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Paying Patients for Prenatal Care: The Effect of a Small Cash Transfer on Stillbirths and Survival

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Abstract:

We study the effects of conditional cash transfers to pregnant women on stillbirths and child survival in Bolivia. Payments are conditional on compliance with medically recommended prenatal care and skilled birth attendance. At a value equivalent to just 1% of monthly household consumption, the payments are the smallest amongst national cash transfer programs in Latin America. Using multiple data sources and empirical methods, we show that the intervention reduced the average rate of stillbirths by 9.5% to 22.3%, and increased the survival rates of birth cohorts exposed to the program by 3.5% to 16.8%. The causal pathways of these effects are consistent with evidence of increased utilization of recommended health care services, including early initiation of prenatal care (17%), at least four prenatal visits (16%), and skilled birth attendance (15%). Given the modest transfer amounts and considerable effects on health care utilization, we posit that final health outcomes are likely driven by the health care conditionality, rather than an income effect. The intervention is highly cost-effective, at $184.54 per DALY averted, making the program's pay-per-compliance design a promising policy alternative.

JEL codes: H51, I12, I38, J18, O15.

Key Words: Conditional Cash Transfer; Demand Incentives; Stillbirths; Child Survival; Health Care Utilization; Impact Evaluation.

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I. Introduction

Despite the expansion of free or low-cost preventive care in the developing world, the utilization of preventive services often remains below recommended levels (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 (Banerjee and Duflo 2011). As a response many countries have implemented conditional cash transfer (CCT)[2] programs (Fiszbein, Schady, and Ferreira 2009; Adato and Hoddinott 2011). CCTs work through different mechanisms (Filmer and Schady 2008; Baird, McIntosh, and Özler 2011; Benhassine et al. 2015). For instance, they can work as a signaling device for the services they promote or as a positive income shock if payments are sizable, or both. The distinction of pathways is relevant, and most studies of CCTs are not able to disentangle one effect from the other (Attanasio, Oppedisano, and Vera-Hernández 2015).

We study the health effects of the Bono Juana Azurduy (BJA)[3,4], a CCT program in Bolivia that pays $7 for eligible prenatal care checkups and $18 for the joint compliance with childbirths attended by skilled health personnel and a postpartum visit. These modest transfer amounts, equivalent in total to 1% of monthly household consumption,[5] are the smallest amongst national CCT programs in Latin America (Table 1). Using instrumental variables and a

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

[2] CCTs are demand side incentives that consist of monetary payments to households, conditional on compliance with as the mentioned "co-responsibilities" such as medical visits for children or pregnant women, or school enrollment and attendance for children. CCT programs are widespread throughout Latin America and much of the developing world (see Fiszbein, Schady, and Ferreira 2009; Adato and Hoddinott 2011).

[3] Juana Azurduy is a Bolivian heroine of independence from Spain.

[4] The BJA program provides incentives for health care utilization during pregnancy, childbirth, and for children under 2 years old. We focus on the prenatal period in this study.

[5] The maximum potential transfer from BJA during the 9 month prenatal period is Bs. 320, which is 1% of average monthly household consumption of Bs.3,381 or 4.8% of per-capita consumption of Bs.735.96 (author's calculation with ESNUT 2012).

difference-in-difference strategy, we analyze health information system and national census data matched to program administrative records to estimate effects on the rate of stillbirths[6] and child survival. Compared to baseline, a municipality with average BJA enrollment rates experienced declines in stillbirths of 9.5% to 22.3% and increased survival rates of children exposed to BJA during pregnancy by 3.5% to 16.8%.

To explore causal pathways, we use household survey data and a sibling fixed-effects model. We analyze detailed information regarding a mother's five most recent pregnancies, including prenatal health care utilization and birth outcomes. Following BJA eligible and ineligible pregnancies for the same mother, we find that program participation increased the early initiation of prenatal care (in the first 20 weeks of the pregnancy) by 17%. The results on early initiation are relevant since there is strong empirical support for the relationship between early initiation of prenatal care and birth outcomes (see Rosenzweig and Schultz 1983, Grossman and Joyce 1990, and Evans and Lien 2005). Our results also show that women enrolled in the BJA were 16% more likely to comply with at least four antenatal care visits, as established in the national protocol and recommended by the World Health Organization (WHO 2006).

Our results also show that mothers in rural areas were more likely to have deliveries attended by skilled health personnel (as opposed to non-qualified personnel or relatives). 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). Furthermore, we find that the program significantly reduced the rate of low birth weight in urban areas. We interpret the

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[6] Stillbirth refers to a dead born fetus that dies at or after the 28th pregnancy week. Before that, the death is defined as caused by abortion or miscarriage. Intrauterine deaths occur either before onset of labor (antepartum death) or during labor (intrapartum death) (see WHO 2006). We study stillbirths in Bolivia because of its high 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 than mortality records. Once a birth is recorded, the data indicates whether the fetus is born dead or alive.

absence of significant birth weight effects in rural areas with caution given the program's effect on stillbirths, which could change the composition of birth weights in surviving newborns.

Our results contribute to the small pool of studies on the effects of CCTs on final health outcomes such as birth weight and early-life mortality. One reason for the relative dearth of empirical evidence in this area is that changes in mortality are unlikely to be detected using household surveys due to the low prevalence of these events, requiring instead large, population level data sources such as the national health systems and census data we use here. Moreover, the evidence to date in Latin America comes predominantly from programs that provide large and sustained income transfers such as Progresa in Mexico (Barham 2011), Bolsa Familia in Brasil (Rasella et al. 2013) and PANES in Uruguay (Amarante et al. 2016). Our results indicate that even small transfers linked to specific care-seeking events can have large health impacts, with important cost-effectiveness implications. Our analysis shows that the cost of the BJA per DALY averted was 7.4% of GDP per capita, suggesting that the program's pay-per-compliance design is highly cost-effective.

Furthermore, to our knowledge, this paper is the first to study the effects of CCTs on the rate of stillbirths, an indicator that itself has received less attention than maternal or neonatal mortality[7]. Barham (2011) and Rasella et al. (2013) show that CCTs were effective in reducing infant and child mortality in Mexico and Brazil, respectively; and Lim et al. (2010) and Randive, Diwan, and De Costa (2013) find effects on neonatal and perinatal mortality in India. This is the first study to provide evidence of the potential contribution of CCTs on increasing child survival at even earlier stages in life.

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[7] According to Lawn et.al (2016), 2.6 million stillbirths occur in a given year with approximately 98% of them low- and middle-income countries, where most CCTs have been implemented. This is a sizable number compared to the 6.3 million children that die before the age of 5, a much more studied outcome (Wang et al. 2014).

We also contribute to recent evidence on the marginal impact of attaching conditions to cash transfer programs (e.g., Baird, McIntosh and Özler 2011 and Benhassine et al. 2015). CCTs may affect health by signaling the value of the co-responsibilities and nudging increased health care utilization. Or, if payments are sizable relative to a household's income, CCTs may work through a positive income shock that boosts consumption of food, medication, health care and other inputs to the health production function (e.g., Aizer et al. 2016). As discussed by Filmer and Schady (2008) and Attanasio, Oppedisano, and Vera-Hernández (2015), among others, this distinction is highly policy relevant. If CCTs only work through an income effect, conditioning cash transfers on "co-responsibilities" would not be necessary,[8] whereas if the effect was explained through the signaling channel, payment amounts could be adjusted downwards and conditioned on specific medically recommended events, for a more cost-effective design. Considering the small transfer amounts and conditionality structure of the BJA, we argue that final health effects are likely driven by increased health care utilization induced by the co-responsibilities, rather than through increased income and consumption.

The remainder of the paper is organized as follows. In section 2 we discuss the country context, explain the program rules and discuss the mechanisms through which the program may affect health outcomes. In the third section we describe data sources, and in section 4 we present the different identification strategies used in our analysis. Section 5 presents the results on stillbirths, cohort survival, prenatal care utilization and birth weight. In section 6 we present a cost effectiveness analysis of the program and section 7 concludes.

[8] There is evidence that transfers that do not condition on behavior or do not monitor the compliance with the announced requirements can have positive health effects on children. See Amarante et al. (2016) and Aizer et al. (2016) for a discussion. Black et al. (2014) also find effects of pure subsidies on academic outcomes of children.

II. Context and Intervention

A. Country Health Context

While Bolivia has experienced significant improvements in its population health and nutrition indicators over recent decades, the country continues to lag behind other Latin American countries (WHO 2013).[9] According to the 2008 Demographic and Health Surveys (DHS), approximately four of every ten under-five deaths occurred during the first month of life. Moreover, neonatal mortality reported very little progress between 2003 and 2008, remaining at 27 deaths per 1,000 live births. The maternal mortality rate in 2003 was as high as 229 deaths per 100,000 live births according to the DHS of that year.[10] In terms of child nutrition, 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 births in Bolivia were delivered by skilled health personnel and 72% of pregnant women had at least four antenatal care visits during their pregnancy (see Table 2). However, these national averages hide considerable disparities within the population. In rural areas, coverage is significantly lower, reaching 51% of skilled birth attendance and 60% of pregnancies with at least four prenatal visits. Table 2 shows that the rates of utilization for different health services in Bolivia are much lower relative to the Latin American average, where 94% of deliveries were attended by skilled personnel and 86% of pregnant women had at least four antenatal visits.

