAM EFFECTS BY BIRTHWEIGHT

Notes: The figure reports the estimated percentage change in the probability of being below each level of birthweight as a result of *PANES* treatment. Each point comes from a separate 2SLS regression including controls (as in column 5 of Table 2, row 2). 95 percent confidence intervals around the estimates also reported. See also notes to Table 2.

A: Proportion of low weight births,
pre-program period

B: Proportion of low weight births,
program period

C: Receipt of *PANES* income transfer during
pregnancy, program period

FIGURE 2: PROPORTION OF LOW BIRTHWEIGHT AND TREATED BIRTHS AS A FUNCTION OF THE PREDICTED INCOME SCORE

Notes: Panel A reports the proportion of low weight births in the pre-program period (conditional on month of conception, month of baseline visit and all controls) among *PANES* applicant mothers as a function of the normalized income score during the pre-program period (horizontal axis). A vertical line corresponds to the *PANES* eligibility threshold at the normalized predicted income score of zero. The figure also reports a linear fit in the income score estimated on either side of the threshold (solid lines) as well as the 95 percent confidence intervals around the linear prediction (thin dotted lines). Panels B and C report analogous graphs where the variables on the vertical axis are, respectively, the proportion of low weight births in the program period, and the fraction of mothers in receipt of the income transfer during pregnancy (during the program period). The series in Panels A and B are residuals from a regression of the outcome variable on the set of controls in Table 2, row 2. The size of the points in all panels is proportional to the number of observations in that cell.

TABLE 1 — DESCRIPTIVE STATISTICS, ALL BIRTHS IN URUGUAY

Panel A. Pre-program period (January 2003 – March 2005)	*PANES* Applicants		Non-applicants
	Eligible	Ineligible	
	(1)	(2)	(3)
1. Low birthweight	0.102	0.093	0.084
2. Birthweight (g)	3141.05	3161.35	3217.92
3. APGAR - 1 minute	8.48	8.50	8.51
4. APGAR - 5 minutes	9.60	9.60	9.62
5. Gestational length (weeks)	38.50	38.50	38.56
6. Premature (gestational length <37 weeks)	0.101	0.099	0.092
7. Total number of prenatal visits	6.53	7.53	8.28
8. Week of first prenatal visit	17.50	16.24	14.16
9. Number of visits, first trimester	0.31	0.40	0.63
10. Number of visits, second trimester	1.61	1.92	2.19
11. Number of visits, third trimester	4.61	5.22	5.46
12. Average birthweight area of residence	3193.53	3196.20	3200.88
13. Average birthweight health center	3170.43	3185.76	3207.59
14. Public health center delivery	0.77	0.55	0.33
15. Birth delivery paid by private health insurance	0.06	0.14	0.43
16. Mother incomplete primary education	0.12	0.05	0.04
17. Number of previous pregnancies	2.37	1.44	1.26
18. Out-of-wedlock birth	0.80	0.72	0.52
19. Missing child father information	0.61	0.51	0.31
20. Father incomplete primary education	0.095	0.095	0.092
21. Mother works during pregnancy	0.12	0.18	0.43
22. Mother earnings during pregnancy ±	94.19	253.01	-
23. Household earnings during pregnancy ±	585.14	1678.35	-
24. Household non-*PANES* benefits during pregnancy ±	578.60	874.08	-
25. Household total income during pregnancy ±	1163.90	2553.66	-
26. Mother age	25.43	24.78	27.50
27. Father age	30.77	29.62	31.93
28. Birth assisted by doctor	0.49	0.55	0.71

