Conditional Cash Transfers and Infant Health: Evidence from the Uruguayan *PANES*

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This paper estimates the impact of a large temporary anti-poverty program - the Uruguayan *PANES* - on outcomes at birth. Using the differential incorporation of households into the program and longitudinal Vital Statistics data, we find a significant reduction in the proportion of infants with moderately low birth weight (up to 3 kg).The program closed between 25% and 75% of the gap between poor and non-poor households. We show that the program increased prenatal care, potentially due to the perceived built-in conditionalities, although we are unable to say if and to what extent this is the (only) channel behind the estimated effects.

This paper provides preliminary estimates and a discussion of the impact of a large temporary anti-poverty program - the Uruguayan *PANES* - on birth outcomes. The analysis is based on matched administrative data from *PANES* and vital statistics. In order to identify the effect of the program, we restrict to households who eventually benefitted from the program and had at least one birth over the period of observation, To identify the effect of *PANES* we exploit the gradual incorporation of households into the program and we compare outcomes of children of the same mother using a mother fixed effect approach.

Section 1 reviews the literature about the socioeconomic determinants and consequences of infants' health. Section 2 describes the program. Section 3 presents the data and summary statistics. Section 4 presents the empirical model. Section 5 estimates the models based on vital statistics and section 6 presents some preliminary conclusions and directions for future research.

1. Infants' health: determinants, outcomes and the role of policies

In this section we review the literature about the determinants of child's health, and in particular birth weight, and its importance for future child outcomes. We start by summarizing the evidence on the effect of different parental characteristics (both socioeconomic and biological) and the effect of policies on child's health. Later on, we present evidence relating infants' health to subsequent educational and labor market outcomes.

1.a Determinants

Existing empirical analyses of the determinants of child's heath in the economics literature are rooted in Becker (1960) and Becker and Lewis (1973) seminal model that sees a child's health as a good in the household utility function, potentially mediated via a health production function (Wolfe and Behrman, 1982; Rosenzweig and Schultz, 1983; Thomas and Strauss, 1992; Thomas et al., 1996, among many others; for an up-to-date review see Currie, 2009). Parents might care about their children's well being either because they derive a direct utility from it or because child's health is an important predictor of earnings in adult life and parents expect to share part of these earnings.

One obvious implication of this model is that - at fixed number of children - wealthier families will be able to purchase better inputs, such as better quality medical care and food, better housing, safer neighborhoods etc. Changes in the prices of health inputs will also affect their demand. If children’s health is seen as a productive investment (as opposed to consumption good), this should not be affected by parental resources unless households are credit constrained.

Existing studies vary widely in the data, the identification assumptions and the measures of health (anthropometric measures, morbidity, survival, weight at birth) used. It appears that parental education, and especially mother's education, shows a significant and positive association with child's health. The same happens with other variables reflecting household economic status (Currie, 2009), although results here seem less robust, and of course this does not necessarily imply causality.

One of the main components of child health and determinant of future health condition is weight at birth. Low weight at birth is an important predictor of neonatal morbidity and mortality.[1] It can arise because of preterm delivery or because of low fetal growth. The medical literature emphasizes that mother's health is in turn a major determinant of the incidence of low weight at birth. Maternal under-nutrition, anemia, malaria and acute and chronic infections contribute to higher risks of intrauterine growth retardation and low weight at birth in developing countries, whereas the most important risk factors in developed countries are cigarette smoking and pre-eclampsia (Kramer, 1987).

Attempts to link weight at birth and socio economic status in the economic literature have to face an obvious problem of endogeneity. Some papers have attempted to face that problem for the specific outcome of birth weight, mainly through the estimation of sibling fixed effects. Conley and Bennett (2000) find that income during pregnancy has no effect on the risk of low birth weight when mother's birth weight or family fixed effects are included in the model. Currie and Moretti (2003) find that higher maternal education improves infants' outcomes. These effects arise because education affects maternal behavior (for example, by reducing smoking), it increases earnings, it improves women's marriage markets and it reduces fertility.

In the medical literature the effects of the provision of food supplements to pregnant women have been modest in enhancing fetal growth. A review of 13 programs presented in Kramer (2002) shows that balanced energy/protein supplementation has a modest effect on mean birth weight (weight mean difference of 25 grams), but the effect on the reduction of intrauterine growth restriction (IUGR) is somehow bigger. The studies reviewed include Colombia, Indonesia, Taiwan and Gambia. In this last country, where the supplement provided an additional 900 kcal per day (compared to 200 or 250 in other trials), the magnitude of the

[1] Low weight at birth is usually defined as weight under 2500 grams.

birth weight increase was higher (136 g). Supplementation was not associated with an increase in mean gestational age or a reduction in preterm birth. The author also reports that four trials encompassing nutritional advice to women lead to an increase in dietary intake, but had no effects on mean birth weight or IUGR.

Studies from developed countries have indicated an association between the use of antibiotics during pregnancy and a reduction in low birth weight, although some caution is needed in interpreting these results, as some of the evidence comes from trials of questionable methodological quality.

An association is also found with the number of previous offspring - the first child having a higher probability of low weight - and with birth spacing. Medical studies also find that extreme maternal age, both high and low, are associated with low birth weight. Obstetric history, including primiparity, multiple pregnancy and previous miscarriages have also been found to be associated to low birth weight (Bailey and Barion, 2007).

Prenatal care is often included among the inputs in the birth weight production function. In some studies, prenatal care use is defined as the delay (in months) until the first medical visit since the time of conception, while other studies prefer to capture potential nonlinear effects through a measure like the Kessner index.[2] Quantitative indexes however do not provide information about the quality of prenatal care, and moreover, they may be misleading in some cases. On the one hand, women who deliver prematurely mechanically undergo fewer controls than those who deliver at full term. On the other hand, a number of visits greater than normal is usually indicative of a high risk pregnancy, implying reverse causality. Finally, problems associated with the measurement of this variable, especially in official birth certificates, are common.

Besides the measurement issues, the interpretation of the association between these variables is complex, as unobserved characteristics of women who receive adequate prenatal care, rather than parental care itself, might increase the chances of normal weight at birth. The existence of adverse selection in the use of prenatal care is a major problem that empirical studies have to face.

Some studies provide evidence of a positive association between prenatal care and birth weight. Rosenzweig and Schultz (1983), for example using information from a natality survey, find that a delay in seeking prenatal care reduces weight and gestational age at birth. Along the

[2] The Kessner index considers as correct prenatal care starting medical care at the first quarter and attending nine or more controls in the whole pregnancy period. Less than nine medical visits or starting controls in the third quarter are considered as incorrect prenatal care, whereas the rest of situations constitute intermediate prenatal care (Kotelchuk, 1994). The index can be corrected to consider the duration of pregnancy.

same lines, Liu (1998) finds that prenatal care is a significant determinant of birth weight, with effects ranging from 36 to 107 grams for each additional month of delay in care initiation. Although many authors have established the existence of a positive association, other papers in the literature indicate that this evidence is not clear or consistent (Kramer, 1987; Huntington and Connell, 1994). In general, studies based on survey data that try to control for selectivity bias find smaller effects of prenatal care on low birth weight than clinical studies.

