Social Assistance and Birth Outcomes: Evidence from the Uruguayan *PANES*

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This paper estimates the impact of a large temporary poverty relief program - the Uruguayan *PANES* - on birth outcomes. Using program administrative data and longitudinal vital statistics, we estimate a significant and precisely estimated reduction in the fraction low weight newborns (less than 2,500 g.) on the order of 10 to 20% as a result of treatment.

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Introduction

Birthweight is a major predictor of health conditions and economic outcomes in further stages of life. Recent evidence from the economics literature shows significant negative effects of low birthweight - defined by the World Health Organization as weight under 2,500 grams - on both short run health outcomes and long run economic and non-economic outcomes, such as height, IQ, earnings, education and even birthweight of the next generation (Almond et al. 2005, Behrman and Rosenzweig 2004, Black et al., 2007, Currie and Moretti, 2007, Royer, 2009).

If low birthweight is associated with worse future outcomes, there is a potential economic gain from reducing its incidence (Behrman, 1996, Alderman and Behrman, 2006) and possibly a rationale for government intervention. Early childhood interventions might prove particularly cost effective since they have presumably higher rates of return than later interventions, due to their benefits extending over a longer time span and potential complementarities with other inputs (Heckman 1995, 2000).

Indeed, there is evidence for the USA that targeted programs aimed directly to improving pregnant women's health and nutrition have large positive effects on infants' health. There is still little agreement though as to whether unrestricted social assistance programs, especially in the form of cash transfers, lead to improvements in birth outcomes.

This paper addresses some of these questions by estimating the impact on low birthweight of an emergency poverty relief program, the Uruguayan *PANES*, that provided beneficiaries with cash and a (comparatively modest) food transfer for approximately two years between 2005 and 2007. Relative to existing studies, there are some major advantages to our research.

First, we are able to link administrative data on program participation for women of childbearing age to vital statistics for the period 2003 (hence two years before the start of the program) to 2007 using mothers' unique national identity number. This allows us to study the effect of the program at the individual level, potentially comparing siblings' outcomes, and cover the universe of the country's births, an endeavor that is rarely possible in other settings, including the USA.[1]

Second, as explained below, because of the rules determining eligibility, we are able to control for the non-random assignment of households to the program using quasi experimental techniques. This is particularly relevant, as social assistance beneficiaries are typically more

[1] Most studies for the USA, reviewed below, either use survey data on self reported program receipt and birth outcomes, or, in the absence of micro data, they link program and vital statistics data at some geographical level of aggregation (i.e. county). Barber and Gertler (2008), also reviewed below, use self-reported birthweight for Mexico.

likely to give birth to low weight children, a problem that plagues any credible attempt to estimate causal program effects.

Third, this is among the first papers that specifically investigate these effects in a middle income Latin American country, ultimately advancing our knowledge of the determinants of early child development and the role of policies in the region.

Finally, it is worth emphasizing that differently from several other middle and low income countries, and even the USA, Uruguay has a universal public health system in place (that coexists with a large private health sector), with free access for low income households as well as for all pregnant women. This allows us to identify the effect of welfare transfers in isolation from increased health insurance coverage, again something that is not always true of studies that examine the effect of welfare programs in other contexts.[2]

In order to estimate the effect of the program, we start by comparing the difference in the incidence of low birthweight between infants of program beneficiaries born before and after program participation. Because different households entered the program at different points in time, we can use a simple difference in difference estimator that allows us to control for generalized trends in low birthweight that might be correlated with trends in program take up. Because we have data on repeated births from the same mothers, we can refine this strategy by comparing treated and untreated siblings, hence allowing us to control for unobservable time invariant household and mother characteristics. Because, as explained below, program eligibility depended on a discontinuous function of a baseline poverty score, we are finally able to control for a continuous function of such score and estimate the effect of the program in the neighborhood of the eligibility threshold, in the spirit of a regression discontinuity design. Our results are extremely robust, showing a reduction in the incidence of low birthweight on the order of 10% to 20% as a result of program participation.

In the second part of the paper we investigate the channels behind the estimated effects. We find little evidence that increased prenatal care utilization, selective fertility or reduced mother work involvement, with an associated reduction in physical and possibly psychological stress, drive our findings. The nutrition channel appears the most plausible, suggesting that unrestricted social assistance programs -even when largely in the form of cash transfers- appear to lead to improvements in birth outcomes.

[2] Currie and Cole (1993) for example report that AFDC mothers have a higher probability of access to Medicaid, as well as of participating in the Food Stamp Program, and being given priority in the allocation of public housing and renting subsidies.

The paper is organized as follows. Section 1 reviews the literature on the determinants of weight at birth, and the effect of government transfer policies. Section 2 provides some institutional information about the program. Section 3 presents the data. Section 4 presents the empirical strategy while section 5 presents the regression results. Section 6 finally concludes.

1. Determinants of low birthweight and the role of government policies

According to the medical literature, mother's health and nutrition are major determinants of birth outcomes, with maternal under-nutrition, anemia, malaria, acute and chronic infections, pre-eclapmsia and cigarette smoking typically identified as the most important risk factors leading to intrauterine growth retardation, in turn a major determinant of low birthweight (Kramer, 1987). The other primary cause of low birthweight is preterm delivery. Prematurity is typically associated in the medical literature to genital tract infections, poor antenatal care and mother's physical and psychological stress and anxiety.

Attempts in the economic and social science literature to link weight at birth with household economic conditions and indicators of socio-economic status that are thought to be correlated with the above risk factors, though, lead to mixed conclusions. Currie and Moretti (2003) find that higher maternal education improves infants' outcomes, arguably due to its effect on maternal behavior (for example, by reducing smoking), increased earnings, improved women's marriage markets and reduced fertility. Although not necessarily in contrast with these findings, Conley and Bennett (2000) though find that income during pregnancy has no effect on the risk of low birthweight.

More direct evidence is available from studies that analyze government welfare and transfer policies. There is evidence that nutritional programs specifically targeted to pregnant women have significant positive effects on birth outcomes. Bitler and Currie's (2004) study of the USA Special Supplemental Nutrition Program for Women, Infants and Children (WIC), which provides food and nutritional advice to pregnant women, for example, finds that WIC reduces the probability of bearing low weight infants.

One channel through which WIC appears to have an effect is via prenatal care utilization. There is a consensus in the literature that prenatal care, especially in the first trimester, is effective in improving infant health through the opportunities that it provides for early diagnosis and prognosis, and information and education about best practices (Kramer, 1987, Alexander and Korenbrot, 1995).

There is more mixed evidence on the effect of unrestricted social assistance, i.e. programs that are not specifically targeted to improving the nutritional status of pregnant women or birth outcomes. Currie and Cole (1993) for example find a positive although imprecisely estimated effect of participation in a cash program, the Aid to Families with Dependent Children (AFDC) on birthweight. While Almond et al. (2009) find sizeable and precisely estimated negative effects of the Food Stamps program on low birthweight,[3] Currie and Moretti (2008) find the opposite for California, a fact that they explain with increased fertility among mothers more likely to have negative birth outcomes.

