Cash and Care: Conditional Cash Transfers and Birth Outcomes

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Abstract

The objective of this paper is to evaluate the impacts of the introduction of a cash transfer for pregnant women in the design of the Bolsa Família Program (BFP) on infant health. In particular, we focus on the effects on birth weight and on the incidence of low birth weight, but we also assess the impacts on pregnancy outcomes such as the incidence of preterm births and prenatal care. The new benefit consists of nine monthly transfers which start to be paid when the woman is identified as pregnant by the health management system of the BFP, regardless of the stage of pregnancy at the moment of the identification. The empirical strategy explores the fact that when the transfer was implemented eligible women at different stages of pregnancy were exposed to different income shocks before giving birth. Using administrative data from the BFP and from the Brazilian Ministry of Health, we create links between benficiary women and their children's natality outcomes. The main results show that the transfer had a positive impact on birth weight and reduced significantly the likelihood of low birth weight. We also find that higher exposures to the transfer during pregnancy reduced the incidence of preterm births. While we don't find significant impacts on the number of prenatal visits, the results show that the transfer reduced the likelihood of delayed prenatal care.

Keywords: conditional cash transfers, birth weight, infant health.

JEL Classification: JEL I14, I32, I38, J13, J16, O15

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1 Introduction

Conditional Cash Transfer (CCT) programs have gained a lot of ground over the past few decades, especially in developing countries but also in developed nations. In the case of Brazil, the Bolsa Família Program (from now on BFP) was created in October 2003 as the unification of the existing conditional income transfer programs from the federal government. Transfers to households consist of a monthly basic benefit paid to extremely poor families (monthly income per person up to R$85 or approximately U$22) and monthly variable benefits paid to poor households (monthly income up to R$170 per capita or approximately U$45) that meet specific prerequisites and comply with program conditionalities in health and education. Currently, the BFP is the largest CCT in the world, reaching approximately 14 million families, and is recognized for its cost-effectiveness: it is considered a relatively inexpensive and efficient program due to its focus on the poorest of the poor (Barros et al., 2010).

Until the end of 2011, the BFP had two types of benefits on its design in addition to the basic benefit for extremely poor families: one monthly variable benefit for poor families with children aged between 0 and 15 and another monthly transfer for poor families with teenagers aged 16 and 17, with a maximum limit of three variable benefits for ages 0-15 and two benefits for teenagers per family. In November 2011, the federal government implemented important changes in the program design, creating two additional variable benefits: the Variable Benefit for Nursing Mothers, paid to poor families with children between 0 and 6 months of age, and the Variable Benefit for Pregnant Women, paid to poor households with pregnant women aged between 14 and 44 (*Benefício Variável Gestante*, from now on BVG). The transfer for nursing mothers started to be paid in November 2011 and the BVG started to be paid in December 2011. The monetary values of the new transfers were defined to be the same as the value of the benefit for ages 0-15 (at the time, the value was R$32 or approximately U$8 per month). In the end of 2011, there was also an increase in the maximum limit of variable benefits per family, which went from three to five.

The BVG consists of 9 monthly transfers which start to be paid when the eligible woman is identified as pregnant by the health management system of the BFP, regardless of her pregnancy stage. This last feature is of key importance because women in different stages of pregancy are exposed to different amounts of payments before giving birth, although they receive the same total amount after nine months. Indeed, women in earlier stages of pregnancy receive more transfers before giving birth. To give an example, suppose that two eligible pregnant women start to receive the BVG at the same moment but one of them is in the first month of pregnancy while the other is with eight months. While both of them will receive the same total amount in the end of nine months, the first one will receive eight payments before the birth of her child and one payment after while the latter will receive only one payment before giving birth and eight payments after. The only conditionality for beneficiary women to continue receiving the BVG is to conduct prenatal consultations in addition to complying with the other conditionalities of the program.

The objective of this paper is to evaluate the impacts of the introduction of the BVG on infant health. The empirical strategy explores the variation in the stage of pregnancy at the time of the new transfer implementation. The identification hypothesis is that, when the BVG was implemented, women at different stages of pregnancy were differently exposed to exogenous income shocks before giving birth. We focus on the impacts on birth weight and on the incidence of low birth weight, but we also assess the effects on other health outcomes such as Apgar scores and the incidence of congenital anomalies, as well as on pregnancy features like gestational age and the incidence of preterm births.

We also study one important potential mechanism behind the impacts of the new transfer, which is prenatal care. Because conducting prenatal consultations is a conditionality to continue receiving the BVG, it can be that beneficiary children are born healthier in part because of increased prenatal care. On the other hand, if we don't find any impact of the BVG on prenatal care, there is evidence that the effects of the new transfer operate mainly through the cash transfer itself. Therefore, by assessing the impacts of the BVG on prenatal care, it is possible to understand the mechanism under which the transfer works.

We use administrative data from the BFP and vital statistics natality data from the Brazilian Ministry of Health, with millions of individual records on beneficiaries and health outcomes. Identification variables such as the child's and mother's date of birth, the child's gender and the municipality of residence allow us to connect the two datasets. We link pregnant women who started to receive the BVG when it was implemented with their newborn children and the respective birth outcomes. To estimate the impacts of the new transfer, we use the difference between the child's month of birth and the month in which the mother started to receive the BVG to calculate the total number of months that the mother was exposed to the transfer during pregnancy, which will define our treatment groups.

The main results show a positive impact of higher exposures to the BVG during pregnancy on birth weight. Exposures for four or more months increased birth weight between 72g and 95g depending on the specification, which represent percentage increases between 2.2% and 3.1% compared to mothers that received less than one month, defined as our control group. The results for the incidence of low birth weight are even stronger. Mothers that received four or more payments before giving birth experienced reductions in the likelihood of births with less than 2,500g between 1.6 and 1.8 percentage points, which represent percentage decreases between 19% and 30% in comparison with the control group. The results are robust across different specifications.

The results for gestational age show that, although mothers who received one to three months of BVG presented lower gestational age than mothers who had children in the same month that they started to receive the transfer, the effects are very small and the impact for mothers who received four or more payments is not statistically significant. Regarding the incidence of preterm births, while mothers who received only one month of BVG presented increased incidence of these adverse births, exposures to the transfer for four or more months reduced the likelihood of preterm births by 35%. The results for two and three months of exposures are not statistically significant.

It is important to stress out that the new transfer could also have an impact on reducing mortality for fetuses with low birth weight or preterm born, which would bias the results towards a negative or null effect. However, even with the potential reduction in fetal mortality for more adverse births, we find positive and strong effects of higher exposures to the BVG on birth weight and on the incidence of low birth weight and preterm births, which is a strong finding.

Looking at very low birth weight (i.e, children born with less than 1,500g), the results show that the BVG is also negatively associated with the incidence of these births, although the coefficients are not precisely estimated. The effects for Apgar scores and congenital anomalies are small and not statistically significant.

The results for prenatal care show that, while we don't find significant impacts on the number of prenatal visits, the transfer reduced the average month in which the mother started prenatal care and in particular reduced the likelihood of delayed prenatal care (i.e, after the third month of pregnancy). The effects are substantial and significant for all treatment groups except for mothers that received only one month of the transfer before giving birth. We also have that the impacts are monotonically increasing with higher exposures to the BVG. While exposures for two months reduced the likelihood of delayed prenatal care in approximately 26%, exposures for three and four or more months reduced delayed prenatal care in 42% and 68%, respectively. Therefore, there is evidence that prenatal care seems to be an important mechanism of the BVG to improve natality outcomes altogether with the direct cash transfer.

Finally, we analyzed how the impacts differ among different subgroups such as black and brown mothers and women living in rural areas. While the effects for birth weight are positive for all subgroups but stronger for non-black mothers and rural regions, the opposite happens when we analyze the impacts on the incidence of low birth weight: the effects for non-black mothers and rural areas are not statistically significant but we find strong reductions for black and brown mothers and urban areas. The effect on reducing the likelihood of preterm births is also concentrated for these last subgroups. Similar to the results for the full sample, the impacts on the number of prenatal visits are not statistically significant. Although we find reductions in the likelihood of delayed prenatal care for all subgroups, the effects are also stronger in rural areas.

2 Literature

There is plenty of evidence amongst the literature that conditions during pregnancy significantly impact later outcomes for the affected children in the short but also in the long run (Almond & Currie, 2011). Birth weight is one of the most important and widely used outcomes to measure infant health. Moreover, studies point to a strong correlation between birth weight and infant mortality (Almond & Chay, 2005), which is another outcome that is widely used to measure infant health. In a recent study for Brazil, for example, Carrilo & Feres (2017) use administrative vital statistics data from the Brazilian Ministry of Health and adopt a within-twin identification strategy to assess the effects of low birth weight on infant mortality, finding indeed that children with low and very low birth weight have higher risks of death during infancy. There is also evidence on the impacts of birth weight on long run outcomes such as permanent income (Bharadwaj, Lundborg & Rooth, 2017).

Many studies have analyzed the impacts of expanding the safety net on social outcomes such as education and health for different countries (Fiszbein & Schady, 2009). In particular, there is a rich literature exploring the impacts of safety net programs on infant health, which will be the focus of this study. Although most of the evidence of the impacts on birth weight comes from the U.S, there are also many studies assessing the impacts on other infant health outcomes for Latin Americam countries and for Brazil in particular.

For the U.S, Currie and Cole (1993) assess the impact of participation in the Aid to Families with Dependent Children (AFDC) program during pregnancy on the birth weight of their children. While apparently there seems to be a negative correlation between AFDC participation and birth weight, this association disappears after controlling for omitted variables, which they do by using methods of instrumental variables and a model for siblings with mother fixed effects. Figlio et al. (2009) study the effect of another important program in the U.S, the Supplemental Nutrition Program for Woman, Infants and Children (WIC), on birth weight and gestational age by comparing families in a tight income range close to the WIC eligibility threshold. They make use of a policy change that affected women differentially on both sides of the threshold and find that WIC participation had no effect on mean birth weight and gestational age but reduced the likelihood of low birth weight.

Most of the studies for the U.S focus on the impacts of the Food Stamp Program (FSP), which is the largest cash safety net program in the country. However, because there is little geographic and eligibility variation in the program, many of them rely on strong assumptions to compare participants with nonparticipants. The results are mixed, with some studies finding a negative correlation between FSP participation and health outcomes (Butler and Raymond, 1996). However, this adverse effect could be driven by negative selection in participation in the program (Currie, 2003). Currie & Cole (1991) try to deal with the problem of selection bias by using comparisons between siblings and methods of instrumental variables and don't find significant effects of the FSP on birth weight.

More recent studies have adopted the empirical strategy to use the county-by-county rollout of the program in the 1960s and early 1970s. Currie & Moretti (2008) use the variation in the FSP adoption at the county level for the state of California, finding that the introduction of the program was actually associated with a reduction in birth weight. However, they show that this adverse effect is driven mainly by first births among teens and by the Los Angeles County. Another possible explanation for the negative impact found could be related to decreases in mortality for fetuses with lower birth weights, which would bias the results negatively.

Almond et al. (2011) also use the county-by-county rollout of the program to evaluate the impacts of the FSP on birth weight, as well as on neonatal mortality and fertility rates. However, different from Currie & Moretti (2008), they use the phase-in in the adoption of the program county-by-county at a national level. While both studies use the information on the month in which the program began operating in each county to define their treatment, another difference from Currie & Morreti (2008) is the timing of the FSP adoption used. While the latter consider the availability of FSP early in pregnancy as their treatment, Almond et al. (2011) focus on the availability at the end of gestation, more precisely during the last trimester, following evidence that this period is the most important to determine birth weight.

The authors combine administrative data from the period of the program adoption at each county with vital statistics natality and mortality data collapsed to each county and race. The results show that participation in the FSP increased mean birth weight, with stronger effects for black children. The impacts are also greater for newborn children with low and very low birth weights and for people living in high-poverty areas. They don't find significant effects on fertility and neonatal mortality rates. The study also evaluates the sensitivity of the impact of the FSP on birth weight to changes in the timing of the FSP assignment, finding that moving the treatment from the third to the second trimester of pregnancy reduces the impact of the program. Moreover, the impacts of the FSP availability at the first trimester of pregnancy and earlier exposures conditional on last trimester exposure are not statistically significant, corroborating the evidence that conditions during the last trimester of pregnancy are key to determine birth weight.

For Latin America, many studies make use of randomizations in the implementation and roll-out of different CCT programs to assess their causal effects on infant health. Paxson & Schady (2010) study the effects of *Bono de Desarollo Humano*, a randomized CCT program that transfers money to women in rural Ecuador, on early childhood development, including health outcomes such as hemoglobin levels and deworming treatments. Although they don't find treatment effects on average, they estimate positive effects for the poorest children. Macours, Schady & Vakis (2012) use the randomization in the implementation of the CCT program *Atención a Crisis* in rural Nicaragua, which is also focused on women with children, to estimate its impact on anthropometric measures such as height-for-age and weight-for-age. Although they find positive effects of the program, the magnitudes are modest.

Gertler (2004) and Bahram (2011) study the impact of the Mexican program *PROGRESA* (now *Oportunidades*), one of the first large-scale CCT programs in the world which served as inspiration for many other countries, on health outcomes of beneficiary children. Both studies make use of the random experiment of the Mexican government regarding the implementation of the program, which due to administrative and financial constraints had to be done in different phases. Gertler (2004) finds that the program decreased illness rates and the likelihood of anemia and increased children's height, with cumulative gains the longer the children stayed in the program. Barham (2011), in turn, looks at the impacts on neonatal and infant mortality, finding that the program reduced infant mortality rates but had no significant effect on neonatal mortality.

