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ABSTRACT

Climate Adaptation Policies and Infant Health: Evidence from a Water Policy in Brazil*

This paper studies how in utero exposure to a large-scale climate adaptation program affects birth outcomes. The program built around one million cisterns in Brazil's poorest and driest region to promote small-scale decentralized rainfall harvesting. Access to cisterns during early pregnancy increased birth weight, particularly for more educated women. Data suggest that more educated women complied more with the program's water disinfection training, highlighting that even simple, low-cost technologies require final users' compliance ("the last mile") to be effective. In the context of growing water scarcity, adaptation policies can foster neonatal health and thus have positive long-run implications.

JEL Classification: Q54, Q58, Q25, I15

Keywords: climate, adaptation, birth outcomes, cisterns, water

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1 Introduction

Billions of people worldwide experience water stress. Changing climatic conditions are likely to aggravate this situation by generating less predictable water availability and increasing extreme water stress episodes (UN-Water, 2019). In response, several climate adaptation policies are being implemented to provide alternative water sources to the most vulnerable and minimize the negative impacts of water shortages. The literature has extensively studied the behavioral responses and socioeconomic effects associated with fluctuations in climate variables (Dell et al., 2014). However, evidence on the extent to which adaptation policies can successfully improve socioeconomic outcomes is rather scarce. Assessing the effects of these policies is particularly relevant in low-income and low-skill settings, where even policies with simple designs may fail due to inadequate compliance.

In this paper, we study how in utero exposure to a large climate adaptation program affects birth outcomes. We assess the effects of Brazil's First Water Cisterns (hereafter FWC) program. This program constructed around one million cisterns in the Brazilian semiarid region, the country's poorest and driest area, whose episodes of reoccurring severe droughts have increased over the past 100 years (Lima and Magalhaes, 2018).[1] The program builds rainwater tanks next to houses to promote small-scale decentralized rainfall harvesting.[2] Using a simple low-cost technology, these tanks collect water during the rainy season and have a standard storage capacity sufficient for domestic use (drinking and cooking) during the dry season. To ensure tank water quality, families receive training on point-of-use disinfection and necessary maintenance steps. The program has inspired other similar programs around the world and has been recognized as an important initiative in the fight against drought and aridity.[3]

We use detailed registry data linking birth outcomes and microdata on the timing of the construction of cisterns to assess the extent to which the program improved neonatal outcomes. The richness of our dataset allows us to gain insights into *when*

[1] Climate projections suggest longer periods of dry spells in the region (Marengo et al., 2017; Hoegh-Guldberg et al., 2018).

[2] Rainwater harvesting policies have been implemented in both developed (e.g., Australia and the United States) and developing countries (e.g., China, Kenya, and India).

[3] The program, also known as One Million Cisterns Program, was awarded in 2017 by the World Future Council and the United Nations Convention to Combat Desertification for promoting water access for the poorest.

and *how* adaptation programs work. Our main empirical specification compares the outcomes of women located in the same municipality who conceived in the same calendar month-year but were exposed to cisterns in different stages of their pregnancy.

Our results show significant benefits of in utero exposure to cisterns. According to our baseline estimates, each additional week of exposure to cisterns is associated with a positive effect on birth weight of 1.4–1.5 grams. This increase is not mirrored by increases in gestation length but by increases in fetal growth rate (defined as birth weight divided by weeks gestation). We find small impacts on reducing the incidence of low birth weight. The results on birth weight are robust to a number of robustness checks and placebo exercises.

Heterogeneity analyses point out that the positive effects of intrauterine exposure to cisterns are stronger for more educated women. We hypothesize that more educated women are more likely to comply with the program's training. To investigate this hypothesis, we gather additional data on tanks' maintenance. Using self-reported and laboratory measures of water quality, we document a positive relationship between years of schooling and the tanks' water quality.[4] This result is consistent with the specialized literature that shows that water quality is an important mechanism for neonatal health (Bove et al., 2002) and with studies indicating that low literacy is a barrier limiting effective adaptation to minimize the negative impacts of climate (Fankhauser, 2017). We show that a simple, scalable technology is only effective when the final user's education is higher – i.e., when it travels the "last mile." Since those living in the semiarid have very low levels of education, including many without any formal instruction, our findings suggest that minor changes in education can have important consequences.

The validity of our analysis relies on the identifying assumption that, conditional on selecting a group of people in a given locality, the distribution of the cisterns to pregnant women is unrelated to the exact timing of their pregnancy. We present several exercises to support the plausibility of this assumption. First, we show that the exact timing of cisterns' construction is not correlated with a number of observable characteristics of pregnant women. Second, data on the distribution of cisterns by week of gestation suggest that the program has not targeted women of specific gestational ages. Last and crucially, we follow a large literature that uses siblings to

[4]These data are not available for our sample of pregnant women, but for another sample of FWC beneficiaries. See Section 6.

account for mothers' time-invariant characteristics affecting birth outcomes (Camacho, 2008; Currie and Rossin-Slater, 2013; Currie et al., 2020) and estimate the effect of exposure to cisterns on the birth weight of older siblings born before cisterns were installed. We find coefficient estimates to be small in magnitude and insignificant, suggesting that the estimated effects are likely not driven by unobserved heterogeneity across mothers. Collectively, these findings support the validity of our empirical strategy and reinforce our interpretation that earlier exposure to cisterns is associated with a positive effect on neonatal health.

Our paper relates to several strands of the literature. First, it adds to the literature on climate adaptation. Several adaptation behaviors in response to fluctuation in climate variables have been documented in agricultural production, energy consumption, and locational decisions (see Kahn, 2016 and Fankhauser, 2017, for reviews). However, the impacts of public policies have been largely overlooked and are concentrated on the provision of selected public goods, such as climate information services and disaster defenses (Fankhauser, 2017). We contribute by assessing the effects of a decentralized adaptation policy on neonatal health, an outcome associated with long-lasting impacts on individuals (Currie and Vogl, 2013, Aizer and Currie, 2014, Shah and Steinberg, 2017). We also add to the branch on the adaptation to water availability – which has focused more on water availability for production (e.g., Hornbeck and Keskin, 2014) – by studying the effects of a water policy for consumption.

Second, we contribute to the literature linking weather and climate shocks to birth outcomes (e.g., Deschenes et al., 2009, Maccini and Yang, 2009, Rocha and Soares, 2015, and Lin et al., 2021). Previous studies have mostly relied on aggregate shocks across geographical areas, such as rainfall fluctuations, to identify the effects of water scarcity on health outcomes. Our contribution is to show the effects of a decentralized policy that increases water availability in the household, net of any influence of sanitary conditions or policy responses to aggregate shocks.[5] Moreover, we present suggestive evidence that water quality seems an important mechanism underlying the main results.[6]

Third, our work connects to the literature analyzing the impacts of welfare pro-

[5]Rainfall fluctuations could lead to the proliferation of vectors of diseases or disrupt agricultural production, leading to shortage of food. Besides, negative shocks could induce policy responses to the areas most affected by the shock.

[6]Thus, we also relate to the literature studying how contaminated water negatively affects infants' and children's health outcomes (Currie et al., 2013, Bhalotra et al., 2021, Marcus, 2021).

grams. Using detailed microdata, this paper sheds new light on how public policies affect neonatal outcomes. We find comparable or stronger effects on birth weight than papers studying other welfare programs (e.g., Almond et al., 2011, Hoynes et al., 2011). Importantly, we study how a scalable infrastructure aiming at providing safer access to a scarce factor (water in an arid region) can be ineffective when it does not travel the last mile. Studies show that improving the last-mile is also relevant in other settings, including public services (Muralidharan et al., 2021) and costly infrastructure, such as sewer systems (Ashraf et al., 2016). We show that even a simple, inexpensive infrastructure needs the last mile.[7]

2 Background

2.1 Semiarid region

The Brazilian semiarid is the driest region in the country. It is the most populous dry area in a tropical zone in the world with about 22 million inhabitants (circa 12% of the national population) and is prone to irregular rainfall, low water retention by the soil, and severe droughts. The region has 1,262 municipalities, which are mostly small-sized agricultural-oriented jurisdictions (Da Mata and Resende, 2020). The semiarid presents worse social indicators – such as poorer health and education outcomes – vis-à-vis other regions in Brazil and has the largest concentration of rural poverty in Latin America.

