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Essays on Health Economics

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Tese apresentada ao Programa de Pós-graduação em Economia (PIMES) do Departamento de Economia da Universidade Federal de Pernambuco como requisito parcial para obtenção do grau de Doutor em Economia.

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ABSTRACT

This thesis encompasses two chapters that empirically analyze the effects of positive and negative *in utero* events on birth outcomes in Brazil. Both issues are related to Health Economics literature. Below are the abstracts for the respective chapters.

Chapter 1. ***Water and Birth Weight: Evidence from a large scale program in the Brazilian Semiarid Region***

This paper studies how *in utero* exposure to a large-size water harvesting program affects birth outcomes. We assess the effects of the Cisterns Program, which built approximately one million cisterns in Brazil's poorest and driest region to promote small-scale decentralized rainfall harvesting and storage. Our empirical strategy compares the outcomes of women exposed to cisterns in different stages of their pregnancy. The results show that access to cisterns during early pregnancy increases birth weight, particularly for more educated women. The results suggest that policies for adaptation and reduction of vulnerability may bring about positive effects on an important predictor of future individual outcomes.

Chapter 2. ***Environmental disasters and infant health: Evidence from the Mariana mining disaster in Brazil***

Implementation and enforcement of environmental regulation policies are necessary for promoting environmentally sustainable industrial development. When such policies are poorly enforced, there is a chance significant impact on the local communities. We study the health consequences of one of the largest environmental disasters in the global mining industry, which largely stemmed from regulatory failure. Taking advantage of the timing and location of the Mariana mine tailings dam collapse in Brazil, we show that *in utero* exposure to the tragedy significantly reduced birth weight and increased infant mortality. The adverse effects were stronger for infants born to less educated and single mothers. These findings indicate that poorly enforced environmental regulation may have long-term welfare impacts on local communities.

Keywords: Birth weight. Cisterns. *in utero* exposure. Place-based policies. Preventable disasters. Mining.

RESUMO

Esta tese abrange dois capítulos que analisam empiricamente os efeitos de eventos positivos e negativos durante a fase intrauterina sobre os resultados de saúde de recem nascidos no Brasil. Ambas as questões estão relacionadas à literatura de Economia da Saúde. Abaixo estão os resumos dos respectivos capítulos.

Capítulo 1. ***Água e Peso ao Nascer: Evidências de um programa de larga escala no Semiárido Brasileiro***

Este artigo estuda como a exposição intrauterina a um programa de coleta de água de grande porte afeta os resultados do nascimento. Avaliamos os efeitos do Programa Cisterns, que construiu cerca de um milhão de cisternas na região mais pobre e seca do Brasil para promover a captação e armazenamento descentralizados em pequena escala das chuvas. Nossa estratégia empírica compara os resultados dos recem nascidos de grávidas expostas a cisternas em diferentes estágios da gestação. Os resultados mostram que o acesso às cisternas durante o início da gravidez aumenta o peso ao nascer, principalmente para mães com maior escolaridade. Os resultados sugerem que políticas para adaptação e redução da vulnerabilidade podem trazer efeitos positivos em um importante preditor de futuros resultados individuais.

Capítulo 2. ***Desastres ambientais e saúde infantil: evidências do desastre da mineração de Mariana no Brasil***

A implementação e aplicação de políticas de regulamentação ambiental são necessárias para promover o desenvolvimento industrial ambientalmente sustentável. Quando essas políticas são pouco aplicadas, há um impacto significativo nas comunidades locais. Estudamos as conseqüências para a saúde de um dos maiores desastres ambientais do setor de mineração global, que em grande parte resultou de falhas regulatórias. Aproveitando o momento e a localização do rompimento da barragem de rejeitos de Mariana no Brasil, mostramos que a exposição intrauterina à tragédia reduziu significativamente o peso ao nascer e aumentou a mortalidade infantil. Os efeitos adversos foram mais fortes em bebês nascidos de mães menos educadas e solteiras. Essas descobertas indicam que a regulamentação ambiental mal aplicada pode ter impactos a longo prazo no bem-estar das comunidades locais.

Palavras-chave: Peso ao nascer. Cisternas. exposição intrauterina. Políticas voltadas a localidades. Desastres evitáveis. Mineração.

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1 WATER AND BIRTH WEIGHT: EVIDENCE FROM A LARGE SCALE PROGRAM IN THE BRAZILIAN SEMIARID REGION

1.1 Introduction

Water stress is felt by billions of people around the globe. According to UN-Water (2019), more than 2 billion people experience high water stress, and roughly 4 billion suffer from severe water scarcity during at least one month of the year (Mekonnen and Hoekstra, 2016). Water availability and quality are essential for life in different dimensions, including health. Although all individuals need proper access to water, pregnant women may need more than the average person. Indeed, water availability and quality may be crucial during pregnancy because of their influence on neonatal health (Currie et al., 2013). However, the extent to which public policies aimed at alleviating water constraints can successfully improve neonatal outcomes still needs to be evaluated.

Water stress is a major challenge for poor rural populations who live in areas where traditional water policies (e.g., sanitation) are likely to be unfeasible. Several policies have been devised then to provide alternative sources of water to people living in poor, climate vulnerable rural areas. One set of policies is related to water harvesting. These policies consider rainfall as an important manageable resource so that the storage of rainwater would work as a buffer for poor families living in water-stressed conditions. Large-scale water harvesting has been implemented in both developed (e.g., Australia) and developing countries (e.g., China and India). Assessing the impacts of water policies on neonatal health – in a context of growing water scarcity in the coming decades – can be particularly important as poor birth outcomes have long-lasting adverse impacts on individuals (Currie and Vogl, 2013; Aizer and Currie, 2014; Almond et al., 2018; Shah and Steinberg, 2017).

This paper studies how *in utero* exposure to a large-size water harvesting program affects birth outcomes. We assess the effects of the First Water Cisterns Program, which constructed about one million cisterns from 2003 to 2017 in the Brazilian semiarid, the country's poorest region. The program builds tanks next to houses to promote small-scale decentralized rainfall harvesting and storage. Built with a simple, low-cost technology, rainwater tanks collect water during the rainy season and have a standard storage capacity that is sufficient for domestic use (drinking and cooking) during a dry season. To ensure tank water quality, families receive training on point-of-use disinfection as well as on basic maintenance steps.[1]

To examine the impact of *in utero* exposure to cisterns on birth outcomes, we link different administrative registries. We exploit the richness of the data to create a sample of women who have benefited from the in-kind program during their pregnancy. We connect (i) the national birth registry to gather information on birth outcomes, (ii) the program's registry, which comprises the complete record of the program's implementation including the date of the construction of each cistern and (iii) the national registry of social programs (Unified Household Registry – "CadÚnico") to gather socioeconomic data of the beneficiary and each family member. More details on the data are provided in Section 1.3.

We use the rollout of the Cisterns Program across municipalities in Brazil to compare women located in the same municipality whose cisterns were built in different weeks in their gestational period.[2] Our analysis relies on the identifying assumption that, conditional on the selection of a group of people in a locality, the distribution of the cisterns to pregnant women is unrelated to the exact timing of their pregnancy. Our main empirical specification compares the outcomes of women who were exposed to cisterns in different stages of their pregnancy — we measure exposure by counting the number of weeks between the date of construction and the expected date of birth. In addition, we work with an alternative specification to check for any differential effects across trimesters of pregnancy.

We present several exercises assessing the plausibility of the identifying assumption. We show that there are neither composition differences nor evidence of significant spillovers between early exposed and late exposed pregnant women. Moreover, the salience of the pregnancy does not seem to influence the construction of cisterns. In addition, we check for previous behavioral responses of our sample of pregnant women. Comparing women exposed to cisterns in different stages of their pregnancy, we do not find differences in the outcomes of previous pregnancies (where older siblings born to the same mother were unexposed to the cisterns), and participation in other social assistance programs such as the nationwide cash-transfer "Bolsa Família" and health-related programs.[3]

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[1] The program is also known as the One Million Cisterns Program. The program has inspired other programs around the world such as the "One million cisterns for the Sahel" (FAO, 2018). The Cisterns Program has additional components that build cisterns for improving agricultural production and cisterns in schools. See more details in Subsection 1.2.2.

[2] A municipality is the third and lowest level of governance in Brazil. They are roughly equivalent to counties in the U.S.

[3] Our setting eliminates concerns over sample selection due to adverse climatic shocks affecting fertility decisions (Currie, 2009; Rocha and Soares, 2015). Abiona (2017), for example, find that women in

Our results show that there are differential effects across gestational age at exposure. According to our baseline results, each additional week of exposure to cisterns is associated with a positive effect on average birth weight of 1.5–1.7 gram. The effects on birth weight occur due to an increase in fetal growth ratio (defined as the birth weight divided by the number of weeks of gestation), but not through longer gestational length. We find very small effects on the incidence of low birth weight. When we assess the exposure by trimester of pregnancy, we find larger positive impacts on those exposed earlier in the pregnancy. Comparing women exposed in the first trimester to those exposed in the third trimester, we find an increase in birth weight of about 46 grams, which we interpret as evidence revealing when exposure to cisterns is more effective.

The results for average birth weight are qualitatively unchanged when we use other control variables, different fixed effects and after trimming the tails of our sample. Besides, robustness checks indicate that our results are not driven by particular regions or birth cohorts. The significance of results is highly robust to alternative clustering of the standard errors. We perform a placebo exercise by randomly assigning *in utero* exposure to the program to individuals in our sample. The placebo exercise indicates that our results are unlikely to be an artifact of our data. By and large, the results of the robustness exercises suggest that our baseline estimates may show conservative effects of early exposure to the water harvesting policy.

Earlier intrauterine exposure to rainwater tanks is associated with stronger effects for more educated women in our sample. More educated women may be more likely to use cisterns in accordance with the training promoted by the program. We gather additional microdata on tanks' maintenance, and these data show that there is a positive relationship between years of schooling and the use of chlorine as a disinfectant for water treatment.[4] This result is consistent with several papers in the literature (e.g., Currie and Almond, 2011), which point out that more educated mothers are more likely to respond or compensate for adverse conditions. Our results suggest that how mothers respond or compensate contributes to the success of a program in improving neonatal health.

Policies for water harvesting may affect fetal outcomes mainly through four mechanisms: maternal nutrition, stress, quantity and quality of water. The maternal nutritional channel is much less prominent in our intervention compared to other papers evaluating public policies and birth outcomes.[5] The First Water Cisterns Program does not aim to enhance food security, and the intervention is not associated with the nutritional gains of improvements in rainfed agriculture. Stress levels in drought-prone areas are induced by (i) physical hardship from the time allocated for searching and transporting water and

Uganda use contraceptives strategically as a way to postpone pregnancy during adverse rainfall shocks which lead to lower agricultural yields.

[4]The maintenance dataset is not for our final sample of pregnant women, but for a different sample of beneficiaries in the Semiarid.

[5]For instance, Almond et al. (2011) and Hoynes et al. (2015).

related to (ii) the perennial risk of water shortage. Decreasing stress levels are likely to operate for both more and less educated mothers. As for water availability, if anything, it is likely to increase equally for more and less educated mothers. However, education interferes more with the maintenance of water quality. Our results that birth outcomes improve for more educated mothers highlight a potential role for adoption. Chlorination of drinking water and continuous maintenance of cisterns involve effort and time costs, such that the benefits related to their adoption may only surpass the costs for more educated mothers.

To the best of our knowledge, we are one the few papers to assess the effects of a place-based policy on neonatal health. Place-based policies – whose set of themes ranges from infrastructure and credit to irrigation – have been tailored over the years to support and promote the development of lagging regions.[6] The literature in economics has thus far focused mostly on assessing the effects of *people-based policies* on birth outcomes (e.g., Barber and Gertler, 2008; Amarante et al., 2016; Aizer et al., 2016, among others), while studies on place-based policies have mostly focused on outcomes other than neonatal health (e.g., Glaeser and Gottlieb, 2008; Da Mata and Resende, 2018; Shenoy, 2018). Our first and primarily contribution is to build a link between these two strands of the literature, as we study the effects of a *place-based policy* on birth outcomes. While the papers assessing people-based policies focus on maternal nutrition and stress as the important mechanisms, the main channel in our setting is arguably water quality.[7]

Second, this paper is related to the literature on the impacts of in-kind welfare programs. By using detailed data on the timing of the construction of cisterns, this paper sheds new light on how in-kind programs affect neonatal outcomes. We find comparable or stronger results on birth weight than other papers in the literature (e.g. Hoynes et al., 2011; and Almond et al., 2011).[8] Finally, we contribute by studying the effects of an adaptation policy on health. Climate shocks have important effects on birth outcomes (e.g. Maccini and Yang, 2009 and Rocha and Soares, 2015). In a context of growing water scarcity in the coming decades IPCC (2014), adaption policies may have an important role when it comes to neonatal health. Blakeslee et al. (2019) studies the impacts of long-term water depletion in rural India, using an exogenous variation in groundwater supply, a vital source of irrigation water. They find that the households are able to respond to water scarcity by shifting labor into off-farm employment, mainly in areas with higher levels of local industrial development, resulting in a modest drop in total income or subjective well-

[6]See Neumark and Simpson (2015) for a survey on the impacts on place-based policies.

[7]We are also related to the literature that has been studying how birth outcomes can be affected by shocks of varying stress intensity, ranging from mild exposures to the effects of disasters (Almond, 2006; Almond et al., 2009; Almond and Mazumder, 2011). While some studies have measured potential exposure to life-defining events (as listed in Black et al., 2016; Persson and Rossin-Slater, 2018; Currie et al., 2018), we relate more directly to the ones that study the effects of actual public policies (Almond and Currie, 2011).

[8]We compare our results to the ones of studies of policies in Subsection 1.5.4.

being. We add to this literature by studying the effects of a large-size water harvesting whose goal is to counteract uncertain water supply in drought-prone regions by providing a cheap and scalable technology. Policies for water harvesting in climate vulnerable areas are considered a key intervention in adaptation and to reduce vulnerability (UNEP, 2009). Similar large-scale harvesting policies were implemented in several countries as climate adaptation strategies, but no study has investigated the impacts of such policies on birth outcomes. Our results suggest that these policies may bring about positive effects on neonatal health, and thus may have a preemptive targeting component.

This article proceeds as follows. Section 1.2 presents a short background on the semiarid region and the Cisterns Program. Section 1.3 describes the data and our sample. Section 1.4 presents the empirical model. Section 1.5 reports the results. Section 1.6 concludes.

1.2 Background

1.2.1 Semiarid Region

The Brazilian semiarid is the driest region in the country. The semiarid – whose territory corresponds to twice the size of France – is an area prone to irregular rainfall, low water retention by the soil and severe droughts. 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). Figure A1 in the Appendix shows the location of the semiarid region in Brazil.

The semiarid region is officially defined based on indexes of precipitation, aridness and water deficit.[9] The region has 1,262 municipalities, which are mostly small-sized agricultural-oriented jurisdictions.[10] Since the main economic activities are subsistence farming and livestock raising, the semiarid has very low levels of productivity. The semiarid presents worse social indicators – such as poorer health and education outcomes – vis-à-vis other regions in Brazil. In addition, the region has the largest concentration of rural poverty in Latin America.[11]

Water shortage has been identified as the main source of vulnerability for rural families living in Brazil's semiarid (Bobonis et al., 2017). Figure 1.1 shows that in the period 2011-2016 the yearly average rainfall in the semiarid (700 mm) was approximately half of the mean precipitation in the rest of the country (about 1550 mm). Figure A2

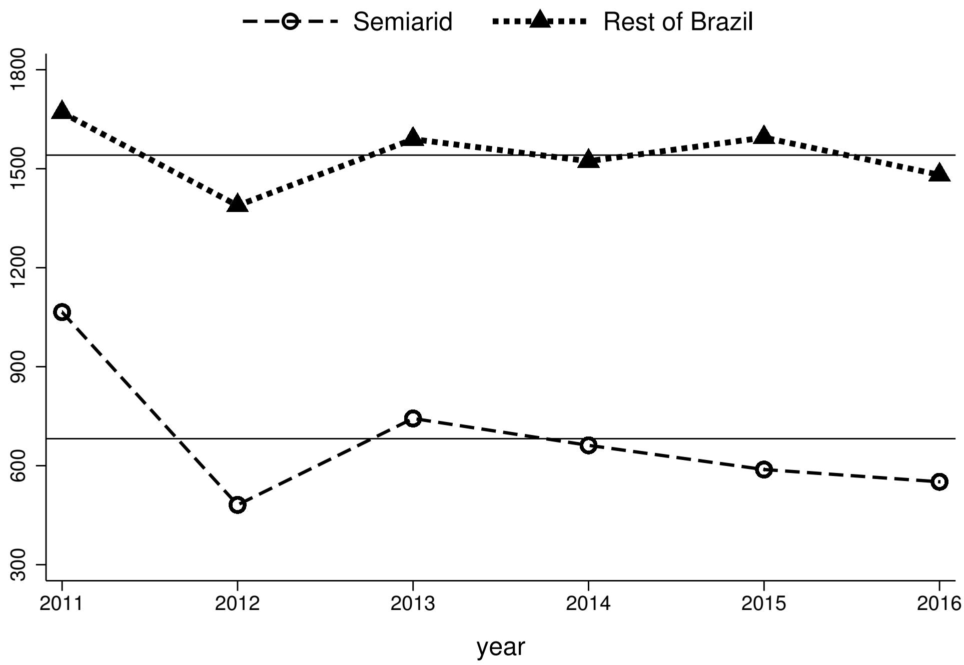
[9]See Da Mata and Resende (2018) for more details on the delimitation of the Brazilian semiarid.

[10]The average population size of a municipality in the semiarid is circa 15,000 inhabitants.

[11]More than half of the Brazilian population living below the poverty line is located in the semiarid region, where over eight million people rely on conditional cash transfers, such as the *Bolsa Família* Program, as their sole source of income (Asa Brasil, 2017). Child mortality rate per thousand live births in the semiarid is twice as large as the level in other areas in Brazil.

in the Appendix shows strong irregularity in monthly rainfall in the semiarid. On top of the unreliable precipitation, high rates of evapotranspiration and the geology of the area make it harder for retaining water (e.g., the rocky, shallow soil of the semiarid has low water retention capacity). In addition, groundwater wells have typically low flow and provide water of high salinity (Cirilo, 2008), so well water is inadequate to meet the region's needs. Approximately 67% of rural households do not have access to the general water supply network (Asa Brasil, 2017). Therefore, water for domestic and non-domestic consumption is usually obtained through rainwater harvesting in ponds and dams. Water-borne diseases caused by land-use and animals such as diarrhea are a continuous threat.

Figure 1.1: Yearly precipitation in the Brazilian Semiarid and in the rest of Brazil, 2011-2016, in mm.



Notes. Municipality averages based on data from the Terrestrial Air Temperature and Terrestrial Precipitation: 1900-2016 Gridded Monthly Time Series, Version 5.01.

Over the years, several public policies have been put in place so as to alleviate water shortage (Passador and Passador, 2010). The first set of policies during the 19th Century and early 20th Century focused on distributing food and building reservoirs, wells, and dams. Later on, planning the rational use of water in the region and irrigation were the priorities. More recently, in 2003, the Cisterns Program – targeting the construction of one million rainwater harvesting systems as described below – was introduced.

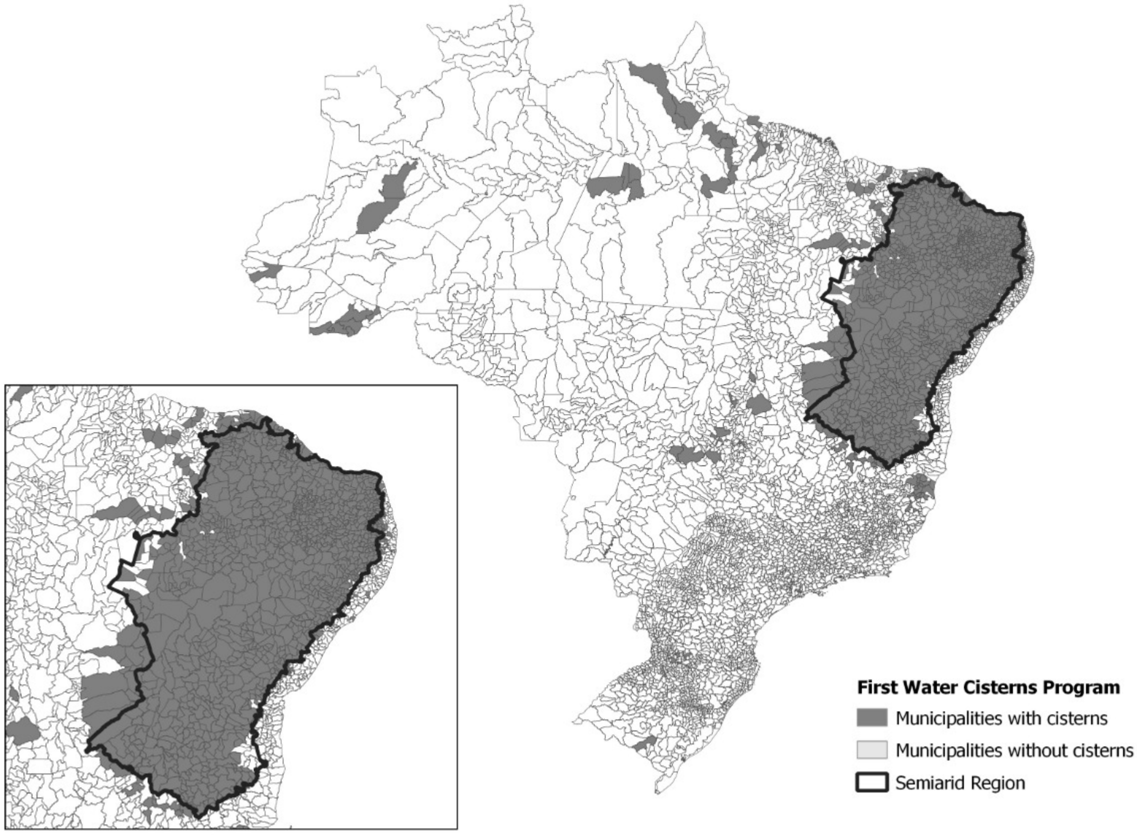
1.2.2 First Water Cisterns Program

The First Water Cisterns Program, also known as the One Million Cisterns Program, aims to provide access to reliable, clean water for households living in rural areas in Brazil. The program builds tanks next to houses to store rainwater harvested from roof catchments (gutters installed on roofs). Figure A3 in the Appendix shows a typical cistern constructed by the program. Each tank has a standard storage capacity of 16,000 liters, which is sufficient for domestic use (drinking and cooking) for a family of up to five members during a dry season. Tanks are built with precast concrete plates — a simple, low-cost technology that is suitable for dry conditions and is easily scalable.[12] Before the construction of cisterns, families regularly relied on alternative sources for obtaining water, such as small ponds and reservoirs, which are often vulnerable to pathogen contamination.

The program focused on providing cisterns to households living in the rural area of semiarid region as shown in Figure 1.2. Almost one million cisterns were built in the Semiarid. The region has experienced in recent years a rapid expansion of the program's roll-out: the number of tanks built more than doubled between 2010 and 2016 (see Figure A4 in the Appendix). This rapid expansion is associated with the large rural population historically facing severe restrictions of water availability and quality.

[12] Similar rainwater tanks have been adopted in several regions of the world (see Zhu et al., 2015). Mintz et al. (2001) argue that "decentralized approaches to making drinking water safe, including point-of-use chemical and solar disinfection, safe water storage, and behavioral change merit far greater priority for rapid implementation." To start providing cisterns in a specific location, the implementers rely on local labor force to build the cisterns.

Figure 1.2: First Water Cisterns Program



Notes. The figure shows municipalities where at least one family received a raintank provided by First Water Cisterns Program during 2003-2016. The figure shows the high concentration of Semiarid municipalities benefited by the program.

To ensure tank water quality, families receive training on point-of-use disinfection (sodium hypochlorite). In addition, families are instructed to remove the gutters during droughts, separate a water bucket to handle exclusively the tank's water, cover the outer walls with lime, and clean the tank yearly using the season's first rain added with bleach (Palmeira, 2006). The program does not promote any refresher training on point-of-use disinfection.

Brazil's Ministry of Social Development (MDS) partners with subnational governments and private non-profit entities (selected through public calls) to execute the program. These partners are responsible for identifying and selecting households based on criteria set by the federal government (Brasil, 2018).[13] To be eligible to participate in the in-kind program, households living in rural areas with no regular access to water must be registered in the "CadÚnico" (Unified Household Registry) from the Federal Government. The selection process gives priority to households with the following characteristics: *i*) low income; *ii*) women-headed; *iii*) large number of children up to six years or schooling-age children; *iv*) households with people with special needs; and *v*) households

[13]The process of finding eligible families is carried out at local meetings involving different entities such as local public authorities, civil society organizations, social assistance councils, among other entities.

with elderly people (Brasil, 2018).[14]

The Cisterns Program does not promote any special training for pregnant women. The program, apart from building rainwater tanks for domestic consumption, has two other components: 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.

