Do Cash Transfers Improve Birth Outcomes?

Evidence from Matched Vital Statistics, Program and Social Security Data

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There is limited empirical evidence on whether cash social assistance to poor pregnant women improves children's birth outcomes. Using program administrative micro-data matched to longitudinal vital statistics on the universe of births in Uruguay, we estimate that participation in a generous cash transfer program led to a sizeable 15 to 17% reduction in the incidence of low birth weight. Improvements in mother nutrition and a fall in labor supply, out-of-wedlock births and mother's smoking all appear to contribute to this effect. Effects are not driven by changes in fertility. We conclude that, by improving child health, cash transfers may help break the cycle of intergenerational poverty.

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1. Introduction

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*), which provided households with a sizeable cash transfer – on children's early health outcomes. An unusually rich dataset of matched micro-data from vital statistics, hospital data, social security and program administrative records allows us to exploit multiple sources of quasi-experimental variation in the receipt of cash assistance, enabling us to estimate the causal effect of additional disposable income during pregnancy on birth outcomes and investigate a range of underlying mechanisms.

Although there appears to be 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; see McCrary and Royer 2010 for a critique), there is less evidence that temporary interventions in the form of cash transfers to pregnant women can significantly affect birth outcomes. In their comprehensive discussion of the determinants and consequences of early human capital development, Almond and Currie (2011a, p. 1368), for example, 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 have argued, 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 (e.g., education, 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 1995, 2000).

Improvements in household financial resources brought about by social assistance could, in principle, increase children's wellbeing through better nutrition, sanitation and health care (Case 2000; Case, Lubotsky and Paxson 2005). The reduction in maternal stress brought about by welfare transfers might also positively impact birth outcomes.

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 2000, 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 (Banerjee and Mullainathan 2010), imperfect altruism (Udry 2004), intergenerational commitment problems (Baland and Robinson 2000), or limited information about the technology of, or returns to, their children's human capital accumulation (Jensen 2010). Social assistance could even potentially increase the consumption of certain "bads" (such as cigarettes, drugs or alcohol) that negatively affect birth outcomes, could lead to family break-up (Moffitt 1998) with detrimental effects on child wellbeing, or could increase the fraction of children born in poor health by creating incentives for poor women to boost their fertility (Currie and Moretti 2008).[1]

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

[1] Indeed, it is precisely because of some of these undesirable effects that in-kind (e.g., food stamps) or conditional social assistance is often advocated. As long as households would consume less of the good if provided with an equivalent monetary transfer, and the goods and services transferred are not fungible for money, in-kind transfers have the potential to increase the consumption of such goods (see Currie and Gahvari 2008). The conditionalities attached to many recent cash transfer programs in less developed countries are similarly meant to induce specific desirable behaviors among participants (Fiszbein and Schady 2009).

birthweight, defined by the World Health Organization as weight under 2,500 g (or roughly 5.5 pounds). This is a widely available measure, and a considerable body of research shows that it is a major determinant of both short-run child health outcomes and long-run life outcomes, including height, IQ, earnings, education and even birthweight of the next generation (see Almond, Chay and Lee 2005, Almond and Currie 2011a, Almond, Hoynes and Schanzenbach 2011b, Behrman and Rosenzweig 2004, Black, Devereux and Salvanes 2007, Currie and Hyson 1999, Currie and Moretti 2007, Currie 2009, Royer 2009, among others).

With a few notable exceptions, evidence on the effect of *in utero* exposure to cash 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 cash transfers. The main channels of impact are also poorly understood, again in part due to data limitations. Another important open question is the stage of pregnancy at which such programs are most effective. Since pregnancy status is often ascertained with some delay, and this is likely to be particularly true for women whose children are most likely to benefit from early interventions, *in utero* targeting might be particularly challenging.

This paper contributes to filling these gaps. Beyond specifically focusing on a social assistance program whose major component was a cash transfer, the contribution of this paper lies in the data set that we have assembled and the opportunities that it offers for econometric identification of both program effects and mechanisms. We link multiple sources of administrative micro-data to build a monthly longitudinal data set spanning five years of individual women's pregnancy and birth outcomes, clinical histories and circumstances surrounding the pregnancy and birth; program transfers; and socio-demographic characteristics and labor market participation, earnings, and the receipt of other public benefits for the universe of female program applicants of child-bearing age, approximately 157,000 women.

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 they rely on geographically aggregated program enrollment and vital statistics data. Because of the aggregate nature of the data used in many related studies, estimation of program effects 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 eligibility, possibly inducing considerable omitted variable bias. In contrast, our data allows us to exploit variation in individual eligibility induced by exact program assignment rules and timing, leading to more credible identification of causal program effects.

To estimate program effects, we compare the incidence of low birthweight among the infants of program beneficiaries versus non-beneficiaries born before and after the *PANES* program was launched. Households entered the program at different points in time during its phased national roll-out, inducing further variation in treatment timing and allowing us to control for any aggregate trends in low birthweight.

In addition, our results remain essentially unchanged when we use alternative sources of treatment variation in certain subsamples. We first restrict the analysis to mothers with more than one child and include maternal fixed effects in order to control for any unobserved time-invariant heterogeneity across mothers. Second, since *PANES* program eligibility depended on a discontinuous function of a baseline predicted income score, we restrict attention to children whose households are in the neighborhood of the program eligibility threshold and compare "barely eligible" to "barely ineligible" children using a regression discontinuity design. The lack of low birthweight trends in pre-program data serves as a useful falsification exercise.

To preview our results, we find that the program led to a roughly 15 to 17 percent decrease in the incidence of low birthweight (1.5 to 1.7 percentage points on baseline incidence of 10 percent), using all three econometric approaches. In two years, the program completely 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. The analysis shows that providing cash social assistance to the poor improves early child health outcomes. We show in particular that these results are driven by exposure during the entire last trimester of pregnancy, suggesting that cash transfers explicitly targeted to poor pregnant women might be successful in improving birth outcomes if they can be identified sufficiently early in their pregnancy.

We also show that compared to the ineligible control group, *PANES* mothers display weight gains during pregnancy, a drop in their labor supply and a reduction in smoking. The results are consistent with the view that improved nutrition as well as, more speculatively, a reduction in stress likely contribute to our findings. There are also sharp drops in out-of-wedlock births among program beneficiaries.

Just as we can identify mediating factors that help explain the birthweight impacts, we can also rule out a number of alternative mechanisms. In particular, neither increases in prenatal care utilization nor changes in fertility patterns drive our results. We are also able to rule out effects due to residential mobility, changes in health insurance coverage, access to other government benefits, and to the smaller in-kind (food card) component of the program. In all, despite the body of theoretical and empirical research suggesting that welfare transfers may induce behavioral responses that largely offset any potential child health gains, these concerns seem to be absent (or second-order) in our setting. Instead, cash transfers to the poor positively affect children's birth outcomes, and thus may help break the cycle of inter-generational poverty.

The paper is organized as follows. Section 2 reviews the literature on the determinants of birthweight and in particular on the effect of government transfer programs. Section 3 provides institutional information about the program. Section 4 describes the data, section 5 discusses multiple identification strategies and presents the main results, while section 6 investigates the mechanisms, and presents a speculative rate of return analysis that suggests program transfers may be an attractive policy option. The final section concludes.

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

A large body of research points to maternal nutrition and both physical and mental health during pregnancy as major determinants of birth outcomes in general, and low birthweight in particular. Although low birthweight is mechanically linked to prematurity, the major concern is related to low birthweight resulting from intra-uterine growth retardation, which is thought to depend on mother's poor health, smoking and undernutrition.[2] There is also a consensus that prenatal care, especially in the first trimester of pregnancy, is effective at improving infant health through the

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[2] Maternal under-nutrition, anemia, malaria, infections, pre-eclampsia and cigarette smoking are typically identified as important risk factors for intrauterine growth retardation (Kramer 1987). Almond and Mazumder (2011) show that maternal fasting has negative effects on birthweight. Other risk factors include environmental pollution (Currie and Schmieder 2009, Currie, Neidell and Schmieder 2009, Currie and Walker 2010), exposure to violence (Camacho 2008, Aizer, Stroud and Buka 2009, Aizer 2010) and mother's labor supply, possibly due to stress (Del Bono, Ermisch and Francesconi 2008). Kramer (1987) identifies genital tract infections, employment and physical activity, smoking behavior, stress, general morbidity and prenatal care as the main predictors of gestational length. The evidence also points to the role of mother's nutrition (Murtaugh and Weingart 1995), anthropometric measures, genetic factors, and stress (Clausson, Lichtenstein and Cnattingius 2000).

opportunities it provides for early diagnosis and for educating the mother about best practices (Kramer 1987, Alexander and Korenbrot 1995).

Recent attempts to link birthweight to household socioeconomic status and to economic characteristics that correlate with the above risk factors have generated mixed results. There is evidence that higher maternal education improves infant outcomes, arguably due to its effect on maternal behavior (for example, by reducing smoking), increased earnings, improved marriage outcomes and reduced fertility (Currie and Moretti 2003), although other work does not corroborate this result (McCrary and Royer 2010). There is limited evidence that mother's disposable income during pregnancy affects low birthweight, though some claim that an effect may be present for specific subpopulations, such as mothers who were themselves born with low birthweight (Conley and Bennett 2000, 2001).[3]

More direct - and relevant (for this paper) - evidence comes from studies that analyze government welfare and transfer programs. A body of evidence, largely from the United States, focuses on programs that aim to improve the nutritional and health status of pregnant women. Bitler and Currie (2004) and Hoynes, Page and Huff Stevens (2010) 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 the program reduces the incidence of low birthweight infants. One channel through which WIC appears to have an effect is via greater prenatal care utilization. One limitation of these studies, though, is that in order to control for selection into treatment they use either a simple selection-on-observables strategy or exploit

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[3] Aggregate macroeconomic conditions also appear to matter, with different studies again reaching divergent conclusions. Some point to increases in low birthweight during economic downturns and others to decreases (Dehejia and Lleras-Muney 2004, Bozzoli and Quintan-Domeque 2010). Interpretation of these aggregate results is complicated by compositional differences in the types of mothers giving birth throughout the business cycle.

the variation in take-up generated by program roll-out across counties, rather than the household-level variation in treatment status and timing that we are able to exploit.

Additional evidence comes from the conditional cash transfers literature. Barber and Gertler (2008) evaluate the impact of the Mexican *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%) which they attribute to better quality prenatal care and the adoption of better health behaviors. Admittedly, the interpretation of their results is complicated by the fact that the *Progresa/Oportunidades* program also increased the local supply of health care, and that it featured well-publicized health and education conditionalities.

Other studies exploit the roll-out of the Food Stamps program across U.S. counties, with mixed results, a possible indication of the pitfalls of this econometric identification strategy. 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. Yet Currie and Moretti (2008) do not find this pattern for California, a fact that they explain with increased fertility (due to the program) among a subset of mothers more likely to have worse birth 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 impacts on birth outcomes. 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 eligibility criteria for AFDC across U.S. states.

One of the most convincing and relevant papers is Hoynes, Miller and Simon (2011), who focus 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 a sizeable average reduction of 7% in low birthweight incidence, with particularly pronounced effects among less educated and ethnic minority mothers. Although the authors hypothesize about various channels, a limitation is that their data does not allow them to directly investigate them empirically.

3. 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.[4] The program was devised by the center-left government that took office in 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

[4] 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 the *Plan de Equidad*.

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 dissimilar to the U.S. (appendix Table A1).[5]

3.1 Program Eligibility

Following an initial program application phase (which mainly occurred in April and May 2005), 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 and resources needed to administer the survey, these household visits took place throughout most of the second half of 2005, sometimes with considerable delay from the original application date (see appendix Figures A1 and A2).

The baseline survey allowed program officials to compute a predicted income score based on a linear combination of a large number of household socioeconomic characteristics, which in turn determined program eligibility.[6] Households with a predicted income score below

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[5] Uruguay is 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 generous old-age pensions. Uruguay is still among the most developed Latin American countries according to the UNDP Human Development Index, with relatively high life expectancy and schooling indicators.

[6] The eligibility score, which was devised by researchers at the University of the Republic in Montevideo (Amarante et al., 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 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", in this paper we use a predicted income

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 ever informed about the exact variables that entered into the score, the weights attached to them, or the program eligibility threshold, easing concerns about score manipulation (which we further assess in appendix Figure A3).

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). [7]

3.2 Program Components

PANES eligible households were entitled to a monthly cash transfer whose value was originally set at US$103 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 50 percent of average pre-program household self-reported income for recipient households (and nearly 100% of pre-treatment income among those households where a member gave birth during the period of analysis). Most households first received the 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 among the earliest applicants (appendix Figure A2).

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.

[7] 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% of GDP.

Successful applicants were entitled to the transfer for the duration of the program until December 2007, provided their formal sector earnings remained below a predetermined level (approximately US$100 per month per capita in PPP terms).[8]

A second, smaller program component only launched midway through the program, in mid-2006, was an electronic food card, whose monthly value varied approximately between 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.[9]

Although *PANES* transfers were originally meant to be conditional on health checks for pregnant women and children (plus school attendance for children), similar to other recent Latin American cash transfer programs such as *Progresa/Oportunidades,* the exact conditionalities were not laid out until mid-2007 – two years into the program and only months away from the end of *PANES* transfers.[10] Even then, due to a lack of coordination among the multiple institutional actors involved, the conditionalities were *de facto* not enforced. This was eventually acknowledged by the government and widely discussed in the local press (*El Pais* 2007, *El Espectador* 2007), and there is no record of any *PANES* household having lost eligibility due to failure to fulfill the conditionalities. Additional evidence that health conditionalities were not a central part of the program comes from a sample of around 2,000 beneficiary households surveyed in early 2007, among whom only 12% named "prenatal visits" as a condition for

[8] The social security administration performed periodic checks on *PANES* beneficiaries' records to enforce this condition. As shown below, there is evidence that 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.