Water shortage has been identified as the main source of vulnerability for rural families living in Brazil's semiarid (Bobonis et al., 2017). In the period 2011–2017, the yearly average rainfall in the semiarid (700 millimeters) was approximately half of the mean precipitation in the rest of the country (about 1,550 millimeters) and highly irregular. Episodes of droughts have plagued the region since the sixteenth century, with devastating consequences, such as massive migrations to other regions of the country, hunger, and deaths. It is estimated that more than 3 million people have died as a direct result of the droughts between 1825 and 1983 (Villa, 2000).

In addition to the unreliable precipitation, high evapotranspiration rates and the

[7]We also connect to the strands on how cash-transfer programs affect neonatal health (e.g., Barber and Gertler, 2008; Amarante et al., 2016) and how birth outcomes can be affected by shocks of varying intensity, ranging from mild exposures to aggregate macroeconomic fluctuations (Bozzoli and Quintana-Domeque, 2014), and disasters (e.g., Almond, 2006; Black et al., 2016; Persson and Rossin-Slater, 2018).

area's geology make it harder to retain water (e.g., the rocky, shallow soil of the semiarid has low water retention capacity). Groundwater wells are inadequate to meet the region's needs, as they provide water of high salinity (Cirilo, 2008). Approximately 67% of rural households do not have access to the general water supply network (Asa Brasil, 2017).

2.2 First water cisterns program

The FWC program aims to provide access to reliable, clean water for households in rural areas in Brazil.[8] The program builds tanks next to houses to store rainwater harvested from roof catchments (gutters installed on roofs).[9] Each tank has a standard storage capacity of 16,000 liters, which is sufficient for domestic use (drinking and cooking) for a household of up to five members during an eight-month dry season. Tanks are built with precast concrete plates – a simple, low-cost technology that is suitable for dry conditions and is easily scalable.

Almost one million cisterns were built in the Brazilian semiarid. The region has experienced a rapid expansion of the program's roll-out: the number of tanks more than doubled between 2010 (362,475) and 2016 (864,278). Before the construction of cisterns, households regularly relied on alternative sources for obtaining water, such as small ponds and reservoirs (which are often vulnerable to pathogen contamination) and water provided by (government-sponsored) water tankers during prolonged dry periods. With the cisterns, households can harvest rainwater or store larger amounts of water from water tankers.

To ensure cisterns' water quality, households receive training on point-of-use disinfection (with sodium hypochlorite). In addition, households are instructed to remove the gutters during droughts, use a water bucket to handle the tank's water exclusively, cover the tank's outer walls with lime, and clean the tank yearly using the season's first rain with added bleach (Palmeira, 2006). The program does not promote any refresher training on point-of-use disinfection, nor promote any special training for pregnant women.

Brazil's Ministry of Social Development (MDS) partners with subnational gov-

[8] There are two other related programs: cisterns for rainfed agriculture (called "Second Water Cisterns Program"), which aims at enhancing food security, and cisterns for schools ("Cisterns in the Schools"). In this paper, we focus only on the cisterns for domestic use (the FWC program).

[9] Appendix Figure A.1 shows a typical cistern constructed by the program.

ernments and private nonprofit entities – mainly the NGO *Articulação no Semiárido Brasileiro* (ASA) – to execute the program. These partners are responsible for selecting households based on criteria set by the federal government. To be eligible to participate, households living in rural areas with no regular access to water must be registered in the national registry of social programs (Unified Household Registry – "CadÚnico"). The selection process gives priority to households with the following characteristics: (1) low income, (2) women headed, (3) large number of children up to six years of age or school-age children, (4) households with people with disabilities, and (5) households with elderly people (Brasil, 2018).

3 Data

We merge three administrative registries of the Brazilian government to perform our analysis.[10] First, we use the administrative registry of the FWC program, which contains the complete record of the program's implementation. The data identify each beneficiary (household head) by name, date of birth, municipality of residence, and ID number. The registry further includes the exact date of the tank construction, which typically lasts two or three days.

Second, we use the CadÚnico to gather socioeconomic data for the household head and each household member. CadÚnico is an integrated registry of about 80 million people who are beneficiaries of various programs of the Brazilian national government. From CadÚnico, we obtain data on the date of birth, sex, and educational attainment of each household member. CadÚnico also provides data on selected characteristics of the housing unit (such as access to electricity and piped water).

Last, we use the publicly available birth registry SINASC (System of Information on Live Births). The birth registry contains data only for live births and gives us our main outcome variable (birth weight) and the date of conception. In this paper, the date of conception is equal to the date of the last menstrual period.[11] The registry provides additional variables on (1) the newborn, such as the APGAR score,[12] (2) the pregnancy, such as gestation duration in weeks and number of prenatal appointments

[10] More details on the registries and merging are provided in online Appendix A.

[11] This date is based on medical records and is widely used in the medical literature to calculate gestational time. See, for example, Papageorghiou et al. (2014).

[12] The APGAR score measures the immediate vital signs of the newborn based on five criteria: appearance, pulse, grimace, activity, and respiration. The registry provides the APGAR at 1 and 5 minutes.

and (3) the birth delivery, such as natural or cesarean childbirth, and multiple births.

Our period of analysis is 2011–2017 and was chosen due to data constraints – it is the only period when we are able to match the three administrative registries and work with three key dates: the cistern's construction date, the conception date, and the birth date.[13] We explore our data to set well-defined start and end points of pregnancy to counteract the fact that tank delivery may affect pregnancy duration. We create a fixed window of 40 weeks for each pregnant woman by pinpointing her conception date and the child's expected date of delivery at full term (which corresponds to 280 days after conception). Any women in the birth registry receiving a cistern within 280 days after the conception date are included in our final sample. Intuitively, by setting the endpoint of 280 days after conception, we let the gestation length be unrelated to the potential influence of cisterns.[14]

Our final sample excluded multiple pregnancies and those individuals who benefited from policies that could be regarded as confounders.[15] Appendix Figure A.2 shows that the 4,701 pregnant women of our final sample are evenly distributed across the semiarid, while Appendix Figure A.3 depicts the (positive) correlation between the number of weeks of exposure to cisterns during pregnancy and birth weight.[16]

4 Empirical strategy

Our main goal is to estimate the effects of prenatal exposure to cisterns on birth outcomes. The strategy is to compare women located in the same municipality who conceived during the same month of the same year but received cisterns in different weeks of their gestational period. Our identification assumption requires that once a group of people is selected to receive a cistern in a given municipality, pregnant women receive the cisterns independent of the exact timing of their pregnancy.

The following equation summarizes our econometric strategy:

$$Y_{imts} = \mu_s + \gamma_{mt} + \beta \, \mathit{Weeks\ of\ Exposure}_{imts} + \mathbf{X}'_{imts}\Theta + \varepsilon_{imts} \quad , \tag{1}$$

[13]More specifically, the SINASC registry has information on mothers' birth data only after 2011.

[14]For papers similarly addressing the endogeneity in date of birth, see Currie and Rossin-Slater (2013), Black et al. (2016), and Persson and Rossin-Slater (2018).

[15]We excluded beneficiaries of "Água para Todos" program, operated by Brazil's Ministry of Integration, and dropped families who benefited simultaneously from the FWC and the Second Water Cisterns Programs.

[16]Appendix Table A.1 presents summary statistics for our sample and, for comparison, for all births in the semiarid and Brazil.

where Y_{imts} is an outcome of interest observed for child i, conceived in month m and year t, with a mother residing in municipality s. The right-hand side variable of interest is *Weeks of Exposure*, which measures the difference in weeks between the expected date of birth and the cistern's date of construction. The municipality fixed effects μ_s control non-parametrically for municipality unobserved fixed determinants of birth outcomes, while the inclusion of month-year of conception fixed effects γ_{mt} adjust non-parametrically for shocks that are common to all pregnant women at a specific moment in time. Standard errors are clustered at the municipal level.