1.2.3 Adoption behavior

In more recent years, some works has demonstrated the individual education as a key driver of new technologies effectiveness, particularly in the developing world, either focused on agricultural innovations (Conley and Udry, 2010; Bandiera and Rasul, 2006) or in the context of health technologies (Björkman-Nyqvist, 2013; Dupas, 2014). We try to shed some light on this in our setting using a sample of 1.328 families located in rural areas of the Semiarid region from Embrapa survey from 2005. We estimate the relationship between educational attainment and some outcomes related to the proper use of the rainwater tanks. Our results strongly suggest that families with higher of interviewees make a better management of the tanks, with a higher probability to carry out water treatment. In addition, we found some evidence of lower probability to have fecal coliform in the cisterns, although the small sample for this regression (see Table A1 in the Appendix). These findings are in line with the adoption literature.

1.2.4 Water Harvesting and Birth Outcomes

Cisterns can affect fetal health through multiple pathways. In this subsection, we discuss the main potential channels as well as other mechanisms that are less likely to operate given the characteristics of the intervention. Differently from most papers in the vast literature studying the determinants of birth outcomes, policies for water harvesting may affect fetal outcomes mainly through four mechanisms: quantity of water, quality of water, stress level, and maternal nutrition. We provide more details on the channels in the Appendix A.1.

Quantity of Water. The total body volume of water in a pregnant woman is increased by seven to eight liters by gestational weeks 36-38, with an additional 0.5-1 litter at the 40th week (Hytten, 1980). During pregnancy, there is a rise in water intake and dilution of body fluids (Davison and Lindheimer, 1989), making pregnant women and their fetuses particularly vulnerable to water scarcity. By providing a buffer against rainfall fluctuations, cisterns may increase water availability that is essential during pregnancy.

[14] In this study, elderly people are defined as those aged 65 years and over at the time of construction of the water tank.

Quality of Water. Households receive training on how to use chlorination to ensure water quality. Maintenance of quality is key because cistern water can be as bad as the one from ponds and dams. Even when rainwater is adequate for consumption, animal remains can contaminate the roof used as a catchment. Therefore, families need to check regularly the integrity of roof and tank structures, and disinfection by using chlorine ensures higher water quality. Preventing access by animals into the rainwater tanks is equally important. Increasing water quality affects neonatal health by helping to reduce water-related health problems such as diarrhea, commonly found in our area of study. In our setting, the magnitude of this channel depends on the adherence to training, but take-up rates of water chlorination training can be low. Null et al. (2018) show that adherence to a chlorination training in rural Kenya decreased from 45% one year after the intervention to 25% two year after their trial. Adherence to training can be especially difficult to be achieved at scale in a nationwide program.

Stress level. The recurring risk of water shortage may increase stress levels for pregnant women. Mental health surveys have found significant levels of stress associated with the effects of droughts (Austin et al., 2018). Stress and more severe conditions (such as maternal depression) are relevant factors and have far-reaching consequences for neonatal health and beyond (Persson and Rossin-Slater, 2018). Therefore, if cisterns reduce maternal stress (and antenatal exposure to stress hormone) by reducing the likelihood of water shortage, they will affect fetal health. Policies targeted at the poor can reduce the stress hormone cortisol (e.g., Haushofer and Shapiro (2016) for large amounts of cash-transfer payments). The program's goal to decentralize water harvesting and improve water availability to rurally located households may generate, as a by-product, reductions in the time allocated for searching and transporting water. As a result, pregnant women spend less time fetching water. More time availability may also reduce stress induced by physical hardship.[15]

Maternal Nutrition. Even though maternal nutrition is a common channel in other studies, this channel is less likely to be prominent in our intervention. Small amounts of water are stored to be able to improve rainfed agriculture. The intervention is less likely to stimulate even the growing of small gardens for self-consumption. The amounts of stored water may interfere though with personal hygiene and food cleanliness and thus affecting neonatal health.

Other mechanisms. While it is impossible to rule out all other mechanisms, there are few mechanisms that are unlikely to operate given the characteristics of our intervention. For instance, the intervention is unrelated to improving household sanitation or to changes in healthcare infrastructure. Moreover, although our data do not allow for testing this mechanism, changes in maternal behavior related to drinking and smoking

[15] In more extreme cases, time availability may interfere with labor market outcomes, bringing about income effects and affecting nutrition.

during pregnancy are less likely to be affected.

Time collecting water. According to WHO and UNICEF (2017), families without access to safe water, need to spend at least 30 minutes walking or queuing to collect their water. This task falls disproportionately to women and girls, especially in rural areas. The burden is even heavier for pregnant women or for those carrying small children. A survey of Embrapa (EMBRAPA, 2009) - with 1.328 families located in rural areas of the Semiarid region - shows that 35.2% of the families used to spend more than one hour collecting water from alternatives sources before the rainwater tank implementation. This percentage dropped to 0.2% after the rainwater (see Table A2). This unveils a positive externality impact of the cisterns, a more free time to other activities, such as to visit more doctor, mainly during the pregnancy.

1.3 Data

1.3.1 Data Sources and Sample

We merge three administrative registries of the Brazilian federal government to match the timing of the construction of the rainwater tank with the gestational weeks. This level of detail allows for greater precision in estimating the impact of *in utero* exposure on birth weight. In this section, we describe the main attributes of the data. More details on the dataset are provided in the Appendix A.1.

First, we use the administrative registry of the Cisterns Program, which comprises the complete record of the program's implementation. The data identify each beneficiary – by name, date of birth and id number – and provide a set of socioeconomic characteristics of the family.[16] The registry further includes the exact date of the tank construction, which typically lasts two or three days. Most entries in the registry also include the geographic coordinates of each tank.

Second, we use the national registry of social programs (Unified Household Registry - "CadÚnico") to gather socioeconomic data of the beneficiary and each family 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 education attainment of each family 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 comprises data only for live births and gives us our

[16]The registry provides the beneficiaries' CPF ("Cadastro de Pessoa Física") and NIS ("Número de Identificação Social"). NIS is a registration number assigned by the "Caixa Economica Federal" to beneficiaries of social assistance programs that do not have PIS (Social integration program) registration.

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.[17] The registry provides additional variables on (i) the newborn such as the APGAR score[18], (ii) the pregnancy (e.g., gestation duration in weeks and number of prenatal appointments) and (iii) the birth delivery such as natural or cesarean childbirth, hospital or other health facilities, multiple birth, etc. The additional data on the newborn, pregnancy and delivery is used either as secondary outcomes or for the heterogeneity analyses.

Our period of analysis is 2011–2017 and was chosen due to data constraints — it is the only period where we are able to match the three administrative registries and work with three key dates to build our final dataset: the construction date, the conception date and the birth date. The period of 2011–2017 coincides with a rapid expansion of the program, which facilitates finding pregnant women receiving cisterns during different gestational weeks.

To merge the three registries, we proceeded as follows. We found each Cisterns Program's beneficiary in CadÚnico by using the national id number. CadÚnico then provides a direct link between the beneficiary and the family members. Next, we selected the beneficiaries and family members who gave birth during 2011-2017 and then trimmed the dataset further to select only those whose cistern's construction date laid within the gestational period (between the conception date and the expected birth date). We are able to find beneficiaries and family members in the birth registry by using four characteristics: (i) the newborn's birth date, (ii) the newborn's sex, (iii) the mother's birth date and (iv) the municipality of residence of the mother.

Notice that the tank delivery may affect the duration of the pregnancy. We use the fact that our data allow us to set well-defined start and end points of the relevant events to counteract the potential influence of cisterns in pregnancy duration. To add individuals in our sample, 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 the interval of 280 days after the date of conception is included in our final sample. Put differently, our sample P is given by:

$$P = \{i : c \leq Cisterns \leq b_{exp}\} = \{i : c \leq Cisterns \leq c + 280\},$$

where i is the mother-child pair, c is the child's date of conception and b_{exp} is the expected date of birth, which is equivalent to $b_{exp} = c + 280$. Intuitively, by setting the endpoint of 280 days after conception, we let the gestation length to be unrelated to the potential

[17]The date of the last menstrual period is based on medical records. This measure is widely used in the medical literature to calculate gestational time. See, for example, Papageorghiou et al. (2014).

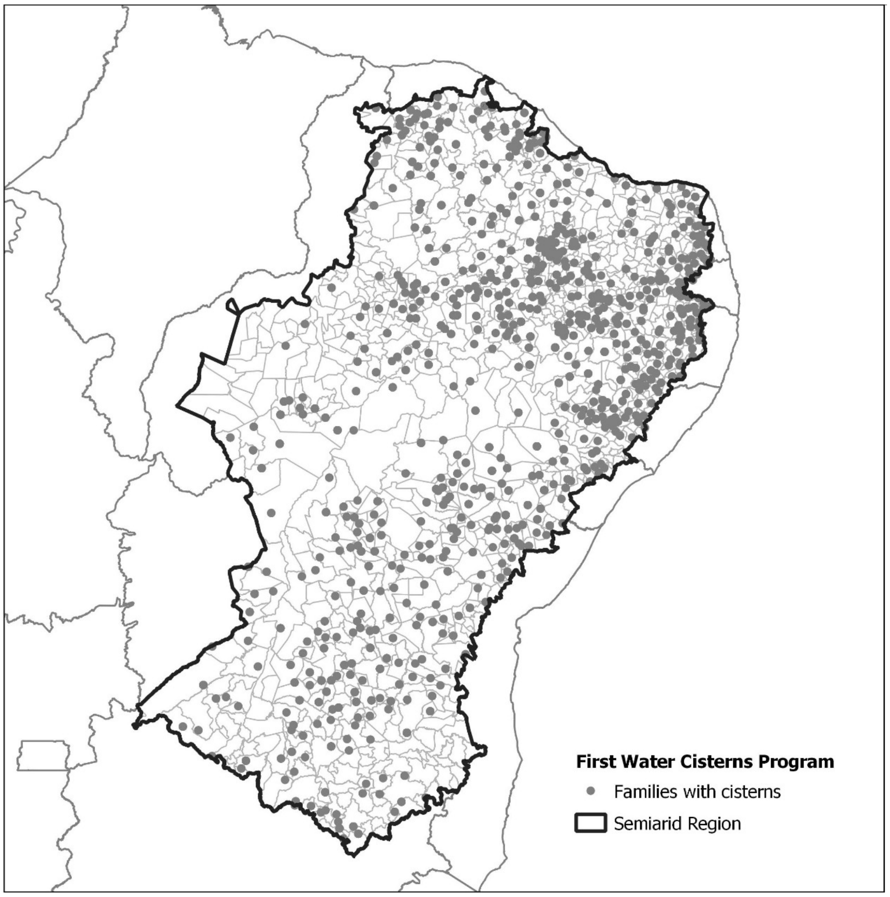
[18]The APGAR score measures the vital signs of the newborn in the immediate extra-uterine life. It consists of five criteria, each with scores of up to 2 points: appearance, pulse, grimace, activity, respiration. The birth registry provides the APGAR score at 1 minute and at 5 minutes.

influence of cisterns.[19]

To be included in our sample, birth $\in [c, b_{exp}]$. In other words, individuals born after their expected date of birth are out of our sample. If access to cisterns is associated with longer lasting pregnancies, our sample would not include women receiving cisterns in early stages of their pregnancy and delivering after the 40th week. For the robustness exercises, we work with samples of different intervals (41, 42, 43 and 44 weeks after conception) as explained in Subsection 1.5.2.

Our final sample excluded multiple pregnancies and those individuals who have benefited from policies that could be regarded as confounders.[20] We also excluded families who have benefited simultaneously from the First Water and the Second Water Cisterns Programs. Figure 1.3 shows the location of the individuals of our final sample.

Figure 1.3: Location of the Individuals of our Sample



Notes. The figure shows the location of each of the 4,208 individuals of our full sample.

[19]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). Figure A5 in the Appendix illustrates two different cases of pregnancy that were included in our sample.

[20]For instance, we were granted access to microdata on beneficiaries of the program "Água para Todos", from the Brazilian Ministry of Integration. Because of the small scale of this program and the absence of clear criteria for the selection of beneficiaries, we dropped the sample of all families served by this program as a way to eliminate this confounding policy.

1.3.2 Descriptive Statistics

Our final sample has 4,208 observations. Table 1.1 presents selected characteristics of our sample of pregnant women from the SINASC birth registry. The table also shows how our sample compares to all births in the Semiarid and Brazil using data from 2011 to 2017. The relevant characteristics are our main outcome variables: birth weight, fetal growth (defined as the birth weight divided by the number of weeks of gestation) and an indicator for low-birth-weight. The World Health Organization (WHO) defines low-birth-weight as weight under 2,500g. Table 1.1 also presents the following additional variables from the birth registry used in our analysis.

Table 1.1: Summary statistics of our sample and all live births in the semiarid region and Brazil

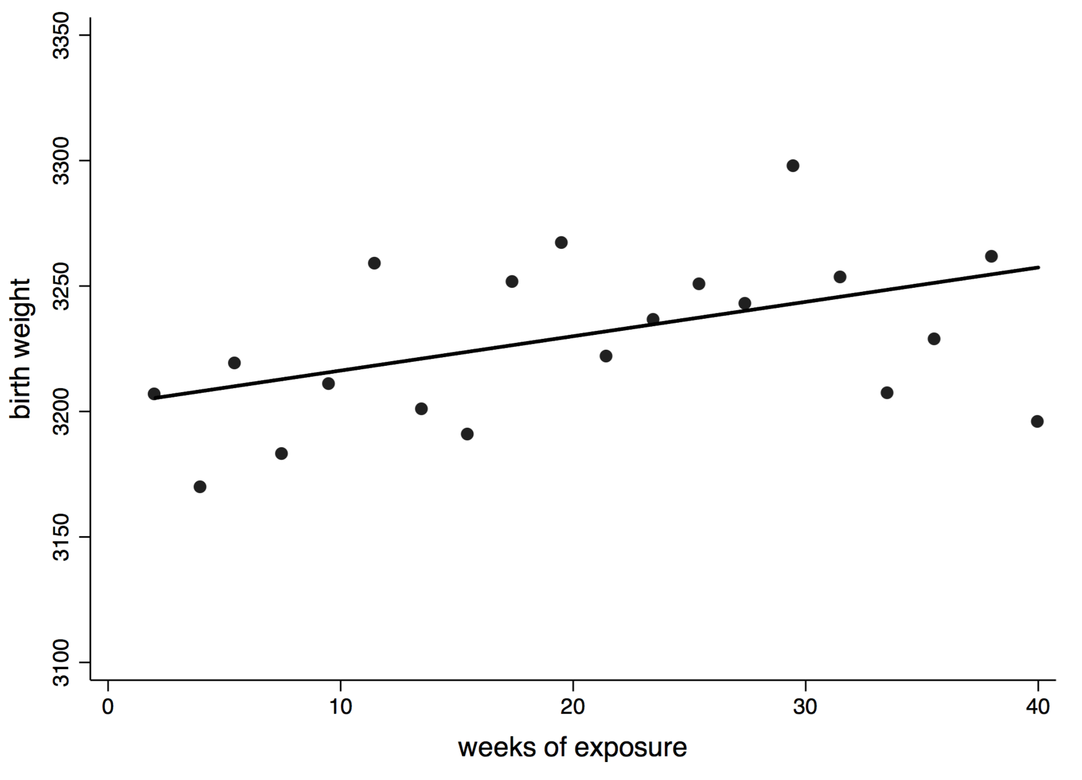
Variables	Our sample	Semiarid all births	Brazil all births
	(I)	(II)	(III)
Birth weight (g)	3,230.4	3,227.8	3,201.0***
	(512.4)	(544.5)	(539.8)
Low birth weight	0.0630	0.0721**	0.0764***
	(0.243)	(0.259)	(0.266)
Fetal growth	84.47	82.89***	82.53***
	(13.45)	(24.79)	(17.68)
Apgar 1 min	8.090	8.150**	8.334***
	(1.158)	(1.182)	(1.215)
Weeks of gestation	37.84	38.69***	38.54***
	(1.862)	(2.352)	(2.203)
Weeks of gestation < 37	0.155	0.112***	0.107***
	(0.362)	(0.315)	(0.309)
Mother's age	27.37	25.40***	26.13***
	(5.935)	(6.600)	(6.641)
Mother' s education	3.330	3.631***	3.879***
	(0.815)	(0.802)	(0.754)
Number of births	1.623	1.162***	1.035***
	(1.570)	(1.451)	(1.326)
Observations	4,208	2,303,427	20,042,851

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).

The rules of the Cisterns program explain most of the differences when we compare our sample to all births in the Semiarid and Brazil. Table 1.1 shows that our sample has older, less educated mothers, whose pregnancies were shorter in duration. The children present lower APGAR scores, but slightly higher birth weight. Our sample has a lower prevalence of low birth weight in newborns. Pregnant women in our sample had more births during 2011-2017 because the program prioritizes households with children up to six years or schooling-age children.

Figure 1.4 presents a positive correlation between the number of weeks of exposure to cisterns during pregnancy and birth weight. In the next section, we detail our empirical strategy to assess the causal impacts of intrauterine exposure to cisterns as well as heterogeneous effects.

Figure 1.4: 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.

1.4 Empirical Strategy

We aim to estimate the effects of prenatal exposure to cisterns on birth outcomes. A key empirical challenge in estimating the causal effect of exposure relates to the endogenous nature of the program. Cisterns were not randomly allocated to pregnant women to create groups of exposed and unexposed individuals. It might be that unobserved family/mother characteristics that may influence the probability of being selected for program participation may also determine birth outcomes, leading to spurious associations between time of exposure and neonatal health.

To overcome this problem, our empirical strategy uses variation in *when* and *where* a pregnant woman received a cistern from the program. We use the rollout of the Cisterns Program across municipalities in Brazil to compare women located in the same municipality whose cisterns were built in different weeks in their gestational period. Municipalities entry (endogenously) into the program and then groups of people living in districts within each municipality are selected by the program. Our identification assumption requires that once a group of people is selected, pregnant women receive the cisterns independent of the exact timing of the pregnancy.

To estimate the effects of the program, we use the following specification:

$$Y_{imts} = \mu_s + \gamma_{mt} + \beta \cdot \mathbf{weeks\_exposure}_{imts} + \mathbf{X}'_{imts}\Theta + \varepsilon_{imts} \quad , \tag{1.1}$$

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_exposure**, which measures the difference in weeks between the expected date of birth and the cistern's date of construction. The municipality fixed effect μ_s control non-parametrically for municipality unobserved fixed determinants of birth outcomes, while the inclusion of month-year fixed effects γ_{mt} adjust non-parametrically for shocks that are common to all pregnant women at a specific moment in time. Standard errors ε_{imts} are clustered at the municipal level.

The vector $\mathbf{X}_{imts}$ in Equation (1.1) includes the following set of baseline characteristics potentially correlated with birth outcomes. First, variables related to the priority criteria set by the government: an indicator for woman-headed family, number of elderly people in the family, number of children in the family, number of teenagers in the family, number of people with special needs in the family, and *per capita* family income. Second, control variables related to characteristics of the mother: mother's age and an indicator for illiterate mother. Finally, indicator variables for the delivery: an indicator for hospital birth and an indicator for the newborn sex.[21]

Alternative specification. We also analyze pregnancy trimester-specific impacts using the following specification:

$$Y_{imts} = \mu_s + \gamma_{mt} + \alpha \cdot \mathbf{trim1}_{imts} + \delta \cdot \mathbf{trim2}_{imts} + \mathbf{X}'_{imts}\Theta + \varepsilon_{imts} \quad , \tag{1.2}$$

Equation (1.2) differs from the main specification by creating indicator variables captur-

[21]In the robustness exercise, we also add the following indicator variables for the housing structure: electricity, bathroom, water treatment, asbestos roof, thatched roof, ceramic roof tiles and other types of roofs.

ing the construction of cisterns in the expected first ($\mathbf{trim1}_{imts}$) and second ($\mathbf{trim2}_{imts}$) trimesters of pregnancy relative to the expected third trimester of pregnancy.[22] Notice that Y_{imts}, μ_s, γ_{mt}, X_{imts} and ε_{imts} are as above in the main specification. In this alternative specification, the identification assumption is similar to the one in Equation (1.1). The alternative specification aims to verify whether there are differential impacts associated with receiving a cistern in the early stages in the pregnancy.

Assessing the research design. Our primary identifying assumption is that 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 be then unrelated to our outcomes of interest. Under the validity of this assumption, we can interpret β in Equation (1.1) 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 uncorrelated with individual characteristics.

We start by providing some evidence on whether observable characteristics of pregnant women who received cisterns in distinct gestational periods are similar. Table 1.2 presents the correlation between **weeks_exposure** and observable characteristics of the pregnant women. There is little evidence for significant differences between mother characteristics and the timing of the construction of the cistern. The coefficients are small and not significantly different from zero. This suggests that much of the variation in the exact timing of the implementation of cisterns appear to be idiosyncratic.

[22] $trim1_{imts}$ 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.

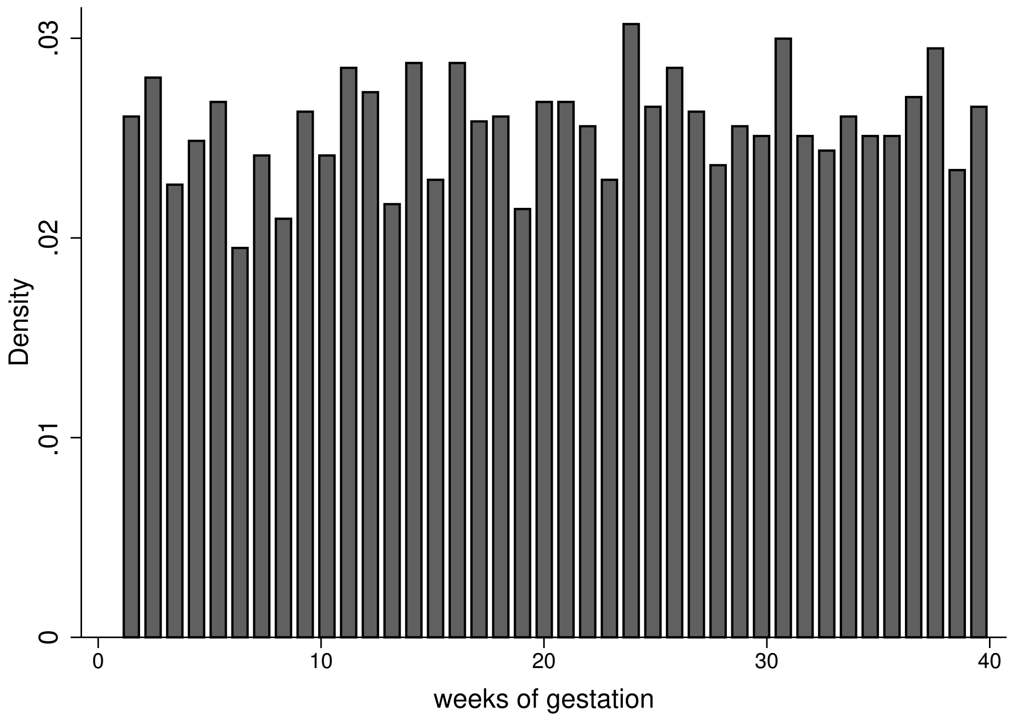
Table 1.2: Weeks of Exposure to Cisterns and Control Variables (Balance Test)

	(I)	(II)	(III)	(IV)	(V)
	Mean	Univariate OLS		FE & controls	
		coef.	*R2*	*coef.*	*R2*
Panel (A): Baseline control variables					
Woman-headed family	0.913	0.0002	0.0001	0.0004	0.2237
	(0.281)	(0.0004)		(0.0004)	
# elderly in the family	0.0133	0.0002	0.0003	0.0003	0.1613
	(0.137)	(0.0002)		(0.0002)	
# children in the family	0.759	-0.0017	0.0006	-0.0020	0.2162
	(0.824)	(0.0011)		(0.0012)	
# teenagers in the family	0.735	0.0018	0.0004	0.0017	0.4227
	(1.079)	(0.0014)		(0.0014)	
# people with special needs	0.0366	-0.0002	0.0002	-0.0004	0.2299
	(0.204)	(0.0003)		(0.0003)	
Per capita family income	37.49	-0.0806	0.0002	-0.0522	0.2727
	(58.87)	(0.0726)		(0.0758)	
Mother's age	27.53	-0.0023	0.0000	-0.0029	0.4368
	(5.922)	(0.0082)		(0.0076)	
Illiterate mother	0.0694	0.0001	0.0000	-0.0000	0.2077
	(0.254)	(0.0003)		(0.0004)	
Born in hospital	0.970	0.0002	0.0001	0.0001	0.3326
	(0.171)	(0.0002)		(0.0002)	
Female newborn	0.481	0.0005	0.0001	0.0004	0.1497
	(0.500)	(0.0007)		(0.0008)	
Panel (B): Controls in the robustness					
Electricity	0.923	0.0001	0.0000	0.0003	0.3506
	(0.266)	(0.0003)		(0.0004)	
Bathroom	0.711	0.0004	0.0001	0.0001	0.3709
	(0.452)	(0.0007)		(0.0007)	
Water treatment	0.843	-0.0010*	0.0009	-0.0005	0.3969
	(0.363)	(0.0005)		(0.0006)	
Asbestos roof	0.0091	0.0000	0.0000	0.0000	0.2750
	(0.0947)	(0.0001)		(0.0001)	
Thatched roof	0.869	-0.0004	0.0001	-0.0003	0.3829
	(0.338)	(0.0005)		(0.0005)	
Ceramic roof tiles	0.120	0.0003	0.0001	0.0002	0.3976
	(0.325)	(0.0004)		(0.0004)	
Other types of roof	0.0020	0.0000	0.0000	-0.0000	0.1693
	(0.0448)	(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 controls 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

We also examine the histogram of the frequency of cisterns distributed according to each week of gestation. If the salience of the pregnancy is an important factor influencing the distribution of cisterns, there must be a positive relationship between advancing in the pregnancy and number of women receiving cisterns. Besides, there is also a mechanical factor to may generate a positive relationship since the longer the pregnancy, the more likely to receive a cistern. However, Figure 1.5 depicts no such positive relationship.

