Can Small Incentives Have Large Payoffs? Health Impacts of a National Conditional Cash Transfer Program in Bolivia †

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Abstract

Conditional Cash Transfers often consist in large payments making it hard to separate income effects from the conditionality component. Most studies in health find that demand for health services increases, however, little is known about the effect of demand side incentives on outcomes such as infant or newborn mortality. We study a Bolivian program that transfers mothers an equivalent to 1% of a household's per capita consumption, upon compliance with prenatal and child care services. This is not only relatively small in general but also the smallest transfer among similar programs in Latin America. We implement different identification strategies and data sets to study program effects on final outcomes and their pathways. We use plausibly exogenous variation in program restrictions to instrument enrollment rates across municipalities and time and show that the program reduced the rate of stillbirths in 38.8% using administrative data and vital statistics. Consistently, using Census data we find that the program increased the survival rates of cohorts exposed

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to the program in 18.2%. Using household data and comparing across pregnancies for the same mother, the results show higher rates of early prenatal care (17%), of having four prenatal visits (16%), and skilled birth attendance (15%) among women enrolled. Using a discontinuity on age-eligibility rules we also find that the program increases utilization of health services for children. Comparisons across siblings show mixed results on final health outcomes. According to standard thresholds, the intervention is highly cost-effective, at $718.8 USD per DALY averted. *(JEL H51, I12, I38, J18, O15).*
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1. Introduction

Despite the expansion of free or low-cost maternal and child healthcare in the developing world the utilization of preventive services often remains below recommended levels, particularly for poor, rural, and indigenous populations (Mills, 2014).[1] One explanation is that households have non-monetary restrictions, e.g. informational or psychological, that prevent them from adopting better health practices that would improve their well-being (Banerjee and Duflo, 2011). As a response, many countries have implemented conditional cash transfer (CCT) programs to promote investments in human capital (Fiszbein et al., 2009; Adato and Hoddinott, 2011). CCTs are demand side incentives that consist of monetary payments to households, conditional on compliance with requirements such as medical visits for children or pregnant women, or school enrollment and attendance for children (also known as "co-responsabilities"). In addition, most CCTs have a short-term goal of reducing monetary poverty which is why payments are often equivalent to 10 to 25% of household income (see Table 2) and paid in regular (bi)monthly installments (Fiszbein et al., 2009; Stampini and Tornarolli, 2012).

[1]See also the report "Universal Health Coverage report" by the World Health Organization in *http://www.who.int/universal_health_coverage/en/*. Access 05/11/2015.

In this paper we study the effects of a national conditional cash transfer program in Bolivia, the Bono Juana Azurduy (BJA). Two aspects in the design of the BJA makes it particularly salient. First, the BJA pays recipients an amount that represents only a small fraction of an eligible household's per capita consumption. Our estimates show that, on average, participants received the equivalent of 1% of their total consumption. The transfer amounts of the BJA program are also small compared to other CCT programs implemented in the LAC region (see Table 2). Second, the program pays transfers individually for each eligible health visit completed by the mother or child with the specific amount related to the requirement that is due (e.g., prenatal check-up or skilled birth attendance and postpartum visits), and as such is time-finite. In contrast, other CCT programs usually pay flat monthly or bi-monthly payments on an ongoing basis (Fiszbein et al., 2009). Providing evidence of potential returns to small monetary incentives is not only policy relevant but also theoretically appealing if "nudges" help to overcome fixed costs related to health seeking behavior (e.g, gender and cultural barriers or time inconsistencies).[2]

CCTs work through different mechanisms (see Filmer and Schady, 2008; Banerjee et al., 2010; Baird et al., 2011; Benhassine et al., 2015). Transfers may work as a signaling device for the value of the "co-responsibilities" or they may work through the positive income shock if payments are sizable relative to a household's income. The distinction is relevant. If CCTs only worked through the income effect, conditioning cash transfers on "co-responsibilities" would not be necessary,[3] whereas if the effect was explained through the signaling channel, payments could be adjusted downwards to a more cost-effective design. Given the relatively small monetary incentives provided by the BJA and its particular pay-for-compliance design, any effect on final health outcomes is likely to be driven by the utilization of health services rather than a sizable income effect.

[2]See Thaddeus and Maine (1994), Ensor and Cooper (2004), Thaler and Sunstein (2009), Banerjee and Duflo (2011).

[3]There is evidence that transfers that do not condition on behavior can also have positive health effects on children. See Aizer et al. (2016a) for a discussion. Black et al. (2014) also find effects of pure subsidies on academic outcomes of children.

In addition to poverty alleviation, most CCTs also seek to improve human development outcomes in health and education. Yet despite their popularity in the health sector throughout the developing world, there is little evidence on the effect of CCT programs on important health outcomes such as stillbirths or maternal and infant mortality.[4] One reason for the dearth of empirical research on the effects of CCTs on mortality outcomes is that relatively small changes are unlikely to be detected using household surveys due to the low prevalence of the events. In this paper we study the effects of the BJA program on the rate of stillbirths and map the causal pathways through increased health service utilization.[5] [6] According to de Bernis et al. (2016) 2.6 million stillbirths occur annually, a sizable global issue compared to the 4.5 million of infants who die before the first year of life (see WHO, 2013). Cousens et al. (2011) and WHO (2013) show that the rate of stillbirths in Bolivia was one of the largest of the LAC region in year 2009 (see Table 3).[7]

We gathered municipal-level data on the rate of stillbirths for several years before the program started and for years after the program. Using administrative data from the BJA we compute the municipality enrollment rate for each year since the start of the program. We then estimate the effects of enrollment rates at the municipality level on the rate of stillbirths. Our OLS estimates show that there is a significant correlation between enrollment rates and the rate of stillbirths after controlling for municipality fixed effects and time shocks that are common to all municipalities in the country. For instance, a municipality with average enrollment rates experienced a 11.8% decline in the rate of stillbirths with respect to the

[4] Noticeable exceptions are Barham (2011) for Mexico and Rasella et al. (2013) for Brazil and who show that CCTs were effective in reducing infant and child mortality. Lim et al. (2010) and Randive et al. (2013) find effects on neonatal and perinatal mortality in India.

[5] Stillbirth refers to a dead born fetus that dies at or after the 28th pregnancy week. Before that, the death is defined to be caused by abortion or miscarriage. Intrauterine deaths occur either before onset of labor (antepartum death) or during labor (intrapartum death) (see WHO, 2006).

[6] We study stillbirths in Bolivia because of its large prevalence compared to other countries. Also, stillbirths are likely to be better recorded than other indicators such as neonatal or infant mortality since the report of births in the country is of much higher quality. Once a birth is recorded, the data indicates whether the fetus is born dead or alive.

[7] We compute a similar rate of stillbirths from our data. However, the sample that we work with shows a slightly higher rate of stillbirths since it excludes the principal urban centers where the rate of stillbirths is much lower.

baseline average. However, enrollment rates may be endogenous to stillbirths making the association unlikely to be causal. One possibility is that municipalities that were experiencing larger increases in the rate of stillbirths, and expected these to keep growing, also enrolled their population at a higher rate. To estimate a causal effect of enrollment rates on the rate of stillbirths we use arguably exogenous limitation to program participation as an instrument for municipality enrollment rates: the availability of formal payment centers, i.e. points of payments set up by the central government, across municipalities and over time. Qualitative household data shows that frequent reasons for not enrolling to the program were long queues, trips to payment centers, and the delay in payments.

The program relied entirely on payment centers to manage payments of the cash transfers. Payments were managed by local branches of two national banks in urban areas. In most rural areas payment centers were initially managed through the Armed Forces and gradually expanded to other financial intermediaries. Anecdotical evidence shows that financial institutions had better infrastructure to administer large lists of payments and to automatize the process of these transfers, reducing transaction costs of making payments for beneficiaries and the government. Whether beneficiaries had a financial payment center available at the start of the program was not determined by the BJA but rather by the installed capacity of banks in the municipalities. We approximate these constraints with the number of financial payment centers available per eligible population in each municipality since the start of the program. The data shows large variation in the availability of financial centers across municipalities and over time. We also find indirect evidence that financial entities had no other effects on participant households, such as improving or promoting savings, as we find that consumption patterns are relatively similar for participants and non-participants.

We use the change in financial payment centers over time as an instrumental variable for enrollment rates and combine this identification strategy with municipality fixed effects while also control for time shocks that are common to all municipalities. Our first stage results show that the number of payment centers significantly (F-test = 40.06) affects enrollment rates. The estimated elasticity is 0.10, i.e. a 10% increase in formal payment centers is associated with a 1% increase in enrollment rates. The IV results show that a municipality with average enrollment rates experienced a 38.8% decline in the rate of stillbirths with respect to pre-treatment averages. The results are robust to different specifications of the instrument and subsamples of the data that assess the sensibility of the results to data quality issues. Furthermore, we use auxiliary data from the National Census of 2012 to study whether BJA enrollment rates are related to cohort-specific survival rates. Consistent with the results on stillbirths, our IV results show that a municipality with average enrollment rates experienced a 18.2% increase in the size of the cohorts that were exposed to the BJA program during their pregnancy stage.

The main assumption in the IV analysis is that the change across time in the number of centers within municipalities only affects the rate of stillbirths through its effect on enrollment rates, usually known as the exclusion restriction.[8] As suggested by Meyer (1995), we indirectly test for this assumption by exploring whether trends in stillbirths during the pre-program period are related to changes in the number of centers. We find evidence that there is no relation between pre-trends of stillbirths and the expansion of financial payment centers across municipalities.

One of the primary mechanisms through which the BJA may affect stillbirths is by increasing prenatal care utilization. In general, CCT programs have shown positive impacts on increasing health care utilization.[9] With relation to maternal and newborn health, past studies have linked CCTs to increases in prenatal visits, skilled attendance at birth, delivery at a health facility, tetanus toxoid vaccination for mothers, and in some countries to the reduced incidence of low birth weight (Glassman et al., 2013). A series of papers published

[8]This is an important distinction since the exclusion restriction in levels may not hold, i.e. the number of payment centers may be correlated with unobservable variables that explain the level of stillbirths. However, we argue that changes over time in the number of payment centers within a municipality should only affect the rate of stillbirths through its effect on enrollment rates.

[9]Some examples are Gertler (2004), Gaarder et al. (2010), Cecchini and Madariaga (2011), and Cecchini and Soares (2015).

in The Lancet (2011) argue that most of the risks associated to stillbirths are highly preventable, such as adolescent pregnancies, maternal infections, non-communicable diseases, and nutrition, among other lifestyle factors.[10] Table 3 shows some evidence that these type of problems are of high prevalence in Bolivia (see also Torrico et al., 2004). Likewise, de Bernis et al. (2016) state that half of all stillbirths occur during labor, part of it due to lack of skilled birth attendance particularly in low-income countries. Using individual data from a nationally representative household survey, we find that mothers enrolled in the program are more likely to initiate prenatal care earlier, before the 20th week of pregnancy, and visit the health center more often. These findings are based on comparisons between siblings whose differences in age are less than 3 years on average, limiting variation in short-lived shocks, while also controlling for unobserved differences between mothers that could bias estimates (see Aizer et al., 2016b; Aizer and Doyle Jr, 2014).

We also analyze whether the BJA was effective to improve utilization of health services and health outcomes for children. Although unconnected to the results on prenatal care and stillbirths this analysis is helpful to further assess the program's effectiveness. In child health and nutrition, CCTs have shown reductions in stunting and underweight in some countries and population subgroups (Fernald et al., 2008; De Brauw et al., 2014; Levy and Ohls, 2010). Using a regression discontinuity analysis based on age specific eligibility rules for children we show that the BJA also promoted medical visits for children. Comparing across siblings we also find that the BJA stimulates the demand for health services for children (e.g., medical visits and immunization) with mixed results on final health outcomes (e.g., anaemia rates and height-for-age Z-Scores).

With the exception of the BJA program in Bolivia, CCT models with checkup-specific demand side payments to mothers and children have not been widely applied or studied, despite their potential cost effectiveness as an instrument to increase health service utiliza-

[10]See Frøen et al. (2011), Lawn et al. (2011), Bhutta et al. (2011), Pattinson et al. (2011), Flenady et al. (2011), Goldenberg et al. (2011).

tion and improve health outcomes. Our most conservative estimates of costs and benefits generated by the program show that the BJA had a cost of $718.8 USD per Disability Adjusted Life Year averted, equivalent to 26% of the country's per capita GDP, making the intervention highly cost-effective according to standard thresholds provided by the World Health Organization.[11]

This paper is organized as follows. In section 2 we provide a country context, explain the program specific rules and discuss the mechanisms through which the program may affect health outcomes. In section 3 we describe the data sources used in the impact evaluation analysis. In section 4 we show the different methods used in our analysis and in section 5 present the results. In section 6 we present a cost effectiveness analysis of the program and section 7 concludes.

2. Context and Intervention

2.1. Country context

During the past decades, prior to the implementation of the BJA program, Bolivia experienced significant improvements in its population health and nutrition indicators; however, compared to other Latin American countries, health indicators in Bolivia continued to be among the worst in the region (WHO, 2013).[12] According to Demographic Health Surveys (DHS) available for the country, approximately four of every ten under-five deaths occurred during the first month of life in 2008. Moreover, neonatal mortality reported very little progress between 2003 and 2008, remaining at 27 deaths per 1,000 live births. Maternal mortality rate in 2003 was as high as 229 deaths per 100,000 live births according to the DHS of that year.[13] In terms of child nutrition, despite large improvements in the past two

[11] See Marseille et al. (2015) for a discussion

[12] Mortality of children under 5 years old dropped from 115.6 per 1,000 live births in 1994 to 56 per 1,000 live births in 2013, whereas the World Health Organization estimated average for the Latin America and the Caribbean region in 2011 was 16 per 1,000 live births (WHO, 2013; UDAPE, 2015).