The vector $\mathbf{X}_{imts}$ includes variables related to the priority criteria set by the government (an indicator for woman-headed family, number of elderly people older than age 65 in the family, number of children, number of teenagers, number of people with special needs, and per capita family income); variables related to characteristics of the mother (age and educational attainment); and indicators for hospital birth and for the newborn sex.[17]

4.1 Assessing our research design

Our primary identifying assumption is that, conditional on covariates, children born to mothers exposed to cisterns during different periods of the gestational duration would have had similar birth outcomes in the absence of the program. The timing of the cistern arrival between conception and the expected date of birth would then be independent of the error term ε_{imts}. Under the validity of this assumption, we can interpret β in Equation (1) as the causal effect of each additional week of exposure to rainwater tanks during pregnancy on birth outcomes. We use our data to examine the plausibility of the identifying assumption that program entry is not correlated with individual characteristics.

We first provide evidence on whether observable characteristics of pregnant women who received cisterns in distinct gestational periods are similar. Table 1 presents the correlation between *Weeks of Exposure* and a set of observable characteristics of the pregnant women. Column (II) presents coefficient estimates using univariate regressions, while column (IV) shows results conditional on other covariates and fixed effects. There is little evidence for significant differences between mother characteristics and the timing of the construction of the cistern. Parameter estimates are small in magnitude and not significantly different from zero.

[17]In section 5, we show that our results are robust to excluding these controls.

We also examine the histogram of the frequency of cisterns distributed according to each week of gestation. If, for instance, the salience of the pregnancy were an important factor influencing the distribution of cisterns, we would expect a positive relationship between advancing in the pregnancy and number of women receiving cisterns. Appendix Figure A.4, however, depicts no such positive relationship. After presenting our baseline results, we discuss additional possible threats to our econometric strategy and empirically evaluate their relevance.

5 Results

5.1 Baseline results

Table 2 presents our baseline results. In odd-numbered columns, we show coefficient estimates of Equation (1) that only include municipality and month-year of conception fixed effects; in even-numbered columns, we present results using a specification that includes the full set of controls. In columns (I) and (II), we show that each additional week of in utero exposure to cisterns is associated with a positive effect on average birth weight of about 1.4 grams. Columns (III) and (IV) document an increase in fetal growth ratio, defined as the birth weight divided by the number of weeks of gestation, which suggests lower intrauterine growth restrictions. Columns (V) and (VI) display small and statistically insignificant effects of exposure to cisterns on gestation length. These results suggest that early exposure to cisterns increases birth weight through positive fetal growth – which implies a reduction in a potential adverse perinatal outcome. Columns (VII) and (VIII) indicate that earlier exposure to the program is associated with a small (about 1%) reduction in the likelihood of a low-birth-weight child. Importantly, we note that the inclusion of individual controls marginally changes our coefficients of interest.

The average effects presented in Table 2 can mask substantial heterogeneity across individuals receiving cisterns. We study how the birth weight effects vary by mothers' characteristics to gain additional insights and uncover potential mechanisms through which cisterns can affect birth weight. Table 3 shows how the effect varies with mothers' marital status and educational attainment. In columns (I) and (II), we split the sample according to marital status. The coefficient of weeks of exposure on birth weight for non-married women is similar to that of married women, and neither is statistically significant. Therefore, changes in bargaining power after the cistern's arrival (which might have affected intra-household resource allocation) do not seem to play a role in explaining birth outcome results.

In columns (III) and (IV), we check if the effects of earlier prenatal exposure to cisterns are stronger for children of mothers who have higher educational attainment. The administrative registries have few categories of educational attainment, which allows us to create two definitions: (1) less educated mothers with up to three years of formal education and (2) more educated mothers with more than three years of formal education. Results suggest that more educated mothers benefit more from exposure. The estimated impact for the educated subgroup is 1.8 grams per additional week of exposure. For less educated mothers, coefficient estimates are small and not significant. Adoption of chlorine disinfection and better use of cisterns may be correlated with educational attainment. Below, we further investigate the relation between education, adequate tank maintenance, and water quality as a potential mechanism driving the results.

5.2 Additional outcomes

We now assess the effects of exposure on additional outcomes available in the birth registry. Columns (I) and (II) of Appendix Table A.2 show that the likelihood of a low 1-minute or 5-minute APGAR score is unaffected by the varying antenatal exposure. These outcomes, however, are known to be imprecise measures of health at birth. For instance, Koppensteiner and Manacorda (2016), who also use data for Brazil and analyze the effects of in-utero exposure to local violence on birth outcomes, find significant effects for birth weight but null results for APGAR scores. Column (III) indicates that the likelihood of delivery via cesarean section is unaffected by program exposure. Columns (IV) and (V) show that program exposure has no impact on the number of prenatal appointments. This suggests that the program does not affect birth outcomes through access to prenatal care – the program does not promote greater access to health care services. Finally, column (VI) indicates that earlier prenatal exposure is not associated with a lower prevalence of preterm birth (defined as fewer than 37 weeks gestation), consistently with null effects we found on pregnancy duration.

5.3 Timing of effects

In Figure 1, we investigate the differential effects of exposure using a more flexible specification. In this analysis, which is similar to that of Currie et al. (2020), we include all mothers who received cisterns from conception date to 16 months after the conception month. We include mothers who received cisterns a few months after pregnancy to test if birth weight somehow correlates with cisterns' arrival. That could indicate, for instance, that the program targets beneficiaries based on measures of health at birth.[18] We estimate separate coefficients for each month of the cistern's arrival relative to those that received cisterns in the four weeks following the 40th week of pregnancy, resembling an event-study approach. This will allow us to (1) better characterize how the effects of the program vary with respect to the timing of the cistern's arrival relative to pregnancy stage and (2) visually assess the possibility of nonlinearity in the relationship between weeks of exposure and health at birth.

Estimates show that mothers who had longer exposure to cisterns (more than four months) present larger and statistically significant gains in their child's birth weight. There is little to no benefit of exposure on the third trimester of pregnancy (those exposed less than four months), and there is no correlation between birth weight and cistern arrival for those who received cisterns postpartum. Importantly, we note that the effects appear to be increasing for those receiving cisterns earlier during pregnancy, suggesting that our linear model, depicted in dashed gray, captures quite nicely how the average birth weight varies with exposure.

5.4 Robustness checks

In this subsection, we present several robustness exercises. In Appendix Table A.3, we show that the results for average birth weight are qualitatively unchanged when we use other control variables as well as conditional on a different set of fixed effects. Average birth weight growth per week of exposure varies between 1.43 and 1.91 grams depending on the specification, while our baseline point estimate is 1.48 grams. In column (I), we control for municipality-level characteristics. Column (II) controls for housing structure characteristics. Column (III) adds state-specific trends, while column (IV) includes municipality-specific time trends. These control, respectively,

[18] Post-pregnancy mothers have been used as the control group in several analyses (e.g., Black et al. (2016), Persson and Rossin-Slater (2018), and Currie et al. (2020)). Following this approach, we obtain significant coefficient estimates of 40.24 (14.03).

for substantial cross-state and cross-municipality differences within regions. Finally, in column (V), we show results for birth weight expressed in logs, to downweight the potential influence of outliers in birth weight. Estimates are quite robust and support the findings of beneficial effects of earlier exposure to the intervention.

Appendix Figure A.5 displays that our results are robust to excluding subsets of our sample. In panel (a), we drop specific states; in panel (b), we exclude each month-year of conception combination; and in panel (c), we drop each one of the 50 largest (rural) municipalities in our sample. These show that our results are not driven by particular regions, municipalities, or cohorts. Appendix Table A.4 reports the results are highly robust after testing alternative clustering of standard errors. Finally, we perform a placebo exercise by randomly assigning in utero exposure to the program to individuals in our sample. In this permutation test, we create a random "weeks of prenatal exposure" to cisterns (between 1 and 40 weeks) for each pregnant woman, and then estimate Equation (1). We repeat this procedure 1,000 times. Appendix Figure A.6 plots the 1,000 placebo coefficients. The baseline coefficient is greater than that of the 95th percentile of the placebo coefficients' distribution, suggesting that our results are unlikely to be driven by chance.