Figure 1.5: 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.

Further, we check for the Stable Unit Treatment Value Assumption (SUTVA) between the "treatment" and "control" groups. Because Equation (1.1) compares individuals who receive a cistern in a given municipality in different weeks of their gestational period, spillovers from early-treated pregnant women to late-treated may pose a threat to our identification. One possible spillover happens when an early treated pregnant provides water for an unexposed pregnant until the latter receives the cistern. When we compare pregnant women who received a cistern in a given municipality *in the same year*, contamination is likely to be minimized. To formally implement this comparison, we substitute the municipal fixed-effect for municipal-year of construction fixed-effect in Equation (1.1). The results are qualitatively unchanged, which alleviates concerns of (strong) spillovers.[23]

[23]We show the results with the municipal-year of construction fixed effect in a robustness check in Subsection 1.5.2.

We carry out further plausibility tests by focusing on behavioral responses. We look at three possible behavioral responses: (i) outcomes of previous pregnancies looking at older siblings, (ii) behavior during the early stages of the pregnancy as measured by prenatal appointments and (iii) participation in other social assistance programs.

To match siblings born to the same mother, we use the family identifier of the CadÚnico registry.[24] By comparing pregnancies of the same mother where none of the older siblings had cisterns *in utero*, we are essentially performing a placebo exercise. We estimate Equation (1.1) keeping the corresponding **weeks_exposure** for each woman, but now the left-hand side outcome variable is the birth weight in grams of the *unexposed* older sibling. Even though we do not find older siblings for all 4,208 children of our sample, we are able to find 2,521 older siblings to test whether their birth weight differs. We have a density of mother-child pair to perform this exercise because women-headed families with children have the priority to receive the in-kind transfer from the Cisterns Program (recall Section 1.2.2). The results in columns (I) and (II) of Table 1.3 show that there is no statistically difference between unexposed older siblings when it comes to birth weight.

Table 1.3: Older Siblings

	(I)	(II)	(III)	(IV)
Dependent Variable:	Birth Weight Older Sibling		Birth Weight Treated Sibling	
weeks_exposure	0.575	0.695	1.907*	1.824*
	(1.063)	(1.056)	(1.069)	(1.064)
Month-year fixed effects	Yes	Yes	Yes	Yes
Municipality fixed effects	Yes	Yes	Yes	Yes
Controls	-	Yes	-	Yes
Observations	2,521	2,521	2,521	2,521

Notes. This table shows the results of estimating Equation (1.1). 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 illiterate mother, indicator for hospital birth, indicator for newborn sex.
*** p<0.01, ** p<0.05, * p<0.1

We also test whether the subgroup of treated (exposed) siblings of those unexposed older siblings differ in birth weight. We use Equation (1.1) for the subsample of exposed siblings that we were able to find older siblings. The results point out for higher point estimates, statistically significant at 10 percent (see columns (III) and (IV) of Table 1.3). In summary, the results using the older siblings support the plausibility of the identifying assumption.

[24]The details of the matching steps are provided in the Appendix A.1.

As for responses regarding welfare programs, we track our sample of individuals in the microdata of the "Bolsa Família" program to assess whether the women who received cisterns earlier in their pregnancy are more likely to be pro-active when it comes to receiving government programs. Recall that cistern is an in-kind transfer, while the nationwide program Bolsa Família is a cash-transfer program. We ask the following question: Do pregnant women who have received cisterns in the early stages of their pregnancy are more likely to apply for Bolsa Família? We investigated this possibility by using microdata on Bolsa Familia payroll List for our sample of pregnant women in 2011. Table A3 in the Appendix indicates insignificant variation in the likelihood of being benefited by BFP and time of exposure to cisterns.

In addition, we check for differential access to healthcare facilities. We use municipal-level data on the number of hospitals, hospital beds, neonatal beds, and ultrasound machines (all measured per 1,000 inhabitants). We want to check if women who have benefited from the program in the early stages of pregnancy had differential access to health services. For this exercise, we modify the baseline specification – as it originally compares pregnant women within the same municipality – and estimate Equation 1.1 without the municipal fixed effects μ_s. The results in Table A4 in the Appendix show no differential access between early treated and late treated pregnant women. Table A4 also shows whether the timing of exposure to cisterns is also associated with access to units and personnel of Brazil's Family Health Program.[25] We find again no differential access. This alleviates further concerns over the interference of an important health policy in Brazil.[26]

Notice that we cannot comment on selection in our sample regarding surviving children, because the administrative registry comprises data only for live births, and thus do not have information on miscarriage and stillbirths.

1.5 Results

1.5.1 Baseline results

Table 1.4 presents the baseline results on birth outcomes. In all specifications, we estimate Equation (1.1) controlling for an indicator woman-headed family, number of elderly people in the family, number of children in the family, number of teenagers in the family, number of people with special needs in the family, *per capita* family income, mother's age, an indicator for illiterate mother, an indicator for hospital birth and an indicator for the newborn sex.

[25] The Family Health Program provides free primary health care in poor areas through multidisciplinary health-care teams that are responsible for covering a set of few thousand households.

[26] A similar exercise for two large government assistance programs – Bolsa Família and the Continuous Cash Benefits (BPC) – shows no evidence that the timing of exposure to cisterns is associated with living in municipalities with more people receiving government assistance (see Table A5 in the Appendix).

Table 1.4: Effects of Exposure to Rainwater Tanks on Birth Outcomes

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)	(IX)	(X)
	Birth Weight		ln Birth Weight		Fetal Growth Rate		Low Birth Weight		Weeks of Gestation	
weeks_exposure	1.562**	1.741**	0.001**	0.001**	0.035*	0.042**	-0.001*	-0.001*	0.003	0.002
	(0.756)	(0.754)	(0.000)	(0.000)	(0.020)	(0.020)	(0.000)	(0.000)	(0.003)	(0.003)
Month-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	-	Yes	-	Yes	No	Yes	-	Yes	-	Yes
Observations	4,057	4,054	4,057	4,054	4,057	4,054	4,057	4,054	4,059	4,056

Notes. This table shows the results of estimating Equation (1.1). 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 illiterate mother, indicator for hospital birth, indicator for newborn sex.
*** p<0.01, ** p<0.05, * p<0.1

We find differences across the gestational age at exposure. Columns (I) and (II) of Table 1.4 show that each additional week of intrauterine exposure to cisterns is associated with a positive effect on average birth weight of 1.5–1.7 gram. Columns (III) and (IV) show the result for birth weight expressed as log and support the claim that there are significant differences across the gestational age at exposure. The increase in birth weight occurs due to 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 (see columns (V) and (VI)). We find a small impact on the likelihood of low birth weight (in columns (VII) and (VIII)). This result may be a consequence of the low prevalence of low birth weight newborns (≤2,500 grams) in our sample, as previously discussed. In columns (IX) and (X), we find no statistically significant effects on gestation length.

Taken together, our results suggest that early exposure to cisterns increase birth weight through positive fetal growth. The increase in fetal growth – which implies a reduction in a potential adverse perinatal outcome – was not associated with longer gestation periods as we did not find impacts on the average duration of the pregnancy.

To gain additional insights, we now document how the effects on birth weight vary by selected characteristics of the mothers and the newborns known in the literature to be associated with neonatal health, namely: mother's age, educational attainment and marital status, and the newborn's sex.[27] Notice that power is lower in the following analysis since there will be more singletons observations given the municipal fixed effects in Equation (1.1). The results in Table 1.5 aim to shed more light on the mechanisms through which cisterns affect birth weight. Columns (I) and (II) report the results for mothers above and below 24 years old at the time of the birth delivery. Earlier exposure to rainwater tanks during pregnancy is associated with stronger effects for older women in our sample.

[27] There is some evidence in the medical literature that boys are more vulnerable to negative shocks during the gestational period than girls because of lower reserve capacity, increasing the risk of malnutrition. However, boys have more efficient placentas, which may boost the effects of positive shocks (see, for example, Eriksson et al. (2010)).

Table 1.5: Results by Characteristics of Mothers and Newborns

	Dependent variable: Birth Weight (g)							
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
	Mother's age		Illiterate mother		Marital status		Newborn sex	
	> 24 yrs	< 24 yrs	No	Yes	Married	Other	Female	Male
weeks_exposure	2.124*	1.391	2.255**	-5.503	2.072	1.143	0.523	1.799
	(1.108)	(2.072)	(0.996)	(4.588)	(1.523)	(1.305)	(1.476)	(1.308)
Month-year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,895	1,308	3,445	599	1,683	2,443	2,024	2,179

Notes. This table shows the results of estimating Equation (1.1). 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 illiterate mother, indicator for hospital birth, indicator for newborn sex.
*** p<0.01, ** p<0.05, * p<0.1

We also check if the effects of earlier prenatal exposure to cisterns are stronger for children of mothers who have higher educational attainment. As adoption of chlorine disinfection and better use of cisterns may be correlated with educational attainment, stronger results for mothers with more education may be an indirect assessment of the role of adoption. The administrative registries have information on education that allows us to create two categories: (i) mothers with up to three years of formal education and (ii) mothers with more than three years. We consider the individuals of the first group as illiterate or with functional illiteracy. The results in Table 1.5 (columns (III) and (IV)) suggest that more educated mothers benefit from the exposure. The estimated impact for the educated subgroup is 2.25 grams per additional week of exposure. By contrast, the results suggest that less educated mothers seem to present worse birth outcomes: The coefficient of birth weight is negative, but is not precisely estimated due to lower power.[28]

The results of Table 1.5 thus far indicate that older, more educated mothers benefit from earlier exposure to the policy. More educated women may be more likely to use cisterns in accordance with the training promoted by the program. To provide additional evidence we exploit a different microdata from the Cisterns Program that provides information on the maintenance of cisterns as well as socioeconomic information for roughly 1,300 beneficiaries. Even though the sample is distinct from our sample of pregnant women, the new data show a positive correlation between the use of chlorine and educational attainment. This result is in line with Currie (2011), who point out that more educated mothers are more likely to respond or compensate for adverse conditions. According to Currie (2011), children of more educated mothers are less likely to be exposed

[28]Moreover, in Table A6, we repeat this exercise by examining the influence of mother educational attainment on the following outcomes: prenatal visits; fetal growth ratio; and Weeks of gestation. The results suggest a larger effect of mother educational attainment on fetal growth ratio.

to pollution *in utero*. This provides additional insights into how rainwater programs can affect birth outcomes: How mothers respond or compensate may dictate the success of a program in improving neonatal health.

When we split the sample according to the marital status in columns (V) and (VI), we find stronger (but not statistically significant) effects for married individuals. The coefficient of weeks of exposure on birth weight for non-married women is virtually half of the one for married women. The disaggregated results for the newborn sex are shown in columns (VII) and (VIII). The results show a higher point estimate for males compared to females, even though none is statistically significant at 10 percent.[29]

We also test for the effects on additional outcomes from the birth registry. According to Table 1.6, the likelihood of a low 1-minute or 5-minute APGAR is unaffected by the varying antenatal exposure. The type of delivery (cesarean section or normal) does not vary either with respect to the timing of the cistern construction. In column (III), the dependent variable equals one if the delivery was via cesarean section. Table 1.6 further disaggregates the data for the prevalence of the newborn sex, which we expect to be unaffected by earlier *in utero* exposure to cisterns, bar any extreme behavioral responses. In column (IV), the dependent variable equals one if the newborn sex is female. The result points out that the timing of exposure does not interfere with the prevalence of female newborns in our sample.

Table 1.6: Additional Birth Outcomes

	(I)	(II)	(III)	(IV)	(V)	(VI)
Dependent variables:	Apgar 1 min	Apgar 5 min	Cesarean Delivery	Newborn Female	Prenatal visits	Weeks of gestation < 37
weeks_exposure	-0.001	-0.000	0.001	-0.000	0.001*	-0.001
	(0.002)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
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	3,806	3,809	4,052	4,056	4,044	4,056

Notes. This table shows the results of estimating Equation (1.1). 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 illiterate mother, indicator for hospital birth, indicator for newborn sex.
*** p<0.01, ** p<0.05, * p<0.1

We find a small, borderline significant increase in the number of prenatal visits. In column (V), the dependent variable equals one if the number of prenatal appointments during the pregnancy was greater or equal to seven. There is a statistically significant

[29] We also conduct further analysis regarding birth weight categories in Table A7. We find that the effects are concentrated in infants with birth weight between 2500-3000 grams and 3500-4000 grams.

increase, but the estimated impact is of only 0.1%. Even though the access to healthcare facilities be thought of as one potential mechanism, the small coefficient does not allow us to conclude that changing response toward increasing basic healthcare services is the relevant mechanism to explain the increases in birth weight.

We found previously that *in utero* exposure to cisterns is not associated with an increase in the average duration of the pregnancy. We now check for the impact on the prevalence of pre-term birth (defines as less than 37 weeks gestation). Recall that our sample in Table 1.1 has a higher prevalence of pre-term birth compare to all births in the Semiarid or Brazil. In column (VI) of Table 1.6, we create an indicator for pre-term birth. Earlier prenatal exposure is not associated with a lower prevalence of pre-term birth.

1.5.2 Robustness checks

In this subsection, we perform several robustness exercises. Table 1.7 shows that the results for average birth weight are qualitatively unchanged when we use other control variables as well as different fixed effects. Average birth weight growth per week of exposure varies between 1.68–2.47 grams depending on the specification, while our baseline point estimate was 1.74 gram, close to the lower bound of the range of the robustness results. The results suggest that our baseline estimates may show conservative effects of early exposure to the rain harvesting policy.

Table 1.7: Robustness: Additional Controls and Fixed Effects

	Dependent variable: Birth Weight (g)				
	(I)	(II)	(III)	(IV)	(V)
weeks_exposure	2.2639**	1.6840**	1.7109**	1.6940*	2.4731*
	(1.0950)	(0.7880)	(0.7549)	(0.8659)	(1.3332)
Month-year fixed effect	Yes	Yes	Yes	Yes	Yes
Municipality fixed effect	Yes	Yes	Yes	Yes	-
Controls	Yes	Yes	Yes	Yes	Yes
Municipality charact. by year-of-construction	Yes	-	-	-	-
Housing structure	-	Yes	-	-	-
State linear trend	-	-	Yes	-	-
Municipality linear trend	-	-	-	Yes	-
Municipality by year-of-construction fixed effect	-	-	-	-	Yes
Observations	4,050	3,584	4,054	4,054	3,518

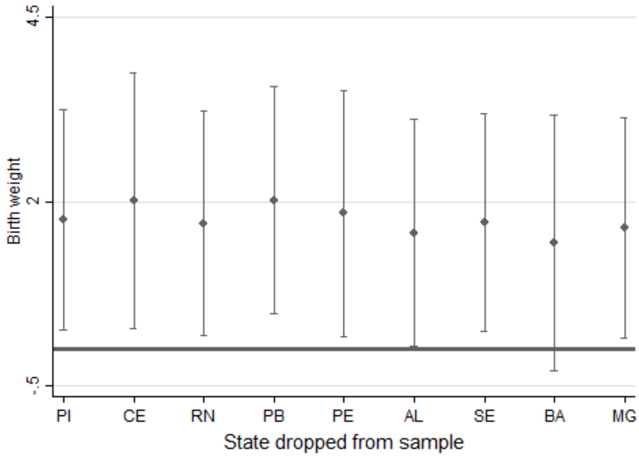
Notes. This table shows the results of estimating Equation (1.1). Standard errors clustered at the municipal level. Control variables in all columns are: an indicator woman-headed family, # elderly, # children, # teenagers, # people with special needs, per capita income; mother's age, indicator for illiterate 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 1.7 reports the following modifications in Equation (1.1): In column (I) we control for municipality-level characteristics. In columns (II) we control for housing structure characteristics. In column (III) we add state specific trends, while in column (IV) we include municipality specific trends. Finally, in column (V) of Table 1.7 we substitute in Equation (1.1) the municipal fixed-effect for a municipal-year of construction fixed-effect. The idea is to compare pregnant women who lived in the same municipality and received cisterns in the same year (but in different moments of their pregnancy). We carry out this exercise in column (V) to formally implement a test for checking the SUTVA assumption, as discussed previously in Section 1.4. We show that the results are robust to the inclusion of municipal-year of construction fixed-effect and thus there is no evidence of (strong) spillovers between early-exposed and late-exposed individuals.

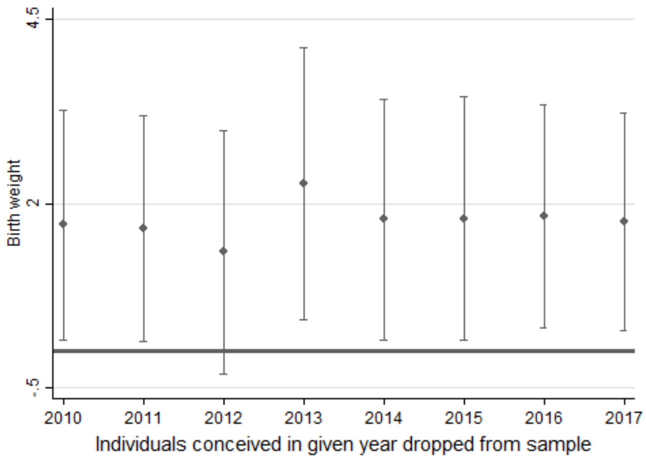
Our baseline results are also robust to dropping regions where the program was implement as well as birth cohorts. By and large, Figure 1.6 shows similarly sized point estimates after excluding subsets of our sample (one at a time) from the analysis. More specifically, Panel (A) in Figure 1.6 reports that the results are robust when we drop (one at a time) each of the nine states of the Semiarid region. Panel (B) presents that estimates do not change much when we drop birth cohorts one at a time. This exercise is important because the Semiarid suffered from a severe drought in 2012-2016 and thus this robustness check points out that the results are not driven by particular cohorts. Finally, Panel (C) reports that coefficients are unchanged when dropping (one at a time) each of the ten largest municipalities in our sample. This set of robustness exercises indicates that our results are not driven by particular regions or birth cohorts.

Figure 1.6: Robustness: Dropping subsamples

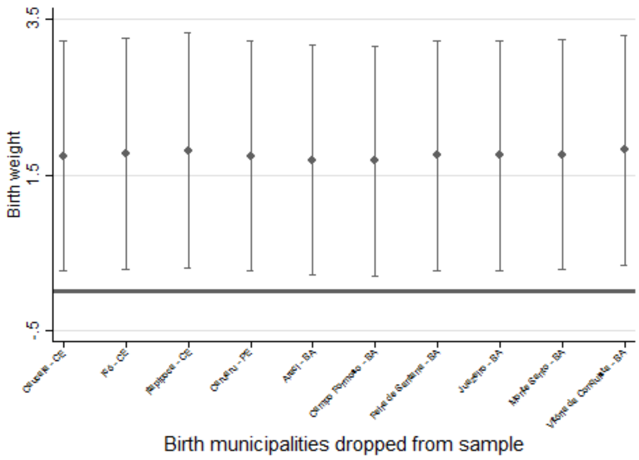
(a) Dropping states



(b) Dropping conception years



(c) Dropping 10 largest municipalities by rural population

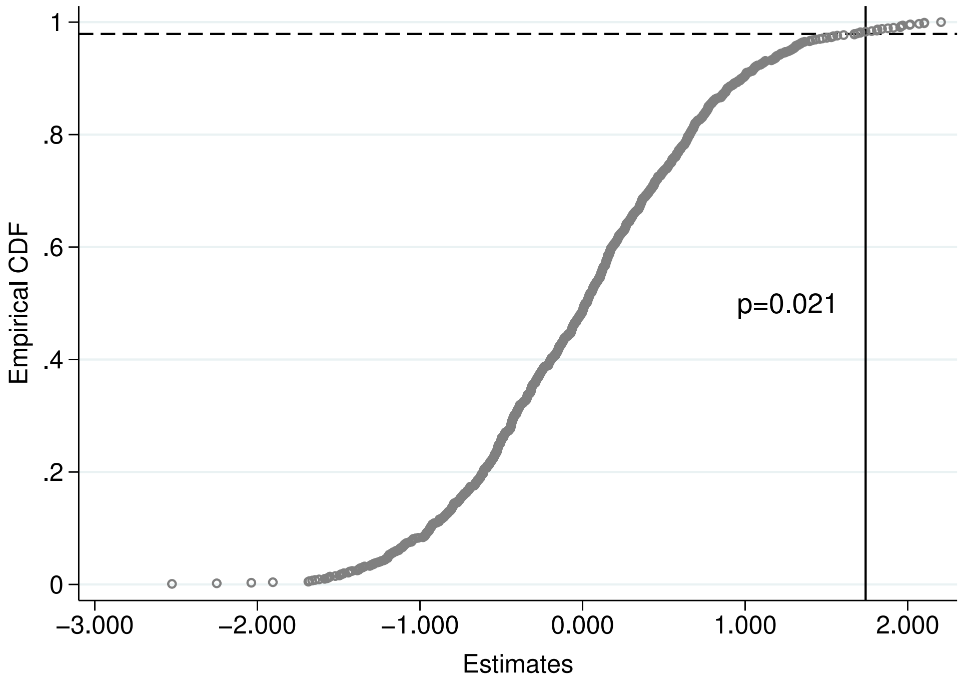


Notes. This figure plots point estimates for the effect of in utero exposure to rainwater program, denoted by square markers, and corresponding 95-percent confidence intervals represented by lines, as different subsets of observations are excluded from the sample. Each estimate is obtained from a separate regression estimating Equation (1.1) 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 1.6a); conception years (Panel 1.6b) and the 10 largest municipalities based on rural population (Panel 1.6c).

In Tables A8, A9 and A10 in the Appendix A.1, we perform additional checks. Table A8 shows that the positive impact of early exposure to the policy on birth weight remain after trimming the tails of our sample. By trimming the sample, the coefficients are smaller ranging from 1.28–1.64 gram, but they all remain statistically significant. Table A9 reports the results after increasing the pregnancy intervals to include individuals in our sample. Recall that our baseline analysis uses an interval of 40 weeks (280 days), which corresponds to the difference between the date of conception and the expected date of birth. In Table A9, we expand the intervals for 41, 42, 43 and 44 weeks after the date of conception. The coefficients for weeks of exposure on birth weight remain statistically significant and range between 1.43–1.67 gram. In Table A10, we test whether the results are robust to alternative clustering of standard errors. Significance of results is highly robust to alternative clustering such as two-way clustering (quarter of year and municipality) and spatial clustering using Conley (1999)'s approach.

To further check for robustness, we perform a placebo exercise by randomly assigning *in utero* exposure to the program to individuals in our sample. More specifically, we perform a permutation test by creating a random "weeks of prenatal exposure" to cisterns (between 1 and 40 weeks) for each one of the pregnant women, and then estimate Equation (1.1) using this placebo intervention. This procedure is repeated 1,000 times. Concerns that our results have arisen by chance – rather than by a true relationship – would play a major role if a high share of the 1,000 placebo coefficients were greater than our baseline (actual treatment) coefficient. Figure 1.7 plots the 1,000 placebo coefficients, with the solid line representing the baseline coefficient reported in Table 1.4. The baseline coefficient is greater than the 95th percentile the placebo coefficients' distribution, suggesting that our results are unlikely to be an artifact of the data.