Panel B. Program period (April 2005 – December 2007)	*PANES* Applicants		Non-applicants
	Eligible	Ineligible	
	(1)	(2)	(3)
1. Low birthweight	0.091	0.091	0.082
2. APGAR - 1 minute	8.49	8.46	8.50
3. APGAR - 5 minutes	9.61	9.59	9.63
4. Gestational length (weeks)	38.51	38.51	38.53
5. Prematurity (gestational length <37 weeks)	0.095	0.10	0.09
6. Ever received *PANES* income transfer	0.97	0.11	-
7. *PANES* income transfer during pregnancy (0/1)	0.63	0.08	-
8. Amount of *PANES* income transfer during pregnancy	650.57	73.37	-
9. Ever received *PANES* food card	0.80	0.12	-
10. *PANES* food card during pregnancy (0/1)	0.41	0.05	-
11. Amount of *PANES* food card during pregnancy	149.91	13.74	-
12. Mother works during pregnancy	0.12	0.18	0.43
13. Mother earnings during pregnancy ±	114.75	298.40	-
14. Household earnings during pregnancy ±	825.31	1996.78	-
15. Household non-*PANES* benefits during pregnancy ±	722.78	937.76	-
16. Household total income during pregnancy ±	2348.57	3021.66	-
Observations	50,939	20,872	163,370

Notes: Panel A contains information on all births between January 2003 and March 2005. ±: data available only since March 2004 and only for program applicants. The earnings and transfers variables are in UY$ per month. 1 UY$ = US$ 0.075 at 2005 PPP adjusted exchange rate. Panel B contains information on all births between April 2005 and December 2007. Some of the additional data presented in Panel A is from the *PANES* program baseline (pre-program) applicant survey.

TABLE 2— 2SLS ESTIMATES OF THE EFFECT OF THE *PANES* TRANSFER ON LOW BIRTHWEIGHT (<2.500 KG)

POOLED PRE-PROGRAM AND PROGRAM PERIOD DATA

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1. No controls	-0.020***	-0.019***	-0.020***	-0.020**	-0.020**	-0.020**	-0.023**	-0.023**	-0.023**
	(0.007)	(0.007)	(0.007)	(0.010)	(0.010)	(0.010)	(0.011)	(0.011)	(0.011)
2. With controls	-0.019***	-0.019***	-0.019***	-0.024**	-0.024**	-0.024**	-0.025**	-0.025**	-0.025**
	(0.007)	(0.007)	(0.007)	(0.009)	(0.009)	(0.009)	(0.011)	(0.011)	(0.011)
Observations	65,655	65,655	65,655	31,512	31,512	31,512	24,212	24,212	24,212
Range	All	All	All	0.1	0.1	0.1	0.075	0.075	0.075
Order of polynomial	1	2	3	1	2	3	1	2	3

Notes: Each cell refers to a separate regression. Entries are 2SLS estimated coefficients from regressions that pool pre-program and post program data where the dependent variable is an indicator for birthweight less than 2.5 kg and the independent variable is an indicator for the mother participating to the program during pregnancy (equation (1)), where the latter is instrumented by an indicator for mother eligibility for *PANES* for births occurring during the program period only (equation (2)). First stage estimates are reported in Appendix Table A4. Regressions differ in terms on the sample used (in different ranges around the eligibility threshold) and the order of the polynomial in the baseline income score. All regressions include an indicator for program eligible mothers, an indicator for births occurring in the program period, month of conception and month of baseline survey indicators. Row 1 presents regressions with no additional controls while row 2 reports results with the following additional controls: indicators for geographic departamento (19), for mother's age and mother's education (incomplete primary, complete primary, complete secondary), sex of the new-born, number of previous pregnancies, number of new-borns (twins or more), whether dwelling is a house, whether the household has centralized hot water, heater, stove, micro-wave, refrigerator, freezer, washing machine, dishwasher, central heating, TV, VCR, cable TV, COMPUTER, car, or phone, whether the block has electricity, piped water, sewage, trash collection, paved streets, public lighting, whether the home is owned, indicators for material of the floor and walls, whether the home has a toilet, number of rooms, number of bedrooms. Standard errors clustered by income score and number of observations reported below each coefficient. ***, **, *: Significant at 99, 95 and 90 percent level.