[9] See Appendix B for a detailed discussion of these and other aspects of the program.

[10] The information in this this paragraph comes largely from personal communication with Gerardo Lorber, at the time Coordinator of the Emergency Health Plan – the health component of *PANES* – at the Ministry of Social Development. Once specified, the health conditionalities for pregnant women included monthly prenatal visits, biweekly visits starting in week 32 of pregnancy, and weekly visits after week 36 (*Ministerio de Salud Publica* 2007). At the same time as the specification of the conditionalities in mid-2007, the government also mailed *PANES* households a personalized "health card" where prenatal checks could be recorded and certified by medical staff. It is worth noting that prenatal visits for pregnant women were already legally mandatory and free of charge in Uruguay since long before the *PANES* program, although these requirements were never strictly enforced.

program receipt.[11] The bottom line is that there were effectively no health (or education) conditionalities associated with *PANES* transfers and only a small minority of households were even aware that such conditionalities might formally exist or eventually be introduced.

4. Data

The analysis brings together several individual-level data sets (appendix figure A1). *PANES* administrative records provide information from the initial survey visit for both successful ("eligible") and unsuccessful ("ineligible") applicants on baseline 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 household members, and allow us to identify individuals belonging to the same household. 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). Vital statistics come from certificates completed by physicians at the time of birth and they contain information on birthweight, some parental characteristics, and the reproductive history of the mother. At 98 percent, the fraction of registered births in Uruguay is the highest in Latin America (UNICEF 2005, Cabella and Peri 2005, Duryea, Olgiati and Stone 2006). Vital statistics data are available every year since 1997, although they only not report mother's *cédula* from 2003 onwards. For this reason, in most of the analysis we limit the use of vital statistics to the

[11] Authors' calculations based on the first wave of the *PANES* follow-up survey (*Primera Encuesta de Seguimiento*).

period 2003-2007 that also includes the period before the start of *PANES*, in April 2005. These data also include some information on prenatal care utilization that is collected as the pregnancy progresses. The confidential version of the data used in this paper includes the mother's *cédula*, and this allows us to link the vital statistics to program data.

Additional information on maternal health during pregnancy is provided by the SIP (in Spanish, *Sistema de Informático Perinatal*, or prenatal information system) database, devised by the Latin American Centre of Perinatology and collected in multiple Latin American countries (Fescina, Butrón, De Mucio, Martínez, Diaz-Roselló, Simini, Camacho and Mainero 2007). This dataset collects detailed pregnancy information, including mothers' weight at the time of both the first and final pre-natal visits, as well as smoking in the first trimester of pregnancy. One drawback of the data, though, is that there is incomplete coverage during the program period (full national coverage was only achieved in 2009), although during the 2003 to 2007 period its coverage increases. As a result, SIP data are available for a subset of roughly one third of the births in our main analysis. Fortunately, we show below that coverage rates are identical among *PANES* eligible and ineligible mothers.

Finally, we also link program and vital statistics data to Social Security records for all members of *PANES* applicant households, again using the unique *cédula* individual number. These data contain monthly information on income from formal employment (for both employees and the self-employed, excluding non-civilians, i.e., the military and police), and all public transfers, including pensions, unemployment benefits, disability and a small pre-existing child allowance (that had negligible transfer amounts relative to *PANES*). Social security data are available to us starting in March 2004, and thus are available for more than a year before the launch of *PANES*.

The data are summarized in Table 1. The top three panels report averages for the period January 2003 to March 2005 before the start of the program, while the bottom panel reports information for April 2005 to December 2007. We report means for three groups of mothers: those who applied and eventually became eligible for *PANES* (column 1), those who went through the full program application process but were unsuccessful (column 2), and those who did not apply (column 3). Roughly speaking, these three groups correspond to increasingly higher levels of income and socio-economic status.

The data show a clear gradient in birthweight across groups (rows 1 and 2). While among *PANES* eligible households the fraction of births below 2,500 grams is 10.2 percent, among non-applicant households it is 8.4 percent, and for ineligible applicants it lies in between, at 9.3 percent. There is also clear 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, row 4, 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 5). *PANES* eligible mothers were also more likely to live in areas with lower average birthweight (row 9), more likely to give birth in public health centers (row 11) and less likely to be privately insured (row 12).[12]

There is 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 13) and positively correlated with the number of previous

[12] 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.

pregnancies (row 14). *PANES* eligible mothers are less likely to be married to the father's child (row 15).[13] *PANES* fathers also display lower levels of education (row 17).

Unsurprisingly, *PANES* eligible mothers are also less likely to report being employed at the time of birth (row 18), have lower formal sector earnings during pregnancy (row 19) and belong to households with less labor and non-labor income (rows 20 to 22). Total household monthly income (including earnings and benefits) in the first two trimesters of pregnancy is UY$1,113 (in April 2005 UY$) for *PANES* mothers, and around twice as much for ineligible applicant mothers. Although this figure is likely to underestimate true income levels among these households, as it excludes earnings from informal employment and any non-governmental transfers, it remains very low at approximately US$90 (PPP-adjusted).

In the pre-program period, the SIP data show that a large share of mothers smoked, at 31% in the *PANES* eligible group and 25% among ineligibles (row 26), and as expected, these mothers displayed lower average body weight (rows 27 and 28). The finding that poor mothers weigh less is consistent with the existence of some under-nutrition at baseline.

Panel D in Table 2 reports data for the program period. It is notable that the gap in low birthweight 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 low birthweight incidence of 9.1 percent (row 29). Around 97 percent of *PANES* eligible mothers received the program at some point during the period (row 30), although only around 55 percent received it sometime in the first two trimesters of pregnancy (row 31). This gap is due both to the staggered incorporation of households into the program (discussed above) as well as to some beneficiaries losing eligibility due to their eventual failure to meet the income means test.

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

Although a small share of ineligible mothers also eventually received transfers, initial eligibility remains a very strong predictor of program receipt.[14] *PANES* eligible households do not receive more cash benefits through other government programs than ineligible households (row 38), and in fact they receive a somewhat lower level of non-*PANES* transfers, as they also did pre-program (row 21). Taken together, the gap in total household income between eligible and ineligible households closes substantially (row 39), largely due to the *PANES* transfer (row 32).

5. Econometric analysis

The discussion of the program implies that for a child to have been exposed to the *PANES* program *in utero* two conditions must be satisfied: first, the mother must be a program beneficiary, and second, the child must have been born after the mother entered the program. This immediately suggests a difference-in-differences strategy for estimating program effects that relies on a comparison of birth outcomes for children born to program eligible versus ineligible mothers, both before and after program expansion. The basic regression model is then:

$$Y_{imt} = \alpha + \beta^{DD} T_{imt} + d_t + d_p + u_{imt} \quad (1)$$

where t is the month of conception of child i of mother m, Y is the birth outcome variable (e.g., low birthweight), and T is an indicator for treatment. The terms d_t and d_p are, respectively, indicators for month of conception and month of the household's first *PANES* cash payment.

Equation 1 exploits the staggered entry into the program across mothers (appendix Figure A2), and compares the difference in birthweight between a treated child (one born after her

[14] A related paper (Manacorda et al. 2011) presents 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, plus it excludes homeless households, who were always incorporated into *PANES* regardless of their predicted income score. In the data used in the present paper, we find evidence of slightly laxer enforcement of the eligibility rules in the final six months of the program (namely, the second semester of 2007).

mother was enrolled in the program) and an untreated child to the difference between two children with identical dates of conception who were either both treated or both untreated. By conditioning on date of conception indicators, equation 1 controls for general trends in the incidence of low birthweight due to, for instance, secular improvement in health care quality or living standards, while conditioning on d_p controls for the possibility that birthweight outcomes might vary across mothers with different program entry timing, perhaps due to the selective nature of application timing or in the length of application processing. The panel data means we can account for any pre-existing differences in birth outcomes among those with different program entry timing using data from the pre-program period.

As our dataset contains detailed information on weeks of gestation, we are able to measure program exposure in terms of time elapsed since conception as opposed to the time before birth (as is customary in most existing studies). This subtle distinction is important and allows us to circumvent the potential selection bias that would arise if program participation affected gestational length, where the latter were correlated with birth outcomes. A related estimation issue is that children with shorter gestational lengths will mechanically have a shorter period of program "exposure", potentially biasing program impact estimates. To address this, in the analysis below we define treatment as a child's mother having started to receive *PANES* payments at any point up to six months after conception, regardless of the timing of the birth. For children with normal gestational length, and in the absence of program drop-out, this measure is equivalent to *in utero* exposure during the entire third trimester, a potentially critical period.

A further issue relates to program drop-out driven by the income conditionality attached to the program, which over time disqualifies a non-trivial fraction of beneficiary households. A household economic shock during pregnancy, e.g., finding a job, might affect both birth outcomes and household income, and hence program participation, again potentially biasing program impact estimates. A related issue is imperfect enforcement of the eligibility rules, which we showed above affects a moderate proportion of originally ineligible households. If such receipt among ineligible households is correlated with their birth outcomes, i.e., if mothers who know how to "work the system" to eventually get *PANES* benefits are also more determined in accessing prenatal care, this could bias estimates. To address these issues, we instrument the *PANES* treatment term with an indicator E_{imt} that takes on a value of one for *PANES* eligible women's pregnancies in which the date of the first cash payment occurs sometime before the end of the second trimester.[15] The first stage equation is then:

(2) $$T_{imt} = \phi + \delta^{DD} E_{imt} + f_t + f_p + v_{imt}$$

where f_t and f_p are again, respectively, indicators for month of conception and month at which the household received its first *PANES* payment.

Two further issues are worth mentioning. First, fertility might be endogenous to program eligibility and cash transfer receipt. This might be the case because the program affects the likelihood of conception (via changing access to contraception or evolving fertility preferences), or of successfully completing a pregnancy through selective fetal survival or abortion.[16] Endogenous fertility choices could lead to bias if the types of mothers whose fertility is affected have different risk of low birthweight. A second issue is selective program entry times, if the incidence of low birthweight is correlated with this timing. We return to both of these important issues below after presenting the main results.

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[15] In practice, if we denote the month of conception of child *i* by t_i and the month of the first *PANES* payment for mother *m* by p_m, then $E_{imt}=1(p_m \leq t_i + 6)$, which takes on a value of one for all children of *PANES* eligible mothers who are conceived no earlier than six months before their household joins the program.

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

5.1 Main Low Birthweight Results

Figure 1 presents the incidence of low birthweight as a function of the time to and since the first *PANES* payment, restricting attention to mothers who were (eventually) eligible and treated. Each point corresponds to a three month period (and thus the numbers on horizontal axis correspond to years), and the data are standardized such that zero corresponds to births with positive but less than one full trimester of *in utero* exposure, meaning that the mother started receiving the program sometime during the third trimester of pregnancy. There is a visible drop in the incidence of low birthweight among children with positive program exposure relative to those born before the mother entered the program (with negative exposure). There is an additional drop among those whose mother entered the program in the second semester of pregnancy (who had at least one full trimester of exposure). Beyond that, the incidence of low birthweight remains roughly constant (with some apparent sampling variability), implying that there is no additional gain for mothers entering the program even earlier in the pregnancy or before conception.

Table 2 presents 2SLS estimates of the effect of program exposure based on equations 1 and 2. Each row presents results for a different dependent variable, and standard errors are clustered at the mother level throughout. Although the model can also be estimated using only *PANES* eligible mothers (as we do in certain specifications below), including non-*PANES* mothers (unsuccessful applicants) increases precision and so they are included in most of the analysis. In this case, we include an indicator variable for the household predicted income score, N_m, falling in the *PANES* eligible range $1(N_m < 0)$ and we set all d_p and f_p terms to zero for the ineligible mothers. Consistent with Figure 1 and Table 1, the estimates in rows 1 and 2 of Table 2 show that low birthweight incidence falls by around 1.5 percentage points, while mean child birthweight increases by 24 grams (or almost exactly one ounce) following program exposure, with both estimates significant at 95% confidence. In column 2 of Table 2, results remain virtually unchanged and retain statistical significant with the inclusion of additional covariates (including indicators for mother's age and education, sex of the child, twin births, the number of previous pregnancies, month of the baseline survey visit, and month of program enrollment), with the low birthweight effect increasing slightly to -1.7 percentage points.

Figure 2 reports the implied proportional change in the fraction of newborns below any given weight level, together with the associated 95 percent confidence interval; these coefficients are estimated based on a series of regressions similar to those in row 1, column 2 (of Table 2). Program exposure significantly reduces the incidence of a range of birthweights below 3,000 g, with effects again on the order of 10 to 15 percent, although estimates lose statistical significance for weights below approximately 2,300 g due to the drop in observations in this range.

The data also allow us to estimate the differential impacts of the program at different points in the pregnancy. Table 2, row 3 reports OLS estimates from a regression of low birthweight on indicators for different lengths of *PANES* exposure (i.e., less than one trimester, one full trimester, two full trimesters, or three full trimesters and more, the latter meaning that the mother entered the program before conceiving). Coefficients are expressed as differences relative to zero exposure, i.e., to children who were not eligible for the program at any time during the pregnancy. For comparison, row 4 reports reduced form estimates that pool as "treated" those eligible starting in the second trimester or earlier. Consistent with Almond, Hoynes and Schanzenbach (2011a) and with the evidence in Figure 1, we find a significant reduction in the incidence of low birthweight, on the order of -1 to -2 percentage points, from entering the program at any point in the second trimester or earlier, and this pattern is consistent with full program exposure during the third trimester mattering most for child growth. Moreover, conditional on exposure during the entire third trimester, we cannot reject the hypothesis that the effect of *PANES* is the same for those who began receiving transfers in the second trimester, the first trimester, and before conception at conventional confidence levels (not shown).