[13] The official maternal mortality figure in Bolivia has been recently updated to 160 per 100,000 live births for the year 2011 (MINSAL, 2016).

decades, DHS data from 2008 show that 26% of children under 3 years old were stunted and stunting rates of children in rural areas almost doubled that of urban areas. Low coverage of basic maternal and child health services may explain why mortality and malnutrition have remained at such high levels. Prior to 2009, 71% of total births in Bolivia were delivered by skilled health personnel - either at home or at a health facility - and 72% of pregnant women had at least four antenatal care visits during their pregnancy (see Table 3). In rural areas, these numbers were significantly lower, reaching 51% and 60%, respectively. Table 3 shows that the rates of utilization for different health services are much lower relative to the Latin American region, where 94% of deliveries were attended by skilled personnel and 86% of pregnant women had at least four prenatal medical visits as recommended in the medical guidelines established by WHO (2006).

2.2. The Bono Juana Azurduy Program

Since 1997, pregnant women and children under five years old in Bolivia have been covered by publicly financed health insurance schemes that have provided a free basic health care package.[14] In particular, since 2003, the Universal Maternal and Child Insurance (SUMI by its Spanish acronym) has covered the cost of key maternal and child health interventions, thus lowering the financial barrier to health care, and improving coverage of key health service indicators that at the time of its implementation were considerably low.

To incentivize demand for maternal and child-care health services provided by the SUMI, in May of 2009 the government launched the nation-wide conditional cash transfer program Bono Juana Azurduy (BJA).[15] BJA incentivizes the use of maternal and child health services by pregnant women and children under two years old through the payment of cash transfers

[14]In particular: the National Maternal and Child Insurance (SNMN) from 1997 to 1998; the Basic Health Insurance (SBS) from 1999 to 2002; the Universal Maternal and Child Insurance (SUMI) from 2003 to 2013; and the Comprehensive Health Services Benefits Law from 2014 to present. All these schemes have covered a basic package of maternal and child interventions, including prenatal care, skilled birth attendance, as well as outpatient and inpatient care for children under five; and the number and complexity of covered interventions have been increasing over time.

[15]Juana Azurduy, born in Chuquisaca, Bolivia, is considered a heroine of the independence of South America from Spanish dominance.

that are conditioned on the use of select clinically recommended preventive services. Enrollment in the program is voluntary and all pregnant women and under-one-year-olds not covered by the social security system are eligible to enroll. According to Census estimates, over 82% of women and children in the country were eligible to enroll in 2012.

The program's "co-responsibilities" and associated payments are detailed in Table 4. BJA pays pregnant women 50 Bs ($7 USD) for each prenatal visit up to a maximum of four visits, and 125 Bs ($18 USD) for births assisted by qualified health personnel, whether delivery takes place at a health facility or at home, and an additional follow up check-up within the first seven days after birth. For children under two years old, the program pays 120 Bs ($17 USD) for each health check-up in the first 2 years of life, up to a maximum of 12 visits (one visit every other month). With full compliance of the "co-responsibilities", covering 9 months of pregnancy and the initial 24 months of the child's life, the maximum cumulative transfer amounts to 1,820 Bs ($261 USD) over a 33 month period.

BJA's conditionality structure differs from most CCT programs with an anti-poverty focus and design in a number of ways. First, the program excludes education and general poverty reduction components in its payment structure and conditionality design. Instead, it focuses exclusively on human capital development in terms of maternal and child health. Furthermore, the program includes a clearly defined exit strategy based on the age limit of two years old, after which children are not able to participate anymore. BJA is also universally available to all eligible women and children, and does not target the poor.[16] In addition, the program pays transfers individually for each eligible health visit completed by the mother or child with the specific amount related to the conditionality that is due, in contrast to flat monthly or bi-monthly payments in most CCT programs (Fiszbein et al., 2009). Finally, the total transfer amount is small. We estimate that total potential cumulative transfers under full compliance are equivalent to 2.2% of average household consumption over the 33-month period. In practice, households receive only half of this amount. This is

[16] In fact, BJA beneficiaries are nearly evenly distributed among income quintiles.

relatively small compared to other programs in the region that pay an equivalent of 7% to 31% of households' consumption (Fiszbein et al., 2009).[17]

2.3. Enrollment in the BJA

The BJA started enrolling women and children on May 11th, 2009. The first payment was delivered on May 27th of the same year. Operational rules establish that enrollment should be done at the public health center that is closest to the beneficiary's home. Only pregnant women and children not covered by the social security system are eligible to enroll. In addition, although the BJA benefits children until they turn two years old, they are required to be younger than 12 months at the time of enrollment, in order to guarantee a minimum exposure of 12 months to the program. As a pre-condition for enrollment, an identity card and birth certificate must be presented for the pregnant women and children, respectively, in addition to a pregnancy test for pregnant women.

Figure 2 and Figure 3 show the evolution of enrollment rates in the program for pregnant women and under-one-year-olds obtained from the Health and Nutrition Evaluation Survey 2012 (ESNUT 2012), and BJA administrative records.[18] On average, the enrollment rate of eligible women was approximately 33% between 2009 and 2012, with a decreasing trend over time. The enrollment rate of children during the same period was approximately 52%. According to ESNUT 2012, amongst non-enrolled eligible mothers, the main reasons for not enrolling are the lack of information about the program's enrollment procedures (27.5%), not having the required legal documents at the moment of enrollment (19.9%) and time costs of program participation associated to long queues or long trips to health facilities (20.3%).

[17]Fiszbein et al. (2009) show that the average transfer in CCTs implemented in LACs is approximately 17% of average household consumption.

[18]Enrollment rates are based on retrospective data from the Health and Nutrition Evaluation Survey (ESNUT) 2012 survey and BJA enrollment records (as the numerator), and official population projections (pregnancies and one-year olds) for the denominator.

2.4. The role of payment centers

The program relied entirely on payment centers to manage payments of the cash transfers. Payments were managed by local branches of two national banks in urban areas. In most rural areas, payment centers were initially managed through the Armed Forces and gradually expanded to other financial intermediaries. Early payments by the Armed Forces were heterogenous in terms of geographic coverage and timing, and beneficiaries lacked information regarding payment activities. If no payment centers were available in a municipality, then households would have to travel to the nearest municipality with either an Armed Forces or a bank payment center available. As such, whether beneficiaries had a payment center available at the start of the program was not determined by the BJA program, but rather by the installed capacity of banks and the Armed Forces in the municipalities. In practice, anecdotical evidence shows that financial institutions had better infrastructure to administer large lists of payments and to automatize the process, reducing transaction costs for both, beneficiaries and the government, to make payments effective.[19]

Table 1 shows the evolution of payment centers administered by local bank branches since the start of the program in 2009. During the first year, 26.5% of the municipalities had at least one payment center available to administer transfers to BJA participants. Amongst municipalities with at least one payment center there were on average 9.4 payment centers per 1,000 beneficiaries of the program, with large variation across municipalities. In the next years, the percentage of municipalities with payment centers increased to 34.8% in year 2012, with an average of 16.8 centers per 1,000 enrollees. Anecdotal evidence from the ESNUT 2012, shows that frequent reasons for not enrolling to the program, or discontinuing enrollment, were long queues, trips to payment centers, and the delay in payments. In addition, anecdotical information about the functioning of the program shows that there have been delays of up to 3 months in payments to enrollees.[20] The low coverage of payment

[19] See https://boliviasol.wordpress.com/2010/06/08/excluyen-a-las-fuerzas-armadas-del-pago-del-bono-%E2%80%9Cjuana-azurduy%E2%80%9D, consulted in October 11th, 2016.

[20] see http://www.cedla.org/content/40786, consulted in October 11th, 2016.

centers shown in Table 1 are complemented by Figure 1, which shows the percentage of co-responsibilities that are actually paid by the program by year and type. For instance, in year 2009, only 33.9% of prenatal check-ups that qualified for a transfer were effectively paid to women while a third of attended births and eligible visits by children were effectively paid for. The gap between complied and paid co-responsibilities was reduced over the following years, and persisted above 90% in the last years of our data.

2.5. Mechanisms to improve health outcomes embedded in the BJA Program

According to national norms and protocols, the content of the prenatal care visit includes at least: a) registration of basic information in the prenatal history form, b) capture of vital signs (blood pressure, heart rate, breathing rate, body temperatures), c) measurement of BMI, d) evaluation and assessment of the pregnancy risk level (high, medium or low), e) implementation of a health promotion and prevention package that includes information about risk signs, deworming, nutritional assessment, iron supplementation, immunizations, HIV test and scheduling of prenatal visits, among others (see MINSAL, 2011).

A series of papers published in The Lancet (2011) argue that most of the risks associated to stillbirths are highly preventable, such as maternal infections, non-communicable diseases, and nutrition, among other lifestyle factors.[21] Compared to other countries in the LAC region Bolivia has the highest adolescent birth rate, a low prevalence of pregnant women who are tested for syphilis at the first prenatal care visits, and a higher prevalence of women who are positive for syphilis among those tested (see Table 3).

As such, one of the primary mechanisms through which the BJA may affect stillbirths is by improving prenatal care utilization, i.e. increasing the number of average visits during pregnancy and increasing early initiation of prenatal care. Early initiation of prenatal care

[21]See Frøen et al. (2011), Lawn et al. (2011), Bhutta et al. (2011), Pattinson et al. (2011), Flenady et al. (2011), Goldenberg et al. (2011). Stillbirth refers to a dead born fetus that dies at or after the 28th pregnancy week. Before that, the death is defined to be caused by abortion or miscarriage. Intrauterine deaths occur either before onset of labor (antepartum death) or during labor (intrapartum death) (see WHO, 2006). Our definition of stillbirths includes stillbirths, abortions, or any other definition of death during the first 24 hours after birth.

is part of the standard training in nursing schools throughout the world (WHO, 2006) and has been linked to positive maternal and newborn health outcomes (Carroli et al., 2001b; Campbell and Graham, 2006). In particular, early detection of medical conditions such as maternal infections or anemia in the period in which the fetus is most at risk can improve outcomes at birth, such as low birth weight, prematurity and early neonatal mortality (Carroli et al., 2001a; Hawkes et al., 2013). Early prenatal care also allows providers to advise mothers on proper prenatal nutrition and prevention activities (Phavichitr and Catto-Smith, 2003). The number of medical visits while pregnant may also be associated to birth outcomes since they provide an opportunity to detect risk factors, monitor complications and reinforce healthy behavior throughout the complete period of pregnancy.[22] In addition to preventive prenatal care, the presence of skilled health professionals at delivery can have positive outcomes on early neonatal mortality (Moss et al., 2002).

Another group of health outcomes that we study are related to child nutrition. Medical visits incentivized by the BJA could improve nutrition through at least two channels. First, the BJA not only incentivizes the number of medical visits but also that visits are completed with a minimum frequency of regular bi-monthly intervals during the first two years. As recent literature in economics of human capital has evidenced, investments at earlier stages of growth can have larger returns (Bharadwaj et al., 2013; Heckman and Mosso, 2014). To this end, medical visits can educate mothers about how to best care for their children while also help to detect any health complications that affect a child's growth (see Thapar and Sanderson, 2004).[23] Second, children in Bolivia are entitled to a free nutritional supplement administered at health facilities during medical visits, containing iron, zinc, vitamins A and C, and folic acid, which can have a positive effect on reducing the prevalence of anemia (Lopez et al., 2015).

[22]The World Health Organizations recommends a number of at least four prenatal care visits (see WHO, 2006).

[23]See Strauss and Dietz (1998), Tamura et al. (2002), and Martorell et al. (2010) for a discussion on preventive care for young children, including immunizations and nutrition supplementation.

3. Data

3.1. National Health Information System (SNIS) and Census data

Our primary outcome of stillbirths comes from data of the National Health Information System (SNIS by its Spanish acronym), a national registry of information on different indicators of health services provision and outcomes to which local health facilities must report. The information is interactively available on the Ministry of Health website.[24] We downloaded information for each municipality on the number of total births, live and stillbirths.[25] Health facilities consolidate information on the outcome of each birth on a monthly basis. For birth information to find its way into the system, the birth must have been attended by a doctor, nurse or other qualified health professional at a health facility or at home.[26] We define the variable to be a count of stillbirths due to all causes within a year and municipality.[27]

To evaluate the effects of the BJA on stillbirths using SNIS data we construct a municipal level panel of stillbirths per 1,000 live births from 2005 to 2012. The resulting dataset includes 327 municipalities and 2,616 municipality-year observations.[28] For some municipality-year observations the system reports zero stillbirths or live births. This may reflect intermittent reporting from some municipalities in particular years and not necessarily a true absence of births or stillbirths. We argue in section 4 that our identification strategy for the effect of the BJA on the rate of stillbirths is robust to the differential rates of birth reporting across municipalities in the country.

[24] *http://www.minsalud.gob.bo/*. Access 05/30/2015.

[25] The information in SNIS is aggregated at the municipality level. This aggregation actually corresponds to information provided by all health facilities within each municipality and does not necessarily represent the indicator for the population living in that municipality. This is because people often travel to other health facilities outside their municipalities to receive services, particularly in basic or specialized hospitals.

[26] As such, the data are reliable only for births attended by a health professional in health facilities or at home.

[27] There is no clear information on the time frame considered in the system for intrauterine deaths, so this classification may include abortions or miscarriages before the 28th pregnancy week. The registry is also limited to stillbirths, which is why early neonatal death (referring to the first 0-7 days of life) is not included.

[28] Currently, Bolivia has 339 municipalities; however, the SNIS data for period 2005 to 2012 correspond to the previous administrative division of 327 municipalities.