In Brazil, there are some studies assessing the impacts of the *Programa Saúde da Família* (Family Health Program, from now on FHP) on infant health. The FHP is an important program from the Brazilian Ministry of Health focused on the provision of free primary health care in poor areas through professional health-care teams that are responsible for following a number of household over time. Most of the studies explore the roll-out of the program, which was implemented by municipalities in different periods of time, to estimate its effects on different health outcomes. Aquino et al. (2009) assess the effects of FHP coverage on infant mortality. They use vital statistics data at the municipality level combined with information on the provision of healthcare services, including the FHP, and find that the program reduced infant mortality rates. Rocha & Soares (2010) also use the same data from the Brazilian Ministry of Health to estimate the impacts of the FSP on overall mortality throughout the age distribution, as well as on household behaviors related to schooling, employment and fertility. They find that the program reduced mortality throughout the whole age distribution, with stronger effects for ealier ages and for municipalities in the poorest regions. The results also show that the FHP reduced fertility rates and increased school enrollment and labor supply of adults. More recently, Reis (2014) use the variation in the program's availability among siblings of different ages in the same municipality to estimate its effects on infant health outcomes. Different from the other studies, he uses data from the Brazilian National Household Survey (PNAD) supplement for 2003, which has information on self-reported health status for individuals and their children, to define the outcomes of interest. He finds that availability of the program during the prenatal period reduced the prevalence of bed and restrictive activity days, vomiting or diarrhea and the need for medical assistance.

Morris et al. (2004) analyzed the impacts of another important Brazilian program, the *Bolsa Alimentação*, on anthropometric measures of beneficiary children. The *Bolsa Alimentação* was a CCT program from the federal government designed to transfer money to poor families with children and with pregnant and nursing women in their composition, and served as one of the main basis for the creation of the BFP. The empirical strategy explores the fact that, when the program was launched, in some Brazilian municipalities there were individuals selected to become beneficiaries that ended up excluded of the program due to administrative errros. They use data from surveys conducted by an anthropometry team to evaluate the impacts of the program and find that beneficiary children actually gained less weight than similar exluded children with the same ages.

Regarding the BFP, there are recent studies assessing its impacts on infant health. However, because selection in the program is not random and there isn't enought geographical variation in its rollout, it is more challenging to estimate its causal effects. Andrade et al. (2012) investigate the impact of the BFP on the immunization of children by using propensity score matching techniques. They use data from a household survey specially conducted to evaluate the program and don't find any effect on the immunization profile of beneficiaries, although the fulfillment of the immunization schedule is one of the health conditionalities of the BFP. Using the same techniques and data, Andrade et al. (2013) also estimate the impact of the program on children's nutritional status, finding a positive impact on BMI-for-age but no effect for height-for-age. Paes-Sousa et al. (2011) also look at the effects of the BFP on nutrition. Using data with anthropometric measures obtained from vaccination campaigns promoted by the Brazilian federal government, they use logistic regressions methods and find that children with 12 to 59 months of age from beneficiary families were more likely to have normal height-for-age and weight-for-age, although there is no difference for children between 0 and 11 months of age.

Rasella et al. (2013) and Shei (2013) estimate the impacts of BFP coverage on childhood and infant mortality, respectively, both at the municipality level. They use vital statistics data and the information on the provision of primare healthcare services for each Brazilian municipality, both from the Ministry of Health, combined with aggregated data on BFP coverage by municipality.

Rasella et al. (2013) use negative binomial regression models with fixed-effects for municipalities in the estimation. They also control for FHP coverage and include an interaction term between BFP and FHP coverage. The results show that BFP coverage reduced overall under-5 mortality rates even after controlling for FHP coverage. Shei (2013) also controls for the FHP and finds that BFP coverage was associated with reductions in overall infant mortality rates, with stronger effects for postneonatal mortality in comparison with neonatal mortality. The effects are also stronger for municipalities with higher FHP coverage. Finally, Shei et al. (2014) make use of a special household survey conducted with randomly selected BFP beneficiaries and non-beneficiaries in a Braziliam slum community to assess the impact of the program on health care utilization. Using propensity score methods, they find that the BFP increased the likelihood of children's visits to the health post for preventive services, children's growth monitoring, vaccinations and checkups, with positive spillover effects for older siblings.

The main contributions of this paper are fourfold. First, to the best of our knowledge, this is the first study that evaluates the impacts of the BFP on important infant health outcomes such as birth weight and the incidence of low birth weight, as well as on pregnancy features like gestational age and on the incidence of preterm births. Actually, even the studies for the FHP focused on other health outcomes such as infant mortality and nutrition. Secondly, while most of the studies for Brazil focused on the municipality level, especially the ones using vital statistics data, we use administrative data at the individual level both from the BFP and for the Ministry of Health. The rich data that we have access cointain millions of individual records with detailed information on program beneficiaries and health outcomes. Identification variables allow me to connect the different datasets and create links between beneficiary women and their children's natality outcomes. Thirdly, we explore the moment of the introduction of the benefit for pregnant women and the fact that it starts to be paid regardless of the woman's stage of pregnancy as a quasi-experimental variation in the exposure to the transfer before giving birth. By comparing beneficiary women that started to receive the new transfer at the moment of its implementation but were at different stages of pregnancy (and therefore were differently exposed before giving birth), it is possible to deal with some problems of endogeneity and provide estimates of the causal effects of the benefit. Finally, by assessing the impacts on prenatal care, we can understand the mechanism under which the transfer works.

3 Data

We use administrative data both from the BFP, more precisely the Brazilian Unified Registry for Social Programs (CadÚnico) and the BFP Payroll, and from the Brazilian Ministry of Health.

The Unified Registry has information on families with monthly per capita household income of up to half a minimum wage or total monthly family income of up to 3 minimum wages, covering approximately 36% of the Brazilian population and including all beneficiaries of the BFP. The data cointain a rich set of characteristics of the household, as well as socioeconomic and demographic characteristics of each family member. The administrative data have also personal identification variables such as the name and social identification number of each member of the registered families. Today, there are approximately 27 million families enrolled in the Unified Registry and approximately 14 million of them are beneficiaries of the BFP. The data are collected annually, usually by the end of the year, and have around 80 million observations per year.

The BFP Payroll consists of monthly data with information on all transfers that beneficiary families receive from the program, including each type and monetary value, as well as characteristics of the head of each household and family members directly associated with each benefit, which are children, teenagers and pregnant and nursing women. The administrative data have also the same personal identification variables as the Unified Registry, which allow me to link the two BFP datasets. There are around 30 million observations per month.

Regarding the data from the Brazilian Ministry of Health, we use vital statistics natality data from the National System of Information on Live Birhts (SINASC). The SINASC was developed to collect epidemiological information on live births throughout the whole country and its implementation started in 1996, with gradual improvements along the years. In addition to the information on health outcomes of the newborns such as birth weight, incidence of congenital anomalies and Apgar scores, among others, the data have information on pregnancy features like gestational age, the number of prenatal consultations and the month in which the mother started prenatal care. The SINASC data have also information on characteristics of the mothers such as date of birth, municipality of residence, marital status and education, among others.

The main challenge is to link the pregnant women that started to receive the BVG on December 2011, and therefore appear in the BFP Payroll for this month, with natality data for their newborn children. To connect the different datasets, the initial and ideal strategy was to obtain the SINASC data with personal identification variables for the mothers such as name or social identification number, which would enable us to directly link beneficiary women that appear in the BFP Payroll with their children's health outcomes. Until the moment, unfortunately, we were unable to get access to the personally identified data and had to adopt an alternative strategy. The strategy implemented was to use the information on the mother's and child's dates of birth, child's gender and the municipality of residence. With this information, we create an identification variable that allows me to connect the BFP data with SINASC. However, to collect some of identification variables in the BFP data, more precisely the child's gender and date of birth, we had to make use of the Unified Registry in addition to the BFP Payroll, as explained below.

We started by looking at the BFP Payroll data for December 2011 to identify all pregnant women that received the BVG in the first month of its implementation. We excluded women that appeared twice in the data because they presented inconsistent information and represented only 0.5% of the total number of observations. Excluding these cases, we have in total 23,735 pregnant women receiving the transfer in December 2011, which represent our ideal sample.

The next step was to connect these women with their children to get each child's gender and date of birth, which we need to link with the SINASC data. Therefore, we had to make use of the Unified Registry because there is not enought information in the Payroll data to correctly link benefificary women with their children. More precisely, in the sample of all women that received the BVG in December 2011, there is information on the pregnant women and on each household head, which can be the pregnant woman herself or not. However, there is no direct information on their children, who will only appear in the data later on after their births. We tried to deal with this challenge by looking at all births that occurred between 2010 and 2013 and appeared in the BFP Payroll at any moment of time until 2017. However, there isn't enough information to precisely infer who are their mothers in the data. In particular, these children appear in the Payroll data linked to the variable benefit for ages 0-15, and for each child we also have information on the household head. However, in many cases the household head is not the child's mother. As an example, in the sample of pregnant women that received the BVG on December 2011, 4% of the household heads were male and in 27% of the cases the household head was female but not the pregnant woman. Therefore, by using only the BFP Payroll data, it is not possible to correctly connect beneficiary women with their children.

The Unified Registry, on the other hand, has detailed information on all individuals from families enrolled in the BFP, including the names of the parents of each family member. Therefore, using the Unified Registry data we are able to directly connect every child with his or her mother. Moreover, registration in the Unified Registry is a necessary condition to be enrolled in the BFP, which means that all beneficiaries of the program that appear in the BFP Payroll data have to be in the Unified Registry data as well.

We started by looking at all births that occurred between 2010 and 2013 that appear in any of the Unified Registries that we have access, which go from 2012 to 2017. For each child and

Unified Registry that he or she appears, we use the information on the mother's name to recover her social identification number and date of birth using the same Registry. We end up with a sample of 5,789,433 children linked with their mothers' names, social identification numbers and dates of birth. We need the mother's social identification number to connect the Unified Registry with the BFP Payroll data and the children's and mothers' dates of birth to connect the BFP data with SINASC.

One characteristic of the Unified Registry and therefore of the BFP Payroll is that, for the children to appear in the data, it is necessary that their mothers or other family member register them. However, in many cases households beneficiaries of the BFP don't have incentives to register the children in the Unified Registry, especially if they wouldn't get any additional transfer from the program by doing so. In particular, this can happen with families that are receiving the maximum limit of benefits. This potential misreporting of births in the Unified Registry could explain some of the problems we had to connect the pregnant women that received the BVG in December 2011 with their children in the BFP data, which we will detail below.

As explained above, we make use of the mother's social identification number to connect the pregnant women that appear in the BFP Payroll with the sample of births from the Unified Registry already linked with the mothers. However, probably because of misreporting of births, we could only find 18,557 children for our initial sample of 23,735 pregnant women. After going back to the Unified Registry data to gather additional geographic and socioeconomic characteristics of the mother and household, which we will use as controls in the estimation, we end up with a sample of 17,881 observations. Finally, as our sample from the Unified Registry consisted of births that occurred between 2010 and 2013, we had to exclude the births that were not related to the BVG, that is, that occurred before the mother started to receive it or much later on. Therefore, we restricted our sample to the births that occurred between December 2011, which is the first month that the mother received the transfer, and September 2012, which is approximately 9 months later. After excluding births that occurred before December 2011 and after September 2012, we stay with 9,088 observations.

There are some important things to consider regarding the matching rates. First, there is evidence of an overall misreporting of births in the Unified Registry and therefore in the BFP Payroll data, which could be driven by families that are receiving the maximum limit of BFP benefits and therefore don't have incentives to register their newborn children because they won't get any additional transfer. As a matter of fact, when we excluded the births that occurred before December 2011 and after September 2012, most of the children that we eliminate were born before December 2011. The distribution of births before excluding these cases can be seen in Figure 1. The higher incidence of births that occurred before December 2011 in relation to births that happened after September 2012 could be related to the fact that mothers indeed had more incentives to register older children who were already born at the time because most likely they would start to receive a cash transfer for them immediately, especially because there was an increase in the maximum limit of variable benefits per family at the end of 2011.

The last step was to connect the data from the BFP to the SINASC data. From the 9,088 children who were born between December 2011 and September 2012 from pregnant women that received the BVG on December 2011, we could match 6,749 of them to their natality outcomes using the information on the mother's and child's dates of birth, child's gender and municipality of residence. It must be noted that in the SINASC data there is a significant number of missing values for the mother's date of birth, approximately 23% of all births that occurred from 2011 to 2012, which could explain some of the births that we cannot match.

Despite some limitations with the matching rates, we believe that the strategy implemented was the most feasible to connect the BFP data with the SINASC in the absence of natality data with personal identification variables for the mothers. In the case we had access to the SINASC identified data, it would be straightforward to directly connect all pregnant women that appear on the BFP Payroll receiving the BVG with their children's natality outcomes by using the mother's name or social identification number. However, in the absence of the identified data from SINASC, we need the child's gender and date of birth in addition to the mother's date of birth and municipality of residence to connect the different datasets. While we have the mother's date of birth and municipality of residence directly in the BFP Payroll data, we have to make use of the Unified Registry data to get the information on the children, as explained above.