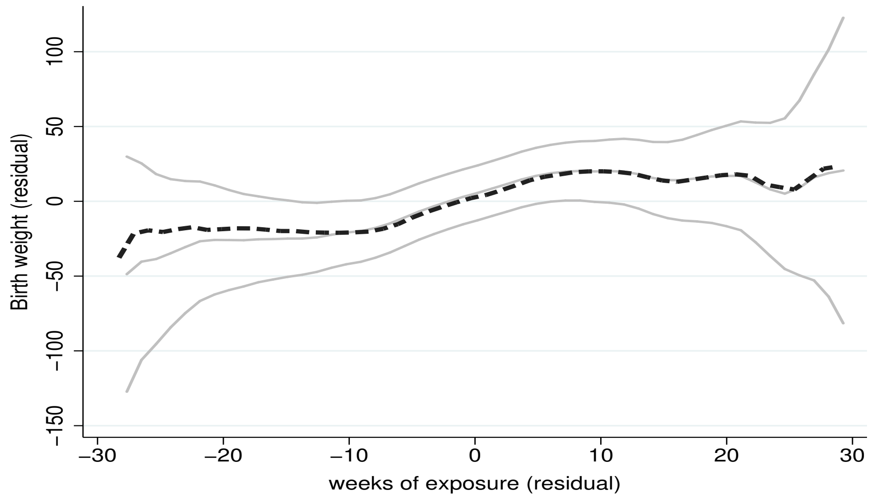
Figure 1.7: Placebo interventions: Randomization of weeks of exposure



Notes. Results of estimating Equation (1.1) for 1,000 placebo treatments. The solid line represents the baseline coefficient reported in Table 1.4. 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 illiterate mother, indicator for hospital birth, indicator for newborn sex.

Additionally, to check whether our (linear) specification misses important aspects of the data, we reestimate the effect of weeks of exposure on birth weight using nonparametric local polynomial estimators. Figure 1.8 presents nonparametric local polynomial estimates of the rainwater program on birth weight. We use an Epanechnikov kernel and select the bandwidth as suggested by cross-validation criteria. Our final strategy considers the methodology proposed by Oster (2017) to generate bounds on the treatment effect of interest. The consistency of results across alternative strategies in Table A11 lends additional credibility to our findings.

Figure 1.8: Nonparametric local polynomial



Note: —Estimates come from specification presented in equation 1.1. Controls include: priority criteria variables: Woman head of the family, # elderly in the family, # children in the family, total de # teenagers in the family, # disabled in the family, per capita family income; mother's age, indicator for illiterate mother, indicator for born in hospital, indicator for newborn sex. Standard errors clustered at the municipality by quarter of year.

1.5.3 Additional specification: effects by trimester

So far we have focused on the results regarding the number of weeks of exposure to the water harvesting policy. In this subsection, we concentrate on the results for exposure by trimester using Equation (1.2), which compares women whose construction of cisterns took place in the expected first ($trim1$) and second ($trim2$) trimesters of pregnancy relative to the ones whose construction happened during the expected third trimester of pregnancy.

Table 1.8 reports similar findings regarding the positive relationship between access to cisterns in early gestational periods and increases in birth weight. Table 1.8 reports that antenatal exposure in the first trimester increases birth weight and fetal growth compared to exposure in the third trimester. The coefficients for the second trimester dummy are smaller and not statistically significant. Column (I) shows that newborns exposed to cisterns in the first trimester *in utero* were born on average with roughly 46 grams more. To put this magnitude into perspective, we compare these results with the ones of the literature below in Subsection 1.5.4. The results in addition offer some indicative evidence of when the cistern affects birth weight as we find a large, positive impacts on those exposed earlier in pregnancy during the first trimester.

Table 1.8: Robustness: Specification with effects by trimester of exposure

	Dependent variables:				
	(I)	(II)	(III)	(IV)	(V)
	Birth Weight	ln Birth Weight	Fetal Growth	Low Birth Weight	Weeks of Gestation
trim1	45.846**	0.016**	1.028*	-0.015	0.085
	(21.813)	(0.008)	(0.577)	(0.011)	(0.083)
trim2	24.916	0.009	0.650	-0.006	0.009
	(21.957)	(0.008)	(0.557)	(0.010)	(0.074)
Month-year fixed effect	Yes	Yes	Yes	Yes	Yes
Municipality fixed effect	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Observations	4,054	4,054	4,054	4,054	4,056

Notes. This table shows the results of estimating Equation (1.2). 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 illiterate mother, indicator for hospital birth, indicator for newborn sex. *** p<0.01, ** p<0.05, * p<0.1.

1.5.4 Comparing the magnitude of the results

In this subsection, we compare the magnitude of our main findings with that of the literature. We focus on studies on in-kind welfare programs as well as cash-transfers. Hoynes et al. (2011) studied the U.S. Special Supplemental Nutrition Program for Women, Infants, and Children (WIC). They found a 18g to 29g impact on birth weight among participating mothers. Our estimates are larger than those in Almond et al. (2011), who found that Food Stamp Program participation increased birth weight between 15 and 20 grams for whites and 13 to 42 grams for blacks.

Our results are lower than the ones found for Mexico's *Progresa*, a large-scale conditional cash-transfer program that connects transfers to investments in children education and health. Pregnant women in *Progresa* must complete a prearranged prenatal care plan, purchase specific nutritional supplements, and attend meetings on pregnancy health education. Barber and Gertler (2008) look at the impacts on birth outcomes of participating mothers, finding that the children exposed to *Progresa* while *in utero* were born 127 grams heavier. In addition, Kumar and Gonzalez (2018) studied insurance coverage on neonatal outcomes in Mexico. Their main findings suggest insurance during pregnancy was associated with an increase of 108 grams in birth weight. This result is larger than the 46-gram increase in birth weight in our trimester specification (see Subsection 1.5.3).[30]

[30] Amarante et al. (2016) find a strong impact of a cash transfer program in Uruguay: the increase in

By and large, our results are comparable or greater in magnitude when it comes to studies of in-kind welfare programs. By contrast, several studies have reported stronger impacts cash-transfer programs on birth outcomes.

1.6 Concluding Remarks

This paper studies the effects of a large-sized water harvesting policy on birth outcomes. Using comprehensive data from various sources, we are able to estimate the effect of antenatal exposure to cisterns in Brazil's poorest region. We found evidence of a positive impact on birth outcomes, particularly on birth weight and fetal growth ratio. Our findings point out that the impacts are driven by more educated women. Besides, results indicate that the earlier the exposure to the intervention, the greater the positive impacts on birth outcomes. Even though we cannot rule out all possible mechanisms, the analysis suggests that water quality is an important channel explaining the results. We cannot exclude also the possibility that other channels can have a stronger impact in other settings.

An important set of place-based policies focuses on improving water resources to poor, climate vulnerable areas since several regions worldwide suffer from droughts and long dry spells. We estimate the effects of an actual place-based policy that has been implemented at large-scale affecting millions of people. In our setting of a developing country where a large number of families lives in climate vulnerable areas, water-related policies that can foster birth outcomes have the potential to be particularly cost-effective.

household income generated by the program led to a drop in the incidence of low birthweight of 19 to 25 percent.

2 ENVIRONMENTAL DISASTERS AND INFANT HEALTH: EVIDENCE FROM A MINING DISASTER IN BRAZIL

2.1 Introduction

In many developed and developing countries, mineral production plays a critical role in local economic performance and government revenue. This prominent role has given rise to debates about how the sector should be regulated to minimize its environmental impacts, and in particular, to avoid environmental disasters due to technical failures — such as reoccurring tailings dam disasters.[1] Unlike natural disasters, such as floods and earthquakes, most tailings dam failures are preventable and are largely the result of poor regulatory enforcement. In a recent report, the United Nations Environment Program stressed that:

> "Tailings Dam failures are a shared responsibility, caused as much by regulatory as management failure. In cases of catastrophic failures, the regulatory system has failed to ensure good design, and to implement, monitor and enforce adequate standards. ... Regulatory systems with multiple, independent checks are required to ensure standards and detect impending failures" (UNEP, 2017, p. 60).

Tailings dam incidents generally release a wide range of contaminants and toxic waste that are known to be hazardous to health. Yet, we lack a good understanding of health, and other consequences of regulatory failures. Such estimates are crucial for the optimal design of environmental regulation policies intended to reduce tailings dam incidents, and for evaluating the overall welfare impacts of mineral production in developing countries.

[1]Tailings dams are engineering structures designed to safely store the residues of mineral processing, an operation that concentrates the minerals containing the substances of interest (in this case, iron oxide minerals, mostly hematite) by separating them from other minerals. Processing ore requires crushing and grinding, producing fine enough grains to allow the physical or physical-chemical separation of ore from other minerals. While ore is used for further processing, often offsite, tailings are disposed in the mine site.

In this paper, we provide the first evidence of infant health consequences due to tailings dam incidents by examining the Mariana failure in Brazil, one of the largest tailings disasters registered to date.[2] Given the importance of infant health in subsequent human capital accumulation (Black et al., 2007, Figlio et al., 2014), these results may have important long-lasting intergenerational implications, and influence the debate on the benefits of implementing costly environmental regulatory policies.[3] Our paper is closer in spirit to a relatively little explored niche in the literature that focuses on the costs stemming from failure industrial and environmental regulations, interacted with imperfect institutions (Duflo et al., 2013; Oliva, 2015) and especially its impact on health (Hansman et al., 2018). In this sense, the current paper also builds on the wider literature on consequences from recurring natural disasters, such as floods and tropical cyclones, since they can pollute the environment and damage infrastructure (Torche, 2011; Hsiang and Jina, 2014; Deryugina, 2017; Rosales-Rueda, 2018; Deryugina et al., 2018; Karbownik and Wray, 2019).[4]

To identify the effects of *in utero* exposure to disaster, we explore the exact timing of the dam collapse, and geographical variation across municipalities affected by the mud and those not affected – based on potential municipalities that could be affected by other dams which were built with the same technology as Mariana's dam. We find robust evidence that *in utero* exposure to mining failure had meaningful negative effects on average birth weight, with a sharp reduction of 29.03-32.26 grams concentrated in the third trimester of gestation, pointing to an underlying mechanism related to the nutritional shock related to the disaster. These results are robust to multiple checks for combinations of controls and fixed effects, pre-trend effects, and a variety of alternative definitions of control groups. Furthermore, we find that *in utero* exposure to the Mariana catastrophe had a non-trivial impact on infant mortality and no effects on gestational length.

The remainder of the paper proceeds as follows. Section 2.2 describes the consequences of the disaster. Section 2.3 explains the data sources and sample, and presents descriptive statistics of birth outcomes and mother characteristics. Section 2.4 presents the identification strategy. In Section 2.5, we analyse the main empirical results and Section 2.6 presents the final remarks.

[2] Samarco, which began operating in the mid-1970s, is currently jointly owned by two of the largest mining companies in the world, Brazilian Vale and Anglo-Australian BHP.

[3] Just recently, another large tailings dam failure occurred (three years and two months after the Mariana disaster) in Brumadinho, killing more than 240 people.

[4] Regarding natural disasters in Brazil, Oliveira and Quintana-Domeque (2016) use a triple-difference approach to measure the intrauterine impact of the 2008 floods in Santa Catarina, and find a strong effect on birth weight for the newborns exposed to the hurricane about 30 km from the eye of the storm (90 grams for 1st trimester, 105 grams for 2nd trimester, and 120 grams for 3rd trimester).

2.2 The Mariana mine tailing disaster

On November 5, 2015, a large dam containing 52 million cubic meters of iron mining residues (tailings) collapsed in Mariana, located in the state of Minas Gerais (MG), Brazil (see Figure B1 in Appendix). Approximately 32.6 million m^3 of residues dropped from Fundão, as a mudwave flowed through a narrow valley and quickly reached the community of Bento Rodrigues, it destroyed several buildings and killed 19 people. The mudflow traveled further downstream, affecting wildlife, vegetation and other settlements before reaching Rio Doce, a river that flows eastward into the Atlantic Ocean, which might still affect many generations to come. This tragedy was attributed to a severe lack of preparedness and oversight by the companies managing the project, as well as insufficient regulation of Brazil's mining sector.[5]

The environmental and socioeconomic consequences of the disaster were highly publicized. Hundreds of houses were destroyed; water supply and sanitation systems were interrupted in many cities, leaving thousands of people facing a serious water shortage and enduring queues to be supplied with potable waters. The disaster also destroyed hydroelectric power plants and civil infrastructure, limiting regional trade, tourism and other activities such as agriculture and fishing – entire fish populations were immediately killed –, and causing massive job loss in the region. (UNEP, 2017).

Communities were affected in many ways, with more than 1 million inhabitants in 39 municipalities along the 600 kilometer river path to be exposed to the toxic mud.[6] This dam burst is considered the largest environmental disaster in Brazil and the largest in the world involving tailings dams. Such a destructive shock can affect all individuals, including pregnant women. So far, there is no comprehensive assessment of the extent of those impacts on infant health.

2.3 Data

2.3.1 Data Sources and Sample

We make use of publicly available birth registry data provided by the System of Information on Live Births (SINASC) from 2011 to 2016. This data includes information about the pregnancy (birth weight, gestational length), the delivery (C-section, multiple births, sex) and the mother (age, education, marital status, municipality of residence). More importantly, it provides the date of the last menstrual period – based on medical information obtained from doctors via a physical examination or other methods–, which in

[5]See, for more details: http://webdoc.france24.com/brazil-dam-mining-disaster-mariana/.

[6]See, for more details: https://www.bbc.com/portuguese/noticias/2015/12/151201_dados_mariana_cc

this study is considered to be the date of conception, similar to Lautharte Junior and Rasul (2019).

We also use microdata from the System of Information on Mortality (SIM) related to infant mortality. The death certificates from SIM contains information on natural and non-natural deaths, including detailed cause of death and characteristics of the deceased as well as some mothers characteristics. For the cases in which the infant dies within the first year of life, we linked this information to the birth data via their unique birth identifier.[7] This merge allows us to assign the specific period of conception for those in the death register.

Our treated group consists of women living in Minas Gerais municipalities affected by the disaster and who were exposed to the tragedy during their pregnancies.[8] To add individuals to the treatment group, 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). In the spirit of an ITT estimation, the trimester of exposure is based on a 93-day window ([0,3] months, (3,6] months, (6,9] months) from conception date, irrespective of gestational length.[9] Due to common endogenous fertility and selective migration after disasters, we limit our analysis to mothers who made their fertility decisions before the event.[10] In other words, our analysis does not include cohorts conceived after November 5th, 2015.

To construct the comparison group, we exploit the fact that there are other dams located in the same state, built with similar technology to the Mariana dam.[11] We simulate the mud path for all such dams as if they had suffered a similar failure[12]. To increase the comparability and reduce the risk of differential trends driven by other factors, our baseline comparison group is composed of pregnant women living in the five most affected municipalities in each simulated dam failure.[13] The reason for taking these areas into

[7]The matching rate is not 100 percent because of unique personal identifiers are missing in SIM dataset. For more details see Carrillo and Feres (2017).

[8]This information is from the Renova Fundation, available at https://www.fundacaorenova.org/mapa-de-atuacao/. All the municipalities where Renova operates is shown in Figure B1. This entity is responsible for mobilizing the repair of damage caused by the dam's breach. To date, nearly R$ 7 billion have been disbursed in actions to repair the accident damage (Fundação Renova, 2019).

[9]This treatment definition, in contrast to using birth dates to measure exposure, avoids the problems that can arise if a gestational length is affected by a disaster (Brown, 2017; Currie and Schwandt, 2016; Persson and Rossin-Slater, 2018 and Currie et al., 2018). Table B8 in the Appendix shows that results are qualitatively similar if we measure exposure using the gestational age to calculate the conception date (panel A), or if we measure exposure backward from the date of birth, assuming that each pregnancy lasted precisely nine months (panel B) (Currie and Rossin-Slater, 2013).

[10]It is quite plausible that family planing decisions made after the catastrophe are endogenously related to characteristics correlated with birth outcomes.

[11]This information comes from *Agência Nacional de Mineração*. The report can be downloaded at https://bit.ly/2MADOOb.

[12]The simulated path for each dam with the same technology was calculated using the QGIS software, based on spatial data of municipalities and the hydrography of Minas Gerais. We detail the construction of the 31 municipalities in the baseline comparison group in the Appendix B.1

[13]We assess the robustness of our findings under several different control group definitions.

consideration is that they are prone to the same disaster, with a comparable level of risk.[14] Moreover, in order to keep the data from the control group more similar to the treated group, we left out the municipalities from the metropolitan region of Belo Horizonte from our sample analyses. As we can see in Figure B2 in the Appendix, the region where the dam collapsed is indicated by an X. The brown area shows the flooded region, and the blue area depicts the five closest municipalities for each dam with the same technology and with similar age, already excluding the metropolitan region of Belo Horizonte.

2.3.2 Descriptive Statistics

Our final sample contains 100,704 individuals. Appendix Table B1 shows the descriptive statistics for some specific characteristics in our sample. Table 2.1 presents summary statistics for all individual-level covariates used in our analysis between treated and untreated areas for newborns exposed from trimester eighteen to four relative to the disaster date. Columns (1)-(3) show the number of observations, the mean, and standard deviation for non-affected areas, while columns (4)-(6) exhibit the same statistics for newborns in the treated area. A simple comparison of these variables suggests that pregnancies within affected areas present very similar characteristics in the baseline when compared to pregnancies in not affected areas.

Table 2.1: Summary Statistics - Newborn and Mother characteristics in the pre-treatment period

	(1)	(2)	(3)	(4)	(5)	(6)
	Not affected area			Affected area		
	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.
Mother's age <=20 yrs	41,181	0.168	0.374	42,937	0.190	0.392
Mother's age >40 yrs	41,181	0.0199	0.140	42,937	0.0143	0.119
Mother's education >=12 yrs	39,123	0.183	0.387	41,806	0.167	0.373
Married	40,408	0.509	0.500	42,487	0.538	0.499
Female baby	41,170	0.487	0.500	42,924	0.485	0.500

Notes. Table 2.1 presents number of observations, means and standard deviations for mothers' characteristics in the sample for the period prior to disaster.

More importantly, in Table B2 in the Appendix, we check whether the timing of the collapse is associated with the baseline mothers characteristics and fail to find statistically significant differences. See the Appendix B.1 for more details on the data and summary statistics.

[14] In addition, in the Mariana disaster, the five first municipalities were the most directly affected.

2.4 Empirical strategy

Our objective is to evaluate the average impact of exposure *in utero* to the Mariana disaster and the resulting newborns' outcomes. The ideal scenario would be to compare the child's health outcomes observed in each affected municipality with its counterfactual. We would therefore like to observe what would have happened to the same child if they had never been affected by the disaster event. However, since we are unable to observe such counterfactual cases, we approach this problem using a quasi-experimental strategy.

Our empirical strategy exploits the timing and location of the Mariana dam collapse to identify the impact of *in utero* exposure to the disaster on birth outcomes. In other words, it basically compares cohorts based on their place of residence and the timing of disaster. We estimate the following difference-in-differences (DID) specification:

$$Y_{imts} = \beta \cdot \mathbf{exposure}_{imts} + X'_{imts}\Theta + \omega_m + \lambda_t + \mu_s + \varepsilon_{imts} \tag{2.1}$$

where Y_{imts} is an outcome observed in a child i, conceived in month m and year t, with the mother residing in municipality s, and X_{imts} is a vector of individual controls: indicator for married mothers, newborn sex, highly educated mothers (12 or more years of schooling), and dummies for the mother's age when above 40 years old and when below 20 years old. The municipality fixed effect μ_s accounts for unobserved time-invariant determinants of birth outcomes shared among mothers residing in the same municipality, while the inclusion of month and year fixed effects, ω_m and λ_t, adjusts non-parametrically for shocks that are common to all pregnant women at a specific moment in time. The indicator variable **exposure** equals one for women exposed to the disaster during their pregnancy and zero for pregnant women not exposed to the disaster. The key parameter of interest is then β, which measures whether infants exposed to the disaster during the gestational period have worse outcomes.

We also analyze trimester-specific impacts of the disaster by flexibly allowing the effect of exposure to vary by trimester of pregnancy. For instance, the fetus may be more sensitive to stress due to environmental shocks when exposed during the first trimester of gestation (e.g Lee et al. (2003) and Bozzoli and Quintana-Domeque (2014)). To do so, we refined our model to:

$$Y_{imts} = \alpha \cdot \mathbf{T1}_{imts} + \delta \cdot \mathbf{T2}_{imts} + \theta \cdot \mathbf{T3}_{imts} + X'_{imts}\Theta + \omega_m + \lambda_t + \mu_s + \varepsilon_{imts} \tag{2.2}$$

where **T1**, **T2**, and **T3** are dummy variables indicating first, second and third trimesters of pregnancy at the day of the collapse, respectively.

Furthermore, we perform an event study analysis which allows us to more formally test for pre-trends in outcome variables in the pre-period. The specification is as follows:

$$Y_{imts} = \left[\sum_{\tau=-15}^{-2} \beta_\tau I(t_{mts} - t^* = \tau) + \sum_{\tau=0}^{2} \beta_\tau I(t_{mts} - t^* = \tau) \right] + X'_{imts}\Theta + \omega_m + \lambda_t + \mu_s + \varepsilon_{imts} \tag{2.3}$$

where the indicator $I(t_{mts} - t^* = \tau)$ measures the time (in trimesters) relative to the day of the collapse, t^*. We set the coefficient β_{-1} equal to zero to use the trimester immediately prior to the disaster as the reference. If prenatal exposure to the disaster in the third (β_0), second (β_1) or first trimester (β_2) has adverse consequences on birth outcomes, one would expect the relevant coefficients to be negative and significant.

Our primary identifying assumption is that children born to mothers living in municipalities exposed to the disaster during their gestational duration would have had similar trends in birth outcomes to those living in similar municipalities outside of the affected area in the absence of the shock. The estimation of β_τ; with $\tau < 0$ serves as a direct test of the plausibility of the identifying assumption. If cohorts of newborns across municipalities have similar trends before the date of disaster and diverge only after the dam collapse, it provides strong evidence that such changes were caused by the mining failure rather than an unobservable factor.

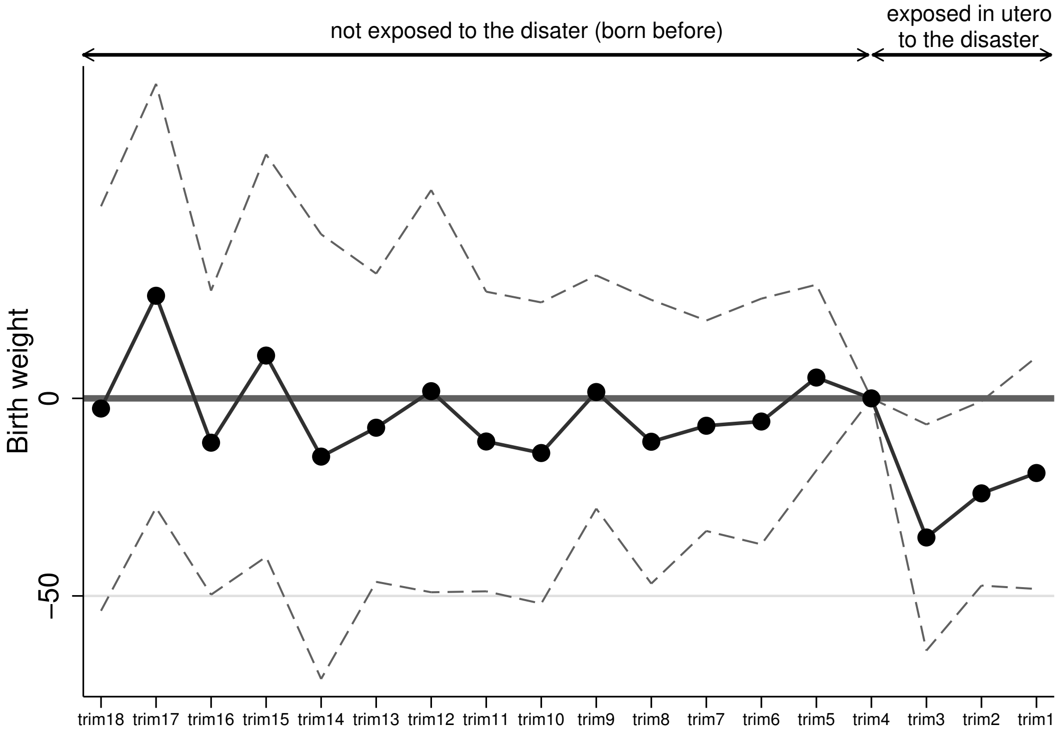
We provide the first piece of evidence supporting the parallel trends assumption for birth weight, which is the preferred outcome measure (Figure B3, Appendix). It displays the evolution of the average birth weight over trimesters of exposure for the municipalities in our sample. This figure provides a preview of the major finding of this paper, showing a sharp reduction in the average birth weight of newborns exposed in the third trimester in the affected areas. Finally, we emphasize that pregnant mothers located in municipalities further from the affected municipalities might suffer from maternal stress caused by the disaster, also causing our estimates to be biased downwards.