Finally, the first stage relationship is presented in row 5 of Panel B, and implies that program eligibility increases program participation by 86 percentage points, suggesting a high degree of compliance with assigned program status.

5.2 Testing for endogenous program entry dates

A potential concern with the previous estimates is selective program entry times, if the incidence of low birthweight is correlated with this timing. A leading concern is that the beneficiary households with earlier program entry dates were also the ones whose children's birthweight was likely to increase for reasons unrelated the program. This might be the case if the program first targeted women who were most severely hit by the crisis and thus whose birth outcomes were also likely to rapidly revert to pre-crisis levels during the ensuing economic recovery. In this case, program exposure could be spuriously associated with improved birth outcomes.

As a way to test for this potential source of bias, we allow the instrumental variable for treatment in equation 2 (E_{imt}) to take on a value of one for all pregnancies that started no earlier than January 2005 among *PANES* eligible mothers, regardless of the actual date of the respondent's entry into the program.[17] This approach classifies all children of *PANES*-eligible mothers born in the second trimester of 2005 or later as program participants. This is a conservative assumption that introduces some measurement error in actual program participation

[17] Note that in this case, the ineligible households are always included in the analysis for identification purposes.

dates but eliminates any bias due to selective entry timing. The coefficient estimate on *PANES* participation using this approach is reported in Table 3, row 1. For brevity, we focus on specifications with additional controls (as in column 2 of Table 2), although the results are unchanged without controls (not shown). The point estimate is very similar to the baseline estimate, at -0.020 (s.e. 0.008) and significant at 95% confidence. This approach removes all variation driven by differential beneficiary program entry timing, and the robustness of results in this case greatly eases concerns about bias due to endogenous household program entry timing.

Even if we abstract away from the variation in entry dates among the eventually eligible, a second concern is that pre-existing differential trends leading to convergence in low birthweight between the eligible and ineligible predated the program. To check for this, one would ideally have access to a sufficiently long time series starting well before *PANES* inception in order to show that the birthweight gap between children of those eventually eligible and ineligible for the program closed precisely when the program began. As the Uruguayan vital statistics records do not report mother's *cédula* before 2003, it is impossible for us to link earlier natality data to program administrative data and hence exactly identify eligible and ineligible mothers over a long period of time, unfortunately. To partially circumvent this data limitation, we present low birth weight results over time for two groups of mothers defined based on their educational attainment: those who have not completed primary school and those who completed primary school. Using the years where we do have program records, the ratio of eligible mothers to ineligible mothers among the "incomplete primary" group is 5 to 1, while the ratio of eligible to ineligible mothers is 2 to 1 among those who completed primary school. Thus education is a reasonable proxy for *PANES* eligibility status.

Figure 3 reports the average birthweight (Panel A) and the fraction of low birthweight children (Panel B) for these two educational groups between 1996 and the first semester of 2008. One can see a clear deterioration in birth outcomes among children of less educated mothers during the economic crisis starting in 2002. Trends start to reverse in the second half of 2005, exactly when the first cohort of children exposed to the program was born. [18] The patterns in this figure are at odds with the claim that there were already pre-existing trends leading to convergence in birth outcomes across those households likely to be *PANES* eligible and ineligible in the years before 2005, but are consistent with the hypothesis that the program improved birth outcomes. While some of the convergence in birth outcomes may be driven by mean reversion during the economic recovery, the gap between birth outcomes in the two educational categories is rapidly reduced in the post-2005 (*PANES*) period, and becomes much smaller than it was in the pre-2002 (crisis) period, and the program is the leading explanation for this rapid convergence in outcomes.

As a final piece of evidence against endogenous program timing as a driver of our findings, we present a falsification test in which we impose “fake” program start dates. In particular, we assume that the program started in turn in each month prior to January 2005. If there were pre-existing differential trends in low birthweight before the program, one would expect the coefficients on the placebo treatment indicators to turn negative. Results from this exercise are reported in Figure A4 in the appendix, where each point is the reduced form coefficient obtained by setting the treatment assignment variable E_{imt} equal to one for all pregnancies (of eventually eligible mothers) that started in that month or later; 95% confidence intervals are also reported. Analysis is restricted to the sample of pregnancies that were initiated

[18] There are similar patterns over time in the two educational groups, but trends are much more pronounced in the group that did not complete primary school, where the proportion of eligible households is far higher.

before January 2005 (pre-program). There is no clear pattern in the estimates, with some being small and positive and others negative but none significant at conventional levels. This specification check provides further evidence against the hypothesis that the *PANES* program just happened to start at a time when birth outcomes among the poorest households were already improving for other reasons.

5.3 Alternative identification strategies

As an alternative approach to demonstrating the robustness of the results, we present estimates from alternative identification strategies that exploit different sources of variation in the data. Because of the longitudinal nature of the data, we can first refine the difference-in-differences strategy with the inclusion of mother fixed effects, d_m and f_m, in equations 1 and 2, rather than the d_p and f_p terms. This allows us to control for unobserved time-invariant mother heterogeneity to address any concerns about compositional changes in the population of mothers.

(3) $$Y_{imt} = \alpha + \beta^{FE}\, T_{imt} + d_t + d_m + u_{imt}$$

We also present results that use only those mothers eventually eligible for *PANES* as opposed to both the eligible and the ineligible.

Finally, given program assignment rules, an alternative estimate of program impacts can be obtained by comparing outcomes of "barely eligible" and "barely ineligible" children in the neighborhood of the program eligibility threshold based on a regression discontinuity design. Following Card and Lee (2008), we estimate the following model using program period data:

(4) $$Y_{imt} = \alpha + \beta^{RD}\, T_{imt} + g_1(N_m) + 1(N_m\text{<}0)\, g_2(N_m) + u_{imt}\,,$$

where $g_1(N_m)$ and $g_2(N_m)$ are two parametric polynomials in the predicted income score normalized to the value of the threshold for mother m (N_m) such that $g_1(0)=g_2(0)=0$. We again use 2SLS, where we instrument T_{imt} with $1(N_m<0)$. This approach allows us to address the variation in eligibility associated with different application and entry dates.

All of these alternative identification strategies only make use of subsets of the data, and because of this they could potentially generate somewhat less precise estimates. Row 2 of Table 3 presents estimates including mother fixed effects. Despite cutting the sample size in half, including mother fixed effects leads to an estimate (-0.018) that is nearly identical to the baseline estimate and is significant at 95% confidence, suggesting that unobserved time-invariant individual heterogeneity is not generating a spurious regression result.

While the baseline difference-in-differences specifications in Table 2 include both *PANES* eligible and ineligible mothers (with N=68,858), the point estimate computed only among the sample of *PANES* eligible mothers in row 3 of Table 3 (N=48,891), which exploits changes in their eligibility over time, is nearly identical at -0.016 and significant at 95%.

We finally turn to the regression discontinuity estimates. Figure 4 presents the proportion of low weight births as a function of the predicted income score in the narrow range from -0.1 to +0.1 (denoting plus or minus 10 percentage points in the likelihood that the household qualifies as *PANES* eligible based on its characteristics as measured in the baseline survey) near the program eligibility threshold, both during (Panel A) and before (Panel B) the program, as well as showing the proportion of births in treated households (Panel C). Linear and non-parametric regression curves estimated on both sides of the threshold (and the confidence intervals on the linear estimates) are also presented. The data visually indicate a drop in the incidence of low birthweight at the eligibility threshold during the program period on the order of 1.5 percentage points (Panel A). There is no discontinuity in the proportion of low weight births in the pre-program period (Panel B), consistent with the key identifying assumption that, absent *PANES*, low birthweight would be a continuous function of the predicted income score. Program participation is highly discontinuous at the predicted income score threshold, with a jump of approximately 70 percentage points, as dictated by program assignment rules.[19] The natural conclusion from Figure 4 is that *PANES* program participation significantly reduces the incidence of low birthweight.

This finding is confirmed by the "fuzzy" regression discontinuity estimates in rows 4 to 6 of Table 3. The sample here is restricted to pregnancies that occurred after the program started.[20] Except for indicator variables for month of entry into the program, the model includes the same regressors as in column 2 of Table 2, and the treatment variable is instrumented with an indicator equal to one for mothers with a predicted income score that makes them eligible for *PANES* (namely, those with negative predicted scores, as the threshold is normalized to zero). The estimate in row 4 that uses all available observations is similar to the baseline estimate, at -0.013, and significant at 95% confidence. The estimate in row 5 that restricts the analysis to observations in the narrow range near the eligibility threshold, from -0.1 to +0.1, is virtually unchanged (-0.014) and significant at 90% confidence; standard errors increase here with the substantially smaller restricted sample, which falls by more than half. We further estimate the model with a linear polynomial control in the predicted income score (in row 6), and the point estimate remains similar (-0.014) although it is no longer statistically significant. When higher

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[19] Appendix Figure A3 Panel A shows that the distribution of the predicted income score is smooth around the program eligibility threshold, suggesting an absence of score manipulation (McCrary 2008). Panel B shows that the distribution of birthweights is smooth around the low birthweight cut-off (2,500g), further indication of data quality.
[20] Specifically, cconsistent with the difference-in-differences estimates, for eligible mothers we define treated pregnancies as those that started no more than six months before the receipt of first program payment. For ineligible mothers (for whom there is no comparable date of first payment) we consider pregnancies that started no earlier than five months before the baseline visit in the sample; we choose five months since the median difference between the date of first payment and the date of the baseline survey is one month, thus preserving the six month "lead time" we use throughout in defining program treatment. Estimates are nearly identical when using the timing of the baseline survey to define treated births among eligible households although somewhat less precise (not shown), presumably in part due to the mis-measurement of treatment this approach generates.

order polynomials are included, point estimates are negative but not statistically significant at conventional levels (now shown). While not our main specification, due to the greatly reduced sample size near the eligibility threshold, the regression discontinuity analysis confirms the main findings using the difference-in-differences and mother fixed effect estimators.

6. Mechanisms underlying the reduction in low birthweight

The richness of the dataset allows us to investigate many behavioral channels that could potentially be behind the findings in the previous section, including those related to improvements in the quantity or quality of child care, changes in income and labor supply, family structure and residential mobility, risky behavior, fertility choices, and selective child survival. This section also investigates whether the in-kind transfer component of the program or the possibly even mis-perceived conditionalities drive our results. Estimates are reported in Tables 3, 4 and 5, where, unless otherwise specified, we use to the main specification (from Table 2, row 1 and column 2).

6.1 Prenatal care and mother's health

Evidence from the medical literature suggests that the week of first 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, as discussed above. This raises the question of whether and to what extent program effects are driven by improvements in the quantity and quality of prenatal care among program beneficiaries. At first glance, such improvements seem unlikely. The *PANES* program was de facto unconditional, and the survey evidence indicates that the vast majority of beneficiary households were unaware of any formal prenatal check conditionalities 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.

Table 4, rows to 1 through 5, report the effect of *PANES* on the number of prenatal visits overall and by trimester, as well as the week of first visit. We estimate an increase in the total number of prenatal visits, including in the third trimester, but this increase is very small at about 2 percent of the baseline average. Consistent with this finding, estimated program impacts change little (from -0.017 to -0.014, in row 7 or Table 3) when we explicitly control for the number of prenatal visits in each trimester as well as the week of first visit.

There is also no evidence in Table 4 that the quality of prenatal care, as proxied by the number of births assisted by a doctor (row 6), the fraction of births in public hospitals, which are generally of lower quality (row 7), the average (pre-*PANES*) birthweight at the mother's health center (row 8) or the fraction of births paid for by private health insurance (row 9), is affected by program participation.[21]

Despite very modest changes in prenatal or care, a number of maternal health and nutrition measures (recorded in SIP) do improve significantly during pregnancy. Most importantly, mother's weight is higher among eligible mothers and it improves significantly during pregnancy. We find that in the first visit treated mothers are 0.5 kg (approximately one pound) heavier than untreated mothers (row 14), although this effect is not significant at conventional levels. By the final prenatal visit, the average weight gain for treated mothers is 0.95 kg (approximately two pounds, row 15). These findings are consistent with the hypothesis

[21] Recall that *PANES* did not involve the creation of new health centers or any other health supply intervention.

that the higher household income generated by the *PANES* transfer led to improved maternal nutrition, and thus greater child growth *in utero*.[22]

A second notable health outcome in the SIP dataset is the sharp reduction in maternal smoking during the first trimester, with a drop of 3.2 percentage points (row 16, significant at 95% confidence), on a base of roughly 31% in the pre-program period.[23] Smoking is strongly linked to low birthweight in the medical literature (Kramer 1987). While any explanation for this drop in smoking is necessarily speculative, a possible factor is falling stress in *PANES* beneficiary households as a result of their improved economic circumstances.[24]

In sum, we find no evidence of improvements in the quality of prenatal care, and evidence of only a very modest increase in quantity, and this small increase is unable to account for the large program impacts we find. We also find evidence of improvements in mother's nutrition and a reduction in smoking, a key risky health behavior.

6.2 The role of in-kind transfers

An important policy question pertains to the role of in-kind transfers. Although Hoynes and Schanzenbach (2009) show that Food Stamps act essentially as cash transfers in the U.S., one cannot presume that the same holds in *PANES*, especially given how poor its beneficiaries were.