5.5 Alternative check: siblings

We follow a large literature that uses siblings to account for mothers' time-invariant characteristics affecting birth outcomes (Camacho, 2008; Currie and Rossin-Slater, 2013; Currie et al., 2020) and estimate the effect of exposure to cisterns on the birth weight of older siblings born before cisterns were installed.[19] To match siblings born to the same mother, we use the family identifier of the CadÚnico registry.[20] We estimate Equation (1) keeping the corresponding *Weeks of Exposure* for each woman, but now the left-hand side outcome variable is the birth weight in grams of the *unexposed* older sibling. We do not find older siblings for all 4,701 children of our sample, but we are able to find 2,521 older siblings to test whether their birth weight differs. The results presented in columns (I) and (II) of Table 4 show that there is no statistically difference between unexposed older siblings when it comes to birth weight.

We also test whether the subgroup of treated (exposed) siblings of those unexposed

[19] Due to sample size, we do not estimate a mother-fixed effects specification, although our strategy fundamentally performs such exercise.

[20] The details of the matching steps are provided in online Appendix A.

older siblings differ in birth weight. We use Equation (1) for the subsample of exposed siblings for whom we were able to find older siblings. The results indicate higher point estimates, statistically significant at 10% (see columns (III) and (IV)). This exercise suggests that our main results are likely not driven by unobserved heterogeneity across mothers.

6 Water quality

This section provides suggestive evidence that water quality appears to be an important mechanism underlying our findings. We examine the complementarity between education and adequate tank maintenance (as a proxy for compliance with the program's training). The training that families receive is critical to ensure water quality: chlorination of drinking water inactivates many microbial waterborne pathogens, while checking the integrity of roof and tank structures avoids contamination by animal remains.

We access additional microdata on tanks' maintenance with self-reported and laboratory measures of water quality for a sample of 1,328 beneficiaries. This survey was conducted by the Brazilian Agricultural Research Corporation (EMBRAPA, 2009). The survey has individual-level information on gender and educational attainment categories, from which we create a dummy that equals one if the woman is less-educated. We create two water-quality variables: a self-reported dummy (based on the question "Does the water inside the cistern receive any sort of treatment?") and another variable from a professional enumerator's assessment (based on the question "Is the maintenance of the cistern's water adequate?"). For a subset of 245 beneficiaries, we also have microdata on laboratory tests, from which we create a third water-quality dummy that equals one if the level of fecal coliforms is below a threshold indicated by the specialized literature.[21]

Appendix Table A.6 reports the coefficients of a linear cross-section regression (estimated by OLS) with water-quality measures as dependent variables, and a dummy variable for less-educated women as the explanatory variable. The results suggest that less-educated women are less likely to carry out water treatment. This is also true when using objective laboratory measures, which are estimated on a much smaller

[21]The threshold is when the *most probable number* (an index of the number of coliform bacteria) per 100 ml of sample equals 16. See, for instance, Bartram et al. (1996).

sample size. The magnitude for the laboratory measures is high: less educated mothers have a 20% lower probability of having low levels of fecal coliforms.

While it is impossible to rule out other mechanisms, selected mechanisms found in the literature are unlikely to operate, given our intervention's characteristics. For instance, the intervention is unrelated to improving household sanitation or to changes in healthcare infrastructure. The maternal nutritional channel seems much less prominent in our setting compared with that in other papers (e.g., Almond et al. (2011) and Hoynes et al. (2015)), as the small amount of stored water is insufficient to improve rainfed agriculture or even grow gardens for self-consumption.[22] Finally, we use our baseline data to check if education attainment affects birth outcomes through more prenatal visits or longer gestational length. Results in Appendix Table A.7 indicate no association between education and the number of prenatal visits or weeks of gestation.

7 Interpretation

7.1 Magnitude

To put our results' magnitude into perspective, we compare them with those from the literature analyzing the effects of welfare programs. The literature usually estimates exposure by trimester of pregnancy and assesses the effects of receiving selected programs in the first trimester (until the 13th week) or the second trimester (between the 14th and 27th weeks) compared with the third trimester. We use our estimates to calculate the impact of receiving a cistern in the first trimester compared with the third trimester. We multiply our baseline estimate of 1.47 grams increase per week by 26.66 weeks (which corresponds to the duration between the midpoint of the first and third trimesters of the pregnancy). This simple calculation generates a 39.2 grams increase.[23] By and large, this magnitude is comparable to or greater in magnitude than those obtained in the literature, whose estimates range between 13–42 grams. For instance, Hoynes et al. (2011) study the U.S. Special Supplemental Nutrition

[22] Bobonis et al. (2017) study how the program fosters clientelism and find no significant impact on wealth or child food security.

[23] We also generate trimester-specific impacts by estimating Equation (1) substituting weeks of exposure by exposure in each specific trimester. We find an increase of 35.37 grams comparing women whose construction of cisterns took place in the expected first trimester with those whose construction was in the expected third trimester of pregnancy. Results are in Appendix Table A.5.

Program for Women, Infants, and Children (WIC) and find an 18g–29g birth weight increase among participating mothers. Almond et al. (2011) find that Food Stamp Program participation increased birth weight by 15–20 grams for whites and 13–42 grams for Blacks.

7.2 Cost-benefit analysis

We now use our estimates to calculate implicated benefits and compare them with the costs of the program. The unitary cost of an FWC program's cistern is approximately US$640 (R$3,500). To evaluate the benefits, we use recent estimates provided by Clarke et al. (2021), who calculate labor market returns of approximately US$14 for each additional gram of birth weight. Their numbers are based on the point estimate of the returns to birth weight in the United States provided by Behrman and Rosenzweig (2004). Using a simple estimate of birth weight gains of approximately 60 grams when pregnant women are fully exposed to the cisterns program (40 weeks $\times$ 1.47 grams per week), we calculate benefits in the order of US$840 (R$4,600), which implies a net gain of approximately US$200 (R$1,100) for each cistern installed. This suggests that the program yields substantial net gains relative to its costs even when considering only the benefits generated through increased weight at birth.

These back-of-the-envelop calculations are informative but may provide benefits that are arguably underestimated. One reason is that labor market returns to birth weight are likely to be much larger in lower- and middle-income countries than those for developed economies (Currie and Vogl, 2013; Rosenzweig and Zhang, 2013). This further reinforces the conclusion that the program is highly cost-effective.

8 Concluding remarks

This paper studies the effects of a large-scale climate adaptation policy in Brazil's driest and poorest region. Our results that exposure to cisterns positively affects birth outcomes, particularly for more educated mothers, have relevant implications. Building resilience among the most impoverished families is a policy-relevant issue in the context of growing water scarcity, less predictable rainfall, and climate change (Rodell et al., 2018; UN-Water, 2019; Da Mata and Resende, 2020). Another implication is that in settings of decentralized technologies and services, devising measures to improve last-mile compliance seems essential to policies' effectiveness. Finally, our findings suggest that adaptation policies can have far-reaching consequences by positively influencing an important predictor of future individual outcomes.