Specific evidence for Latin America focuses on the impact of Conditional Cash transfers on weight at birth. Barber and Gertler (2008) evaluate the impact of *Progresa/Oportunidades* on birthweight using the random assignment of the program across communities. Based on a sample of 840 women, they find a very pronounced negative effect of the program on the incidence of self reported low birthweight that they attribute to women's empowerment, i.e. their enhanced abilty or opportunity to take actions that positively affect the health and welfare of their families.

2. The intervention: *PANES*

In this paper we focus our attention on the Uruguayan *Plan de Atención Nacional a la Emergencia Socia*l (*PANES*), a temporary social assistance program implemented between April 2005 and December 2007.[4] The target population consisted of the 20% poorest households among those below the national poverty line.

The program was devised by the centre-left government that gained office following the 2004 elections in response to the severe economic crisis of 2001-2002, when per capita income fell by more than 10%, 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 transfers to the elderly population, a fact reflected in marked differences in poverty incidence by age, with nearly 50% of children age zero to five living in poverty compared to 8% for the over 65 (UNDP, 2008). Consistent with widespread child poverty, Uruguay fared poorly relative to other middle income Latin American countries such as Costa Rica, Chile and Cuba in terms of infant mortality, low birthweight and indicators of health care utilization (Table 1).

[3] The program is estimated to have reduced the incidence of low birthweight by 7-8 % for whites and 5-12% for blacks.

[4] The program was replaced in January 2008 by a new system of family allowances accompanied by a large health care reform and a major overhaul of the tax system.

2.1 Eligibility

Applications opened in April 2005 and were accepted for the entire life of the program, i.e. until December 2007. Following an early application phase, which for most households happened by June 2005, households were visited by Ministry of Social Development personnel and administered a detailed baseline survey (Figure A1). The survey served the purpose of computing a predicted poverty score (a linear combination of a large array of household socioeconomic characteristics) that in turn determined eligibility.[5] Only households with a poverty score above a predetermined level were assigned to the program. Because of the assignment criteria, 95% of participant households had at least one child, a feature of the program that needs to be kept in mind when we analyze its impact on fertility.

A second condition for eligibility pertained to income. The program was means tested: eligibility could be gained and retained only if and as long as per-capita income as resulting from social security data was below approximately US$50 per month.

Of the 188,671 applicant households (around 700,000 individuals), around 102,000 eventually became program beneficiaries, approximately 10% of all Uruguayan households (and 14% of the population of around 3.3 million). The cost of the program - that was financed by internal resources - was approximately 250 million US$, i.e. US$2,500 per beneficiary household. On an annual basis, this is equivalent to 0.41% of GDP and 1.95% of government social expenditures.

2.2 Program components

PANES was a conjoint of social policies, the most sizeable and widespread ingredient being a monthly cash transfer (*ingreso ciudadano*, "citizenship income"), whose value was originally set at US$56 (UY$1,360 at the 2005 exchange rate, and later adjusted for inflation) independent of household size, amounting to approximately 50% of average pre-program household self-reported income.

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[5] The poverty score, devised by researchers at the University of the Republic (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 (household age structure and headship, an indicator for public employees in the household, an indicator for pensioners in the household, average years of education of individuals over age 18 and its square, interactions of age indicators with gender, 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 first 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. 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 its manipulation. The eligibility thresholds were allowed to vary across five regions. There is evidence of almost perfect enforcement of the eligibility rule (Manacorda et al., 2009).

Households were also entitled to an electronic Food card (*tarjeta alimentaria*), whose monthly value varied between US$13 and US$30, depending on the number of children and pregnant women in the household. This component was launched in the first semester of 2006, and its expansion was slow.[6]

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. Similar to other recent Latin American transfer programs, *PANES* participation was in principle conditional on health checks for pregnant women and children (plus children's school attendance). In particular, for pregnant women, the program prescribed monthly prenatal visits (weekly from week thirty-six) and three mandatory ultrasound scans. Although around 45% of *PANES* households were mailed a health card (*carnet de compromiso sanitaria*), which was supposed to help monitoring health controls, due to institutional weaknesses and scarce inter-institutional coordination, conditionalities though were not enforced, an issue publicly acknowledged by the Government after the end of the program, and there is evidence that most households were unaware of their existence.[7]

3. Data

The empirical analysis brings together a number of micro data sets from different sources. Baseline program data provide information at baseline on socio-demographic characteristics, housing conditions, income, labor market participation and schooling and durable possession for both successful and unsuccessful applicants at one point in time. For each individual, these data also provide the unique national identification number (*cedula*) and the exact value of the household poverty score used to determine eligibility.

These data are matched to vital statistics microdata that provide information on all registered live births in the country.[8] These data are available every year from 2003 to 2007, so

[6] The program encompassed 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 17% 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 test, prenatal visits and mammography for women and prostate exam for men) and were offered dental care and prostheses and eye surgery.

[7] A sample survey in 2007 asked beneficiaries about knowledge of conditionalities: 58% of households were aware that conditionalities were attached to the program and, among these, only 20% mentioned gynecological controls. Amarante et al., (2008, 2009) though find some evidence of increased care utilization (vaccinations and health checks) due to the program.

[8] The Uruguayan Vital statistics system defines a newborn as the expulsion or extraction from the mother's body of a product that, after the separation and independent of the length of the pregnancy, breaths or shows any sign of life as heart activity (INE, 2009).

before the inception of *PANES* (April 2005) (Figure A1). Uruguayan vital statistics constitute an extremely reliable data source: at 98%, the country has the highest level of registered births in Latin America (UNICEF, 2005, Cabella and Peri, 2005, Duryea *et al,* 2006). Vital statistics come from certificates filled by physicians at the time of birth and they gather information on the circumstances of birth, including birthweight, parental characteristics and the reproductive trajectory of the mother. The confidential version of the data used in this paper also includes the mother's *cedula*. This allows us to link the vital statistics micro data to the program data. Finally, we further link program and vital statistics data to Social Security records for all household members of *PANES* household applicants. These data contain month to month information on income from formal employment and the value of the *PANES* transfers. Again, the confidential version of these data provides the unique *cedula* number for each individual.

Vital statistics data are summarized in Table 2 that reports averages over the period 2003-2005, before program inception. Here we report information for three groups of mothers: (those who eventually became) *PANES* beneficiaries (column 1), those who unsuccessfully applied to the program (column 2), and those in households that did not apply (column 3). Roughly speaking, these three groups correspond to increasingly higher levels of income and socio-demographic status. *PANES* births account for more than 20% of all births. As *PANES* individuals represent around 14% of the population, this is clear indication of higher fertility in this group relative to the rest of the population.

The data show a clear gradient in birthweight across groups. While among *PANES* households the fraction of births below 2,500 grams is 10%, among non-*PANES* applicant households this fraction is 8%. There is a very clear indication that at baseline *PANES* mothers underwent the lowest number of prenatal visits (6.54 versus 8.42 for non-applicant mothers) and that they had their first prenatal visit later on during the pregnancy (in the seventeenth week compared to the fourteenth week for the group of non-applicants). *PANES* mothers were also more likely to give birth in the public health system, and to use birth centers that - on average - deliver lighter children. This is most likely the result of the stratification of households across health centers and residential areas based on socio economic status. There is also a clear gradient in the number of weeks of gestation, which was the lowest for *PANES* mothers and the highest for non-*PANES* mothers.