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[9] 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 that the average for the Latin America and Caribbean (LAC) region in 2011 was 16 per 1,000 live births (WHO 2013; UDAPE 2015).

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

B. 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 provide a free basic health care package.[11] The Universal Maternal and Child Insurance (SUMI by its Spanish acronym), implemented since 2003, covers the cost of over 500 maternal and child health interventions, thus lowering the financial barrier to health care. Yet despite those efforts on the supply side, utilization of key health services remained low.

To incentivize the demand for maternal and child health services provided by the SUMI, in May of 2009 the government launched the BJA nation-wide.[12] 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 that are conditioned on the use of selected preventive services. The program's "co-responsibilities" and associated payments are shown in Table 3. BJA pays pregnant women 50 Bs ($7 USD) for each antenatal visit up to a maximum of four visits, and 125 Bs ($18 USD) for births that jointly comply with assistance by skilled health personnel (at a health facility or at home) and an additional follow up check-up within the first seven days after birth. While this study focuses on the prenatal period, the BJA also covers children up to the age of two years, paying 120 Bs ($17 USD) for each health check-up in the first 2 years of life, up to a maximum of 12 visits that are scheduled every other month.

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[11] 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 antenatal 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.

BJA's conditionality structure differs from traditional CCT programs designed to provide income support. 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 the child. BJA is also universally available to all eligible women and children, and does not target the poor.[13] In addition, the program pays separately for each eligible health visit completed by the mother or child, with a specific amount for each type of visit. Finally, the total maximum transfer amount in the prenatal period is small, equivalent to 1% of household or 4.8% of per-capita consumption. With less than full compliance, actual transfers are only half this amount on average. This makes the BJA transfers small compared to other CCT programs in the region that pay regular monthly or bi-monthly payments equivalent to an average of 17% of per-capita consumption (Fiszbein, Schady, and Ferreira 2009).

C. 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. 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. 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

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

of enrollment, to guarantee a minimum exposure of 12 months to the program. To reduce fertility incentives, beneficiary pregnancies must be separated by at least 24 months. As a pre-condition for enrollment, an identity card and birth certificate must be presented for the pregnant women and child, respectively, in addition to a pregnancy test for pregnant women.

Figure 1 shows the evolution of enrollment rates in the program for pregnant women, obtained from the Health and Nutrition Evaluation Survey 2012 (ESNUT 2012), and BJA administrative records.[14] On average, the enrollment rate of eligible women was approximately 33% between 2009 and 2012, with a decreasing trend over time. 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%).

D. The role of payment centers

BJA payments were initially managed by local branches of two national banks.[15] In 2010, the program expanded the number of payment centers by contracting the services of a financial service platform that acted as a broker between the program and additional private providers (banks and credit cooperatives). As such, the availability of payment centers was determined by the installed capacity of financial services in each municipality. If no points of payment were

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[14] Enrollment rates using survey data are based on self-reported retrospective data from the ESNUT 2012. Enrollment rates using BJA administrative data, use official enrollment records as the numerator and eligible population projections as the denominator.

[15] In distant rural communities, payments were sometimes managed by the Armed Forces through mobile field teams. Payments by the Armed Forces were heterogeneous and non-continuous in terms of geographic coverage and timing. Payments through the Armed Forces ceased in mid-2010 when the program contracted additional financial service providers.

available in a municipality, then beneficiaries had to travel to the nearest municipality with a payment center in operation.

Table 4 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 municipalities had at least one payment center available. Amongst municipalities with at least one payment center there were on average 10 payment centers per 1,000 enrolled women, with large variation across municipalities. By 2012, the percentage of municipalities with payment centers increased to 34.9%, with an average of 16.8 centers per 1,000 enrollees. The consequences of the low coverage of payment centers are partially reflected in Figure 2, which shows the percentage of co-responsibilities that are actually paid by the program by year and type.[16] In 2009 only 33.9% of antenatal care check-ups and only a third of attended births that qualified for a transfer were actually paid. This gap between complied and paid co-responsibilities was reduced over the following years, reaching 90% of completed payments in the final years of our data.

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

According to national clinical guidelines, the content of the antenatal 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 antenatal visits, among others (see MINSAL 2011). A series of papers published in Lancet in 2011 argue that most of the risks associated to stillbirths are highly preventable, such

[16] Other causes included changes to the program’s information system that delayed payment activations

as maternal infections, non-communicable diseases, and nutrition, as well as other lifestyle factors.[17] Compared to other countries in the Latin America and Caribbean (LAC) region, Bolivia has one of the highest adolescent birth rates, a low prevalence of pregnant women who are tested for syphilis at the first prenatal care visits, and a high prevalence of women who test positive for syphilis among those tested (see Table 2). Likewise, Cousens et al. (2011) and WHO (2013) show that the rate of stillbirths in Bolivia was one of the highest in the LAC region in 2009 (see Table 2).[18]

As such, one of the primary mechanisms through which the BJA may affect stillbirths is by improving pre- and perinatal care utilization, i.e. increasing the early initiation of prenatal care and number of visits during pregnancy, as well as increasing skilled birth attendance. Early initiation of prenatal care 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, Rooney, and Villar 2001; Campbell and Graham 2006). In particular, the 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. 2001; Hawkes, Gomez, and Broutet 2013). Early prenatal care also allows providers to advise mothers on proper prenatal nutrition and prevention activities (e.g. Fiscella 1995, Kramer 1996, and Slattery and Morrison 2002). 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

[17] See Bhutta et al. (2011), Flenady et al. (2011), Frøen et al. (2011), Goldenberg et al. (2011), Lawn et al. (2011), and Pattinson 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 (intra-partum death) (see WHO, 2006). Our definition of stillbirths includes stillbirths, abortions, or any other definition of death during the first 24 hours after birth.

[18] 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.

complete period of pregnancy.[19] In addition to preventive prenatal care, de Bernis et al. 2016 state that half of all stillbirths occur during labor, partially due to lack of skilled birth attendance in low-income countries. Furthermore, the presence of skilled health professionals at delivery can have positive outcomes on early neonatal mortality (Moss et al. 2002).

III. Data

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

The stillbirth outcome data comes from the National Health Information System (the *Sistema Nacional de Información de Salud*, or 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.[20] We downloaded information for each municipality on the number of total births, live and stillbirths.[21] Health facilities consolidate monthly information on the outcome of each birth. 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.[22] We define the variable as a count of stillbirths due to all causes within a year and municipality.[23]

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[19] The World Health Organizations recommends at least four prenatal care visits (see WHO 2006).
[20] See http://snis.minsalud.gob.bo/. Access 05/30/2015.
[21] 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.
[22] As such, the data are reliable only for births attended by a health professional in health facilities or at home.
[23]The guidelines for the SNIS monthly service production report does not explicitly specify the minimum number of weeks of pregnancy to identify a stillbirth or fetal death, so this classification may include some abortions or miscarriages occurred before the 28th pregnancy week. Stillbirths and neonatal deaths are generally underreported (WHO, 2016). We address this issue in Appendix A.

To evaluate the effects of the BJA on stillbirths, we use SNIS data to construct a municipal level panel of stillbirths per 1,000 live births from 2005 to 2012.[24] The resulting dataset includes 327 municipalities and 2,616 municipality-year observations.[25] For some municipality-year observations the system reports zero stillbirths or live births. This may reflect intermittent reporting from some municipalities and not necessarily a true absence of births or stillbirths. In section 4 we argue that our identification strategy is robust to differential rates of reporting across municipalities in the country, and provide additional details in Appendix A.

Our second source of information is the Population Census of 2012.[26] For each municipality, we count the number of children aged 0 to 6 years, constructing a municipal level panel where each observation represents a municipality-year birth cohort of surviving children.[27] Children aged 4 years and older in 2012 were never exposed to the BJA while in-utero, since they were born after the implementation of the program in year 2009. Hence, the BJA should have no effect on the cross-municipality differences in cohort-size for these age-cohorts, but could have affected younger cohorts by increasing child survival. We develop this idea further in section 4.

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[24] We follow a similar definition of rate of stillbirths used in Lancet Series 2011: Bhutta et al. (2011), Flenady et al. (2011), Frøen et al. (2011), Goldenberg et al. (2011), Lawn et al. (2011), and Pattinson et al. (2011). These authors use the ration of stillbirths to total births, we use the ratio of stillbirths to total live births. The results do not change if we use one or the other.

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

[26] In addition to information on the number of people and households, and their geographic distribution over the country, the census also provides information on demographic characteristics, economic wellbeing 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.

[27] A similar approach was implemented by Jayachandran (2009) to study the effect of wild fires on early life mortality in Indonesia and Miller and Urdinola (2010) for the effect of economic shocks and child survival in Colombia.

B. 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 with detailed information on maternal and child health outcomes. The full sample covers 8,433 households (2,456 urban and 5,977 rural) in 424 PSUs.[28] 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. 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.

IV. Empirical Strategy

Our quasi-experimental identification strategies detect the effects of BJA 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 data source. Below, we divide them accordingly.