Table 1 displays summary statistics for three different samples, which are numbered from 1 to 3 in the columns. The first one refers to the ideal sample, which consists of the 23,735 pregnant women that appear in the BFP Payroll receiving the BVG in December 2011. The second is the sample in which we linked these women to the births that appear in the Unified Registry and occurred between December 2011 and September 2012. The third column is the sample in which we matched these women and children with the SINASC data, which is our final sample.

The results show that, despite losing a significant number of observations after matching the three different samples, especially if we compare the first to the second, the summary statistics for the three of them are similar. Although the total value of family BFP benefits is lower for the final sample, the differences are very small. There are also some small differences between the second and the third sample regarding some characteristics of the household. In particular, in the final sample there are more people living in the Northeast region of Brazil and fewer people living in the North, South, and Southeast. However, it doesn't seem like the final sample is significantly different from the initial one.

4 Empirical Strategy and Results

The empirical strategy is to use the timing of the implementation of the BVG, which started in December 2011, and the fact that it begins to be paid when the eligible woman is identified as pregnant by the health management system of the BFP, regardless of the stage of pregnancy at the moment of the identification. This last feature is of key importance because women at different stages of pregnancy were exposed to different amounts of monthly payments before giving birth, although there is no differential income effect after nine months. As a matter of fact, women in earlier stages of pregnancy in December 2011 were more exposed to the transfer before giving birth. The identification hypothesis is that when the BVG was implemented eligible women at different stages of pregnancy were exogenously exposed to different income shocks before giving birth. We use the variation in the stage of pregnancy among mothers who started to receive the new transfer in December 2011 to calculate the total months of exposure to the BVG during pregnancy, which will define our treatment groups.

We will initially restrict the analysis for December 2011 to avoid potential problems regarding women who became pregnant in order to receive the new benefit after its implementation, which would weaken the empirical strategy. In Appendix A, we also present the results for an augmented sample which we constructed by using the BFP Payrolls from December 2011 to August 2012 and exluding the births that occurred after September 2012, i.e, births that occured more than nine months after the BVG implementation.

Figure 2 shows the distribution of births from December 2011 to September 2012 in the final sample. We can see that, among the 6,749 births, most of them occurred before April 2012. Because the number of births after April 2012 is small, we aggregated all births from April 2012 to September 2012 into the same subgroup.

For the estimation of the effects of exposures to the BVG during pregnancy on the outcomes of interest, we defined one control group and four different treatment groups. The control group was defined as the births that occurred in December 2011 (i.e, mother received less than one month of BVG). The four treatment groups are: births that occurred in January 2012 (i.e., mother received one month of the BVG during pregnancy), births that occurred in February 2012 (mother received two months), births that occurred in March 2012 (mother received three months) and births that occurred from April 2012 to September 2012 (mother received four or more months).

We estimate the following regression:

$$Y_{it} = \alpha + \beta_1 BVG_{i1} + \beta_2 BVG_{i2} + \beta_3 BVG_{i3} + \beta_4 BVG_{i4more} + X'_{it}\gamma + \varepsilon_{it}$$

where i indexes each birth and t refers to the date of birth (month/year). Y_{it} is the outcome of interest, X_{it} is a vector of characteristics of the mothers, children, and household that we will use as controls from the information on both BFP Payroll and Unified Registry data, as well as from SINASC.

The treatment variables defined by BVG_{in} are indicators for the period of exposure to the BVG during pregnancy, where n correspond to the total of months. As explained above, there are 4 possible treatments depending on the difference between the month/year of birth and December 2011. We are interested in estimating the values for the β's, which correspond to the effect of different exposures to the transfer in comparison to the control group. In particular, our main interest is to assess the effects of exposures for four or more BVG payments before giving birth, which would cover the last trimester of pregnancy, considered of key importance to determine birth weight. Therefore, our main treatment group will consist of pregnant women exposed to the transfer for four or more months during pregnancy and We are especially interested in the estimated coefficients for β_{4more}. Note that, for the control group, $BVG_{i1} = BVG_{i2} = BVG_{i3} = BVG_{i4more} = 0$. The main outcomes of interest are mean birth weight and the likelihood of low birth weight, but we will also calculate the impacts of the BVG on other health and pregnancy outcomes.

Before showing the results from the estimation, we present summary statistics for the control and the four treatment groups, as well as results from testing differences between the means of each treatment group in comparison with the control group, which are displayed in Table 2. The results show that the five groups are similar in general. There are some small difference regarding the total value of BFP benefits that the family receive, with women more exposed to the BVG during pregnancy receiving lower amounts. Mothers who received more transfers before giving birth are also less likely to live in rural areas, to be black or brown and to be single.

Table 3 shows the results from the estimation of the impact of the BVG on birth weight and on the incidence of low birth weight for five different specifications, which are displayed in the columns. The first specification refers to a simple regression model without controls. In the second specification, we added controls for geographic household variables from the Unified Registry. In particular, we controlled for the state of residence and if the household was located in a rural area. In the third specification, we also added as controls characteristics of the mother and the child, which we got from the BFP Payroll, Unified Registry and SINASC data. More precisely, we used the information on the age of the mother in December 2011 to construct 5-year brackets of age going from 14-19 years old to 40-44, as well as dummy variables indicating if the mother was single when she gave birth, if she was illiterate, black or brown and if the child was female. For the fourth specification, we controlled for the total number of people in the household, the total number of children aged 0 to 15 in the family and per capita household income, in addition to the geographic controls. Finally, in the last specification, we used the controls of the previous specifications altogether.

The results show a positive impact of exposures to BVG during pregnancy on birth weight and are robust across all specifications. The impacts are stronger for higher exposures. The estimated impacts of the main treatment (i.e, exposures to the BVG for four or more months during pregnancy) are increases between 72 and 95 grams approximately, depending on the specification. We also divided the estimated coefficients by the constant term of the regression to calculate the percentage impact in comparison with the control group. The main treatment increased birth weight by 2.22% to 3.12%. The results for the likelihood of low birth weight, which are displayed in the last five columns, are even stronger. Although the coefficients for receiving one to three months of BVG are not statistically significant in most specifications, the coefficients for the main treatment show that exposures to four or more BVG transfers during pregnancy reduced the likelihood of births with less than 2,500g between 1.6 and 1.8 percentage points depending on the specification, which represent reductions of 19% to 30% in comparison with the control group.

Comparing the main findings with other results in the literature, the estimated effects of the BVG seem to be very strong. Almond et al. (2011), for example, find that FSP participation increased birth weight between 15 and 20 grams for whites and 13 to 42 grams for blacks, which imply percentage increases between 0.5% and 0.6% for the fist group 0.4% and 1.4% for the last. As for low birth weight, they find that the program decreased the likelihood of these adverse births by 7% to 8% for whites and 5% to 12% for blacks, although the coefficients for the last group are not precisely estimated. Considering that the new transfer could also have an impact on reducing mortality for fetuses with low birth weights, which would tend to bias the results toward a negative or null effect on mean birth weight and on the incidence of low birth weight, the results seem pretty strong.

The next step is to assess the impacts of the BVG on other infant health and pregnancy outcomes. For the rest of the study, we will use the third specification as the benchmark. Table 4 shows the results for the impacts on gestational age (measured by the number of weeks of gestation), incidence of preterm births (births with less than 37 weeks of pregnancy), incidence of very low birth weight (births with less than 1,500g), Apgar scores and on the incidence of congenital anomalies.

The results for gestational age show that, although mothers who received one to three months of BVG during pregnancy presented lower gestational age than mothers who had children in the same month that they started to receive the transfer, the effects are very small. The impact for the main treatment is not statistically significant.

Looking at the effects on the incidence of preterm births, the results show that while mothers exposed to only one monthly payment before giving birth experienced increases in the incidence of these adverse births, exposures for four or more months reduced the likelihood of preterm births by approximately 4 percentage points, which represent a 35% decrease in comparison with the control group. As discussed above, considering the potential reduction in fetal mortality for preterm births, the impacts of the new transfer could be even stronger.

The results for the incidence of very low birth weight show that higher exposures to the BVG are negatively associated with the likelihood of these adverse births, although the coefficients are not precisely estimated and these births are very rare as depicted previously in Table 1. The effects for Apgar scores and congenital anomalies are not statistically significant.

4.1 Mechanisms

One important issue is to evaluate the mechanisms behind the impact of the BVG on infant health. In addition to the cash transfer, one potential and natural mechanism is the increase in prenatal care, which is a conditionality of the new transfer. If the transfer indeed had a positive impact on health outcomes but also on prenatal care, it could be that beneficiary children are healthier in part because of increased prenatal care. On the other hand, if I don't find any impact of the BVG on prenatal care, there is evidence that the effect of the transfer is mainly through the cash transfer itself. Therefore, by estimating the impact of the BVG on prenatal care, it is possible to understand the mechanism under which the transfer works.

Table 5 shows the estimated impacts of the BVG on the number of prenatal visits, the month in which the mother started prenatal care and on the likelihood of delayed prenatal care (i.e., after the third month of pregnancy). The results are very interesting because while we don't find any effect of the transfer on the number of prenatal visits, there is a negative impact on the month in which the mother started prenatal care, as well as on the likelihood of delayed prenatal care. The effects are strong and significant for all treatment groups except for mothers that received only one month of the transfer before giving birth. The impacts are also monotonic in the sense that the effects are increasingly stronger with higher exposures. While the exposure to the BVG for two months during pregnancy reduced the likelihood of delayed prenatal care in approximately 26%, exposures for three and four or more months reduced the incidence of delayed prenatal care in 42% and 68%, respectively. Therefore, there is evidence that prenatal care seems to be an important mechanism of the BVG to improve natality outcomes altogether with the direct cash transfer.

In Appendix B, we also look at potential trends in the main outcomes of interest before and after the introduction of the BVG, as well as in characteristics of the mothers such as age, marital status and education.

4.2 Distributive Effects

In this subsection, we assess how the impact of the BVG is distributed among different subgroups. In particular, we analyze if the effects are different for black or brown mothers and for those living in rural areas. We focus on the effects on mean birth weight and on the incidence of low birth weight, as well as on gestational age and prenatal care.

Table 6 shows the results for mean birth weight and for the likelihood of low birth weight. The effect of the main treatment on mean birth weight is positive and significant for all subsamples, with stronger effects for people living in rural areas and for non-black mothers, with increases of approximately 140 and 106 grams, respectively. In percentage terms, the main treatment increased birth weight for households in rural areas in approximately 4.6% in comparison with the control group, almost 2 percentage points above the average effect of 2.75%, while the increase for individuals living in urban areas was only 1.94%. The increase for non-black mothers was approximately 3.4% against 2.3% for black women. Regarding low birth weight, despite finding earlier that BVG reduced the likelihood of these adverse births on average, the results for different subgroups show that all the effect is concentrated for households in urban areas and for black mothers, which points in the opposite direction as the results for birth weight. Although the coefficients are not precisely estimated, BVG exposures for four or more months before giving birth reduced the likelihood of low birth weight in approximately 30% for women living in urban areas, which is higher than the average effect of 24%. The effects for women living in rural areas and non-black mothers are not statistically significant.

Table 7 presents the effects on gestational age and on the likelihood of preterm births. For gestational age, the results are very close to the ones for the full sample in the sense that there is a negative but modest effect of the transfer on gestational age for exposures from one to three months and no effect for the main treatment. Regarding preterm births, the effects of the main treatment are significant but concentrated for black or brown mothers and urban areas. While exposures to the BVG for four or more months during pregnancy reduced the likelihood of preterm births by 35% on average, the estimated reduction for people living in urban areas is approximately 45%.

Finally, Table 8 displays the results for prenatal care. We also have results very similar to the full sample, showing that the impacts on the number of prenatal visits are not statistically significant for all treatment groups in general. Although women living in rural areas and black and brown mothers that received only one transfer experienced reductions in the number of prenatal visits, the coefficients are not precisely estimated. As for the likelihood of delayed prenatal care, higher exposures to the BVG during pregnancy decreased the incidence of delayed prenatal care for all subgroups, with small differences among them.

5 Discussion and Future Extensions

The results of this study provide evidence that higher exposures to the BVG during pregnancy at the time of its implementation had positive impacts on birth weight and pregnancy outcomes. The impacts for mothers that were exposed to the transfer during the last trimester of pregnancy on the likelihood of low birth weight and preterm births are particularly strong. The results seem to be even stronger if we take into account that the new transfer could also have reduced fetal mortality for more adverse births. I also studied one potential mechanism under which the BVG could work, which is through an increase in prenatal care. The results for the main treatment show that, while the transfer didn't have any impact on the number of prenatal visits, it decreased the likelihood of delayed prenatal care. Therefore, part of the total effect of the new transfer can be related to an increase in prenatal care. Anyway, given that the BVG consist of monthly transfers in the amount of U$ 10, there is evidence that the new transfer is very effective for its relatively small cost.

We also assessed how the effects of the BVG differ among specific subgroups, in particular women living in rural areas and black mothers. While the effects for birth weight are positive for all subgroups but stronger for non-black mothers and in rural areas, the opposite happens when we analyze low birth weight: the effects for non-black mothers and rural areas are not statistically significant but I find strong reductions in the likelihood of low birth weight for black or brown mothers and in urban areas. The effect on reducing the likelihood of preterm births is also concentrated for these last subgroups. Similar to the results for the full sample, although we don't find any effects on the total number of prenatal visits, the transfer reduced the likelihood of delayed prenatal care for all subgroups.