TABLE 3 — *PANES* PROGRAM EFFECTS ON ADDITIONAL OUTCOMES

Dependent variable:	
1. Weeks of gestation	0.101
	(0.067)
2. Premature birth	-0.010
	(0.010)
3. Birthweight	30.853[*]
	(18.438)
4. Apgar-1 minute	0.089[**]
	(0.037)
5. Apgar-5 minutes	0.055[**]
	(0.027)
6. Value of program income transfer during pregnancy ('000 UY$)	1.028[***]
	(0.008)
7. Value of food card during pregnancy ('000 UY$)	0.193[***]
	(0.004)
8. Other household government benefits during pregnancy	-0.116[**]
	(0.052)
9. Mother works during pregnancy	-0.039[***]
	(0.012)
10. Mother formal earnings during pregnancy ('000 UY$)	-0.058[**]
	(0.028)
11. Household total income during pregnancy ('000 UY$)	1.000[***]
	(0.121)
12. Total number of prenatal visits	0.019
	(0.106)
13. Number of prenatal visits, first trimester	-0.035
	(0.026)
14. Number of prenatal visits, second trimester	0.045
	(0.043)
15. Number of prenatal visits, third trimester	0.009
	(0.081)
16. Week of first prenatal visit	0.013
	(0.240)
17. Birth assisted by a medical doctor	-0.028[*]
	(0.016)
18. Public hospital delivery	0.031[**]
	(0.015)
19. Birth delivery paid by private health insurance	-0.024[**]
	(0.010)
20. Average pre-*PANES* birthweight in health center (g)	-1.136
	(1.731)
21. Average pre-*PANES* birthweight in area of residence (g)	-1.102
	(1.051)
22. Out-of-wedlock birth	-0.044[**]
	(0.013)

Notes: 2SLS estimates of the effect of *PANES* participation on various dependent variables, in a specification equivalent to Table 2, column 5, row 2. See also notes to Tables 1 and 2.

TABLE 4 — *PANES* PROGRAM EFFECTS ON BIRTHWEIGHT, ADDITIONAL SPECIFICATIONS

1. Fixed entry date	-0.024*
	(0.013)
Observations	*31,512*
2. Event study approach	-0.015*
	(0.008)
Observations	*46,614*
3. *PANES transfer*	-0.033*
	(0.018)
+ Food card	0.015
	(0.023)
Observations	*23,675*
4. Reweighted	-0.025**
	(0.010)
Observations	*31,001*

Notes: Row 1 presents estimates like the ones in table 2 where it is assumed that the program period starts in April 2005 for all households. Row 2 presents results from the event study analysis computed on the sample of ever-treated mothers (see text for details). Row 3 separates the effect of the *PANES* transfer from the additional effect of receiving the food card. Row 4 reports re-weighted estimates (as in row 2 of Table 5). All specifications include all controls and except the one in row 1 also include a quadratic polynomial in the predicted income score and are restricted to the range -0.1 to 0.1 for the predicted income score. See also notes to Table 2.

TABLE 5 — *PANES* PROGRAM EFFECTS ON FERTILITY (X 100)

	(1)	(2)	(3)	(4)	(5)	(6)
1. No controls	-0.249***	-0.105**	-0.085***	0.081***	0.022	0.016
	(0.011)	(0.016)	(0.018)	(0.017)	(0.017)	(0.019)
2. With controls	-0.247***	-0.102**	-0.082***	0.083***	0.024	0.018
	(0.011)	(0.016)	(0.018)	(0.017)	(0.017)	(0.019)
Observations	*10,048,632*	*5,677,586*	*4,464,166*	*10,048,632*	*5,677,586*	*4,464,166*
Weighted	No	No	No	Yes	Yes	Yes
Range	All	0.10	0.075	All	0.01	0.075
Order of polynomial	2	2	2	2	2	2

Notes: 2SLS estimates of the effect of *PANES* participation on a fertility indicator (multiplied by 100), in a specification equivalent to Table 2, columns 5, row 2. Row 1 of the table reports specifications with the inclusion of month of observation and month of visit fixed effects only while row 2 reports specifications with additional controls (see Table 2). Columns (1) to (3) report unweighted regressions while columns (4) to (6) report specifications weighted by matching weights (see section 7.3 of the text for details). See also notes to Tables 1-4.