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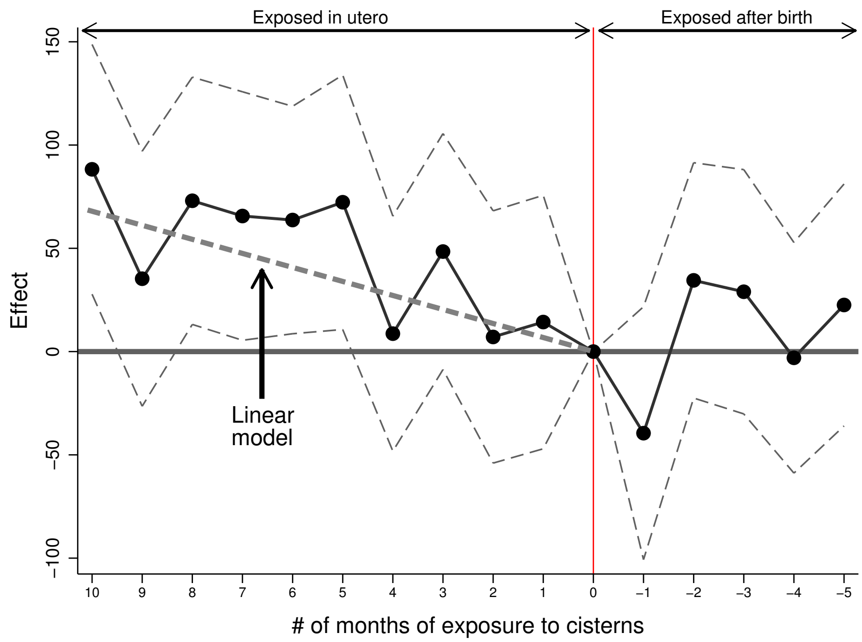
Figure 1: Timing of effects of exposure on birth weight



Notes. The figure shows coefficient estimates and the corresponding 90% confidence intervals from a flexible specification that include indicators for tank's arrival during each month relative to those that received cisterns in the four weeks following the 40th week of pregnancy. For instance, mothers that received cisterns on the last month of pregnancy are represented by $x = 1$ (exposed for ¡1 month), mothers that received on the first month of pregnancy are represented by $x = 10$ (exposed for 9 month), while mothers that received cisterns after 40 weeks of pregnancy (cisterns arrived after birth) are represented by $x < 0$. In this analysis, all mothers that received cisterns in the window from conception date to 16 months after the conception month are included. The dashed grey line shows the baseline results from Table 2 estimating Equation (1).

Table 1: Weeks of Exposure to Cisterns and Control Variables (Balance Test)

	Mean	Univariate OLS		FE & controls	
		coef.	R^2	*coef.*	R^2
	(I)	(II)	(III)	(IV)	(V)
Panel A: Baseline control variables					
Woman head of the family	0.930	0.0002	0.0001	0.0002	0.2241
		(0.0003)		(0.0003)	
# elderly in the family	0.0166	0.0002	0.0001	0.0003	0.2034
		(0.0002)		(0.0003)	
# children in the family	1.445	-0.0013	0.0001	-0.0013	0.4256
		(0.0016)		(0.0018)	
# disabled in the family	0.0359	-0.0002	0.0001	-0.0002	0.2283
		(0.0003)		(0.0003)	
Per capita family income	38.13	-0.0732	0.0002	-0.0590	0.2993
		(0.0695)		(0.0819)	
Mother's age	27.28	-0.0025	0.0000	0.0061	0.4064
		(0.0079)		(0.0083)	
Less-educated mother	0.825	0.0004	0.0001	0.0003	0.4407
		(0.0005)		(0.0005)	
Panel B: Controls in the robustness					
Electricity	0.919	0.0000	0.0000	0.0000	0.3610
		(0.0004)		(0.0004)	
Bathroom	0.713	0.0004	0.0001	0.0001	0.3714
		(0.0006)		(0.0006)	
Asbestos roof	0.0104	0.0000	0.0000	-0.0000	0.2957
		(0.0001)		(0.0001)	
Thatched roof	0.865	-0.0004	0.0001	-0.0000	0.9321
		(0.0004)		(0.0001)	
Ceramic roof tiles	0.123	0.0003	0.0001	0.0003	0.3937
		(0.0004)		(0.0004)	
Other types of roof	0.00203	0.0000	0.0000	0.0000	0.1837
		(0.0000)		(0.0001)	

Notes. The table reports the correlation between the number of weeks of exposure to cisterns during pregnancy and the characteristics used as control variables in the regressions. Column (I) reports the mean and the standard deviation (in parenthesis) of each variable. Columns (II) and (III) report respectively the coefficient and the R^2 of the univariate OLS regression of weeks of exposure on each characteristic. Columns (IV) and (V) report the coefficient and the R^2 after adding control variables and fixed effects. Standard errors in columns (II) and (IV) are clustered at the municipal level.
*** p<0.01, ** p<0.05, * p<0.1

Table 2: Effects of Exposure to Rainwater Tanks on Birth Outcomes

	Birth Weight		Fetal Growth Rate		Weeks of Gestation		Low Birth Weight	
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
Weeks of exposure	1.4391**	1.4795**	0.0371**	0.0393**	0.0008	0.0005	-0.0006*	-0.0006*
	(0.7177)	(0.7179)	(0.0183)	(0.0182)	(0.0026)	(0.0027)	(0.0004)	(0.0004)
Month-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Mean outcome	3,226.9	3,226.9	84.4	84.4	38.3	38.3	0.065	0.065
Observations	4,558	4,558	4,558	4,558	4,558	4,558	4,558	4,558

Notes. This table shows the results of estimating Equation (1). Standard errors clustered at the municipal level. The odd-numbered columns present results without controls, while the even-numbered columns present results controlling for: an indicator woman-headed family, # elderly, # children, # teenagers, # people with special needs, per capita income; mother's age, indicator for less-educated mother, indicator for hospital birth, indicator for newborn sex. There are 4,558 observations in the regressions, which corresponds to the sample 4,701 observations excluding the singleton observations (pregnant women living in municipality s who conceived in the month-year t, in which no other program's beneficiary resided in the same municipality and conceived in the same month-year) dropped by Equation (1).
*** p<0.01, ** p<0.05, * p<0.1

Table 3: Results by Characteristics of Mothers

	Dependent variable: Birth Weight (g)			
	Marital status		Less-educated mother	
	Married	Non-Married	No	Yes
	(I)	(II)	(III)	(IV)
Weeks of exposure	1.3794	1.3857	1.8437**	0.2237
	(1.2089)	(1.0975)	(0.8181)	(2.0331)
Month-year fixed effect	Yes	Yes	Yes	Yes
Municipality fixed effect	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	1,728	2,508	3,709	598

Notes. This table shows the results of estimating Equation (1). Standard errors clustered at the municipal level. Less-educated mothers are those with up to three years of formal education. Control variables are: an indicator woman-headed family, # elderly, # children, # teenagers, # people with special needs, per capita income; indicator for hospital birth, indicator for newborn sex.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4: Older Siblings

Dependent Variable:	Birth Weight Older Sibling		Birth Weight Treated Sibling	
	(I)	(II)	(III)	(IV)
Weeks of exposure	0.646	0.655	1.610*	1.643*
	(1.013)	(1.023)	(0.937)	(0.933)
Month-year fixed effects	Yes	Yes	Yes	Yes
Municipality fixed effects	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Observations	2,299	2,299	2,299	2,299

Notes. This table shows the results of estimating Equation (1). Standard errors clustered at the municipal level. Control variables are: an indicator woman-headed family, # elderly, # children, # teenagers, # people with special needs, per capita income; mother's age, indicator for less-educated mother, indicator for hospital birth, indicator for newborn sex.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

For Online Publication

Online Appendix to "Climate Adaptation Policies and Infant Health: Evidence from a Water Policy in Brazil"

Daniel Da Mata, Lucas Emanuel, Vitor Pereira, and Breno Sampaio

April 13, 2021

A Data

The paper links three administrative registries: (1) the administrative registry of the First Water Cisterns (FWC) program, (2) Brazil's national registry of social programs (Unified Household Registry – "CadÚnico") and (3) the birth registry SINASC (System of Information on Live Births).

The FWC registry identifies each beneficiary by name, date of birth, CPF (a nine-digit individual taxpayer identification number), and NIS (registration number assigned to beneficiaries of social assistance programs). It pinpoints the municipality of residence and the exact date of the tank construction. It also provides the socioeconomic characteristics of each household.