Future extensions for this research are to obtain vital statistics natality and mortality data with personal identification. With the natality data with personal identification of the mothers, we will be able to directly connect all pregnant women that appear in the BFP Payroll to their children's natality outcomes by using the mother's name or social identification number, which we have on the Payroll data. With the mortality data, we will be able to assess if the new transfer impacted fetal, neonatal and infant mortality, which will enable us to disentangle the effects of the BVG on birth weight and preterm births from potential impacts on fetal mortality. In particular, if we find that the transfer indeed reduced fetal mortality for children with lower birth weights and preterm born, there is evidence that the impacts of the BVG are even stronger than the estimated.

Finally, we are also interested in analyzing the effects of the BVG on fertility and mortality. As a matter of fact, it is possible that after the introduction of the BVG women had more incentives to become pregnant to receive the extra transfer. Because of this potential problem of endogeneity which would bias the results, we focused on the month in which the new transfer was implemented. However, if we don't find significant effects of the BVG on fertility, we can also use the BFP Payrolls for later months, with more observations.

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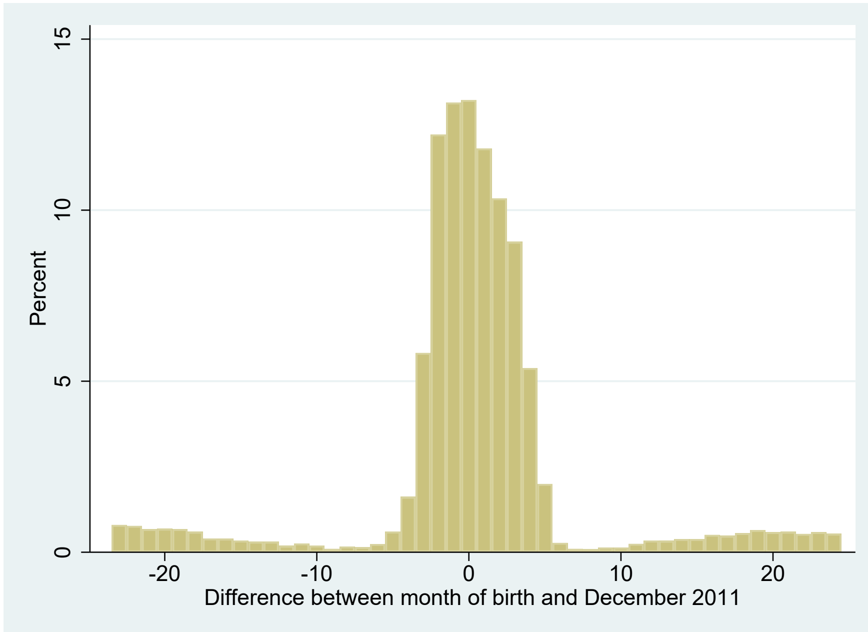
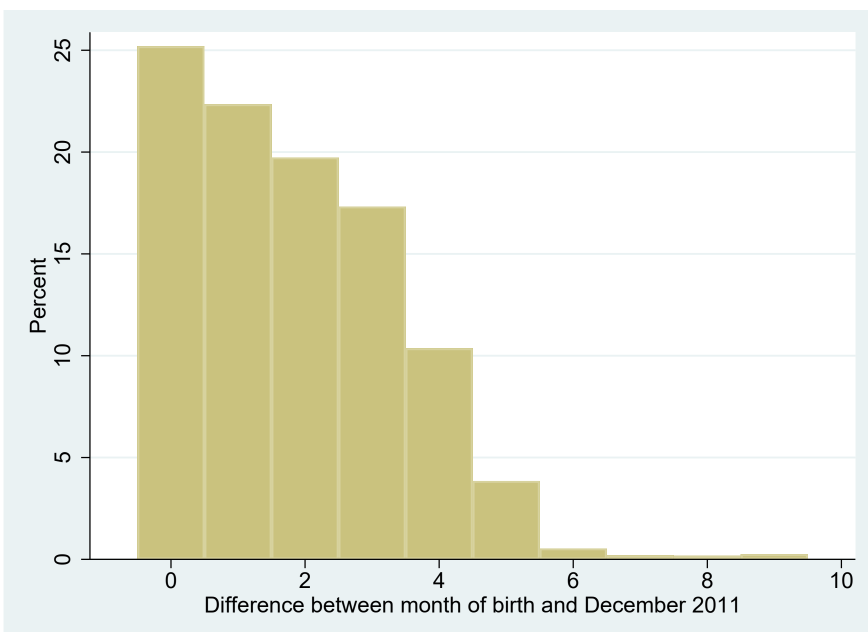
Figure 1: Distribution of births among mothers who received the BVG relative to December 2011 (in months)



Note:TBA.

Figure 2: Distribution of births among mothers who received the BVG relative to December 2011 excluding births before December 2011 and after September 2012 (in months)



Note:TBA.

Table 1: Summary Statistics

Variables	(1) Mean	(1) s.d	(2) Mean	(2) s.d	(3) Mean	(3) s.d
BFP Payroll						
Total value of BF benefits in the family	158.74	43.18	157.63	42.05	157.14	41.97
Number of BVG in the family	1.0087	0.0954	1.0070	0.08622	1.0076	0.0900
Children 0-6 receiving benefit in the family	0.6868	0.7425	0.7137	0.74555	0.7032	0.7381
Children 0-6 not receiving benefit in the family	0.0017	0.0468	0.0016	0.05448	0.0021	0.0620
Children 7-15 receiving benefit in the family	0.9639	0.9470	0.9748	0.94327	0.9658	0.9383
Children 7-15 not receiving benefit in the family	0.0010	0.0343	0.0008	0.02774	0.0009	0.0298
Children 16-17 receiving benefit in the family	0.1209	0.3437	0.1114	0.33195	0.1114	0.3312
Children 16-17 not receiving benefit in the family	0.1104	0.3284	0.0958	0.30934	0.0962	0.3110
Number of children 0-15 in the family	1.6534	1.0370	1.6909	1.03066	1.6719	1.0269
Mother's age in December 2011	26.94	6.32	27.10	6.02	27.13	6.03
Unified Registry						
Household						
North			0.1430	0.3501	0.1393	0.3463
Northeast			0.4647	0.4988	0.4925	0.5000
Southeast			0.2415	0.4280	0.2211	0.4150
South			0.1043	0.3057	0.0985	0.2981
Center-West			0.0464	0.2104	0.0486	0.2150
Rural			0.3272	0.4692	0.3243	0.4682
Per capita household income			72.58	91.18	72.21	90.20
Mother/Children						
Black/Brown			0.7329	0.44245	0.74511	0.43583
Illiterate			0.0464	0.21044	0.04431	0.20580
Children female			0.4974	0.50002	0.49963	0.50004
SINASC						
Birth weight (in grams)					3,305.51	513.06
Low birth weight (<2,500g)					0.0470	0.2116
Very low birth weight (<1,500g)					0.0031	0.0557
Gestational age (in weeks)					38.92	2.04
Preterm birth (<37 weeks)					0.0891	0.2849
Number of prenatal visits					7.84	7.24
Month in which started prenatal care					2.93	1.64
Delayed prenatal care (after month 3)					0.2501	0.4331
Apgar minute 1					8.25	1.12
Apgar minute 5					9.32	0.82
Congenital anomaly					0.0043	0.0653
Single Mother					0.4176	0.4932
Number of observations	23,735		9,088		6,749	

Notes: Table 1 shows summary statistics for three different samples, which are displayed in columns 1 to 3. Column 1 refers to the sample of pregnant women that appear in the BFP Payroll receiving the BVG in December 2011. Column 2 refers to the sample of pregnant women that received the BVG in December 2011 for which I linked with their newborn children that appear in the Unified Registry and were born between December 2011 and September 2012. Finally, column 3 refers to the sample in which I matched these women and children with the SINASC data, which corresponds to the final sample used in the estimations.

Table 2 - Differences between control and treatment groups

Variables	Control	1 month of BVG		2 months of BVG		3 months of BVG		4 or more months of BVG	
	Mean	Mean	Difference	Mean	Difference	Mean	Difference	Mean	Difference
Total value of BFP benefits in the family	159.23	160.46	1.22794	155.88	-3.35085**	152.82	-6.41229***	155.83	-3.40497**
	(41.81)	(43.45)	(1.52911)	(41.46)	(1.53442)	(41.10)	(1.57195)	(41.24)	(1.63291)
Numer of individuals in the family	4.35	4.44	0.08969*	4.33	-0.02199	4.21	-0.14371***	4.26	-0.09247
	(1.42)	(1.45)	(0.05359)	(1.44)	(0.05499)	(1.39)	(0.05533)	(1.37)	(0.05688)
Number of children 0-15 in the family	1.72	1.73	0.00794	1.67	-0.04942	1.58	-0.14099***	1.63	-0.09403**
	(1.02)	(1.06)	(0.03734)	(1.02)	(0.03757)	(1.02)	(0.03866)	(1.00)	(0.03978)
Mother's age in December 2011	27.02	27.29	0.27092	27.10	0.07998	27.09	0.07341	27.18	0.15920
	(6.09)	(6.11)	(0.21860)	(5.96)	(0.22210)	(5.94)	(0.22788)	(5.99)	(0.23742)
Rural	0.3498	0.3416	-0.00822	0.3148	-0.03501**	0.3001	-0.04970***	0.3025	-0.04726**
	(0.477)	(0.4744)	(0.01705)	(0.4646)	(0.01734)	(0.4585)	(0.01771)	(0.4596)	(0.01837)
Black/Brown Mother	0.7687	0.7412	-0.02750*	0.7504	-0.01829	0.7453	-0.02340	0.7087	-0.05998***
	(0.4218)	(0.4381)	(0.01542)	(0.4330)	(0.01578)	(0.4359)	(0.01632)	(0.4546)	(0.01740)
Illiterate	0.0506	0.0447	-0.00586	0.0423	-0.00827	0.0379	-0.01266	0.0439	-0.00670
	(0.2192)	(0.2068)	(0.00763)	(0.2014)	(0.00773)	(0.1911)	(0.00773)	(0.2049)	(0.00830)
Children female	0.4966	0.5043	0.00773	0.4811	-0.01554	0.5054	0.00875	0.5145	0.01787
	(0.5001)	(0.5001)	(0.01792)	(0.4998)	(0.01843)	(0.5002)	(0.01899)	(0.5000)	(0.01969)
Single Mother	0.4419	0.4240	-0.01794	0.4041	-0.03787**	0.4165	-0.02548	0.3894	-0.05257***
	(0.4968)	(0.4943)	(0.01793)	(0.4909)	(0.01834)	(0.4932)	(0.01895)	(0.4878)	(0.01952)
Number of previous pregnancies	2.18	2.18	0.00063	2.06	-0.12300	1.92	-0.26845***	1.96	-0.22519**
	(2.99)	(3.09132)	(0.11160)	(1.6)	(0.08829)	(1.51252)	(0.08801)	(1.60907)	(0.09092)
Observations	1,621	1,499		1,347		1,213		1,071	

Robust standard errors in parentheses
**** p<0.01, ** p<0.05, * p<0.1

Notes: Table 2 shows summary statistics and differences in means between the control group (i.e, mothers that received less than one month of BVG before giving birth) and each of the four treatment groups used in the estimations. To estimate the differences in means, we ran simple regressions on the mean of each variable controlling for a dummy variable for each of the four treatment groups separately.

Table 3: Impacts of the BVG on birth weight

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
			Birth weight (in grams)					Low birth weight (<2,500g)		
1 month of BVG	31.45203*	36.32579*	35.03564*	30.51308	29.28691	-0.00417	-0.00413	-0.00545	-0.00274	-0.00417
	(18.86087)	(18.85145)	(18.80015)	(19.63086)	(19.59320)	(0.00801)	(0.00802)	(0.00809)	(0.00846)	(0.00852)
% impact	0.96%	1.16%	1.13%	1.00%	0.96%	-7.60%	-4.99%	-7.30%	-2.91%	-4.73%
2 months of BVG	39.87124**	49.73063**	47.14273**	50.96527**	47.76534**	-0.01110	-0.01184	-0.01183	-0.01136	-0.01138
	(19.34581)	(19.49447)	(19.52065)	(20.54425)	(20.60151)	(0.00795)	(0.00801)	(0.00811)	(0.00845)	(0.00855)
% impact	1.22%	1.59%	1.52%	1.66%	1.56%	-20.22%	-14.32%	-15.84%	-12.08%	-12.90%
3 months of BVG	10.34029	24.48031	21.55883	28.27893	24.44718	-0.01204	-0.01374*	-0.01472*	-0.01230	-0.01356
	(18.99089)	(19.05515)	(19.07143)	(20.01332)	(20.06516)	(0.00812)	(0.00819)	(0.00826)	(0.00865)	(0.00872)
% impact	0.32%	0.78%	0.69%	0.92%	0.80%	-21.93%	-16.61%	-19.71%	-13.08%	-15.37%
4 or more months of BVG	72.66605***	85.37065***	85.45630***	95.59135***	94.19544***	-0.01659**	-0.01789**	-0.01793**	-0.01797**	-0.01801**
	(19.92414)	(19.99331)	(20.08229)	(20.99504)	(21.11624)	(0.00815)	(0.00825)	(0.00838)	(0.00867)	(0.00880)
% impact	2.22%	2.72%	2.75%	3.12%	3.08%	-30.22%	-21.63%	-24.00%	-19.10%	-20.41%
Constant	3,277.18816***	3,136.60086***	3,109.71810***	3,065.71499***	3,060.96066***	0.05490***	0.08271***	0.07470***	0.09406***	0.08825***
	(13.04657)	(27.21779)	(34.39433)	(36.98083)	(42.40521)	(0.00566)	(0.01411)	(0.01750)	(0.01812)	(0.02143)
Observations	6,749	6,749	6,629	6,137	6,137	6,749	6,749	6,629	6,137	6,137
R-squared	0.00225	0.01823	0.04216	0.02199	0.04570	0.00079	0.00702	0.01178	0.00737	0.01288
Geographic Controls	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Mother and Children Controls	No	No	Yes	No	Yes	No	No	Yes	No	Yes
Add Controls	No	No	No	Yes	Yes	No	No	No	Yes	Yes

Robust standard errors in parentheses
**** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: Table 3 shows the results of the estimation of the impacts of different exposures to the BVG during pregnancy on mean birth weight and on the incidence of low birth weight (i.e, children born with less than 2,500g). The estimated coefficients show the impact of different exposures to the BVG in comparison with the control group (i.e, mothers what received less than one month of the BVG before giving birth). We also divided the estimated coefficients by the constant term of the regression to calculate the percentage impact of different exposures in comparison with the control group. Columns 1 to 5 show the impacts on mean birth weight and columns 6 to 10 on the incidence of low birth weight for five different specifications. The first specification refers to a simple regression model without controls. In the second specification, we controlled for geographic characteristics of the household, more precisely the state of residence and if the household was located in a rural area. In the third specification, we also controlled for the age of the mother in December 2011 and dummy variables indicating if the mother was single when she gave birth, if she was illiterate, black or brown and if the child was female. In the forth specification, we controlled for the total number of people in the household, the total number of children aged 0 to 15 in the family and per capita household income in addition to the geographic controls. In the last specification, we used the controls of the previous specifications altogether.