Our second source of information is the Population Census of 2012, which we use to validate our analysis using the SNIS data. The principal goal of the Census is to update information on the number of people and households, and their geographic distribution over the country. It also provides information on demographic characteristics, economic well-being and housing of each person or household covered. It is implemented every 10 years, and in 2012 it covered a total of 10,027,254 inhabitants.

To complement the SNIS analysis described above, we use aggregated 2012 census data on age-cohort population sizes to study the effect of the program on child survival. For each municipality we count the number of children aged 0 to 6 separately and treat these data as a panel of municipalities, where each observation represents a municipality-year of birth cohort of surviving children.[29] Children ages 5 or 6 years in 2012 were never exposed to the BJA, since they were born two years before its implementation. Hence, the BJA should have no effect on the cross-municipality differences in cohort-size for these age-cohorts, but could have had an effect on younger cohorts by increasing child survival. We develop this idea further in section 4.

3.2. Health and Nutrition Evaluation Survey (ESNUT 2012)

The Health and Nutrition Evaluation Survey (ESNUT, by its Spanish Acronym) 2012 is a nationally representative household survey implemented by the Plurinational State of Bolivia to provide information for the evaluation of national health and nutrition programs, including the BJA. The survey provides information about the health and nutritional status of the Bolivian population, and allows to construct different health-system coverage indicators, with a strong emphasis on maternal and child health care. The sample design allows disaggregation by urban and rural areas, as well as by ecological regions (highlands, valleys and lowlands). It considers a multistage probabilistic sample selection based on the 2001 Census as the sampling frame for the selection of primary sampling units (PSU). The survey

[29] A similar approach was implemented by Jayachandran (2009) to study the effect of wildfires on early life mortality in Indonesia.

provides sampling weights to adjust for different selection probabilities. The full sample covers 8,433 households (2,456 urban and 5,977 rural) in 424 PSUs.

The ESNUT 2012 includes a basic demographic household questionnaire, a questionnaire for women in reproductive age (14 to 49 years) and a questionnaire for children under 5 years old living in the household. For all women interviewed that reported at least one pregnancy since January 2007, the survey collected retrospective information on every pregnancy, including information on the number of antenatal care visits, birth outcomes and post-partum visits. For children under 5 years old, the survey collected information for all medical visits, immunization, current nutritional status and anthropometric measures of height and weight, as well as hemoglobin levels for children 3 months or older. In addition, the ESNUT 2012 asked retrospective questions about participation in the BJA program for all pregnancies and children, allowing us to identify beneficiary households in the survey.

4. Empirical Strategy

As discussed above, BJA was implemented nation-wide in 2009, targeting pregnant women and children younger than 12 months at the time of enrollment. Yet enrollment rates varied substantially across the country, and only about one in three eligible pregnancies and one in two eligible children were enrolled in the program during the study period. Our quasi-experimental identification strategies identify the impacts of the BJA program through arguably exogenous variation in program eligibility and enrollment over time and across space. Our empirical strategies are implemented depending on the outcome of interest and the data source. Below, we divide them accordingly.

4.1. Municipality Level Outcomes: Rate of Stillbirths

We estimate the effect of BJA coverage within a municipality on the rate of stillbirths using the SNIS data. We construct the number of stillbirths (numerator) per 1,000 live births (denominator) in each municipality for each year between 2005 and 2012. The treatment variable is the percentage of eligible women enrolled in the BJA in each municipality for each year between 2009 and 2012.[30] We estimate the following regression:

$$Y_{j,t} = \phi_t + \phi_j + \delta_1 Av.Enroll_{j,t,t-1} + X'_{j,t}\gamma + \varepsilon_{j,t} \tag{1}$$

Where, $Y_{j,t}$ is the rate of stillbirths for municipality j at year t; $Av.Enroll_{j,t,t-1}$ is the average enrollment rate in year t and year $t-1$. We compute the average enrollment rate to account for the fact that women that gave birth in the first months of a given year were exposed to the program during the previous year for an important period of their pregnancy.[31] The enrollment rate in each year is computed as the ratio of number of enrolled women (numerator) to total number of eligible women (denominator) in municipality j at year t. We include a vector of control characteristics in $X_{j,t}$ for municipality j at year t; and $\phi_t, \phi_j, \varepsilon_{j,t}$ are municipality fixed effects, time fixed effects, and unobservable characteristics that vary across municipalities and time, respectively. We include a binary indicator for each year to control for shocks that are common to all municipalities, such as changes in medical guidelines or health supply shocks that are common to all municipalities. Municipality fixed effects control for unobserved variables specific to each municipality that are fixed over time, such as altitude, weather, or cultural barriers to health care, among others.

We restrict our sample to municipalities with fewer than 250,000 inhabitants, eliminating six large urban municipalities which are very different from the rest of the country. The final sample size consists of 321 municipalities of the 327 available in the country, for a total municipality-year sample size of 2,568. We run regression (1) weighting each observation by the total number of pregnancies in a municipality in year 2008.[32]

[30]For years 2005 to 2008 the enrollment rate is equal to zero.

[31]We restrict the analysis to the average between the two years since in practice we only have one instrument available for enrollment rates. Allowing for differential effects of contemporaneous and lagged year leads to severe multicollinearity issues. Using the average of two years also helps to smooth out possible measurement error in the independent variable, in particular in the projections of total pregnancies in each municipality used in the denominator.

[32]See Solon et al. (2015). In practice our results are robust to using different weights.

The key identifying assumption in estimating (1) through OLS, is that the change in the outcome observed in municipalities with high enrollment rates would have been the same, had they experimented lower levels of enrollment, as those that actually experimented lower levels of enrollment. And vice-versa. Although this assumption is not testable, commonly known as "parallel-trends" in potential outcomes, we are able to explore whether the pre-intervention trends in outcomes were similar across municipalities with different take-up levels. If the trends are the same in the pre-intervention period, they are more likely to have been the same in the intervention period had the policy not been implemented.

Figure 4 shows the evolution of the rate of stillbirths per 1,000 live births in our time period. The y-axis shows the rate of stillbirths in deviations from the group-average, and the x-axis shows calendar years. We divide municipalities evenly into three groups, or terciles, according to the enrollment rate in year 2009, the first year of the BJA. Figure 4 provides a graphical representation of what we aim to estimate in (1). We plot the rate of stillbirths over time for the highest, middle, and lowest tercile.[33] The figure shows that the trends in the rate of stillbirths are similar between the lowest and highest enrollment groups, however for the group of municipalities in the middle, the trends in years before the program look different.

The figure also shows that municipalities with a higher average rate of stillbirths in pre program years (solid line) enrolled beneficiaries at a larger rate. Another feature of the figure is that the stillbirth rate decreases at a much faster rate in municipalities with higher enrollment rates in the program. Although this may indicate that the program was effective it may also show that some municipalities enrolled at a higher rate because they were expecting stillbirths to rise in the next years. As such, there are reasons to think that enrollment rates are endogenous to trends in stillbirths. If this were true, estimating (1) through OLS would lead to a biased estimate of the effect of the program on the rate of stillbirths.

[33]In this graph we drop 2005 and observations with 0 rate of stillbirths to smooth trends.

To overcome this issue we use the number of financial payment centers, discussed in section 2, available in each municipality and period since the start of the program as an instrument for enrollment rates. Since our analysis includes municipality fixed effects, we need that the number of payment centers varies across municipalities and time, otherwise there would be no variation left out to estimate δ_1. As we show in Table 1 there are significant changes in the number of municipalities with a financial entitity available, and the population covered by payment centers also varies importantly across municipalities as evidenced by the standard deviation. The variation in the number of payment centers available per 1,000 eligible women is arguably exogenous to similar variations in the unobserved component of (1). In other words, we argue that the instrument is independent of potential outcomes. Although this is not testable directly we can examine whether changes in payment centers over time since the start of the program are related to the trends in stillbirths before the program started. In particular we run the following regression using data for years 2005 to 2008:[34]

$$Y_{j,t} = \alpha + \sum_{t=2006}^{2008} \beta_t Year_t + \sum_{t=2006}^{2008} \gamma_t Year_t \Delta Ln(P.C.)_{j,2009} + \phi_j + \mu_{j,t} \tag{2}$$

Where $\Delta Ln(.P.C)_{j,2009}$ is the change in the logarithm of payment centers from years 2008 to 2009 in municipality j. We explain in the next sub-section why we use the logarithm instead of levels. In this regression, the γ_t coefficients summarize the correlation between the change in payment centers at the start of the program and any change in the outcome in years 2006 through 2008 with respect to year 2005. We include time and municipality fixed effects. If the coefficients are jointly significant, there would be evidence against the assumption that the change in payment centers, i.e. the instrumental variable we use, is not

[34] This is commonly known as a test to support the assumption of parallel trends but in this case we want to study whether trends are related to the instrument.

related to potential outcomes; a necessary condition to identify δ_1 through 2SLS in (1). The results are shown in the first column of Table 6. The change in the rate of stillbirths before year 2009 is uncorrelated with changes in the number of payment centers at the beginning of the BJA. The joint test, provided at the bottom of the Table, shows a p-value of 0.995. Overall, the results suggest that there is no statistically significant relation between the expansion of payment centers and the trends in the rate of stillbirths before the program started. The results are similar when we use the change of payment centers for subsequent years. This supports the assumption that the instrument is not related to potential outcomes, and hence provide evidence in favor of the exclusion restriction assumption, i.e. that the expansion in the number of payment centers affect changes in the rate stillbirths only through its effect on enrollment rates.

4.1.1. First Stage Results

To estimate the effect of the BJA program on the rate stillbirths we implement the method of Two Stage Least Squares (2SLS). We use the number of payment centers available in municipality j at time t to instrument for the average enrollment rate in (1). To take into account that an additional payment center is not the same in a municipality with 100 eligible women than in a municipality with 10,000 eligible women we construct the rate of entities per eligible women in each period and municipality. We take the log of the rate and construct:

$$Ln(\frac{Payment\ \ Centers}{Eligible\ \ Women}) = Ln(P.C.) - Ln(E.W.)$$

We are interested in how the rate of payment centers affects the enrollment rate in the BJA program. However since both variables share the same denominator we would be inducing spurious correlation in our first stage estimates by including the rate of payment centers in the RHS of the first stage equation. As such, our first stage equation is:

$$Av.Enrollment_{j,t,t-1} = \phi_t + \phi_j + \lambda Ln(P.C.) + W'_{j,t}\gamma + \omega_{j,t} \tag{3}$$

Where $W_{j,t} = [X_{j,t}, Ln(E.W.)]$, which also replaces $X_{j,t}$ in the second stage estimation. The results for the first stage regression are shown in column (4) of Table 6. The results show that $\hat{\lambda} = 0.032$, i.e. a 1% increase in the number of payment centers increases the enrollment rate by 3 percentage points. This corresponds to an elasticity of 0.11, i.e. a 10% increase in the number of payment centers increases the enrollment rate by 1.1%. At the bottom of Table 6 we report the F-test (40.06) and p-value for the significance of $\hat{\lambda}$. The results show that payment centers are a strong predictor of the enrollment rate. We use the predicted values of (3) to obtain a consistent estimate of δ_1 using 2SLS.

4.1.2. Analysis of the Effect of the BJA on Age-cohort Size

We use aggregated data from the 2012 National Census on age-cohort population sizes to study the effect of the program on child survival. We construct the size of each age-cohort in a municipality and study the correlation between the rate of enrollment in the BJA with observed differences in the size of age cohorts. In the Census of 2012 only cohorts between ages 0 to 3 (born in years 2009 and 2012) were exposed to the program, while age-cohorts 4 to 6 (born in years 2008 and 2006), where too old to be enrolled.

If we had data on all pregnancies in the country and information on whether each pregnancy persists through birth, the child survives the first year, second year, and so on, we could estimate the following regression:

$$S_{ijc} = \beta_1 Enr_{ijc} + \phi_c + \phi_j + \varepsilon_{ijc} \tag{4}$$

where S_{ijt} is the probability that a child i of cohort c in municipality j survives until a determined age and Enr_{ijc} is an indicator for whether the child participates in the BJA or not. A positive β_1 would indicate that the program had a positive impact on the probability of surviving in each period. Aggregating (4) at the municipality level we would be able to estimate the following regression:

$$Mort\_rate_{jc} = \beta_1 Av.Enroll_{j,c,c-1} + \phi_c + \phi_j + \varepsilon_{jc} \tag{5}$$

where $Mort\_rate_{ijc}$ is the mortality rate of cohort c in municipality j; $Av.Enroll_{j,c,c-1}$ is the same as before. The only difference is that the enrollment rate for age-cohort 0-1 in the Census would be the average enrollment rate of year 2012 and year 2011, for age-cohort 1-2 we use years 2011 and year 2010, and so on. For age cohorts 3 through 6 the enrollment rate is equal to zero since they were not exposed to the program at any stage of their life cycle. ϕ_c, ϕ_j are fixed effects for the cohort and municipalities, and ε_{jc} are unobserved factors that vary across cohorts and municipalities. Each age-cohort in this regression corresponds to an age cohort in the Census 2012: 0-1 year old, 1-2 year old, and so on. A fixed effects regression controls for ϕ_c and ϕ_j, while variations in ε_{jc} that are correlated with variation in the enrollment rates of the program remain uncontrolled.

In practice, there is no data available on mortality levels for each age cohort and municipality in Bolivia. One indirect way to infer anything about the survival rates is to work with the size of each cohort. Assuming, for now, that there is no internal migration across municipalities, and following Jayachandran (2009), mortality rate has a direct relationship to the size of the cohorts and the number of births, given by:[35]

$$\log(size)_{jc} = \log(births)_{jc} - Mort\_rate_{jc} \tag{6}$$

[35]For simplicity, we assume no internal migration. We discuss below how migration may affect our estimates.

