Access to treated water in utero and childhood well-being: Evidence from rural China

Li Li and Yun Xiao *

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Abstract

This paper examines the impacts of in utero access to treated water on childhood health. We exploit the variation in the timing of tap water connection across communities imposed by a major drinking water program in rural China. Using data extracted from the China Family Panel Studies 2010, we find that prenatal access to treated water significantly raises 2-11 year-old children's height by 0.239 standard deviations and reduces the probability of at least three doctor visits the previous year by 9.0 percentage points. The height increase is likely to be more for children of less educated mothers. Event study estimates confirm that prenatal period is the crucial window for the impact on health. Mechanism analysis suggests that the health benefits may stem from the improvement in drinking water quality, rather than the increase in water quantity.

Keywords: Water infrastructure; Treated water; In utero; Childhood health; Rural China

*Li: Institute of Water Policy, Lee Kuan Yew School of Public Policy, National University of Singapore, Singapore 259770, sppll@nus.edu.sg. Xiao: Tinbergen Institute, the Netherlands, y.xiao@tinbergen.nl.

1 Introduction

It is believed that environmental factors during pregnancy can have significant impacts on the developing fetus, and consequently health at birth.[1] One potentially important environmental factor that pertains to fetal development is the availability of safe drinking water during pregnancy. In utero exposure to contaminated water has been found to have large and statistically significant effects on birth weight and gestation of infants born to less educated mothers in New Jersey (Currie et al., 2013). In the Brazilian semiarid, water scarcity due to negative rainfall shocks is robustly correlated with higher infant mortality, lower birth weight, and shorter gestation periods, and such negative effects can be greatly minimized if local sanitation and water infrastructure is sufficiently developed (Rocha and Soares, 2015). A number of studies, mainly in developing countries, show that providing households with better access to safe drinking water may significantly reduce mortality and improve health of newborns and infants (Merrick, 1985; Lee et al., 1997; Galiani et al., 2005; Gamper-Rabindran et al., 2010).

In this study, we explore how improved access to treated water in utero impacts health in early and middle childhood.[2] Our study is set in rural China, where substantial improvement in access to safe drinking water has been made over the past few decades. Specifically, we exploit the variation in the implementation of a large-scale drinking water safety program in rural China across communities over time. The Chinese government launched the program in 1984, aiming to improve the accessibility of safe drinking water in rural areas. The major part of the program is to build water infrastructure, including water plants with water purification technology and equipment and pipelines, to supply treated tap water to rural residents. Since the launch of the program, great progress in access to treated water has been made. Between 1990 and 2015, the proportion of rural households with access to on-premise piped water increased

[1]Recent studies, mostly in developed countries and a few in developing countries, have offered causal evidence that exposure to risk factors such as pollution and maternal smoking has negative effects on birth weight and length of gestation. On the other hand, mothers' access to social programs such as the Supplemental Feeding Program for Women, Infants, and Children (WIC) and Food Stamps in the United States has been found to increase birth weight. See Almond and Currie (2011b), Currie (2009) and Currie (2011) for more detailed surveys of this literature.

[2]Following the literature of developmental psychology, a person's life-cycle is divided into the following stages: in utero (conception to birth), early childhood (birth to age 6), middle childhood (age 7 to age 11), adolescence (age 12 to age 17) and adulthood (age 18 to death) (Piaget, 1976). By saying access to treated water in utero, we mean mothers' access while the children are in utero.

by 45 percentage points, from 10% to 55% (WHO/UNICEF, 2015).

There are several significant implications for the understanding of the impacts of in utero access to treated water on childhood health in a developing country context. Although a rising literature has shown that fetal conditions and health at birth can have lasting consequences for health and other dimensions of human capital (Almond and Currie, 2011b), there are few attempts to causally link in utero access to safe water with childhood health, an important predictor of future educational and labor market outcomes (Currie, 2009). Furthermore, in all developing regions access to improved drinking water sources increases with wealth, and access to piped water on premises is much higher among the richest (UNICEF/WHO, 2011). For those interested in intergenerational transmission of inequality, prenatal access to treated water might serve as one channel by which health and human capital are passed from parents to children in developing countries. Finally, for the design of effective policy interventions, it is essential to know whether the prenatal period is the crucial time window when access to safe drinking water has the biggest impact on later life health, skills and labor market outcomes.