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[22] As expected, there is no "impact" on treated mothers' height (point estimate 0.43 cm, s.e. 0.34). This serves as a useful specification check.

[23] It is reassuring that, when we run a reduced form regressions of the probability of smoking in the first trimester (dependent variable) on indicators for program exposure at different stages of pregnancy (as in row 3 of Table 2) we find that this probability is only significantly affected by program exposure before conception: regression coefficients for exposure in the third, second, and first trimesters and the trimester before conception are, respectively, -0.002 (s.e. 0.021), -0.014 (0.021), -0.020 (0.022), -0.045** (0.018). Once again, there is no evidence of pre-existing positive trends in outcomes among those eventually eligible for the program. Note that we do not have data on maternal smoking in later trimesters, and thus cannot directly assess the impact of smoking at different stages of pregnancy. Smoking in the first trimester is our best proxy for smoking at later stages of pregnancy, including the third trimester, which we showed above is the critical period for child low birthweight outcomes.

[24] Banerjee and Mullainathan (2010) discuss why "temptation goods" like cigarettes might constitute a particularly large share of consumption among the poor.

Table 3 reports results from regressions that exploit the fact that the food card was only rolled out starting in the second half of 2006 (appendix Figure A2) and that the date of entry into this program component did not coincide with the first payment of the cash transfer. Row 9 restricts the sample to births that started before June 2006, i.e., pregnancies concluded before any food cards were distributed. The effect of program participation is nearly identical to the baseline specification in Table 2, row 1, column 2 and statistically significant at 99% confidence level, indicating that the cash transfer alone reduced low birthweight incidence. This implies that in-kind transfers are not necessary for the effects that we document.

We also present regressions that include the monetary values of both the cash and in-kind transfers as regressors, again using a 2SLS strategy where we instrument the value of each program component with an indicator for exposure to each of them during the last trimester of pregnancy among the *PANES* eligible only. As selection into the food card component among *PANES* eligible households is non-random, we focus on the mother fixed effects specification. Regressions results in row 10 of Table 3 show that each additional UY$1,000 in cash reduces the incidence of low birthweight by -0.013, while the point estimate for the impact of an additional UY$1,000 in food cards is roughly twice as large but also imprecisely estimated (-0.027, s.e. 0.066 – not shown), and thus we cannot reject that the effects of cash and food cards (of the same monetary value) are the same. If we restrict the effect of UY$1,000 in cash and in-kind to be equal, the point estimate is -0.016 (significant at 95% confidence, now shown).

Overall, there is no evidence that the low birthweight results are driven mainly by the in-kind (food card) component of the program, and we cannot reject the hypothesis that the effect of one extra dollar in cash transfers is the same as the effect of a dollar in food cards, implying that the food card was in practice fungible for money. This is precisely what one would predict from economic theory, given that the value of the food card is a small share of total household income and thus far less than most beneficiary households' total food expenditures. We are able to test this prediction more directly than existing studies, given the variation in both cash transfers and food cards in the program we study.

6.3 Income and labor supply

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, by allowing pregnant women to withdraw from the labor market, on the other hand, the drop in work hours might reduce physical and psychological stress, improving birth outcomes.

Table 5 estimates program effects on household and mother socioeconomic measures during pregnancy. The dependent variable in row 1 is the average value of the *PANES* cash transfer over the first six months following conception. The estimated coefficient is UY$1,040, or approximately US$78 at the 2005 PPP exchange rate.[25] This number is also roughly 30 percent lower than the program transfer (UY$1,360), consistent with the imperfect compliance with program assignment documented above.

Row 2 reports the effect of eligibility for the food card. Eligibility is associated with in-kind food card transfers during pregnancy of UY$191 per month, or US$14 (in PPP terms). 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 fact that the food card component was

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[25] This explains why the effect of being a program beneficiary (in row 1 of Table 2) is approximately equal to the effect of receiving extra UY$1,000 (in row 10 of Table 3).

rolled-out mid-program (appendix Figure A2, panel D). Row 3 shows there was no significant change in the total amount of other government benefits received by eligible households.

Rows 4 and 5 specifically examine mother earnings and household earnings, respectively. There is evidence that *PANES* mothers and their households earned somewhat less in the formal labor market than their non-eligible counterparts, but the differences are relatively small (at UY$40 and 175 for mothers and households, respectively, or approximately US$3 and US$13 in PPP). Consistent with this finding, row 6 shows there was a moderate fall of 1.3 percentage points in mother's labor supply during pregnancy, as reported in the birth records. Despite these negative labor supply effects, program eligible households experience a large increase in total household income, by UY$968 per month (row 7), where this figure includes all sources of formal sector income and transfers as recorded in the Social Security database during the first six months after conception. As a conservative counterfactual, using the fact that total household income between July 2006 and December 2007 (when the program had completely rolled out) among unsuccessful applicants was roughly UY$3,650 (or just over US$270 per month in PPP terms), this implies that the program increased total household income by at least 25 percent. Recall that pre-program total household income among *PANES* eligible households was UY$1,113 per month, but this is a questionable "base" figure for a counterfactual given mean reversion, and the recovering Uruguayan economy during this period.

Thus it appears that labor supply responses to the *PANES* program were modest in magnitude and far too small to offset the direct transfer amount. However, the reduction in mother's work might have affected birth outcomes through a reduction in stress.

6.4 Gestational length

One reason why the program might have affected birthweight is via an increase in gestational length. This increase mechanically leads to a drop in low birthweight. The point estimate of program exposure on gestational length in row 10 of Table 4 is 0.08 weeks, or roughly half a day This increase is very modest (at just 0.2 percent on average gestational length of 38.5 weeks) and there are no impacts on premature births (row 11).

To measure the extent at which greater gestational length can account for the program's reduction in low birthweight incidence, in row 8 of Table 3 we re-estimate equation 1 restricting the sample to non-premature births (defined as 38 or more weeks): the estimated impact on low birthweight of -0.009 is significant at 99% confidence, though it is smaller than the main estimates, suggesting that increased gestational length may be playing some role. We also report the effect of the program on the probability of being in the bottom decile of the birthweight distribution conditional on gestational length (in row 12 of Table 4): there is a sharp reduction in being in the bottom decile, with a drop of 1.7 percentage points, suggesting that the program also boosted growth per unit of time *in utero*.

6.5 Fertility choices and selective fetal survival

A further possibility that we explore at length is that the program affected fertility or fetal survival among mothers at risk of having low birthweight children, and via this, possibly affected observed birth outcomes. Cash transfers could theoretically lead to either higher or lower 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.[26] The existing empirical evidence is mixed (Moffitt 1998).

As an initial piece of evidence, row 11 of Table 3 presents estimated program impacts on low birthweight among the subsample of pregnancies that started before the first *PANES* payment was made. This sample restriction should largely control for endogenous fertility responses since these pregnancies reflect choices made before exposure to the program (unless individuals could accurately predict the exact timing of their incorporation into the program, which seems highly unlikely given the uncertainties surrounding this new program, which was the first of its kind in Uruguay). Results are similar (at -0.019) to the baseline estimates and significant at 95% confidence, suggesting that bias due to endogenous fertility is unlikely to explain the main results.

We also investigate fertility patterns directly. As a first check Panel C of Figure 3 reports the share of births by mother's level of education between 1996 and the first semester of 2008. Again, there is no apparent change induced by the program.

We also perform a more formal analysis using the universe of *PANES* applicant women of child-bearing age (12 to 50 years old) between 2003 and 2008 (recall that we have no information on mother's *cédula* in the natality files prior to 2003). We create a quarterly panel that spans the period January 2003 to December 2008. For each trimester, we construct an indicator variable equal to one if the woman gave birth and zero otherwise. Overall, we have information on approximately 157,000 women over 24 quarters, with more than 3 million individual-quarter observations, who have an average monthly fertility rate of 0.73 percent

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[26] 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 2006). However, *PANES* consisted of a fixed transfer per household.

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.[27] 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 be erroneously considered a causal program effect. This is simply regression to the mean, which arises in many other contexts, for example, in analyses of the effect of job training on wages (Ashenfelter 1978, Card and Sullivan 1988). Consistent with this mechanism, the difference in fertility rates between *PANES* eligible and ineligible applicants (Figure 5) displays a sharp drop precisely at the time of the baseline survey used to determine program eligibility.[28]

A simple way to correct for any bias induced by this compositional effect is to re-weight observations for *PANES* ineligible mothers so that the distribution of pre-program characteristics is the same as for eligible mothers. We use past fertility (births between January 2003 and the baseline survey) and mothers' observable characteristics in the reweighting.[29] Differences in

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[27] This is confirmed by Panel A in appendix Figure A5, which shows a clear discontinuity in the probability of household *PANES* treatment as a function of the child's date of birth.

[28] A similar result is obtained if we plot fertility rates as a function of time to and since the first payment for *PANES* eligible households only, similar to Figure 1. This series is plotted in appendix Figure A5, panel B. Panel C reports fertility rates as a function of time to and since baseline (as in Figure 5), plotted separately by eligibility status. Unsurprisingly, among eligible households the data show a similar pattern to that in Panel B, while, among unsuccessful applicant households, there is no clear time pattern. That the compositional differences between eligible and ineligible households is driving the results in Figure 5 is confirmed in panels D to F, which successively plot fertility rates for eligible and ineligible applicants separately by the number of children born between January 2003 and the administration of the baseline survey (namely, 0, 1, or 2 or more children). Conditional on households' past fertility history, fertility patterns during the program appear nearly identical for the eligible and ineligible.

[29] In practice, we define cells based on mother's age (in years), mother's years of education, month of baseline survey, and number of pregnancies at different lags (1-3 months, 4-6 months etc.) from the baseline survey.

fertility rates between eligible and ineligible women after reweighting are displayed in Panel B of Figure 5, where by construction, pre-program differences are zero. Visually, there appears to be a slight increase in fertility among *PANES*-eligible women during the program.

Estimation results are reported in Table 5, rows 11 and 12, where a fertility indicator is the dependent variable (in the 2SLS specification). Consistent with the graphical evidence in Figure 5, the point estimate implies a very slight but significant rise in fertility of 0.0013 (row 11), and with pre-program average monthly fertility among eligible mothers of around 0.008, this is equivalent to roughly a 16 percent rise in fertility. As a bound, for the extreme and unrealistic case where this rise in fertility was entirely driven by individuals with exactly zero probability of giving birth to low weight children, this change in fertility could account for the roughly 16 percent drop in the incidence of low birthweight documented above. However, under more realistic but still conservative assumptions, the increase in fertility would explain only a small fraction of the overall effect. For instance, if the proportion of low birthweight children among the additional births was as low as among the much more affluent and educated *PANES* non-applicant households (8.2 percent, Table 1), then the increase in fertility would account for a drop of only 0.3 percentage points in the incidence of low birthweight, which is less than one fifth of the low birthweight reduction we estimate (namely, 1.5 to 1.7 percentage points).[30]

Because any change in fertility induced by the program would take at least nine months to materialize, an arguably cleaner test is provided by restricting attention to the first year of the program. Row 12 of Table 5 restricts the analysis to births that took place within one year of the baseline survey, and yields an effect in fertility of just 0.0002, implying a rise of around 3

[30] To arrive at this figure, we assume that the incidence of low birthweight among the "inframarginal" births in the *PANES* eligible group would be unchanged, at 10.2 percent (Table 1), while the low birthweight proportion among the additional 16 percent of births would be 8.2 percent (Table 1). The incidence of low birthweight among eligible households under these assumptions would fall to 9.9 percent

percent, though this small effect is not significant at conventional levels. Yet the estimated program impact on low birthweight incidence during exactly the same time period (row 12 of Table 3) is again virtually identical to the baseline estimate, at -0.012, and statistically significant, providing another piece of evidence that endogenous fertility changes do not account for the main birthweight result.

6.6 Family structure and residential mobility

As a final channel we investigate whether welfare transfers affected the incentives for marriage, either because of the conditionalities associated with program receipt (i.e., as the income conditionality includes the husband's income) or the additional financial resources they make available to women. Although the existing evidence from the U.S. points to zero or only modest negative effects of welfare transfers on single motherhood and marital stability (Bitler, Gelbach, Hoynes and Zavodny 2004), again this need not extend to Uruguayan households.

Rows 9 and 10 of Table 5 show that *PANES* led to a large and statistically significant reduction in the proportion of children born to unmarried parents, as well as in the proportion without a father named on the birth certificate. Contrary to what is often presumed, welfare transfers lead to a moderate increase in marital stability, with a drop of 2.1 percentage points (on a base of 80%) in out-of-wedlock births. To the extent that intact marriages and the presence of the father have positive impacts on mother and child, this could partly explain the program effects on low birthweight. The increase in marriage rates and identified fathers could be due to multiple factors, including the possibility that income transfers led to greater relationship stability (perhaps by reducing stress), that it made the mother a more attractive partner (in those cases where she was the household's program recipient), or that the improvement in economic circumstances simply allowed the couple to feel secure enough to get married. In any case, there is no evidence of the hypothesized adverse behavioral responses with respect to marriage.

We also investigate whether residential mobility increased due to the transfer, and specifically whether it led households to move to neighborhoods with better average health outcomes. There is no indication (row 8 of Table 5) of an improvement in the average health in households' residential area (as proxied by the pre-*PANES* average birthweight in the area) among program households.

6.7 Treatment effect heterogeneity

We next explore some obvious dimensions of maternal heterogeneity to determine if the effects of cash transfers are more pronounced among particular subgroups. We first find that the reduction in low birthweight is particularly large among the roughly one fifth of our sample who are teen mothers, with a drop of 2.9 percentage points (relative to 1.6 percentage points among non-teen mothers, Table 3, rows 13 and 14) and among married mothers, with a drop of 2.8 percentage points (versus 1.3 points for unmarried mothers, rows 15 and 16). However, these differences are not significant at conventional levels (p-values of 0.37 and 0.19, respectively).