In relation to specific interventions to increase prenatal care, the literature is scarce. In 1985, a report from the Institute of Medicine on Preventing Low Birthweight identified problems of access to prenatal care as being of central importance for the design of public policies aimed at improving birth weight in the US. More recently, evaluations of the Medicaid expansion for pregnant women in the US suggest that this large policy initiative reduced infant mortality and low birth weight (the first effect being stronger), although the success of the expansion of the program was limited (Currie and Gruber, 1994).

In any case, the mechanisms underlying the positive relationship between prenatal care and birth weight are difficult to identify. Most studies argue that routine prenatal care is an opportunity for the identification of problems and risks, as well as an opportunity for education on best practices during pregnancy.

There is also considerable and growing evidence on the effect of other policy interventions, especially antipoverty programs, on infant early outcomes and birth weight. Currie (2006) analyses different policies that affect children health in the U.S. (Food Stamps, WIC - a feeding program for women, infant and children - , school lunch programs, school breakfast programs, etc.) and argues that, while these in kind programs have effectively attacked the consequences of poverty for children, there is relatively little evidence that modest cash transfers have large effects on children's outcomes. A more positive picture comes from the recent study by Almond et al. (2009) who find sizeable effects of the expansion of the U.S. Food Stamp Program during the 1960s and early 1970s on low birth weight, especially among African Americans.

During recent years many developing countries have implemented conditional cash transfer programs (CCTs) that aim, among other objectives, at improving nutritional and health conditions of more deprived population.

Most impact evaluations of CCTs on child health have concentrated on changes in household consumption, stunting and health controls. There is a growing body of evidence that indicates that in some cases CCTs have improved child health and nutritional outcomes. Basset

(2008), Leroy et al. (2009) and Gaardner et al. (2009) review impact evaluation studies in health outcomes in a set of Latin American programs that included interventions and conditionalities in relation to health outcomes.[3] They conclude that existing impact evaluations provide evidence that cash transfers, accompanied by the set of complementary components, improve children's nutritional status, particularly the incidence of stunting. Nevertheless, due to the multiplicity of interventions involved, it is not easy to understand what component, or combination of components, explain the impacts of these programs (Gaarder et al., 2009).

Fiszbein and Schady (2009) also analyze the effects of CCTs on health outcomes. They argue that CCTs programs have a positive effect in the use of preventive health services, although the effect is less clear-cut than the one on school enrollment. According to these authors, CCTs have contributed to a substantial reduction in preexisting differences in access to health. Nevertheless, they also point out that final effects on health outcomes appear modest. Some studies show positive outcomes, especially related to a child height or weight for height

Evidence regarding the impact of CCTs on cognitive development in early childhood is encouraging (Macours, Schady and Vakis, 2008; Paxson and Schady, 2008). Very early interventions may produce larger payoffs associated to future gains in terms of future school enrollment and progress.

Specific evidence on the impact of CCTs on weight at birth is scarce. Barber and Gertler (2008) is the first paper to document the impact of CCTs on birth weight. They evaluate the impact of *Progresa/Oportunidades* on birth weight using a randomized trial across communities, based on a sample of 840 women who experienced a live birth between 1997 and 2003, and reported valid birth weight. They regress birth weight on a binary variable indicating if the woman was a beneficiary at any time before delivering her most recent live birth. Once they include individual, household and community covariates (including, in some specifications community fixed effecters), their results indicate that CCTs have a very pronounced effect on weight at birth. They find that birth weight of beneficiaries' infants was 127.3 grams higher than controls (corresponding to a 4.1 percent of increase in mean birth weight) and that the incidence of low birth weight was 44.5 percent lower.

To investigate the channels behind the estimated effect, the author's measure the importance of nutritional supplements and food purchased based on the household exposure to the program. They argue that the number of program months is exogenous because the actual timing of incorporation to the program was random, and a previous study did not find any effect

[3] They summarize evidence from impact evaluation studies for Mexico, Colombia, Nicaragua, Honduras and Brazil.

of the program on fertility (Steklov *et al*, 2006). The longer someone has been on the program, they argue, the more food they have been able to purchase and the longer they could have benefited from supplements. They also explore the impact of the health care quality on birth weight, using an instrumental variables approach. Results indicate that the program effects on birth weight largely operated through improved service quality. This increase in the quality of health was not due to supply side improvements, so their remaining hypothesis is that it was due to the action of women demanding better services. Different activities included in the program could have improved women's capabilities to take actions that positively affected the health and welfare of their families.

1.b Subsequent outcomes

The subsequent impacts of low weight at birth have also been empirically analyzed in both the medical and economic literature.

Medical studies indicate that low birth weight is associated with increased risk of neonatal death and cardiovascular diseases, diabetes and hypertension later in life (Barker, 1995). Many of the children who survive low birth weight suffer from cognitive and neurological impairment and show nutritional disorders as adolescents and adults (Branca and Ferrari, 2002).

The importance of health and nutritional status during early years in life for the development of mental, emotional and physical skills has been stressed by researchers and practitioners from different disciplines, including economists. Several studies document association between nutritional status and early outcomes, including weight at birth, and subsequent attainment. In effect, early childhood nutrition is found to have important effects on later education and labor market outcomes (Alderman et al., 2006; Behrman, 1996; Behrman and Levy, 1998; Glewwe and Jacoby, 1995; Glewwe et al., 1999; Glewwe and King, 2001, Currie and Hyson, 2001; Currie, 2000; Case et al., 2005, Garces et al., 2002; among others). Again, the crucial question is whether birth weight is itself a determinant of later outcomes or it simply reflects other hard to measure characteristics.

In recent years, empirical studies have tried to exploit within twin variation to isolate the effects of birth weight. Examples of this literature are Conley et al. (2006), Almond et al. (2005), Black et al. (2007), Behrman and Rosenzweig (2004). These studies tend to find significant and negative effects of low weight at birth on health and other outcomes, although the magnitudes are generally smaller than previously thought.

For Latin America, perhaps the most cited study is Maluccio et al. (2006). This study investigates the long term impact of a randomized community level health and nutritional intervention in rural Guatemala that took place between 1969 and 1977. They link information collected in the 70s on individuals exposed to the program when they were 0 to 15 years old, with data on those individuals in 2002-2004. Their results indicate significantly positive effects of the nutritional intervention on many outcomes: increased grade attainment and speedier school progression for women, higher scores on cognitive tests for both men and women, and higher scores on educational achievement tests for both men and women.

If low birth weight is associated with worse educational, economic and other outcomes, there are potential economic gains from reducing its incidence (Behrman, 1993; Alderman and Behrman, 2004; Almond et al, 2008). A bulk of literature has recently argued that early childhood interventions have higher rates of return than later interventions. The main argument of this line of thought, championed by Heckman (1995; 2000), is that cognitive ability is more malleable early in the life cycle and that early childhood interventions might be more effective due to their benefits extending over a longer time and the potential complementarities with other inputs. Heckman and Masterov (2007) summarize evidence from a variety of early childhood programs targeted towards disadvantaged children and suggest that these interventions reduce crime, promote high school graduation and college attendance, reduce grade repetition and education costs, help prevent teenage births and raise achievements as measured by test scores.