2.5 Results

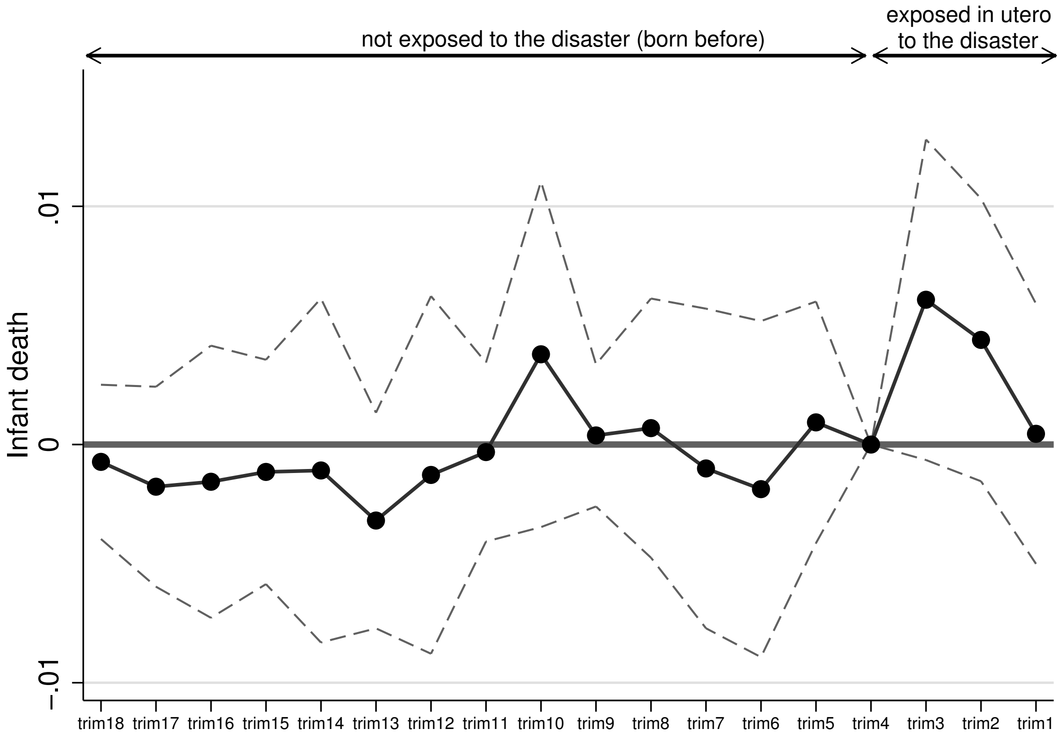
We begin by plotting in Figure 2.1 the event-study estimates for the relationship between birth weight, infant mortality and the disaster date. Birth weight and infant mortality in affected and control groups evolved similarly for cohorts not exposed to the disaster during pregnancy (conceived 4 trimesters or more before the shock). This suggests the absence of different pre-trends and yields strong supportive evidence for the main identifying assumption. After the disaster, we document a sharp decline in birth weight and an increase in infant mortality for children exposed *in utero*, especially among those exposed in the third trimester.

Figure 2.1: Event-Studies: Effects of the dam incident on birth weight and infant mortality

(a) Birth weight



(b) Infant mortality



Notes. This figure plots the coefficients of Equation (2.3). The trimester immediately prior to the disaster (trim4) is used as the reference period. Dashed lines show 95 percent confidence intervals.

Table 2.2 reports the difference-in-differences estimates. We find that exposure to the disaster during pregnancy is associated with detrimental effects on birth weight (columns 1–6). There is a 23.56-24.03 gram reduction in the birth weight among children exposed *in utero* to the dam collapse. In columns 4–6, we allow the estimated effects to vary by trimester of the pregnancy. Coefficients are more precisely estimated for those exposed in the third trimester, with a estimated impact of 29.03-32.36 grams. Results are similar across specifications when we include only municipality, month, and year of conception fixed effects (without the control variables as in our main specification), and when we include month×year of conception fixed effects control for time varying characteristics common to births in all municipalities and for the effects of seasonality on birth outcomes.

Although we observe sizable effects for all trimesters, our estimates are more precisely estimated towards the end of pregnancy. This appears to be consistent with a nutritional channel (see, for instance, Almond et al. (2011), Amarante et al. (2016), Currie and Almond (2011)), but also may be related to exposure to pollutants and short-term disruptions in water provision, which could have direct effects on women health. To provide suggestive evidence on this potential channel, we investigate if the number of hospitalizations of water-related diseases increased after the disaster. Figure B4 shows that the shock caused an increase of about 3% in per capita hospitalizations by diarrhea. In Figure B5 we show that measures of water quality were negatively affected by the collapse. While we cannot rule out the possibility that the effects of the dam collapse operate through other mechanisms, these patterns are consistent with pollution and water provision driving in part the impacts of the dam collapse on infant health.

In terms of magnitude, these findings are larger than some previous studies that looked at the *in utero* exposure effect caused by terrorism attacks and by natural disasters. Brown (2017), for instance, finds that infants exposed *in utero* to the 9/11 attacks are on average 15 grams smaller. Camacho (2008) finds that living near a landmine explosion during pregnancy reduces birth weight by 9 grams, and Simeonova (2011) finds a natural disaster exposure reduces birth weight by 1 gram. This comparison highlights the importance of health consequences stemming from regulatory failures.

In columns 7–8, we investigate if a reduction in birth weight is the result of the disaster's direct impact on gestational duration. Birth weight will reduce mechanically if gestational length responds to the stress caused by the event. We do not find an exposure effect on the number of gestational weeks. Finally, columns 9–10 present estimates of the effect of the disaster on infant mortality. We find strong evidence that the event led to an increase in infant death. To the extent that the weakest fetuses are probably more dramatically affected, our estimates for birth weight are likely to be underestimated.

Table 2.2: Effects of exposure on birth weight, weeks of gestation and infant mortality

	Birth weight						Weeks of gestation		Infant death	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Exposure (any trimester)	-24.029	-23.562	-23.790				-0.022		0.004	
	[12.761]*	[13.591]*	[14.066]*				[0.047]		[0.001]***	
3^{rd}trim.				-30.689	-32.360	-29.031		-0.062		0.007
				[8.426]***	[9.669]***	[12.827]**		[0.058]		[0.002]***
2^{nd}trim.				-21.370	-19.698	-28.384		-0.019		0.005
				[16.201]	[16.729]	[19.516]		[0.058]		[0.001]***
1^{st}trim.				-20.162	-18.856	-13.788		0.014		0.001
				[19.950]	[20.217]	[21.899]		[0.059]		[0.002]
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month and year of conception FE	Yes	Yes		Yes	Yes		Yes	Yes	Yes	Yes
Control variables		Yes			Yes		Yes	Yes	Yes	Yes
Month×year of conception FE			Yes			Yes				
Number of obs.	100,704	96,089	100,704	100,704	96,089	100,704	96,089	96,089	96,089	96,089
R^2	0.007	0.020	0.008	0.007	0.020	0.008	0.011	0.011	0.003	0.003

Notes. Control variables are: indicators for married mothers, newborn sex, high-education mothers (12 or more years of schooling), and dummies for mothers older than 40 and for those younger than 20. Standard errors clustered at the municipal level. ***p<0.01, **p<0.05, *p<0.1.

2.5.1 Heterogeneous effects

We first discuss some heterogeneous effects on birth weight along with some mother characteristics. For this purpose, in Table 2.3, we split the sample according to the mother's level of education, marital status and age to investigate whether disadvantaged mothers are more affected by the disaster, using the same specification as in columns (2), (5) and (7)-(10) of Table 2.2. We find that less educated mothers are largely affected by exposure to the disaster during their third trimesters. For more educated mothers, the effect is quantitatively close to zero, irrespective of the trimester of pregnancy when exposed. A possible explanation for these findings is that less educated mothers are more likely to be exposed to the dam burst, whereas more educated mothers – with a higher socio-economic status – have ways to buffer the adverse consequences of disorders in their perinatal period. We find weak evidence that married mothers are affected, while single mothers (i.e. single, separated or divorced) are significantly affected when exposed during the third trimester of pregnancy[15]. Regarding age, we find statistically significant effects only for 20-year-old mothers and older, although coefficient estimates are larger but imprecisely estimated for younger women.

In Table 2.4, we dig deeper by checking the estimated effects of the disaster on the probability that a birth occurs in a specific birth weight category. In these columns the dependent variables are indicators of birth weight from less than 1500 grams and up to 4000 grams, respectively. The results suggest that the almost complete effect of the Mariana disaster on the average birth weight is due to fewer infants with birth weights between 2500–3000 grams. Moreover, we investigate whether effects differ as a function of the gender of the newborn. The results in Table B3 in the Appendix suggest a larger effect

[15]This is in line with the literature showing that married women tend to have better birth outcomes (Quintana-Domeque and Ródenas-Serrano, 2017).

Table 2.3: Heterogeneous effects

	Dependent variable: Birth weight											
	Mother's education				Married				Mother's age			
	More than 12 years		Less than 12 years		Yes		No		Younger than age 20		Older than age 20	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Exposure (any trim.)	-9.351		-27.796		-18.809		-30.217		-7.416		-26.346	
	[17.633]		[15.198]*		[14.403]		[15.815]*		[25.732]		[14.051]*	
3^{rd}trim.		1.698		-40.984		-5.240		-65.533		-45.922		-27.974
		[31.361]		[11.893]***		[17.529]		[24.649]***		[38.844]		[12.339]**
2^{nd}trim.		3.907		-26.034		-19.687		-20.857		-32.468		-16.851
		[26.428]		[22.818]		[12.908]		[24.365]		[33.098]		[14.868]
1^{st}trim.		-31.973		-16.415		-31.047		-4.773		55.749		-34.478
		[30.944]		[20.766]		[20.539]		[23.893]		[34.496]		[20.375]*
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month and year of conception FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	17,300	17,300	78,789	78,789	50,632	50,632	45,457	45,457	17,009	17,009	79,080	79,080

Notes. All regressions include municipality, year and month of conception fixed effects, and control variables (the same variables as in Table 2.2). Standard errors clustered at the municipal level. ***p<0.01, **p<0.05, *p<0.1.

on newborn males. This is in line with the substantial literature on "fragile males" showing that the male fetuses are biologically more vulnerable to detrimental influences *in utero*, with higher risks of miscarriage or spontaneous abortion (Kraemer, 2000; Dinkelman, 2013; Almond and Mazumder, 2011).

Next, in Table 2.5, we investigate the consequences of pregnancy exposure to the dam burst on infant health by examining the following outcomes from the birth registry: *ln* birth weight; fetal growth ratio; probability of being born within 28 weeks of gestation; C-sections and APGAR scores (1 and 5 minutes after delivery). First, the resulting birth weights, expressed as a log, reinforce the evidence that the dam collapse during pregnancy adversely affected birth weights. We also find significant evidence of the fetal growth ratio (defined as the birth weight divided by the number of weeks of gestation) responding negatively to the disaster, with an average reduction of 0.75 grams per each week of gestation. This augments the fact that the effects occur as a result of a birth weight reduction, not because of a shorter gestational length. Additionally, we find no evidence of significant impact even when looking at the incidence of births before 28 weeks, which implies there is no increased risk of prematurity. The estimates for the incidence of C-section, and for APGAR scores at one and five minutes are also statistically insignificant.[16]

[16]In Table B4 in the Appendix, we find (i) no impact of the disaster on the probability of preterm births and (ii) that the effects are largely driven by increased post-neonatal mortality (from the second to the twelfth month of life).

Table 2.4: Estimated effects of dam collapse by birth weight categories

VARIABLES	Incidence in birth weight categories											
	< 1500g		< 2000g		< 2500g		< 3000g		< 3500g		< 4000g	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Exposure (any trimester)	0.002		0.003		0.005		0.019		0.011		0.004	
	(0.002)		(0.003)		(0.004)		(0.012)		(0.013)		(0.005)	
3^{rd}trim.		0.004		0.003		0.005		0.022***		0.019		0.005
		(0.004)		(0.005)		(0.006)		(0.008)		(0.012)		(0.007)
2^{nd}trim.		0.001		0.001		-0.001		0.017		0.008		0.009
		(0.003)		(0.004)		(0.006)		(0.021)		(0.017)		(0.005)
1^{st}trim.		0.001		0.005		0.010		0.017		0.008		-0.001
		(0.002)		(0.003)		(0.009)		(0.014)		(0.013)		(0.006)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month and year of conception FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	96,089	96,089	96,089	96,089	96,089	96,089	96,089	96,089	96,089	96,089	96,089	96,089

Notes. All regressions include municipality, year and month of conception fixed effects, and control variables (the same variables as in Table 2.2). Standard errors clustered at the municipal level. ***p<0.01, **p<0.05, *p<0.1.

Table 2.5: Other birth outcomes

	log birth weight		fetal growth ratio		weeks of gestation smaller than 28		C-section		APGAR 1		APGAR 5	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Exposure (any trim.)	-0.009**		-0.590*		0.001		0.013		0.015		-0.043	
	(0.004)		(0.334)		(0.001)		(0.012)		(0.060)		(0.060)	
3^{rd}trim.		-0.013***		-0.749***		0.003		0.010		0.035		-0.030
		(0.005)		(0.233)		(0.002)		(0.012)		(0.058)		(0.080)
2^{nd}trim.		-0.007		-0.462		-0.000		0.006		-0.015		-0.045
		(0.005)		(0.421)		(0.001)		(0.013)		(0.057)		(0.048)
1^{st}trim.		-0.007		-0.562		0.001		0.021		0.027		-0.053
		(0.007)		(0.477)		(0.001)		(0.015)		(0.072)		(0.056)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month and year of conception FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	96,089	96,089	96,089	96,089	96,089	96,089	95,622	95,622	93,727	93,727	93,856	93,856

Notes. All regressions include municipality, year and month of conception fixed effects, and control variables (the same variables as in Table 2.2). Standard errors clustered at the municipal level. ***p<0.01, **p<0.05, *p<0.1.

2.5.2 Robustness checks

We perform several robustness and sensitivity tests to confirm our main findings that the birth weight responded to exposure to the Mariana collapse, mainly during the third trimester of pregnancy, separating it from any possible sample selection bias. The initial test was to investigate whether our results are sensitive to different control group definitions incorporated into our analysis. One potential concern is that our estimates could be driven by the specific demographics of our baseline comparison group. We vary the number of municipalities belonging to the simulated mud paths control group; we include municipalities located in the metropolitan region of the state capital; we drop municipalities near São Paulo state; we use all municipalities in the Doce River basin as well as all municipalities belonging to Minas Gerais; and we use all municipalities affected by the Brumadinho disaster and the municipalities directly adjacent to the treated group.[17]

We present estimates based on several alternative definitions of the comparison group in Table 2.6. The coefficients of birth weight are consistently negative across specifications and have similar magnitude when compared to our main results. Overall, they all remain statistically significant at least at the 10% level – for the municipalities directly adjacent to the treated group, likely stemming from a spillover effect in this control group. We conclude that while the choice for the five closest municipalities belonging to the simulated mud paths group seems to be somewhat arbitrary, varying the control group would have little effect on our qualitative results.

[17]Our definition of Doce River basin comes from Viana (2016).

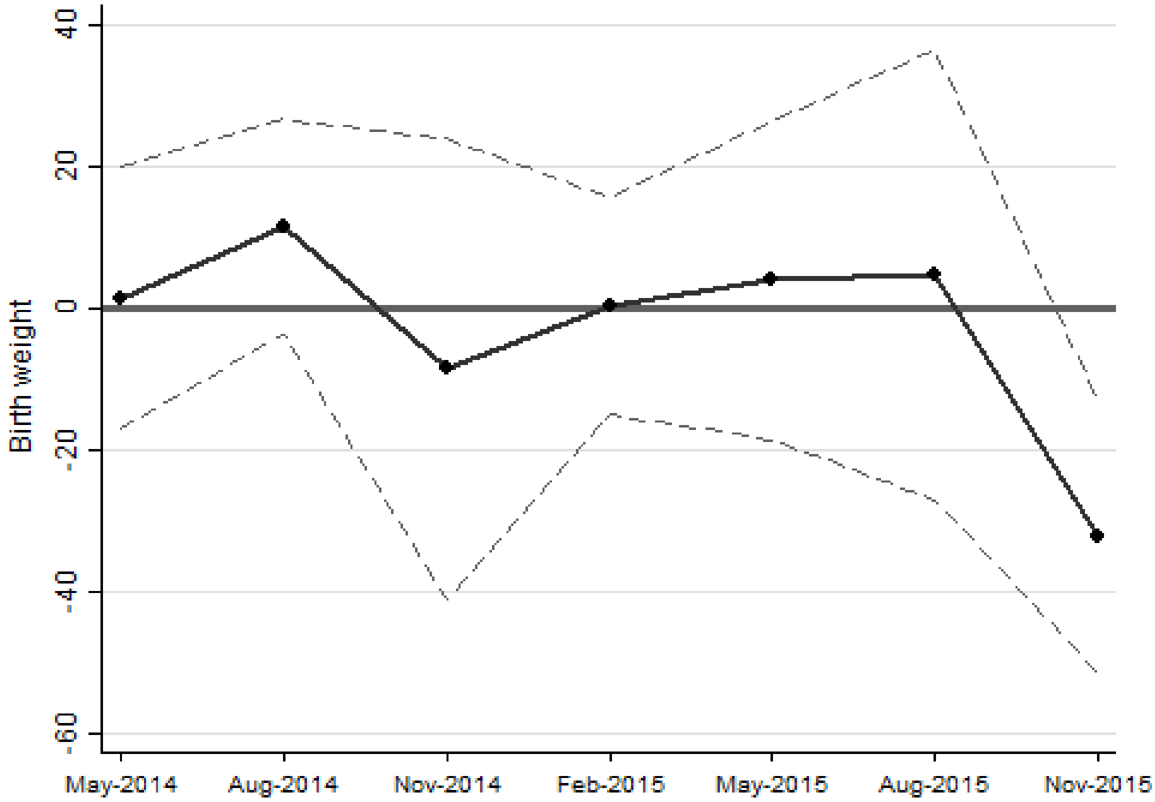
Table 2.6: Other control control groups

VARIABLES	(1) 10 nearest municipalities	(2)	(3) 15 nearest municipalities	(4)	(5) 20 nearest municipalities	(6)	(7) all nearest municipalities	(8)	(9) MG state	(10)
Exposure (any trim.)	-20.667		-17.761		-18.631		-14.134		-13.963	
	(13.807)		(13.824)		(13.751)		(13.850)		(13.669)	
3^{rd}trim.		-29.438***		-25.210***		-26.231***		-22.967**		-22.728**
		(9.714)		(9.606)		(9.505)		(9.451)		(8.852)
2^{rd}trim.		-18.154		-14.337		-15.605		-10.643		-8.988
		(17.007)		(17.363)		(17.301)		(17.478)		(17.749)
1^{rd}trim.		-14.612		-13.913		-14.236		-8.990		-10.388
		(20.376)		(20.231)		(20.188)		(20.278)		(20.004)
Observations	117,697	117,697	136,953	136,953	146,988	146,988	164,556	164,556	708,861	708,861
	(11) including RM BH	(12)	(13) dropping municipalities near SP	(14)	(15) Doce River Basin	(16)	(17) Brumadinho path	(18)	(19) Border Neighbours	(20)
Exposure (any trim.)	-18.164		-24.897*		-16.738		-33.625**		-9.161	
	(14.175)		(13.463)		(13.782)		(14.541)		(14.315)	
3^{rd}trim.		-28.848***		-32.871***		-25.023**		-43.157***		-21.503*
		(9.354)		(9.888)		(9.636)		(14.224)		(10.873)
2^{rd}trim.		-13.024		-21.302		-14.161		-26.647		-1.195
		(17.693)		(16.457)		(17.334)		(16.567)		(17.451)
1^{rd}trim.		-12.886		-20.716		-11.224		-31.386		-5.180
		(20.958)		(20.024)		(20.119)		(19.613)		(20.483)
Observations	194,105	194,105	89,224	89,224	139,758	139,758	58,237	58,237	81,803	81,803
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month and year of conception FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes In Table 2.6, we use the following definitions of the control group respectively: pregnant women living in the 10, 15 and 20 most affected municipalities in the simulated path; including all pregnant women living in Minas Gerais; including pregnant women living in municipalities located in the Belo Horizonte metropolitan region; dropping pregnant women living near São Paulo state; including those living in all municipalities in the Doce River basin; including the ones living in the municipalities affected by the Brumadinho disaster; and including pregnant women living in areas adjacent to municipalities of the treated group.

To further check for robustness of our results in the third trimester, we perform a placebo exercise by assigning false disaster dates and rerun the equation 2.2. This analysis provides additional evidence in support of our nutritional hypothesis. Theoretically, if the disaster mainly affects water system and fishing activities, then the effect for previous cohorts in their third trimester (nutritional restriction is not expected) should be insignificant. Figure 2.2 plots the results of these placebo effects for dummies representing the third trimester of gestation, based on each three months before the real accident date. It can be seen that the actual effect is higher than the false effects estimated from previous third trimester cohorts.

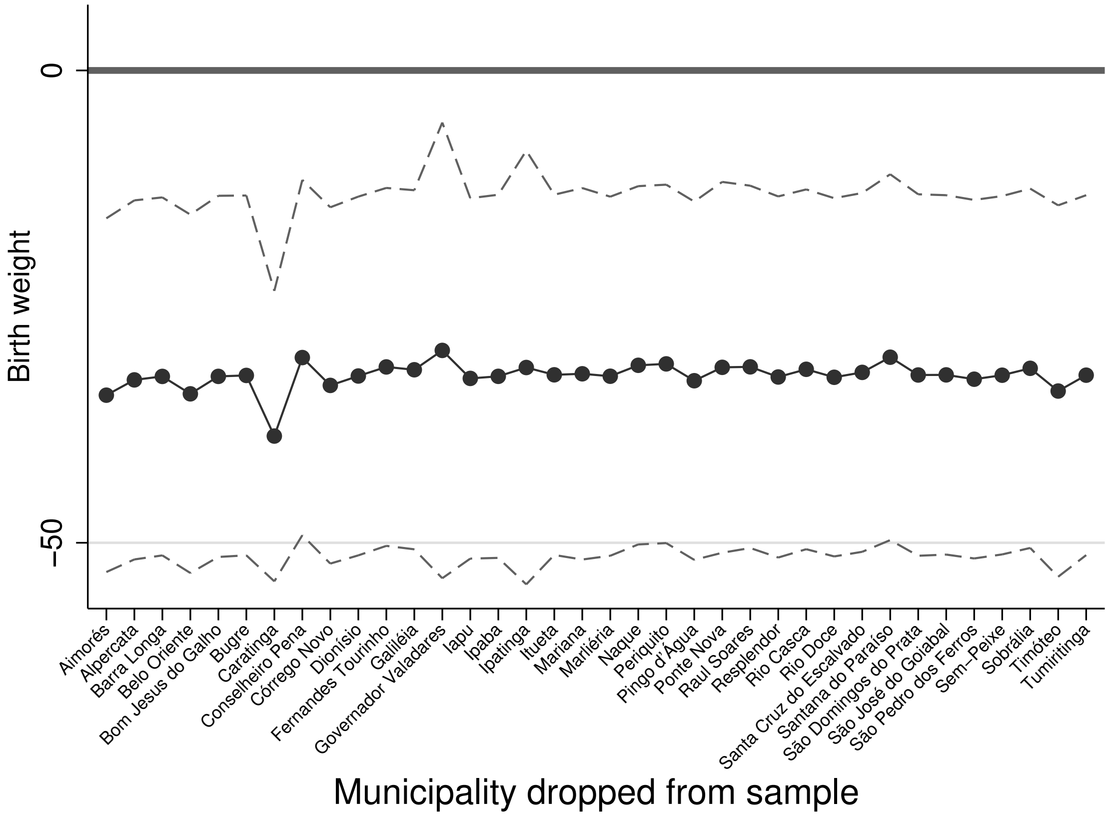
Figure 2.2: Placebo test



Notes. Figure 2.2 plots the estimated effect from separate regressions in which each municipality is excluded. Dashed lines show 95-percent confidence intervals. All regressions include municipality, year and month of conception fixed effects, and control variables (the same variables as in Table 2.2). Standard errors are clustered at the municipal level. Dashed lines show 95 percent confidence intervals.

Our analysis exploits variation across birth cohorts and municipalities. However, one could be concerned that the estimates are driven by particular areas, calling into question the generalizability of our findings. To address this issue, we re-estimate Equation 2.2, repeatedly, but each time excluding each treated municipality at a time. We find that the results are remarkably stable as shown in Figure 2.3.

Figure 2.3: Dropping each treated municipality



Notes. Figure 2.3 plots the estimated effect from separate regressions in which each municipality is excluded. Dashed lines show 95-percent confidence intervals. All regressions include municipality, year and month of conception fixed effects, and control variables (the same variables as in Table 2.2). Standard errors are clustered at the municipal level. Dashed lines show 95 percent confidence intervals.

Again, since birth weight will reduce mechanically if gestational length responds to stress caused by the event, we test whether our results are robust to normal periods of gestation. We focus on babies who were born i) at 37 weeks or more, or ii) between 39 and 42 weeks. Results for third trimester exposure are robust to this exercise as shown in 2.7. Moreover, Table B5 in the Appendix explores the extent to which our birth weight results are driven by the tails of the income distribution. We trim the top and bottom 1 to 2 percent of the birth weight distribution, as well as both tails simultaneously. The *in utero* results nonetheless remain statistically significant in all cases. We view these results as supporting the notion that our main *in utero* findings are not driven by individuals with extreme values of birth weight.