Finally, since the magnitude of the income transfer was fixed regardless of household size, households with fewer members effectively received a larger per capita transfer than larger households. We divide the sample of mothers into those in smaller households (three or fewer members) and larger ones (at least four members) to test if this led to larger gains in smaller households but find no evidence that it did, with identical estimated reductions of 1.8 percentage points in both groups (rows 17 and 18). There are many possible explanations, including the fact that there is limited variation in household size in the sample overall (given the very low fertility rates in Uruguay, which are among the lowest in Latin America even among the poor) and that the effect of a similar cash transfer might vary across households of different sizes for reasons other than the amount of the per-capita transfer. Imperfect income pooling among adults in the household would also serve to dampen the effect of household size on the magnitude of the program treatment effect (although our survey data does not allow us to directly test this).

6.8 Estimating a lower bound on the internal rate of return

A standard way to assess the attractiveness of an investment is to compute its internal rate of return (IRR). We carried out a simple exercise that yields a bound on the IRR for the *PANES* program; for reasons of space, we discuss the details in appendix C. Under a set of conservative assumptions detailed in the appendix, a lower bound on the IRR (in real terms) is positive and moderate at 3.2 percent per annum, which is somewhat higher than current real interest rates in Uruguay. This exercise suggests that the child investments made through *PANES* may indeed be an attractive public policy in Uruguay. Needless to say, this remains a highly speculative calculation in the absence of actual earnings data for our sample, who are currently still children.

7. Summary and conclusions

This paper studies the impact of a social assistance program on the incidence of low birthweight in Uruguay, a middle income Latin American country. The program consisted principally 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, similar to rates found among poor populations in the U.S.

Using a unique matched micro-dataset with vital statistics, program, hospital and social security records, our estimates imply a fall in the incidence of low birthweight on the order of 15 to 17 percent, allowing beneficiaries to entirely close the baseline gap in low birthweight incidence with the ineligible program applicants. A back-of the envelope internal rate of return calculation suggests that it could be an attractive public policy option.

Our data allow us to explore a large number of mechanisms associated with the estimated effects. 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, likely leading to improved nutrition. Consistent with this, we show that beneficiary mothers' weight during pregnancy is positively affected by the program. We also find a reduction in smoking, which we speculatively attribute to reduced stress (itself a potential result of reduced work hours during pregnancy), and a reduction in out-of-wedlock births among program beneficiaries.

Similarly, our data allow us to rule out a number of potential alternative explanations for the estimated effects. The behavioral effects that could in theory offset the benefits of such transfers (e.g., reduced income, increased drinking or smoking, or family break-up) appear non-existent or at best of second-order importance in our setting. Crucially, we show that neither an increase in prenatal visits – whether induced by a misperception about program conditionalities, or by increased household resources – nor the in-kind food card component can account for the reduction in low birthweight documented in this paper. We cannot reject that the effect on birthweight of one extra dollar in cash is the same as the effect of one extra dollar in food cards. Equally important, positive fertility effects among welfare eligible women appear of second order importance. Additionally, we show that there are no program impacts on either residential migration or take-up of other government programs, implying that program effects cannot be attributed to these alternative channels. Taken together, the evidence points unequivocally to the large cash transfer component itself as the key driver behind improved birth outcomes.

One important implication of our analysis, and one that is consistent with recent U.S. findings, is that exposure to income transfers in the third trimester of pregnancy crucially affects birthweight. We are able to arrive at this conclusion precisely because the program we study was not targeted to pregnant women alone, meaning that in our data we have women incorporated into the program before conception as well as at different stages of pregnancy. If the policymaker's objective is to improve birth outcomes, this finding provides a rationale for interventions that are specifically targeted to pregnant women, as long as pregnancies can be detected before the end of the second trimester.

The results of this paper imply that cash social assistance may help to break the cycle of intergenerational poverty by improving child health. Although we are unable to say anything conclusive about how outcomes would have differed in the presence of strictly enforced prenatal visit conditionalities for pregnant women, we are able to show that birth outcomes improved substantially even in the absence of such conditions, an important result given the current debate in development on the role of conditionalities in the design of social programs (for instance, as discussed in Baird, McIntosh and Özler 2011). The findings in this paper also appear relevant not just for Latin American countries but also potentially for wealthier societies given Uruguay's generally good infant health measures (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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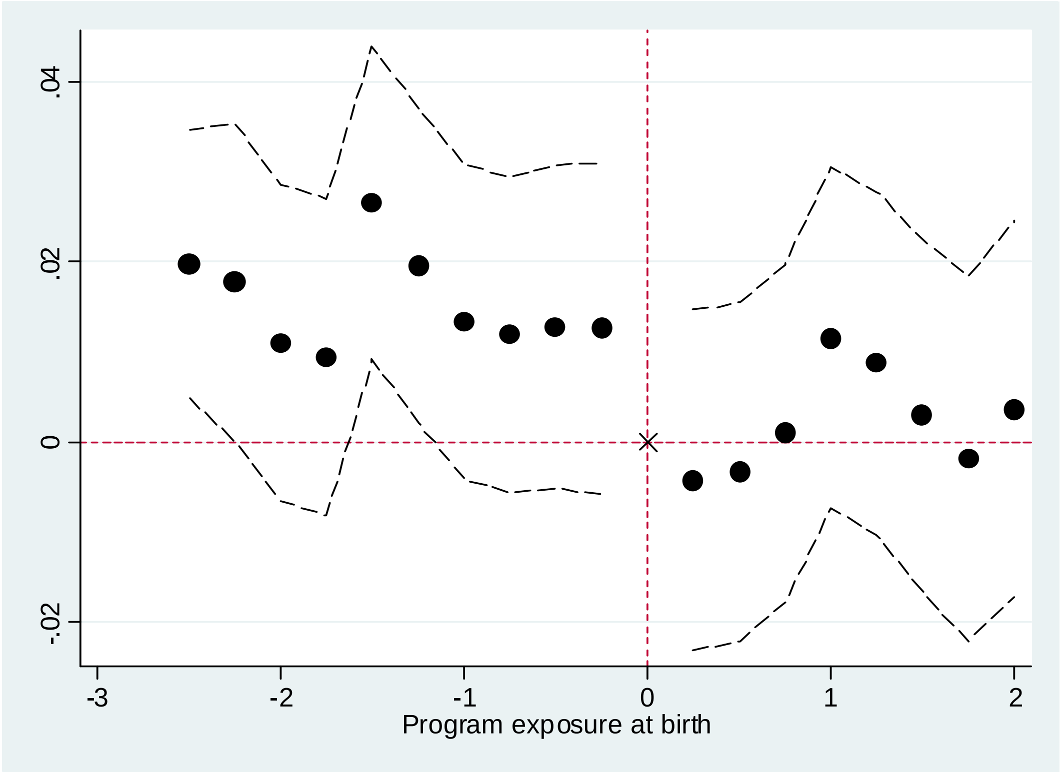
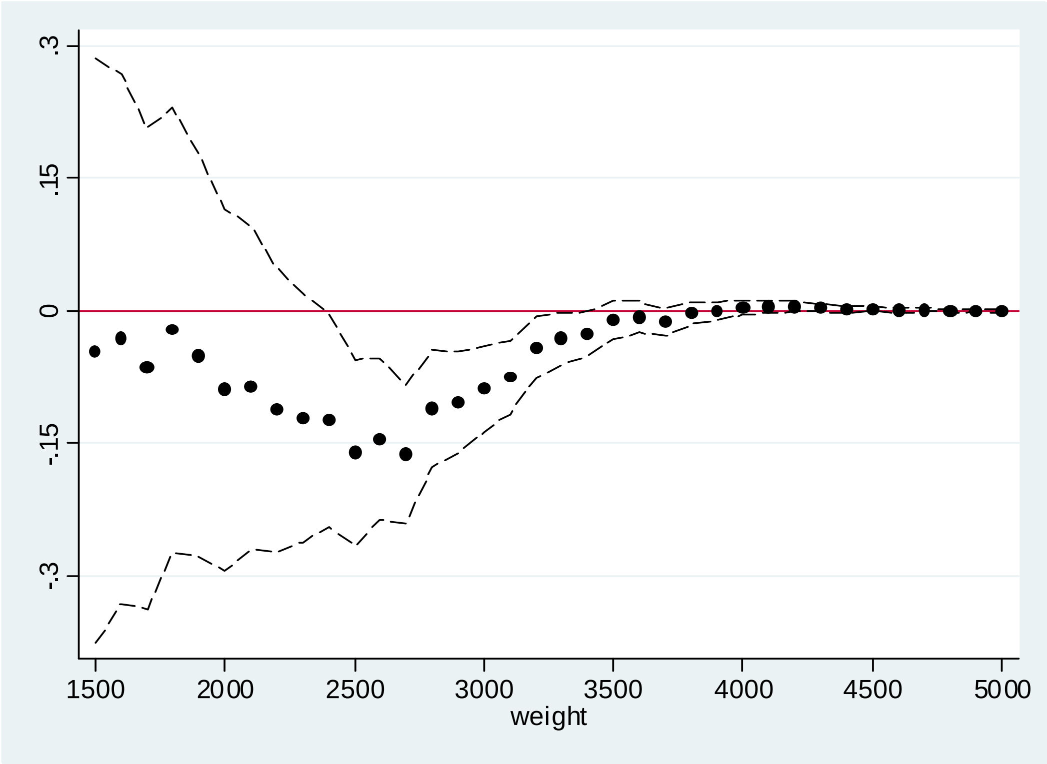
Figure 1: Fraction of low birthweight and treated births as a function of time to/since first income transfer



Notes. The figure reports the fraction of low weight births as a function of the difference between the time of birth and the time of first payment of the cash transfer (for *PANES* eligible mothers only). Data are expressed as differences with respect to positive program exposure *in utero* of less than a full trimester (denoted by a vertical line). Dotted lines denote 95 percent confidence intervals.

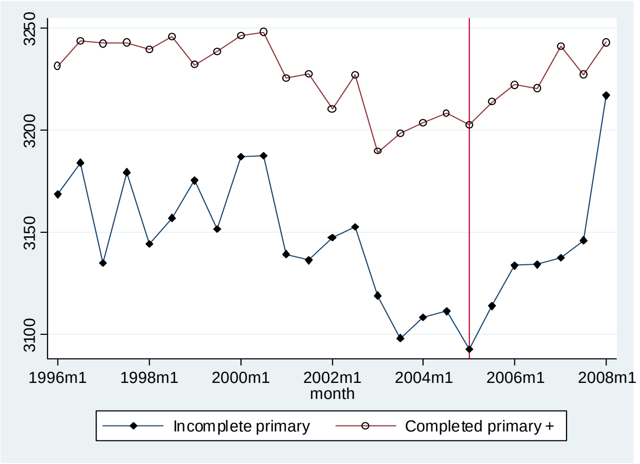
Figure 2: Estimated proportional program effects by birthweight



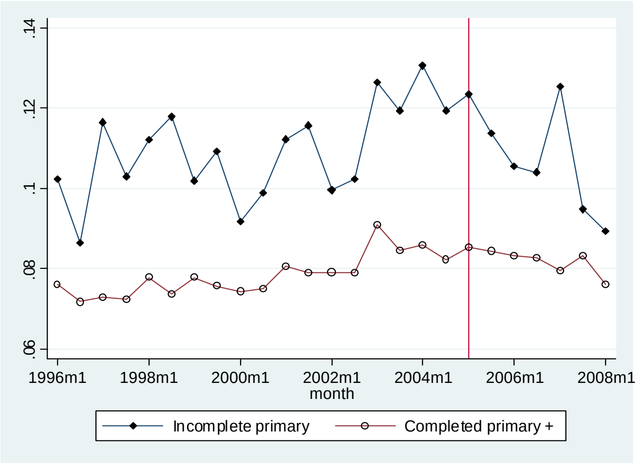
Notes. The figure reports the estimated percentage change in the probability of being below each level of birthweight as a result of treatment. Each point comes from a separate 2SLS regression including controls (as in column 2 of Table 2). 95 percent confidence intervals around the estimates also reported.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

Figure 3: Trends in birthweight and composition of births by mother’s education

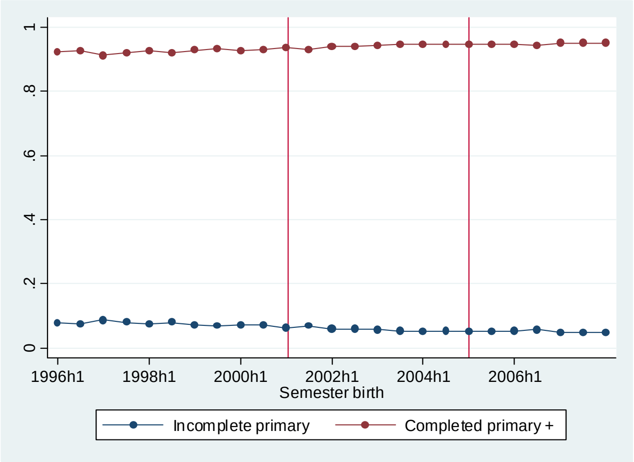
A: Birthweight



B: Fraction low birthweight



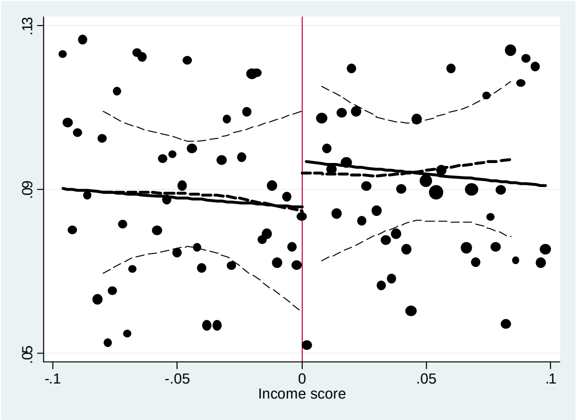
C: Fraction of births



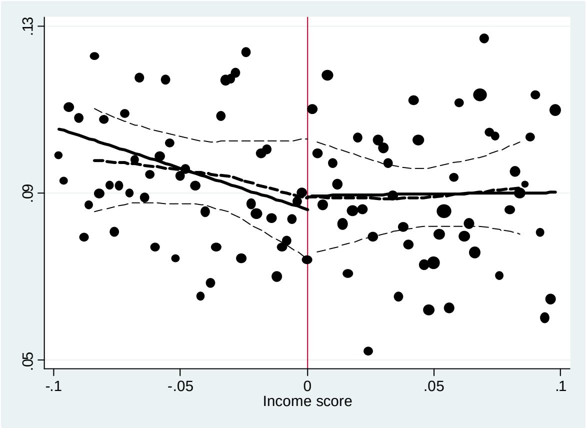
Notes. Panels A and B report average birthweight and the fraction of low birthweight children by semester between the first semester for 1996 and the first semester of 2008 separately by mother’s education. Panel C reports the fraction of children born to mothers with different levels of education.