Currie and Moretti (2007) find evidence of intergenerational transmission of low birth weight based on data from California. That children's malnutrition gets compounded across generations suggests that the benefits of reducing a generation's low birth weight might be higher than what originally assumed. Almond et al. (2005) emphasize that there might be social costs of low birth weight, as children this category impose a substantial financial burden on the health system, providing another rationale for policy intervention.

2. The intervention: *PANES*

2.a Background

Uruguay is a small middle income Latin America country home to 3.3 million individuals, 88% living in urban areas. The country is 50^{th} in the Human Development Index ranking third among the countries in the continent (after Chile and Argentina). Its poverty and inequality indexes are among the lowest in the region and PPP-adjusted annual per capita income is currently just below US$10,000.

The country experienced a rapid economic growth in the first decades of the twentieth century and was one of the first countries in the region to implement universal primary education and establish a universal old age pension system.

Economic growth stagnated in the second half of the twentieth century and the country went through a severe economic crisis at the start of this decade. Per capita income fell 11.4% between 2001 and 2002, unemployment reached its highest level in twenty years (at 17%), the exchange rate collapsed, and a financial crisis led to bank runs, while the poverty rate doubled over the course of four years (1999-2003).

The crisis laid bare the weakness of the existing social safety net, which was largely focused on transfers to the elderly population. In 2002, total expenditure on elderly pensions represented 65% of all government social expenditures and almost 13% of GDP. This was reflected in marked differences in poverty incidence by age, with nearly half of children under age five living in poverty compared to 8% for the over 65 (UNDP, 2008).

Infant mortality is low (1.1%) in Uruguay compared to the regional average, but it is high compared to other middle income Latin American countries such as Costa Rica, Chile and Cuba (0.1, 0.72 and 0.5% respectively). Although gains have been made in the last twenty years, the reduction has been lower than in other countries in the region starting from similar levels.

Indicators on risk factors related to infant mortality such as low weight at birth, births assisted by health personnel and prenatal controls show a good performance relative to the Latin American average (Table 1). Jewell and Triunfo (2008) find that low weight at birth (less than 2,500 g.) and infant mortality (particularly post-neonatal) in Uruguayan public hospitals are associated with low maternal schooling, maternal controls, first pregnancy and age of the mother. Matijasevich et al (2004) study a sample of newborns at public hospitals in Uruguay and find substantial heterogeneity among low weight children. They find that premature births only explain extremely low birth weight (less than 1,500 g) while it is reduced intrauterine-growth that explains the incidence of moderate low birth weight (1,500 to 2,500 g). Being a smoking mother is also a good predictor of low birth at weight while previous abortions and miscarriages and pre-eclampsia are good predictors of very low weight at birth. Nutritional supplementation for mothers that are not severely undernourished also proved to be ineffective to overcome low weight incidence.

2.b Program

Following the national elections of November 2004, in March 2005 a centre left (*Frente-Amplio*) administration took power for the first time in Uruguay. The government swiftly moved to create a new Ministry for Social Development (*Ministerio de Desarrollo Social, MIDES*) and design and implement the *Plan de Atención Nacional a la Emergencia Social* (*PANES*), one of pillars of its electoral campaign.

PANES was a temporary antipoverty program, running from April 2005 to December 2007. The temporary nature of the program and the precise closing date were announced since the beginning.[4] The program was replaced in January 2008 by a comprehensive system of family allowances and a health care reform accompanied by a major overhaul of the tax system and other minor policy interventions (*Plan de Equidad*).

The aims of *PANES* were to provide direct assistance to households which had experienced a worsening in living standards during the economic crisis of the early 2000s, and to strengthen the human and social capital of the poor.

The target population consisted of households below the bottom quintile among those below the national poverty line, with 95% of participant households having at least one child. In all, around 102,000 households became program beneficiaries, approximately 10% of all households (and 14% of the population) in the country. The total cost of the program - that was financed by internal resources - was US$247,657,026, i.e., US$2,428 per beneficiary household. On an annual basis, this represents 0.41% of GDP and 1.95% of government social expenditures.

2.c Enrollment and eligibility

Enrollment occurred in two phases. All low income households were publicly invited to apply. The application form (F1) recorded the name, sex, age, nationality and ID number of all household members and self-reported per capita income. Concurrently, the government made a large outreach effort, sending enumerators to poor communities in an attempt to boost applications and ensure program take-up among the most marginalized. Application was accepted for the entire period of life of the program and rejected households could then reapply.

[4] Indeed, data from the first round a small up survey (carried out from December 2006 to March 2007), show that 61% of beneficiaries knew that the program will come to an end, 37% did not know and only 2% believed it would not finish.

The program was means tested: only households with a level of per-capita income below approximately US$50 per month were eligible and were subsequently visited.[5] The income condition disqualified around 10% of initial applicants.

Eventually, 188,671 applicant households were visited by Ministry of Social Development personnel and administered a detailed baseline survey (F2).

The F2 is very similar to a typical household survey, collecting information on demographic and socioeconomic characteristics of individuals (age, sex, access to health insurance, education and schooling, labor market participation, income), possession of durables and housing conditions.

Among visited households, assignment to *PANES* was determined using a predicted income score that depended on household socioeconomic characteristics collected in the baseline survey (F2).[6] Only households with a score below a predetermined level were assigned to the program.[7] The decision of using a predicted score rather than income itself was driven by a number of factors. First, many households in the objective population had highly unstable income flows, so current income was seen as a bad proxy for permanent income. Second, because the target population was often employed in the informal sector, it was difficult to verify their reported income against Social Security records, opening up the possibility of misreporting. By using a wide array of socioeconomic characteristics, as opposed to self-reported income, the government also hoped to minimize strategic misreporting. Existing evidence shows a successful targeting relative to most Latin American cash transfer programs (World Bank, 2007).

Similar to other Latin American CCT programs, *PANES* participation was in principle conditional on children's school attendance and health checks. Children aged 6 to 14 years old

[5] Per capita income was computed as the greatest between Social Security income (excluding non contributory benefits, i.e. child allowances and non contributory pensions) and self-declared income.

[6] The income score was devised by researchers at the University of the Republic (Amarante et al., 2005), including some of the authors of this paper, and was based on a probit model of the likelihood of being above a critical per capita income level, using a highly saturated function of household variables (household structure, 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 a 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 then used to predict an income 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.

[7] The eligibility thresholds were allowed to vary across the country's five main administrative regions. The regional thresholds were set to entitle similar shares of poor households in each area to the program. The regions are: Montevideo, North (Artigas, Salto, Rivera), Center-North (Paysandú, Río Negro, Tacuarembó, Durazno, Treinta y Tres, Cerro Largo), Center-South (Soriano, Florida, Flores, Lavalleja, Rocha) and South (Colonia, San José, Canelones, Maldonado).

were expected to be enrolled and attend regularly school, pregnant women had to attend to monthly prenatal controls (plus weekly controls starting from week thirty-six) and three mandatory ecographies, while children aged 0 to 5 were supposed to comply with the mandatory paediatric checks and vaccinations prescribed by the Ministry of Health. Due to scarce inter-institutional coordination, conditionalities though were *de facto* not enforced, an issue publicly acknowledged by *MIDES* after the end of the program.