Table 2.7: Robustness: Trimming the gestational length

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: $\geq$ 37 weeks					
VARIABLES	birth weight		ln birth weight		fetal growth ratio	
Exposure (any trim.)	-20.033		-0.006		-0.515	
	(15.688)		(0.005)		(0.404)	
3^{rd}trim.		-31.332***		-0.010***		-0.735**
		(11.607)		(0.003)		(0.313)
2^{rd}trim.		-23.588		-0.007		-0.547
		(21.078)		(0.007)		(0.544)
1^{rd}trim.		-5.225		-0.002		-0.266
		(18.781)		(0.006)		(0.441)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Month and year of conception FE	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	84,858	84,858	84,858	84,858	84,858	84,858
	(7)	(8)	(9)	(10)	(11)	(12)
	Panel B: 39-42 weeks					
VARIABLES	birth weight		ln birth weight		fetal growth ratio	
Exposure (any trim.)	-25.723		-0.008		-0.631	
	(18.215)		(0.006)		(0.457)	
3^{rd}trim.		-44.094***		-0.014***		-1.090***
		(14.730)		(0.004)		(0.382)
2^{rd}trim.		-32.450		-0.009		-0.798
		(25.519)		(0.008)		(0.637)
1^{rd}trim.		-0.713		-0.001		-0.008
		(17.773)		(0.006)		(0.439)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Month and year of conception FE	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	48,719	48,719	48,719	48,719	48,719	48,719

Notes. All regressions include municipality, year and month of conception fixed effects, and control variables (the same variables as in Table 2.2). Standard errors clustered at the municipal level. ***p<0.01, **p<0.05, *p<0.1.

Up to the present moment, we have only considered robust standard errors clustered at the municipality level in all regressions. In order to account for possible spatial dependence between municipalities and for serial correlation, we compute Conley standard errors (Conley, 1999), with cutoff distances varying from 50 to 500 kilometers. In addition, we also compute a two-way clustered standard errors at the municipality and trimester of conception to adjust flexibly for both serial and spatial correlation in error terms. Significance of results is highly robust to alternative clustering as shown in Table B6 in the Appendix. In sum, spatial and two-way clustered standard errors are similar to regular clustered standard errors, confirming that these issues play no role in our context.

Finally, since not all individuals have last menstrual information in the SINASC dataset, one might worry that some potential nonramdom attrition in the third trimester could lead us to biased estimates. To check this, in Table 2.8, we perform a bounding exercise in the spirit of Lee (2009) and find empirically-computed 95 percent confidence intervals that range from -53.810 to -2.323 for the third trimester dummy.[18] We conclude that the effects we find are unlikely to be caused by missing in the conception date information that are systematically correlated with exposure to disaster in the third trimester.

Table 2.8: Lee bounds

	(1)	(2)	
VARIABLES	baseline	Randomly drop observations	
	birth weight	Average	95% CI
3^{rd}trim.	-22.248	-28.118	[-53.810, -2.322]
	(9.946)		
Number of random draws		500	
Municipality FE	Yes	Yes	
Month and year of conception FE	Yes	Yes	
Observations	100,704	99,988	

Notes. To this analyses, we adopted the 2.2, only considering the third trimester dummy as treatment variable. Standard errors clustered at the municipal level. We reestimate the model droppping missing variables 500 times. To more details, see Carrell et al. (2018).
*** p<0.01, ** p<0.05, * p<0.1.

2.5.3 Other health responses to the disaster

We try to shed additional light on a potential underlying mechanism behind our results. First, we test the impact of disaster on access to medical care. By exploring the National Inventory of Health Establishments (CNES) with monthly infrastructure information from 2012 to 2016, we check whether the disaster disrupted the supply of some local health services, which could drive part of our results. To address this concern, we examine the relationship between timing of disaster and local health resources, including number of Basic Health Units (BHU), Family Health Team (*Equipe Saúde da Família* - ESF), hospitals, hospital beds, ultrasound machines, and number of physicians (all measured per 1,000 inhabitants). Results in Table 2.9 suggest insignificant changes on the provision of several health services after the dam burst.[19]

[18]To this exercise, we use the Equation 2.2 without the dummies for the first and the second trimester. In other words, we only consider the third trimester dummy as treatment variable. By doing this, our objective is directly evaluate whether missing information in the third trimester is driving the results. We follow Carrell et al. (2018) to apply the Lee bound procedure in our analysis.

[19]In Table B7 in the Appendix, we also examines the demand side by estimating regressions using categories of medical visits care and of prenatal visits provided by doctors and nurses. These estimates also no indicate that increases in the demand for prenatal visits is a potential mechanism explaining our findings.

Table 2.9: Estimated effects of dam collapse on local health resources

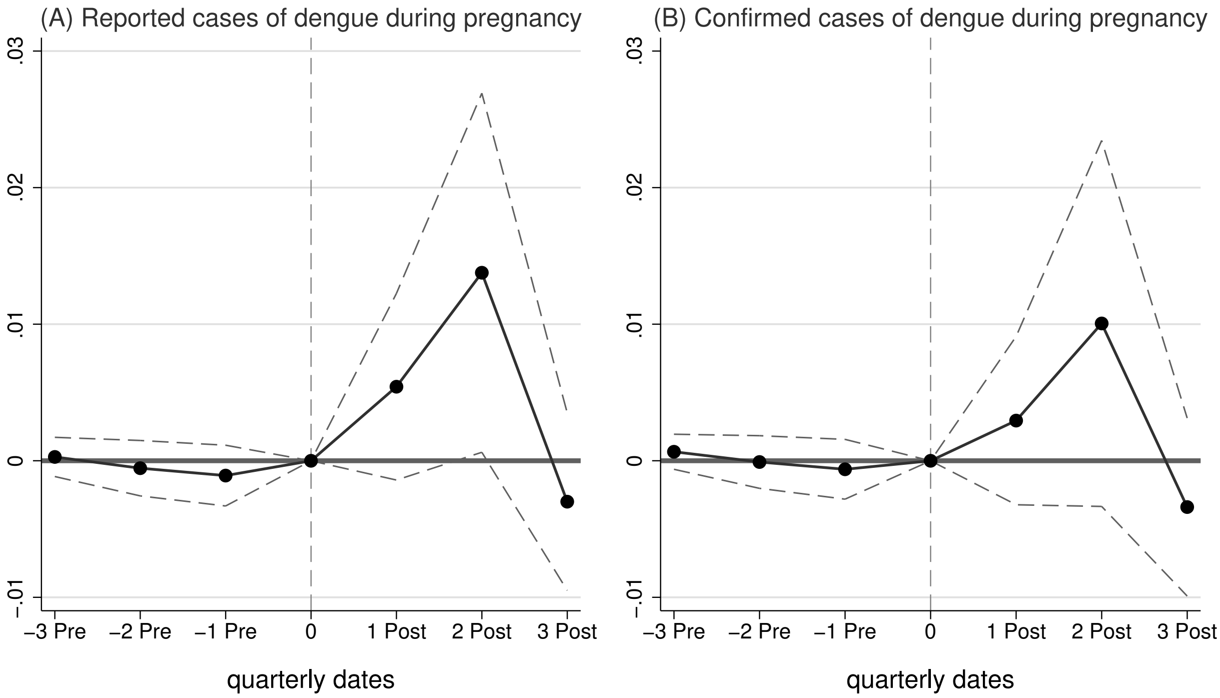
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	BHU	ESF	Hospital	Hospital neonatal beds	Hospital beds	Ultrasound machines	physicians
treat	-0.026	0.017	-0.001	-0.003	-0.039	-0.012	0.043
	(0.018)	(0.015)	(0.001)	(0.002)	(0.032)	(0.011)	(0.047)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month and year of conception FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,618	3,618	3,618	3,618	3,618	3,618	3,618

Notes. Dependent variable in each column is measured per 1,000 residents. This table shows the results of estimating time of disaster on health service. Estimation includes trimester relative to the accident date fixed effects and municipality fixed effect. Standard errors clustered at the municipal level.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Next, we use the number per capita of monthly reported and confirmed dengue cases during pregnancy (multiplied by 1000) in each municipality.[20] To this analysis, we use an event-study approach similar to our Equation 2.3 but considering a shorter range of time – 4 trimesters pre-accident and 3 trimesters after. We set the trimester composed by August-October as baseline. Figure 2.4 reveals visual evidence of an increase in those outcomes after the disaster. These findings point to the role of detrimental sanitation conditions through which the mud path might have affected birth outcomes. Dengue infections during pregnancy may lead to several types of adverse outcomes, including an increase in the likelihood of miscarriage, preterm birth, and low birth weight (Paixão et al., 2016).

[20]This data comes from *Sistema de Informação de Agravos de Notificação - SINAN.*

Figure 2.4: Estimated effects of dam collapse on cases of dengue during pregnancy



Notes. This figure plots the coefficients a event-study approach using monthly data. The trimester immediately prior to the disaster (August-October) is used as the reference period. Dashed lines show 95 percent confidence intervals. Estimation includes trimester relative to the accident date fixed effects and municipality fixed effect. Standard errors clustered at the municipal level.

We also examine whether our results are partly driven by sample selection. A potential issue pervading our analysis concerns fetal selection, since we only observe outcomes at birth for surviving fetuses.[21] Figure 2.5 (A) explores the disaster impact on fetal deaths per capita (number of fetal deaths per 1,000 residents), indicating no statistical effect.[22] In addition, if the disaster increases the probability of fetal death, we would expect to observe a lower number of live births during the pos-disaster in treated areas relative to the comparison group, leading to systematic changes in cohort sizes. Figure 2.5 (B) looks at the number of live births per 1,000 residents, suggesting no significant association with the disaster. Finally, by checking possible changes in the sex ratio at birth in Figure 2.5 (C), measured as the fraction of total births coming from boys, we obtain small and insignificant results.[23] Although the disaster effects seem to be unaffected by potential

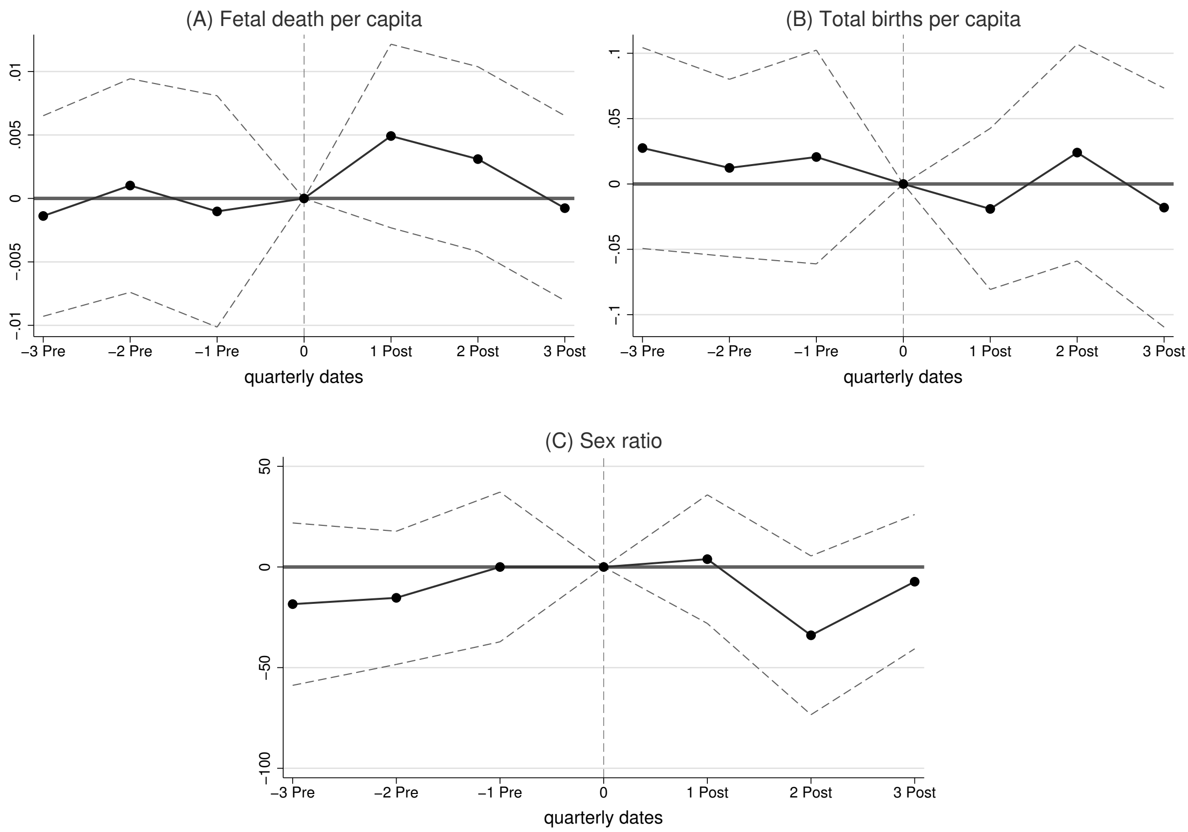
[21]Adverse shocks tend to cull weak fetuses before birth, which may lead the population of surviving newborns to be positively selected. Currie (2009) and Almond and Currie (2011), for instance, highlight this type of caveat.

[22]We extend this analysis by estimating the separate effect for boys and girls. Figure B6 in the Appendix illustrate the results. Although we did not find significant evidence, we found a higher change in the coefficient result for males after the disaster.

[23]A further issue is whether the dam collapse caused pregnant women to migrate to different cities to give birth, which could contaminate our estimates. We check for this in Figure B7 in the Appendix where the outcome is an indicator variable equal to 1 if the city of residence and city of birth differ.

selective mortality, this needs to be taken into account when interpreting the results on birth weight and infant mortality.

Figure 2.5: Sample composition analysis



Notes. This figure plots the coefficients an event-study approach using monthly data. The trimester immediately prior to the disaster (August-October) is used as the reference period. Dashed lines show 95 percent confidence intervals. Estimation includes trimester relative to the accident date fixed effects and municipality fixed effect. Standard errors are clustered at the municipal level.

Using the same event-study approach, we also check whether the disaster has resulted in any complication during pregnancy. We test this by collapsing individual data from SUS Hospital Admissions System (SIH/SUS) to create a count of hospitalization by municipality per month of the year. We focus on the hospitalizations cases in pregnancies associated with congenital complications (0-1 years old) and in the cases of mental disorders in women (14-49 years old) due to use of psychoactive substances, in order to exploit other potential mechanisms. Figure B9 in the Appendix indicates a slight increase in these outcomes in the periods. This piece of evidence suggests that the effects may

For this analysis, we use our equation 2.3 for a period of seven trimesters. The visual results suggest that endogenous population migration is not a source of major concern to us. Moreover, since we do not have high-frequency data on population movements, we provide further suggestive evidence by examining cohort size variables that would have been affected by migration. Figure B8 shows that the shock is not correlated with the number of students enrolled in schools. Migration (at least at large scale) would generate differences in school enrollments.

be explained in part by nutritional mechanisms and also by substance-use due to the consequences of the disaster.

Put together, these results suggest that the dam burst effect on birth outcomes was diffused through the environment, causing a number of hazardous consequences on pregnancy health. Additionally, we obtain some evidence to claim that selection into pregnancy cannot explain the drop in birth weights.

2.6 Conclusion

Using public information about registered births in Minas Gerais, we show that the unexpected *in utero* exposure to the Mariana catastrophe had a detrimental impact on birth weights, with a more precise estimate for those in their third trimester, as well as increased infant mortality. The adverse effects were stronger for infants born to less educated and single mothers, and for the male offspring. We provide suggestive evidence on different potential mechanisms, especially on the role of water quality. Given that nutritional deprivation in utero can have long-lasting impacts, a possible future expansion of this work could be evaluating the long-term health Banerjee et al. (2010), educational (Greve et al., 2017 and Shah and Steinberg, 2017) and labor market (Majid, 2015) effects on the cohort exposed *in-utero* to the accident.

The main contribution of this study, besides being the first one proposing an assessment of infant health consequences due to the Mariana mining failure – one of the largest environmental disasters of the global mining industry, which largely stemmed from regulatory failure –, lies in endeavoring to provide policy-relevant evidence to the mining debate to avoid future environmental disasters.

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APPENDIX A

WATER AND BIRTH WEIGHT: EVIDENCE FROM A LARGE SCALE PROGRAM IN THE BRAZILIAN SEMIARID REGION

A.1 Data

The paper links three different administrative registries. In order to obtain information on all the family members benefited by the water tanks, we merged the previous information on the head of the family with “Cadastro Único” (CadÚnico), from 2011 to 2017. CadÚnico is the main instrument for the identification and socioeconomic characterization of low-income Brazilian families[1], primarily a census of poor families of the country, with about 80 million registered people. This database serves as the main reference for beneficiary selection and the integration of various social programs administered by the Federal Government.

The CadÚnico must be updated every two years and contain information regarding identification and characterization of the domicile, identification and documentation of each family member. Morever, the registry has information on educational attainment and income. By merging the CadÚnico database and the First Water program registry, we obtained some fundamental information for the study, such as the date of birth of the mother and the child, place of residence, date of tank construction, and some characteristics consistent with the priority of participating in the First Water Program.

CadÚnico considers a “family" all tenants of the same domicile. Even individuals who are not relatives, since split lace and a same household expenses, are considered a family. Moreover, a person who lives by herself is considered a single-member family. The variables from CadÚnico were: family identifier; family member identifier; municipality of residence; full name; date of birth; CPF; NIS; gender; degree of kinship with the head of the family; disability indicator and per capita income. From the date of birth and the date of delivery of the water tanks, it was calculated the age of the individual.

To find the elder siblings within each treated family, we again use the richness from CadÚnico data. First, for each member of our sample, besides the key merge variables as described before (mother’s date of birth, child’s birth date, baby’s gender, and the

[1]Families whose income are up to half the value of the national monthly minimum wage.

municipality of residence), we also maintain the family code variable. Thus, we were able to merge this sample again with the CadÚnico, getting all the siblings of the child benefited during the gestation. For this analysis, we consider those children born between 2000 and 2017.

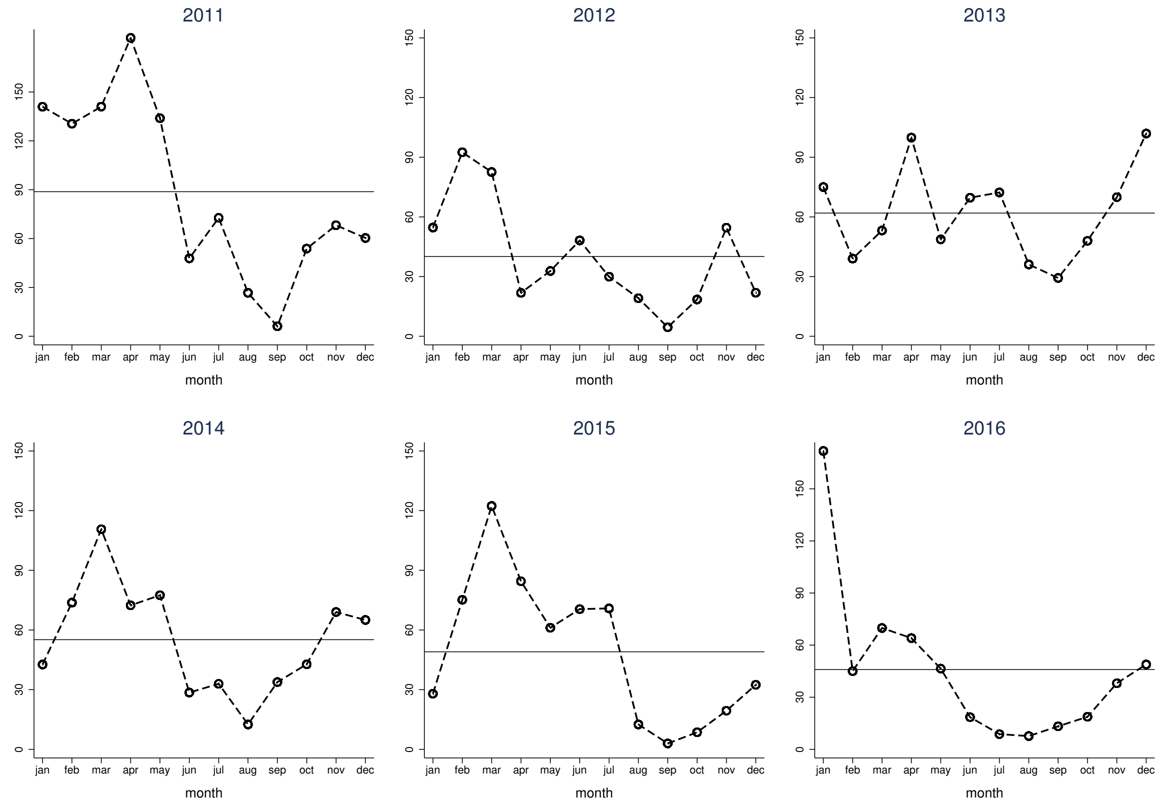
The next step was to merge these children information with SINASC as before, but now using the mother's age variable instead of the mother's birth date. The reason for this is because the mother's date of birth only starts to be collected in the public SINASC data from 2011. Our final sample for this analysis had some attrition, either for the lack of merge or because the child necessarily had an older brother. Based on this sample, keeping the same covariates from the child effectively benefited and treatment variable, we could compare the birth weight of the babies in which the mothers were benefited at the beginning of gestation with those benefited at the end of pregnancy, but now using the birth weight from the immediately elder sibling as outcome variable. We consider in this analysis the immediately elder sibling as the one with no missing in our merge for each family. As a final test to validate this analysis, even with the sample attrition problem, we repeat the same estimation for the sample with the real birth weight value.

Our final sample comprise women whose construction of cisterns happened between the date of conception and the expected date of birth. Figure A5 illustrates two different cases of pregnancy that were included in our sample: (i) the construction of the cistern took place few weeks after the conception and the birth date was in the 39th week of gestation (so within the 280-day window); and (iii) the construction of the cistern took place few days after the birth date, but within the 280-day window after the conception day.

Figure A1: Delimitation of Brazilian semiarid region

Notes. The semiarid region is in beige and comprises 1,262 municipalities.

Figure A2: Monthly Precipitation in the Semiarid: 2011-2016, in mm

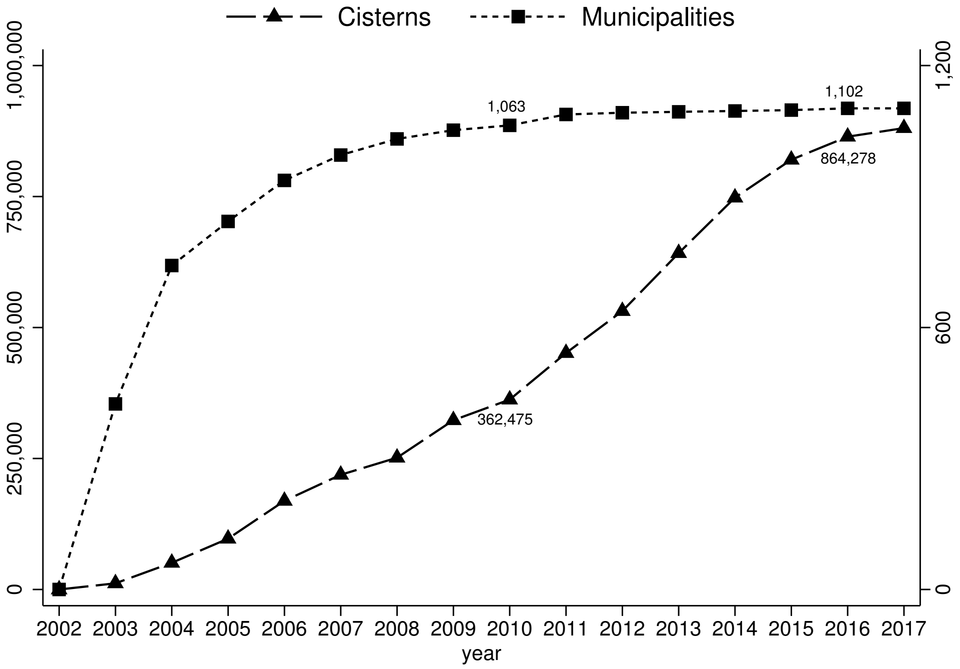


Note: Municipality averages. Author's calculation based on data from the Terrestrial Air Temperature and Terrestrial Precipitation: 1900-2017 Gridded Monthly Time Series, Version 5.01.

Figure A3: Cistern in the Brazilian Semiarid

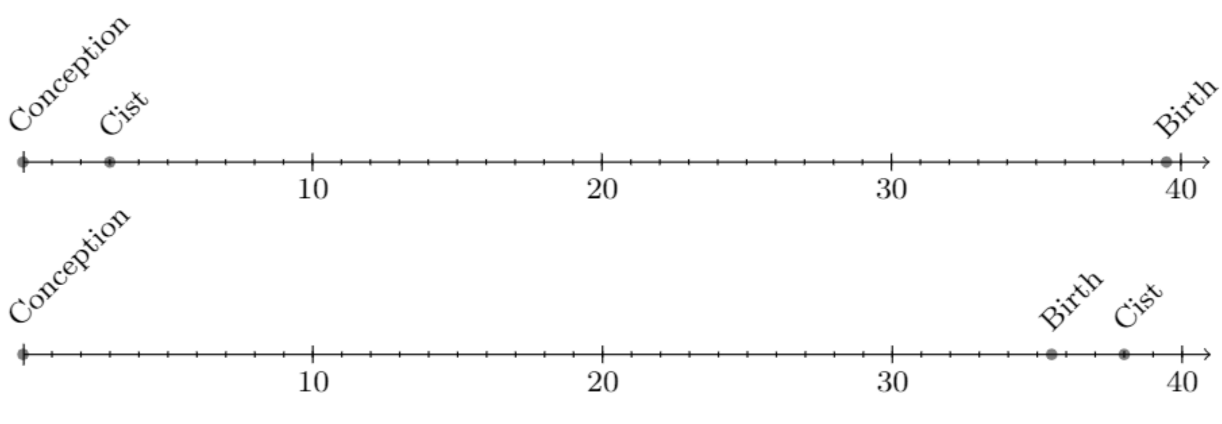
Source: Ministry of Social Development.