Appendix

Appendix A: Additional tables and figures

APPENDIX TABLE A1 — CHILD BIRTH OUTCOMES AND INCOME LEVELS
IN URUGUAY, U.S. AND LATIN AMERICA/CARIBBEAN

Country/Region	Low birth weight, percent [a]	Infant mortality rate (per 1000) [b]	Births assisted by health personnel, percent [c]	At least one prenatal visit, percent [c]	Per capita GDP (PPP US$) [d]
Uruguay	8	11	99	97	13,189
United States	8	7	99	99	45,989
Latin America/Caribbean	9	19	96	95	10,575

Notes: (a) Source: United Nations Children's Fund (2009), *Global database on low birthweight,* and *Ministerio de Salud Pública (2011), Estadísticas Vitales,* Montevideo, Uruguay. The column reports the fraction of low weight births defined as children weighting less than 2.5 kg per 100 births. (b) Source: Pan American Health Organization, reported in World Health Organization (2009), *World Health Statistics*, Geneva. The column reports the probability of dying between birth and one year per 1,000 births. (c) Source: World Health Organization (2011). *World Health Statistics*, Geneva. (d) Source: World Bank (2011), W*orld Development indicators*. The World Bank, Washington. The column reports PPP-adjusted GDP per capita in US$.

	Coefficient (standard error)	Sample means
1. By gestational length		
Normal gestational length ($\geq$ 37 weeks)	-0.008	
		0.04
	(0.007)	
Observations	*28,493*	
Premature (< 37 weeks)	-0.116**	
		0.60
	(0.056)	
Observations	*3,019*	
2. By mother's age		
Teen mothers	-0.029	
		0.09
	(0.025)	
Observations	*6,544*	
Non-teen mothers	-0.022**	
		0.11
	(0.010)	
Observations	*24,968*	
3. By mother's marital status		
Married mother	-0.014	
		0.08
	(0.023)	
Observations	*6,122*	
Unmarried mother	-0.020*	
		0.10
	(0.010)	
Observations	*25,390*	
4. By household size		
Smaller households (three or fewer household members, avg: 2.7)	-0.011	0.10
	(0.014)	
Observations	*12,747*	
Larger households (at least four household members, avg: 5.8)	-0.029**	0.09
	(0.013)	
Observations	*18,765*	

Notes: Column 1 of the table reports 2SLS estimates of the program on low birthweight by subgroups (by gestational length, mother's age, baseline marital status, and household size). All specifications include all controls and restricted to observations in the range -0.1,0.1 as in column 5 of Table 2. Column 2 reports average incidence of low birthweight by subgroup. See also notes to Table 2.

APPENDIX TABLE A3 ─ 2SLS ESTIMATES OF THE EFFECT OF THE *PANES* TRANSFER ON LOW BIRTHWEIGHT (<2.500 KG) – PROGRAM PERIOD DATA ONLY

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1. No controls	-0.006	-0.013	-0.012	-0.022	-0.027	-0.046	-0.023	-0.040	-0.053	-0.022
	(0.010)	(0.016)	(0.021)	(0.016)	(0.026)	(0.034)	(0.019)	(0.030)	(0.041)	(0.017)
2. With controls	-0.018*	-0.013	-0.001	-0.019	-0.011	-0.034	-0.015	-0.022	-0.053	-
	(0.010)	(0.015)	(0.020)	(0.016)	(0.025)	(0.033)	(0.019)	(0.029)	(0.040)	-
Observations	*22,534*	*22,534*	*22,534*	*11,276*	*11,276*	*11,276*	*8,756*	*8,756*	*8,756*	*22,534*
Range	All	All	All	0.1	0.1	0.1	0.075	0.075	0.075	All
Order of polynomial	1	2	3	1	2	3	1	2	3	Local polynomial

Notes: Regressions similar to those in Table 2 computed on program period data only. Column (10) additionally reports regressions where a local linear polynomial is fitted on either side of the threshold. See also text and notes to Table 2.

APPENDIX TABLE A4 ─ FIRST STAGE ESTIMATES OF THE EFFECT OF THE ELIGIBILITY ON *PANES* INCOME TRANSFER

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1. No controls	0.722***	0.723***	0.722***	0.691***	0.691***	0.691***	0.686***	0.686***	0.686***
	(0.014)	(0.014)	(0.014)	(0.017)	(0.017)	(0.017)	(0.019)	(0.019)	(0.019)
2. With controls	0.727***	0.727***	0.727***	0.690***	0.690***	0.690***	0.684***	0.685***	0.685***
	(0.010)	(0.014)	(0.014)	(0.016)	(0.016)	(0.016)	(0.019)	(0.018)	(0.018)
Observations	*65,655*	*65,655*	*65,655*	*31,512*	*31,512*	*31,512*	*24,212*	*24,212*	*24,212*
Range	All	All	All	0.1	0.1	0.1	0.075	0.075	0.075
Order of polynomial	1	2	3	1	2	3	1	2	3

Notes: See notes to Table 2.