Replacing this term in the previous equation we obtain:

$$log(size)_{jc} = \pi_1 Av.Enroll_{j,c,c-1} + \phi_c + \phi_j + \underbrace{\varepsilon_{jc} + \log(births)_{jc}}_{Unobserved} \tag{7}$$

Here, we fit a linear regression of the size of each cohort in a municipality as a function of the enrollment rate of pregnant women at the pregnancy stage. We use the count in the Census for age cohorts 0 to 6 years in each municipality. One advantage of using the size of the cohort lies in the inclusion of the survival of children at birth and to later life stages, which is believed to be measured with lower error than mortality rates that are usually under reported. However, the use of this variable to infer variations in mortality has its own complications. First, the cohort size is a measure of survival over time, which differs between cohorts, independent of how much time they were exposed to treatment. For the cohort born in 2012, the outcome in that year measures survival during the first year of life, while for those born in 2009 it reports survival during the first three years of life. We include a dummy for each age cohort to control for average differences in survival rates.

In addition, the coefficient π_1 is consistently estimated if there are no changes in unobserved or uncontrolled variables that are also correlated with the variation in enrollment rates. These could be variables reflecting variations in the supply of health services, such as changes in the number of health facilities in each municipality. We estimate equation (7) above including municipality fixed effects that account for unobserved characteristics that are common to each cohort. Changes in fertility rates remain uncontrolled; however, to date there is no evidence that the BJA generated any changes in fertility rates. Finally, if municipalities that have experienced higher enrollment rates over time are receiving migrant population from municipalities with lower enrollment rates, the effects of the program would be biased away from the null hypothesis of no impact. To overcome these problems, we apply the same instrumental variable approach as in the previous section to (7).

4.2. Individual Level Outcomes of Maternal and Newborn Health: Prenatal Care Utilization and Low Birth Weight

The ESNUT 2012 collects information on utilization of health services for every pregnancy experienced by women living in the household since 2007. We take advantage of the event-study type information for each mother, which allows us to define BJA eligible and ineligible pregnancies for the same mother, based on the date of birth. Accordingly, among eligible pregnancies across mothers, we know which of them were actually enrolled in the BJA. Our analysis is similar in spirit to a linear differences-in-differences approach, where we compare BJA enrolled to not enrolled pregnancies, using differences of ineligible pregnancies for the same mothers as the baseline. In particular, we use a subsample of women with at least two children born since January 2007 for which at least one child was born before the start of the program and at least one after the start of the program. Consider the following regression:

$$Y_{is} = \alpha + \delta D_{is} + X_{is}'\beta + \eta_i + \gamma_s + \varepsilon_{is} \tag{8}$$

Subscript s refers to a pregnancy and i refers to a mother. Y_{is} is an outcome, such as weeks pregnant at the first prenatal visit, for pregnancy s of women i; D_{is} is a binary indicator for whether pregnancy s of mother i was enrolled in the BJA; X_{is} is a vector of controls that are observed and can be fixed or vary across different pregnancies for the same mother (e.g. education or order of birth); η_i are unobserved fixed variables for mothers (e.g. parental skills); γ_s are unobserved fixed variables for pregnancies (e.g. common unobserved risk measures); and ε_{is} are unobservable variables that are allowed to vary within a mother across her different pregnancies. Cross-sectional estimation of equation (8) by OLS will generally give biased estimates of the treatment parameter, δ, because unobserved components of η_i and γ_s may influence the probability of treatment take-up and pregnancy outcomes. To eliminate this potential source of bias we estimate a fixed-effects model comparing the same outcome accross different pregnancies, treated and non-treated by the BJA, of the same mother, thus eliminating the fixed unobserved component, η_i , in equation (8).

We estimate the following linear regression:[36]

$$\tilde{Y}_{is} = \delta\tilde{D}_{is} + \tilde{X}_{is}'\beta + \phi_s + \tilde{\varepsilon}_{is} \tag{9}$$

Where the transformation of a variable Z_{is} to $\tilde{Z}_{is}$ indicates that the variables deviate from the group mean. Assuming that the treatment status is uncorrelated with the new unobservable component, $\tilde{\varepsilon}_{is}$, we can estimate (9) by OLS and obtain an unbiased estimate of the treatment effect. The assumption would be violated if mothers behave systematically different from one pregnancy or child to another. However, in the context of BJA, we argue that the variation in the treatment status for the same mother is due mostly to exogenous program eligibility rules (namely the implementation of the program for children under 12 months of age in May 2009) rather than to unobserved components that change from one pregnancy to another. However, conditional on a pregnancy or child being eligible, mothers still make the choice of enrolling to the BJA during their pregnancy or enrolling their children after birth. To address this issue, we include controls for order of birth, age of the mother at delivery, sex of the child and cohort of birth, thus controlling for common time trends and factors such as gains in parenting experience over subsequent pregnancies or children.[37] Moreover, the average time between pregnancies or children in our sample is approximately 2.5 years,

[36]We estimate OLS regressions for both continuous and discrete outcomes. For the latter, index models in fixed effects settings may be computationally intractable and impose assumptions on the functional forms of the data that could lead to worse bias than Linear Probability Models. See a discussion of this by Steve Pischke in the online blog of his book "Mostly Harmless Econometrics". $http://www.mostlyharmlesseconometrics.com/2012/07/probit-better-than-lpm/$. A main restriction on running linear models is that the predicted probabilities may be outside the unit interval. In most cases, estimating equation (9) without vectors $\tilde{X}_{is}$ provides estimates of the treatment effects close to those obtained when such vector is included. As such, the "uncontrolled" version of (9) is a fully saturated model in which the predicted probabilities are constrained to the unit interval by construction. In addition, our main purpose is to estimate the partial effect on the response probability, averaged across the distribution in the sample, as opposed to the effect on particular values of the distribution. In this case, the linear probability model is appropriate. See page 454 - 457 in Wooldridge (2010). The heteroskedasticity problem for variance estimation is addressed by using heteroskedasticity-consistent robust standard error estimates.

[37]For a similar analysis see Salm and Schunk (2012).

which constrains the likely variation in household environments, especially if such changes must differ systematically between treated and untreated observations to pose a risk to the identification strategy. Furthermore, albeit at a different scale and for a different sample, the analysis at the municipality level in the previous section supports the parallel trend assumption also needed in this analysis.[38]

Another important aspect of our research design, is that the variation in $\tilde{D}_{is}$ that allows to identify δ is induced by mothers with at least one pregnancy ineligible and another one eligible for the BJA, based on the program's age eligibility restriction, which defines that only children under one year of age could enroll in May 2009. This allows for the selection into treatment of pregnancies to be mainly due to the eligibility rule, which is arguably independent of the potential outcomes of each observation. Finally, the validity of our results depends, at least in part, on the fact that mothers enrolled in the BJA are not different, other than in their treatment status, from non-enrolled mothers. In the Online-Appendix C we show that demographics characteristics of mothers enrolled and not enrolled in the program are very similar.[39]

4.3. Individual Level Outcomes of Child Health and Nutrition: Health Services Utilization and Nutritional Outcomes

The mentioned eligibility rule based on a child's age at the moment of enrollment provides a natural discontinuity. To estimate the effects of the BJA on the utilization of health services by children we employ a regression discontinuity (RD) design (see Lee and Lemieux, 2010).

To explore compliance with the age-specific eligibility rule, we use the ESNUT 2012 and study the relation between date of birth of each child in our sample and enrollment rates

[38] We also attempted to do the parallel trends test using mothers with more than two children in pre treatment periods. However the sample size are small.

[39] Even though all these characteristics are fixed and hence controlled for in the fixed-effects analyses, they are informative about the differences in composition across households that should be considered in the interpretation of results. In addition, our results are not externally valid and they should be interpreted as the effect within households similar to the sample, i.e. households with mothers with at least two pregnancies at the time period of the survey.

in the BJA. We use June 1st of year 2008, as the cutoff date for birth date.[40] Children born before that date were too old to enroll at program start in May of 2009. A graphical representation of this relation is shown in Panel A - Figure 5. The x-axis is the date of birth of children, measured in week-deviations from Junae 1st 2008, and the y-axis shows shows the percentage of enrolled children. The horizontal line in the middle of the graph is fixed at June 1st of year 2008, exactly one year before the BJA started. Children that are born before the cut-off date are grouped to the left of the vertical line, and those born after are grouped to the right. Each bin corresponds to a week and we use 72 days before and after the cutoff date as the bandwidth. The graph shows the take-up rate is very low or zero as we move further to the left in the x-axis. To the right of the eligibility date cutoff, the take-up rate increases substantially. Our RD estimates show that the take-up rate increases by more than 30 percentage points at the eligibility cut-off. The Figure also shows that the change in BJA take-up is not sharp at the cut-off, i.e. take-up is not 100% after the cutoff date. To account for this, we implement a "fuzzy" RD design.

Formally, let B_i be date of birth of child i and consider a cut-off c, set at June 1st of year 2008. Let D_i be an indicator variable for whether child i is enrolled in the BJA, and Z_i, be an indicator variable equal to one when the child's birth date is equal or exceeds the cut-off of June 1st in year 2008, indexed by c, i.e. $Z_i = 1(B_i \geq c)$. The two equations we estimate in the "fuzzy" RD are:

$$Y_i = \beta_0 + \beta_1 D_i + f(B_i) + \varepsilon_i \tag{10}$$

$$D_i = \gamma_0 + \gamma_1 Z_i + f(B_i) + \mu_i \tag{11}$$

Where Y_i is an outcome, such as number of medical visits that occur during months 12 to 24 of age, for child i. The function $f(B_i)$ is a smooth function of the birth date, which is the

[40] In particular, we use each child's date of birth to construct the difference in weeks to June 1st of year 2008. Within each week we plot the mean enrollment rate.

forcing variable that determines whether a child is eligible or not for the BJA. It captures smooth, seasonal effects of birth dates on outcomes. The central assumption underlying the RD design is that we have correctly specified $f(B_i)$. We estimate a local (kernel-weighted) linear regression to the left and right limits of the discontinuity choosing different bandwidths. We work with the sample of children that are two years old or more at the moment of the survey. This restriction drops all children that may still have on-going treatment since they can be covered by the BJA until they are 2 years old.

In practice, a "fuzzy" RD exploits discontinuities in the probability of treatment conditional on a covariate. The discontinuity is used as an instrumental variable for actual take-up of the program and the usual assumptions of Instrumental Variable (IV) estimation are needed (see Hahn et al., 2001). Hence, the parameter that we estimate using the "fuzzy" RD is the Local Average Treatment Effect (LATE), which is the effect on those observations that comply with the instrument, i.e. children that are moved into the treatment because of the age-specific rule (see Imbens and Angrist, 1994).

Finally, the identification strategy in RD relies on the assumption that observations are randomly assigned to the treatment at the cut-off. An indirect test for this assumption is to check the balance across different covariates within the cut-off. We test for this by running a local linear regression version of (10) for different demographics and covariates that should not vary with treatment status. All results are commented in section 5.

In addition to the RD design we use the longitudinal structure of the ESNUT 2012 data and run a similar analysis as that for maternal and newborn health outcomes. In this case, we select two children for each mother, one eligible and the other non-eligible, and estimate the effect of the BJA using a fixed effects approach. The same assumptions apply. The effects may differ from the RD analysis in that we are using different samples and estimating different treatment parameters. Finally, the validity of our results depend, at least in part, on the fact that children enrolled in the BJA are not different, other than in their treatment status, from non-enrolled children. In the Online-Appendix C we show that the demographics characteristics of children enrolled are different than those enrolled. Even though these characteristics are controlled in the fixed effect analysis, the internal validity of our results depends on the extent that all selection into treatment by the BJA depends on the unobserved fixed effects. We analyze this assumption in the next sections as we comment the results

5. Impacts of the BJA

5.1. Municipality Level Outcomes: Rate of Stillbirths

Table 5 shows the main results. Column (2) shows the results of estimating equation (1) by OLS. The estimated coefficient shows that a 1-percentage point (ppt) increase in average enrollment in the BJA is associated with a reduction of 0.081 stillbirths per 1,000 live births. To approximate this association in an average municipality under typical enrollment rates, we convert these results for the average enrollment rate of the sample. The average enrollment over time across municipalities is 32%. As such, an average municipality experienced a decline of $0.32 \times 8.1 = 2.6$ stillbirths per 1,000 live births, which corresponds to a 11.8% decline with respect to the average rate of stillbirths at baseline. We have discussed in 4 that the OLS estimate gives an inconsistent estimate of the effect of enrollment rates on the rate of stillbirths. We use the number of payment centers in a municipality for a given year to instrument for the enrollment rate variable in (1). Column (3) shows the reduced form estimates where we substitute the average enrollment rate with the natural logarithm of payment centers, $Ln(P.C.)$, in (1). The results show that a 1% increase in the number of payment centers reduces the rate of stillbirths by 0.834, which corresponds to a 3.8% decline with respect to the average rate of stillbirths at baseline.

Column (4) shows the First Stage of the 2SLS estimation. The results show that a 1% increase in the number of payment centers increases the enrollment rate by 3.2 percentage points. This corresponds to an elasticity of 0.11, i.e. a 10% increase in the number of payment centers increases the enrollment rate by 1.1%. At the bottom of Table 6 we report the F-test (F=244.81) and p-value for the significance of $\hat{\lambda}$. The results show that payment centers are a strong predictor of the enrollment rate. We use the predicted values of (3) to obtain a consistent estimate of δ_1 in (1) using 2SLS. Column (5) in Table 5 shows that a 1-ppt increase in average enrollment in the BJA is associated with a reduction of 0.265 stillbirths per 1,000 live births. To approximate this effect in an average municipality under typical enrollment rates, we convert these results for the average enrollment rate of the sample. As such, an average municipality experienced a decline of 0.32×26.453=8.5 stillbirths per 1,000 live births, which corresponds to a 38.8% decline with respect to the average rate of stillbirths at baseline.