2.d Program ingredients

PANES was a conjoint of social policies, the most widespread element 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) independent of household size, amounting to approximately 50% of average pre-program household self-reported income. Together with the first payment, households received arrears dating back to the date of initial application. Since there was an administrative backlog in processing applications, the average waiting time was on the order of 8 months, implying that in the first month of actual program participation, households received on average US$440 dollars, a considerable amount of money given their resources. Overall, 97% of *PANES* beneficiaries received this transfer.[8]

Households with children or pregnant women were also entitled to a food card (*tarjeta alimentaria*), an in-kind transfer that operated through an electronic debit card whose monthly value varied between US$13 and US$30, depending on the number of children and pregnant women in the household. The food card allowed households to purchase food and goods for personal hygiene in participating stores. 70% of beneficiary households eventually received the food card. This component was launched in the first semester of 2006, and its expansion was gradual, as participating stores had to be endowed with electronic readers in order to accept electronic card payments. This explains the lower coverage of this component in relation to the cash transfer, despite most *PANES* households having at least one child aged 0 to 18 among its members.[9]

Rutas de Salida (Exit Routes) was conceived as a component to foster social inclusion by recovering the lost work habits of participants, promoting knowledge of rights and strengthening social ties through training and educational activities and meetings. In some cases, good health and nutrition practices were included. These activities were subcontracted to

[8] Not all *PANES* beneficiaries received *Ingreso Ciudadano*. For instance, homeless persons who were all included in *PANES*, only received the income transfer if their income was below the threshold set by *MIDES*.

[9] Prior to scaling up of the program, food baskets were distributed to all households (whether *PANES* or not) whose children attended schools with a high proportion of *PANES* beneficiaries.

local NGOs. Because of this, there is substantial heterogeneity (and no systematic information available) about the actual activities implemented. This, together with the circumstance that attendance was not monitored and that no specific rule on which household member was meant to attend, make it difficult to assess its potential effects. Although originally conceived as a conditionality of the program, eventually only 15% of *PANES* households took part in these activities, largely due to supply constraints.

PANES also included a workfare program, *Trabajo por Uruguay* (Work for Uruguay). Households could volunteer to participate, and among applicants, assignment was determined on a random basis. Participants worked six hours a day during a four months period in exchange for salary twice the amount the *Ingreso Ciudadano*. These activities were mainly organized in agreement with public institutions and included gardening in public parks, weeding of railways, cleaning and painting schools, repairing streets, etc. By December 2007 17% of households had benefited from this component. As a way to increase beneficiaries' employability, health interventions, such as dental care and prostheses, and standard health tests (smear test, prenatal controls, mammography for women and prostate exam for men) at Public health clinics were offered.[10]

2.e Postulated effects on birth outcomes

The multiplicity of *PANES* ingredients might have exerted both direct and indirect effects on infants' outcomes.

A first effect could be due temporary changes in household income due to the income transfer that could directly affect mother and fetal nutrition. This effect could be enhanced by the food card. To the extent that the food card was not completely fungible for money and that household food expenditure in the absence of treatment would have been below the value of the card, this component could directly increase food expenditure. If the increase in food expenditure translated into an increase in food quality, as found in impact evaluations for Mexico, Colombia and Nicaragua (Fiszbein and Schady, 2009), this may have an additional

[10] Additional but substantially less common components included health care subsidies, an agreement with the Cuban government to carry out cataract eye surgery, financial support for housing investment and help to the homeless. Concurrently, an inter-sectoral agreement between the Ministry of Social Development and the Ministry of Health led to a substantial refinancing of Public health system. Since the last months of 2006, a reform of the health care system has also been introduced. The *System Integrado de Salud* was created with the objective of expanding access of the Uruguayan population to health care and improving its quality. A new public health care insurance scheme was introduced that mainly consisted in extended coverage to the all household members of workers contributing to social security. Until then, many public sector workers did not have access to health care, and private health insurance only covered workers and not their families. Specific targets for improvement were set for each health centre (public or private). Again, this reform is likely to have affected both *PANES* and non *PANES* beneficiaries, provided their contributory status was not affected by program participation.

effect on health outcomes. This is particularly relevant as most beneficiaries were women Given that women had presumably financial control over the transfer, one might expect this to lead to an improvement in their infants' outcomes though improved in uterus feeding.[11]

A second major effect refers to the demand for health services of pregnant women. Despite poor monitoring and lack of enforcement of the conditionalities, most households might have perceived these checks as prerequisites for program receipt.

A third effect could be exerted through modifications in preferences and behavior resulting from educational activities and meetings, as well from promotional campaigns (and direct health interventions for beneficiaries of *Trabajo por Uruguay)*. These could affect women's attitudes towards health care during pregnancy, including nutritional practices and attendance to prenatal controls and medical checkups.

A fourth channel pertains to women's empowerment. As said, most program recipients were women. The transfer might have affected their bargaining power in the household. If - as often argued - women have stronger preferences for their children's well-being compared to men, one might expect this to increase the well being of the fetus.

A fifth potential channel attains to the possibility that households were facilitated in their access to the Public Health system as a result of program participation. Although access to Public health is universal in Uruguay, this is fee based for households with sufficiently high income and those without a Health Card (*Carne de Asistencia*). Anecdotal evidence suggests that Program participants were facilitated in obtaining a Health Card and potentially in booking appointments within the Public health system.

Although it is hard to establish what channels might be at work in the case of pregnant women, it is important to summarize preliminary evidence based on two small (approximately 3,000 households) follow up surveys of the program run in 2007 and 2008.

First, existing evidence (Amarante et al., 2008, 2009) on the effect of *PANES* shows an increase in disposable income, although there is also some evidence of the *ex-post* increase being lower than what predicted *ex-ante*, due to the income effect of the transfer or - most likely - to the substitution effect induced by the income test.

There is also evidence of increased health care utilization. Consistent with what established by the program, an increase in vaccinations and health checks among beneficiaries is found.

[11] Existing evidence shows no effect *PANES* on child school attendance and adult labor supply (Amarante et al., 2008). Some effects on knowledge of labor rights are found in the follow up survey (Amarante et al., 2008 and Amarante et al., 2009). A significant increase in government support and subjective well-being is also found (Manacorda et al., 2009).

The 2007 follow up asked beneficiaries about knowledge of conditionalities: 42% of households did not know that conditionalities were attached to the program, 24% were aware that school attendance was a condition, 20% mentioned paediatric controls, and 17% mentioned participation in *Rutas de Salida*, while only 11% referred to gynaecological (including prenatal) controls and 23% mentioned other conditionalities, including work or helping others. This is evidence against households' clear understanding of the conditionalities.

Finally, the follow up survey shows little evidence of increased women's empowerment.

In sum, it appears that, despite households being overall unaware of the program conditionalities and despite there being little evidence of increased women's empowerment, *PANES* increased income and access to the public health system, potentially through increased awareness and the mechanical effect of endowing these households with a Health card.

3. Data

For the purpose of this research we combine data from the *PANES* administrative records with Uruguayan vital statistics micro data.