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[28] The sampling design allows disaggregation by urban and rural areas, as well as by macro-regions (highlands, valleys and lowlands). The survey's multistage probabilistic sample uses the 2001 Census sampling frame for the selection of primary sampling units (PSU). The survey provides sampling weights to adjust for different selection probabilities.

A. Municipality Level Outcomes

a. Rate of Stillbirths

We construct the number of stillbirths (numerator) per 1,000 live births (denominator) in each municipality for each year between 2005 and 2012, using SNIS data. The treatment variable is the percentage of eligible women enrolled in BJA by municipality over the same period.[29] We estimate the following regression:

$$Y_{j,t} = \phi_j + \phi_t + \delta_1 Av.\mathrm{Enroll}_{j,t} + X'_{j,t}\beta + \varepsilon_{j,t} \qquad (1)$$

where, $Y_{j,t}$ is the rate of stillbirths for municipality j at year t; $Av.\mathrm{Enroll}_{j,t}$ 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.[30] 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. $X'_{j,t}$ controls for supply side expansion of health services using the number of health facilities per 1,000 inhabitants in municipality j at year t and data on night light per capita per each municipality x year observation, and interactions between controls; and $\phi_j, \phi_t, \varepsilon_{j,t}$ are municipality fixed effects, time fixed effects, and unobservable characteristics that vary across municipalities and time, respectively. We

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

[30] 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 multi-collinearity. Using the average of two years also helps to smooth out possible measurement error in the independent variable, in particular for projections of total pregnancies in each municipality used in the denominator.

include a binary indicator for each year to control for shocks, 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 315 municipalities of the 327 in the country, with a total municipality-year sample size of 2,520.[31] We run regression (1) weighting each observation by the total number of pregnancies in a municipality in year 2008.[32]

The key identifying assumption in estimating (1) through OLS is that the outcome level observed in municipalities with high enrollment rates would have been the same as in municipalities with low enrollment rates, had they experienced equally low levels of enrollment. And vice-versa. Although this assumption is not testable, commonly known as "parallel-trends" in potential outcomes, we explore whether the pre-intervention trends in outcomes were similar across municipalities with different take-up levels as suggested in Meyer (1995).

Figure 3 shows the evolution of the rate of stillbirths per 1,000 live births over time. 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 3 provides a graphical representation of what we 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

[31] We drop municipalities with fewer than 250,000 inhabitants. These are the biggest urban centers in the country and are very different from the rest of the municipalities included in the study. We also drop municipalities with very large values for the number of payment centers and municipalities that had at least one observation with an enrollment rate above 100%. We do this to avoid the effect of implausible outliers in the data. The final sample corresponds to 96% of all municipalities.

[32] See Solon, Haider, and Wooldridge (2015). In practice, our results are robust to using different weights.

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

between the lowest and highest enrollment groups, but appear to differ for municipalities in the middle tercile.

The figure also shows that municipalities with a higher average rate of stillbirths in pre-program years (solid line) are those with a higher enrollment rate among pregnant women. Another aspect to observe in the figure is that the stillbirth rate decreases at a much faster rate in municipalities with higher enrollment rates in the program. Although this is suggestive of program impacts, it may also be that some municipalities enrolled their eligible population at a higher rate because they were expecting stillbirths to rise in future years. If this were the case, enrollment rates would be endogenous to trends in stillbirths, and estimating (1) through OLS would lead to a biased estimate of the program's effect on the rate of stillbirths.

To overcome this potential source of endogeneity, we use the number of financial payment centers in each municipality and period, discussed in section 2, as an instrument for enrollment rates.[34] Since our analysis includes municipality fixed effects, we need the number of payment centers to vary across municipalities and time (otherwise there would be no variation to estimate δ_1). As we show in Table 4, there are significant changes in the number of municipalities with a financial entity available, and the population covered by the payment centers also varies. 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). Although this is not testable directly, we can examine whether changes in payment centers over

[34] Another potential source of variation in enrollment rates might be the change in political affiliation of local governments. We find no correlation between program enrollment rates and political affiliation of mayors.

time since the start of the program are related to the trends in stillbirths before the program started. We run the following regression using data for the years 2005 to 2008:[35]

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

Where $\Delta(P.C.)_{j,2009}$ is the percentage change in the number of payment centers from 2008 to 2009 in municipality j.[36] 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 the years 2006 through 2008 with respect to year 2005. We include time and municipality fixed effects. If the coefficients were jointly significant, there would be evidence against the assumption that the change in payment centers, i.e. the instrumental variable we use, is not 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 5. The change in the rate of stillbirths before 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 0.96. 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.[37] 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 provides evidence in favor of

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[35] This is commonly used 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 (e.g., Barham 2011).

[36] We transform the number of payment centers using the inverse hyperbolic function (see Burbidge, Magee, and Robb 1988). A justification for this is provided in subsection 4.B.

[37] The main assumption in the 2SLS analysis is that the change across time in the number of payment centers within municipalities affects the rate of stillbirths only through its effect on enrollment rates, usually known as the exclusion restriction. 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.

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.

First Stage Specification

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

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

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

$$A\text{v.Enroll}_{j,t} = \phi_j + \phi_t + \lambda Ln(P.C.) + W'_{j,t}\beta + \omega_{j,t} \qquad (3)$$

Where $W'_{j,t} = [X'_{j,t}, Ln(E.W)]$, which replaces $X'_{j,t}$ in the second stage estimation. In section 5, we show that payment centers are a strong predictor of the enrollment rate, and therefore propose using the predicted values of (3) to obtain a consistent estimate of δ_1 using 2SLS. Finally, since

we have many observations with the value of zero, we prefer to use the inverse hyperbolic sine (IHS) of the number of payment centers, which is defined at zero, instead of the natural logarithm in order to avoid imputing an arbitrary number to the zero-valued observations.[38] The results are not sensible to these two different strategies.

b. Analysis of the Effect of the BJA on Child Survival

To estimate how child survival is affected by enrollment in the BJA, we construct residence municipality x birth year counts using Bolivia's 2012 Census. In particular, cohorts between the ages 0 to 3 (born between the years 2009 and 2012) were exposed to the program in-utero, while the age-cohorts of 4 to 6 (born in the years 2006 to 2008), where too old to be enrolled while in-utero. To illustrate this further, suppose 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 then estimate the following regression:

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

where S_{ijc} 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 was enrolled in the BJA while in-utero. 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:

[38] See Burbidge, Magee, and Robb (1988) or MacKinnon and Magee (1990) for a discussion. The inverse hyperbolic sine of x is $h(x) = \ln(x + \sqrt[2]{x^2 + 1})$. See a discussion in http://worthwhile.typepad.com/worthwhile_canadian_initi/2011/07/a-rant-on-inverse-hyperbolic-sine-transformations.html.

$$Mort\_rate_{jc} = \beta_1 Av.Enr_{jc,c-1} + \phi_c + \phi_j + \varepsilon_{j,c} \quad (5)$$

where $Mort\_rate_{jc}$ is the mortality rate of cohort c in municipality j; $Av.Enr_{jc,c-1}$ is similar as before in (1).[39] However, data on mortality levels for each birth cohort and municipality in Bolivia are unavailable. An indirect way to infer survival rates is to work with the size of each cohort in the Census of 2012. Assuming, for now, that there is no internal migration across municipalities, the mortality rate is directly related to the size of the cohorts and the number of births, given by:[40]

$$\log(size)_{jc} = \log(birth)_{jc} - \text{Mort\_rate}_{jc} \quad (6)$$

Replacing this term in the previous equation we obtain:

$$\log(size)_{jc} = \pi_1 Av.Enr_{jc,c-1} + \phi_c + \phi_j + \underbrace{\varepsilon_{j,c} + \log(birth)_{jc}}_{unobserved} \quad (7)$$

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. We use the count in the Census for age cohorts 0 to 6

[39] The only difference is that the enrollment rate for age-cohort 0-1 in the Census would be the average enrollment rate of the years 2012 and 2011, for age-cohort 1-2 we use years 2011 and 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 $\varepsilon_{j,c}$ 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 years old, 1-2 years old, and so on.

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

years in each municipality. One advantage of using the size of the cohort instead of mortality reports lies in the inclusion of the survival of children at birth and to later life stages, which is likely measured with lower error than mortality rates (see e.g. Jayachandran 2009, Miller and Urdinola 2010, Barham 2011). 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 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 were 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 discussed for stillbirths to (7). The key identifying assumption is the same: potential outcomes in survival rates should have no relation to changes in the number of payment centers available. Similar to the analysis of stillbirths, we find that the trends in survival of unexposed cohorts are

unrelated to changes in the number of payment centers available (see column 1 in Table 7). Therefore, we interpret the 2SLS results of the effects of the BJA on population counts as the effects on the survival of different cohorts or cumulative mortality (see e.g., Miller and Urdinola 2010).