APPENDIX TABLE A5 — 2SLS ESTIMATES OF THE EFFECT OF THE *PANES* INCOME TRANSFER ON LOW BIRTHWEIGHT (<2.500 KG) – POOLED PRE-PROGRAM AND PROGRAM PERIOD DATA - PANEL SAMPLE

	(1)	(2)	(3)	(4)	(5)	(6)
1. No controls	-0.009	0.000	-0.011	0.002	-0.011	0.002
	(0.014)	(0.013)	(0.020)	(0.018)	(0.023)	(0.021)
2. With controls	-0.009	-0.015	-0.016	-0.008	-0.012	-0.006
	(0.013)	(0.013)	(0.017)	(0.018)	(0.020)	(0.021)
Observations	23,691	23,691	9,658	9,391	7,113	7,113
Range	All	All	0.1	0.1	0.075	0.075
Order of polynomial / specification	2	FE	2	FE	2	FE

Notes: Regression results similar to those in Table 2 estimated on a sample of mothers giving birth in both the pre-program and the program periods. Specifications in even number columns include mother fixed effects. See also notes to Table 2.

	Non-eligible mean	Coef.	s.e.
1. Mother's Age	25.377	0.294	(0.266)
2. Incomplete primary	0.053	-0.013	(0.009)
3. Complete primary	0.802	0.020	(0.014)
4. Complete secondary	0.084	-0.002	(0.009)
5. Sex of new-born	0.511	0.012	(0.017)
6. Number of previous pregnancies	1.529	-0.054	(0.077)
7. Number of new-borns	1.018	-0.008	(0.007)
8. House	0.880	-0.009	(0.015)
9. Heating	0.336	-0.009	(0.023)
10. Water heater	0.192	0.017	(0.019)
11. Stove	0.717	0.010	(0.024)
12. Micro-wave	0.061	-0.002	(0.008)
13. Refrigerator	0.676	0.029	(0.028)
14. Freezer	0.104	-0.007	(0.011)
15. Washing Machine	0.222	-0.020	(0.017)
16. Dish Washer	0.002	0.002	(0.002)
17. Heater	0.122	-0.003	(0.013)
18. Central heating	0.007	-0.001	(0.003)
19. TV	0.818	-0.005	(0.026)
20. VCR	0.056	-0.005	(0.008)
21. Cable TV	0.134	0.030***	(0.013)
22. COMPUTER	0.014	-0.002	(0.004)
23. Car	0.035	0.004	(0.006)
24. Phone	0.263	-0.030	(0.019)
25. Block has electricity	0.977	0.003	(0.007)
26. Block has piped water	0.944	0.009	(0.010)
27. Block has sewage	0.407	0.053**	(0.021)
28. Block has trash collection	0.903	0.019	(0.012)
29. Block has paved streets	0.668	0.017	(0.019)
30. Block has public lighting	0.695	0.047**	(0.019)
31. Home owned	0.388	-0.031	(0.021)
32. Floor	0426	0.035*	(0.021)
33. Wall	0.924	0.007	(0.013)
34. Toilet	0.877	-0.015	(0.015)
35. Number of rooms	2.473	0.107	(0.072)
36. Number of bedrooms	1.749	0.091**	(0.044)
37. Low birthweight	0.089	-0.005	(0.012)
38. Birthweight (g)	3,180	3.303	(23.414)
39. APGAR - 1 minute	8.522	-0.005	(0.046)
40. APGAR - 5 minutes	9.633	0.009	(0.034)
41. Mother works	0.171	0.022	(0.015)
42. Mother earnings during pregnancy	223.98	25.265	(31.791)
43. Household total income during pregnancy	861.284	-74.215	(179.572)

Notes: The table reports reduced form regressions of each variable in column 1 on eligibility indicator and quadratic polynomials in the income score plus month of baseline survey dummies for observations in the range -0.1,0.1. Rows 1 to 36 refer to variables measured in the baseline survey. The remaining rows report results for birth outcomes among pregnancies that occurred before the program period. Rows 38 to 43 additionally control for month of conception dummies. See also notes to Table 2.