Additional information pertains to the reproductive history of the mother and parents' socio-demographic characteristics. As expected, there is a clear indication of *PANES* status being negatively correlated with mother's education and employment status, and positively correlated with the number of previous births. *PANES* mothers are also more likely to be unmarried to the father's child.[9] *PANES* children have the highest probability of displaying no information on the father. Conditional on this information being reported, *PANES* fathers display the lowest level of education and the highest probability of non-employment.

Table 3 reports regressions of the probability of low birthweight on a number of mothers' and birth characteristics between 2003 and 2005. These regressions are only meant to give a sense of the correlations in the data, and help interpret the estimated impact of the program below, rather than carrying a causal interpretation. It is clear that there are considerable differences in birthweight among children from different socio-economic backgrounds. There is a clear gradient in mother's education: for example children of mothers with completed primary education are (conditional on the other variables) 2 percentage points less likely to be underweight relative to mothers with incomplete primary education. The number of pre-natal visits and the week of first visit are also negatively associated with low birthweight (with coefficients respectively of -0.005 and -0.001) and children born out of the wedlock are 1 percentage point more likely to be underweight. As expected, the incidence of low birthweight falls with the length of gestation (coefficient -0.07).[10] In sum there are pronounced differences in the incidence of low birthweight across socioeconomic groups and groups with different attributes and behaviors that are also reflected in differences between *PANES* and non-*PANES* households

4. Model specification and identification

In order to ascertain the causal effect of program receipt on outcomes, we exploit the timing of incorporation of households into the program. Exposure to *PANES* is defined as:

$$EXP_{imt} = t_{im} - t_{0m} \text{ if } t_{im} > t_{0m}$$
$$= 0 \qquad \text{otherwise}$$

where t_{im} is the date of birth of child i of mother m and t_{0m} is date of entry of mother m into the program, defined as the time in which the household received the first *PANES* payment. Both time variables are expressed in months. Our basic model is:

$$(1) \qquad Y_{imt} = \beta_0 + \beta_1 e_{imt} + X_{imt}'\beta_2 + P_m + d_t + u_{imt}$$

[9] . This fraction is extremely high in Uruguay, with two out of three children born out of the wedlock.

[10] Similar results are reported by Matijasevich et al. (2004) and Jewell and Triunfo (2007) for births in the public health sector.

where Y is the outcome variable (e.g. low birthweight), P_m is dummy for *PANES* eligibility (that is time invariant) and $e_{imt} = I(EXP_{imt}>3)$ is a dummy equal to one for pregnancies that have concluded at least one trimester after the mother entered the program, effectively measuring exposure in uterus of at least one quarter. The treatment variable is time variant, as mothers gradually join the program.

The model is estimated using all applicant mothers, including those who did not qualify. For children of unsuccessful applicants, the treatment variable is set equal to zero.[11] The vector X_{imt} includes exogenous mother's and birth's characteristics, including potentially time of entry into the program effects (d_{0m}). d_t are time of birth fixed effects. By conditioning on time of birth, model (1) abstracts from generalized trends in the incidence of low birthweight due to secular improvements in the quality of health care, improvements in living standard or changes in measurement accuracy over time. By conditioning on d_{0m} the model abstracts from the circumstance that latent birthweight might vary across mothers with different time of entry, perhaps due to potentially endogenous differential application times or differential lengths of application processing.

Identification of the model is warranted by the interaction of time of entry and time of birth. Because time of entry varies across mothers, both time of entry and time of birth effects can be identified. This is simply a diff-in diff model that compares the difference in birthweight between a treated and an untreated child to the difference in birthweight between a couple of otherwise identical children who were both treated and untreated.

We also present regressions that include mother fixed effects. Although at the cost of some precision, there are obvious advantages from using repeated births from the same mother, as this allows to controls for any potential correlation between exposure to the program and birth outcomes due to mother's unobserved time invariant characteristics.[12]

A concern arises if time of birth (t_{im}) is endogenous. For example, if following entry into the program, mothers with a higher propensity to give birth to low weight children tend to have children, or tend to anticipate fertility (the birth data are right censored), then one will find that low birthweight is negatively correlated with program exposure, although this is no indication o casual program effect. A related concern is selective survival: if treatment increases survival

[11] Model (1) can be identified using exclusively successful applicants. The inclusion of unsuccessful applicants should enhance the precision of the estimates though.

[12] One concern is that low birthweight might affect subsequent fertility choices (Del Bono et al, 2008). While this is in principle problematic for the fixed effects estimates, this should not be a major source of concern in our analysis, to the extent that this margin of selection affects treated and untreated mothers equally.

rates among fetuses at risk of being born low weight, then this is likely to lead to the opposite bias. We return to the issue of endogenous fertility and selective survival below.

A complementary identification strategy exploits the discontinuous assignment of households to the program based on the baseline poverty score.

If by S_m we denote the household poverty score, standardized to the eligibility threshold and, as above, $P_m = I(S_m>0)$ is a dummy for eligibility and $e_{imt} = I(EXP_{imt}>3)$ is a dummy equal to one for children exposed to the program at least one quarter before birth, the RD model is:

$$(2) \qquad Y_{imt} = \phi_0 + \phi_1 e_{imt} + X_{imt}'\phi_2 + f(S_m) + P_m + d_t + v_{imt}$$

where $f(\ )$ is continuous function of its arguments that in practice we model as a linear or quadratic function, with potentially different parameters on either side of the threshold. By fitting splines on either side of the threshold, the model attempts to identify the effect of program exposure in the neighborhood of the discontinuity, i.e. comparing children in barely eligible and barely eligible households, in the spirit of a regression discontinuity design.

5. Regression results

Based on the above discussion, one can speculate about the potential effects of *PANES* on birthweight. The cash transfer, together with the Food card, had the potential to improve mother's nutritional status and possibly intrauterine growth and, via this, weight at birth.[13]

In what follows, we first analyze the impact of the program on low birthweight and then we explore whether other correlated outcomes and indicators of behavior were affected.

5.1 Effects on birth outcomes

Table 4 reports the main regression estimates of the coefficient β_1 on positive exposure in equation (1). Each row refers to a separate dependent variable and each column to a different specification. Standard errors in all specifications are clustered at the mother level. The total number of observations, once we drop those with no valid *cedula* (2.5% of the sample), is 68,174.

Column (1) reports regressions with no controls other than a dummy for *PANES* eligibility (P_m) and time dummies. Column (2) reports results with the inclusion of a number of

[13] Standard economic analysis suggests that in-kind food transfers should be particularly effective in increasing food consumption among rationed households, i.e. those that, in the absence of the program, would spend less on food than the value of the transfer.

additional time varying controls (dummies for mother's age, sex of the child, multiparity, number of previous pregnancies, plus dummies for month of first *PANES* payment). Colum (3) reports results with the addition of mother fixed effects. Results in columns (1) to (7) control for parametric splines of the poverty score in the neighborhood of the eligibility threshold. We report four specifications, respectively for observations with a poverty score in the range -/+0.2 -/+0.1 around the discontinuity, and with the inclusion of a linear and a quadratic polynomial in the score.