B. Individual Level Outcomes of Maternal and Newborn Health: Antenatal Care Utilization, Assisted Delivery, and Low Birth Weight

The ESNUT 2012[41] collects information on the utilization of health services for every pregnancy experienced by women living in the household since 2007. We take advantage of the event-study 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 enrolled in the BJA. Our analysis is similar in spirit to a linear differences-in-differences approach, comparing the difference in outcomes between eligible and ineligible (baseline) pregnancies for the same mother, across BJA enrolled and not-enrolled mothers. We analyze the 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

[41] For a full description of the ESNUT survey see http://www.udape.gob.bo/portales_html/docsociales/Libroesnut.pdf (accessed April 14, 2017).

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 across the different pregnancies of the same mother. Cross-sectional estimation of equation (8) by OLS would generally give biased estimates of the treatment parameter, δ, because unobserved components of $\eta_i,\ \gamma_s$ 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 across different pregnancies, treated and non-treated by the BJA, of the same mother, thus eliminating the fixed unobserved component, η_i, in equation (8).

The main identifying assumption would be violated if mothers behave systematically different from one pregnancy or child to another. In the context of BJA, any variation in the treatment status of different pregnancies for the same mother is arguably due to program availability as of 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 in the BJA during their pregnancy. To address this issue, we include controls for the 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.[42] Moreover, the average time between pregnancies in our sample is approximately 2.5 years, which constrains the likely variation in household

[42] 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”:

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.[43]

Another salient aspect of the research design is that any variation in treatment is limited to mothers with at least one ineligible and one BJA eligible pregnancy. This allows for the selection into treatment of pregnancies to be mainly due to the program start date in May 2009, which is arguably independent of the potential outcomes of each observation. Finally, the validity of our results depends, at least in part, on the assumption that mothers enrolled in the BJA are not different, other than in their treatment status, from non-enrolled mothers. Table 10 show that demographic characteristics of mothers enrolled and not enrolled in the program are very similar.[44]

V. Impacts of the BJA

A. Municipality Level Outcomes: Rate of Stillbirth

Table 5 shows the main results of the BJA on the rate of stillbirths. 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 0.078

[43] We also attempted to do the parallel trends test using mothers with more than two children in pre-treatment periods. However, the sample size is too small.

[44] 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 the results. In addition, the comparison of households in and out of sample in Table 10 suggests that 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.

stillbirths per 1,000 live births.[45] To approximate this association in an average municipality under typical enrollment rates, we convert these results for the average enrollment rate of the sample of 0.28, shown at the bottom of the table. As such, an average municipality experienced a decline of 0.28 x 7.8=2.18 stillbirths per 1,000 live births, which corresponds to a 9.5% decline with respect to the average rate of stillbirths at baseline. In section 4 we discuss why the OLS estimate may be inconsistent and how we can use the ratio of payment centers in a municipality for a given year to instrument the enrollment rate variable in (1). Column (3) of Table 5 shows the reduced form estimates where we substitute the average enrollment rate with the instrumental variable in (1). The results show that a 1% increase in the number of payment centers reduces the rate of stillbirths by 1.08, which corresponds to a 4.7% decline with respect to the average rate of stillbirths at baseline.

Column (4) of Table 5 shows the First Stage of the 2SLS estimation. A 1% increase in the number of payment centers increases the enrollment rate by 5.9 percentage points. This corresponds to an elasticity of 0.21, i.e. a 10% increase in the number of payment centers increases the enrollment rate by 2.1%. At the bottom of Table 5, we report the F-test (F=40.78) and p-value for the significance of the instrument. 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 the average enrollment in the BJA is associated with a reduction of 0.184 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. An average municipality experienced a decline of 0.28 x 18.42=5.1 stillbirths per 1,000 live births, which corresponds to a

[45] The rate of stillbirths is calculated using the number of total live births as the denominator. Results are unchanged if the denominator is constructed as total births (live births plus stillbirths).

22.1% decline with respect to the average rate of stillbirths at baseline. In Column (6) we show that the results are robust to adding control variables.

Finally, there is a concern that the 2SLS effects found could be too large, as they more than double those found in the OLS estimate. One plausible explanation is that there is a strong selection of municipalities that were experiencing high mortality rates previous to the implementation of the BJA. For instance, if municipalities that were experiencing a high rate of stillbirths also enrolled their population at a faster rate, then OLS estimates could be interpreted as a lower bound estimate of the effect of the program. Additionally, there could be measurement error in the rate of enrollment due to the projection of eligible population that we use in the denominator. This could attenuate OLS estimates. When we instrument the enrollment rate, both of these issues together could explain why the 2SLS estimates are high relative to those estimated by OLS. Hence, we interpret our findings as a lower and upper bound, rather than as specific point estimates of the program effects on stillbirths.

Robustness checks

We run several robustness checks. Column (1) in Table 6 shows the results of specifying the instrument as the number of payment centers as opposed to the ISH transformation. The results show that the effects are similar but that we lose statistical power. In column (2), we run a similar regression using dummy variables for the number of payment centers. We construct three categories: no payment centers available, one payment center available, and two or more payment centers available. We exclude the first category from the first stage regression. The results are very similar but marginally non-significant. Although these specifications are more flexible, using the number instead of the logarithm or the HIS transformation allows scaling for

the fact that municipalities vary in the number of eligible women, using the logarithm of eligible women as a control.

In columns (3) to (5) we assess whether our results are sensitive to measurement error in the data. Column (3) shows that the 2SLS results in Table 5 are robust to dropping observations that show zero stillbirths, suggesting that results are not driven by municipalities reporting zero stillbirths. In columns (4) and (5), we drop the years 2005 and 2008 respectively. We drop year 2005 because it shows the highest stillbirth rate and, as the first year, might also suffer from lower quality registration of stillbirths. The reason for dropping year 2008 is the spike shown in Figure 3 that may be due to reporting error by municipalities.[46] 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.

Next, while the national government planned the expansion of payment centers centrally, we might be concerned that planning decisions responded to local governments' demand for more payment centers. If in fact payment centers increase in a municipality because of a higher demand for the program, our IV strategy may be subject to additional endogeneity problems. As an alternative, we use the (exogenous) initial number of payment centers available in year 2009 and interact it with the national expansion of payment centers in subsequent years. For each municipality we use the expansion of all other municipalities in the country. The results in column (6) and column (7) show that the 2SLS estimates are slightly higher in absolute terms but the magnitudes are similar to those in column (4) of Table 5.

Finally, our identification strategy would be threatened if the changes in the number of available payment centers reflected local changes in economic growth, which in turn could affect

[46] We further comment on the problem of misreporting mortality in Appendix A

mortality at birth or later in life. This would invalidate the exclusion restriction of our instrument. To address this issue, we construct municipality x year per capita levels of night light data as a proxy variable for economic growth over time.[47] We regress night light data per capita on the instrumental variable including municipality fixed effects and time fixed effects. The results in column (8) show a small and non-significant relation between the expansion of payment centers and a local measure of economic growth.

B. The BJA and Survival of Age-Specific Cohorts

Given the well-known problems in reporting episodes of mortality at any stage of child development (e.g., at birth, neonatal, first year), we show the results of the effects of the BJA on the survival rate of cohorts that were exposed to the program. This serves both as a separate outcome analysis and also as a consistency check for the results on stillbirths. In the presence of negative effects on stillbirths, one should also expect a positive effect of the program on the size (survival) of birth cohorts over time. Column (2) in Table 7 shows the results of estimating equation (7) by OLS. The estimated coefficient shows that a 1-percentage point (ppt) increase in the average enrollment in the BJA is associated with an increase of 0.001% in the size of cohorts exposed to the BJA program. A municipality with average enrollment rates experienced an increase of 0.28 x 12.5 = 3.5% in the size of the cohorts exposed to the BJA during pregnancy.

As with stillbirths, we use payment centers to instrument the enrollment rate variable in (7). Column (3) shows the reduced form estimates substituting the average enrollment rate with the inverse hyperbolic sine transformation of payment centers and controls for the logarithm of

[47] For a discussion on the use of night light data to proxy economic growth see Mellander et al. (2015). Some examples of papers that have used these type of data for a similar purpose are Bleakley and Lin (2012), Henderson, Storeygard, and Weil (2012), and Michalopoulos and Papaioannou (2013), to name a few.

pregnancies. The results show that a 1% increase in the number of payment centers increases the size of the treated cohorts by 3.4%, on average. Column (4) shows the same First Stage of the 2SLS estimation as in Table 5. Column (5) in Table 7 shows that a 1-percentage point (ppt) increase in average enrollment in the BJA is associated with a 0.006% increase in the size of the cohorts exposed to the BJA program. To approximate this effect in an average municipality under typical enrollment rates, we convert the results for the average enrollment rate of the sample. A municipality with average enrollment rates experienced an increase of 0.28 x 0.569=16.1% in the size of the cohorts exposed to the BJA program during the pregnancy stage. Column (6) controls for additional variables and shows a similar effect on child survival.

We run robustness checks akin to those estimated for the results of stillbirths. Table 8 shows that the effects of the program on child survival are robust to flexible specifications of the instrument: number of payment centers as opposed to the ISH transformation, using dummy variables for the number of payment centers, and using national growth rates to predict local expansion in payment centers. The differences between the 2SLS effects found are large in this case when compared to those estimated by OLS. The same explanation given in the previous section applies in this case. We interpret our findings as a lower and upper bound for the effects of the program rather than specific point estimates of the effects of the program on child survival.