CadÚnico provides socioeconomic data for both the primary beneficiary and each household member (e.g., educational attainment and income). CadÚnico also has data on the housing unit (such as access to electricity and piped water). CadÚnico is the main instrument for identifying socioeconomic characteristics of low-income Brazilian families (whose monthly income is up to half the value of the national monthly minimum wage). It is considered a census of the country's low-income families, with about 80 million registered people. This database serves as the primary reference for beneficiary selection and the integration of various social programs administered by the federal government. CadÚnico must be updated every two years.

SINASC provides information on our main outcome variable (birth weight) and the date of conception. The registry provides additional variables on (1) the newborn, such as the APGAR score, (2) the pregnancy, such as gestation duration in weeks and number of prenatal appointments and (3) the birth delivery, such as natural or cesarean childbirth, hospital or other health facilities, and multiple births. The APGAR score measures the vital signs of the newborn in the immediate extra-uterine life. The lower the APGAR, the higher the risk of the newborn. It consists of 5 criteria, each with scores of up to 2 points: appearance, pulse, grimace, activity, respiration. The registry provides the APGAR at 1 minute and 5 minutes.

To merge the three registries, we proceeded as follows. We found each FWC beneficiary in CadÚnico by using the NIS number. CadÚnico then provides a direct link between the beneficiary and the family members. By merging these two registries, we obtained essential information for the study, such as the date of birth of the mother and the child, place of residence, date of rainwater tank construction, and some characteristics consistent with the priority of participating in the FWC program. CadÚnico considers a household all tenants of the same housing unit, even if individuals are not relatives. A person who lives by herself is considered a single-member household.

Next, we selected the beneficiaries and household members who gave birth between 2011 and 2017, and then trimmed the dataset further to select only those whose cistern's construction date was within the gestational period (between the conception date and the expected birth date). We were able to find household members in the birth registry by using four characteristics: (1) the newborn's birth date, (2) the newborn's sex, (3) the mother's birth date, and (4) the municipality of residence of the mother. The final dataset was de-identified.

To find the elder siblings within each household, we again used the richness from CadÚnico data. First, for each member of our sample, besides the key merged variables (mother's date of birth, child's birth date, baby's gender, and the municipality of residence), we also worked with the family code variable. Thus, we could merge this sample again with CadÚnico, getting the siblings of the child benefiting from the cistern during gestation. For this analysis, we considered those children born between 2000 and 2017.

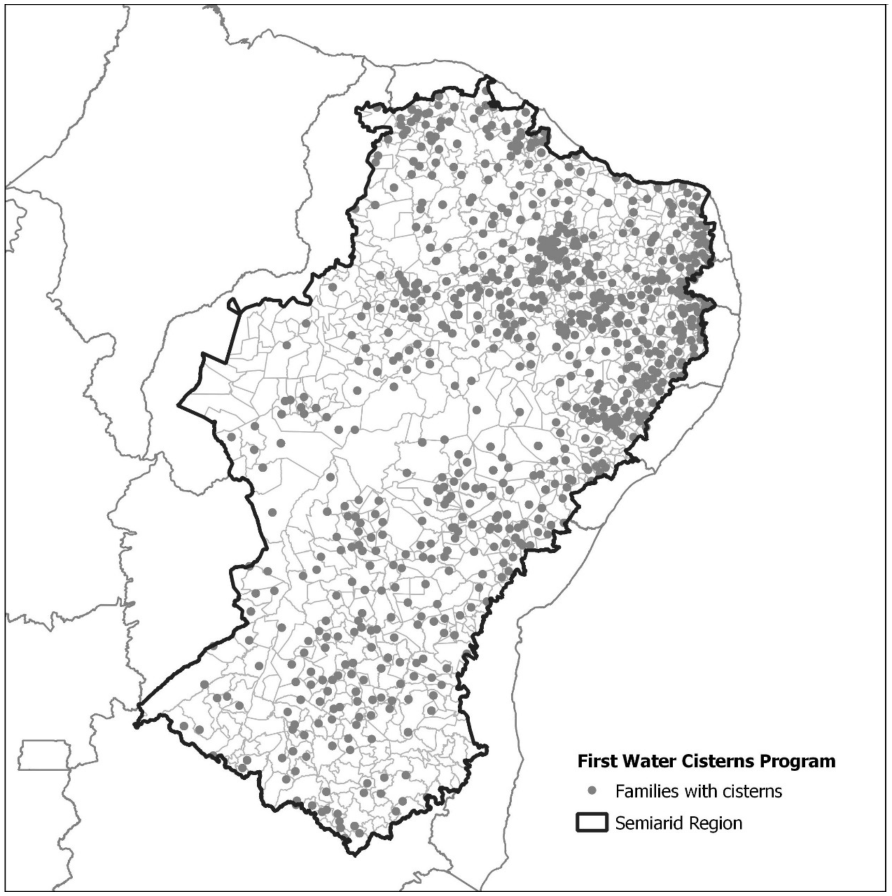
The next step was to merge these children's information with SINASC using the mother's age variable instead of her birth date: the mother's birth date only started to be collected in the public SINASC data from 2011. Based on this sample, we compared mothers who received a cistern at the beginning of gestation with those who benefited at the end of pregnancy but now using the unexposed sibling's birth weight as the outcome variable. In this analysis, we considered the immediate elder sibling as the unexposed sibling. As a final test to validate this analysis, we repeated the same analysis and checked birth weight for the subgroup of treated (exposed) siblings of those unexposed older siblings.

List of Figures

Figure A.1: Cistern in the Brazilian Semiarid

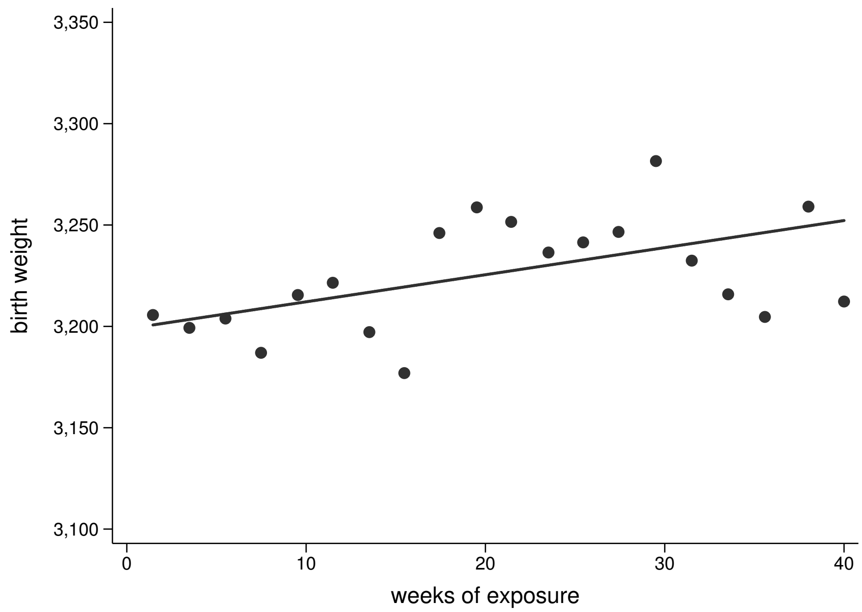
Notes. Ministry of Social Development.