APPENDIX FIGURE A1 — THE TIMING OF *PANES* PROGRAM MILESTONES

Notes: The figure reports the distribution of key program dates: application date (A), date of baseline survey (B), date of first payment of income transfer (C), date of first payment of the food card (D). Panels E reports the distribution of the differences between the variables in panels B and A, and Panel F reports the distribution of the differences between the variables in Panels C and B.

PANES application	Baseline visit	Administrative decisions on eligibility	Income transfer	Food card	Program ends
April 2005 →	April 2005 →		May 2005 →	April 2006 →	December 2007

PRE-PROGRAM PERIOD **PROGRAM PERIOD**

time

Baseline survey

April 2005 →

Administrative program data
May 2005 - December 2007

Social security data (program applicants only)

April 2004 - December 2007

Vital statistics data (universe)

January 2003 - December 2007

APPENDIX FIGURE A2 ━ TIMING OF *PANES* PROGRAM ACTIVITIES AND DATA COLLECTION

APPENDIX FIGURE A3 — INCIDENCE OF LOW BIRTHWEIGHT BY WEEKS OF GESTATION AND DISTRIBUTION OF WEEKS OF GESTATION

Notes: The left hand side panel of the figure reports the fraction of low weight births (<2,500 g.) by weeks of gestation. The right hand side panel reports the frequency distribution of weeks of gestation. A vertical line corresponds to 36.5 weeks of gestation. Births to the left of it are labeled as premature. The data refer to *PANES* applicant mothers (whether eligible or ineligible) over the entire period (January 2003 to December 2007).

APPENDIX FIGURE A4 — DIFFERENCE IN THE INCIDENCE OF LOW BIRTH WEIGHT BETWEEN ELIGIBLE AND INELIGIBLE HOUSEHOLDS AT DIFFERENT LAGS AND LEADS TO/SINCE THE TIME OF FIRST PROGRAM PAYMENT

Notes: The figure plots the raw difference in low birth outcomes between eligible and ineligible mothers at different leads and lags from the data of program entry (defined precisely as in the regressions) in a range of plus or minus 2 years (as in Appendix Figure A9, Panel B). We plot this figure for the entire universe of births in the left panel, and just for observations in the range -0.1/+0.1 in the right panel. We superimpose our localized diff-in-diff estimates onto the figure (the fitted lines).

APPENDIX FIGURE A5 — FUZZY RD ESTIMATES OF THE EFFECT OF THE *PANES* TRANSFER ON LOW BIRTHWEIGHT (<2.500 KG)

PROGRAM PERIOD DATA − USING LOCAL LINEAR POLYNOMIALS FOR DIFFERENT VALUE OF THE BANDWIDTH

Notes: The figure reports RD estimates and the associated 95 percent confidence intervals of the effect of the program on the incidence of low birthweight during the program period only using a local linear polynomial in the score. Different estimates for different values of the bandwidth (from half to twice Imbens and Kalyanaraman (2012)'s optimal bandwidth) are reported.

APPENDIX FIGURE A6 — PROPORTION OF LOW WEIGHT BIRTHS PRE-PROGRAM PERIOD, OVER ENTIRE RANGE OF VARIATION OF THE PREDICTED INCOME SCORE

Notes: The figure reports the fraction of low birthweight pre-program, over the entire range of variation of the discretized (in multiples of 0.1) predicted income score. We also present a quadratic fit to the data while two vertical lines report the range of values that we restrict our attention to in our preferred regression specifications.

Panel A: Distribution of the standardized *PANES* predicted income score, McCrary (2008) test

Panel B: Distribution of the birthweight measure (in grams)

APPENDIX FIGURE A7 — DATA INTEGRITY CHECKS FOR THE PREDICTED INCOME SCORE (PANEL A) AND BIRTHWEIGHT (PANEL B)

Notes: Panel A reports the frequency distribution of the income score and a smoothed kernel density estimator on either side of the threshold with the associated confidence interval. Panel B presents a histogram of birthweights in our sample, as recorded in the Uruguay vital statistics system data.