C. The Effect of the BJA on Antenatal Care Utilization and Birth Weight

In this section, we identify the effects of the BJA on prenatal care and birth outcomes, the primary mechanisms that might explain the effects on stillbirths and survival.[48] Estimates of regression (8) are presented in Table 11. The first row in column (1) shows that women enrolled

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[48] The definition of each outcome for prenatal care analysis is shown in Table 9.

in the BJA had their first prenatal check-up 2.6 weeks earlier than women who did not enroll in the program, a 17% improvement. The effect holds in column (2) when controlling for a quadratic specification of age at birth, order of pregnancy and gender of the child, covariates that proxy pregnancy risks and gains in parenting skills. To the extent that these variables are potential sources of bias, these results suggest that mother fixed effects account for most of the unobserved differences. Furthermore, our findings on prenatal care and birth outcomes are based on comparisons of pregnancies within an average period of fewer than 3 years for the same mother, limiting variation in short-lived shocks, while also controlling for unobserved differences between mothers that could bias estimates (e.g., see Aizer and Doyle Jr 2014 and Aizer, Stroud, and Buka 2016).

The next columns show the effect of the BJA on women who live in urban areas and rural areas. Comparing columns (4) to (6), 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 antenatal care for the first time before the 20th week of pregnancy. Panel B in Table 11 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 compared to women not enrolled in the BJA, which corresponds to an increase in the probability of early initiation of prenatal care of 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(see Panel B column 4 and 6).

The next Panel in Table 11 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. Women enrolled in the BJA are 10.3 percentage points more likely to complete at least

four prenatal check-ups during their pregnancy, 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 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 by 5.0 percentage points, which corresponds to a 14% increase relative to the control group average.

Table 12 shows the effects of the BJA on birth weight and the probability of low birth weight. Birth weight information is available for approximately 75% of the sample. In columns (3), (6) and (9) we adjust for the probability of having birth weight information by Inverse Probability Weighting (IPW).[49] The results show that the BJA had no significant effect on average birth weight. However, Panel B shows that the program reduced the rate of low birth weight 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 in urban areas. We interpret the results on birth weight with caution since the effects found on stillbirths suggest that the BJA had important compositional effects on surviving children. To the extent that reduced mortality due to BJA is concentrated amongst lower birth weight babies, the aggregate effect of BJA on birth weight is ambiguous.

VI. Cost effectiveness

This section presents a Cost Effectiveness Analysis (CEA) to quantify the impacts of the BJA in terms of monetary costs per disability adjusted life year (DALY) averted. We compute

[49] 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.

both fixed costs of managing the program, the costs generated by the program's impacts on medical visits, 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 and the rate of stillbirths. Each of these has an associated measure of DALYs averted, published by the World Health Organization.[50]

Appendix B presents the detailed parameters and components of the costs and benefits associated with the BJA used for the computation of our measure of total cost per DALY averted. The fixed costs of the program come from official BJA administrative records and amount to a total of $20,284,455.59 Bs (approximately $2,935,521.79 USD) for year 2013.[51] These correspond to costs of the program's operation, personnel, and other management costs. Variable costs are not as straightforward to compute since they are affected by program impacts on the number of health visits, 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 the value of travel time to each health visit and each payment center to receive the transfer. The BJA generated an impact on prenatal visits of 39,375 additional prenatal care visits.[52] Valuing each medical hour at 37.73 Bs and assuming that each visit takes on average 0.5

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[50] The data on DALYs averted for Bolivia for each cause were obtained from http://www.who.int/healthinfo/global_burden_disease/estimates/en/index2.html. For stillbirths we considered the DALYs associated to neo natal births as a proxy measure.

[51] To compute the costs that are associated to the prenatal care component we adjust the fixed costs per enrollee. Since 39% of enrollees are pregnant women we adjust total fixed cost by 0.39, and take this to be the component of the fixed costs associated to pregnancies. Otherwise we would be overestimating costs significantly. Fixed costs exclude the cash transfers to mothers (Bs. 15,103,970 in 2013), since these are not incremental costs to society. Even if these costs were included, the intervention continues to be highly cost effective ($311.9 per DALY averted).

[52] This is computed taking the impact on the probability of having four visits (0.117) multiplied by 4, giving 0.47 additional prenatal care visits on average per women enrolled, 84,134 women per year.

hours, the costs associated to these additional visits amount to 742,811 Bs. The same rationale is followed for the other components of the cost, travel to health facilities and travel to payment centers. As such, the total cost of the BJA for its prenatal component is estimated to be $3,168,208.1 per year.

To compute the effects of the BJA we focus on the DALYs averted associated to the impacts on low birth weight and the rate of stillbirths - the two primary health outcomes with statistically significant program effects. We explain the computation of effects for low birth weight, although the same rationale applies to stillbirths. The number of children that are no longer born with low birth weight as a consequence of the BJA is 4,391 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 number of births in urban areas of 54,889.5 per year, yielding an effect of 14,897 DALYs averted using WHO DALYs for low birth weight. A similar analysis is performed for the rate of stillbirths but using the OLS estimates that represent the lower bound of the effect. These two components sum to a total of 17,167.90 DALYs averted thanks to the BJA.

The WHO provides cost-effectiveness guidelines for different thresholds of cost per DALY averted. As a rule of thumb, health interventions are considered cost-effective if costs per DALY averted are less than three times the country per capita GDP (see Marseille et al. 2015 for a discussion). Our estimates show that the prenatal component of the BJA is highly cost effective, at $184.54 USD per DALY averted, or just 7.4% of the country’s $2,480 GDP per capita in 2012.

VII. Discussion

Many of the leading causes of under-five mortality in developing countries such as pneumonia, diarrhea, preterm birth and intrapartum-related complications are avoidable through preventive medical care and timely treatment. Yet despite the expansion of free or subsidized maternal and child healthcare services in many countries, the demand for preventive services often remains below recommended levels, particularly for poor, rural and indigenous populations (Mills 2014).[53] One potential explanation for the sub-optimal utilization of potentially life-saving care 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).

CCTs have been shown to be an effective policy instrument to stimulate the demand for health services. Yet these programs vary widely in the transfer amounts and the conditionality structure. Many CCT programs have a short-term poverty-alleviation objective, paying up to 30% of per-capita consumption in regular (bi)monthly installments over many years (Fiszbein, Schady, and Ferreira 2009; Stampini and Tornarolli 2012). The BJA program that we study differs from existing CCT programs in at least two salient dimensions. First, the amount of the transfers is small. Our estimates show that, on average, participant women are eligible to receive the equivalent of 1% of total household consumption, or 4.8% of per capita consumption, if they comply with all requirements in the prenatal period. Thus, the transfer amounts of the BJA are the smallest of any national CCT program implemented in the LAC region (see Table 1). Second, the program pays transfers individually for each eligible health visit completed by the

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

mother with a specific amount related to the type of visit that is due (e.g., prenatal check-ups or skilled birth attendance and postpartum visit), and as such is time-finite.

We analyze multiple independent data sources and estimate consistent effects of the BJA on stillbirths and child survival. We argue that BJA's effect on incentivizing health care utilization is likely to dominate other potential explanations for the identified reductions in early-life mortality, including an income effect. Increases in health care utilization at the critical periods of pre-natal care and birth, such as those promoted by BJA, are well established in the medical literature as life-saving practices. And given the modest size of the transfers it is unlikely that results could be driven by an income effect that caused increased consumption.

Apart from the BJA program in Bolivia, CCT models with checkup-specific demand side payments to mothers have not been applied or studied in the LAC region, despite their potential cost-effectiveness in improving health outcomes. Our estimates of costs and effects generated by the program in its prenatal care component show that the BJA implies a cost of $184.54 USD per DALY averted, equivalent to 7.4% of the country's per capita GDP, making the intervention highly cost-effective according to standard thresholds provided by the WHO.[54] Evidence of the potential cost-effectiveness of small monetary incentives is not only policy relevant but also theoretically appealing if "nudges" help to overcome fixed costs related to health seeking behavior.[55] These results suggest that small monetary incentives paid for critical preventive maternal health services are an appealing policy alternative for reducing the cultural, economic and behavioral demand side barriers for health services that persist in some population groups around the world, and can have important effects on child mortality and survival.