The main OLS specification in column (1) shows that low birthweight falls by around 1 percentage point following program exposure, and the estimate is significant at conventional levels. This is equivalent to a 10% fall in the incidence of low birthweight and implies that, as a result of treatment, *PANES* beneficiaries close the gap in low birthweight with unsuccessful applicants (see Table 2).

When we include controls, in column (2), the point estimate falls to -0.14. Estimates that include mother fixed effects and are restricted to mothers with at least two births in column (3) are larger in absolute value (-0.020) and still statistically significant. Estimates in the neighborhood of the discontinuity are in the same range as the diff-in-diff estimates and statistically significant (-0.13 to -0.14).

To get a visual impression of the estimated effect, Figure 1 reports trends in low birthweight as a function of exposure time (EXP_{imt}), i.e. time to and since the first *PANES* payment. The points in the figure are average residuals by time of exposure from a regression of low birthweight on time of birth dummies (d_t), a dummy for being a *PANES* eligible household (P_m) and dummies for time of entry (d_{0m}). Straight lines are estimated means on each side of the zero cut-off. The figure shows a clear fall in low birthweight for children exposed to the program in uterus for at least a full quarter relative to those unexposed.

We find a small but imprecisely estimated effect of program exposure on average weight (on the order of 10 to 20 grams), which is consistent with the program having an effect only at the bottom of the weight distribution. Figure 2 reports the implied proportional change in the fraction of newborns below any given weight level, with the associated 95% confidence interval. Coefficients are estimated based on regressions similar to those in row 1, column (3), of Table 4. Indeed, it appears that the program reduced the incidence of low birthweight, especially in the range below 2,700 g., with effects on the order of 10-15%.

It appears that program exposure marginally increased gestation length but the effect is extremely small (on the order of half a day, i.e. an increase of 0.2% from a baseline average gestation length of 38.5 weeks) and generally not significant at conventional levels. This very modest fall in gestational length is unlikely to explain the observed fall in low birthweight: the addition of dummies for weeks of gestation in row 1 makes virtually no difference to the estimated values of β_1 (results available upon request). The fall in low birthweight is accompanied by an increase in the number of prenatal visits, although again estimates are at the margin of significance and the effect is small, on the order of 0.10 more visits, i.e. an increase of around 1.5% relative to baseline. More important, although there is some evidence that total number of visits increases, there is little evidence that the week of first visit, considered a crucial variable for the detection and treatment of risk factors and the prevention of negative birth outcomes, falls. Again results stay unchanged if in the low birthweight equation in row 1 we include dummies for total number of visits and weeks of first visit (result not reported).[14]

In sum, the table shows clear evidence of *PANES* affecting the incidence of low birthweight. This is unlikely to be due to longer gestation. We find some evidence of the program affecting prenatal care utilization. However, consistent with the circumstance that health ocnditionalities were not enforced, this effect is small, and highly unlikely to explain the sizeable program effect on low birthweight.

5.2 Effects on income and labor supply

Having ascertained that the program had an effect on low birthweight, we turn to assess its effect on household income, both from labor and other sources. For this purpose, we merge the data in Table 4 with social security data from March 2004 to December 2007 (no data is available before March 2004).

The Social Security administration collects month by month information on employment spells and earnings for the almost the universe of formal workers (both in the private and public sectors, and both employees and self-employed), as well as social benefit payments, including *Ingreso Ciudadano*.[15] We also use data on the monetary value of Food card that was administered directly by the Ministry of Social Development.

Rows 1 to 5 of Table 5 report regression estimates of the effect of the program on income and labor supply. The table has the same structure as Table 4 but for brevity we omit reporting regressions in the neighborhood of the eligibility threshold (estimates, available upon request, are very similar). Row 1 reports the eligibility coefficient in a regression where the

[14] Similarly, the effect of treatment on prenatal visits disappears when we include in row 4 dummies for length of gestation, suggesting that perhaps the effect of the program on the number of visits is mechanically due to increased gestational length.

[15] Excluded workers are those who contribute to separate retirement funds (army and police, banking and some liberal professions).

dependent variable is the value of the *Ingreso Ciudadano*. This measures the average monthly value of the transfer in the previous nine months. All monetary values are expressed in April 2005 pesos. The estimated coefficient is on the order of UY$1100, approximately US$45 at the 2005 exchange rate, implying that, as expected, eligible households were more likely to receive the cash transfer. Results stay essentially unchanged when additional controls and mother fixed effects are included. This number is around 20% lower than the value of the transfer (UY$1,360), implying less than full compliance. An inspection of the data reveals that this is almost entirely driven by some eligible household not receiving the transfer rather than non-eligible households benefitting from the program. The main reason for this is that some eligible households lost the transfer as their income grew and they failed to satisfy the income test for program participation. Still, the effect of eligibility on the value of the cash transfer is sizeable and significant.

Row 2 reports the effect of the program on the value of the Food card. Estimates vary little across specifications: it appears that eligibility is associated to in-kind transfers of an average value of UY$200 to 250, around US$10. As in the case of the income transfer, the average value of the Food card is below what one would have expected ex-ante assuming full take up.

Row 3 reports the effect of eligibility on household monthly earnings as resulting from social security records: the program was associated to a fall in household earnings on the order of UY$150 to 300 per month. It is possible that the transfer reduced labor supply due to an income effect. However, given these households' low income, one would not expect labor supply to be very elastic. Perhaps a more plausible explanation is that the means tested nature of the program reduced the incentives to engage into formal employment. It is possible though that *PANES* households were more likely to be employed in the informal sector in order to avoid losing eligibility. This suggests that results in row 3 - if anything – are downward biased estimates of program impact on earnings. Despite these offsetting effects, *PANES* households appear to have experienced a rise in total income (earnings plus *PANES* transfers, both in-kind and in cash) on the order of $UY750. This is a remarkable increase: among unsuccessful applicants, total household income from formal employment between July 2006 and December 2007 (when the program had completely rolled out) was on the order of UY$2,500, i.e. just over 100 USD per month, implying that the program increased income by around 30%.

Rows 4 and 5 look specifically at mother's earnings and labor supply. There is little evidence that *PANES* mothers worked less or earned less (the estimated impact on earnings is on the order of UY 20 to 50, i.e. around US$1-2,5 per month) than their non-eligible counterparts. This should be no surprise, as labor market participation, even among non-*PANES* mothers, was low, at around 17%.

In sum, despite offsetting labor supply responses, *PANES* households enjoyed a significant rise in total income, with potential effects on mothers' and fetuses' nutritional status. Failure to find a significant effect on mother's labor supply is an interesting finding in its own. Del Bono et al. (2008) find that work interruptions before birth are beneficial for birthweight, especially if occurring in the last two months of pregnancy. Because we find no significant effect of the program on mother's labor supply, we can confidently rule out that this was a major channel behind the improvements in birth outcomes among the *PANES* population found in Table 4.