APPENDIX FIGURE A8 — BALANCE TEST: RELATIONSHIP BETWEEN COVARIATES AND THE *PANES* PREDICTED INCOME SCORE

Notes: The figure reports residuals from a regression of each variable on the controls, as in Table A6 (i.e., on month of baseline survey indicator variables and month of conception indicators, depending on the variable) as a function of the income score, in multiples of 0.002. Estimated quadratic regression lines on either side of the threshold plus 95 percent confidence intervals also reported. See also notes to Table A6.

APPENDIX FIGURE A8 — BALANCE TEST: RELATIONSHIP BETWEEN COVARIATES AND THE *PANES* PREDICTED INCOME SCORE (CONTINUED)

APPENDIX FIGURE A8 — BALANCE TEST: RELATIONSHIP BETWEEN COVARIATES AND THE *PANES* PREDICTED INCOME SCORE (CONTINUED)

A: Fraction of children whose household was ever treated, as a function of date of birth relative to date of baseline survey visit

B: Fertilty rate as a function of (imputed) time to/since first income transfer by *PANES* eligibility status

C: Fertilty rate as a function of (imputed) time to/since first income transfer by *PANES* eligibility status - no children before the program

D: Fertilty rate as a function of (imputed) time to/since first income transfer by *PANES* eligibility status – one child before the program

E: Fertilty rate as a function of (imputed) time to/since first income transfer by *PANES* eligibility status - two or more children before the program

APPENDIX FIGURE A9 — FERTILITY RATES AND FRACTION TREATED AS A FUNCTION OF TIME TO/SINCE KEY PROGRAM DATES

Notes: Panel A reports the fraction of children of applicant mothers whose household ever benefitted from the program as a function of the child's day of birth relative to the day of the baseline survey. Panel B reports the fraction of mothers giving birth as a function of the time before/after the month of first program payment, separately for *PANES* eligible and *PANES* ineligible women, in the range (-0.1, 0.1) around the eligibility threshold. Panels C, D and E report the same series as in Panel B separately by number of children born between January 2003 and the baseline survey.

Appendix B: *PANES* Program components

The program also contained a variety of other minor components. Around 15 percent of *PANES* households had one member attending training and educational activities organized by local NGOs (*Rutas de Salida*) with the aim of fostering social inclusion by recovering the lost work habits of participants, promoting knowledge of rights, strengthening social ties and, in some cases, promoting good health and nutrition practices. Around 16 percent of *PANES* households had one member participating in a workfare program (*Trabajo por Uruguay*). Some participants were also incentivized to undergo routine medical checks (smear tests, prenatal visits and mammography for women and prostate exam for men) and were offered dental care and prostheses and eye surgery. Households in the treatment group received the monthly income provided they were not involved in public works employment (*Trabajo por Uruguay*), which paid a monthly salary of UY$2,720 in lieu of the cash transfer. Participation in this employment scheme was voluntary and, among households who applied for jobs, participants were selected by lottery. In the paper we define program beneficiaries those receiving either the *Ingreso Ciudadano* (Citizen Income, the cash transfer) or *Trabajo por Uruguay*. As of spring 2007, nearly all eligible households declared having received the cash transfer at some point during the program, 71 percent reported having received the Food Card while only a minority (17.6 percent) benefited from public works employment. Additional components of the *PANES* program included: regularization of beneficiaries' connection to public utilities networks (water and electricity) for a nominal fee, in-kind transfers of building materials for home improvements; health care including free dental and eye care (e.g., cataract surgery performed in Cuba) and prostheses; micro-finance loans and technical assistance for small entrepreneurial activities; and temporary accommodation for homeless households. Overall, around 13 percent of beneficiary households reported having received at least one of these additional components. *PANES* also encompassed schooling and health investments (additional school teachers in disadvantaged neighborhoods and public health investments). These affected beneficiary and non-beneficiary households equally. Although an emergency health plan (*Plan de Emergencia Sanitaria*) was also originally conceived as an integral part of *PANES*, this was not *de facto* implemented.