Using a difference-in-differences (DID) approach, we study the health effects of in utero access to treated water for a sample of children aged 2-11 in 2010. The data for analysis is extracted from the China Family Panel Studies (CFPS) 2010. Combining the information on the first connection year of water infrastructure at community level and the child's year of conception, we create a dummy indicator of water infrastructure availability in utero, which takes value one if water infrastructure is available in the child's birth community in the year of conception, and zero if otherwise. The outcomes of interest are children's health status at the survey time. A child's height-for-age z-score at survey time and an indicator of at least three doctor visits in the year prior to survey are constructed to estimate the longer-term effects of prenatal access to treated water on childhood health. As the program has gradually been rolled out among rural communities, the DID approach with community and conception year fixed effects allows us to exploit the variation in prenatal exposure across cohorts within a given community.

To account for the difference in time trends across provinces and communities with different characteristics, we control for province-specific linear trends as well as the interactions between observable community characteristics and linear trends in year of conception. We argue that after partialling out community- and year-specific variation as well as observable differential trends in child health across communities, the placement of water infrastructure is not correlated with any unobserved determinant of child health. Results from validity tests support this argument.

We find robust evidence on the causal effects of in utero access to treated water on child health. We find in utero access increases children's height-for-age z-score at the survey time by 0.239 standard deviations and reduces the probability of having at least three doctor visits the previous year by 9.0 percentage points. The effects are large in magnitude, equivalent to a three centimeter increase in average height, and approximately a one third drop in the average probability of at least three doctor visits the previous year.

We use an event study to show that the beneficial impacts of water infrastructure on childhood health are concentrated in utero with limited additional impact after birth. This echoes the findings in previous studies that early life interventions can generate disproportionately large returns relative to interventions in later life (Almond and Currie, 2011a,b). The event study results also support the validity of our empirical strategy by showing that the estimated effects are not driven by pre-existing trends in child health. Moreover, we support the validity of our DID identification strategy using a battery of validity checks. We compare the pre-intervention trends of outcome variables between communities that received connection early with those that received connection late. We conduct a placebo test by shifting treatment status among communities in the same county, and a permutation test by randomly assigning the tap water introduction year to communities. We find ample evidence that the assumption of parallel trends is valid. Our estimates also remain robust to a series of sensitivity analyses, including addressing the possible reporting error and measurement error in the treatment variable, and discussing the possible impact of sample selection.

In the last part of this paper, we discuss the extent to which the estimated effects of in utero access to treated water could be attributed to improved access to water, or to better quality of drinking water enjoyed by the mothers. By estimating the treatment effects on the probability of getting different types of disease since birth, we show that having access to water infrastructure in utero significantly reduces the probability of getting gastrointestinal diseases, but exerts little impact on waterborne infectious and parasitic, respiratory or other types of disease. Since the literature has documented a stronger correlation between drinking water quality and digestive diseases than non-gastrointestinal disease, we take this evidence as suggestive of a strong link between drinking water quality and health (Ballester and Sunyer, 2000). Our findings are consistent with the argument that the health benefits may largely stem from the improvement in drinking water quality, rather than improved access to water (Devoto et al., 2012; Zhang, 2012).

We intend to make contributions to at least three strands of literature. The first is the literature on microeconomic effects of public infrastructure in developing countries. Previous studies have shown that improved access to electricity (Dinkelman, 2011), schools (Duflo, 2001, 2004), health facilities (Gruber et al., 2014), roads (Qin and Zhang, 2012), and landline phones (Lu et al., 2016) can influence varied household- and individual-level outcomes. In particular, a large number of studies have investigated how the existence of water infrastructure affects a variety of outcomes, including health, education, household time allocation, and perceived quality of life, through reducing the time burden of water collection and/or improving drinking water (Mangyo, 2008; Devoto et al., 2012; Zhang, 2012; Koolwal and van de Walle, 2013; Meeks, 2016; Zhang and Xu, 2016).[3] This research, as an addition to this line of literature, examines how prenatal access to water infrastructure in less-developed areas affects childhood well-being, particularly in the dimensions of health status and health care utilization.

The second is the literature on the relationship between prenatal period and later life outcomes. The prenatal period plays a critical role in a person's life-cycle. It has been documented that prenatal conditions has lasting effects on cognitive function, health and labor market performance.[4] This research advances this literature by examining the role of prenatal access to public infrastructure–water supply system–in human capital accumulation over the life-cycle. Further, existing studies commonly examined the effects of negative environmental shocks such

[3] Zwane and Kremer (2007) review the non-experimental evidence on how water infrastructure can affect child health, in particular the incidence of waterborne diseases. Ahuja et al. (2010) review experimental evidence on the effects of domestic water access and quality in developing countries.