Table 4: Impacts of the BVG on pregnancy and health outcomes

Variables	(1) Gestational age (in weeks)	(2) Preterm	(3) Very low birth weight	(4) Apgar 1	(5) Apgar 5	(6) Congenital anomaly
1 month of BVG	-0.47738***	0.02379**	-0.00153	-0.04517	-0.02844	0.00182
	(0.08041)	(0.01114)	(0.00249)	(0.04102)	(0.02998)	(0.00247)
2 months of BVG	-0.50408***	0.01794	-0.00329	-0.03281	-0.00720	-0.00244
	(0.08210)	(0.01135)	(0.00236)	(0.04255)	(0.03057)	(0.00194)
3 months of BVG	-0.28296***	-0.00421	-0.00475**	-0.03091	-0.03773	0.00457
	(0.08112)	(0.01120)	(0.00217)	(0.04279)	(0.03209)	(0.00322)
4 or more months of BVG	-0.03626	-0.03819***	-0.00356	-0.04258	-0.02616	-0.00209
	(0.07986)	(0.01004)	(0.00235)	(0.04497)	(0.03332)	(0.00210)
Constant	38.93155***	0.10901***	0.00991**	8.44899***	9.51162***	0.00509
	(0.14726)	(0.02073)	(0.00468)	(0.07363)	(0.05079)	(0.00445)
Observations	6,315	6,315	6,629	6,238	6,216	6,439
R-squared	0.02232	0.01538	0.00595	0.03434	0.05424	0.00565
Geographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Mother and Children Controls	Yes	Yes	Yes	Yes	Yes	Yes
Add Controls	No	No	No	No	No	No

Robust standard errors in parentheses
**** p<0.01, ** p<0.05, * p<0.1

Notes: Table 4 shows the results of the estimation of the impacts of different exposures to the BVG during pregnancy on gestational age, the incidence of preterm births (i.e, births with less than 37 weeks of pregnancy), very low birth weight (i.e, children born with less than 1,500g), Apgar scores and on the incidence of congenital anomalies. The estimated coefficients show the impact of different exposures to the BVG in comparison with the control group (i.e, mothers what received less than one month of the BVG before giving birth). We used as controls geographic characteristics of the household, more precisely the state of residence and if the household was located in a rural area, as well as the age of the mother in December 2011 and dummy variables indicating if the mother was single when she gave birth, if she was illiterate, black or brown and if the child was female.

Table 5: Impacts of the BVG on prenatal care

Variables	(1) Prenatal visits	(2) Month which started prenatal care	(3) Delayed prenatal care
1 month of BVG	-0.39633	-0.01224	-0.00876
	(0.26970)	(0.06061)	(0.01735)
2 months of BVG	0.01343	-0.18848***	-0.06539***
	(0.30338)	(0.06050)	(0.01709)
3 months of BVG	0.10465	-0.25458***	-0.10664***
	(0.30921)	(0.06555)	(0.01721)
4 or more months of BVG	0.38252	-0.54013***	-0.17177***
	(0.30649)	(0.06371)	(0.01619)
Constant	8.93793***	2.71521***	0.25204***
	(0.56461)	(0.10795)	(0.02968)
Observations	6,490	6,114	6,114
R-squared	0.01606	0.05275	0.05339
Geographic Controls	Yes	Yes	Yes
Mother and Children Controls	Yes	Yes	Yes
Add Controls	No	No	No

Robust standard errors in parentheses
**** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: Table 5 shows the results of the estimation of the impacts of different exposures to the BVG during pregnancy on the number of prenatal visits, the month which the mother started prenatal care and on the incidence of delayed prenatal care (i.e, after the third month of pregnancy). The estimated coefficients show the impact of different exposures to the BVG in comparison with the control group (i.e, mothers what received less than one month of the BVG before giving birth). We used as controls geographic characteristics of the household, more precisely the state of residence and if the household was located in a rural area, as well as the age of the mother in December 2011 and dummy variables indicating if the mother was single when she gave birth, if she was illiterate, black or brown and if the child was female.

Table 6: Distributive impacts of the BVG on birth weight

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Birth Weight				Low birth weight			
Variables	**Rural**	**Urban**	**Black/Brown**	**Non Black**	**Rural**	**Urban**	**Black/Brown**	**Non Black**
1 month of BVG	56.95118*	23.12865	42.66624**	14.22911	-0.01221	-0.00208	-0.01060	0.00919
	(30.77283)	(23.74549)	(21.49872)	(39.25793)	(0.01146)	(0.01075)	(0.00914)	(0.01741)
% impact	1.86%	0.74%	1.37%	0.46%	-22.00%	-2.84%	-13.29%	10.35%
2 months of BVG	50.13279	42.86436*	48.81937**	37.14457	0.00171	-0.01846*	-0.01170	-0.01211
	(34.65493)	(23.80608)	(22.77221)	(38.12538)	(0.01310)	(0.01034)	(0.00925)	(0.01688)
% impact	1.64%	1.37%	1.57%	1.20%	3.08%	-25.19%	-14.67%	-13.63%
3 months of BVG	33.64006	15.59992	26.81945	10,43213	-0.00125	-0.02227**	-0.01259	-0.02033
	(33.37708)	(23.31842)	(21.94248)	(38.68777)	(0.01318)	(0.01045)	(0.00951)	(0.01647)
% impact	1.10%	0.50%	0.86%	0.34%	-2.25%	-30.39%	-15.78%	-22.89%
4 or more months of BVG	139.90117***	60.93880**	72.86109***	105.82780***	-0.01053	-0.02194**	-0.01708*	-0.01935
	(35.30774)	(24.65248)	(23.62551)	(39.05563)	(0.01316)	(0.01075)	(0.00979)	(0.01652)
% impact	4.57%	1.94%	2.34%	3.42%	-18.97%	-29.94%	-21.41%	-21.78%
Constant	3,061.22883***	3,136.15642***	3,109.60896***	3,094.95225***	0,05551	0.07328***	0.07976***	0.08883***
	(91.03354)	(39.24507)	(44.05426)	(59.36889)	(0.05428)	(0.01978)	(0.02256)	(0.03159)
Observations	2,133	4,496	4,932	1,697	2,133	4,496	4,932	1,697
R-squared	0.05257	0.04307	0.04361	0.06335	0.01574	0.01694	0.01634	0.02300
Geographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mother and Children Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Add Controls	No	No	No	No	No	No	No	No

Robust standard errors in parentheses
**** p<0.01, ** p<0.05, * p<0.1

Notes: Table 6 shows the results of the estimation of the impacts of different exposures to the BVG during pregnancy on mean birth weight and on the incidence of low birth weight (i.e, children born with less than 2,500g) for different subgroups, more precisely households located in rural and urban areas and black or brown and non-black mothers. The estimated coefficients show the impact of different exposures to the BVG in comparison with the control group (i.e, mothers what received less than one month of the BVG before giving birth). We also divided the estimated coefficients by the constant term of the regression to calculate the percentage impact of different exposures in comparison with the control group. For the impacts for households living in rural and urban areas, we controlled for the state of residence, the age of the mother in December 2011 and dummy variables indicating if the mother was single when she gave birth, if she was illiterate, black or brown and if the child was female. For the impacts for black or brown and non-black mothers, we controlled for the state of residence and if the household was located in a rural area, as well as for the age of the mother in December 2011 and dummy variables indicating if the mother was single when she gave birth, if she was illiterate and if the child was female.

Table 7: Distributive impacts of the BVG on pregnancy outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Gestational Age				Preterm			
Variables	**Rural**	**Urban**	**Black/Brown**	**Non Black**	**Rural**	**Urban**	**Black/Brown**	**Non Black**
1 month of BVG	-0.34350**	-0.54132***	-0.47052***	-0.54258***	0.02333	0.02409*	0.02345*	0.03199
	(0.13711)	(0.09924)	(0.09537)	(0.14991)	(0.01850)	(0.01395)	(0.01319)	(0.02094)
2 months of BVG	-0.66011***	-0.42057***	-0.40142***	-0.84738***	0.04125**	0.00648	0.00489	0.06032**
	(0.14800)	(0.09853)	(0.09393)	(0.16994)	(0.02055)	(0.01367)	(0.01295)	(0.02371)
3 months of BVG	-0.43302***	-0.22719**	-0.27049***	-0.39079**	0.01826	-0.01343	-0.00037	-0.00743
	(0.14640)	(0.09790)	(0.09453)	(0.16177)	(0.02023)	(0.01349)	(0.01332)	(0.02093)
4 or more months of BVG	-0.12974	-0.00233	0.05268	-0.34941**	-0.02348	-0.04340***	-0.04917***	0.00312
	(0.14579)	(0.09614)	(0.09389)	(0.15246)	(0.01803)	(0.01224)	(0.01161)	(0.02035)
Constant	38.37335***	39.07193***	38.76629***	39.40010***	0.13573**	0.09642***	0.14470***	0.05815*
	(0.51201)	(0.16483)	(0.19940)	(0.23701)	(0.06877)	(0.02286)	(0.02818)	(0.03304)
Observations	2,016	4,299	4,682	1,633	2,016	4,299	4,682	1,633
R-squared	0.03803	0.02611	0.02328	0.04716	0.03288	0.01443	0.02000	0.02822
Geographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mother and Children Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Add Controls	No	No	No	No	No	No	No	No

Robust standard errors in parentheses
**** p<0.01, ** p<0.05, * p<0.1

Notes: Table 7 shows the results of the estimation of the impacts of different exposures to the BVG during pregnancy on gestational age and on the incidence of preterm births (i.e, births with less than 37 weeks of pregnancy) for different subgroups, more precisely households located in rural and urban areas and black or brown and non-black mothers. The estimated coefficients show the impact of different exposures to the BVG in comparison with the control group (i.e, mothers what received less than one month of the BVG before giving birth). For the impacts for households living in rural and urban areas, we controlled for the state of residence, the age of the mother in December 2011 and dummy variables indicating if the mother was single when she gave birth, if she was illiterate, black or brown and if the child was female. For the impacts for black or brown and non-black mothers, we controlled for the state of residence and if the household was located in a rural area, as well as for the age of the mother in December 2011 and dummy variables indicating if the mother was single when she gave birth, if she was illiterate and if the child was female.