Figure A.2: Location of the Individuals of our Sample



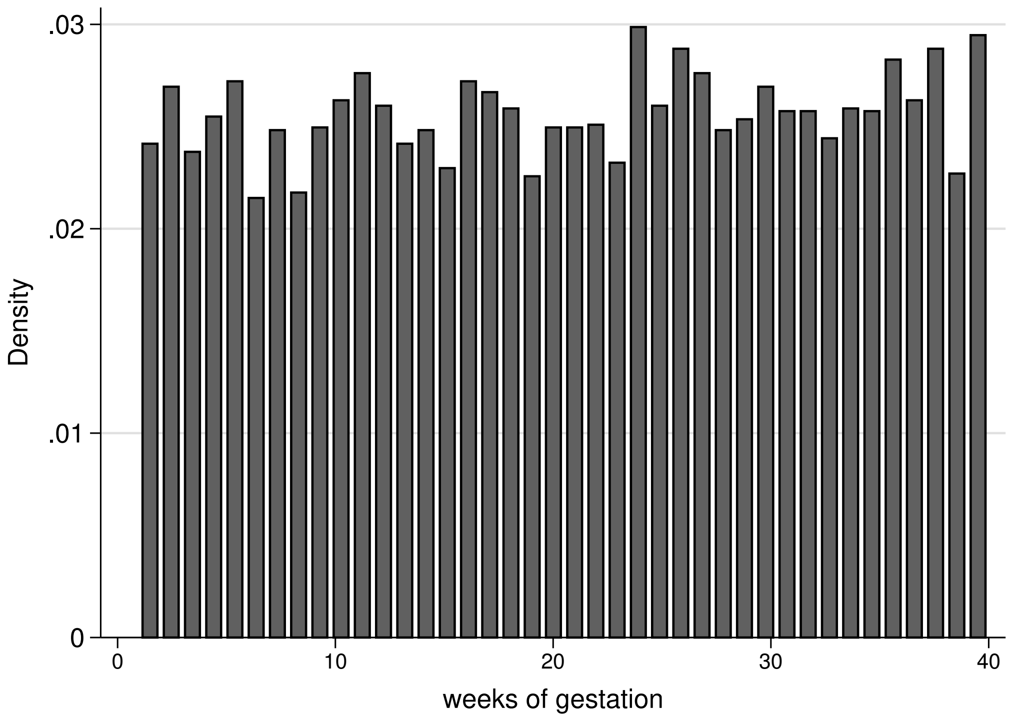
Notes. The figure shows the location of each of the 4,701 individuals of our full sample.

Figure A.3: Correlation between weeks of exposure and birth weight



Notes. The figure shows the correlation between the number of weeks of exposure to cisterns and the average birth weight (in grams). Weeks of exposure is the difference between the expected date of birth and the date of the cistern construction. The solid line indicates a linear fit.

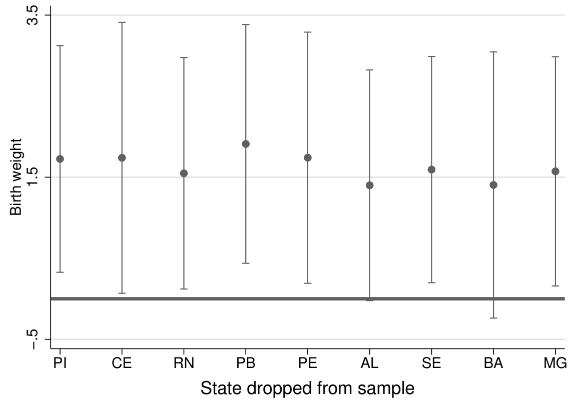
Figure A.4: Histogram of cisterns by week of gestation



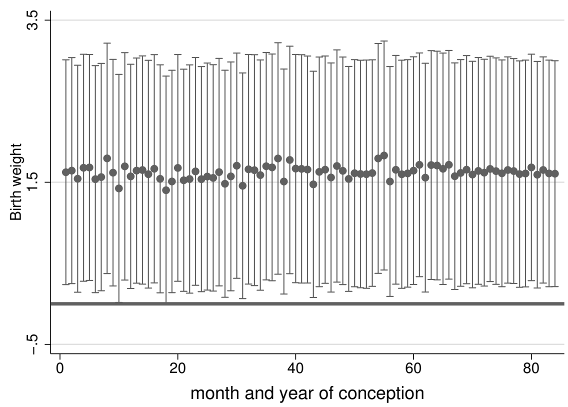
Notes. The histogram shows the frequency of cisterns distributed to pregnant women in our final sample according to each week of gestation.

Figure A.5: Robustness to dropping specific subsamples

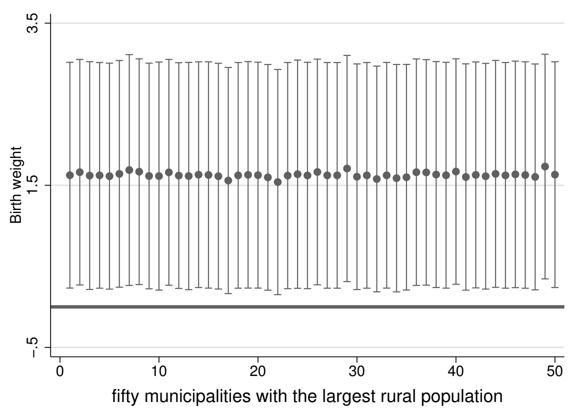
(a) Dropping states



(b) Dropping month-year of conception cohorts

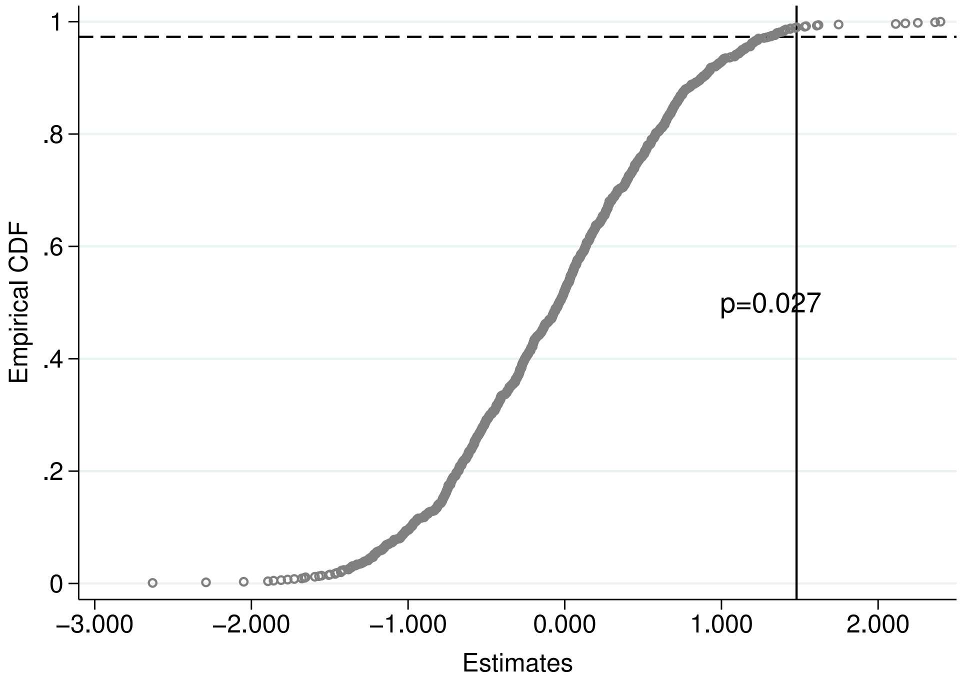


(c) Dropping 50 largest municipalities by rural population



Notes. This figure plots point estimates for the effect of in utero exposure to rainwater program, denoted by square markers, and corresponding 95-percent confidence intervals represented by lines. Each estimate is obtained from a separate regression estimating Equation (1) with birth weight in grams as the dependent variable, and each sub-figure presents a series of coefficient estimates for excluding subsets of different categories of observations: State of birth (Panel a); conception month-years (Panel b) and the 50 largest municipalities based on rural population (Panel c).

Figure A.6: Placebo: randomizing weeks of exposure



Notes. Results of estimating Equation (1) for 1,000 placebo treatments. The solid line represents the baseline coefficient reported in Table 2. Standard errors clustered at the municipal level. Control variables in all regressions are: an indicator woman-headed family, # elderly, # children, # teenagers, # people with special needs, per capita income; mother's age, indicator for less-educated mother, indicator for hospital birth, indicator for newborn sex.