[4] See footnote 1 for the review of literature on how in utero conditions affect later life outcomes. In addition, Almond and Currie (2011a) and Attanasio (2015) provide extensive surveys of the literature on how human capital accumulation is affected by early childhood conditions.

as pollution, maternal smoking, natural disasters, civil conflicts and famine. By contrast, this research focuses on the effects of a positive policy intervention, providing treated water to rural households, which is perhaps more relevant from a policy perspective (Currie et al., 2014).

The third strand of literature documents the relationship between drinking water quality and health. Improved water quality has been proved to improve health, reduce the incidence of gastrointestinal diseases, and reduce mortality in both developed and developing countries (Cutler and Miller, 2005; Kremer et al., 2011; Zhang, 2012). Surface water quality is found to be positively associated with the prevalence of gastrointestinal cancer (Ebenstein, 2012) and infant mortality (He and Perlof, 2016). Our study adds to this line of literature by suggesting that in the context of rural China the health gains of access to water infrastructure mainly come from the improvement in drinking water quality.

The paper is structured as follows. Section 2 provides the basic background information about the development of drinking water infrastructure in China and the literature linking water with fetal development and adulthood health. Section 3 details the data we use for estimation and the identification strategy. Section 4 presents the baseline results, results from validity tests, sensitivity analyses, and mechanism analyses. Section 5 concludes.

2 Background

2.1 Rural drinking water program in China

China as a whole is a water-stressed country. Its annual per capita freshwater resources, about $2,156\ m^3$ in 2007, is among the lowest for a major country (World Bank, 2009). Uneven distribution, both spatially and temporally, further worsened the severity of this problem (World Bank, 2009). Along with the unprecedented economic boom in the past three decades, the scarcity of freshwater in China is worsened by severe deterioration in water quality from industrial, domestic, and agricultural sources (World Bank, 2009). Data from China's water monitoring system shows that in 2004, of all 745 monitored river sections, 28% report survey water quality to be unsafe for any use, and only 32% met the national standards to be safe for industrial and irrigation uses (World Bank, 2009).

The development of public water supply in rural China greatly falls behind of that in urban areas. According to the estimates of WHO/UNICEF (2015), in 1990, 92% of urban residents had access to on-premise treated water from water plants, while 88% of rural residents had no access to on-premise tap water. Rural residents without tap water connections mostly relied on untreated water from wells, rivers, and lakes. In the China Family Panel Studies (CFPS) 2010 survey, more than 87% of rural households in communities without tap water connections in 2010 were using untreated water for cooking.

Owing to the tradition of drinking boiled water and eating cooked food, the major drinking water contaminants in many other developing countries, microorganisms, have less adverse consequences in China (Zhang et al., 2009). Nevertheless, chemical and heavy metal pollutants in water may lead to significant health damages in China as elsewhere. Ebenstein (2012) found that a deterioration in water quality by a single grade (on a six-grade scale) in China increases the incidences of digestive cancer by only 3.3% in counties with higher tap water coverage, but by 13.1% in counties with lower coverage. In addition to health outcomes, the time use of rural residents might be constrained by poor accessibility of safe drinking water. By Meng et al. (2004)'s estimation, rural households without piped water spend an average of 20 to 60 minutes every day to fetch water, and the time may go up significantly during dry seasons.

To improve rural residents' access to safe drinking water, the Chinese government initiated the rural drinking water program in the 1980s. As part of the program, water treatment plants with water purification technology and equipment installed have been constructed to remove microorganisms and chemical impurities. To improve accessibility and to avoid contamination during transport and storage, pipeline systems have been constructed to deliver treated water directly from water plants to households. Under China's centralized political system and decentralized administrative and fiscal system, the central government has stipulated guidelines for the program's implementation,[5] while local governments are taking responsibility in financing, planning, constructing, and management. In general, the central government, local government, rural residents, and other sponsors collectively bear the cost, with the contribution ratios of the parties varying across regions.

[5]Such as *Interim Provision on Rural Drinking Water* issued in 1984 and *Regulations on Construction of Rural Drinking Water Projects* issued in 2000.

No explicit implementation rules have been documented or observed due to the decentralized roll-out of the program. In practice, it is likely that areas in urgent need of safe drinking water may have a greater chance of getting a tap water connection early. It is equally likely that areas geographically close to the city center may get a connection earlier by taking advantage of the expansion of the urban water supply network. We show in Appendix A.1 how the timing of the tap water connection could be predicted by community characteristics using data from the China Family Panel Studies (CFPS) survey. In estimating the effect of early life exposure to tap water, we employ multiple empirical strategies to address the issue of non-random program roll-out. The discussion is detailed in Section 3.2.