3a. Program data

Administrative program data include baseline (i.e. at the time of the visit, F2) socio-demographic characteristics, housing conditions, income, labor market participation and schooling and durable possession for both successful and unsuccessful applicants.[12] For each individual, these data also provide the unique national identification number (*cedula*). Although about 15% of individuals do not report a national identification number, these are almost exclusively children.

The database contains information on around 700,000 individuals from approximately 190,000 households, more than half eventually beneficiaries. In the case of repeated records due to individuals or entire households reapplying, we take baseline characteristics at the time of first application. However, for those who reapply and are eventually admitted to the program, we take baseline information at the time of the first successful application.

3.b Vital statistics

[12] Additional program data - that we do not use in this first version of the paper - provide information on receipt of *PANES* benefits month by month for each beneficiary households. We also have month by month social security data on formal earnings, employment and transfers for all eligible and ineligible applicants from January 2004 to December 2008.

Vital statistics data provide information relating to all registered births of born alive children.[13] These data are available every year from 2003 to 2007. According to recent estimates and contrary to what happens in other courtiers in the region, vital statistics data constitutes an extremely reliable data source, as 98% of Uruguayan births are registered (Cabella and Peri, 2005. Uruguay presents the highest level of registered births in Latin America (see UNICEF, 2005; Duryea *et al,* 2006).[14]

The data are collected by the Service for Population Information (*Dirección de Servicios de Informaciónn Poblacional, DSIP*) at the Ministry of Health. For each newborn, a certificate is filled by a physician at the health centre and a copy is sent to *DSIP* for digitalization. The certificate gathers a wide set of variables on the circumstances of birth, parental characteristics and the reproductive trajectory of the mother.

The confidential version of the data that we have been provided also includes the mother's *cedula*. This allows us to uniquely link the vital statistics micro data to the program data.

The data are summarized in Table 2 that reports averages over the entire period. Here we report information for three groups of mothers: those in *PANES* beneficiary households (column 1), those in households that unsuccessfully applied to the program (column 2), and those in households that did not apply. Roughly speaking these three groups correspond to increasingly higher levels of income and socio-demographic status. Notice that *PANES* births account for more than 20% of all births. As *PANES* households represent around 10% of the population, this is clearly the result of *PANES* mothers having higher fertility.

The first group of variables refers to children's outcomes at birth. In addition to weight at birth, the data include the *APGAR* score at one and five minutes from birth. This score ranks from 0 to 10 and measures five dimensions: breathing effort, pulse, skin color, muscle tone and reflex irritability (response to stimulus). Each item is evaluated on a scale 0 to 2. *APGAR* scores between 8 and 10 are normal, between 4 and 7 are considered low and below 4 are critically low. Although this test is primarily used to assess whether the child needs urgent medical care,

[13] 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 it breaths or shows any sign of life as heart activity (INE, 2009).

[14] Duryea *et al* (2006) examine under registration in six Latin American countries, and find that lack of birth certificate registration varies from 8.2 in Perú, to around 25% in Bolivia and Dominican Republic, taking intermediate values in Brazil, Nicaragua and Colombia.

a low score can indicate neurological damage and Almonds et al. (2005) report that a negative and statistically significant association of the score with the likelihood of various chronic childhood diseases at age three and with child mortality.

The data show a clear gradient in birth weight across groups. While the proportion under 2,500 grams in *PANES* households is 10%, for non *PANES* applicant households this proportion is 8%. Similarly, the proportion below 3,500 grams falls from 36% for *PANES* households to 30% for non *PANES* applicants. There is instead little evidence of the *APGAR* score being correlated with *PANES* status: children in all three subpopulations display relatively high levels of the scores.

There is a very clear indication that *PANES* mothers undergo the lowest number of prenatal controls (6.59 versus 8.46 for non applicant mothers) and that they have their first prenatal control later on (in the seventeenth week compared to the fourteenth week for the group of non applicants). *PANES* mothers are also more likely to give birth in public hospitals run by the Ministry of Public Health, and to use birth centers that - on average - deliver lighter children. This is most likely the result of the stratification of household's across health centers based on their socio economic status.

There is also a clear gradient in the probability of a cesarean section and the number of weeks of gestation, all of which are the lowest for *PANES* mothers and the highest for non *PANES* mothers. This might explain part of the differential incidence of low birth weight between *PANES* and non *PANES* mothers.

Additional information attains to the reproductive history of the mother and parents' socio-demographic characteristics. We find no clear gradient in mother's age across groups while, as expected, there is a clear indication of *PANES* status being negatively correlated to mother's education and employment status, and positively correlated to the number of previous births and spacing between current and previous births (only for mothers with a previously born alive child). *PANES* mothers are also more likely to be single (i.e. reporting being neither married nor in a stable relationship) and to be unmarried to the father's child. Notice that this proportion is extremely high in Uruguay, with two out of every three children being born out of the wedlock. Data on fathers show that *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.[15]

[15] The data also collect information on the place of residence and place of occurrence of birth, information on parents' race and nationality, and other information on birth (such as type of maneuver) that we use in the regressions below but that we omit from the descriptive analysis for brevity.

Table 3 reports regressions on the probability of low birth weight (defined as no more than 3 kg.) and the *APGAR* scores on a number of mothers' and birth characteristics. 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. Each column refers to a separate dependent variable. It is clear that there are considerable differences in both birth weight and - less so - the *APGAR* scores among children from different socio-economic backgrounds. For example children of mothers with completed primary education are (conditional on the other variables) 4 percentage points less likely to be underweight and rank 0.05 and 0.03 points higher respectively in the *APGAR* tests at one and five minutes compared to children of mothers with no education. The number of pre-natal controls is also negatively associated with low birth weight (with a coefficient of -0.03), possibly due to reverse causality, and positively associated with the *APGAR* scores (with coefficients of 0.03 and 0.02). Other variables have the expected sign.

In sum there are pronounced differences in the incidence of low birth weight and to a lower extent in the *APGAR* score across socioeconomic groups 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 restrict to recipient households and we use the length of exposure to *PANES* at the time of birth to identify the effect of the program on each child. We start from the following simple model:

$$Y_{imt}= \beta_0 + f(LAG_{imt})+ X_{itm}\gamma + u_{itm}$$

where Y denotes a generic outcome (low birth weight , *APGAR* score, etc) for child i of mother m born at time t, and X denotes a vector of additional child or mother characteristics that might vary over time.

The variable LAG denotes child i's exposure to *PANES* since the time of birth, which is defined as:

$$LAG_{imt}= t_{iM} - t_{0M}$$

where t_{iM} is the date of birth of child i and t_{0M} is date of entry of mother m into the program, defined as the time in which the household received the first payment. Notice that time of exposure as defined above can also take negative values if children are born before the time the household entered the program. The function $f(.)$ is an identifiable function of time of exposure: we experiment below with different specifications.

The variables X_{imt} include a large array of exogenous parents' and birth's observable characteristics including possibly time of birth effects. By conditioning on time of birth, model (1) abstracts from generalized trends in the incidence of low birth weight due to improvements in the health care system, health campaigns, etc.