5.3 Effects on fertility

We finally investigate the effect of the program on fertility. What evidence there exists on the effect of welfare transfers on fertility rates shows zero or modest positive effects (see for example Moffitt, 1998 and Almond et al., 2009). A positive impact on fertility, that is likely to manifest to only in the medium run, is consistent with the income effect in Becker's (1960) model, where children are a normal good, although income transfers might also increase the investment in the quality of children, hence reducing fertility (as in Becker and Tomes, 1976). It also appears that policies that make program receipt or generosity conditional on the number of children can increase childbearing due to a price effect (see for example Stecklov et al, 2006 for Honduras). In this respect, the temporary nature of the program, the circumstance that the amount of the transfer was independent of the number of children and the fact that the program was not explicitly targeted to pregnant women or women with children, imply that we do not expect a large fertility effect, at least in the short run. [16]

To investigate such effect, we consider all *PANES* applicant women of child-bearing age (age 12-50, whether they had a child or not between 2003 and 2008) and we create a balanced monthly panel that spans from January 2003 to December 2008. For each month, we construct a dummy equal to one if the woman gave birth and zero otherwise.

Entries in the last row of Table 5 are the estimated effects of *PANES* eligibility on the fertility dummy. The model includes time dummies plus, depending on the specification, other controls. Overall we have information on 174,399 women and more than 10 million woman-

[16] Obviously, the program might also have affected fertility rates via its effect on the probability of fetus' survival (as in Currie and Moretti, 2008) or abortion.

time observations, with an average fertility rate of 0.65%, implying that, out of 10,000 women in this group, 65 give birth to a child each month.

The coefficient on the dummy for positive program exposure is negative and significant. Results stay unchanged if we control for time varying covariates or even mother fixed effects. The estimated coefficient is on the order of -0.0012 to –0.0016, implying a very pronounced fall in fertility, of around 20% (0.0014/0.065). This is also evident in Figure A3 in the appendix that plots average fertility rates among *PANES* beneficiaries as a function of the time of entry into the program. There are reasons to be cautions in interpreting this fall as a causal effect of the program: not only, as said, ex-ante would one not expect pronounced fertility effects, but the exact timing and magnitude of the fertility fall seems hard to reconcile with the program incentives structure.

To investigate the issue further, Figure A4 in the appendix plots fertility rates as a function the time to and since the baseline *PANES* survey (as opposed to the time of first payment as in Figure A3), which is precisely the time at which household characteristics used to determine eligibility were measured (Figure A2). The left hand side panel of Figure A4 reports data for eligible applicants. Perhaps unsurprisingly, the data show a similar pattern to that in Figure A3, with a fall in fertility among successful applicants after the time of the baseline survey. Equally, if not more, interesting is the circumstance that, among unsuccessful applicant households, in the right hand panel of Figure A4, fertility is low before the baseline survey and increases abruptly afterwards.

There is an obvious explanation for the findings in Figure A4: because of the rules determining eligibility discussed in Section 2, the probability of being selected for the program was higher for women with young children, including those who had recently given birth. It follows that the probability of giving birth before the baseline survey is higher among successful applicants than among unsuccessful applicants, while the reverse is true after the baseline survey. Combined with state dependence in fertility rates (the probability of giving birth to a child today is essentially zero conditional on having given birth in the last nine months), this is likely to explain the fall in fertility among successful applicants after joining the program.

This is finally confirmed in Figure A5 in the appendix that plots fertility rates for successful and unsuccessful applicants as a function of the time to and since the baseline survey (as in Figure A4) separately by number of children born between January 2003 and the baseline survey date (0, 1 or 2 or more).[17] In practice, we restrict fertility rates across the two groups to be on average the same up to the baseline survey date, so it is no surprise that before that date that two series almost overlap. What is remarkable though is that the fertility behavior of the two groups after the baseline survey date is almost identical. This confirms that the difference in average fertility patterns between the two groups (in Figure A4) is driven by compositional effects.[18]

A simple way to account formally in the regressions for these compositional effects is to interact all variables other than the dummy for positive program exposure with dummies for the number of pregnancies between January 2003 and the baseline survey date. This is done in column (4) of Table 5.[19] The estimated coefficient falls significantly: from -0.015 to -0.0002, i.e. almost an eight-fold fall. The coefficient is still significant, which perhaps should be no surprise given the very large sample size. In all instances, this would point to a small fall in fertility following program participation of around 3% (0.0002/.065). Although there might be ways to rationalize this result as a consequence of program participation, it is likely that this small negative effect is due to our inability to fully account for compositional effects in the regressions. In all cases, even if one takes the results in column (4) at face value, and in the worst case scenario in which all the fall in fertility comes from mothers with low propensity to give birth to low weight children - hence suggesting a potentially confounding selection effect in the estimates in Table 4 - , results in column (4) of Table 5 are still unable to account for the observed fall in the fraction of low birthweight infants, that is on the order of 10 to 20%.

For consistency, in column (8) of Table 4 and in the residual rows of Table 5, column (4), we report estimated program effects on birthweight, other birth outcomes, household income and mother labor supply using the more saturated specification that interacts pregnancies pre-baseline with the other covariates. Results are unchanged, although they are in general less precise, suggesting that compositional changes that appear to drive the fall in fertility among beneficiaries do not drive our results on low birthweight.

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[17] We have no information on number of children ever born, probably a better conditioning variable, for the entire sample as this was not recorded at baseline. Such information is available in the vital statistics but only for mothers who gave birth between 2003 and 2008.

[18] The fraction of women with zero, one or two and more births between January 2003 and the survey date is respectively 0.74, 0.23 and 0.03 for successful applicants and 0.83, 0.16 and 0.1 for unsuccessful applicants.

[19] For simplicity, we interact the baseline number of pregnancies only with time dummies, dummies for time of first payment and the dummy for being a *PANES* household.

6. Conclusions

We have studied the effect of social assistance on the incidence of low birthweight in Uruguay, a middle income country. The program was targeted to households in the bottom 10% of the income distribution, with pre-program formal earnings on the order of US$100 and an incidence of low birthweight of around 10%. The program was mainly composed of a sizeable cash transfer (around 50% of pre-treatment earnings) and a Food card (around 10% of pre-treatment earnings). Although we find evidence of offsetting household labor supply effects, most likely induced by the means tested nature of the program, we show that *PANES* increased beneficiaries' income by around 30%.

The analysis shows considerable improvements in children's birthweight as a result of program participation. Our estimates imply a fall in the incidence of low birthweight on the order of 10% to 20%, allowing beneficiaries to essentially close half of the gap with the rest of the population.

Despite the program initial design, and differently from many other recent social assistance programs in Latin America, participation was in practice unconditional on health checks. Consistent with this, we find positive but modest changes in pre-natal care utilization among beneficiary mothers. We also do not find any significant difference in work involvement between *PANES* and non-*PANES* mothers during pregnancy. This tends to rule out that reduced mother's work involvement - with the associated reduction in physical and perhaps psychological stress - is a relevant channel behind our estimated effects.

We conclude that the cash (and in-kind) transfer components of the program are likely to drive our results, suggesting that unrestricted social assistance has the potential to positively affect birth outcomes, most likely through improved nutrition. Assuming that all the effect of the program was through the transfer, a back of the envelope calculation suggests an elasticity of low birthweight with respect to welfare transfers on the order of around 0.30.