After the implementation of this program, rural residents' access to safe drinking water has greatly improved. They can not only enjoy the ease of getting water at the turn of a tap but also the water is of higher quality than untreated water. The governments monitor water quality precisely and regularly. Treated water is required to meet a variety of standards, including general chemical, toxicological, bacteriological, and radiative indices stipulated by the Sanitary Standard for Drinking Water Quality (**??**). There is evidence that the quality of tap water has been better than that of untreated water in the treated villages after the implementation of this program (Zhang et al., 2009).

Through the end of 2010, the program had incurred a total cost of 182.5 billion yuan (USD 26.8 billion) and cumulatively secured safe drinking water for more than one billion rural residents (Meng et al., 2004; National Development and Reform Commission, 2007, 2012). Yet after thirty years of implementation, the program is still far from completion. By 2010, 400 million rural residents, accounting for 42% of the total rural population, had not obtained access to safe drinking water. Moreover, the program is facing various obstacles such as a lack of long-effect operational mechanism, large capital demand, and pollution of water sources.

2.2 Water, fetal development, and health in adulthood

Toxic chemicals and fetal development

In earlier times, the placenta was regarded as an organ protecting the fetus from exposures to environmental toxicants. However, mounting evidence has shown that environmental chemicals and other xenobiotics can cross through the placenta and enter the fetal bloodstream (Myllynen et al., 2005). Concentrations of nicotine and its major metabolite, illicit drugs, prescription pharmaceuticals, alcohol, and environmental chemicals such as pesticides have been detected in cord blood, amniotic fluid, or meconium (see summary by Barr et al. (2007)). In particular, arsenic and fluoride, two common environmental contaminants in drinking water, can also cross the placenta and transfer to the fetus (Concha et al., 1998; Agency for Toxic Substances and Disease Registry (ATSDR), 2003). Due to immature organ systems and underdeveloped detoxification enzymes or enzymatic processes when exposures occur, fetuses are extremely vulnerable to these environmental insults. For instance, perfluoroalkyl and polyfluoroalkyl substances transferred from the mother to the fetus through the placenta can result in several adverse effects on fetal growth and development, including reduced head circumference and birth weight (Apelberg et al., 2007; Chen et al., 2012; Callan et al., 2016).

Water scarcity and fetal development

Lack of water supply may impact pregnant women in rural households dependent on agricultural outputs or income through reduced nutrient intake, due to lower production and less varied diets. The reduced nutrient intake may cause malnutrition. In addition, poor sanitation due to lack of water supply during pregnancy may increase the risk of infectious diseases such as diarrhea and respiratory infections. As diarrhea reduces the capacity to absorb nutrients (WHO, 2012), it can also lead to increased malnutrition during pregnancy. Accounting for the roles of various nutrients in modulating oxidative stress, enzyme function, signal transduction, and transcription pathways that occur early in pregnancy, malnutrition during pregnancy may influence pregnancy outcomes by altering both maternal and fetal metabolism (Ramakrishnan et al., 2012).[6]

The exercise of water collection may hurt both maternal and fetal health directly via heavy physical work and indirectly by taking up pregnant women's time that could be spent on additional leisure or production. Households in developing countries spend considerable amount

[6] For instance, iron, zinc, iodine and long chain n-3 polyunsaturated fatty acids play critical roles in development of the brain and nervous system, and vitamins A, B-6, B-12 and folic acid influence oxidative pathways and methylation.

of time fetching water. For example, Kremer et al. (2011) estimated that a rural household in Western Kenya does around seven water-fetching trips per day, with each trip requiring a 20-minute walk on average. Meng et al. (2004) estimated that a household without piped water in rural China spends an average of 20 to 60 minutes every day to fetch water, and the time may go up significantly during dry seasons. In spite of this, in many countries, however, the heavy burden of water collection is borne primarily by women and girls (Devoto et al., 2012). A bulk of epidemiologic evidence reveals associations between heavy physical work during pregnancy and risks of lower birth weight for gestational age, miscarriage, and worse maternal health (see MacDonald et al. (2013) for an extensive review). Hence, if the burden of water collection does not spare during pregnancy, it may hamper both maternal health and fetal development.