Table 8: Distributive impacts of the BVG on prenatal care

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Prenatal visits				Delayed prenatal care			
Variables	**Rural**	**Urban**	**Black/Brown**	**Non Black**	**Rural**	**Urban**	**Black/Brown**	**Non Black**
1 month of BVG	-0.74522*	-0.23954	-0.52594*	0.06186	0.03580	-0.03078	-0.02374	0.04198
	(0.42991)	(0.35447)	(0.31060)	(0.59059)	(0.03060)	(0.02099)	(0.02029)	(0.03364)
2 months of BVG	-0.39561	0.19051	-0.01977	0.28208	-0.03841	-0.07405***	-0.07124***	-0.04175
	(0.48407)	(0.39328)	(0.36633)	(0.50255)	(0.03082)	(0.02064)	(0.02010)	(0.03312)
3 months of BVG	0.01800	0.15249	0.04502	0.44508	-0.09789***	-0.11197***	-0.10477***	-0.10196***
	(0.49274)	(0.38916)	(0.37454)	(0.50906)	(0.03104)	(0.02067)	(0.02035)	(0.03208)
4 or more months of BVG	0.54399	0.29141	0.17069	1.11598**	-0.18360***	-0.16815***	-0.17202***	-0.16407***
	(0.57857)	(0.36030)	(0.37102)	(0.55035)	(0.02889)	(0.01951)	(0.01950)	(0.02924)
Constant	8.11254***	8.80137***	8.93454***	8.63422***	0.39523***	0.23733***	0.28461***	0.24050***
	(1.06118)	(0.61511)	(0.78322)	(0.81406)	(0.11258)	(0.03291)	(0.03761)	(0.04968)
Observations	2,070	4,420	4,815	1,675	1,975	4,139	4,516	1,598
R-squared	0.04128	0.01668	0.01732	0.03559	0.07628	0.04904	0.05497	0.06067
Geographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mother and Children Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Add Controls	No	No	No	No	No	No	No	No

Robust standard errors in parentheses
**** p<0.01, ** p<0.05, * p<0.1

Notes: Table 8 shows the results of the estimation of the impacts of different exposures to the BVG during pregnancy on the number of prenatal visits and on the incidence of delayed prenatal care (i.e, after the third month of pregnancy) for different subgroups, more precisely households located in rural and urban areas and black or brown and non-black mothers. The estimated coefficients show the impact of different exposures to the BVG in comparison with the control group (i.e, mothers what received less than one month of the BVG before giving birth). For the impacts for households living in rural and urban areas, we controlled for the state of residence, the age of the mother in December 2011 and dummy variables indicating if the mother was single when she gave birth, if she was illiterate, black or brown and if the child was female. For the impacts for black or brown and non-black mothers, we controlled for the state of residence and if the household was located in a rural area, as well as for the age of the mother in December 2011 and dummy variables indicating if the mother was single when she gave birth, if she was illiterate and if the child was female.

Appendix A - Augmented Sample

In this section, we present the main results for an augmented sample that we constructed by looking at all pregnant women that appear receiving the BVG in any of the BFP Payrolls from December 2011 to August 2012 and excluding births that occurred before December 2011 and after September 2012, which is approximately nine months after the introduction of the BVG. We also used the first month in which each mother appears in the BFP Payroll receiving the BVG to exclude births that happened before she started to receive the transfer.

There are pros and cons in using the augmented sample. On the one hand, in the augmented sample we can find more births, especially for children born between April 2012 and September 2012. While in the sample for the December 2011 Payroll we have 6,749 births, only 16% of them occurring between April 2012 and September 2012, in the augmented sample we have 54,757 births, appproximately 70% of them ocurring between April 2012 and September 2012. As displayed in Figure A1, the distribution of births by each month/year of birth is much more uniform in the augmented sample than in the initial sample, which is displayed in Figure 2.

On the other hand, when we calculate the total number of months that each mother received the BVG during pregnancy, which we did by using the difference betwen the child's date of birth and the first time that the mother appeared in the BFP Payroll reiving the transfer, we have a very similar distribution to the initial sample, which is possible to be seen by comparing Figure A2 with Figure 2. In particular, in the augmented sample most of the mothers also received between zero and three months of the BVG before giving birth. Because of this, similar to what we did for the initial sample, we aggregated mothers exposed to four or more months of the new transfer during pregnancy into the same treatment group. Additionally, it seems that there was some administrative problem in the BFP management system in the months of March 2012 and April 2012. Looking at the distribution of the first month in which each mother appears in the BFP Payroll receiving the BVG, which is displayed in Figure A3, it is possible to see that there are very few women who started to receive the new transfer in these two months. The same pattern appears when we look at the distributions of the first month that each mother appears in the BFP Payroll receiving the BVG by month/year of birth, which are displayed in the panels of figure A4.

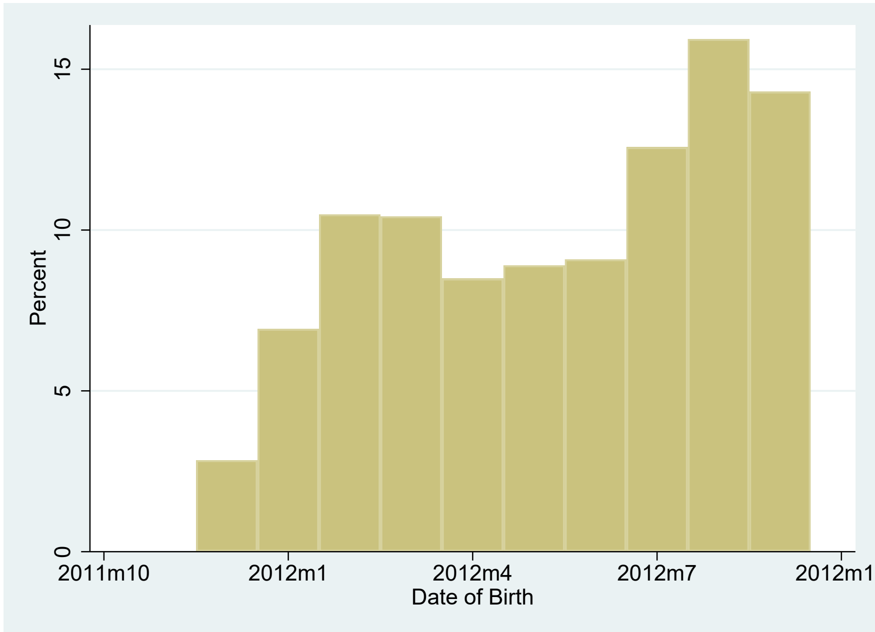
Tables A1 to A3 show the impacts of exposures to the BVG during pregnancy on the main outcomes of interest for the augmented sample. We used the third specification for the econometric model, controlling for state of residence, mother's age and dummy variables indicating if the household was located in a rural area, if the mother was single when she gave birth, if she was illiterate, black or brown and if the child was female.

Table A1 shows the results for mean birth weight and for the incidence of low and very low birth weight. Similar to the results for the sample of December 2011, exposures to the BVG during pregnancy impacted positively birth weight. Although the magnitudes of the effects are lower in the augmented sample, the coefficients for all treatment groups reach statistical significance and are increasingly monotonic with higher exposures. The results for the incidence of low birth weight are also similar, with the main treatment reducing the likelihood of these adverse births by approximately 27%. Regarding the incidence of very low birth weight, the results for the augmented sample are stronger, with significant reductions of 49% and 70% in the likelihood of these births for mothers receiving three and four or more transfers before giving birth, respectively.

Table A2 presents the results for gestational age, the incidence of preterm biths, Apgar scores and the incidence of congenital anomalies. The biggest differences between the results for the augmented and the sample for December 2011 are on the impacts on gestational age and on the incidence of preterm births. In particular, the results for the augmented sample are stronger and more robust. While in the initial sample mothers that received one to three months of the BVG presented small reductions in gestational age and the impact of the main treatment wasn't statistically significant, the results for the augmented sample show increases in gestational age for mothers exposed from two to four or more months, although the magnitudes are also small. Regarding the likelihood of preterm births, while in the initial sample mothers that received only one month of the transfer had increases in the incidence of these births and mothers who received four or more transfers experienced decreases, in the augmented sample all treatment groups experienced reductions in the incidence of preterm births, with stronger impacts for higher exposures. The results for Apgar scores and congenital anomalies are very similar in both samples, with no significant effects of the BVG.

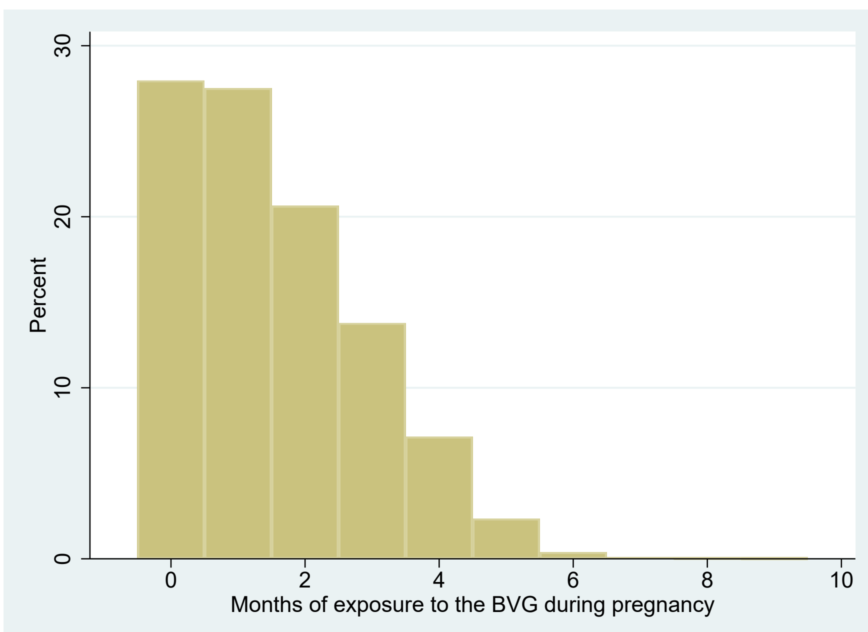
Finally, Table A3 shows the impacts on the number of prenatal visits, the month in which the mother started prenatal care and on delayed prenatal care. It is interesting to note that, while in the initial sample we didn't find any effect of the BVG on the number of prenatal visits, the results for the augmented sample show increases in the number of prenatal consultations for all treatment groups, although the magnitudes are modest. The effects on the month in which the mother started prenatal care and on delayed prenatal care are similar to the results for the initial sample in the sense that exposures to the BVG decreased the month in which the mother started prenatal care and the likelihood of delayed prenatal care. The impacts are also stronger with higher exposures. The main difference is that in the augmented sample the coefficient for receiving only one transfer during pregnancy reachs statistically significance.

Figure A1: Distribution of births by year/month of birth - Augmented Sample



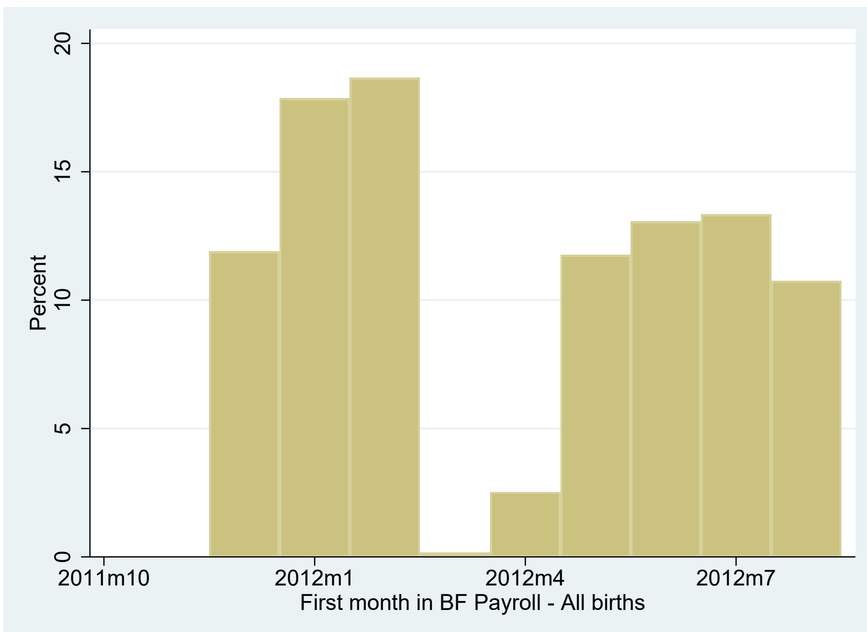
Note:TBA.

Figure A2: Number of months that the mother received BVG before giving birth - Augmented Sample



Note:TBA.

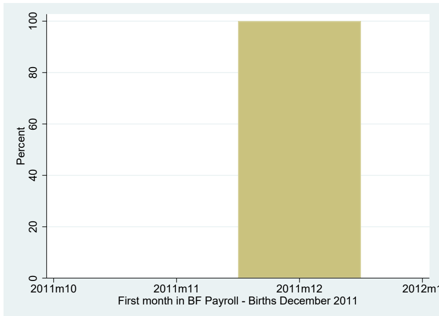
Figure A3: First month that the mother appears in the BFP Payroll - Augmented Sample



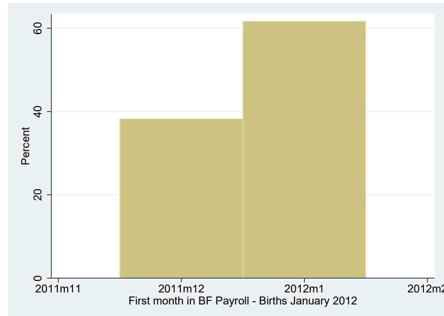
Note:TBA.