5.1.1. Robustness check

We run several robustness checks. Column (1) in 6 shows the results of specifying the instrument as the number of payment centers as opposed to the natural logarithm. The results show that the effect of the BJA is large, i.e. a 1-ppt increase in average enrollment in the BJA is associated with a reduction of 0.566 stillbirths per 1,000 live births, or a an 80% decline with respect to the the average rate of stillbirths at baseline, for an a municipality with average enrollment rates. The differences are largely explain by the fact that in this specification an additional payment center in a municipality with 100 eligible women is treated in the same way as a municipality with 10,000 eligible women. While it is a more flexible specification, using the number instead of the logarithm inflates the coefficient in the reduced form of this specification, and as such, we are likely overestimating the effects of the program. Since the specification in logarithm allows to scale for the fact that municipalities vary in their size of eligible women, using the logarithm of eligible women as a control, we prefer to use the instrumental variable in its logarithmic expression.

In the next columns of Table 6 we assess whether our results are sensitive to measurement error in the data. We do this in three ways. Column (3) shows that the 2SLS results are robust to dropping observations that show zero stillbirths, which suggests that the results are not driven by municipalities reporting a stillbirth rate of zero. In columns (4) and (5), we drop the first year of our data, year 2005, and year 2008 respectively. The reason for dropping year 2005 is to test for whether results are sensitive to dropping the first year of the data which is the year with highest stillbirths and also the year where we expect to have worse registration of stillbirths. The reason for dropping year 2008 comes from the fact that there is a significant blip in this year that may also be due to reporting error by municipalities. [41] The results show that while there is some variation in the coefficient, the effect of the BJA remains significant and at similar levels as the result in column (5) of Table 5.

5.1.2. The BJA and Survival of Age-Specific Cohorts

In this section we explore if the effects found on the rate of stillbirths are consistent with the effects of the BJA program on the survival of specific age-cohorts. Column (1) in Table 7 shows the results of estimating equation (7) by OLS. The estimated coefficient shows that a 1-percentage point (ppt) increase in average enrollment in the BJA is associated with an increase in 0.001% in the size of cohorts exposed to the BJA program. A municipality with average enrollment rates experienced an increase of 0.32×10.9=3.5% in the size of the cohorts exposed to the BJA program during the pregnancy stage. As we argue in previous sections, the OLS estimates may be inconsistent because of endogeneity of enrollment rates and survival of cohorts exposed. We use the number of payment centers in a municipality for a given year to instrument for the enrollment rate variable in (7). Column (3) shows the reduced form estimates where we substitute the average enrollment rate with the natural logarithm of payment centers, $Ln(P.C.)$, in (7), and control for the logarithm of pregnancies. The results show that a 1% increase in the number of payment centers increases the size of cohorts exposed in 1.8% on average. Column (3) shows the First Stage of the 2SLS estimation. The results show that a 1% increase in the number of payment centers increases the enrollment rate by 3.1 percentage points. This corresponds to an elasticity of 0.11,

[41] We further comment on the problem of misreporting mortality in Online Appendix A.

i.e. a 10% increase in the number of payment centers increases the enrollment rate by 1.1%. Column (4) in Table 7 shows that a 1-percentage point (ppt) increase in average enrollment in the BJA is associated with an increase in 0.006% in the size of cohorts exposed to the BJA program. To approximate this effect in an average municipality under typical enrollment rates, we convert these results for the average enrollment rate of the sample. A municipality with average enrollment rates experienced an increase of $0.32 \times 0.569 = 18.2\%$ in the size of the cohorts exposed to the BJA program during the pregnancy stage.

The positive effect between the implementation of the BJA and the cohort size could be the result of three different acting mechanisms: reduction in mortality rates, an increase in the fertility rate, or effects of internal migration. We argue that the results of the previously described mortality models using SNIS data support that the results are driven by the reduction in the rate of stillbirths, for three reasons. First, the results shown in the next subsection posit that the program was successful at generating maternal and newborn health effects such as increased numbers of medical visits or reduced rates of low birth weight that are likely drivers of reduced stillbirth rates. Secondly, we found no significant correlation between program take-up and fertility outcomes such as changes in the inter-genesic period between children.[42] Finally, all women in any region of the country are eligible to the BJA, which makes it unlikely that that differential enrollment rates in the program across municipalities are due to significant changes in internal migration patterns.

5.2. The Effect of the BJA on Prenatal Care Utilization and Birth Weight

In the previous section we showed that municipalities with higher rates of BJA enrollment also experienced a steeper reduction in the rate of stillbirths. One of the main mechanisms that could explain this result is that the BJA incentivized women to seek more prenatal care. We estimate regression (8) and study the effect of BJA take-up on different outcomes

[42] This would be unexpected as the program restricted the enrollment of a pregnant women that already had another child under two years of age enrolled in the program.

for prenatal care utilization.[43] Table 9 presents the results. The first row in column (1) shows that women enrolled in the BJA had their first prenatal check-up 2.5 weeks earlier than women who did not enroll in the program, which corresponds to a reduction in 17% relative to the average mean of women not enrolled. In column (2) we show that this effect holds after we control for a quadratic specification of age at birth, order of pregnancy, and gender of the child. In other words, our results do not change once we account for covariates that proxy pregnancy risks or gains in parenting skills. To the extent that these variables are potential sources of bias in our estimates, the results show that using mother fixed effects account for most of the unobserved differences. The next columns show the effect of the BJA for women who live in urban areas and rural areas. Comparing columns (4) to (6), the results show that the effect of the BJA on early initiation of prenatal care is higher in rural areas, where enrolled women had their first prenatal check-up 2.7 weeks earlier than women not enrolled, compared to 1.8 weeks in urban areas. We also construct the probability that a woman seeks prenatal care for the first time before the 20th week of pregnancy. Panel B in Table 9 shows that women enrolled in the BJA are 8.6 percentage points more likely to have their first check-up before the 20th week of pregnancy than women not enrolled in the BJA, which corresponds to increasing the probability of early initiation of prenatal care in 11% with respect to the control mean. The marginal effect is higher and only significant for the sub-sample of women who live in rural areas of the country (see Panel B column (4) and (6)).

The next Panel in Table 9 shows the effects of the BJA on the probability of completing at least 4 prenatal visits during the pregnancy, the maximum number of visits paid by the program. The results show that women enrolled in the BJA are 10.3 percentage points more likely to complete at least four prenatal check-ups during their pregnancy than women not enrolled, which corresponds to a 16% increase relative to the average rate of 73.9% for women not enrolled. We find no differences between rural and urban areas for this

[43]The definition of each outcome for prenatal and post natal care analysis are shown in Table 8

outcome. Finally, we find no significant impacts of the BJA on skilled birth attendance and postpartum care in urban areas (Panel D). In rural areas, however, the program increased the combined probability of having births attended by skilled health personnel and receiving the first postpartum checkup (as required by the corresponding combined co-responsibility) of 5.0 percentage points, which corresponds to an increase in 14% in the probability of birth and post-partum care relative to the control group average.

Table 10 shows the effects of the BJA on birth weight and the probability of low birth weight. Birth weight information is available to approximately 75% of the sample. In one specification (columns (3), (6), and (9)) we adjust for the probability of having birth weight information by Inverse Probability Weighting (IPW).[44] The results show that the BJA had no significant effect on average birth weight. However, Panel B shows that the BJA reduced the rate of low birth weigh in urban areas. Our preferred estimates in column (6), show that the rate of low birth weight is 7.8 ppt lower for women enrolled in the BJA, which corresponds to a sizable effect with respect to the average of 9.9% in urban areas. We interpret the results on birth weight with caution since the effects found on stillbirths suggest that the BJA had important effects on stillbirths. To the extent that the new births caused by the BJA have a lower birth weight than the average, we may even expect a negative effect of the BJA on birth weight and bias the estimates of the program biased towards the null.

5.3. The Effect of the BJA on Children's Utilization of Health Services and Health Nutritional Outcomes

In addition to promoting prenatal care, the BJA aims to improve health outcomes including nutrition of children through payments for medical visits during the first 24 months of age. Panel B in Figure 5 shows the relation between the number of medical visits for children between 12 and 24 months of age and their birth date. Recall that, at the moment

[44] We estimate a Probit model for the probability of having birth weight information and use the predicted values to construct inverse probability weights as suggested by Wooldridge (2007). Results are available upon request.

of enrollment, children that were already 12 months of age were not eligible. The graph shows that children born before the eligibility cutoff do on average approximately 4 visits to the doctor between 12 and 24 months of age, compared to more than 5 medical visits for children during the same age that were born after the eligibility cutoff. This change is evident at the discontinuity of the age eligibility rule. The ITT estimates in Table 11 show that the number of medical visits during this age is 0.824 (CV bandwidth) to 1.08 (CCT bandwidth) higher for children enrolled in the BJA.[45] The LATE estimate shows that the average number of additional visits among enrolled children ranges from 2.4 (CV bandwidth) to 3.5 (CCT bandwidth). The effect is similar using different bandwidths and specifications of the smoothing function. We find no significant effects with the RD specification on other utilization outcomes such as immunizations or the consumption of micronutrient supplements provided to children at health facilities.

The validity of the results shown in Table 11 relies in part on the assumption that at the threshold the assignment to treatment is as good as random. We test for differences at the cutoff for different covariates. The p-values for whether there are significant differences at the threshold are shown in Table 12 and show no significant differences. An important feature of the data is that it records age-specific numbers of medical visits. As only children of less than one year of age were eligible to the program in May of 2009, that comparisons at the cutoff should have only affected the number of visits at ages close to 12 months rather than the number of visits that were not affected by the program, such as visits at age 0 to 5 months. As such, we can use the number of medical visits that children performed during their first five months for a placebo test. Panel C in Figure 5 shows that the average number of visits in the first five months of age is smooth around the threshold of enrollment.

We complement the analysis of the effects of the BJA using the fixed-effects model ex-

[45] Bandwidths are computed using the *rdrobust* package provided by Calonico et al. (2014b). The CCT bandwidth corresponds to the one defined by Calonico et al. (2014a), the IK bandwidth is defined in Imbens and Kalyanaraman (2011), and the CV bandwidth corresponds to the cross-validation bandwidth (see Calonico et al. (2014b)).

plained in section 4, and estimate a version of equation (8) using different outcomes for the sample of children. Table 13 presents the impact of the BJA on the total number of health checkups for children at ages 0 to 23 months. We use the sample of children that are 24 months of age or older. Column (1) in Table 13 shows that the estimated effect of the BJA on the number of checkups is 3.6 additional visits during the child's first two years. In the next column we include a quadratic specification of the mother's age at birth, sex of the child, and dummy variables for birth order and a dummy variable for each month of child's age. The results in column (2) show that the effect of the BJA on the number of visits is robust to including controls, which suggests that the fixed effects may already control for most of the potential bias. We additionally analyze a set of indicators associated with outcomes of medical visits for children: immunizations and nutritional supplementation. As shown in Table 13, BJA has positive impacts on three vaccination indicators: yellow fever vaccine, measles-mumps-rubella (MMR), and an indicator of complete immunization.[46] The program also increased the probability of the consumption of micronutrient supplementation by 11.4 percentage points.

Using the fixed-effects strategy, we also examine the impact of the BJA on child nutritional and growth outcomes measured as height for age z-score (HAZ) in standard deviations, the probability of stunting (HAZ¡-2) and the probability of anemia. As shown in Table Table 14, we find no statistically significant effects of the BJA on HAZ or on the probability of stunting. However, in rural areas, we find evidence of a 5.8 percentage point decline, significant at the 10% level, in the probability of a child suffering from anemia.

6. Cost Effectiveness Analysis

In this section we perform a Cost Effectiveness Analysis (CEA) to quantify the impacts associated with the BJA in terms of monetary costs per disability adjusted life year (DALY)

[46]Complete immunization includes the BCG vaccine, three doses of the DPT/Pentavalente vaccine, three doses of the polio vaccine, MMR (SRP) vaccine and the yellow fever vaccine.

averted as a consequence of the program's implementation.

We compute both fixed costs of managing the program, the costs generated by the program impacts on medical visits and immunization, and the travel costs induced to enrolled households for each medical visit and for each payment they receive. In terms of the benefits generated by the BJA we use the program's impacts on the prevalence of low birth weight in urban areas, the rate of stillbirths, and the incidence of anemia in rural areas. Each of these has a measure of averted DALYs associated by the World Health Organization (WHO).[47]

Online Appendix D shows the parameters and components of the costs and benefits associated to the BJA used for the computation of our measure of total cost per DALY averted by the program. The fixed costs of the program come from official records of the administration office and amount to a total of $227,832,524 Bs ($32,861,863 USD) for year 2013. These correspond to total cash transfers given to beneficiaries, costs of the program's operation, costs of community health workers, and other management costs. The variable costs are less straightforward in that these are implied by program impacts on the number of health visits and immunization rates, as well as travel costs induced to participating households. For instance, if the BJA has an impact on the number of visits there is an implied additional cost of these visits that in the absence of the program would not have happened. The same argument applies to total number of vaccines and the value of travel time to each health visit and each payment center to receive the transfer. The BJA generated an impact in both prenatal checkups (70,318 additional visits) and child health checkups (109,496 additional visits). Valuing each medical hour at 37.73 Bs and assuming that each visit takes an average of 0.5 hours, the costs associated to these additional visits amounts to 8,306,101 Bs ($1,162,967 USD). The same rationale is followed for the other components of the cost. As such, the total cost of the BJA is estimated to be $ 34,022,475 USD per year in the period of April 2009 to October 2013.