Rare evidence has been detected on the causal link between water scarcity in utero and fetal development. Rocha and Soares (2015) is among the first that investigated the causal link between water scarcity in utero and birth outcomes, and found that negative rainfall shocks lead to higher infant mortality, lower birthweight, and shorter gestation periods.

Fetal origins of adult diseases

The "fetal origins of adult disease" hypothesis postulates that exposure to toxic chemicals in utero and fetal nutrient availability result in developmental adaptations that can permanently change structure, physiology, and metabolism, which further predispose individuals to a number of chronic diseases in adulthood (Law et al., 1992; Barker et al., 1993a; Barker, 1998). This hypothesis has been supported by accumulative evidence from experimental animal studies and epidemiologic studies.

A vast literature has shown that exposure to toxic chemicals in utero is associated with a higher risk of disease in adulthood. Cao et al. (2016) reviewed evidence supporting the association between in utero exposure to widespread environmental toxicants, such as heavy metals, persistent organic pollutants, and tobacco smoke, and risk for immune-related diseases and lung dysfunction in offspring in later life. Wang et al. (2014) reviewed the association between in utero exposure to two groups of environmental toxicants, endocrine disrupting chemicals (EDCs) and heavy metals, and the development of Metabolic syndrome (MetS) later in life.

In terms of arsenic, one of the common contaminants in drinking water, Farzan et al. (2013) summarized the evidence that in utero exposure is associated with elevated long term risk of chronic disease in adulthood, including cancer, respiratory disease, and cardiovascular diseases. Compelling evidence has pointed to the mechanism underlying the observed associations and showed that the diverse environmental chemicals may impair later life health outcomes via dysregulating the fetal epigenome (see review by Perera and Herbstman (2011)).

Limited knowledge has been accumulating on the long-term association between in utero water scarcity and later life health outcomes. Since one of the major consequences of in utero water scarcity is maternal malnutrition, and a series of epidemiologic studies have demonstrated that maternal malnutrition during pregnancy was associated with high susceptibility to cardiovascular disease (Godfrey and Barker, 2000) and obesity (Ravelli et al., 1976) in offspring, in utero water scarcity may hamper later life health outcomes through nutrition deprivation during pregnancy.

Besides the direct links between prenatal environment and birth outcomes or adulthood health, a bunch of epidemiologic studies have looked into the link between birth outcomes and health in later life. For instance, Barker et al. (1993b) found men who had a small head circumference and (or) were thin at birth had higher rates of cardiovascular death than those who had a large head circumference or were fat. Moreover, there is mounting evidence that lower birth weight is associated with higher risk of ischemic heart disease (Huxley et al., 2007) and chronic kidney disease (White et al., 2009) in later life. Since birth outcomes such as birth weight, head circumference, body length, and duration of gestation reflect fetal growth, exposure to toxic chemicals in utero and maternal malnutrition may exert negative impact on health in later life via impairing fetal growth.

3 Data and empirical strategy

3.1 Data

This study relies on data provided by the China Family Panel Studies (CFPS), which was launched in 2010 by the Institute of Social Science Survey (ISSS) of Peking University, China

(Xie, 2012). It is a longitudinal study of a nationally representative sample of Chinese communities, families and individuals. The target sample of CFPS consists of 16,000 households in 25 regions in China, excluding Hong Kong, Macao, Taiwan, Xinjiang, Tibet, Qinghai, Inner Mongolia, Ningxia and Hainan. A total of 14,798 households and 42,590 individuals from 645 communities were interviewed in 2010 in the baseline survey, where the data for our main analysis comes. The survey provides detailed information at community, household and individual level that is essential for our analysis.

Our sample consists of 3,299 rural children who were conceived between 1999 and 2008 and aged 2-11 years old in 2010. Children in this age range are in their early or middle childhood, an important period in the life cycle for human capital accumulation.To investigate the long-term effects of in utero access to water infrastructure, we limit our sample to children who were at least 2 years old. Since our identification strategy relies on the comparisons across different cohorts, we choose to exclude children who had reached 12 years old for the reason that the health status and growth pattern in early and middle childhood (2-11 years old) and adolescents (12-17 years old) might not be comparable. We further restrict our sample to children whose place of birth, place of current residence, and place of *Hukou* (household registration) coincide, so that our sample consists of children whose community of survey is the same as community of birth. We exclude children who migrated into the community of survey after birth, as their access to treated water in utero cannot be calculated without information of their birth communities.[7]

The treatment variable of this study measures the availability of public water infrastructure in the community of birth when a child was in the womb. Specifically, the treatment status takes value one if water infrastructure was operational in the child's birth community in the year of conception, and zero if otherwise.[8] The year of conception for each child is deduced from the information on birth date and length of gestation. The information on availability of

[7] We run regressions using the unrestricted sample, assuming that all children are interviewed in the same community of birth. The estimated effects are smaller and less significant, suggesting that the presence of measurement error in the treatment variable attenuates the estimates. Results are available from the authors upon request.