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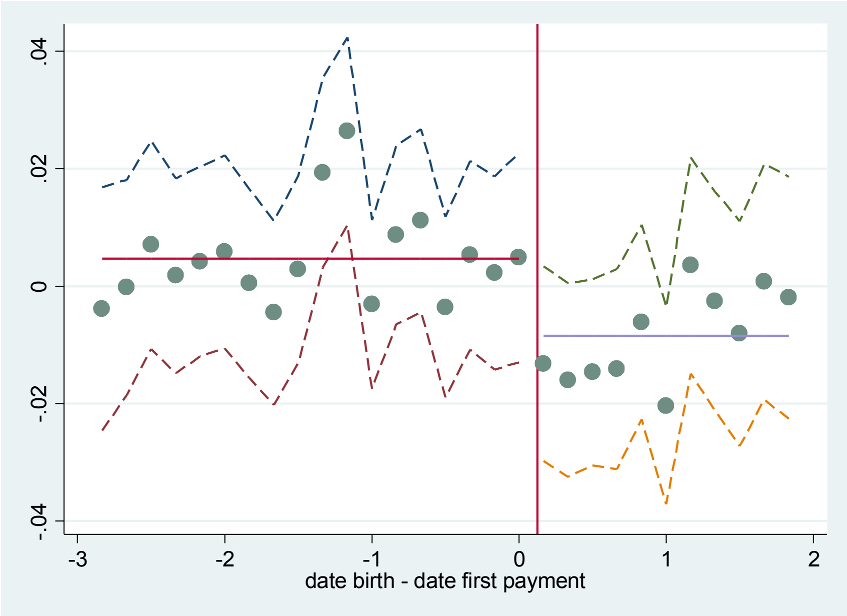
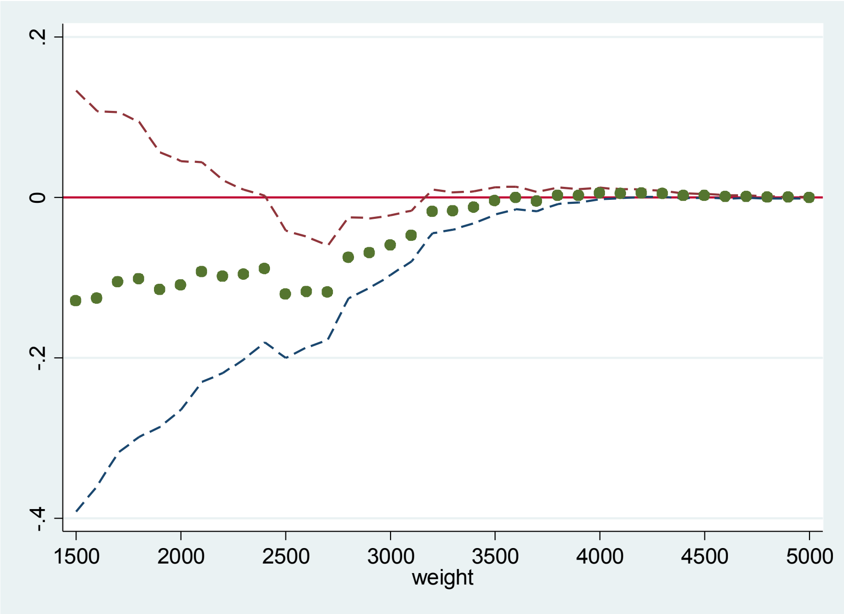
Figure 1: Low Birthweight as a function of time to/since first payment – Eligible households only



Plotted data are residuals of a regression of a dummy for low birthweight on time of birth dummies, a dummy for being a *PANES* eligible household and dummies for time of entry. The figure reports averages of these residuals at subsequent quarters before and after the time of first *PANES* payment (for *PANES* beneficiaries only). A 95% confidence interval around the estimates is also reported. Straight lines are estimated means on each side of the zero cut-off.

Figure 2: Estimated proportional program effects by birthweight



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 regression. Model specification is the same as the one in column (3) of Table 4. 95% confidence intervals around the estimates reported. See also footnotes to table 4.

Table 1: Birth outcomes in selected LAC countries

Country	Infant mortality rate (per 1,000 births)	% Low weight at birth	% Births assisted by health personnel	% with at least one prenatal visit
Uruguay (2002)	**14**	**8**	**99**	**97**
Argentina (1999)	17	7	99	99
Brazil (1996)	28	10	97	97
Chile (2001)	9	5	100	-
Costa Rica (2000)	12	7	-	92
Cuba (2001)	6	6	100	100
Mexico (1999)	22	9	94	-
Peru (1996)	36	11	73	91
Latin America and Caribbean	21	9	96	93

(*) Data refer to 2000.Source: WHO (2009), WHO (2010) and UNICEF (2004).

Table 2: Descriptive statistics - All births: Uruguay 2003-05, by *PANES* status

	PANES Applicants		Non-*PANES* Applicants	All
	Beneficiaries	Non-Beneficiaries		
Weight <2,500g.	0.10	0.09	0.08	0.09
Weight	3144.81	3171.43	3223.03	3200.44
Apgar – 5	9.61	9.60	9.63	9.62
Prenatal visits	6.54	7.64	8.42	7.91
Week of first visit	17.44	16.05	13.88	14.87
Public Health (MSP)	0.83	0.67	0.41	0.52
Birth assisted by doctor	0.49	0.58	0.73	0.66
Average weight in center	3170.15	3187.71	3210.27	3198.95
Weeks gestation	38.49	38.52	38.55	38.53
Out of wedlock	0.80	0.72	0.51	0.60
Mother characteristics				
Age	25.40	24.74	27.58	26.83
Incomplete primary	0.12	0.05	0.03	0.05
Not employed	0.88	0.81	0.55	0.65
# alive children	2.15	1.19	1.02	1.30
Father characteristics				
Missing information	0.37	0.33	0.19	0.24
No schooling	0.12	0.05	0.03	0.04
Not employed	0.11	0.09	0.05	0.05
Observations 2003-2005	31,841	11,469	92,178	135,448
Observations 2003-2007	49,559	19,010	155,957	224,526

The table reports descriptive statistics before the inception of *PANES* for three groups of individuals: successful *PANES* applicants, unsuccessful *PANES* applicants and the rest of the population (non-*PANES* applicants). Source: Uruguayan vital statistics.

Table 3: Correlation between low birthweight and observable characteristics. All births: 2003-2005

	Weight <2,500 g.
Mother's education – completed primary	-0.018***
	(0.002)
- completed secondary	-0.026***
	(0.003)
- completed college	-0.036***
	(0.003)
Out of wedlock	0.012***
	(0.001)
Prenatal visits	-0.005***
	(0.000)
Week first visit	-0.001***
	(0.000)
Number of alive children	-0.005***
	(0.000)
Weeks gestation	-0.073***
	(0.000)
Birth attended by medical personnel	0.025***
	(0.001)

Regressions include: a dummy for child's sex, dummies for mother's age plus date of birth dummies. Standard errors clustered by mother. See also notes to Table 2.