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

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

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Tables

TABLE 1: TRANSFERS RELATIVE TO HOUSEHOLD CONSUMPTION FOR DIFFERENT CCT PROGRAMS IN LATIN AMERICA

Country	Program	Ratio of transfer to per-capita consumption
Bolivia	Bono Juana Azurduy	4.8
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/Progresa	19 – 20
Nicaragua	Atención a Crisis	29 - 31

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

TABLE 2: MATERNAL AND CHILD HEALTH AND HEALTH SERVICE INDICATORS BEFORE THE BJA

Indicator	Bolivia (Urban/Rural) 2008* (%)	Average LAC** (%) (2011)
Current use of birth control methods	61 (66/53)	74
Percentage of women with at least 4 antenatal care visits for last live birth	72 (81/60)	86
Percentage of women with first antenatal care visit in first 3 months of pregnancy	62 (68/53)	
Births delivered by skilled health personnel	71 (88/51)	94
Percentage of women with last live birth protected against tetanus	61 (67/51)	
Immunization rate for BCG Vaccine (18-29 months)	98 (99/98)	--
Immunization rate for Pentavalent (DTPw-HepB-Hib) vaccine (18-29 months)	86 (85/87)	92
Percentage of live births below 2.5 kg.	5.3 (6.1/3.9)	
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 total births) (%) 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 3: CO-RESPONSIBILITIES AND AMOUNTS IN THE BJA

Co-responsibility	Number	Amount ($Bs/$USD)	Maximum ($Bs/$USD)
Women during pregnancy:			
Antenatal care visit	4	50 / 7	200 / 28
Delivery assisted by skilled health personnel	1	120 / 17	120 / 17
Total benefits from pregnancy			320 / 45
Children under 2 years old:			
Preventive health care visit	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 4: EVOLUTION OF PAYMENT CENTERS OVER TIME IN SAMPLE MUNICIPALITIES

Year	Municipalities with at least one payment center	Payment centers per 1,000 enrolled women		
	Percentage	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: Sources: 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 5: EFFECT OF BJA INTENSITY ON THE RATE OF STILLBIRTHS AT THE MUNICIPALITY LEVEL

	(1)	(2)	(3)	(4)	(5)	(6)
	Pre-trends	OLS	Reduced Form	1st Stage	2SLS	2SLS + Controls
Avg. Enrollment t, t-1		-7.824**			-18.425*	-21.814*
		(3.777)			(10.992)	(12.799)
IV: Payment Centers $(\text{P.C.})^{\zeta}$			-1.080*	0.059***		
			(0.621)	(0.009)		
Δ(P.C.) in 2009 x Y2006	0.807					
	(0.815)					
Δ(P.C.) in 2009 x Y2007	0.946					
	(1.415)					
Δ(P.C.) in 2009 x Y2008	-0.534					
	(1.148)					
Observations	1,260	2,520	2,520	2,520	2,520	2,512
R^2	0.004	0.005	0.002	0.683	0.008	0.008
Municipality Fixed Effects	Y	Y	Y	Y	Y	Y
Controls	N	N	N	N	N	Y
Joint F-test γs	0.108					
Joint p-value γs	0.955					
1st Stage F-test				40.773		
1st Stage p-value				0.000		
Mean enrollment rate		0.28				
Baseline mean of stillbirths		22.8				

Notes: In this table we present different regressions for the outcome of rate of stillbirths. ζ: Inverse hyperbolic sine of the number of payment centers. Column (1) shows the results of the pre-trends analysis presented in equation (2) of the text where we regress stillbirths on the change in the number of payment centers using only years before the intervention started. Each coefficient is interpreted as the correlation between the change in stillbirths from year X with respect to year 2005 and the change in the expansion in the number of payment centers in year 2009. The results are robust to any combination of base year or expansion years that we use. Column (2) shows the results of estimating equation (1) through OLS including time and municipality fixed effects. Column (3) shows the results of regressing the rate of stillbirths on the number of payment centers, transformed using the inverse hyperbolic function (see Burbidge, Magee, and Robb 1988). Column (4) shows the results of the first stage regression where we run a regression of enrollment rates on the number of payment centers. Column (5) shows the Two Stage Least Squares regression of instrumenting enrollment rate in equation (1) using the number of payment centers, transformed using the inverse hyperbolic function. Column (6) runs the same regression as in Column (5) but adding number of health facilities, night-light per capita, and interactions between these terms as controls. In all regressions, each observation is weighted by the number of eligible pregnancies in year 2008. The sample excludes municipalities with more than 250,000 habitants in year 2012 and municipalities that have more than 10 payment centers. 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

Dependent Variable:	Rate of stillbirths							Night Light per capita
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	2SLS IV: Number	2SLS IV: Non-Linear	2SLS Drop zeros	2SLS Drop Y2005	2SLS Drop Y2008	2SLS Alternative IV	2SLS Alternative IV: Number	OLS
Avg. Enrollment t, t-1	-23.440	-20.112	-26.679*	-20.622*	-26.590*	-26.675*	-23.967	
	(14.615)	(13.958)	(13.698)	(12.210)	(13.850)	(15.473)	(15.474)	
IV: Payment Centers $(\text{P.C.})^{\zeta}$								0.003
								(0.002)
Observations	2,512	2,512	1,633	2,198	2,198	2,512	2,512	2,512
R^2	0.007	0.009	0.013	0.005	0.013	0.004	0.006	0.049
Municipality Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y

Notes: In this table we present different robustness checks to the Two Stage Least Squares estimation. Column (1) shows the results of using the number of payment centers instead of the hyperbolic transformation. Column (2) uses a non-linear version of the instrument where we construct three categories, no payment centers available, one payment center available, and two or more payment centers available. Column (3) drops zeros in the rate of stillbirths. Column (4) drops observations from year 2005 and Column (5) from year 2008. Column (6) uses an alternative version of the hyperbolic transformation instrument where we predict the number of payment centers in each municipality using national expansion rates. Column (7) uses this alternative instrument in numbers. Column (8) regresses night-light data per capita on the number of payment centers available. In all regressions, each observation is weighted by the number of eligible pregnancies in year 2008. The sample excludes municipalities with more than 250,000 habitants in year 2012 and municipalities that have more than 10 payment centers. Standard errors in parenthesis and are clustered at the municipality level. *p < 0.10, **p < 0.05, ***p < 0.01.

TABLE 7: EFFECT OF BJA INTENSITY ON THE SIZE OF AGE-COHORTS (LOGARITHM) AT THE MUNICIPALITY LEVEL

	(1) Pre-trends	(2) OLS	(3) Reduced Form	(4) 1st Stage	(5) 2SLS	(6) 2SLS + Controls
Avg. Enrollment t, t-1		0.125***			0.576***	0.607***
		(0.031)			(0.124)	(0.148)
IV: Payment Centers $(\text{P.C.})^{\zeta}$			0.034***	0.059***		
			(0.006)	(0.009)		
Δ(P.C.) in 2009 x Y2006	0.011					
	(0.010)					
Δ(P.C.) in 2009 x Y2007	0.017*					
	(0.010)					
Δ(P.C.) in 2009 x Y2008	0.016					
	(0.010)					
Observations	1,260	2,520	2,520	2,520	2,520	2,512
R^2	0.014	0.063	0.027	0.68	0.29	0.29
Municipality Fixed Effects	Y	Y	Y	Y	Y	Y
Controls	N	N	N	N	N	Y
Joint F-test γs	1.629					
Joint p-value γs	0.197					
1st Stage F-test				40.773		
1st Stage p-value				0.000		
Mean enrollment rate		0.28				

Notes: In this table we present different regressions for the outcome of the logarithm of the size of age cohorts. ζ: Inverse hyperbolic sine of the number of payment centers. Column (1) shows the results of the pre-trends analysis presented in equation (2) of the text where we regress the log of the size of age cohorts on the change in the number of payment centers using only years before the intervention started. Each coefficient is interpreted as the correlation between the change in the size of age cohorts from year X with respect to year 2005 and the change in the expansion in the number of payment centers in year 2009. The results are robust to any combination of base year or expansion years that we use. Column (2) shows the results of estimating equation (1) through OLS including time and municipality fixed effects. Column (3) shows the results of regressing the log of age cohorts on the number of payment centers, transformed using the inverse hyperbolic function (see Burbidge, Magee, and Robb 1988). Column (4) shows the results of the first stage regression where we run a regression of enrollment rates on the number of payment centers. Column (5) shows the Two Stage Least Squares regression of instrumenting enrollment rate in equation (1) using the number of payment centers, transformed using the inverse hyperbolic function. Column (6) runs the same regression as in column (5) but adding number of health facilities, night-light per capita, and interactions between these terms as controls. In all regressions, each observation is weighted by the number of eligible pregnancies in year 2008. The sample excludes municipalities with more than 250,000 habitants in year 2012 and municipalities that have more than 10 payment centers. Standard errors in parenthesis and are clustered at the municipality level. *p < 0.10, **p < 0.05, ***p < 0.01.

TABLE 8: ROBUSTNESS CHECKS FOR THE EFFECT OF THE BJA ON THE SIZE OF AGE-COHORTS

	(1) IV: Number	(2) IV: Non-Linear	(3) 2SLS Alternative IV	(4) 2SLS Alternative IV: Number
Avg. Enrollment t, t-1	0.722***	0.554***	0.776***	0.643***
	(0.220)	(0.143)	(0.290)	(0.228)
Observations	2,512	2,512	2,512	2,512
R^2	0.232	0.312	0.201	0.273
Municipality Fixed Effects	Y	Y	Y	Y
Controls	Y	Y	Y	Y

Notes: In this table we present different robustness checks to the Two Stage Least Squares estimation. Column (1) shows the results of using the number of payment centers instead of the hyperbolic transformation. Column (2) uses a non-linear version of the instrument where we construct three categories, no payment centers available, one payment center available, and two or more payment centers available. Column (3) uses an alternative version of the hyperbolic transformation instrument where we predict the number of payment centers in each municipality using national expansion rates. Column (4) uses this alternative instrument in numbers. The sample excludes municipalities with more than 250,000 habitants in year 2012 and municipalities that have more than 10 payment centers. Standard errors in parenthesis and are clustered at the municipality level. *p < 0.10, **p < 0.05, ***p < 0.01.