To make the estimated model operational, we start from a model that defines $f()$ as a completely non parametric function of time of exposure:

(1) $$Y_{imt}= \beta_0 + \Sigma_e z_{eimt}\,\beta_e + X_{itm}\gamma + u_{itm}$$

where z_{eimt} is a dummy variable equal to one is child is born at lag e relative to the time of entry of the household into the program and zero otherwise. Identification of the model relies on variation in exposure, i.e. across children who were exposed to the program for different lengths of time at the time of birth. For the program to have an effect, one would expect β_e to be negative (in the case of low birth weight) for exposures strictly greater than A, where A is zero, or a strictly positive number if we assume that the program takes some time to display its effects.

In order for the OLS estimates of the parameters β_e to be consistent, it is required that - conditional on the included covariates - time of exposure is uncorrelated with the error term u. There are two potential threats to the consistency of the OLS estimates, both potentially due to the correlation between $(t_{iM} - t_{0M})$ and u.

First, conditional on time of entry into the program, time of birth (t_{iM}) can be correlated with the error term. For example, if following entry into the program, mothers whose children have higher propensity to display low birth weight tend to have children (or children earlier on, recall that the birth data are right censored), then one will find that low birth weight is negatively correlated with time of exposure, leading to overestimate the (presumably negative) effect of the program on low birth weight.

A separate but related concern arises if time of entry into the program (t_{0M}) is correlated with low birth weight. For example, if mothers who enter the program earlier on (i.e. with a lower t_{0M}) are more likely to have a low birth weight child, then one will find again that higher exposure tends (spuriously) to increase the incidence of low birth weight.

One first, albeit admittedly weak, way to test for the identification assumption is to include a number of observed covariates in the model, as - for the OLS estimates of model (1) to be consistent - one would expect exposure to be uncorrelated with household and children's characteristics that are known to affect low birth weight.

A second way to implicitly falsify the identification assumption is to examine birth weight among children born before the program, since one would expect β_e to be zero for $e \leq A$. This allows testing for existing pre-program trends due possibly to non random date of entry into the program or anticipation effects.

A simple variant of model (1) is a model where we constraint the coefficients β_e to be zero for $e \leq A$:

$$(2) \qquad Y_{imt} = \beta_0 + \Sigma_{e>A} z_{eimt} \beta_e + X_{itm}\gamma + d_t + u_{itm}$$

and even a more parsimonious specification is:

$$(3) \qquad Y_{imt} = \beta_0 + \beta\, e_{imt} + X_{itm}\gamma + d_t + u_{itm}$$

where:

$$e_{imt} = I(LAG_{imt} > A)$$

The advantage of specification (2) over (3) is that this is deemed to provide more precise estimates, although clearly at the cost of some restrictions. In practice model (3) provides simple diff-in-diff estimates of the treatment effect by exploiting the variation in birth weight before and after treatment among treated and yet-to-be treated households.

Models (2) and (3) can still be identified once mother fixed effects are included, in practice controlling for time-invariant unobserved mother and household characteristics. Notice, though, that model (1) will be unidentified when both mother fixed effects and date of birth effects are included, as the difference in time of exposure between two siblings will be perfectly correlated with the difference in their time of birth.

There are obvious advantages in using repeated births from the same mother but there are also drawbacks that need to be considered. The obvious advantage is that this allows to controls for any potential correlation between exposure and birth outcomes that is due to mother's unobserved time invariant characteristics. These include, among others, time of entry into the program. So this model accounts for the one source of potential correlation between treatment and outcomes.

Other than the difficulty due to the sample size becoming potentially too small to identify precisely the coefficients of interest when we restrict to mothers with repeated births, the problem potentially remains regarding endogenous fertility timing (t_{iM}) or fertility choices. Suppose, for example that only mothers with a previously low birth weight child or only those who gain from the program have a child after joining the program. In this case, estimates of the population parameters will be biased due to a regression to the mean effect or endogenous fertility. We attempt to discuss these issues in the section below where we present the regression estimates.

5. Regression results

5.a Basic Evidence

Before presenting the regression results, we present evidence on the correlation between program exposure and low birth weight in Figure 1. The figure reports the proportion of children classified as low birth weight - defined as weighting at most 3,000 g. - as a function of exposure to the program. Here and in the rest of the paper, exposure to the program is measured in terms of quarters. We present analyses for additional children's outcomes below. Alongside the point estimates, we also report 95% confidence intervals. The results are startling. One can see that the proportion of low weight births before entering the program is on the order of 37 p.p.: more than one third of children of *PANES* mothers are below this threshold. There is a discontinuous and permanent change in the proportion underweight for children born at least 2 quarters following their mother's entry into the program: the proportion at or below this critical level is on the order of 32%, around 5 percentage points lower.

Results in figure 1 are suggestive of a causal effect of program exposure (for at least two quarters) on low birth weight. We investigate these results further by means of regressions. Table 4 reports preliminary regression results for the effect of exposure to the program on low birth weight. Column 1 reports OLS estimates of equation (1) with no further controls. This is essentially the data in Figure 1. We standardize the coefficients around the coefficient lag 0, that we constraint to be zero. One can see that all coefficients before lag zero are small and not statically significant at conventional levels, implying no pre-trends in birth weight. The effect is still small and statistically insignificant at lag one quarter. After that, the incidence of low birth weight falls by 5 percentage points and remains roughly unchanged after that. Column 2 includes dummies for date of birth. Results are similar but point estimates are slightly smaller and less precise. The reason for this is that children with positive program exposure are born in later periods, and so the coefficients in column 1 in part also absorb a generalized trend towards falling birth weight due to improvements in economic conditions and increased quality of the public health system.

5.b Main regressions

In column 3 of Table 4 we report estimates of model (2), where we constrain all coefficients at exposure one quarter or below to be zero. We report again estimates with the inclusion of time of birth effects as in column2. Coefficients are all negative and not dissimilar from those in column 2, but individually largely insignificant. This remains true when we include mother fixed effects in column 4.

In sum, Table 4 suggests that there is a systematic improvement in infants' weight at birth following the mother's exposure to the program for at least two quarters. Although part of this effect appears to be explained by a generalized time trend towards better infants' outcomes, there is also some evidence that these effects persist after controlling for date of birth. Results are imprecise though, so in Table 5 we turn to estimating model (3) where we constraint all coefficients past one quarter of exposure to be the same.

Each cell of Table 4 refers to a separate equation. Here we report OLS estimates of model (3), i.e. the coefficient on a dummy for exposure greater than one quarter. Each row refers to a separate dependent variable and each column to a different specification. The first row reports coefficients for the proportion with birth weight equal or below 3,000 g. Column 1 only includes time of birth effects. Estimates suggest that program exposure is associated to a fall in the proportion of children weighting 3,000 grams or below of 2.6 percentage points. This is about 30% of the unconditional difference in the incidence of low birth weight between *PANES* beneficiaries and non *PANES* eligible. (-0.026/0.060) and corresponds to a fall in the incidence of low weight among *PANES* mothers of 7% (0.026/0.36).

Column 2 includes a very large array of additional covariates. These are: dummies for sex of the child, dummies for mother's and father's age, country of origin, civil status, labour market status, education, a dummy for missing information on father, dummies for the relationship between parents, number of the mother's living and deceased children, number of previous miscarriages/abortions,[16] whether this was a multiple pregnancy, whether this was a caesarean birth, dummies for locality of birth (329), plus dummies for *PANES* score (average within 1,000 equally sized cells). Results are essentially unchanged, although estimates are not statistically significant at conventional levels.