Figure A4: First month that the mother appears in the BFP Payroll by date of birth - Augmented Sample

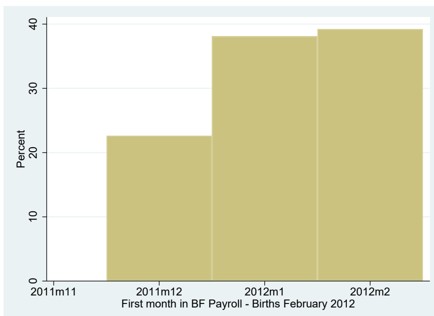
(a) Births December 2011



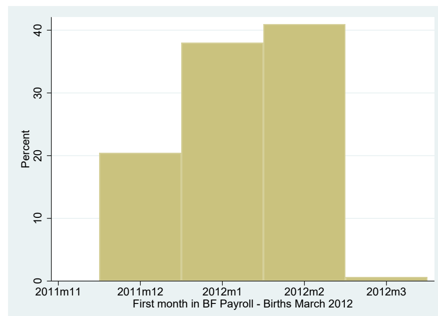
(b) Births January 2012



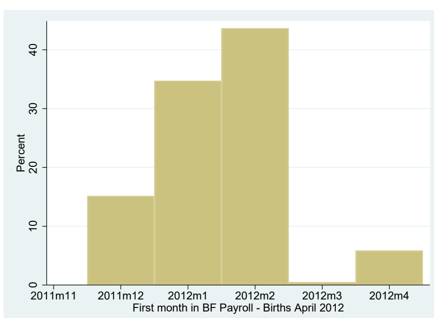
(c) Births February 2012



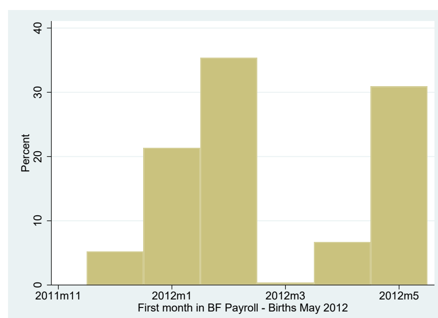
(d) Births March 2012



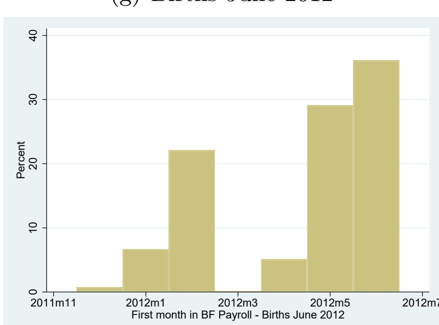
(e) Births April 2012



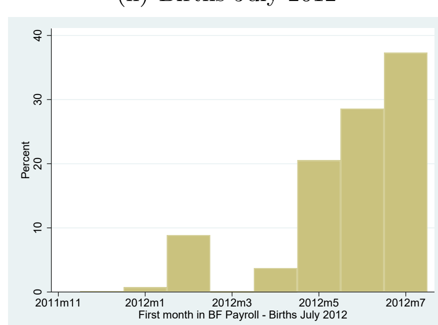
(f) Births May 2012



(g) Births June 2012



(h) Births July 2012



(i) Births August 2012

(j) Births September 2012

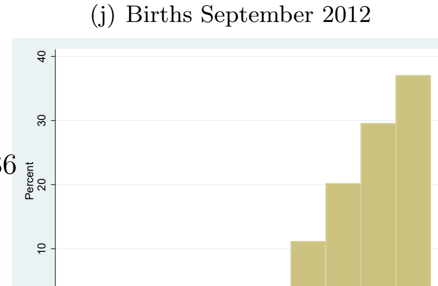


Table A1: Impacts of the BVG on birth weight - Augmented Sample

Variables	(1) Birth weight	(2) Low birth weight	(3) Very low birth weight
1 month of BVG	14.02582**	-0.00238	-0.00137*
	(5.81045)	(0.00261)	(0.00071)
% impact	0.44%	-3.08%	-26.25%
2 months of BVG	19.30319***	-0.00451	-0.00056
	(6.28680)	(0.00278)	(0.00080)
% impact	0.61%	-5.84%	-10.73%
3 months of BVG	30.69045***	-0.01198***	-0.00257***
	(6.91830)	(0.00300)	(0.00075)
% impact	0.97%	-15.53%	-49.23%
4 or more months of BVG	46.12774***	-0.02106***	-0.00366***
	(7.50901)	(0.00311)	(0.00069)
% impact	1.46%	-27.29%	-70.11%
Constant	3,155.25019***	0.07716***	0.00522***
	(9.81519)	(0.00479)	(0.00120)
Observations	54,757	54,757	54,757
R-squared	0.03140	0.00636	0.00112
Geographic Controls	Yes	Yes	Yes
Mother and Children Controls	Yes	Yes	Yes
Add Controls	No	No	No

Robust standard errors in parentheses
**** p<0.01, ** p<0.05, * p<0.1

Notes: Table A1 shows the results of the estimation of the impacts of different exposures to the BVG during pregnancy on mean birth weight and on the incidence of low birth weight (i.e, children born with less than 2,500g) and very low birth weight (i.e, children born with less than 1,500g) for the sample of all births that appear in the BFP Payrolls until August 2011 and that occurred until September 2011. We also excluded births that occurred before the mother started to receive the BVG. The estimated coefficients show the impact of different exposures to the BVG in comparison with the control group (i.e, mothers what received less than one month of the BVG before giving birth). We also divided the estimated coefficients by the constant term of the regression to calculate the percentage impact of different exposures in comparison with the control group. We used as controls geographic characteristics of the household, more precisely the state of residence and if the household was located in a rural area, as well as the age of the mother in December 2011 and dummy variables indicating if the mother was single when she gave birth, if she was illiterate, black or brown and if the child was female.

Table A2: Impacts of the BVG on pregnancy and health outcomes - Augmented Sample

	(1)	(2)	(3)	(4)	(5)
Variables	**Gestational Age**	**Preterm**	**Apgar 1**	**Apgar 5**	**Congenital anomaly**
1 month of BVG	0.02805	-0.01123***	0.00980	0.00398	0.00072
	(0.02461)	(0.00363)	(0.01367)	(0.00975)	(0.00079)
2 months of BVG	0.07124***	-0.01668***	0.01716	0.00743	0.00036
	(0.02608)	(0.00385)	(0.01441)	(0.01034)	(0.00084)
3 months of BVG	0.18436***	-0.03228***	0.02995*	0.01161	0.00181*
	(0.02833)	(0.00413)	(0.01614)	(0.01156)	(0.00107)
4 or more months of BVG	0.39057***	-0.05422***	0.03666**	0.01839	0.00144
	(0.03027)	(0.00416)	(0.01801)	(0.01320)	(0.00118)
Constant	38.42120***	0.14250***	8.33948***	9.41029***	0.00841***
	(0.04164)	(0.00635)	(0.02357)	(0.01666)	(0.00163)
Observations	52,287	52,287	52,211	52,150	53,157
R-squared	0.01084	0.00656	0.02404	0.03763	0.00125
Geographic Controls	Yes	Yes	Yes	Yes	Yes
Mother and Children Controls	Yes	Yes	Yes	Yes	Yes
Add Controls	No	No	No	No	No

Robust standard errors in parentheses
**** p<0.01, ** p<0.05, * p<0.1

Notes: Table A2 shows the results of the estimation of the impacts of different exposures to the BVG during pregnancy on gestational age, the incidence of preterm births (i.e, births with less than 37 weeks of pregnancy), Apgar scores and on the incidence of congenital anomalies for the sample of all births that appear in the BFP Payrolls until August 2011 and that occurred until September 2011. We also excluded births that occurred before the mother started to receive the BVG. The estimated coefficients show the impact of different exposures to the BVG in comparison with the control group (i.e, mothers what received less than one month of the BVG before giving birth). We used as controls geographic characteristics of the household, more precisely the state of residence and if the household was located in a rural area, as well as the age of the mother in December 2011 and dummy variables indicating if the mother was single when she gave birth, if she was illiterate, black or brown and if the child was female.

Table A3: Impacts of the BVG on prenatal care - Augmented Sample

Variables	(1) **Prenatal visits**	(2) **Month which started prenatal care**	(3) **Delayed prenatal care**
1 month of BVG	0.19884**	-0.08267***	-0.02878***
	(0.07895)	(0.01749)	(0.00517)
2 months of BVG	0.39301***	-0.19443***	-0.06653***
	(0.08438)	(0.01844)	(0.00538)
3 months of BVG	0.51100***	-0.31845***	-0.10508***
	(0.08743)	(0.02075)	(0.00575)
4 or more months of BVG	0.64714***	-0.50081***	-0.14934***
	(0.09232)	(0.02144)	(0.00577)
Constant	8.38662***	2.63710***	0.22677***
	(0.12560)	(0.02924)	(0.00840)
Observations	53,831	50,829	50,829
R-squared	0.01581	0.03745	0.03372
Geographic Controls	Yes	Yes	Yes
Mother and Children Controls	Yes	Yes	Yes
Add Controls	No	No	No

Robust standard errors in parentheses
**** p<0.01, ** p<0.05, * p<0.1

Notes: Table A3 shows the results of the estimation of the impacts of different exposures to the BVG during pregnancy on the number of prenatal visits, the month which the mother started prenatal care and on the incidence of delayed prental care (i.e, after the third month of pregnancy) for the sample of all births that appear in the BFP Payrolls until August 2011 and that occurred until September 2011. We also excluded births that occurred before the mother started to receive the BVG. The estimated coefficients show the impact of different exposures to the BVG in comparison with the control group (i.e, mothers what received less than one month of the BVG before giving birth). We used as controls geographic characteristics of the household, more precisely the state of residence and if the household was located in a rural area, as well as the age of the mother in December 2011 and dummy variables indicating if the mother was single when she gave birth, if she was illiterate, black or brown and if the child was female.

Appendix B - Trends in health outcomes and characteristics of the mothers

In this section, we assess potential trends in some of the main health outcomes before and after the implementation of the BVG to examine if its impacts can be related to other factors external to the transfer. In particular, we focus on birth weight and on the incidence of low birth weight and preterm births. We started by looking at all births registered in SINASC that occurred from 2010 to 2014 and calculating each of the above outcomes by month/year of birth. Then, we compared differences in each outcome by month/year of birth in comparison with births that occurred in December 2011, which is the month that we used to define our control group in the estimation. We did this exercise for births occuring between 2011 and 2012 and using four different samples: all births registered in SINASC; births from mothers with 0 to 7 years of schooling; births from mothers with 0 to 3 years of schooling; and births that I could also find in the Unified Registry data. The ideia is that, as we move from the SINASC full sample of births to the Unified Registry sample, we are getting closer to the final sample that we used in the estimations. It is important to note that the most relevant period for the comparison is for births occuring between December 2011 and September 2012, which I used to define our control and treatment groups.

Figures B1 and B2 show the results for birth weight. We can see in Figure B1 that, despite some evidence of seasonality, with births that occur in the first months of each year presenting lower birth weight on average, it doesn't seem to exist any trend in mean birth weight from 2010 to 2014. Figure B2 displays the estimated coefficients of the regressions for mean birth weight by each month/year of birth in comparison with births that occurred in December 2011, as well as the confidence intervals. While there are some differences in the SINASC full sample, there doesn't seem to be any difference for the sample of less educated mothers and especially for the sample of births that we find in the Unified Registry. The results for the incidence of low birth weight, which are are displayed in Figures B3 and B4, are very similar.

To analyze the results for the incidence of preterm births, it is necessary to be more careful because the SINASC data presented some important changes between the years of 2010 and 2012. In particular, the Ministry of Health changed the way some of the variables were reported, including gestational age, as part of the implementation of a new form which started to be used in 2011. The SINASC data for 2011 is also different from the data for 2010 and 2012 because, although the new form started to be used in that year, the old form was still used in 42% of the cases. While in the old form gestational age was captured in different intervals of weeks of gestation, in the new form the variable started to be captured as the discrete number of weeks of gestation. In the new form, the discrete values for the number of weeks of gestation are also grouped into the same intervals used in the old form to enable comparisons among different years. However, as displayed in Figure B5, there was a significant increase in the incidence of preterm births from 2010 to 2012. Altough the number of missing values for gestational age also increased from 0.6% in 2010 to 3.85% 2011 and 4.84% in 2012, the results doesn't change if we exclude them, as is possible to see by comparing the two panels from the Figure.

According to a report from the Brazilian Ministry of Health about the SINASC 2011, the increased magnitude of preterm births is probably related to an improvement in the quality of the information from SINASC. As a matter of fact, different Brazilian surveys show that until 2010 the SINASC data underestimated the incidence of preterm births (Silveira et al., 2013). The fact that the old form was still being used in 2011 also can explain the increases in the incidence of preterm births from 2011 to 2012. Moreover, after 2012 the series for preterm births seem to have an uniform pattern.

Even with overall increases in the incidence of preterm births from 2010 to 2012, we believe that the main results for the estimation of the effects of the BVG on the likelihood of preterm births are not affected significantly by problems in the data because we are comparing children that were born between December 2011 and September 2012. Figure B6 shows the differences in the incidence of preterm births by each month/year of birth in comparison with births that occurred in December 2011. Looking at our period of interest, there is a higher incidence of preterm births between January 2012 and September 2012 in comparison with December 2011 for all samples. Therefore, there seems to be a overall increase in preterm births in the period that we used to define our treatment groups, which could bias the results towards a negative treatment effect. Indeed, our results show that beneficiary women that had birth on January 2012 presented higher incidence of preterm births, which can be related to the the overall increase for this month. However, the results for our main treatment show a decrease in the likelihood of preterm births for mothers exposed to four or more months before giving birth, which go in the opposite direction of what is seen in Figure B6. Indeed, although there was an overall increase in the likelihood of preterm births for our period of interest, we find that higher exposures to the BVG reduced the incidence of these births, which is also a robust finding.

Finally, we also assessed the correlation between year/month of birth and some characteristics of the mothers, following evidence that the correlations between season of birth and later health outcomes found in the literature is driven mainly by differences in characteristics of the mothers such as age, marital status and education (Buckles and Hungerman, 2013). We also did the same exercise as explained above and calculated differences in characteristics by each month/year of birth in comparison to December 2011. In particular, we assessed differences in the percentage of mothers who are teenagers, married and with a high school diploma (12 or more years of education). We used two different samples: all births from the SINASC data that occurred between 2011 and 2012 and the births that we could find in the Unified Registry.