Table 4: Program effects on birth outcomes 2003-2007

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1. Low birthweight	-0.010**	-0.014***	-0.020*	-0.013***	-0.012*	-0.013**	-0.012*	-0.009*
	(0.004)	(0.004)	(0.011)	(0.005)	(0.006)	(0.005)	(0.006)	(0.005)
2. Birthweight	12.739	16.910**	22.045	10.716	9.513	10.453	9.421	7.223
	(8.421)	(8.420)	(18.550)	(9.912)	(12.370)	(9.913)	(12.370)	(10.056)
3. Weeks gestation	0.043	0.035	0.044	0.043	0.044	0.041	0.044	0.033
	(0.030)	(0.031)	(0.080)	(0.037)	(0.046)	(0.037)	(0.046)	(0.037)
4. Prenatal visits	0.105**	0.116**	0.143	0.109*	0.095	0.107*	0.095	-0.017
	(0.049)	(0.050)	(0.118)	(0.059)	(0.074)	(0.059)	(0.074)	(0.061)
5. Week first visit	-0.045	-0.046	-0.352	-0.055	-0.038	-0.055	-0.038	0.193
	(0.109)	(0.114)	(0.320)	(0.133)	(0.164)	(0.133)	(0.164)	(0.140)
Additional controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mother fixed effects	No	No	Yes	No	No	No	No	Yes
Linear score	No	No	No	Yes	Yes	Yes	Yes	No
Quadratic score	No	No	No	No	No	Yes	Yes	No
Controls for births 2003/01 – baseline survey	No	No	No	No	No	No	No	Yes
Sample	All	All	All	[-.2, .2]	[-.1, .1]	[-.2, .2]	[-.1, .1]	All

Entries are coefficients on a dummy for exposure to *PANES* greater than one quarter. Each cell refers to a separate regression. All regression control for time dummies and a dummy for *PANES* eligibility. Additional controls include dummies for mother's age, sex of the child, a dummy for multiple pregnancy, dummies for number of mother's previous pregnancies and dummies for time of entry into the program. Specifications in the last column control for the interaction of the number of births between January 2003 and the baseline survey date with date of birth and date of first payment. Standard errors clustered by mother. ***, **, *: significant at 1%, 5% and 10% level.

Table 5: Program effects on income, labor supply and fertility

	(1)	(2)	(3)	(4)
Income transfer	1,106.166***	1,000.740***	1,118.232***	836.299***
	(83.143)	(7.348)	(47.045)	(8.310)
1. Food card	224.426***	206.277***	235.729***	189.979***
	(34.990)	(2.290)	(20.331)	(2.565)
2. Household earnigns	-329.593*	-150.399***	-307.107	-257.188***
	(176.440)	(41.317)	(204.941)	(44.466)
3. Household total income	763.762***	786.098***	916.306***	628.400***
	(177.849)	(45.090)	(250.849)	(47.863)
4. Mother's earnings	-54.471**	-25.167**	-27.352	-18.640
	(21.424)	(12.525)	(72.153)	(13.253)
5. % months in work	-0.010*	-0.000	-0.002	0.000
	(0.006)	(0.004)	(0.021)	(0.004)
6. Chid born alive	-.0012***	-.0016***	-0.0018***	-0.0002***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Additional controls	No	Yes	Yes	Yes
Mother fixed effects	No	No	Yes	No
Linear score	No	No	No	Yes
Quadratic score	No	No	No	Yes
Controls for births 2003/01 – baseline survey	No	No	No	Yes
Sample	All	All	All	All

All specifications in row 6 include time dummies rather than time of birth dummies as in rows 1 to 5. Additional controls in row 6 are: time of first payment and mother's age dummies. Number of observations in row 6 is 10,463,940. Data in rows 1-5 refer to the period March 2004 to December 2007. See also notes to Table 4.

Figure A1: Timing of *PANES* applications processing

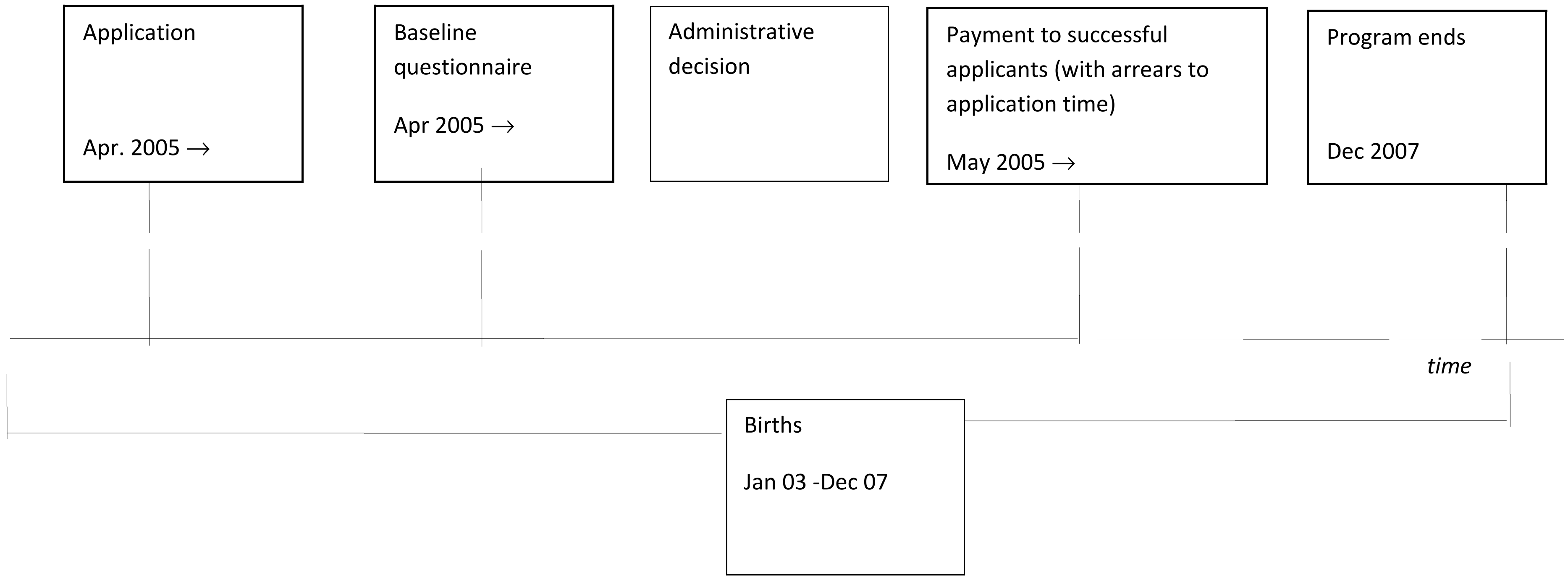
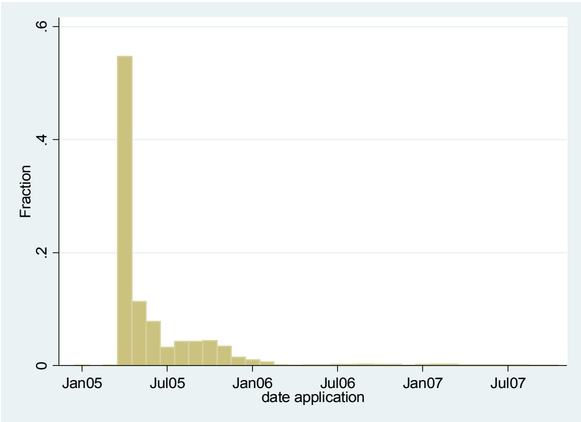
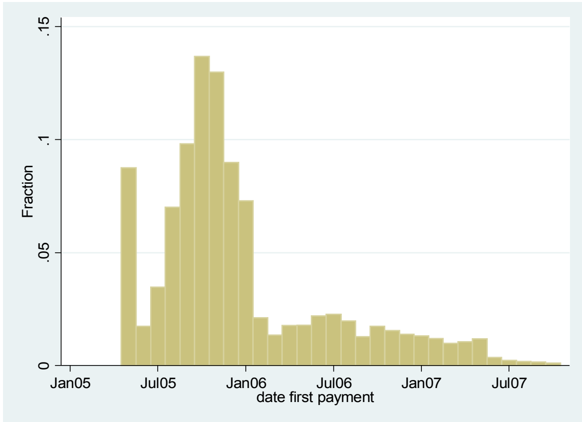