[8] We attempt to use alternative measures of in utero access to tap water, including the availability of tap water in the year of the second trimester of pregnancy, the year of the third trimester of pregnancy, and the birth year. The results presented in Table A.3 suggest that the effects are slightly smaller in magnitude, which may indicate the importance of having access to tap water in the first trimester of pregnancy.

tap water connection and first year of connection is extracted from the community survey data of CFPS 2010.

The community survey was administrated to an individual in each community who knows the community well and has access to statistical materials of the community, such as the director or the accountant of the community committee, or the secretary of the Party branch (Xie, 2012). In the questionnaire, it is asked that "Does your community has this facility: *(facility name)*?" If the answer is yes, the survey further asks in which year the facility was first introduced to the community. We argue that the reported data on tap water availability and introduction year in the community are reliable for two reasons. Firstly, receiving tap water connection is a salient and socially desirable event for rural residents, so the introduction year is likely to be accurately reported even many years after (Beckett et al., 2001). Secondly, since tap water connection and coverage rate is standard information on community statistical materials such as community chronicle of events, the respondents could retrieve the information with ease by referring to these materials.[9] Based on the answers for tap water connection, we plot in Figure 1 the number of new communities connecting to tap water each year and the cumulative distribution of the community level tap water roll-out during 1980 and 2010 among 404 rural communities in CFPS 2010. In 1980, less than 5% of the communities had access to tap water. By 2010, over 55% of the communities had received a connection. In Section 3.2, we will explain that between 1999 and 2008, the period that the variation in the treatment variable comes from, the fraction of communities with water infrastructure increases by almost 30 percentage points, suggesting substantial variation in the treatment variable.

In the absence of the exact month when water infrastructure becomes available, we construct the treatment variable in a way that a child has access to tap water in utero if the child was conceived in the same year of tap water connection or after. To illustrate this, imagine a community that first received the infrastructure in 2000. All children in this community conceived in 2000 or after are considered to have access to water infrastructure in utero. We discuss the possible effect of measurement error in Section 4.3. Panel A of Table 1 shows the summary statistics of the treatment variable.

[9] Nevertheless, the reporting error, if anything, may bias our estimates. We discuss the possible bias in Section 4.3 and show that our estimates are not sensitive to possible reporting errors.

Childhood well-being is measured by height-for-age z-score at survey time and whether visiting doctors at least three times the previous year. While height is argued to be an indicator of early deprivation (Case and Paxson, 2008), both height and the probability of frequent doctor visits are common indicators of long-term health status that are less likely to be influenced by transitory circumstance. To account for differences in height by age and gender, height-for-age z-score is obtained by subtracting the mean of the comparison group and dividing by the standard deviation of the comparison group for children of the same age (by half year) and gender. The comparison group here includes children born and living in communities without tap water connection in 2010, and thus having no access to water infrastructure since the prenatal period. Having a doctor visit means a child went to hospital/medical center for curative (not preventative) medical services. Doctor visits for one continuous course of disease is counted as one time. The indicator of at least three times of doctor visits is a dummy variable equal to one if a child had three or more doctor visits the previous year, and otherwise zero. Among the 3,200 children with doctor visits information, 28% of them have the dummy variable to be one. Because the frequent doctor visit by this definition is "high incidence", it is likely to have the statistical power needed to test our hypothesis.[10] In addition to the two key outcome variables, we explore the impact of tap water exposure on other outcome variables, covering birth outcomes, health status in the first year of life, and other health measures at the survey time. The results are reported in Appendix A.2. Panel B of Table 1 reports the summary statistics of the key outcome variables and other outcome variables.

The summary statistics of the individual and household characteristics, and community characteristics are shown in Panel C and Panel D of Table 1, respectively.