Figure 4: Proportion of low birthweight and treated births as a function of the predicted income score

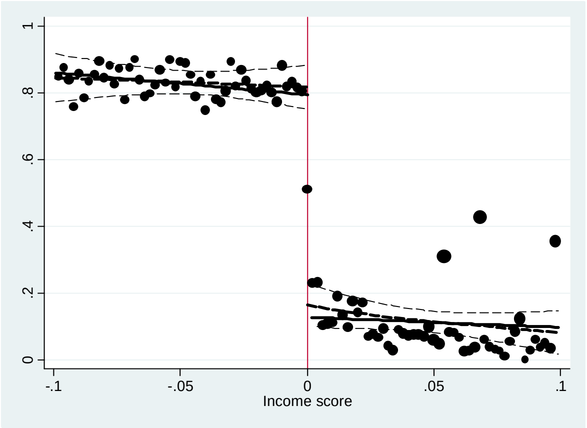
A: Low birthweight, program period



B: Low birthweight, pre-program period



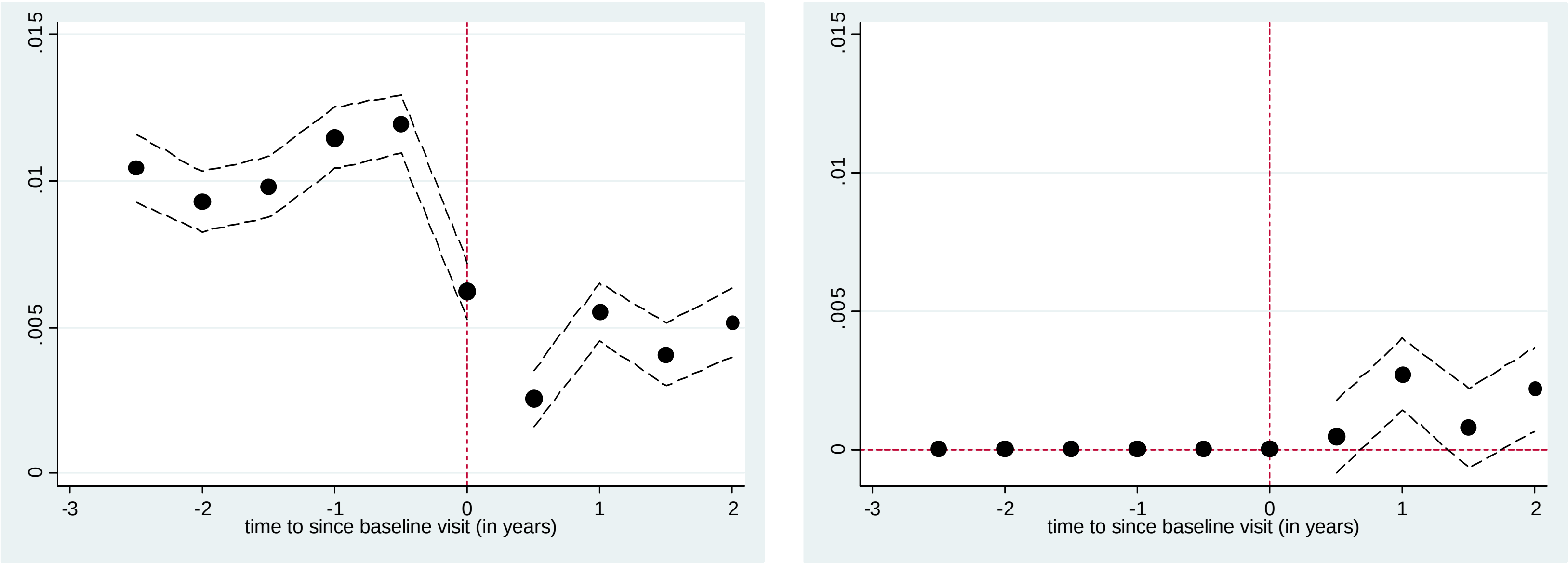
C: Treated



Notes. Panel A reports the proportion of low weight births among *PANES* applicant mothers as a function of the normalized income score during the program period (see text for details). A vertical line corresponds to *PANES* the eligibility threshold at a normalized predicted income score of zero. The figure also reports two estimated curves on either side of the threshold: a linear polynomial in the income score (solid line) and local linear regression (dashed line) computed using the Stata command "rd" with optimal bandwidth (Nichols 2011), as well as the 95% confidence intervals around the estimated linear prediction (thin dotted lines). Panels B and C report similar graphs where the variables on the vertical axis are, respectively, the proportion of low weight births in the pre-program period, and the fraction on mothers in receipt of the income transfer during the first two trimesters of pregnancy (during the program period). The size of the points in these figures is proportional to the number of observations in that cell.

Figure 5: Fertility rates as a function of time to/since the baseline visit, difference between *PANES* eligible and ineligible women

A: Actual fertility difference B: Adjusted fertility difference



Notes. Panel A reports the difference in fertility rates between eligible and ineligible *PANES* applicant women of child bearing age as a function of the time to and since the baseline survey. Panel B reports the same difference reweighted by the fraction of eligible mothers in each cell defined by mother's fertility history, age and education measured in the baseline survey. See text for details. Dotted lines are the 95% confidence interval.

Table 1: Descriptive statistics, all births in Uruguay (2003-2007)

	PANES Applicants		Non-applicants
	Eligible	Ineligible	
	(1)	(2)	(3)
Pre-program period (January 2003 – March 2005)			
Panel A: Birth outcomes			
1. Low birthweight	0.102	0.093	0.084
2. Birthweight (g)	3141.05	3161.35	3217.92
3. Gestational length	38.50	38.50	38.56
Panel B: Prenatal and natal care			
4. Total number of prenatal visits	6.53	7.53	8.28
5. Week of first prenatal visit	17.50	16.24	14.16
6. Number of visits, first trimester	0.31	0.40	0.63
7. Number of visits, second trimester	1.61	1.92	2.19
8. Number of visits, third trimester	4.61	5.22	5.46
Panel C: Socio-economic indicators			
9. Average birthweight area of residence	3193.53	3196.20	3200.88
10. Average birthweight health center	3170.43	3185.76	3207.59
11. Public health center delivery	0.77	0.55	0.33
12. Birth delivery paid by private health insurance	0.06	0.14	0.43
13. Mother incomplete primary education	0.12	0.05	0.04
14. Number of previous pregnancies	2.37	1.44	1.26
15. Out-of-wedlock birth	0.80	0.72	0.52
16. Missing child father information	0.61	0.51	0.31
17. Father incomplete primary education	0.10	0.04	0.02
18. Mother works	0.12	0.18	0.43
19. Mother earnings during pregnancy $^{\pm}$	107.29	283.68	-
20. Household earnings during pregnancy $^{\pm}$	591.85	1669.11	-
21. Household non-*PANES* benefits during pregnancy $^{\pm}$	521.65	807.75	-
22. Household total income during pregnancy $^{\pm}$	1113.50	2476.86	-
23. Mother age	25.43	24.78	27.50
24. Father age	30.77	29.62	31.93
25. Birth assisted by doctor	0.49	0.55	0.71
26. Mother smoker in first trimester$^{\wedge}$	0.31	0.25	0.16
27. Mother weight (kg), first prenatal visit$^{\wedge}$	56.36	60.18	61.81
28. Mother weight (kg), final prenatal visit$^{\wedge}$	63.26	68.48	71.20
Program period (April 2005 – December 2007)			
29. Low birthweight	0.091	0.091	0.082
30. Ever received *PANES* income transfer	0.97	0.11	-
31. *PANES* income transfer during pregnancy (0/1)	0.55	0.06	-
32. Amount of *PANES* income transfer during pregnancy	607.52	69.21	-
33. Ever received *PANES* food card	0.80	0.12	-
34. *PANES* food card during pregnancy (0/1)	0.33	0.04	-
35. Amount of *PANES* food card during pregnancy	134.45	12.06	-
36. Mother earnings during pregnancy $^{\pm}$	132.81	340.96	-
37. Household earnings during pregnancy $^{\pm}$	808.28	1979.12	-
38. Household non-*PANES* benefits during pregnancy $^{\pm}$	683.46	874.04	-
39. Household total income during pregnancy $^{\pm}$	2233.80	2934.43	-
Observations, vital statistics sample	50,939	20,872	163,370
Observations, SIP sample	15,093	6,905	57,477

Notes. *PANES* eligible (column 1), ineligible (column 2), and the rest of the population (non-*PANES* applicants, column 3). Pre-program period (Panels A, B and C) refers to births occurring between January 2003 and March 2005, while program period (panel D) refers to births occurring between April 2005 and December 2007. $^{\pm}$: data available only since March 2004. The earnings and transfers variables are in monthly terms. $^{\wedge}$: data available only for SIP sample. 1 UYS = US$ 0.075 at 2005 PPP adjusted exchange rate.

Table 2: *PANES* program effects on birthweight, 2SLS estimates

Dependent variable:	(1)	(2)
Panel A: 2SLS		
1. Low birthweight	-0.015***	-0.017***
	(0.005)	(0.005)
2. Birthweight	23.774**	29.721***
	(10.440)	(9.871)
Panel B: Reduced form and first stage results		
3. Low birthweight, by length of exposure – reduced form		
Entered 3rd trimester	-0.011	-0.007
	(0.008)	(0.007)
Entered 2nd trimester	-0.022***	-0.020**
	(0.007)	(0.007)
Entered 1st trimester	-0.026***	-0.024***
	(0.007)	(0.007)
Entered before conception	-0.008	-0.010**
	(0.005)	(0.005)
4. Low birthweight – reduced form	-0.013***	-0.014***
	(0.005)	(0.004)
5. Income transfer during pregnancy (0/1) – first stage	0.864***	0.867***
	(0.005)	(0.005)
Controls	No	Yes

Notes. Number of observations: 68,858. Each cell refers to a separate regression. Entries in the panel A are estimated coefficients from a 2SLS regression of each dependent variable on an indicator for receipt of the income transfer during the first two trimesters since conception, where receipt of the transfer is instrumented by an indicator for mother eligibility for *PANES* during pregnancy (see text for details). Row 3 reports coefficients from an OLS regression of low birthweight (as in row 8) on indicators for *in utero* exposure of less than one full trimester, one full trimester, two full trimesters, three full trimesters, and before conception. Rows 4 and 5 report the reduced form and first stage estimates, respectively. All regressions control for month of conception indicators, indicators for month of entry into the program (which takes on a value of zero for ineligible individuals) and an indicator for the *PANES* income score being below the eligibility threshold. Controls in column (2) include indicators for mother's age and education, sex of the child, an indicator for multiple pregnancies (e.g., twins), for the number of previous pregnancies, month of the baseline survey visit, and month of program enrollment. Standard errors clustered by mother. ***, **, *: significant at 99, 95 and 90% level.