Figure A4: Number of cisterns and rollout of the program in municipalities of the semiarid region



Source: MDS

Figure A5: Time window of 40 weeks of gestation after the date of conception.



Notes: The figure reports the sample of pregnant women considered in our analysis.

Table A1: Adoption and educational level

Dependent variable:	Proper use of cistern			Family carries out water treatment			Fecal coliform	
Illiterate householder	-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 interviewee of each family on adoption outcomes. Standard errors clustered at the municipal level.
*** p<0.01, ** p<0.05, * p<0.1.

Table A2: Time cost of collecting water

Round trip to collect water	number of families	%
Before the rainwater tank		
Up to 15 minutes	74	5,6
Between 15 minutes and 1 hour	481	36,2
Between 1 and 2 hours	233	17,5
Above 2 hours	235	17,7
Do not know	299	22.5
Non-responded	6	0.5
After the rainwater tank		
Up to 15 minutes	884	66.6
Between 15 minutes and 1 hour	65	4.9
Between 1 and 2 hours	2	0.2
Above 2 hours	0	0
Do not know	354	26.7
Non-responded	23	1.7
Total	1,328	100

Notes. (EMBRAPA, 2009)

Table A3: Weeks of exposure and PBF in 2011

	(I)	(II)
Dependent Variable:	BFP 2011	Birth Weight
weeks_exposure	0.0004	2.0384**
	(0.0008)	(1.0268)
Month-year fixed effects	Yes	Yes
Municipality fixed effects	Yes	Yes
Controls	Yes	Yes
Observations	3,031	3,031

Notes. This table shows the results of estimating Equation (1.1). 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 illiterate mother, indicator for hospital birth, indicator for newborn sex.
*** p<0.01, ** p<0.05, * p<0.1
For the purpose of this analyses, we merged the BFP Beneficiary Payroll List with the CADÚnico, both for 2011. Since some families in our sample analyses were only created after 2011, we lost some observations in this exercise.

Table A4: Weeks of Exposure and Access to Local Health Facilities

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
Dependent Variables:	# of Hospitals	BHU	ESF	# of Physicians	Hospital Neonatal beds	Hospital beds	Ultrasound machines
weeks_exposure	-0.0000	-0.0000	0.0002	0.0001	-0.0000	-0.0001	-0.0001
	(0.0001)	(0.0002)	(0.0001)	(0.0005)	(0.0001)	(0.0022)	(0.0001)
Month by Year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,208	4,208	4,208	4,208	4,208	4,208	4,208

Notes. This table shows the results of estimating Equation (1.1). 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 illiterate mother, indicator for hospital birth, indicator for newborn sex.
*** p<0.01, ** p<0.05, * p<0.1.

Table A5: Weeks of Exposure and Government Assistance Programs

	(I)	(II)	(III)	(IV)	(V)	(VI)
Dependent variable:	PBF		BPC			
	Families		Elderly		People with special needs	
	quantity	value in $R\$$	quantity	value in $R\$$	quantity	value in $R\$$
weeks_exposure	0.0062	-0.9945	-0.0031	-0.5204	-0.0067	-3.9028
	(0.0355)	(8.8963)	(0.0098)	(6.8581)	(0.0061)	(4.2126)
Month by Year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,208	4,208	4,208	4,208	4,208	4,208

Notes. This table shows the results of estimating Equation (1.1). 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 illiterate mother, indicator for hospital birth, indicator for newborn sex.
*** p<0.01, ** p<0.05, * p<0.1.

Table A6: Results by Educational Attainment

	(I)	(II)	(III)	(IV)	(V)	(VI)
Dependent variable:	Prenatal Visits		Fetal Growth Rate		Weeks of gestation	
	Illiterate mother		Illiterate mother		Illiterate mother	
	Yes	No	Yes	No	Yes	No
weeks_exposure	-0.004	0.001	-0.172	0.054**	0.007	0.003
	(0.004)	(0.001)	(0.131)	(0.026)	(0.021)	(0.004)
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	599	3,435	599	3,445	601	3,445

Notes. This table shows the results of estimating Equation (1.1). 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 illiterate mother, indicator for hospital birth, indicator for newborn sex.
*** p<0.01, ** p<0.05, * p<0.1.

Table A7: Results by Birth Weight Categories

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
Dependent variable:	Birth weight categories						
	> 1000g	> 1500g	> 2000g	> 2500g	> 3000g	> 3500g	> 4000g
weeks_exposure	0.0001*	0.0001	0.0002	0.0007*	0.0005	0.0012*	0.0003
	(0.0001)	(0.0001)	(0.0002)	(0.0004)	(0.0007)	(0.0006)	(0.0003)
Observations	4,054	4,054	4,054	4,054	4,054	4,054	4,054
Month by Year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes. This table shows the results of estimating Equation (1.1). 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 illiterate mother, indicator for hospital birth, indicator for newborn sex.
*** p<0.01, ** p<0.05, * p<0.1

Table A8: Robustness: Trimming the Tails of the Sample

	Dependent variable: Birth Weight (g)					
	(I)	(II)	(III)	(IV)	(V)	(VI)
	Bot 1%	Top 1%	Bot/Top 1%	Bot 2%	Top 2%	Bot/Top 2%
weeks_exposure	1.475**	1.598**	1.347**	1.365**	1.637**	1.285**
	(0.705)	(0.700)	(0.657)	(0.693)	(0.691)	(0.632)
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	4,007	4,011	3,964	3,970	3,965	3,881

Notes. This table shows the results of estimating Equation (1.1). 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 illiterate mother, indicator for hospital birth, indicator for newborn sex.
*** p<0.01, ** p<0.05, * p<0.1.

Table A9: Robustness: Additional Intervals after the Date of Conception

	41 weeks					42 weeks				
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)	(IX)	(X)
Dependent variables:	Birth Weight	ln Birth Weight	Fetal Growth Rate	Low Birth Weight	Weeks of Gestation	Birth Weight	ln Birth Weight	Fetal Growth Rate	Low Birth Weight	Weeks of Gestation
weeks_exposure	1.541**	0.001**	0.033**	-0.000	0.003	1.435***	0.000**	0.036**	-0.000	0.000
	(0.620)	(0.000)	(0.017)	(0.000)	(0.002)	(0.549)	(0.000)	(0.014)	(0.000)	(0.002)
Month-year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,683	5,683	5,683	5,683	5,686	6,632	6,632	6,632	6,632	6,636
	43 weeks					44 weeks				
	(XI)	(XII)	(XIII)	(XIV)	(XV)	(XVI)	(XVII)	(XVIII)	(XIX)	(XX)
Dependent variables:	Birth Weight	ln Birth Weight	Fetal Growth Rate	Low Birth Weight	Weeks of Gestation	Birth Weight	ln Birth Weight	Fetal Growth Rate	Low Birth Weight	Weeks of Gestation
weeks_exposure	1.489***	0.000***	0.040***	-0.000	-0.001	1.674***	0.001***	0.044***	-0.000	-0.000
	(0.512)	(0.000)	(0.013)	(0.000)	(0.002)	(0.481)	(0.000)	(0.013)	(0.000)	(0.002)
Month-year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,100	7,100	7,100	7,100	7,104	7,394	7,394	7,394	7,394	7,399

Notes. This table shows the results of estimating Equation (1.1). 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 illiterate mother, indicator for hospital birth, indicator for newborn sex.
*** p<0.01, ** p<0.05, * p<0.1.

Table A10: Robustness to Alternative Clustering

	Dependent variable: Birth Weight (g)							
	Baseline	Quarter-Year	Conley spatial clustering cutoff distances in kilometers:					
	Result	by Munic.	50 km	100 km	150 km	200 km	250 km	500 km
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
weeks_exposure	1.741**	1.741**	1.741**	1.741***	1.741***	1.741***	1.741***	1.741***
	(0.754)	(0.756)	(0.678)	(0.675)	(0.662)	(0.665)	(0.671)	(0.618)
Month-year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,054	4,054	4,203	4,203	4,203	4,203	4,203	4,203

Notes. Column (I) presents the baseline result reported in column (II) of Tables 1.4. Columns (II) uses cluster by quarter of year × municipality. Columns (III)-(VIII) reports 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 A11: Bounding the Treatment Effect: Oster (2017)

Parameter	R_{max}			
	0.7	0.8	0.9	1
Bounding Set with $\delta = 1$	[1.582, 1.741]	[1.582, 1.741]	[1.582, 1.741]	[1.582, 1.741]
Value of δ for $\beta = 0$	17.735	14.630	12.450	10.836

Notes. This table shows the results of estimating Equation (1.1). 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 illiterate mother, indicator for hospital birth, indicator for newborn sex. See Oster (2017)
*** p<0.01, ** p<0.05, * p<0.1.

APPENDIX B

ENVIRONMENTAL DISASTERS AND INFANT HEALTH: EVIDENCE FROM THE MARIANA MINING DISASTER IN BRAZIL

B.1 Data and Control Group

Data sources. In the paper, we make use of different datasets to assess the health consequences of the Mariana disaster in Brazil. We use (publicly available) individual-level birth records provided by the System of Information on Live Births (SINASC) from 2011 to 2016. The vital registration data include information on the pregnancy (birth weight, gestational length), the delivery (C-section, multiple births, sex) and the mother (age, educational attainment, marital status, municipality of residence). SINASC dataset provides the date of the last menstrual period – based on medical information obtained from doctors via physical examination or other clinical methods – that in this study is considered to be the date of conception. We aim to avoid the influence of endogenous fertility, so we limit our period of study until 2016 to only include mothers who made their fertility decisions before the event.

We also use publicly available microdata from the System of Information on Mortality (SIM) to obtain information on infant mortality from 2011 to 2017. SIM's death certificates contain information on natural and non-natural infant deaths, including (i) the detailed cause of death, (ii) characteristics of the deceased, and (iii) selected mothers' characteristics. For the cases in which the infant mortality occurs within the first year after birth, we linked this information to the birth registry data via a unique birth identifier.[1] This merge allows us to assign the period of conception for those in the death register.

In addition, we use data from different sources to provide suggestive evidence on the role of selective migration, since we only have data on population movements from the 2010 Brazilian Population Census. The number of conceived infants stems from SINASC and SIM datasets. Conceived infants correspond to the sum of live births and fetal deaths. To carry out the analysis, we aggregate SINASC and SIM data from 2011 to 2016 at the municipality-by-month level. The additional cohort size variable that we use is the

[1] Since some unique personal identifiers are missing in SIM dataset, the matching rate is 90.63% when we use data from the state of Minas Gerais, where our sample of pregnant women is located.

number of students enrolled per municipality in 2011–2018. Enrollment data stem from the Brazilian education census, an annual publicly available survey with all enrollment records in the country.

Finally, to investigate the role of water pollution as a potential mechanism through which mothers are affected, we use two sets of data. First, we obtain data on water quality obtained from the Institute of Water Management of Minas Gerais (IGAM). IGAM provides information on water quality parameters such as total dissolved solids and turbidity. For the years 2013-2016, a total of 32 water quality samples (16 samples per parameter) were collected from a single station (hidro code 56338010) of Rio Doce basin in the state of Minas Gerais — 8 samples were measured before the environmental disaster and 24 samples were measured after the dam collapsed. The second data we use comes from the SUS Hospital Admissions System (Sistema de Internações Hospitalares – SIH-SUS). This system, managed by the Brazilian Ministry of Health, is the government's official registry to every patient admission in Brazilian public hospitals, covering virtually all of the country's territory.

Sample and descriptive statistics. The unit of analysis is the mother-child pair, and our final sample contains 100,704 observations. Individuals in our sample live in municipalities directly affected by the disaster (treatment group) or in municipalities in the zone of influence of "similar dams" that did not suffer a failure (comparison group).

More precisely, our treatment group consists of women living in municipalities affected by the disaster who were exposed to the tragedy during their pregnancy. The women in the treated group lived in 36 municipalities in the state of Minas Gerais that were in the mud path of the Mariana dam disaster. Figure B2 shows the location of the municipalities affected by the disaster (in brown). To assign exposure to the disaster, we create a fixed window of 280 days since conception date, irrespective of gestational length — in contrast to using birth dates to measure exposure. This definition of exposure aims to avoid problems that can arise if the gestational duration is affected by the disaster. We also assign the trimester of exposure based on each 93-day window ([0,3] months, (3,6] months, (6,9] months).

The baseline comparison group is composed of pregnant women living in the most affected municipalities in simulated dam failures. We obtained data on other dams located in Minas Gerais state, built in the same years and with similar technology as the Mariana dam. We simulate the mud path for all such dams as if they had suffered a similar failure. We provide more details on the construction of the baseline control group below.

Table B1 presents selected characteristics of our sample of pregnant women. The relevant characteristics are our main outcome variables: birth weight and infant death. Columns (1)-(3) of Table B1 show the number of observations, the mean, and standard deviation of each characteristic.

Baseline comparison group. We now detail the construction of the baseline comparison group. In January 2019, another large tailings dam failure happened in Brumadinho, Brazil, which killed more than 250 people. Brumadinho is located in the state of Minas Gerais. In the aftermath of the Brumadinho mining disaster, the National Institute for Space Research (INPE) applied the HAND model (Height Above the Nearest Drainage) as a tool for flood mapping.[2] Using this model and its calibration by INPE (for the Brumadinho disaster), we created (simulated) flood hazard mappings for 40 "similar dams" located in 15 municipalities in the state of Minas Gerais.[3] For "similar dams" we mean dams built in the same years and with similar technology as the Mariana dam.

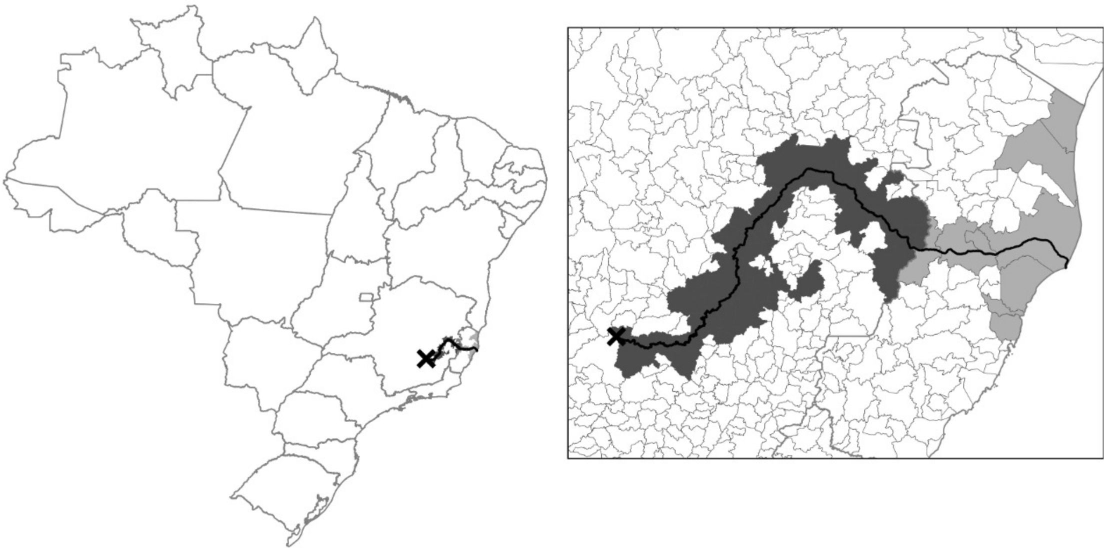
Based on the abovementioned calibration, we simulated the mud path for all such "similar dams" had they experienced a similar failure. Hence, we were able to pinpoint 31 municipalities that are considered affected by our simulations. We exclude two sets of municipalities that were flagged as affected in our simulated mud paths: (i) municipalities located in the metropolitan region of the state capital Belo Horizonte, and (ii) those that were affected by the Mariana disaster. Additionally, for each simulated path we only include the five most affected municipalities to increase comparability and reduce the risk of differential trends driven by other factors.

Figure B2 shows the location of the municipalities in the treatment and baseline comparison groups. In the figure, the location of the Mariana dam is indicated by an "X". The brown area shows the flooded region, and the blue area depicts the baseline comparison group (i.e., the five most affected municipalities for each dam with the same technology and with similar age).

[2] Flood hazard maps based on digital elevation models (DEM) and geomorphic features (e.g., slope, distance to the nearest divide, and topographic index) have been developed as a low-cost alternative in the absence of detailed hydrological and hydraulic data and for regions of large territorial extent. The HAND model is a terrain descriptor that calculates the difference in elevation of each pixel and its nearest drainage point. See Nobre et al. (2011) for more details on the HAND model.

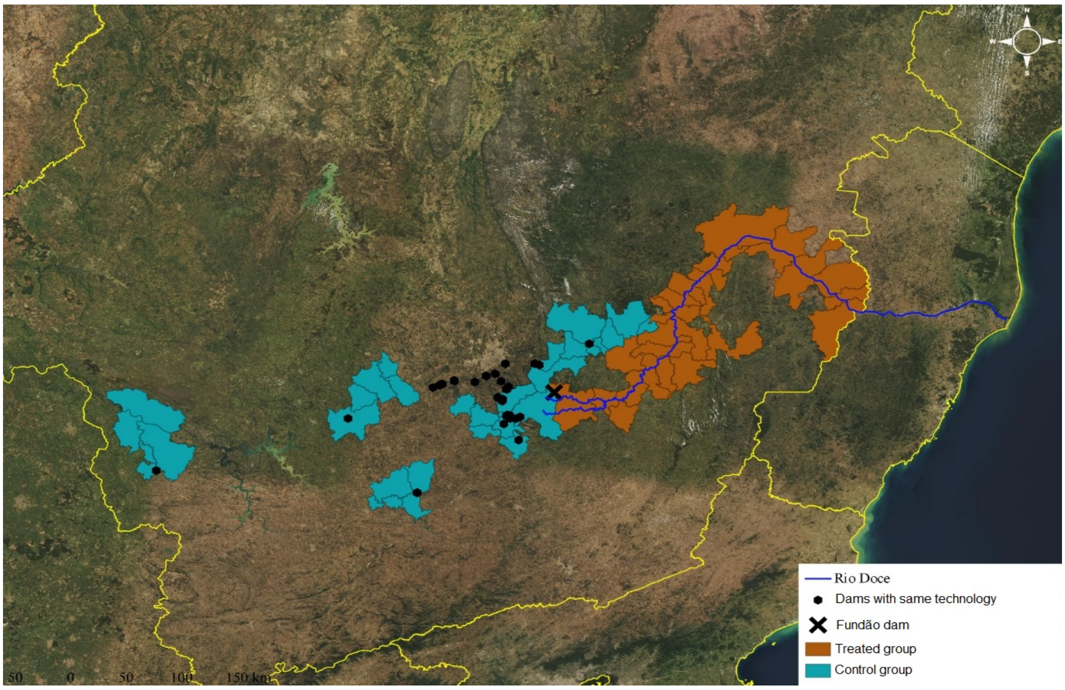
[3] The 15 municipalities are: Barão de Cocais; Bela Vista de Minas; Brumadinho; Caeté; Congonhas; Conselheiro Lafaiete; Fortaleza de Minas; Igarapé; Itabirito; Itapecerica; Itatiaiuçu; Nazareno; Nova Lima; Ouro Preto and Rio Acima.

Figure B1: Mariana dam collapse



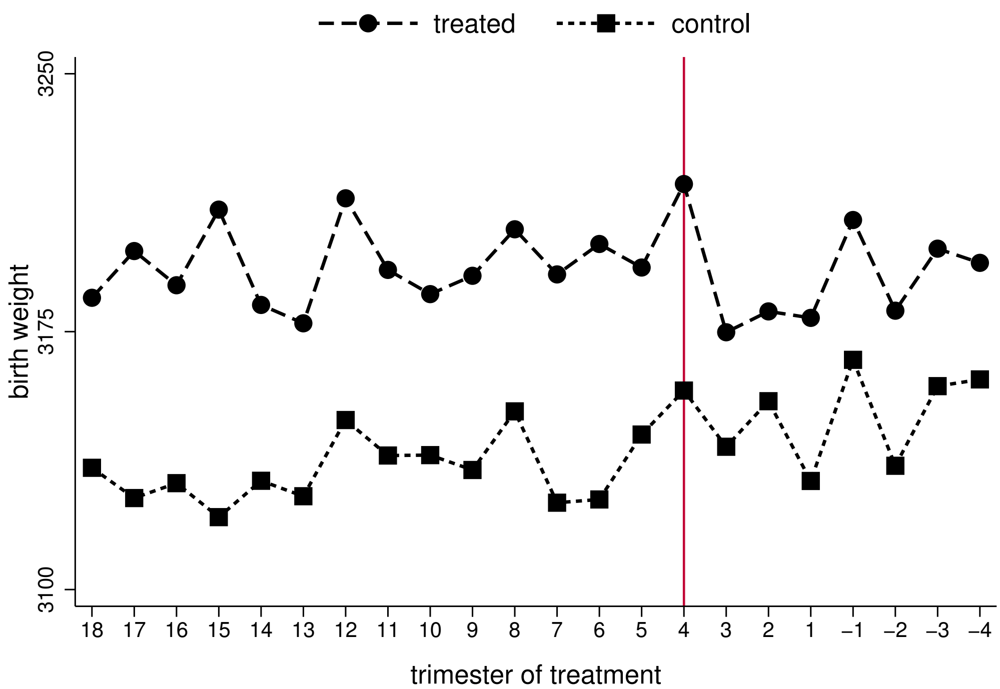
Notes. All municipalities affected by the Mariana dam disaster are in gray. Municipalities in our treated group – that are located in Minas Gerais – are displayed in darker gray, while other municipalities affected by the disaster are in light gray.

Figure B2: Municipalities in our sample



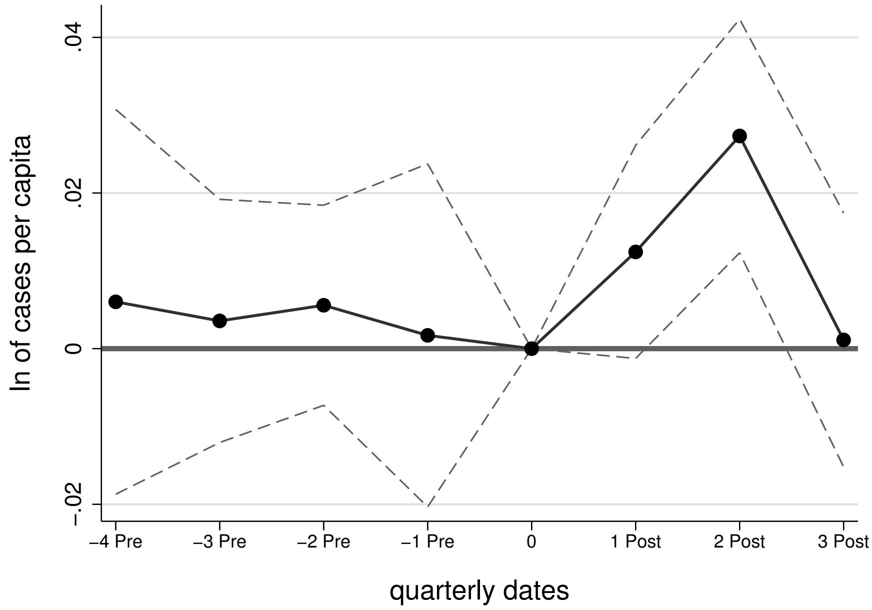
Notes: The figure shows the location of the municipalities in our final sample. The location of the Mariana dam is indicated by an “X". The 36 municipalities in the treatment group are in brown, while the 31 municipalities in our baseline comparison group are in blue. The 36 treated municipalities are: Aimorés; Alpercata; Barra Longa; Belo Oriente; Bom Jesus do Galho; Bugre; Caratinga; Conselheiro Pena; Córrego Novo; Dionísio; Fernandes Tourinho; Galiléia; Governador Valadares; Iapu; Ipaba; Ipatinga; Itueta; Mariana; Marliéria; Naque; Periquito; Pingo d’Água; Ponte Nova; Raul Soares; Resplendor; Rio Casca; Rio Doce; Santa Cruz do Escalvado; Santana do Paraíso; São Domingos do Prata; São José do Goiabal; São Pedro dos Ferros; Sem-Peixe; Sobrália; Timóteo; Tumiritinga. The 31 municipalities in the simulated path comparison group are: Antônio Dias; Barão de Cocais; Bela Vista de Minas; Belo Vale; Bom Sucesso; Bonfim; Carmo do Cajuru; Cássia; Congonhas; Conselheiro Lafaiete; Delfinópolis; Divinópolis; Fortaleza de Minas; Ibituruna; Ijaci; Itabira; Itabirito; Itapecerica; Itaú de Minas; Jaguaraçu; Jeceaba; João Monlevade; Nazareno; Nova Era; Ouro Preto; Passos; Santa Bárbara; São Gonçalo do Pará; São Gonçalo do Rio Abaixo; São Sebastião do Oeste; São Tiago.