TABLE 9: IMPACT EVALUATION INDICATORS USING ESNUT 2012

Indicator	Definition
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

TABLE 10: DIFFERENCES IN MEANS BETWEEN HOUSEHOLDS (HHS) ENROLLED AND NOT ENROLLED IN THE BJA IN THE SUBSAMPLE USED FOR THE FIXED EFFECTS ANALYSIS.

Variables	(1) Mean of HHs in sample with at least one pregnancy enrolled	(2) Mean of HHs in sample with no pregnancy enrolled	(3) P-value for difference (1)-(2)	(4) Mean of HHs in sample	(5) Mean of HHs out of sample	(6) P-value for difference (4) - (5)
Age of mother	27.575	27.524	0.900	27.673	27.554	0.637
Years of schooling of mother	7.667	8.492	0.002	9.327	8.010	0.000
Mother Indigenous=1	0.470	0.526	0.044	0.418	0.493	0.000
Number of children	3.436	3.236	0.087	2.237	3.353	0.000
Labor force participation	0.394	0.401	0.838	0.480	0.397	0.000
Household size	5.647	5.550	0.391	4.901	5.607	0.000
Sex of head of household	0.090	0.082	0.667	0.145	0.087	0.000
Per capita consumption (in $Bs)	579.160	546.138	0.307	708.866	565.417	0.000
Household wealth index	0.447	0.491	0.749	1.215	0.466	0.000
Distance to nearest health facility (in Km.)	3.037	2.769	0.285	2.318	2.925	0.000

Notes: All numbers are computed using the subsample of households with no social security. Means are weighted using household weights provided by ESNUT 2012. Indigenous indicator is equal to 1 if the person reports to belong to any indigenous group in the country.

TABLE 11: EFFECT OF BJA ENROLLMENT ON UTILIZATION OF PRENATAL CARE SERVICES

	All Sample		Urban		Rural	
	(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 R^2	0.029	0.034	0.056	0.064	0.033	0.037
B. Probability that First Visit Occurs in the First Trimester						
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 R^2	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 R^2	0.018	0.021	0.034	0.035	0.022	0.024
D. Probability of Being Attended by Skilled Professional at Birth						
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 R^2	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
Includes 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 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 12: 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 R^2	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 R^2	0.042	0.044	0.040	0.061	0.063	0.066	0.025	0.024	0.024
Observations	3,624	3,624	3,624	958	958	958	2,666	2,666	2,666
Mother fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Includes 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.

Figures

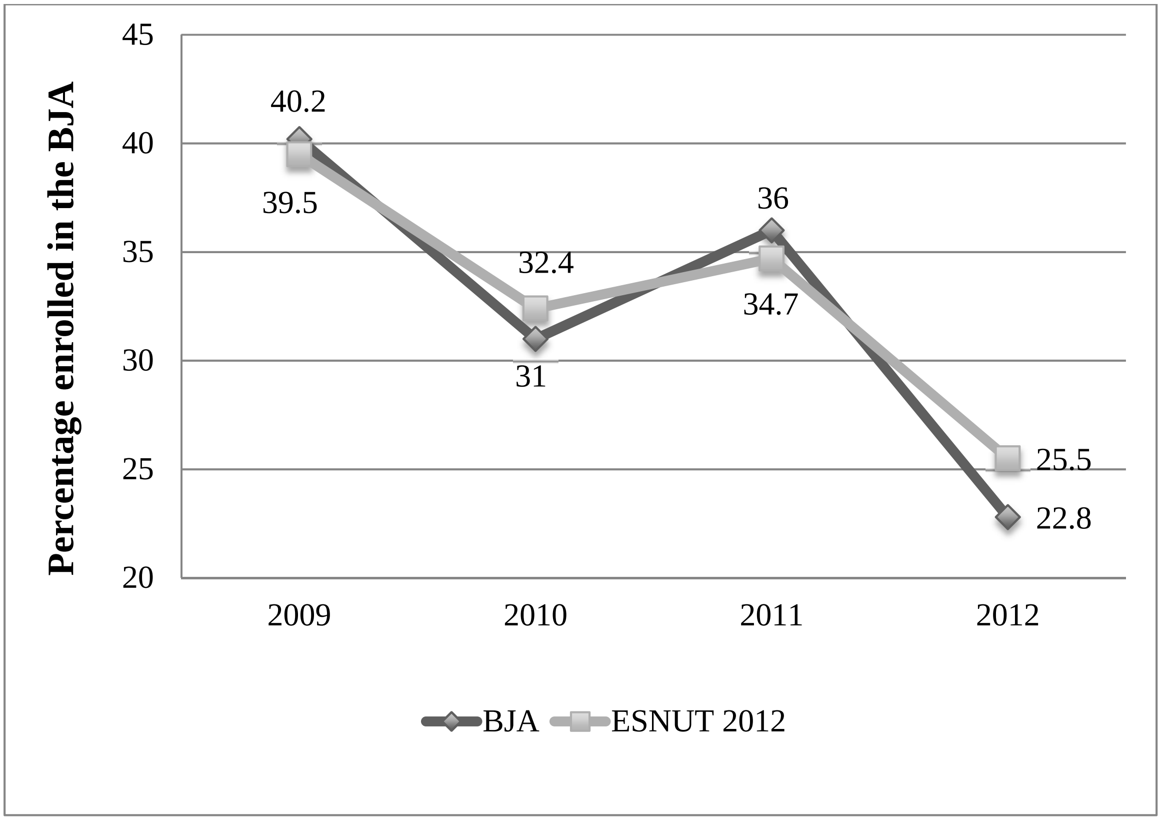

FIGURE 1: ENROLLMENT RATE IN THE BJA OF PREGNANCIES BY YEAR OF PREGNANCY

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.

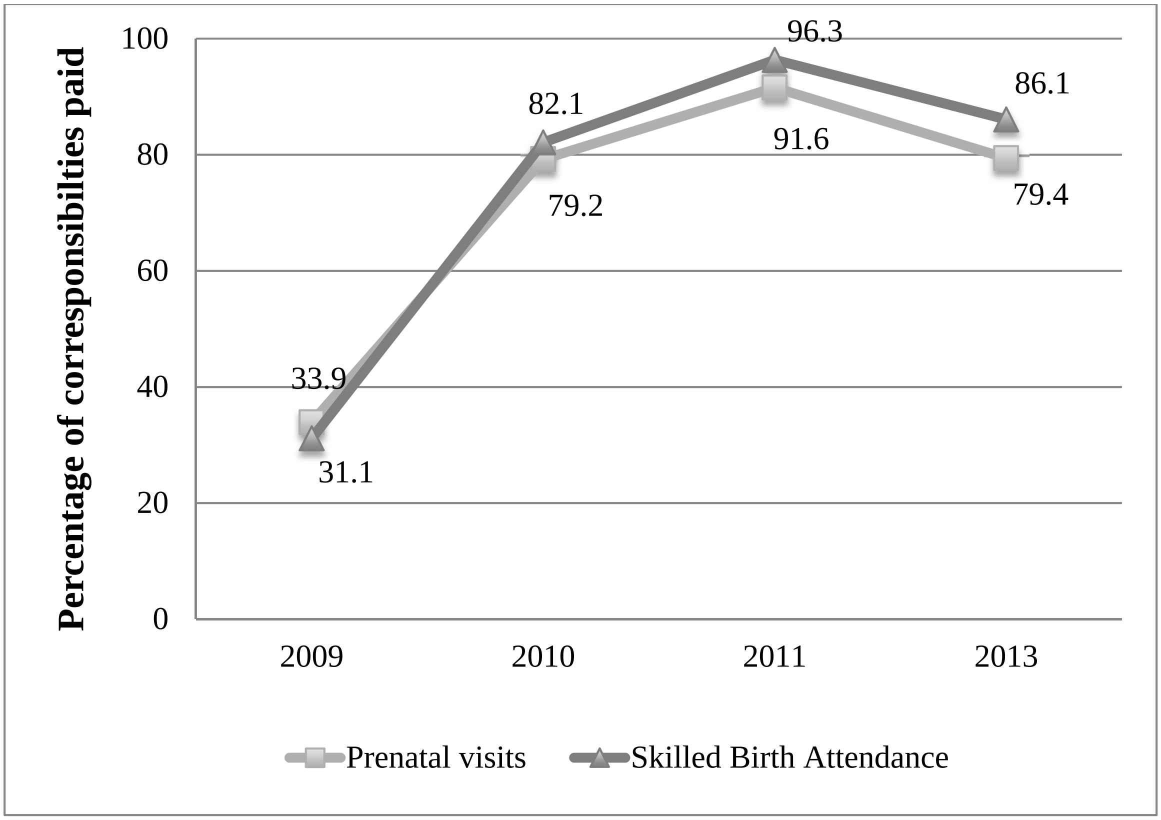

FIGURE 2: PERCENTAGE OF CO-RESPONSIBILITIES PAID BY THE BJA BY YEAR AND TYPE OF SERVICE.