3.2 Empirical specification

We exploit the variation in access to treated water across communities over time and compare children conceived earlier in a given community with those conceived later. A difference-in-differences model is applied for estimation. The health outcome Y_{ict} of a child i conceived in

[10] To show that our estimates are not affected by the transformations of the outcome variables, we report the estimates from regressions with the outcome variables in the original forms in Table A.2.

year t and born in community c in province s can be expressed as:

$$Y_{ict} = \delta \text{Treated water IU}_{ct} + X_{ict}\beta + g_c + \gamma_t + \theta_s \times t + \phi Z_c \times t + \epsilon_{ict}, \tag{1}$$

where Treated water IU_{ct} is an indicator for water infrastructure availability in the year of conception in the community of birth. The parameter of interest is δ, which can be interpreted as the causal effect of having access to treated water in utero on the health outcome. X_{ict} is a vector of individual and household characteristics observed in 2010, including gender, mother's age at birth, birth order, number of siblings, and parents' years of schooling.[11]

We include conception year fixed-effects γ_t to control for any time trends or shocks in infant and child health that is common to all communities, and community fixed-effects g_c to control for any time-invariant community-level characteristics that may be associated with both the timing of water infrastructure construction and changes in infant and child health.[12] To account for the different pre-existing trends in infant and child health across provinces, we additionally control for province-specific linear trends in year of conception $\theta_s \times t$, and to account for the difference in pre-trends across communities, we further control for interactions of a set of exogenous community controls and linear trends in year of conception, denoted by $Z_c \times t$. These community controls include an indicator for suburban village, distance to nearest town/city in kilometers, area in square kilometers, average years of schooling for 25-55 year-old residents, and topographic characteristics.[13]

Our identification could still be invalid if other governmental programs taking place simultaneously with the drinking water safety program might have affected infant and child health. We explore the impacts of potential confounding factors by directly controlling for in utero access to a series of public infrastructures (electricity, cable/satellite televisions, roads, and landline phones) and to other health programs such as the New Cooperative Medical Scheme

[11] We attempted to add household income and whether household doing non-farm work in the regressions and found the estimates unaffected. Considering the endogeneity of household income and non-farm work, we do not control for them in the baseline specifications. The results are available from authors upon request.

[12] Our results do not change if t indexes the year of birth instead of year of conception.

[13] In Appendix A.1, we examine the determinants of tap water connection in community level. We find that communities that are suburban villages and near town or city got connection earlier. While these community characteristics are statistically significant determinants of community tap water connection, we find that they explain little of the overall variation in tap water connection. Nonetheless, we include linear trends interacted with these variables to control for possible different time trends across communities.

(NCMS) in the preferred specifications.

The validity of our identification strategy relies on the assumption of parallel trends that in the absence of water infrastructure, communities would have changed similarly with respect to infant and child health regardless of the timing of water infrastructure construction. using an event study model, we rule out the existence of pre-trends that might have driven our results and confirm in utero as the critical time window for the impact of tap water on childhood health. The validity of the parallel trends assumption is further supported by the comparison of pre-trends between communities getting connection earlier and later, and the two placebo tests. The discussion is detailed in Section 4.2.

A potential problem with our analysis concerns sample selection caused by fetal selection and sample attrition. Fetal selection exists as we could only observe childhood health outcomes for those who were born. Having access to treated water in utero may affect women's fertility or fetuses in utero, resulting in the population of surviving children to be different from what it would otherwise have been and a biased estimate. Sample attrition exists as children who were not in birth communities at the survey time of CFPS 2010 are not included in our analysis. The sample attrition could be caused by household emigration, child home-leaving for study or work, or other reasons. If in utero access to treated water affects the probability of not being surveyed in 2010, our estimate of the effect of in utero access would be biased. To address the concerns, we examine the effect of access to treated water on in Section 4.3. The results imply that the effect on health is unlikely to be affected by fetal selection and sample attrition.

To keep our sample size reasonable, observations with missing individual and household characteristics are included in the regressions. Sample means are assigned to the missing values, and a set of dummy variables is created with each variable being equal to one if the corresponding information is missing. To account for any serial correlation across different cohorts within the same community, standard errors are clustered at community level (Bertrand et al., 2004).

4 Results

4.1 Main results

4.1.1 Baseline results

Table 2 presents the baseline results for health outcomes at ages 2-11, including height-for-age z-scores and the incidence of three or more doctor visits the year prior to survey. In all specifications, we control for demographic controls and community fixed-effects. In columns (1) and (5), we control for conception year dummies in the regressions, assuming the cohort trends in the outcomes to be the same across all communities. In columns (2) and (6), we additionally control for province-specific linear trends and the interactions of the aforementioned community characteristics and linear trends. Here we relax the assumption in column (1) and (2) by accounting for the difference in time trends across provinces and across communities with different characteristics. Column (3) and (7) reports on augmented specification with confounding factors on health interventions controlled for, that is, health insurance (New Rural Cooperative Medical Scheme) and health facility in the community level. In the last specification in columns (4) and (8), we further control for other confounding factors during the prenatal period, including whether having exposure to cable, electricity, road, and landline phone in utero.