The results for the percentage of mothers who gave birth as teengares are displayed in Figure B7. Although there are some differences in the SINASC full sample, the results for the Unified Registry don't show significant differences, especially comparing birhts that occured in December 2011 with births that occurred from January 2012 to September 2012, which is the period that I used in our estimation. Figures B8 and B9 show the results for the percentage of mothers who were married and who had a high school diploma. While we also find small differences in the SINASC full sample, there is no evidence of differences in the sample of births that appear in the Unified Registry.

Figure B1: Birth weight by date of birth - SINASC 2010 to 2014

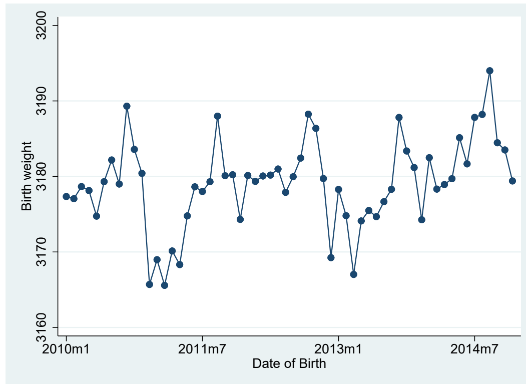
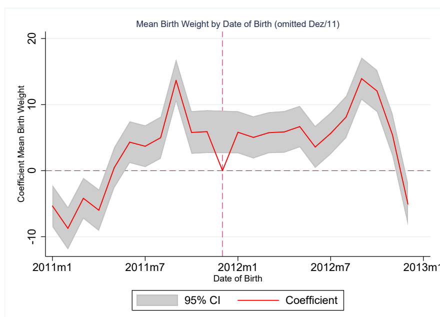
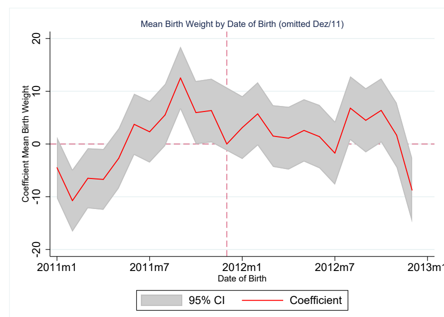


Figure B2: Differences in birth weight by date of birth in comparison to December 2011

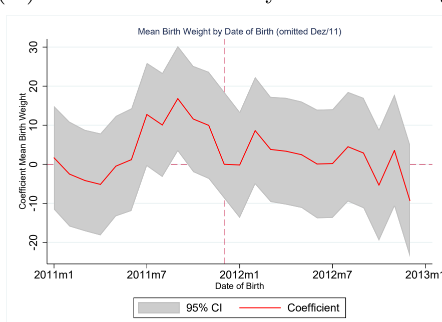
(k) SINASC



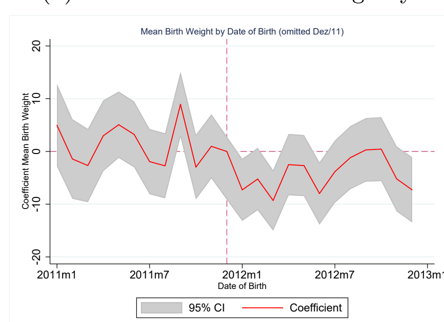
(l) SINASC with 0 to 7 years of schooling



(m) SINASC with 0 to 3 years of schooling



(n) SINASC with Unified Registry



TBA.

Figure B3: Incidence of low birth weight by date of birth - SINASC 2010 to 2014

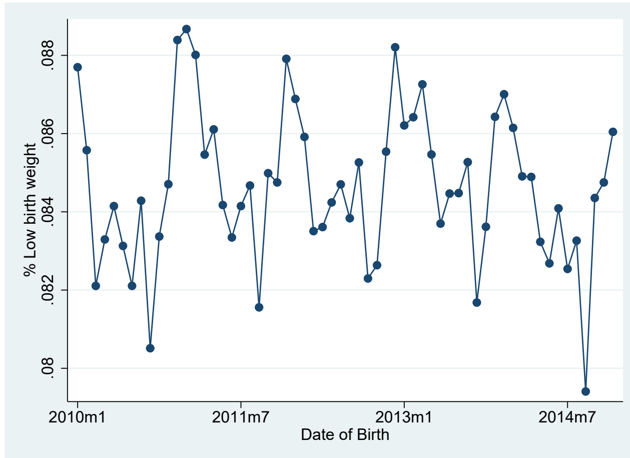
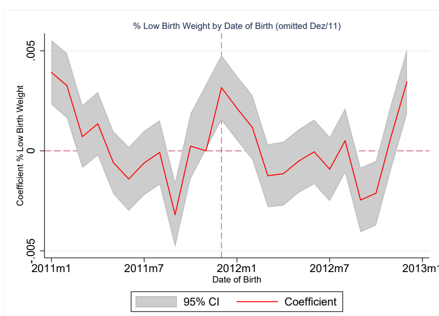
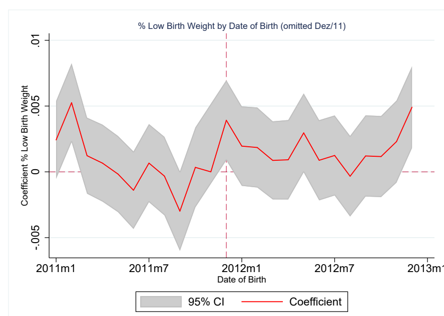


Figure B4: Differences in the incidence of low birth weight by date of birth in comparison to December 2011

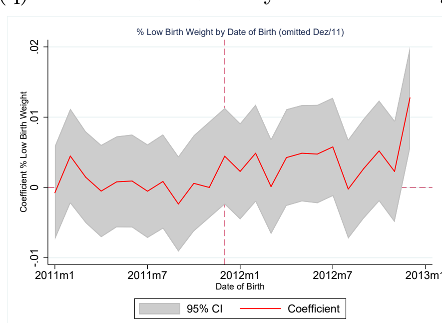
(o) SINASC



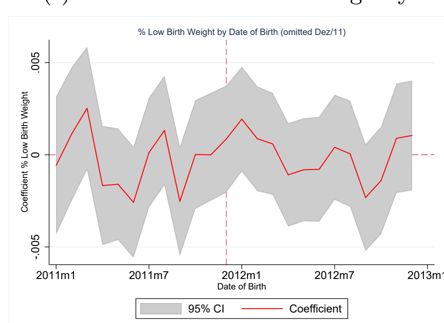
(p) SINASC with 0 to 7 years of schooling



(q) SINASC with 0 to 3 years of schooling



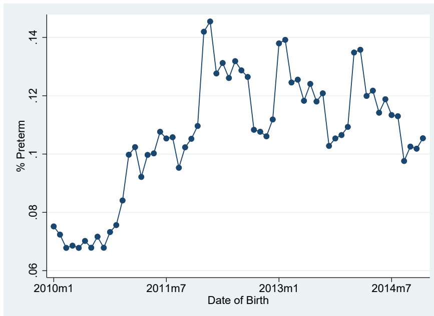
(r) SINASC with Unified Registry



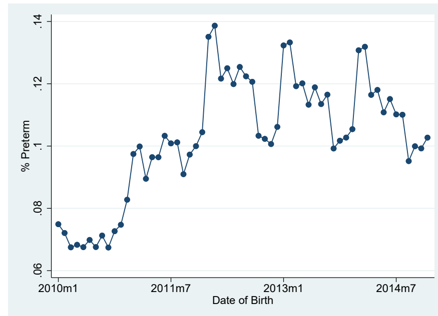
TBA.

Figure B5: Incidence of preterm births by date of birth - SINASC 2010 to 2014

(s) Without missing values



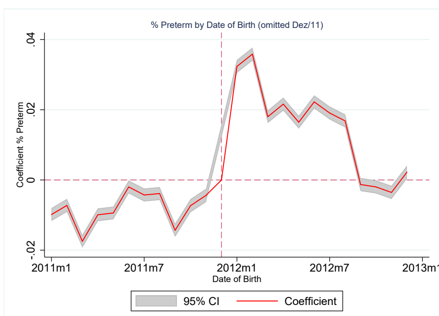
(t) With missing values



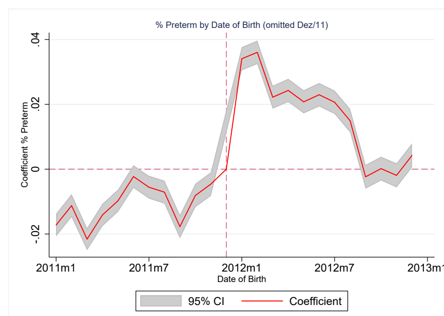
TBA.

Figure B6: Differences in the incidence of preterm births by date of birth in comparison to December 2011

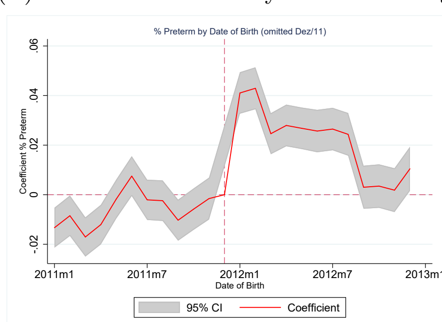
(u) SINASC



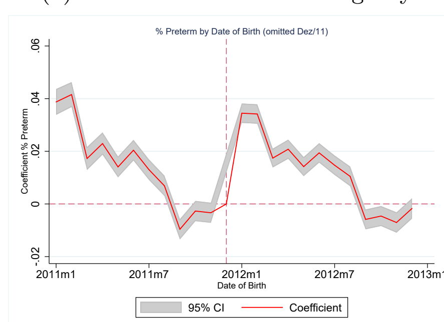
(v) SINASC with 0 to 7 years of schooling



(w) SINASC with 0 to 3 years of schooling



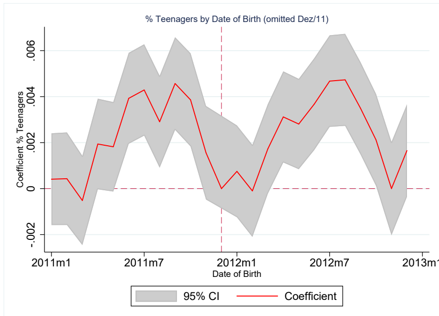
(x) SINASC with Unified Registry



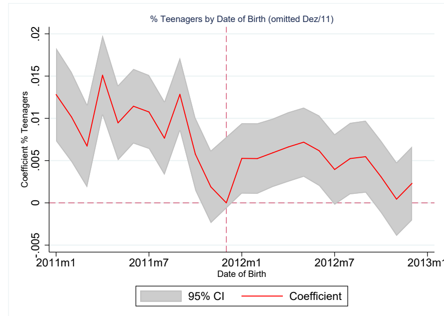
TBA.

Figure B7: Differences in the incidence of teenage mothers by date of birth in comparison to December 2011

(y) SINASC



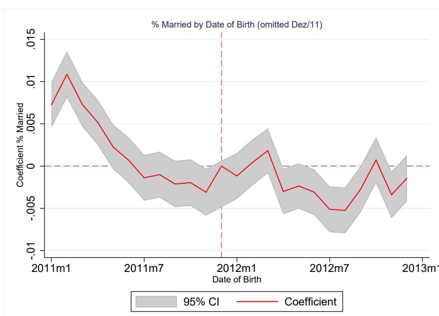
(z) SINASC with Unified Registry



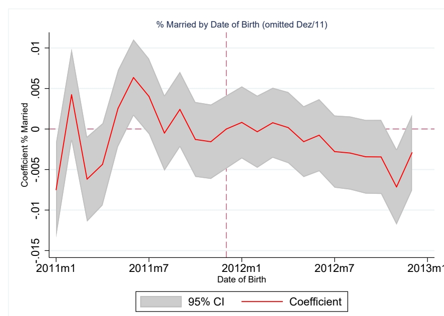
TBA.

Figure B8: Differences in the incidence of married mothers by date of birth in comparison to December 2011

() SINASC



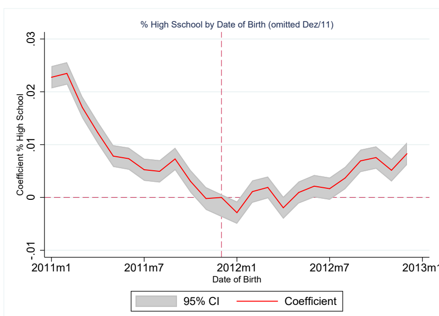
() SINASC with Unified Registry



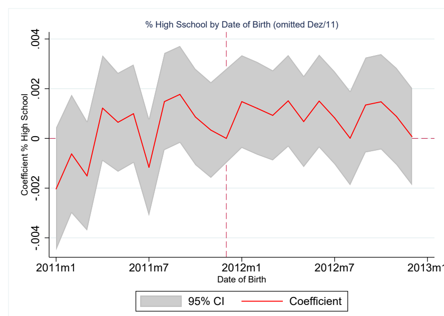
TBA.

Figure B9: Differences in the incidence of mothers with high school diploma by date of birth in comparison to December 2011

() SINASC



() SINASC with Unified Registry



TBA.