Figure B3: Parallel trends on birth weight



Notes: The picture shows the evolution of average birth weight in the 36 municipalities affected by the dam collapse vis-à-vis the average birth weight in the 31 municipalities in the simulated path comparison group.

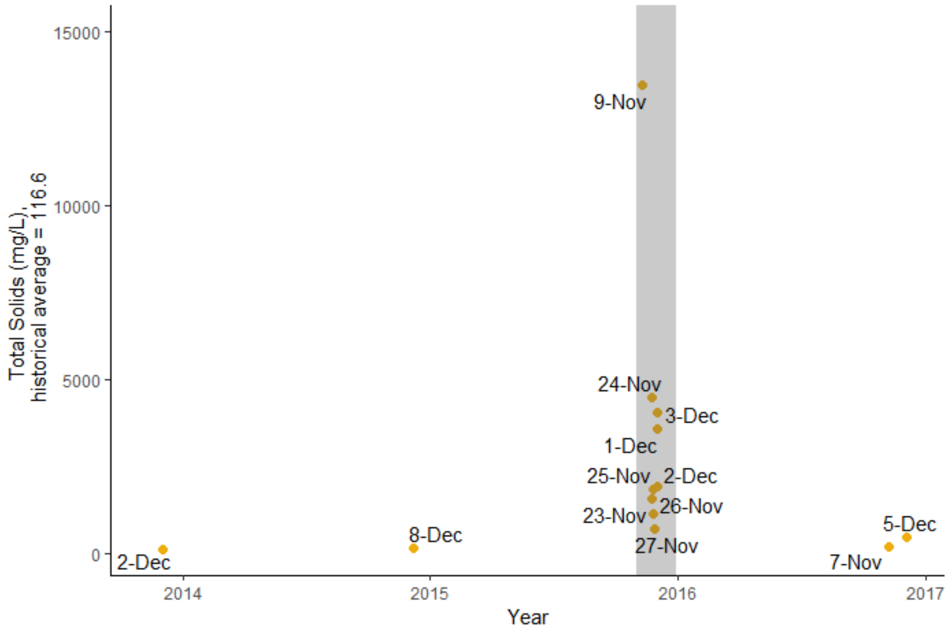
Figure B4: Estimated effects of dam collapse on hospitalization by diarrheal diseases in women



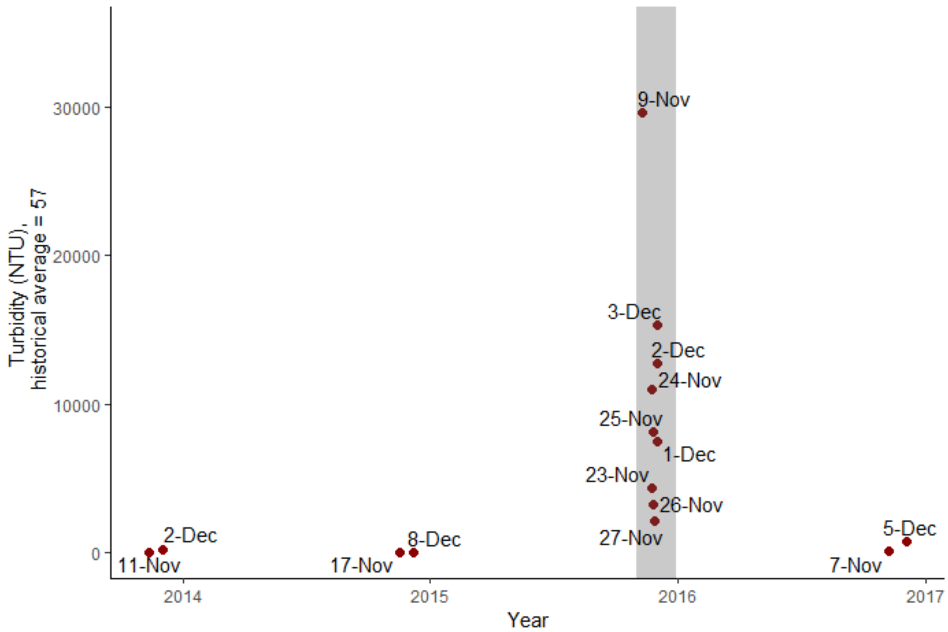
Notes. This figure plots the coefficients of the event-study estimates using Equation (2.3) and eight periods around the treatment. The trimester immediately prior to the disaster (August-October 2015) is used as the reference period. Dashed lines show 95 percent confidence intervals. Estimation includes time and municipality fixed effects. Standard errors clustered at the municipal level. Diarrheal diseases disorders due to the use of psychoactive substance correspond to the ICD A00-A09.

Figure B5: Water quality

(a) Total Solids (mg/L) variation

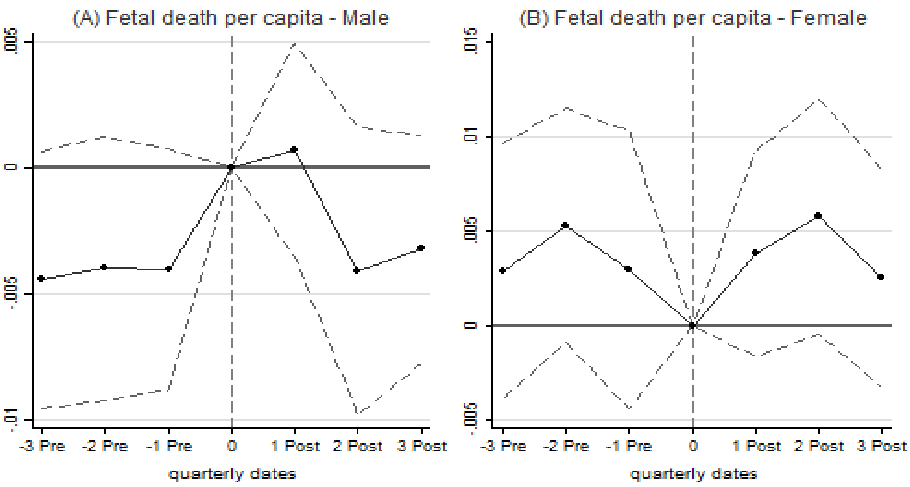


(b) Turbidity (NTU) variation



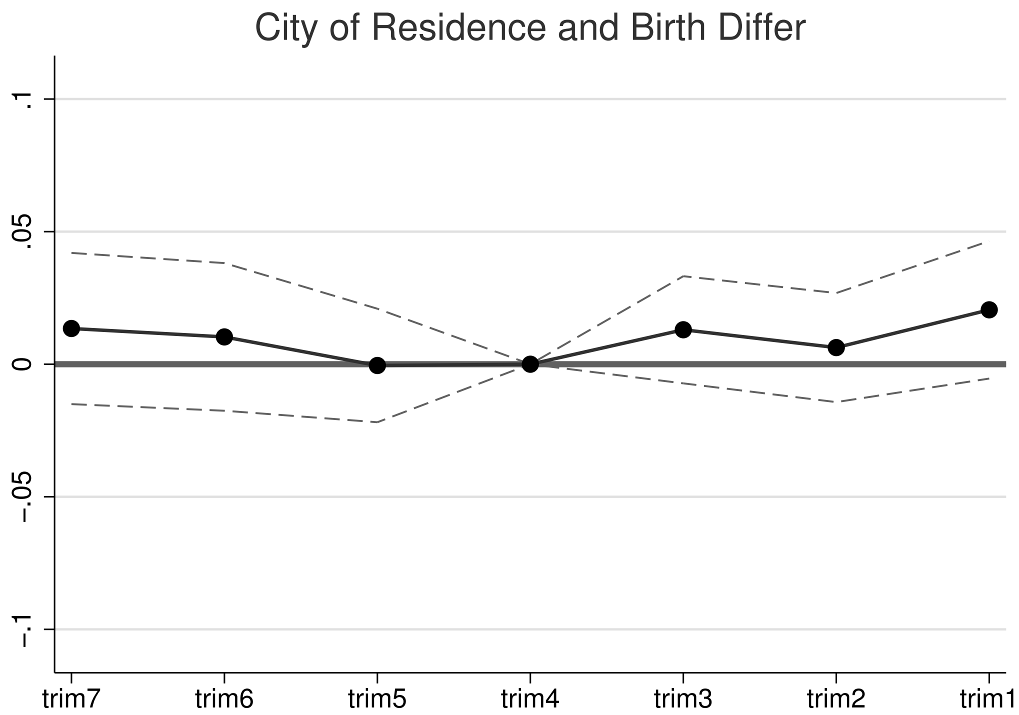
Notes. Data on water quality were obtained from the Institute of Water Management of Minas Gerais (IGAM) using different water quality parameters such as total dissolved solids and turbidity. During 2013-2016, a total of 32 water quality samples, i.e., 16 samples per parameter, were collected from a single station (hidro code 56338010) of Rio Doce basin in the state of Minas Gerais, including 8 samples measured before the environmental disaster, and 24 samples, measured after the dam colapsed in November 5, 2015. For comparability, we censored data points for November and December of each year.

Figure B6: Fetal Death by Gender



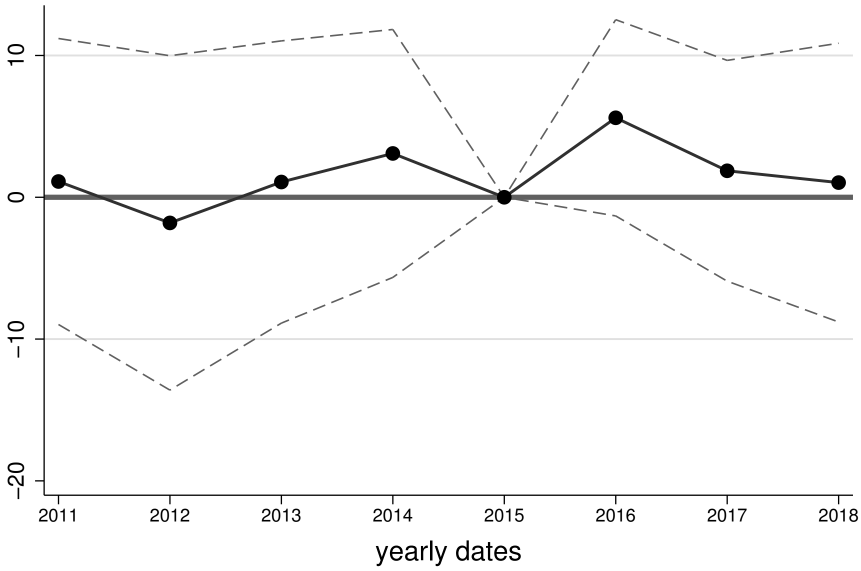
Notes. This figure plots the coefficients a event-study approach using monthly data. The trimester immediately prior to the disaster (August-October) is used as the reference period. Dashed lines show 95 percent confidence intervals. Estimation includes trimester relative to the accident date fixed effects and municipality fixed effect. Standard errors clustered at the municipal level.

Figure B7: Estimated effects of dam collapse on migration



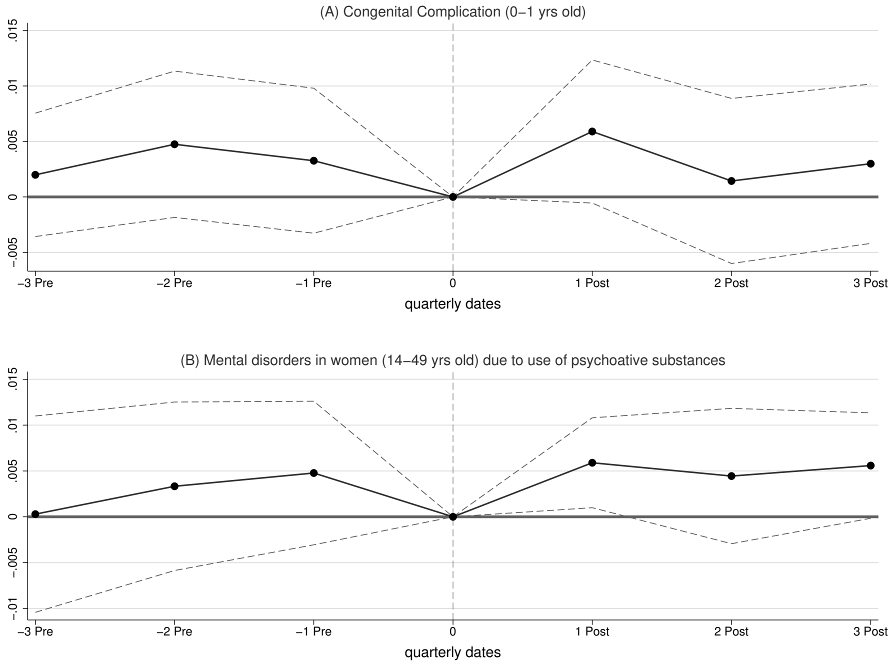
Notes. This figure plots the coefficients of Equation (2.3) for a setting of seven trimesters. The trimester immediately prior to the disaster (trim4) is used as the reference period. Dashed lines show 95 percent confidence intervals.

Figure B8: Number of students per 1,000 inhabitants



Notes. This figure plots the coefficients of the event-study estimates using Equation (2.3) of the paper and eight periods around the treatment. The figure uses yearly data for the number of students enrolled in schools in B8 (where 2015 is the base year). The dependent variable in Figure B8 is the number of students enrolled per municipalities. Estimation includes time fixed effects and municipality fixed effect. Standard errors are clustered at the municipal level. Dashed lines show 95 percent confidence intervals.

Figure B9: Estimated effects of dam collapse on other hospitalization outcomes



Notes. This figure plots the coefficients a event-study approach using monthly data. The trimester immediately prior to the disaster (August-October) is used as the reference period. Dashed lines show 95 percent confidence intervals. Estimation includes trimester relative to the accident date fixed effects and municipality fixed effect. Standard errors clustered at the municipal level. Mental disorders due to the use of psychoactive substance correspond to the ICD F19, and congenital complications correspond to the ICD from Q00 to Q99.

Table B1: Descriptive Statistics

	Observations	Mean	Std. Dev.
	(1)	(2)	(3)
Birth weight (in grams)	100,704	3,165	519.6
Infant death	100,704	0.00615	0.0782
Weeks of gestation	100,704	38.87	2.227
Fetal growth ratio	100,704	81.35	12.72
Low birth weight	100,704	0.0789	0.270
Weeks of gestation < 28	100,704	0.00454	0.0672
C-section	100,176	0.628	0.483
APGAR 1	98,204	8.524	1.244
APGAR 5	98,344	9.385	0.856
Mother's age	100,704	27.28	6.479
Multiple birth	100,249	0.022	0.145
Mother's education $\geq$ 12 yrs	97,288	0.180	0.384
Married	99,282	0.525	0.499
Female baby	100,679	0.487	0.500

Notes. The sources of the individual-level characteristics are the publicly available SINASC and SIM data from the Brazilian Ministry of Health. Fetal growth corresponds to the birth weight divided by the number of weeks of gestation. The World Health Organization (WHO) defines low-birth-weight as weight under 2,500g. C-section stands for Cesarean delivery. The APGAR score measures the vital signs of the newborn in the immediate extra-uterine life, assigned to the newborn infant at one (APGAR 1) and five (APGAR 5) minutes after birth.

Table B2: Estimated effects of dam collapse on mother's characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	mother's age <= 20		mother's age >= 40		mother's educ. >= 12		married	
Exposure (any trim.)	0.001		0.001		0.008		-0.000	
	(0.010)		(0.003)		(0.006)		(0.007)	
3^{rd}trim.		0.008		-0.001		0.005		0.000
		(0.013)		(0.003)		(0.008)		(0.010)
2^{rd}trim.		-0.007		0.004		0.005		-0.009
		(0.011)		(0.004)		(0.009)		(0.011)
1^{rd}trim.		0.002		-0.001		0.014*		0.008
		(0.009)		(0.004)		(0.007)		(0.010)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month and year of conception FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	96,089	96,089	96,089	96,089	96,089	96,089	96,089	96,089

Notes. This table shows the results of estimating Equation (2.1). Standard errors clustered at the municipal level.
*** p<0.01, ** p<0.05, * p<0.1.

Table B3: Estimated effects of dam collapse by birth weight categories and gender

VARIABLES	Incidence in birth weight categories											
	< 1500g		< 2000g		< 2500g		< 3000g		< 3500g		< 4000g	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Panel A - male											
Exposure (any trimester)	0.002		0.005		0.007		0.030**		0.011		0.009	
	(0.002)		(0.004)		(0.006)		(0.012)		(0.016)		(0.008)	
3^{rd}trim.		0.006		0.007		0.010		0.044***		0.023		0.008
		(0.004)		(0.006)		(0.006)		(0.013)		(0.020)		(0.012)
2^{nd}trim.		-0.000		0.004		0.002		0.027		0.015		0.016***
		(0.003)		(0.006)		(0.008)		(0.017)		(0.016)		(0.006)
1^{st}trim.		0.000		0.004		0.009		0.019		-0.005		0.002
		(0.003)		(0.006)		(0.011)		(0.017)		(0.020)		(0.009)
Number of obs.	49,214	49,214	49,214	49,214	49,214	49,214	49,214	49,214	49,214	49,214	49,214	49,214
	Panel A - female											
Exposure (any trimester)	0.002		0.001		0.003		0.007		0.012		-0.001	
	(0.003)		(0.003)		(0.005)		(0.015)		(0.013)		(0.004)	
3^{rd}trim.		0.001		-0.001		0.001		-0.000		0.014		0.000
		(0.005)		(0.005)		(0.009)		(0.011)		(0.010)		(0.004)
2^{nd}trim.		0.003		-0.001		-0.003		0.007		0.002		0.001
		(0.004)		(0.005)		(0.007)		(0.029)		(0.024)		(0.006)
1^{st}trim.		0.002		0.006*		0.011		0.015		0.020*		-0.005
		(0.003)		(0.004)		(0.008)		(0.016)		(0.011)		(0.006)
Number of obs.	46,875	46,875	46,875	46,875	46,875	46,875	46,875	46,875	46,875	46,875	46,875	46,875
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month and year of conception FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	96,089	96,089	96,089	96,089	96,089	96,089	96,089	96,089	96,089	96,089	96,089	96,089

Notes. Control variables are: indicator for married mothers, newborn sex, high education mothers (12 or more years of schooling), and dummies for mother's age above 40 years old and below 20 years old. Standard errors clustered at the municipal level. ***p<0.01, **p<0.05, *p<0.1.

Table B4: Other outcomes: Pre-term births, neonatal and post neonatal mortality

	Weeks of gestation smaller than 37		Neonatal death		Post neonatal death	
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure (any trim.)	-0.001		0.003**		0.002**	
	(0.006)		(0.001)		(0.001)	
3^{rd}trim.		0.002		0.005**		0.002
		(0.006)		(0.002)		(0.001)
23^{rd}trim.		-0.011		0.003***		0.002**
		(0.006)		(0.001)		(0.001)
1^{rd}trim.		0.005		0.001		0.000
		(0.008)		(0.002)		(0.001)
Observations	100,704	100,704	100,704	100,704	100,704	100,704

Notes. The odd-numbered columns present results from the estimation of Equation (2.1), while the even-numbered columns present results partitioning the indicator **exposure** of Equation (2.1) into three dummy variables indicating exposure in the first, second and third trimesters of pregnancy. Infant mortality outcomes in columns (3)–(6) are neonatal mortality (first month of life) and post-neonatal mortality (second to the twelfth month of life). All regressions include municipality, year and month of conception fixed effects, and control variables (the same variables as in Table 2.2). Standard errors clustered at the municipal level. ***p<0.01, **p<0.05, *p<0.1.

Table B5: Robustness: Trimming the Tails of the Sample

	Dependent variable: Birth Weight (g)					
	Bot 1%		Top 1%		Bot/Top 1%	
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure (any trim.)	-19.949		-22.781*		-19.154	
	(14.498)		(12.180)		(12.994)	
3^{rd}trim.		-25.600**		-32.838***		-26.035***
		(11.911)		(9.295)		(9.329)
2^{rd}trim.		-16.602		-11.835		-8.782
		(18.181)		(16.423)		(18.011)
1^{rd}trim.		-17.812		-24.092		-23.002
		(17.500)		(18.545)		(15.866)
Number of obs.	95,128	95,128	95,115	95,115	94,154	94,154
	Bot 2%		Top 2%		Bot/Top 2%	
	(7)	(8)	(9)	(10)	(11)	(12)
Exposure (any trim.)	-19.570		-22.518*		-18.492	
	(15.375)		(12.097)		(13.700)	
3^{rd}trim.		-26.685**		-34.204***		-28.472***
		(12.968)		(9.705)		(9.999)
2^{rd}trim.		-16.964		-10.246		-7.564
		(19.201)		(17.181)		(19.777)
1^{rd}trim.		-15.230		-23.601		-19.875
		(17.876)		(18.184)		(15.792)
Number of obs.	94,177	94,177	94,197	94,197	92,285	92,285
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Month and year of conception FE	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes

Notes. This table shows the results of estimating Equation (2.1). Standard errors clustered at the municipal level.
*** p<0.01, ** p<0.05, * p<0.1.

Table B6: Robustness to Alternative Clustering

	Baseline	Quarter-Year	Conley spatial clustering cutoff distances in kilometers:					
	Result	by Munic.	50 km	100 km	150 km	200 km	250 km	500 km
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
3^{rd}trim.	-32.360***	-32.360***	-32.360***	-32.360***	-32.360***	-32.360**	-32.360**	-32.360**
	(9.668)	(3.574)	(8.994)	(11.484)	(12.440)	(12.812)	(13.190)	(13.198)
Observations	96,089	96,089	96,089	96,089	96,089	96,089	96,089	96,089
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month and year of conception FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes. Column (1) presents the baseline result reported in column (2) of Tables 1.4. Columns (2) uses cluster by quarter of year × municipality. Columns (3)-(8) reports 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 B7: Estimated effects of dam collapse on demand for doctor and nurses visits

	(1)	(2)	(3)	(4)	(5)
VARIABLES	doctor visits	doctor visits 0-1	Prenatal visits	Prenatal visits by physicians	Prenatal visits by nurses
treat	6.407	13.193	0.120	2.268	-2.148
	(17.620)	(14.746)	(2.819)	(1.652)	(1.747)
Bimonth of year FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
Observations	1,206	1,206	1,206	1,206	1,206

Notes. Dependent variable in each column is measured per 1,000 residents. This table shows the results of estimating time of disaster on demand for medical care. This estimation follow the same structure as in the Table 2.9, but using bimonthly data from Carrillo and Feres (2019). Standard errors clustered at the municipal level.
*** p<0.01, ** p<0.05, * p<0.1.

Table B8: Robustness to different measures of prenatal exposure to the disaster

	Birth weight		Weeks of gestation		Infant death	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Using gestational age to measure exposure						
Exposure (any trim.)	-22.643*		-0.054		0.004***	
	(12.860)		(0.042)		(0.001)	
3^{rd}trim.		-27.600**		-0.008		0.007***
		(10.976)		(0.053)		(0.002)
2^{rd}trim.		-22.718		-0.069		0.005***
		(15.559)		(0.050)		(0.002)
1^{rd}trim.		-17.916		-0.081		0.001
		(18.746)		(0.053)		(0.002)
Number of obs.	100,642	100,642	100,642	100,642	100,642	100,642
Panel B: Calculating exposure backward from date of birth						
Exposure (any trim.)	-27.031**		-0.084**		0.003**	
	(13.023)		(0.034)		(0.001)	
3^{rd}trim.		-34.193***		-0.142***		0.004**
		(10.161)		(0.052)		(0.002)
2^{rd}trim.		-35.665		-0.074		0.005***
		(21.840)		(0.048)		(0.002)
1^{rd}trim.		-7.002		-0.006		-0.001
		(19.734)		(0.048)		(0.002)
Number of obs.	100,631	100,631	100,631	100,631	100,631	100,631

Notes. The odd-numbered columns present results from the estimation of Equation (2.1), while the even-numbered columns present results partitioning the indicator **exposure** of Equation (2.1) into three dummy variables indicating exposure in the first, second and third trimesters of pregnancy. Panel (a) uses gestational age to measure prenatal exposure to the disaster, while panel (b) calculates exposure backward from the date of birth. All regressions include municipality, year and month of conception fixed effects, and control variables (the same control variables listed in Table 2.2). Standard errors clustered at the municipal level. ***p<0.01, **p<0.05, *p<0.1.