List of Tables

Table A.1: Summary statistics of our sample and all live births in the semiarid region and Brazil

Variables	Our sample	Semiarid all births	Brazil all births
	(I)	(II)	(III)
Birth weight (g)	3,226	3,228	3,201***
	(512.6)	(544.5)	(539.8)
Low birth weight	0.0651	0.0721*	0.0764***
	(0.247)	(0.259)	(0.266)
Fetal growth	84.34	82.89***	82.53***
	(13.37)	(24.79)	(17.68)
Apgar 1 min	8.099	8.15**	8.334***
	(1.137)	(1.182)	(1.215)
Weeks of gestation	37.84	38.69***	38.54***
	(1.881)	(2.352)	(2.203)
Weeks of gestation < 37	0.154	0.112***	0.107***
	(0.361)	(0.315)	(0.309)
Mother's age	27.11	25.4***	26.13***
	(6.091)	(6.604)	(6.641)
Mother' s education	3.344	3.631***	3.879***
	(0.813	(0.802)	(0.754)
Number of births	1.559	1.162***	1.035***
	(1.587)	(1.451)	(1.326)
Observations	4,701	2,302,570	20,032,165

Notes. Data are mean (SD). Our sample includes pregnant women benefited by the First Water Cisterns Program between the date of conception and the expected date of birth. Column (I) displays statistics for our full sample, while Columns (II) and (III) show data for all live births in the Semiarid region and Brazil, respectively. The period is from Jan 2011 to Dec 2017. The asterisks (*) in Columns (II) and (III) represent whether the difference in the means between our sample in relation to each comparison group is statistically significant (*** p<0.01, ** p<0.05, * p<0.1).

Table A.2: Additional birth outcomes

Dependent variables:	Apgar 1 min	Apgar 5 min	Cesarean Delivery	Prenatal visits ≤ 3	Prenatal visits ≥ 7	Weeks of gestation < 37
	(I)	(II)	(III)	(IV)	(V)	(VI)
Weeks of exposure	-0.0004	-0.0003	0.0001	-0.0002	0.0007	-0.0003
	(0.0016)	(0.0012)	(0.0006)	(0.0003)	(0.0006)	(0.0005)
Month-year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Municipality fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,282	4,285	4,556	4,561	4,547	4,561

Notes. This table shows the results of estimating Equation (1). Standard errors clustered at the municipal level. Control variables are: an indicator woman-headed family, # elderly, # children, # teenagers, # people with special needs, per capita income; mother's age, indicator for less-educated mother, indicator for hospital birth, indicator for newborn sex.
*** p<0.01, ** p<0.05, * p<0.1

Table A.3: Robustness to additional controls and fixed effects

	Dependent variable: Birth Weight (g)				Ln birth weight
	(I)	(II)	(III)	(IV)	(V)
Weeks of exposure	1.9174*	1.4349*	1.4568**	1.5267*	0.0006**
	(1.0071)	(0.7745)	(0.7164)	(0.8125)	(0.0003)
Month-year fixed effect	Yes	Yes	Yes	Yes	Yes
Municipality fixed effect	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Municipality charact. by year-of-construction	Yes	No	No	No	No
Housing structure	No	Yes	No	No	No
State linear trend	No	No	Yes	No	No
Municipality linear trend	No	No	No	Yes	No
Observations	4,554	4,020	4,558	4,558	4,558

Notes. This table shows the results of estimating Equation (1). Standard errors clustered at the municipal level. Control variables in all columns are: an indicator woman-headed family, # elderly, # children, # teenagers, # people with special needs, per capita income; mother's age, indicator for less-educated mother, indicator for hospital birth, indicator for newborn sex. Time-invariant municipal characteristics in column (I) include unemployment rate, child labor rate, low income population rate, Gini Index, rural population, illiteracy rate. Indicator variables for the housing structure characteristics in column (II) are: electricity, bathroom, water treatment, asbestos roof, thatched roof, ceramic roof tiles and other types of roofs.
*** p<0.01, ** p<0.05, * p<0.1.

Table A.4: Robustness to Alternative Clustering

	Dependent variable: Birth Weight (g)								
	Baseline Result	Quarter-Year by Munic.	Conley spatial clustering cutoff distances in kilometers: 10 km	50 km	100 km	150 km	200 km	250 km	500 km
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)	(IX)
weeks_exposure	1.4795**	1.4795**	1.4795**	1.4795**	1.4795**	1.4795**	1.4795**	1.4795**	1.4795**
	(0.7179)	(0.7234)	(0.6705)	(0.6522)	(0.6518)	(0.6295)	(0.6465)	(0.6654)	(0.6231)
Month-year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,558	4,558	4,701	4,701	4,701	4,701	4,701	4,701	4,701

Notes. Column (I) presents the baseline result reported in column (II) of Table 2. Columns (II) uses cluster by quarter of year $\times$ municipality. Columns (III)-(VIII) report results using Conley (1999) standard errors to account for possible spatial correlation with distance cutoffs varying from 50 to 500 kilometers.
*** p<0.01, ** p<0.05, * p<0.1.

Table A.5: Robustness: Specification with effects by trimester of exposure

	Dependent variables:				
	Birth Weight	ln Birth Weight	Fetal Growth	Low Birth Weight	Weeks of Gestation
	(I)	(II)	(III)	(IV)	(V)
Trimester 1	35.3768*	0.0125*	0.8981*	-0.0110	0.0225
	(20.4282)	(0.0072)	(0.5232)	(0.0107)	(0.0781)
Trimester 2	23.0941	0.0081	0.5943	-0.0039	0.0167
	(20.6612)	(0.0072)	(0.5334)	(0.0097)	(0.0682)
Month-year fixed effect	Yes	Yes	Yes	Yes	Yes
Municipality fixed effect	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Observations	4,558	4,558	4,558	4,558	4,561

Notes. This table shows the results of estimating the baseline Equation (1) in which we replace the variable for weeks of exposure by two dummies indicating that the cistern was built in the first ($\mathbf{trim1}_{imts}$) or the second ($\mathbf{trim2}_{imts}$) trimesters of pregnancy (relative to the third trimester). $trim1_{imts}$ equals to one if the cistern was constructed until the 13th week of gestation, 0 otherwise for child i, conceived in month m and year t, with a mother residing in municipality s; $trim2_{imts} = 1$ if the construction occurred between the 14th and the 27th week of gestation. Standard errors clustered at the municipal level. Control variables are: an indicator woman-headed family, # elderly, # children, # teenagers, # people with special needs, per capita income; mother's age, indicator for less-educated mother, indicator for hospital birth, indicator for newborn sex.
*** p<0.01, ** p<0.05, * p<0.1.

Table A.6: Adoption and educational level

Dependent variable:	Proper use of cistern			Family carries out water treatment			Fecal coliform	
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
Less-educated dummy	-0.0714***	-0.0605***	-0.0509***	-0.1065***	-0.1427***	-0.0597**	-0.1966	-0.2118
	(0.0202)	(0.0196)	(0.0165)	(0.0277)	(0.0270)	(0.0249)	(0.5132)	(0.5145)
Observations	1,285	1,285	1,285	1,293	1,293	1,293	163	163
State FE	No	Yes	No	No	Yes	No	No	Yes
Municipality FE	No	No	Yes	No	No	Yes	No	No

Notes. This table shows the results of estimating a cross-section OLS regression with the measure of water quality as the dependent variable, and a dummy for less-educated women as the explanatory variable. Robust standard errors in parenteses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.7: Results by Educational Attainment

Dependent variable:	Prenatal Visits		Weeks of gestation	
	Less-educated mother		Less-educated mother	
	Yes	No	Yes	No
	(I)	(II)	(III)	(IV)
Weeks of exposure	0.0006	0.0003	0.0002	-0.0001
	(0.0030)	(0.0007)	(0.0145)	(0.0034)
Month-year fixed effect	Yes	Yes	Yes	Yes
Municipality fixed effect	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	821	3,870	824	3,880

Notes. This table shows the results of estimating Equation (1). Standard errors clustered at the municipal level. Control variables are: an indicator woman-headed family, # elderly, # children, # teenagers, # people with special needs, per capita income; mother's age, indicator for less-educated mother, indicator for hospital birth, indicator for newborn sex.
*** p<0.01, ** p<0.05, * p<0.1.