To compute the benefits of the BJA we focus on the DALYs averted associated to the

[47] See *http://www.who.int/healthinfo/global_burden_disease/en/*.

impacts on low birth weight, the rate of stillbirths, and the incidence of anemia, the three primary health outcomes with statistically significant program effects. We explain the computation of benefits for low birth weight, although the same rationale applies to the other two indicators. The number of children that are no longer born with low birth weight as a consequence of the BJA is 3,670 births in urban areas. This number is computed using the impact of reducing the probability of low birth weight by 8 percentage points and considering a total of births in urban areas of 45,875 in a year. This amounts to 12,451 DALYs averted using WHO DALYs. A similar analysis is performed for the rate of stillbirths and the incidence of anemia. These three components sum to a total of 47,512 DALYs averted associated to the BJA.

The World Health Organization provides guidelines for different thresholds to consider health interventions as cost effective. In particular, one rule of thumb is to check if the costs per DALY averted is less than three times the country per capita GDP. See Marseille et al. (2015) for a discussion. Our lower bound estimates show that BJA had a cost of $690.2 USD per DALY averted, making the intervention highly cost-effective when compared to the GDP per capita of $2,480 in 2012.

7. Discussion

There is extensive evidence showing large effects of CCT programs in the health sector worldwide. However, most CCTs are designed as anti-poverty mechanisms and transfers usually represent a sizable portion of a household's income, making it difficult to separate effects derived from increased utilization of health services from a direct effect of income. In this paper, we study the effects of the Bolivian CCT program BJA which differs from previous similar programs in at least two interesting aspects. First, the maximum amount that a household could receive from the program corresponded to only1% of the average per capita consumption, making it one of the lowest transfers in the LAC region. Second, the amount and timing of each payment is tied to the occurrence of each corresponding medical visit rather than to a fixed monthly or bi-monthly quota. Taken together, our results are unlikely to be driven by sizable income effects, which could affect health outcomes by increasing the consumption of food or medication, for instance. Instead, our findings posit evidence of large effects on services utilization and health outcomes when providing small cash incentives that are directly tied to the compliance of “co-responsibilities”.

We find that the BJA program effectively stimulated the demand for prenatal services nationwide and for a combined package of skilled birth attendance and postpartum care in rural areas. We study the trends in the rate of stillbirths for years before and after the implementation of the BJA and use a 2SLS method to identify the effect of the BJA on the rate of stillbirths at the municipality level. In particular we find that the implementation of the program reduced the level of stillbirths by a[proximately 38.8% with respect to baseline averages. The positive association of reduced stillbirths and higher enrollment rates of the BJA is a promising signal that CCT models such as the one we study have the potential to improve pregnancy outcomes, including low-birth weight, and the survival and health of newborns.

With respect to child health, the BJA effectively stimulated the utilization of preventive health services, whose protocols include growth and nutrition monitoring, vaccination, and related prevention and counseling strategies. However, while we find a reduction in the prevalence of anemia in rural areas, we find no evidence of longer-term impacts on children’s growth. While final nutrition outcomes such as child growth are influenced by a multi-dimensional set of factors beyond health sector policies alone, sector-specific supply side factors related to the quality of care require further analysis to explain the limited impacts of BJA on final health and nutrition outcomes despite the positive impact on the number and timing of service utilization.

Our cost-effectiveness analysis shows that overall the BJA had a cost of $718.8 USD per DALY averted, making the intervention highly cost-effective when compared to the GDP per capita of $2,480 in 2012.

In most developing countries, similar to Bolivia, pneumonia, diarrhea and other infectious diseases continue to be the main causes of under-five mortality, followed by preterm birth and intrapartum-related complications. Many of the most common mortality and morbidity causes are preventable with timely medical care. The evidence from this experience suggests that directed and relatively small monetary incentives paid for critical preventive maternal and child health services represent a promising and cost-effective policy alternative for reducing cultural, economic and behavioral demand side barriers for health services, which can have important effects on final health outcomes.

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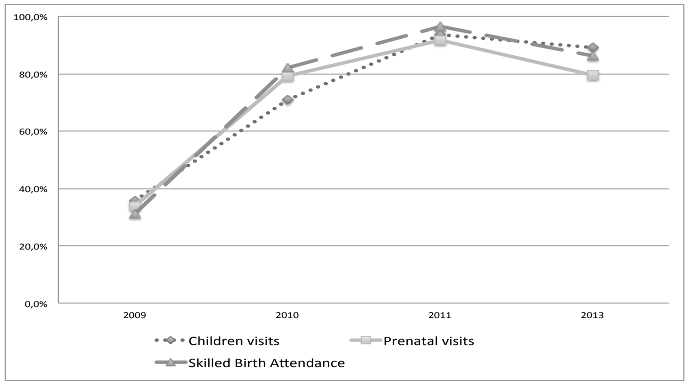

Figure 1: Percentage of co-responsibilities paid by the BJA by year and type of service.

Notes: Own calculations based on BJA enrollment records.

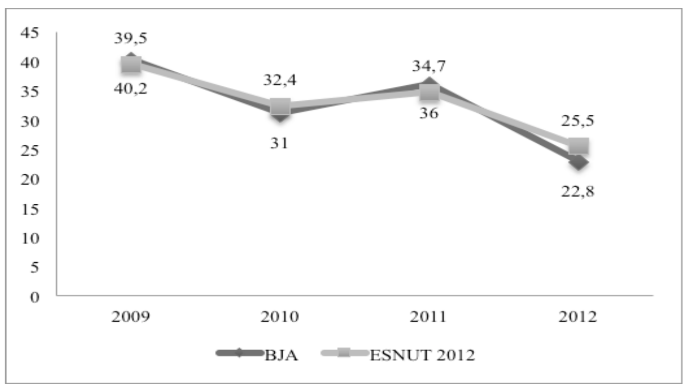

Figure 2: Enrollment rate in the BJA of pregnancies by year of pregnancy

Notes: Source is ESNUT 2012 survey and BJA enrollment records. For the enrollment rate using BJA records, we use official population projections as the denominator of eligible population.

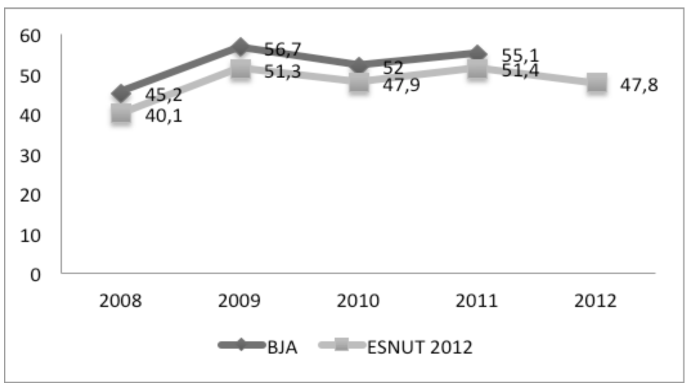

Figure 3: Enrollment rate in the BJA of children by year of birth

Notes: Source is ESNUT 2012 survey and BJA enrollment records. For the enrollment rate using BJA records, we use official population projections as the denominator of eligible population.

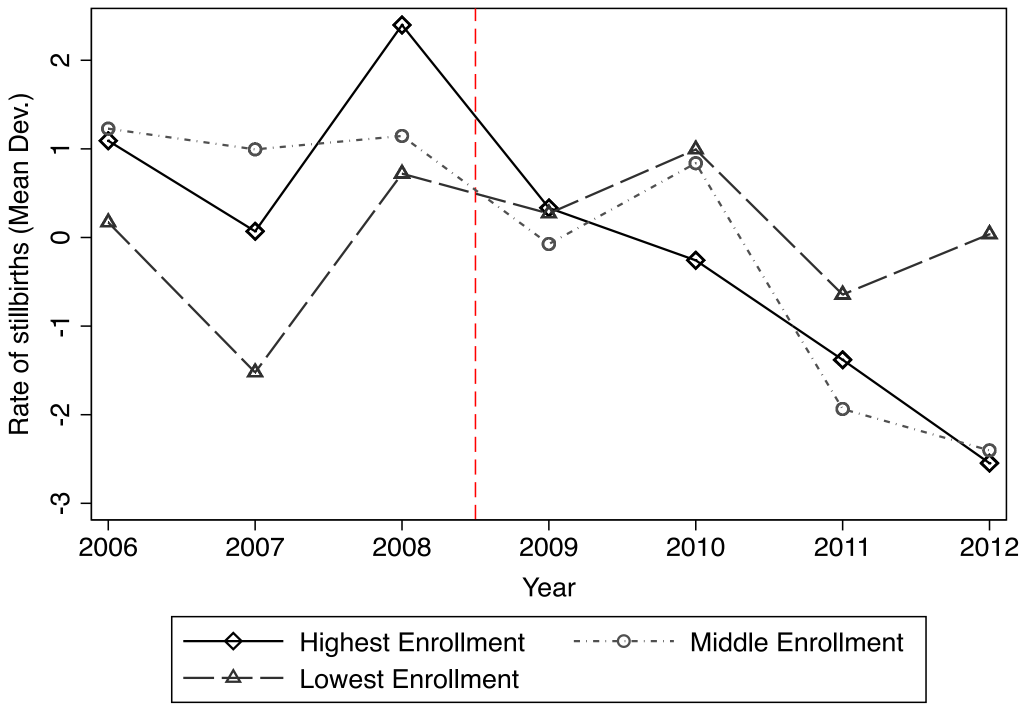

Figure 4: Trends in the rate of stillbirths for municipalities with enrollment rates above the median (High) and below the median (Low) enrollment rate in year 2009

Notes: The Figure shows the evolution of the rate of stillbirths per 1,000 live births in our time period. The y-axis measures the rate in deviations from the average and the x-axis shows calendar years. We divide our sample into terciles according to their enrollment rate in year 2009. We plot the rate of stillbirths over time for the three terciles. In this graph we drop 2005 and observations with 0 rate of stillbirths to smooth trends. The horizontal line is drawn between years 2008 and 2009 the start of the program.

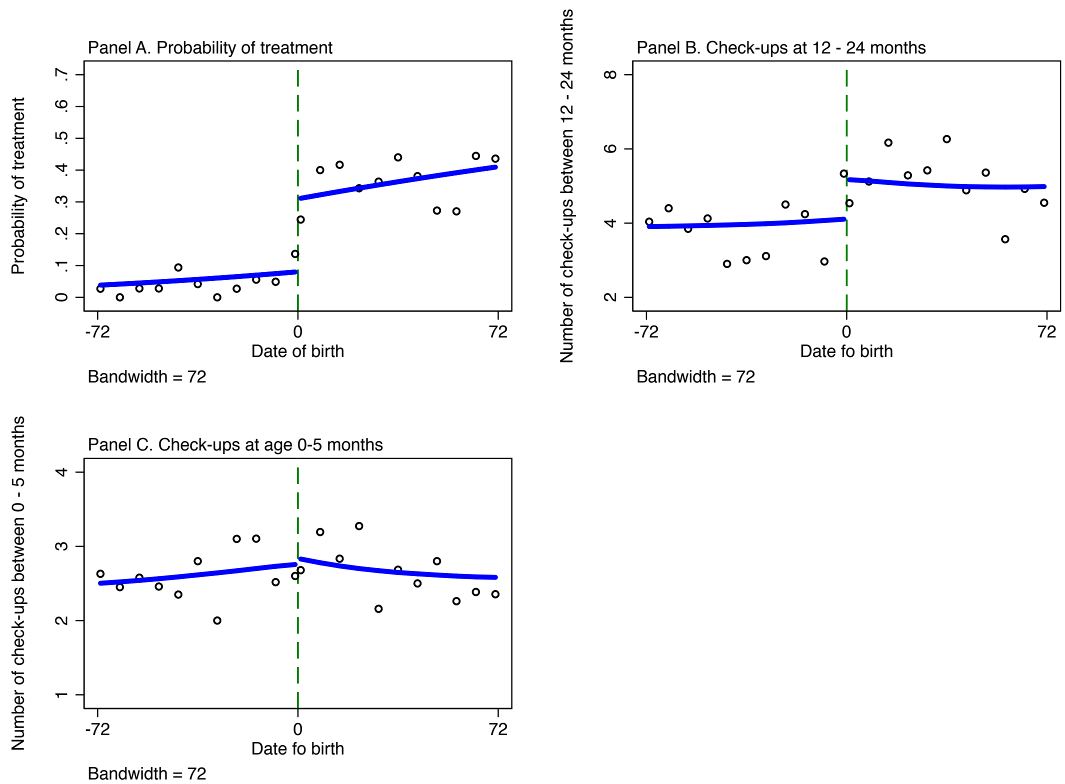

Figure 5: RD-Graphical Analysis. Effects of the eligibility rule on treatment take-up, number of check-ups at 12 - 24 months of age, and placebo test for number of check-ups at 0-5 months of age.

Notes: The Figure is a graphical representation of the effect of the age specific eligibility rule on treatment take up (Panel A), the number of medical visits between months 12 to 23 of age (Panel B), and the number of visits between months 0 to 5 of age (Panel C). The x-axis is the date of birth of children and the y-axis shows the dependent variables mentioned above. The horizontal line in the middle of the graph is fixed at May of year 2010, the month when the BJA started.

Table 1: Evolution of payment centers over time in sample municipalities

Year	Municipalities with at least one payment center	Payment centers per 1,000 enrolled women		
	Perc.	Mean	Std. Dev.	Median
2009	26.48%	9.98	22.05	5.44
2010	33.96%	19.79	56.71	9.68
2011	34.89%	13.47	21.15	8.62
2012	34.89%	16.82	26.06	11.06

Notes: Source: Author's own calculation based on SNIS and administrative records of the BJA program. The average of payment centers per women enrolled is computed amongst municipalities with at least one payment center available.