Column 3 includes mother fixed effects but not the additional controls. Here we control for permanent differences in household and mothers' characteristics. Reassuringly, point estimates are also unchanged, implying that unobserved mother and household characteristics (including time of entry) are uncorrelated with the treatment dummy.

Finally, in column 4 we include both mother fixed effects and the large array of control variables listed above. Most of these variables are likely to vary across time, so the point estimates do not need to be similar to those in column 3. Indeed, we find that the point estimates falls from -0.025 to -0.045, suggesting that simultaneous omission of mother's observed and unobserved characteristics plus birth characteristics leads to estimates of the impact of treatment on low birth weight that are upward biased. These estimate imply a fall in the proportion weighting 3kg or less on the order of 12.5% and a reduction in the unconditional difference in the incidence of low birth weight between *PANES* beneficiaries and non *PANES* eligible of 75%.

The subsequent two rows report estimates of the proportion at or below 2,750 and 2,500 g. respectively, the latter being the definition of low birth weight used in the medical literature. The most saturated specification shows a fall in the outcome variable associated to treatment on the order of 3 and 0.5 percentage points, respectively, although the latter is statistically insignificant. These are large numbers, corresponding to between 25% and 50% of the gap between children of *PANES* mothers and children of non applicant *PANES* mothers.

The fourth row reports estimates on average birth weight. Point estimates in the most saturated specification are positive but not significant at conventional levels. We find that the program increased average birth weight by 14 grams. This result is in line with estimates in row 1, as the reduction in the proportion of children in the bottom tail of the distribution would imply an average increase in birth weight of 49 grams: that is within the confidence interval for the estimates in row 4, column 4.[17]

[16] Although abortion is not legal in Uruguay, illegal abortion is widespread.

[17] To see this, notice that, $E(y)= E(y| y \leq 3kg)Pr(y \leq 3kg)+ E(y| y>3 kg)Pr(y>3 kg)$. Using numbers for those with a zero treatment dummy, this is 2,598*(.366) + 3.469*(1-.366)=3,150. Plugging in the implied change in the proportion at low birth weight, this gives 2,598*(.366-.045) + 3.469*(1-.366+.045)=3,189. Results (not reported) also show that there is no significant change in the proportion of at or below subsequent equally spaced weight intervals from 3.5 kg upward.

Results in the following rows explore the effect of the program on the *APGAR* scores one minute and five minutes post birth. There is no evidence of the program affecting the *APGAR* scores. Interestingly Almond et al. (2005) also find evidence of other risk factors (maternal smoking) in the U.S. affecting the incidence of low birth weight but not *APGAR* scores suggesting that this is potentially an outcome that is harder to manipulate.

5.b Channels

Table 6 explores the channels behind the estimated effects. The structure of the Table is the same as Table 5. We only concentrate on the most saturated specification with all controls and mother fixed effects in column 4. The table shows clearly an improvement in prenatal care as a result of the program. The number of controls in each quarter, and especially in the first and second quarter, increases, with on overall increase in the number of prenatal controls of around 0.2. Similarly there is a fall in week of first controls, that falls by around 0.6, i.e. a fall of around 4.5 days. Increased prenatal care might be responsible for improved outcomes. Recall that this was a perceived conditionality of the program, although one that was never enforced.

We find no changes in any of the other variables: neither in the average quality of the health centre where the child was born (measured by the average weight at baseline), nor in whether the birth took place at a Ministry of health centre, nor in the weeks of gestation (that could explain differences in very severe low birth weight). We find little evidence of birth spacing being affected by the program, suggesting little changes in fertility as a result of the program.

6. Preliminary conclusions and directions for future work

The analysis so far has shown considerable improvements in children's birth weight (but not in *APGAR* scores) as a result of program participation. Our most saturated specifications that include other fixed effects and a very large array of time varying controls, imply that *PANES* children closed between 25% and 75% of the gap with children of non-*PANES* applicants. These correspond to a fall in the incidence of low birth weight between 5% and 12.5% depending on the measure used.

The effects though seem to be concentrated around intermediate levels of birth weight (between 2,500 and 3,000 grams) that are not typically regarded as critical for children's development and survival.

We have also started to investigate the effects on fertility. These are obviously important, as these might lead to endogenous compositional effects and hence potentially biased estimates of program impact. Our preliminary analysis shows no significant effect on birth spacing but more work is needed to investigate the effect on the probability of giving birth to additional children.

We find clear evidence of increased numbers of prenatal controls. This increased number of prenatal controls does not appear to be due to increased access to the public health system. An analysis of baseline data shows that the percentage of mothers with no access to the public health system was extremely low (around 1%, both among treated and non treated households). We are not able to say though whether this increased number of prenatal controls is the result of the (perceived) conditionalities of the program, increased awareness and education due to other program ingredients or other factors. Auxiliary evidence seems to rule out that the majority of household's perceived pre-natal care as a pre-requisite for program receipt.

Although suggestive of a causal role of increased access to health care on children's birth weight, our results are also unable to shed light on the role of increases household income and resources due to the income transfer and food card component of the program *vis à vis* the elements listed above.

Although one possibility is to remedy some of the above drawbacks by including additional program data, assignment of different ingredients to households was not random (or quasi random) so that these results will have to be interpreted with some caution.

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Table 1: Birth outcomes in selected LAC countries

Country	Low weight at birth	Births assisted by health personnel	Prenatal controls (at least one visit)
Uruguay (2002)	**8**	**99**	**97**
Argentina (1999)	7	99	99
Brazil (1996)	10	97	97
Chile (2001)	5	100	n.a.
Costa Rica (2000)	7	n.a.	92
Cuba (2001)	6	100	100
Mexico (1999)	9	94	n.a.
Peru (1996)	11	73	91
Latin America and Caribbean	9	96	93

Source: WHO (2009) and UNICEF (2004)

Table 2: Child characteristics - All births –2003-07, by *PANES* status

	PANES Applicants		Non-*PANES* Applicants	All
	Beneficiaries	Non-Beneficiaries		
Observations	54,192	19,857	156,709	230,75
Weight ≤2,500g.	0.10	0.09	0.08	0.09
Weight ≤2,750g.	0.20	0.18	0.16	0.17
Weight ≤3,000g.	0.36	0.33	0.30	0.31
Weight	3158.58	3183.94	3228.74	3208.3
Apgar-1	8.49	8.48	8.50	8.50
Apgar-5	9.61	9.60	9.62	9.62
Prenatal controls				
First quarter	0.33	0.44	0.68	0.58
Second quarter	1.66	1.97	2.26	2.09
Last quarter	4.61	5.24	5.52	5.28
Total	6.59	7.66	8.46	7.95
Week of first control	17.16	15.75	13.64	14.62
Public Health (MSP)	0.72	0.50	0.30	0.42
Average weight in centre	3166.85	3186.27	3210.64	3198.2
Weeks gestation	38.50	38.51	38.54	38.53
Caesarean section	0.20	0.26	0.36	0.31
Mother characteristics at				
Age	25.28	24.83	27.53	26.77
No schooling	0.12	0.05	0.03	0.05
Not employed	0.89	0.81	0.54	0.64
Single	0.33	0.33	0.22	0.26
Alive children	2.18	1.22	1.00	1.30
Deceased children	0.31	0.19	0.14	0.18
Birth spacing	3.92	4.96	5.26	4.84
Father characteristics at				
Missing	0.46	0.40	0.24	0.31
Age	30.69	29.64	32.01	31.65
No schooling	0.13	0.06	0.03	0.04
Not employed	0.10	0.08	0.04	0.05
Out of wedlock	0.82	0.74	0.54	0.62

Notes. Average weight in centre computed on pre 2006 data.