Figure A2: Distribution of *PANES* program dates

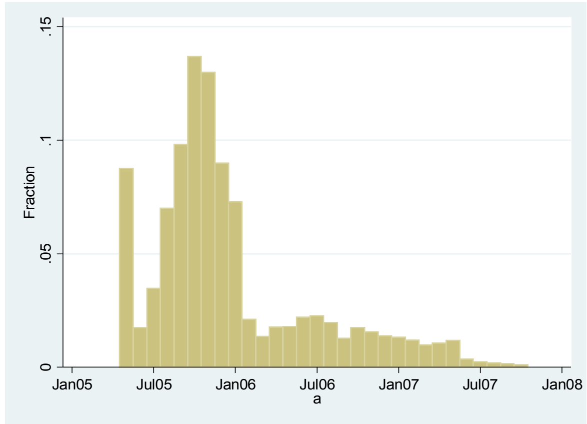
Time of application



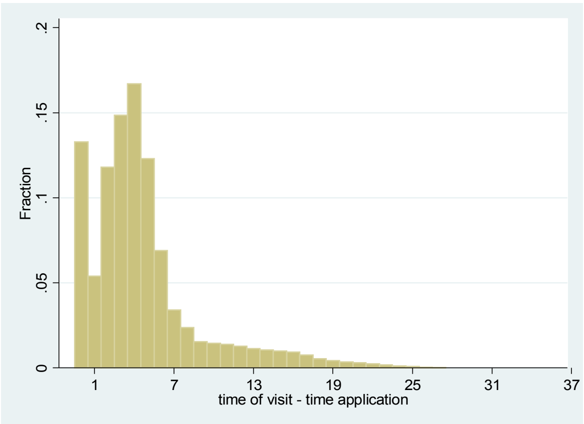
Time of survey



Time of first payment



Time of survey - time application



Time of first payment - time survey

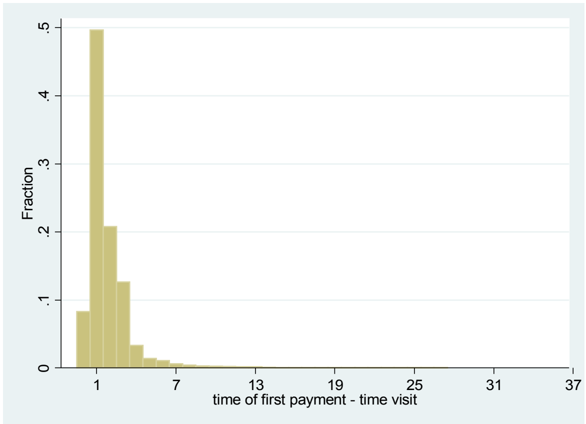


Figure A3. Fertility rates by as a function of time to/since first payment – *PANES* eligible women only

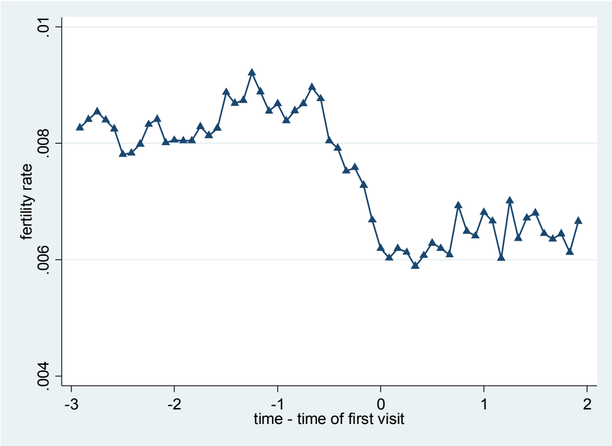


Figure A4. Fertility rates by *PANES* eligibility status as a function of time to/since baseline survey

Eligible women Ineligible women

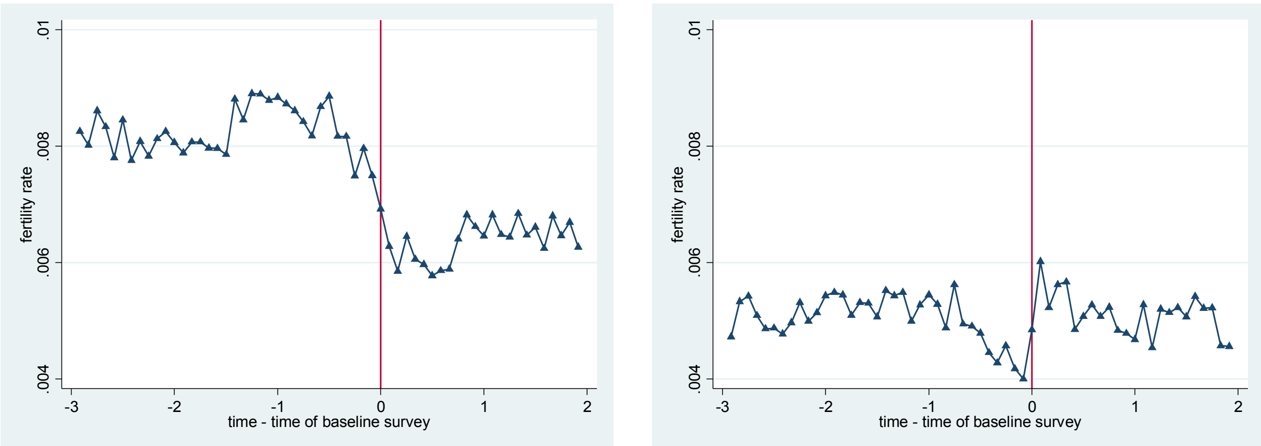


Figure A5. Fertility rates by number of children born by baseline survey date and *PANES* eligibility status as a function of time to/since baseline survey