Table 2: Transfers Relative to Household Consumption for Different CCT Programs in Latin America

Country	Program	Ratio of Transfer to Consumption
Bolivia	Bono Juana Azurduy	1 - 2.2
Ecuador	Bono de Desarrollo Humano	7 - 8
Brazil	Bolsa de Familia	8
Honduras	Programa de Asignación Familiar	9-11
Colombia	Familias en Acción	13 - 17
Mexico	Oportunidades	19 - 20
Nicaragua	Atención a Crisis	29 - 31

Notes: Source: For all countries except Bolivia, see Fiszbein et al. (2009). For Bolivia: author's own calculation based on ESNUT (2012).

Table 3: Coverage indicators for utilization of maternal and child health services before the BJA

Indicator	Bolivia (Urban/Rural) 2008* (%)	Average LAC** (%) (2011)
Use of birth control methods	61 (66/53)	74
Percentage with at least 4 pre-natal medical visits	72 (82/60)	86
Births delivered by qualified personnel	71 (88/51)	94
Immunization rate for BCG Vaccine (18-29 months)	98 (99/98)	–
Immunization rate for DPT vaccine (18-29 months)	86 (85/87)	92
Adolescent birth rate (per 1,000 women 15 - 19 years)	115.6	72.7
Antenatal care attendees tested for syphilis at first visit (%)	60	86
Antenatal care attendees positive for syphilis (%)	1.6	1.1
Malaria cases per 1,000 people (%)	2.1	3.15
Stillbirths (per 1,000) (%) estimated by WHO	17.0	9.3

Notes: Sources: (*) Demographic and Health Survey 2008. (**) WHO (2013) and Global Health Observatory data repository. For the rate of stillbirths LAC averages we only take a subset of countries for which the number is available. See Cousens et al. (2011) for a revision of this number in several countries in the world.

Table 4: Co-responsibilities and amounts in the BJA

Corresponsability	Number	Amount ($Bs/$USD)	Maximum ($Bs/$USD)
For women during pregnancy:			
Prenatal care checkups	4	50 / 7	200 / 28
Skilled birth assistance and first postpartum checkup	1	120 / 17	120 / 17
Total benefits from pregnancy			320 / 45
For children under 2 years old:			
Comprehensive health checkup	12	125 / 18	1.500 / 216
Total benefits from children			1.500 / 216
Total benefits over entire cycle (33 months)			1.820 / 261

Notes: Authors' own elaboration.

Table 5: Effect of BJA Intensity on the Rate of Stillbirths at the Municipality Level

	(1) Pre-trends and IV	(2) OLS	(3) Reduced Form	(4) 1st Stage	(5) 2SLS
Av. Enrollment$_{t,t-1}$		-8.109**			-26.453**
		(3.818)			(12.043)
Ln(payment centers)			-0.834**	0.032***	
			(0.383)	(0.002)	
Δ Ln(P.C.) in 2009 x year 2006	0.056				
	(0.367)				
Δ Ln(P.C.) in 2009 x year 2007	0.305				
	(0.553)				
Δ Ln(P.C.) in 2009 x year 2008	0.366				
	(1.000)				
Observations	1,284	2,568	2,568	2,568	2,568
Adjusted R^2	0.004	0.246	0.246	0.858	0.238
Joint F-test γs	0.108				
Joint p-value γs	0.955				
1^{st} Stage F-test				40.06	
1^{st} Stage p-value				0.000	
Average enrollment rate		0.32			
Baseline mean of stillbirths		21.8			

Notes: All regressions include municipality fixed effects and time fixed effects. Each observation is weighted by the number of eligible pregnancies in year 2008. We also control for number of health facilities per 1,000 births. The sample excludes municipalities with more than 250,000 habitants in year 2012: 6 out of 327 municipalities in the country. Standard errors in parenthesis and are clustered at the municipality level. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table 6: Robustness Checks for the Effect of the BJA on the Rate of Stillbirths

	(1) 2SLS IV: Number	(3) 2SLS Drop zeros	(4) 2SLS Drop Y2005	(5) 2SLS Drop Y2008
Avg. Enrollment t, t-1	-28.719**	-33.190**	-23.856**	-29.906**
	(13.750)	(12.915)	(11.668)	(13.283)
Observations	2,568	1,691	2,247	2,247
Adjusted R^2	0.236	0.505	0.233	0.256

Notes: Standard errors in parenthesis clustered at the municipality level. (1): Principal IV regression of (1) but changing the instrument to be the number of payment centers available in the municipality in a year. The number of payment centers is top-coded at six payment centers. (2): Principal IV regression of (1) model excluding observations that reported 0 stillbirths. (3): Principal IV regression of (1) model excluding year 2005. (5): Principal IV regression of (1) model excluding year 2008. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 7: Effect of BJA Intensity and the Size of Age-Cohorts at the Municipality Level

	(1) OLS	(2) Reduced Form	(3) 1st Stage	(4) 2SLS
Enrollment Rate	0.109***			0.569***
	(0.029)			(0.120)
Log(Payment Centrals)		0.018***	0.031***	
		(0.003)	(0.005)	
Observations	2,240	2,240	2,240	2,240
Adjusted R^2	0.446	0.455	0.855	0.994

Notes: The dependent variable is the logarithm of the cohort population count. All regressions include municipality fixed effects and time fixed effects. The result is interpreted as the correlation between the rate of enrollment and the percentage change in the size of the cohort. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 8: Impact Evaluation Indicators using ESNUT 2012

Indicator	Definition
Maternal and neonatal health Indicators	
Coverage of early antenatal care	Probability of having the first antenatal care checkup before week 20 of pregnancy
Coverage of antenatal care (at least 4 visits)	Probability of having at least four antenatal care checkups by skilled health personnel (doctor, nurse or auxiliary nurse)
Coverage of skilled birth attendance and postpartum care	Probability of having a birth attended by skilled health personnel and receiving a postpartum checkup in the first 7 days after birth
Weight at birth	Weight at birth in grams
Prevalence of low birth weight	Probability of birth weight less than 2,500 grams
Health and nutrition child Indicators	
Coverage of comprehensive child health care	Average number of comprehensive child health checkups between 0 and 23 months of age
Coverage of yellow fever immunization	Probability of children under 5 receiving the Yellow Fever vaccine
Coverage of measles-mumps-rubella (MMR) immunization	Probability of children under 5 receiving the MMR vaccine
Coverage of complete immunization	Probability of children under 5 receiving the complete schedule of vaccines (BCG, 3 doses of the Pentavalente, 3 doses of antipolio, yellow fever and MMR).
Consumption of micronutrient supplementation in children	Probability of having ever consumed micronutrient supplementation (Chispitas Nutricionales)
Height for age Z-score	Average Z-Score of height for age in standard deviations
Prevalence of chronic malnutrition in children	Probability of children under 5 having a Z-score below -2SD from the WHO reference population
Prevalence of anaemia in children (mild, moderate and severe)	Probability of children under 5 having low blood haemoglobin concentration according to specific age group cut-off values

Source: ESNUT 2012.

Table 9: Effect of BJA Enrollment on Utilization of Prenatal Care Services

	All Sample		Urban Households		Rural Households	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Weeks Pregnant At First Prenatal Check-up						
Treatment effect	-2.558***	-2.311***	-2.113**	-1.759**	-2.931***	-2.731***
	(0.542)	(0.546)	(0.816)	(0.848)	(0.716)	(0.687)
Control group mean	13.65		11.92		15.95	
Adjusted R2	0.029	0.034	0.056	0.064	0.033	0.037
B. Probability that First Visit Occurs Before the 20th Week if Pregnancy						
Treatment effect	0.086***	0.080***	0.064	0.056	0.106***	0.101***
	(0.031)	(0.030)	(0.048)	(0.049)	(0.035)	(0.034)
Control group mean	0.746		0.792		0.687	
Adjusted R2	0.019	0.023	0.024	0.029	0.019	0.022
C. Probability of at Least Four Prenatal Check-ups						
Treatment effect	0.117***	0.103***	0.128**	0.110**	0.110***	0.097***
	(0.028)	(0.028)	(0.049)	(0.047)	(0.026)	(0.025)
Control group mean	0.739		0.807		0.648	
Adjusted R2	0.060	0.065	0.117	0.120	0.040	0.046
D. Probability of Skilled Birth Attendance and First Postpartum Checkup						
Treatment effect	0.024	0.024	0.000	0.009	0.054**	0.050**
	(0.026)	(0.026)	(0.044)	(0.045)	(0.022)	(0.022)
Control group mean	0.439		0.506		0.351	
Adjusted R2	0.018	0.021	0.034	0.035	0.022	0.024
Observations	5,505	5,505	1,084	1,084	4,421	4,421
Mother fixed effects	Y	Y	Y	Y	Y	Y
Controls for covariates	N	Y	N	Y	N	Y

Notes: This table shows the effect of the BJA enrollment on different outcomes of prenatal care utilization. Columns (1), (3) and (5) report mother fixed-effects regressions estimated with ESNUT 2012 data. Standard errors are in parentheses, clustered at the mother level. Regressions (2), (4) and (6) control for mother fixed effects, cohort of birth, a quadratic specification for the mother's age at birth, sex of the child, and ranked order of birth. Observations are weighted using survey weights. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table 10: Effect of BJA Enrollment on Birth Weight

	All Sample			Urban Households			Rural Households		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Birth Weight (gr.)									
Treatment effect	-6.520	-6.969	-8.248	24.209	26.435	34.734	-3.502	-8.360	-14.338
	(41.461)	(41.517)	(39.953)	(59.615)	(58.024)	(57.086)	(41.294)	(40.724)	(41.907)
Control group mean		3,295.65			3,269.26			3,345.9	
Adjusted R2	0.031	0.048	0.047	0.054	0.067	0.072	0.032	0.056	0.056
B. Probability of Low Birth Weight (< 2,500 gr.)									
Treatment effect	-0.044*	-0.038	-0.036	-0.084**	-0.074*	-0.078*	0.002	0.002	0.000
	(0.025)	(0.026)	(0.024)	(0.039)	(0.040)	(0.041)	(0.015)	(0.015)	(0.015)
Control group mean		0.086			0.099			0.061	
Adjusted R2	0.042	0.044	0.040	0.061	0.063	0.066	0.025	0.024	0.024
Observations		3,624			958			2,666	
Mother fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls for covariates	N	Y	Y	N	Y	Y	N	Y	Y
IPW Adjusted	N	N	Y	N	N	Y	N	N	Y

Notes: This table shows the effect of the BJA enrollment on birth weight. Columns (1), (4) and (7) report mother fixed-effects regressions estimated with ESNUT 2012 data. Standard errors are in parentheses, clustered at the mother level. Regressions (2), (5), and (8) control for mother fixed effects, cohort of birth, a quadratic specification for the mother's age at birth, sex of the child, and ranked order of birth. Observations are weighted using survey weights. Regressions (3), (6), and (9) adjust survey weights by the inverse probability of having information of birth weight. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table 11: Effect of BJA Enrollment on Utilization of Postnatal Care Services. RD estimates

	Number of check-ups at age 12-24 months		Yellow fever vaccine	Probability of: MMR vaccine	Complete immuniz.	Nutritional suppl.
	ITT	LATE	ITT	ITT	ITT	ITT
Non-parametric	1.08	3.546*	0.063	0.018	0.008	0.034
Bandwidth CCT	(0.783)	(2.369)	(0.065)	(0.053)	(0.073)	(0.083)
Observations	413		663	667	740	539
Non-parametric	0.832**	2.737**	0.03	0.025	-0.005	0.06
Bandwidth IK	(0.572)	(1.763)	(0.067)	(0.039)	(0.055)	(0.059)
Observations	1,283		2,356	1,986	1,742	1,656
Non-parametric	0.824**	2.409**	0.027	0.021	0.012	0.015
Bandwidth CV	(0.509)	(1.401)	(0.044)	(0.036)	(0.051)	(0.052)
Observations	1,622		2,334	2,309	2,366	2,473
Semi-parametric	1.123**	3.69***	0.055	0.003	- 0.01	0.07
CV & quadratic poly.	(0.639)	(1.94)	(0.056)	(0.047)	(0.069)	(0.067)
Observations	1,692		2,429	2,400	2,192	2,376

Notes: Bandwidths are computed using the *rdrobust* package provided by Calonico et al. (2014b). The CCT bandwidth corresponds to the one defined by Calonico et al. (2014a), the IK bandwidth is defined in Imbens and Kalyanaraman (2011), and the CV bandwidth corresponds to the cross-validation bandwidth (see Calonico et al. (2014b)). ITT: Intention to Treat. Differences at the cut-off without adjustments for the probability of being treated. The first stage shows the effect of the eligibility rule on the probability of enrollment into the BJA. LATE: Local Average Treatment Effect is the ITT parameter adjusted by the probability of being treated estimated in the first stage regression. Each row presents the results for different Bandwidths. All regressions are estimated using a local regression with a uniform Kernel weighting method. Standard errors in parenthesis. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table 12: P-value for differences in the eligibility cut-off in RD analysis.

Variables	p-value
Number of check-ups at 0 to 5 months	0.594
Number of check-ups at 0 to 11 months	0.955
Sex, Male=1	0.205
Years of schooling of mother	0.531
Age of mother	0.196
Age of mother at birth	0.201
Household wealth index	0.313
Distance to nearest health facility (km)	0.817
Department, La Paz=1	0.315
Department, Cochabamba=1	0.324
Department, Santa Cruz=1	0.557
Altitude	0.781

Notes: This table shows the p-value of a significance test for the difference in means at the cut-off. The differences in means are obtained by a local linear regression using the bandwidth provided by Calonico et al. (2014a) for the analysis of our main outcome: number of check-ups at age 12 to 24 months.