Notes: Own calculations based on BJA enrollment records.

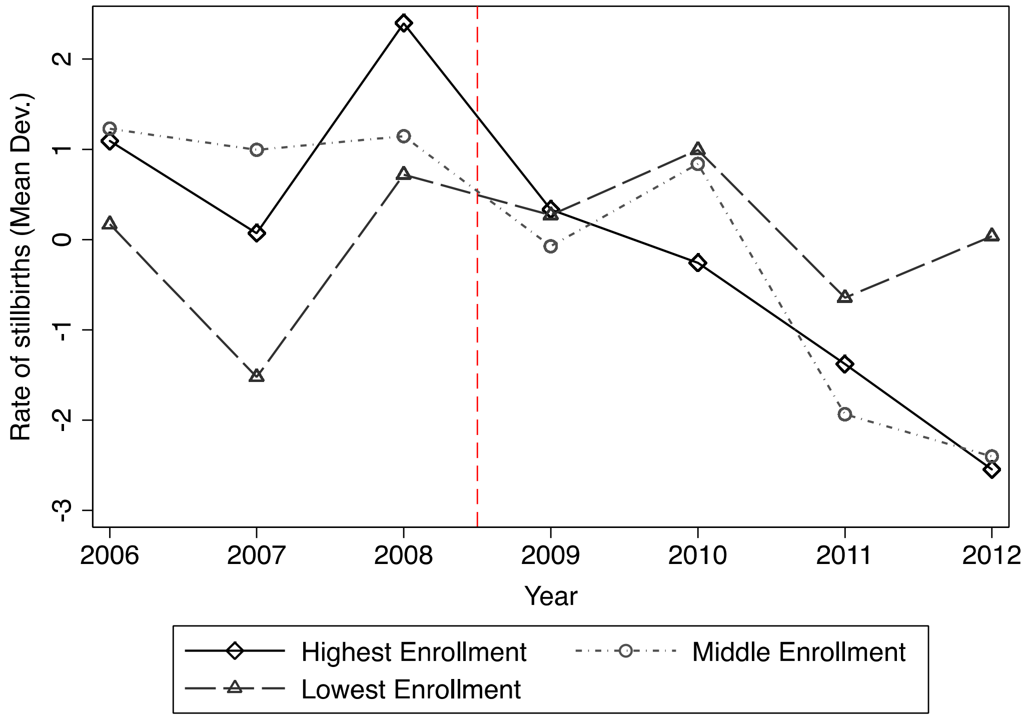

FIGURE 3: TRENDS IN THE RATE OF STILLBIRTHS FOR MUNICIPALITIES WITH ENROLLMENT RATES ABOVE THE MEDIAN (HIGH) AND BELOW THE MEDIAN (LOW) ENROLLMENT RATE IN YEAR 2009

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.

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 1 if a pregnancy ends in a birth and equals 0 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:

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

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

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

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, $P\big(R_{ij} = 1\big)$, is the same for stillbirths and live births. Our variable is constructed as the ratio of the following aggregations at the municipality level:

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

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

$$\frac{\#(still\ births_j | R_j = 1)P\big(R_j = 1\big)}{\#(li□e\ births_j | R_j = 1)P\big(R_j = 1\big)} = \frac{\#(still\ births_j | R_j = 1)}{\#(live\ births_j | R_j = 1)} = Rate_j$$

Hence, the ratio 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.

However, a potential concern is that there actually is differential reporting of stillbirths or live births. To illustrate this consider the following simplification of the outcome we construct:

$$Y^* = \frac{\#(still\ births_j | R_j = 1)}{\#(li□e\ births_j | R_j = 1)P\big(R_j = 1\big)}$$

Suppose that if a pregnancy ends in a complication that subsequently terminates in a stillbirth, the occurrence may have a higher probability of being reported, since the women is more likely to visit a professional doctor because of that complication. Then the probability of reporting a birth, $P(R_i = 1)$, is higher for stillbirths than for live births so that,

$$P(R_i = 1 |\ Stillbirths) > P(R_i = 1 |\ Live\ births)$$

It can be shown that the relation between what we construct and the true rate of stillbirths, Y, is:

$$Y^* = Y \frac{P(R_i = 1 |\ Stillbirths)}{P(R_i = 1 |\ Live\ births)} = Y\alpha.$$

With $\alpha > 1$. This could cause an anti-attenuation bias in the coefficient of an OLS (or 2SLS) regression due to differential reporting of births. Consider the linear regression $y^* = X'B + u$. $\hat{B}_{OLS} = (X'X)^{-1}X'y^* = (X'X)^{-1}X'y\alpha$, with $\alpha > 1$. We would need to adjust $\hat{B}_{OLS}$ by a factor of $1/\alpha$ to get a coefficient free from bias from differential

reporting. If we assume that the probability of reporting a stillbirth is close to one, then from administrative data we could back out the parameter α using only the probability that live births are reported. The upper bound of this estimate is $\alpha \approx 0.69$, according to own estimations based on ESNUT 2012.

Appendix B: Cost Effectiveness Analysis of the BJA

Pre-determined parameters		
Demographics and others	**Source**	**Value**
Eligible Pregnancies	UDAPE	281,272.00
Number of pregnancies enrolled (per year)	UDAPE	84,134.80
Average number of medical visits while pregnancy	ESNUT 2012	3.16
Proportion rural population	ESNUT 2012	0.35
Percentage of medical visits reimbursed	ESNUT 2012	0.60
Total number of births in year 2012	Census 2012	291,158.00
Proportion of eligibility	ESNUT 2012	0.84
Enrollment rate amongst eligible	ESNUT 2012	0.34
Transfers		
Each prenatal care visit ($Bs)	UDAPE	50.00
Institutional Delivery ($Bs)	UDAPE	120.00
Impacts of the Program		
Number of prenatal care visits	Estimated	0.47
Institutional Delivery and Post-Partum visit- Rural	Estimated	0.05
Low birth weight - Urban	Estimated	0.08
Stillbirths - Rural (Lower Bound)	Estimated	0.10
Computing DALYS		
Low Birth Weight Incidence before the BJA	WHO	0.08
DALYS for birth weight per 1,000 births	WHO	81.00
DALYS per birth	Own elaboration	3.39
Stillbirths (per 1,000 births) incidence before the BJA	WHO	17.00
DALYS for perinatal mortality (1,000)	WHO	237.90
DALYS per birth	Own elaboration	48.06

Costs of the BJA		
A. Fixed Costs	**Source**	**Value**
1. Costs of BJA		
Operational Costs in Y2013 ($Bs)	UDAPE	6,091,424.12
Community Doctors in Y2013 ($Bs)	UDAPE	13,436,228.83
2. Management Costs (2013)		
Monetary transfer ($Bs)	UDAPE	520,675.58
National ID ($Bs)	UDAPE	236,127.06
B. Variable Costs		
1. Additional medical hours induced by the BJA:		
Prenatal Care		
Total prenatal care visits induced by the program		39,375.09
Hours per visit		0.50
Value of each medical hour (generalist) ($Bs)	UDAPE	37.73
Cost ($Bs)		742,811.00
Post-partum and institutional delivery		
Institutional deliveries and post-partum visits induced by the program		1,433.02
Hours for post-partum and institutional delivery		12.00
Value of each medical hour (specialist) ($Bs)	UDAPE	43.75
Cost ($Bs)		752,334.22
2. Travel costs to health facilities for households		
Prenatal Care		
Total prenatal care visits induced by the program		39,375.09
Average time from household to health facility (hours)	ESNUT 2012	0.26
Value of time at minimum wage ($Bs)	ESNUT 2012	2.81
Cost ($Bs)		28,746.96
Post-partum and institutional delivery		
Institutional deliveries and post-partum visits induced by the program		1,433.02
Average time from household to health facility (hours)	ESNUT 2012	0.26
Value of time at minimum wage ($Bs)	ESNUT 2012	2.81
Cost ($Bs)		1,046.22

3. Travel costs to payment centers for households		
Number of average travel time for each medical visit		23,625.05
Average time from household to payment center (hours)	ESNUT 2012	1.25
Value of time at minimum wage ($Bs)	ESNUT 2012	2.81
Cost ($Bs)		82,923.93
Total Fixed Costs ($Bs)		20,284,455.59
Total Variable Costs ($Bs)		1,607,862.34
Total Costs ($Bs)		**21,892,317.93**
Total Costs ($USD)		**3,168,208.09**

EFFECTS OF THE BJA		
A. DALYS induced by the BJA		
1. Low Birth weight		
Total births in urban areas		54,889.54
New births above low birth weight threshold induced by the BJA		4,391.16
DALYS for low birth weight		14,897.80
2. Stillbirths		
Total births enrolled in the BJA in rural areas		29,245.26
Total Stillbirths in Rural areas before de BJA		497.17
Stillbirths avoided in Rural areas by the BJA		47.23
DALYS for stillbirths based on perinatal mortality		2,270.10
Total Effects in DALYs		**17,167.90**

Costs per DALY averted ($USD)	**184.54**