Table 3: *PANES* program effects on birthweight, additional specifications

	Coefficient estimate (s.e.)	Obs.
1. Fixed time of first payment (independent of actual entry date)	-0.020** (0.008)	68,858
2. Mother fixed effects (FE) estimates	-0.018** (0.009)	24,346
3. Eligible mothers only	-0.016** (0.003)	48,891
4. RD estimates - All observations	-0.013** (0.006)	20,675
5. RD estimates - Predicted income score range (-0.1, 0.1)	-0.014* (0.008)	9,529
6. RD estimates - Predicted income score range (-0.1, 0.1), linear polynomial	-0.014 (0.018)	9,529
7. With controls for number of visits by trimester and week of first visit	-0.014*** (0.005)	63,512
8. Among non-premature births	-0.009** (0.003)	55,621
9. Pre-food card period only	-0.017** (0.008)	50,953
10. With controls for food card roll-out (UY$ 1,000 cash transfer) –mother FE	-0.013 (0.019)	24,346
11. Among pregnancies initiated before first payment	-0.019** (0.008)	37,054
12. Among births within one year from baseline	-0.012* (0.007)	54,250
13. Teen mothers	-0.029** (0.013)	13,986
14. Non-teen mothers	-0.016** (0.005)	54,872
15. Married mother	-0.028** (0.012)	12,231
16. Single mother	-0.013** (0.006)	56,627
17. Smaller households (three or fewer household members, avg: 2.7)	-0.018* (0.010)	19,593
18. Larger households (at least four household members, avg: 5.8)	-0.018*** (0.006)	49,265

Notes: 2SLS estimates of the effect of *PANES* participation on low birthweight, in a specification equivalent to Table 2, column (2). Row 6 defines as eligible all pregnancies starting after January 2005 to mothers with an income score below the eligibility threshold. Row 2 includes mother fixed effects. Row 3 restricts to program eligible mothers. Rows 4 to 6 present regression discontinuity estimates on program period data only. Row 4 uses a broad neighborhood around the eligibility threshold, while rows 5 and 6 restrict to predicted income scores in the range -0.1 to +0.1. Row 6 additionally includes a linear polynomial in the income score interacted with an eligibility indicator. Row 7 includes dummies for number of controls in teach trimester and for the week of first visit. Row 8 restricts to non-premature births (38 weeks or more). Row 9 restricts to children conceived before November 2005, who were either in the 3rd trimester or already born when the food card was introduced. Row 10 reports the effect of an extra UY$1,000 in cash transfers and includes as an additional regressor the value of the food card during the first two trimesters of pregnancy. Both variables are instrumented by indicators of eligibility. The specification includes mother fixed effects. Row 11 restricts to pregnancies that started before the date of first program payment. Row 12 restricts to pregnancies concluded within one year from baseline. Rows 13 through 18 report results by subgroups (using baseline marital status and household size). See also notes to Table 2.

Table 4: *PANES* program effects on prenatal care and mother's health

Dependent variable:	Coefficient estimate (s.e.)	Obs.
1. Total number of prenatal visits	0.144**	67,863
	(0.059)	
2. Number of prenatal visits, first trimester	-0.025*	67,883
	(0.013)	
3. Number of prenatal visits, second trimester	0.049	67,877
	(0.024)	
4. Number of prenatal visits, third trimester	0.132***	67,875
	(0.045)	
5. Week of first prenatal visit	-0.061	63,721
	(0.134)	
6. Birth assisted by medical personnel	-0.002	68,858
	(0.009)	
7. Public hospital delivery	-0.009	68,450
	(0.008)	
8. Birth delivery paid by private health insurance	-0.003	68,858
	(0.008)	
9. Average pre-*PANES* birthweight in health center (g)	1.461	68,855
	(1.301)	
10. Gestational length (in weeks)	0.084**	68,858
	(0.037)	
11. Premature birth	-0.008	68,855
	(0.005)	
12. In bottom decile of weight per gestational length	-0.017***	68,858
	(0.005)	
13. Maternal health data in SIP dataset	0.003	68,858
	(0.007)	
14. Mother weight, kg (conditional on week of pregnancy),	0.511	21,374
first weighing visit (avg: week 16) (SIP)	(0.384)	
15. Mother weight, kg (conditional on week of pregnancy),	0.966**	21,374
final weighing visit (avg: week 35) (SIP)	(0.406)	
16. Mother smoked during first trimester of pregnancy (SIP)	-0.032**	21,374
	(0.015)	

Notes. 2SLS estimates of the effect of *PANES* participation on various dependent variables, in a specification equivalent to Table 2, column (2). Regressions in rows 14 and 15 additionally control for the week of first and last visit, respectively. See also notes to Tables 1 and 2.

Table 5: *PANES* program effects on socio-economic indicators and fertility

Dependent variable:	Coefficient estimate (s.e.)	Obs.
1. Value of income transfer during pregnancy	1040*** (5)	68,858
2. Value of Food Card during pregnancy	191*** (2)	68,858
3. Other household government benefits during pregnancy ±	-15 (24)	39,870
4. Mother formal sector earnings during pregnancy ±	-40*** (15)	39,870
5. Household formal sector earnings during pregnancy ±	-175*** (047)	39,870
6. Mother works during pregnancy	-0.013** (0.006)	68,858
7. Household total income during pregnancy ±	968*** (55)	39,870
8. Average birthweight in area of residence (g)	1.080 (0.729)	65,541
9. Out-of-wedlock birth	-0.021*** (0.007)	68,763
10. Missing child father information	-0.016* (0.009)	68,858
11. Fertility – all births	0.0013*** (0.0003)	1,037,793
12. Fertility – births within one year from baseline	0.0002 (0.0005)	377,562

Notes. 2SLS estimates of the effect of *PANES* participation on various dependent v1ariables, in a specification equivalent to Table 2, column (2). For rows 11 and 12 see text for details. ±: data available only since March 2004. See also notes to Tables 1 and 2.

SUPPLEMENTARY ONLINE APPENDIX – NOT INTENDED FOR PUBLICATION

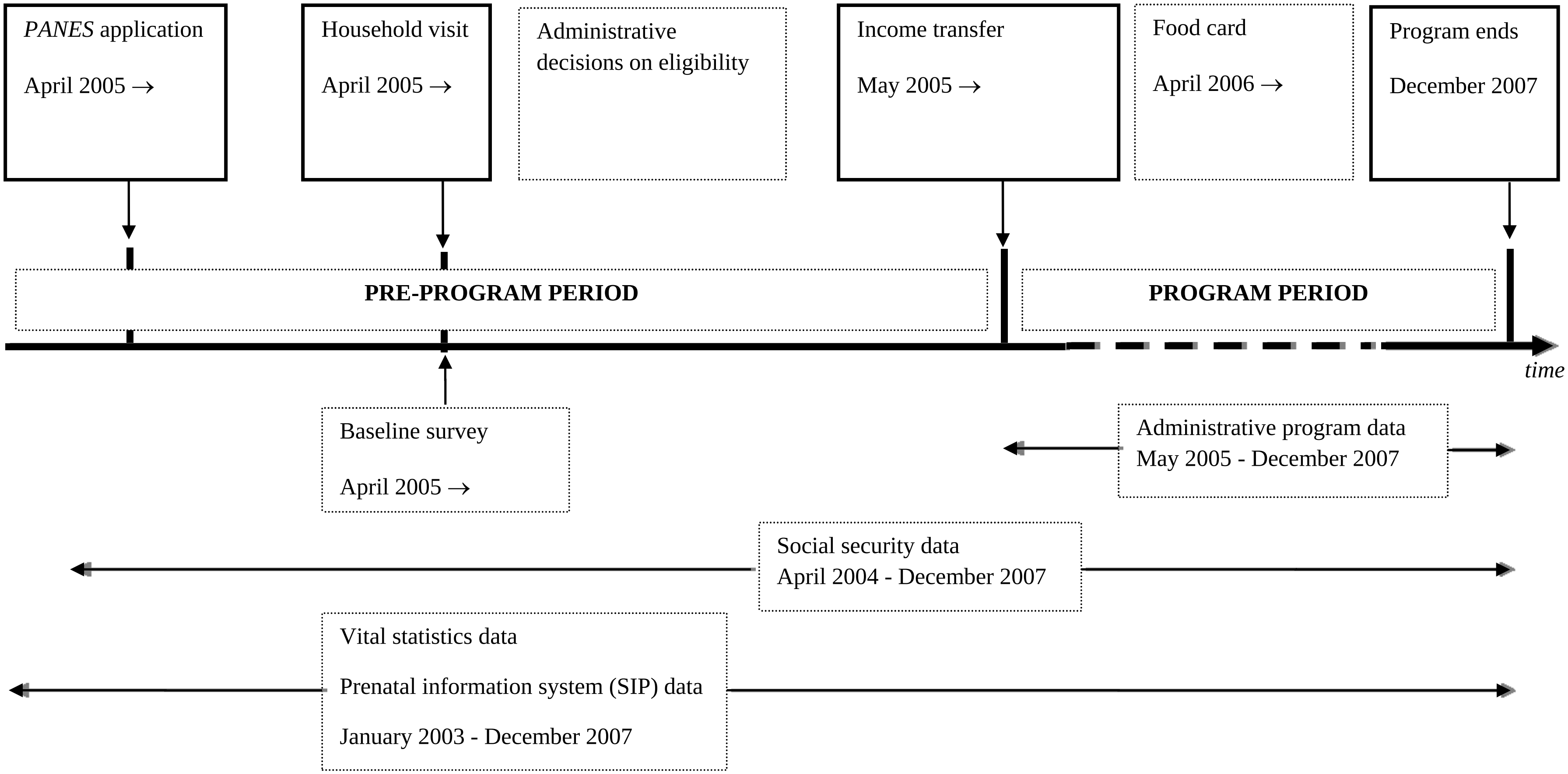
Appendix A: Additional tables and figures

Appendix Table A1: Child birth outcomes and income levels in Uruguay, U.S. and Latin America/Caribbean

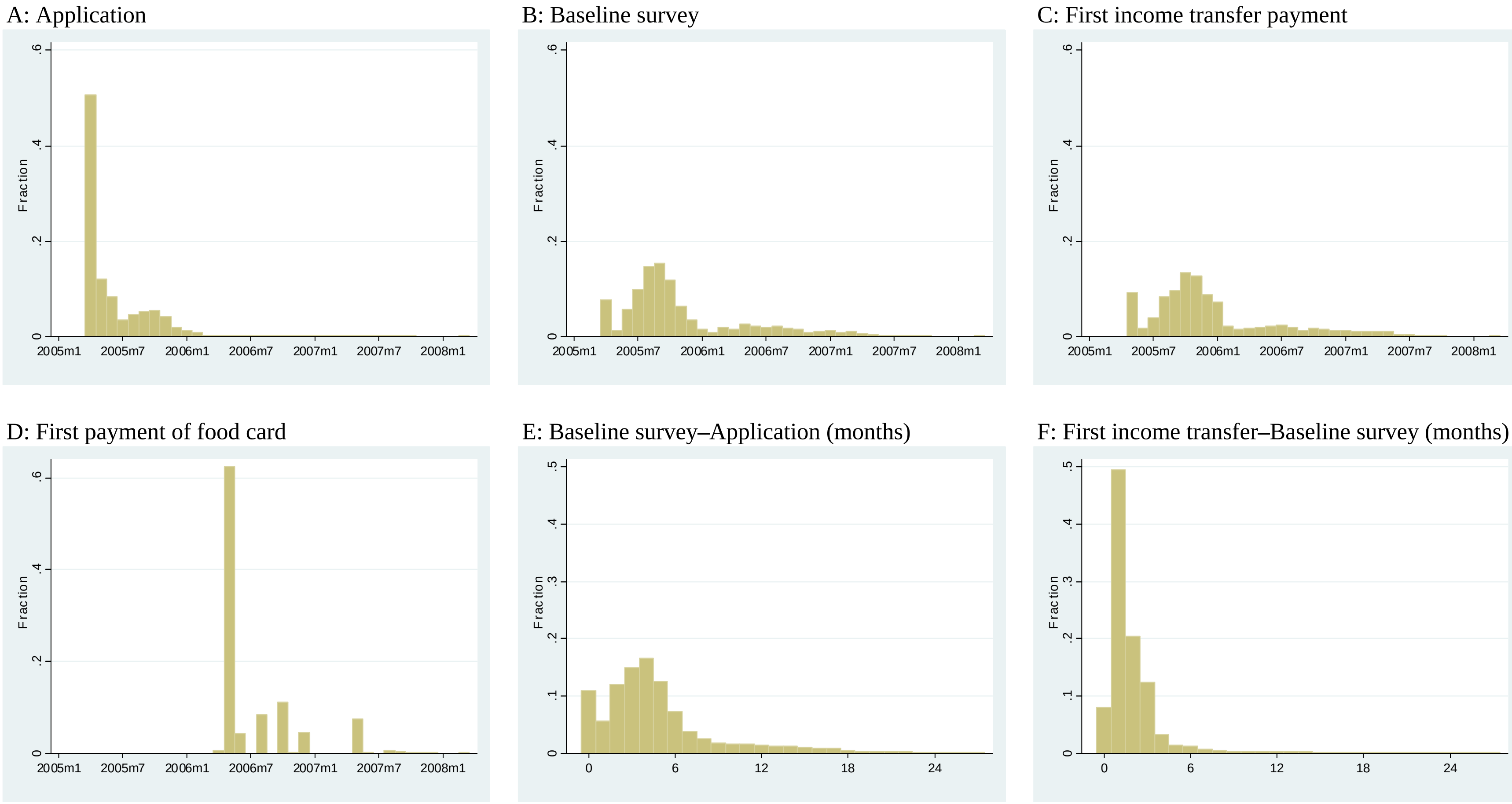
Country/Region	Low birth weight, % [(a)]	Infant mortality rate (per 1000) [(b)]	Births assisted by health personnel, %[(c)]	At least one prenatal visit, %[(c)]	Per capita GDP (PPP US$)[(d)]
Uruguay	8	11	99	97	13,189
United States	8	7	99	99	45,989
Latin America/Caribbean	9	19	96	95	10,575

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$.