Table 13: Effect of BJA Enrollment on Utilization of Postnatal Care Services

	All Sample		Urban Households		Rural Households	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Number of Checkups 0-23 months						
Treatment effect	3.828***	3.619***	4.327***	4.910***	2.844***	2.542***
	(0.748)	(0.739)	(1.373)	(1.477)	(0.794)	(0.783)
Adjusted R2	0.286	0.316	0.416	0.451	0.361	0.377
Control group mean	8.498		8.131		8.992	
Observations	1,980		416		1,564	
B. Probability of Yellow Fever Vaccine						
Treatment effect	0.119***	0.114***	0.147***	0.166***	0.054*	0.051*
	(0.037)	(0.035)	(0.056)	(0.052)	(0.031)	(0.030)
Adjusted R2	0.139	0.158	0.232	0.279	0.091	0.099
Control group mean	0.751		0.707		0.819	
C. Probability of MMR Vaccine						
Treatment effect	0.120***	0.117***	0.122***	0.129***	0.080***	0.079***
	(0.031)	(0.031)	(0.046)	(0.046)	(0.025)	(0.025)
Adjusted R2	0.138	0.161	0.225	0.250	0.077	0.079
Control group mean	0.837		0.819		0.864	
D. Probability of Complete Immunization						
Treatment effect	0.127***	0.124***	0.081*	0.078*	0.138***	0.134***
	(0.031)	(0.030)	(0.042)	(0.043)	(0.034)	(0.033)
Adjusted R2	0.143	0.154	0.296	0.309	0.055	0.060
Control group mean	0.715		0.730		0.692	
E. Probability of Micronutrient Supplement Intake						
Treatment effect	0.110**	0.114***	0.162*	0.177**	0.056**	0.060**
	(0.043)	(0.043)	(0.087)	(0.089)	(0.026)	(0.027)
Adjusted R2	0.111	0.117	0.178	0.190	0.100	0.103
Control group mean	0.574		0.486		0.689	
Observations	3,202		624		2,578	
Mother fixed effects	Y	Y	Y	Y	Y	Y
Controls for covariates	N	Y	N	Y	N	Y

Notes: This table shows the effect of the BJA enrollment on different outcomes of post natal care services utilization for children. Columns (1), (3) and (5) report mother fixed-effects regressions estimated with ESNUT 2012 data. Standard errors are in parentheses, clustered at the mother level. Regressions (2), (4) and (6) control for a quadratic specification of the mother's age at birth, sex of the child, and dummy variables for birth order and each month of child's age. All observations are weighted using survey weights. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table 14: Effect of BJA Enrollment on Health Outcomes of Children

	All Sample		Urban Households		Rural Households	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Z-score length for age (Standard Deviations)						
Treatment effect	0.093	0.089	-0.037	-0.012	0.158	0.157
	(0.110)	(0.109)	(0.181)	(0.192)	(0.135)	(0.134)
Adjusted R2	0.222	0.231	0.380	0.380	0.154	0.157
Control group mean	-1.264		-1.057		-1.539	
B. Probability of chronic malnutrition						
Treatment effect	-0.021	-0.019	-0.018	-0.022	-0.082	-0.081
	(0.053)	(0.053)	(0.081)	(0.082)	(0.055)	(0.055)
Adjusted R2	0.150	0.150	0.249	0.244	0.189	0.193
Control group mean	0.212		0.155		0.283	
Observations	2,599		489		2,110	
C. Probability of Anemia						
Treatment effect	-0.059	-0.057	-0.058	-0.057	-0.061**	-0.058*
	(0.041)	(0.040)	(0.069)	(0.069)	(0.030)	(0.029)
Adjusted R2	0.100	0.106	0.144	0.151	0.093	0.106
Control group mean	0.616		0.523		0.741	
Observations	3,820		743		3,077	
Mother fixed effects	Y	Y	Y	Y	Y	Y
Controls for covariates	N	Y	N	Y	N	Y

Notes: This table shows the effect of the BJA enrollment on different outcomes of health outcomes for children. Columns (1), (3) and (5) report mother fixed-effects regressions estimated with ESNUT 2012 data. Standard errors are in parentheses, clustered at the mother level. Regressions (2), (4) and (6) control for a quadratic specification of the mother's age at birth, sex of the child, a quadratic specification of birth interval and dummy variables for birth order and each month of child's age. All observations are weighted using survey weights. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

APPENDIX A: Report of Stillbirths in SNIS data

We address the quality of reporting as follows. Let S_{ij} be a binary indicator that equals to one if a pregnancy ends in a birth and equals zero if the pregnancy is terminated by an abortion or a stillbirth, for women i in municipality j. Let R_{ij} be an indicator for whether the birth is registered in official records or not. We can decompose the expected value of S_{ij} in:

$$E(S_{ij}) = E(S_{ij}|R_{ij} = 1)P(R_{ij} = 1) + E(S_{ij}|R_{ij} = 0)P(R_{ij} = 0)$$

In addition, let $NS_{ij} = 1 - S_{ij}$, so that we have:

$$E(NS_{ij}) = E(NS_{ij}|R_{ij} = 1)P(R_{ij} = 1) + E(NS_{ij}|R_{ij} = 0)P(R_{ij} = 0)$$

A threat to our estimates is the possibility that municipalities with a better registry will show up with a higher number of stillbirths. However, when we calculate the ratio of stillbirths and total births we can solve this problem by assuming that the probability of being registered in the official records, $P(R_{ij} = 1)$, is the same for stillbirths and live births. Our variable is constructed by the ratio of the following aggregations at the municipality level:

$$\sum_{i=1}^{N_j}(S_{ij}|R_{ij} = 1)P(R_{ij} = 1) = \#(live\_births_j|R_j = 1)P(R_j = 1)$$
$$\sum_{i=1}^{N_j}(NS_{ij}|R_{ij} = 1)P(R_{ij} = 1) = \#(still\_births_j|R_j = 1)P(R_j = 1)$$

Where we additionally assume that $P(R_{ij} = 1)$ is constant across births i within the same municipality j. The variable of interest in each period is formed as:

$$\frac{\#(still\_births_j|R_j = 1)P(R_j = 1)}{\#(live\_births_j|R_j = 1)P(R_j = 1)} = \frac{\#(still\_births_j|R_j = 1)}{\#(live\_births_j|R_j = 1)} = Rate_j$$

Hence, we can control for the fact that different municipalities register births and stillbirths at different rates. However, the interpretation of our results refers to the subset of births that are in fact registered by the municipalities, conditioning the external validity to this sub-population.

APPENDIX C: Differences in means between households (HHs) enrolled and not enrolled in the BJA in subsample of fixed effects.

Table C 15: Differences in Means between Households with Pregnancies Enrolled and Not Enrolled in the BJA in Subsample of Fixed Effects.

	(1)	(2)	P-value for difference	(3)	(4)	P-value for difference
Variable	HHs with at least one pregnancy enrolled	HHs with no pregnancy enrolled	(1)-(2)	HHs in sample	HHs out of sample	(3) - (4)
Age of mother	27,925	27,867	0.869	29,072	27,903	0.000
Years of schooling of mother	7,705	8,460	0.002	10,088	7,994	0.000
Mother Indigenous=1	0.490	0.542	0.064	0.427	0.510	0.000
Number of children	2,210	2,210	0.983	1,332	2,210	0.000
Labor force participation	0.533	0.560	0.355	0.611	0.544	0.000
Household size	5,633	5,539	0.359	4,913	5,597	0.000
Sex of head of household	0.093	0.080	0.485	0.139	0.088	0.000
Per capita consumption (in $Bs)	582,802	554,987	0.297	798,423	572,178	0.000
Household wealth index	5,293	5,324	0.804	6,325	5,304	0.000
Distance to nearest health facility (in Km.)	2,854	2,537	0.217	2,163	2,733	0.002
Number of pregnancies	2.223	3.282		5.505	6.876	

Table C 16: Differences in Means between Households with Children Enrolled and Not Enrolled in the BJA in Subsample of Fixed Effects.

	(1)	(2)	P-value for difference	(3)	(4)	P-value for difference
Variable	HHs with at least one pregnancy enrolled	HHs with no pregnancy enrolled	(1)-(2)	HHs in sample	HHs out of sample	(3) - (4)
Age of mother	27.23	28.85	0.000	29.03	28.25	0.00
Years of schooling of mother	8,004	7,707	0.354	9,680	7,819	0.000
Mother Indigenous=1	0.323	0.512	0.000	0.326	0.440	0.000
Number of children	2,240	2.23	0.763	1,371	2.23	0.000
Labor force participation	0.509	0.557	0.224	0.581	0.539	0.017
Household size	5,543	5,799	0.045	5,058	5,703	0.000
Sex of head of household	0.093	0.081	0.560	0.131	0.085	0.000
Per capita consumption (in $Bs)	678,357	472,216	0.004	751,905	549,398	0.000
Household wealth index	0.795	0.240	0.001	1,331	0.448	0.000
Distance to nearest health facility (in Km.)	2,835	2,968	0.671	2,278	2,918	0.023
Number of children	1.015	2.187		3.202	8.156	

APPENDIX D: Cost-Effectiveness Analysis

Parameters Associated to the BJA Cost-Effectiveness Analysis

Demographics	Source	Value
Eligible population: pregnancies	UDAPE	281.272
Eligible population: Children	UDAPE	198.722
Enrollment rate: pregnancies	ESNUT 2012	0.25
Enrollment rate: children	ESNUT 2012	0.55
Enrolled pregnancies: 2009-2013 yearly average	BJA records	70.318
Enrolled children: 2009-2013 yearly average	BJA records	109.496
Average number of checkups: pregnancies	ESNUT 2012	3.16
Average number of checkups: children	ESNUT 2012	4.33
Rural (%)	ESNUT 2012	0.35
Percentage of check-ups paid: pregnancies	ESNUT 2012	0.60
Percentage of check-ups paid: children	ESNUT 2012	0.65
Average children enrolled per household	ESNUT 2012	1.24
Total number of births in 2012	Census 2012	291.158
Children between 0 and 6 years old in 2012	Census 2012	1,529,689

BJA impact on:	Source	Value
Prenatal care checkups	Evaluation report	0.42
Child health checkups	Evaluation report	3.62
Immunization: Yellow Fever	Evaluation report	0.12
Immunization: MMR	Evaluation report	0.11
Micronutrient supplementation	Evaluation report	0.11
First postpartum checkup (Rural)	Evaluation report	0.07
Probability of low birth weight (Urban)	Evaluation report	0.08
Probability of Anemia (Rural)	Evaluation report	0.05
Rate of Stillbirths	Evaluation report	0.11

Information for DALYs averted	Source	Value
Incidence of low birth weight	WHO	0.08
DALYS averted for low birth weight (1,000)	WHO	81.00
DALYS for each low birth weight case	Own elaboration	3.39
Perinatal mortality (1,000)	WHO	19.00
DALYS averted for perinatal mortality (1,000)	WHO	237.90
DALYS per each dead birth	Own elaboration	43.00
Incidence of anemia in children 0 to 6 years old	WHO	0.52
DALYS for anemia (1,000)	WHO	39.50
DALYS per each case of anemia	Own elaboration	0.05

Costs Associated to the BJA Cost-Effectiveness Analysis

A. Fixed costs	
Cash transfers ($Bs) (year 2013)	$175,064,055
Costs of program's operation ($Bs) (year 2013)	$15,619,036
Community health workers ($Bs) (year 2013)	$34,451,869
Cost of management (transfers fees, etc) ($Bs) (year 2013)	$1,940,520
B. Variable costs	
Prenatal and child health checkups	
Number of additional prenatal checkups	29.604
Number of additional child health checkups	396.376
Total number of additional checkups	425.979
Hours per each visit	0.5
Value of each hour ($Bs) (skilled health personnel)	37.73
Item Cost ($Bs)	$8,036,101
First postpartum checkup	
Number of additional postpartum checkups	4.571
Hours per each visit	0.5
Value of each hour ($Bs) (specialized health personnel)	43.75
Item Cost ($Bs)	$99.98

B. Variable costs	
Yellow fever vaccine	
Number of additional vaccines	12.811
Unitary cost ($Bs)	10.48
Item Cost ($Bs)	$134.26
MMR vaccine	
Number of additional vaccines	7.946
Unitary cost ($Bs)	20.33
Item Cost ($Bs)	$161.54
Miconutrient supplement	
Number of additional supplements	11.826
Unitary cost ($Bs)	20.50
Item Cost ($Bs)	$242.42
Household's travel cost to health facility	
Total travels for prenatal care checkups (1 per visit)	29.604
Estimated time per travel (hours)	0.26
Value of each hour (valued at minimum wage) ($Bs)	2.81
Item Cost ($Bs)	$21.61
Total travels for child health checkups (1 per visit)	319.658
Estimated time per travel (hours)	0.26
Value of each hour (valued at minimum wage) ($Bs)	2.81
Item Cost ($Bs)	$233.38
Household's travel cost to payment centers	
Total travels	225.54
Estimated time per travel (hours)	1.25
Value of each hour (valued at minimum wage) ($Bs)	2.81
Item Cost ($Bs)	$791.65
Fixed cost	$227,075,480
Variable Cost	$9,720,943
Total costs ($Bs)	**$236,796,423**
Total costs ($USD)	**$34,022,475**

Benefits Associated to the BJA Cost-Effectiveness Analysis

Type	Value
Low birth weight (Urban)	
Total births enrolled in BJA (Urban areas)	45.875
New births without low birth weight	3.67
DALYs averted from reducing low birth weight	12.451
Rate of stillbirths	
Total births enrolled in BJA	70.318
Total deaths before BJA	1.336
New births due to a reduction of rate of stillbirths	147
DALYs averted from reducing stillbirths	34.963
Incidence of anemia (Rural)	
Total children enrolled in BJA (Rural)	38.061
New children without anemia (Rural)	1.979
DALYs averted from reducing incidence of anemia	98
Total DALYs averted	**47.512**
Costs per DALY averted ($USD)	**$690.2**