Table 3: Correlation between outcomes and observable characteristics – All births 2003-07

	Weight ≤3,000 g.	Apgar-1	Apgar-5
Mother's education – completed primary	-0.041***	0.054***	0.028***
	(0.004)	(0.011)	(0.008)
Mother's education - completed secondary	-0.052***	0.087***	0.060***
	(0.005)	(0.013)	(0.010)
Mother's education - completed college	-0.050***	0.147***	0.113***
	(0.006)	(0.015)	(0.011)
Mother not working	0.007***	-0.031***	-0.017***
	(0.003)	(0.006)	(0.005)
prenatal Controls	-0.028***	0.028***	0.023***
	(0.000)	(0.001)	(0.001)
Average weight in centre	-0.000***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)
Alive children	-0.027***	0.048***	0.033***
	(0.001)	(0.002)	(0.001)

Regressions include: a dummy for child's sex, dummies for mother's age, dummies for out of wedlock birth and single mother plus date of birth dummies. Reference group: mother's education incomplete primary.

Table 4: Program effects on low birth weight (≤3,000g) – Unrestricted specification

Years of Exposure to *PANES*	(1)	(2)	(3)	(4)
-3.25	-0.006	-0.036		
	(0.021)	(0.025)		
-3.00	0.008	-0.022		
	(0.018)	(0.022)		
-2.75	0.010	-0.020		
	(0.014)	(0.020)		
-2.50	0.006	-0.025		
	(0.014)	(0.019)		
-2.25	0.010	-0.021		
	(0.013)	(0.018)		
-2.00	-0.006	-0.032*		
	(0.013)	(0.017)		
-1.75	-0.011	-0.033*		
	(0.013)	(0.017)		
-1.50	-0.001	-0.020		
	(0.013)	(0.016)		
-1.25	0.010	-0.006		
	(0.013)	(0.015)		
-1.00	-0.008	-0.019		
	(0.013)	(0.015)		
-0.75	0.004	-0.005		
	(0.013)	(0.014)		
-0.50	-0.015	-0.021		
	(0.013)	(0.014)		
-0.25	-0.000	-0.005		
	(0.013)	(0.014)		
0.25	-0.009	-0.006		
	(0.014)	(0.015)		
0.50	-0.050***	-0.044***	-0.037***	-0.029
	(0.014)	(0.015)	(0.012)	(0.022)
0.75	-0.035**	-0.026*	-0.019	-0.022
	(0.014)	(0.015)	(0.013)	(0.023)
1.00	-0.034**	-0.030*	-0.023*	-0.061**
	(0.014)	(0.016)	(0.014)	(0.025)
1.25	-0.032**	-0.028*	-0.021	-0.019
	(0.014)	(0.017)	(0.014)	(0.026)
1.50	-0.035**	-0.025	-0.018	0.003
	(0.014)	(0.018)	(0.015)	(0.029)
1.75	-0.045***	-0.032*	-0.025	-0.004
	(0.015)	(0.019)	(0.017)	(0.031)
2.00	-0.029*	-0.018	-0.011	-0.038
	(0.016)	(0.020)	(0.019)	(0.034)
Time effects	No	Yes	Yes	Yes
Mother fixed effects	No	No	No	Yes

Table 5: Diff-in-diff estimates: program effect on birth outcomes

	(1)	(2)	(3)	(4)
Weight ≤3,000g.	-0.026***	-0.028	-0.025***	-0.045***
	(0.009)	(0.017)	(0.009)	(0.017)
Weight ≤2,750g.	-0.010	-0.013	-0.010	-0.030**
	(0.008)	(0.015)	(0.007)	(0.015)
Weight ≤2,500g.	0.000	0.003	0.000	-0.005
	(0.006)	(0.012)	(0.006)	(0.012)
Weight	12.652	-9.096	4.786	13.737
	(10.696)	(18.668)	(10.401)	(17.894)
Apgar-1	0.018	-0.050	-0.008	-0.006
	(0.021)	(0.048)	(0.021)	(0.047)
Apgar-5	0.024	-0.036	-0.004	-0.004
	(0.016)	(0.037)	(0.016)	(0.037)
Time effects	Yes	Yes	Yes	Yes
Mother fixed effects	No	Yes	No	Yes
Additional controls	No	No	Yes	Yes

Entries are coefficients on a dummy on exposure to *PANES* greater than one quarter. Each cell refers to a separate regression. Controls include: sex of the child, dummies for mother's and father's age, country of origin, civil status (single, married, cohabiting, separated, divorced, widow, missing), labour market status (employed, unemployed, out of the labour force, missing), education (less that completed primary, completed primary, completed secondary, completed tertiary and missing), a dummy for missing information on father, dummies for the relationship between parents (married, unmarried in a stable relationship, unmarried in an unstable relationship, missing), number of the mother's living and deceased children, number of previous miscarriages/abortions, whether this is a multiple pregnancy, whether this was a caesarean birth, dummies for locality of birth (329), plus dummies for *PANES* score (average within 1,000 equally sized cells). Number of observations 51,145.

Table 6: Diff-in-diff estimates: channels

	(1)	(2)	(3)	(4)
prenatal controls				
First quarter	0.002	0.074***	0.013	0.060**
	(0.013)	(0.024)	(0.013)	(0.025)
Second quarter	0.033	0.122**	0.077***	0.089*
	(0.025)	(0.048)	(0.025)	(0.049)
Last quarter	0.082*	0.072	0.123***	0.059
	(0.049)	(0.092)	(0.048)	(0.093)
Total	0.116*	0.266**	0.212***	0.208*
	(0.065)	(0.114)	(0.062)	(0.114)
Week first control	-0.251*	-0.887***	-0.490***	-0.652**
	(0.147)	(0.325)	(0.146)	(0.328)
Paid by Public Health Service	0.019**	-0.011	0.005	0.000
	(0.008)	(0.014)	(0.009)	(0.016)
Weeks gestation	0.024	-0.122	0.009	-0.085
	(0.039)	(0.080)	(0.038)	(0.086)
Average weight in centre	9.876***	2.935	-0.461	2.562
	(1.582)	(2.094)	(0.906)	(1.896)
Birth spacing	-0.257***	-0.139**	0.049	-0.071
	(0.064)	(0.055)	(0.101)	(0.096)
Time effects	Yes	Yes	Yes	Yes
Mother fixed effects	No	Yes	No	Yes
Additional controls	No	No	Yes	Yes

See notes to Table 5.

Figure 1
Proportion born with weight less or equal to 3,000g
as a function of time of entry into Program
PANES households only