Appendix Figure A1: Timing of *PANES* program activities and data collection



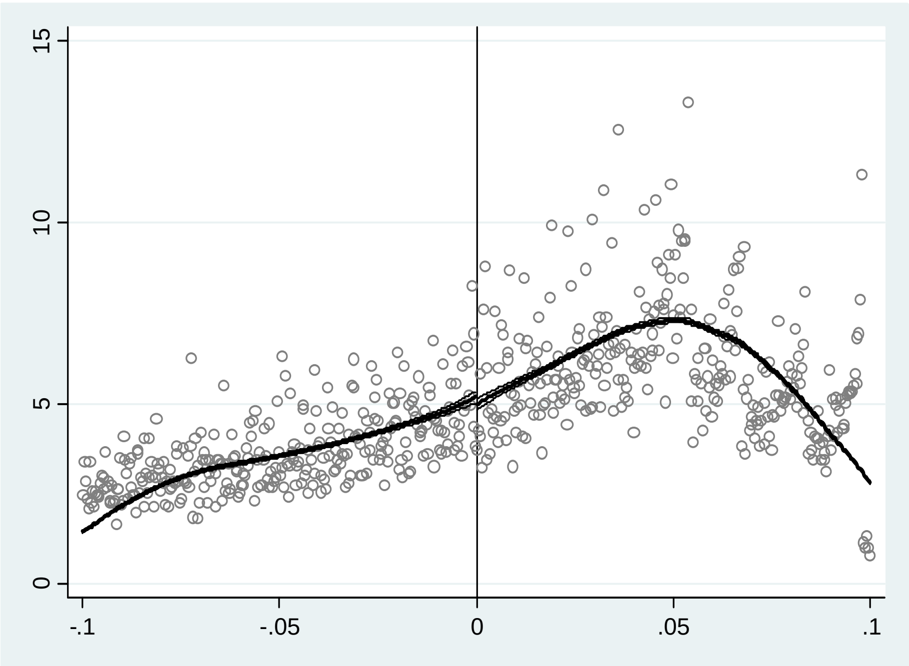
Appendix Figure A2: The timing of *PANES* program milestones



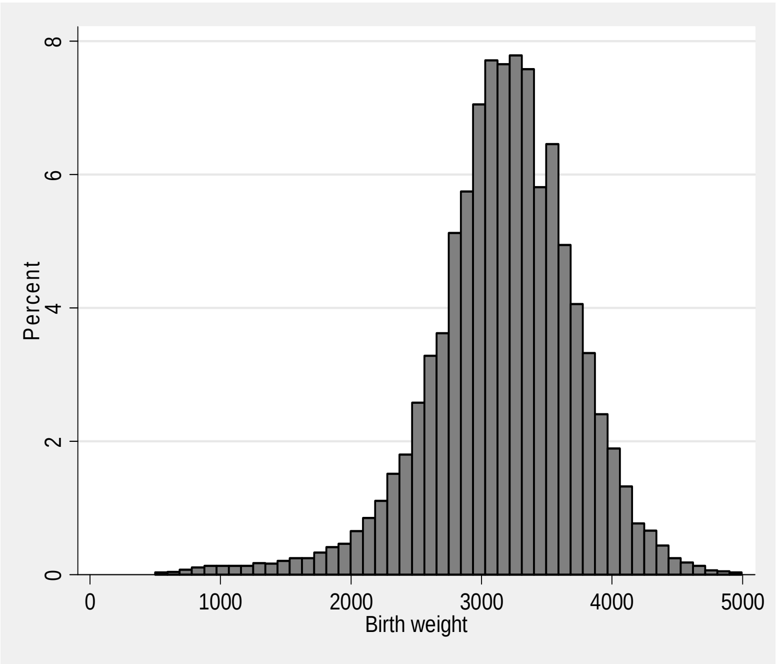
Notes. The figure reports the distribution of key program dates: application date (A), date of baseline survey (B), date of first payment of income transfer (C), date of first payment of the food card (D). Panels E reports the distribution of the differences between the variables in panels B and A, and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

Appendix Figure A3: Data integrity checks for the predicted income score (panel A) and birthweight (panel B)

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

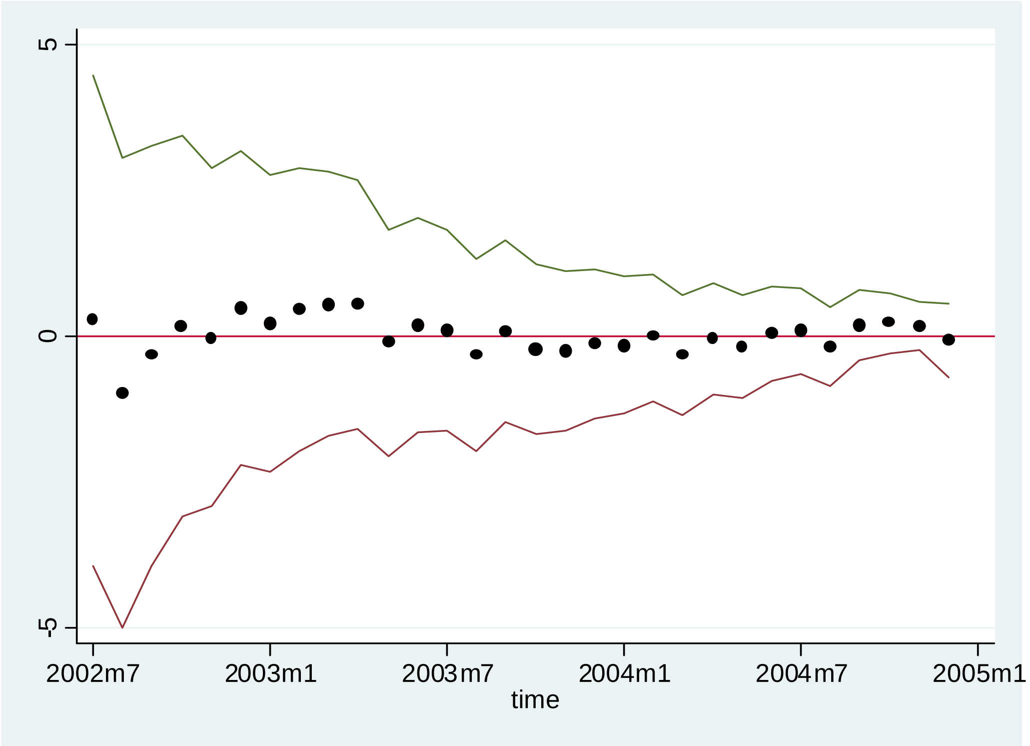


Panel B: Distribution of the birthweight measure (in grams)



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.

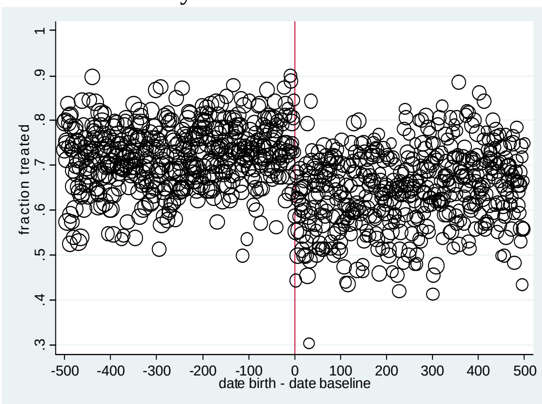
Appendix Figure A4: “Placebo” pre-program treatment effect estimates



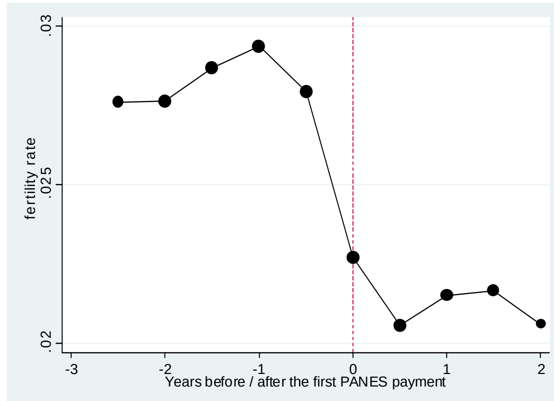
Notes. Each point is the reduced form coefficient obtained by artificially setting the program start date to each month preceding January 2005. 95% confidence intervals are also reported. The vertical axis is the fraction of low birthweight children.

Appendix Figure A5: Fertility rates and fraction treated as a function of time to/since key program dates

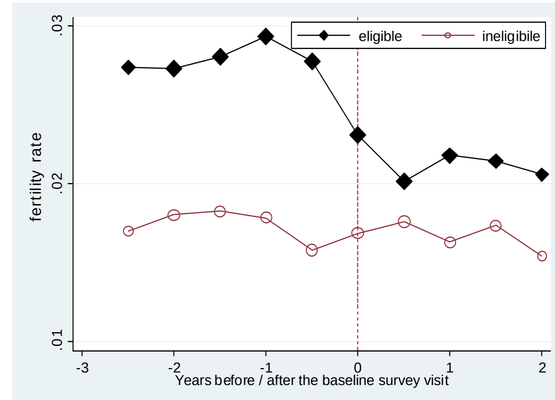
A: Fraction of children whose household was ever treated, as a function of date of birth relative to date of baseline survey visit



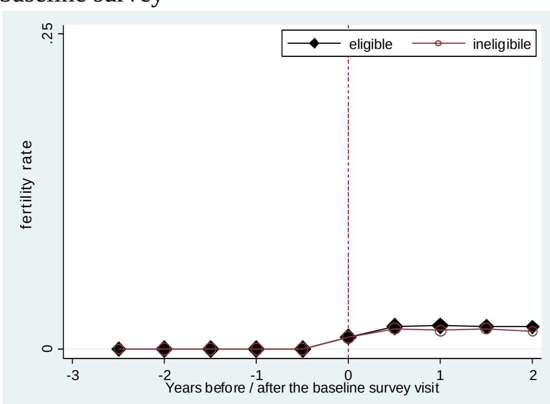
B: Fertilty rate as a function of time to/since first income transfer



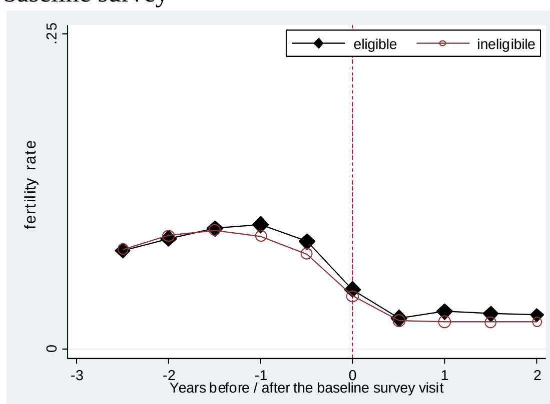
C: Fertilty rate as a function of time to/since baseline visit, by *PANES* eligibility status



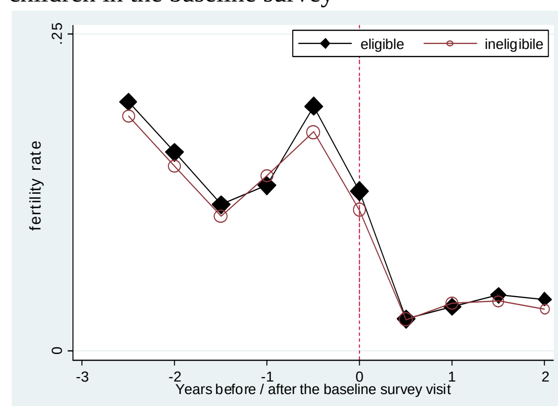
D: Fertilty rate as a function of time to/since baseline visit by *PANES* eligibility status – no children in the baseline survey



D: Fertilty rate as a function of time to/since baseline visit by *PANES* eligibility status – one child in the baseline survey



E: Fertilty rate as a function of time to/since baseline visit by *PANES* eligibility status – two or more children in the baseline survey



Notes. Panel A reports the proportion of children of applicant mothers whose household ever benefitted from the program as a function of the child's date of birth relative to date of the baseline survey. Panel B reports the proportion of *PANES* eligible mothers giving birth as a function of the time before/after the first payment. Panel C reports the fraction of *PANES* mothers giving birth as a function of the time before/after the baseline survey, separately for eligible and *PANES* ineligible women. Panels D, E and F report the same series as in Panel C 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% 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% 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% reported having received the Food Card while only a minority (17.6%) 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% 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.

Appendix C: Discussion of the internal rate of return (IRR) calculation

There are a range of plausible but hard to quantify benefits of child health investments, including the individual utility gains of simply feeling better by avoiding illness, as well as the cost savings in early life medical care and hospital costs of the kind documented in Almond, Chay and Lee (2005). The benefits to the parents of the children and other household members, as well any social benefits (due, for example, to reduced health care utilization) are also disregarded entirely below. We adopt a narrower and highly conservative approach, focusing only on the estimated future wage benefits of the intervention when the children who benefited reach adulthood. Thus the IRR estimates below are clearly lower bounds on the true social returns.

Since the *PANES* program was only carried out during 2005 to 2007 and the children born have not yet reached adulthood, we obviously do not have actual measures of their adult labor market outcomes, and need to make assumptions about the likely returns to reducing low birthweight. We use the estimated reduction in low birthweight from Table 2 (row 1, column 2) in the current paper, namely, a 1.7 percentage point reduction for children of mothers who received the transfer throughout most of their pregnancy. Following Currie and Moretti's (2007) U.S. study (in their Table 6), we further assume that low birthweight reduces adult earnings by 2.6 percent, although returns are plausibly even higher in a poor Uruguayan population, again making this a conservative assumption. We assume that individuals work from 18 to 65 years of age with earnings at the level of Uruguayan per capita income, and that the country's average per capita income growth rate of 2.3 percent during 1989-2009 continues over the coming decades.[31] In terms of costs, we assume the social cost of the program is the deadweight loss incurred on the tax revenue raised to fund the *PANES* transfers, which we assume is 20 percent.[32]

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[31] The source of the income data is the World Development Indicators (http://data.worldbank.org/country/uruguay).

[32] We are not aware of deadweight loss estimates for Uruguay but a recent paper estimates that the marginal cost of public funds in other developing countries is roughly 17%; refer to Auriol, E. and M. Warlters. (2012). "The Marginal Cost of Public Funds and Tax Reform in Africa", *Journal of Development Economics*, 97(1): 58-72.