Consistently, we find having access to tap water in utero increases height-for-age z-scores across specifications. In our preferred specification (column (1)), we find prenatal access to safe drinking water leads to a 0.239 standard deviation increase in children's height-for-age z-scores. As one standard deviation is equivalent to 15 centimeters within each age group, access to treated water in utero would make a child around 3 centimeters taller than her/his same-age counterparts without access. While gender, mother's age at birth, number of siblings, and mother's years of schooling have a small and insignificant effect on height, a child who has fewer elder siblings or whose father is better educated is more likely to be tall.

We also find evidence that in utero exposure to tap water lowers the probability of visiting doctor at least three times the previous year. When only community fixed-effects and cohort fixed-effects are controlled for, the estimated effect on the probability is negative and insignificant. Further controlling for province-specific linear trends and the interactions of community controls and linear trends generates an estimate of slightly larger magnitude and with a *p*-value of 0.075. It suggests there exists difference in the time trends of the outcome variable across provinces or communities of different characteristics. In our preferred specification (column (8)), we find that having access to tap water in utero leads to a nine percentage point decrease in the probability of having three or more doctor visits. As approximately 28% of the children aged 2-11 reported visiting doctor at least three times the previous year, it suggests that the effect of prenatal access to treated water is equivalent to a one third reduction in the average probability of having two or more doctor visits.

For both outcome variables, additionally controlling for in utero exposure to health programs and in utero exposure to other public facilities seldom changes the estimated effect of in utero exposure to tap water.[14] The estimates on health programs and other public facilities are not significant both individually and jointly. The exercise shows that the estimated effect of in utero exposure to tap water is unlikely to be driven by other simultaneous governmental programs.

[Comparing magnitude with other studies]

4.1.2 Heterogeneity analysis

The effect of in utero access to treated water may depend on basic characteristics of the children and their parents. In this section, we investigate the heterogeneous treatment effects that may vary by children's gender, mothers' educational level, and household economic status. Mother's educational level is considered as low if years of schooling is not greater than the median level among all mothers of 2-11 year-old children in the same community. Household's economic status is considered as low if annual household income is not greater than the median annual household income of all 2-11 year-old children in the same community. We run separate regressions by subsamples. Columns (1) and (2) of Table 3 present the results for boys and girls, respectively. Columns (4) and (5) report the results for children with low-educated mothers

[14] The estimates on in utero exposure to tap water remain almost unchanged when we additionally control for any one of the confounding factors at a time or control for all the confounding factors at the same time. The results are available from the authors upon request.

and those with high-educated mothers, respectively. Columns (7) and (8) report the results for children from low-income households and those from high-income households, respectively. Hausman tests are implemented to test whether the differences in estimated treatment effects are statistically significant across subsamples. The *p*-values are reported in columns (3), (6), and (9).

In Panel A of Table 3, we report estimates for height-for-age z-score. In utero access to tap water brings in a 0.369 standard deviation increase in boys' height-for-age z score, and a 0.124 standard deviation increase for girls, but the difference is statistically insignificant. The comparison by household income reveals that children from households of low economic status benefit more from prenatal exposure to tap water than those from households of high economic status, although the difference is insignificant.

When looking at mother's schooling, those whose mother is less educated benefit more from in utero exposure to tap water than those whose mother is better educated, 0.323 standard deviations compared with -0.159 standard deviations, and the difference is significant at 5% level. The finding is consistent with literature: better-educated women are more likely to take measures to protect themselves and their children from negative environmental factors such as polluted drinking water (Currie et al., 2013; Graff Zivin et al., 2011). Therefore, it is reasonable to expect in utero access to publicly provided treated water to have different effects on children born to more or less educated mothers. From a policy perspective, the result also suggests that providing treated water through public infrastructure would have the potential to reduce health inequality across children from different socioeconomic backgrounds.

The estimates for the probability of seeing doctor at least three times the previous year are reported in Table 3. Although having access to tap water in utero is found to lower the probability of frequent doctor visit, none of the estimates are significant. Hence, we do not detect any evidence of heterogeneous effects on the probability of frequent doctor visit by gender, mother's schooling, and household income.

4.2 Validity of DID strategy

The validity of our identification strategy relies on the parallel trends assumption that in the absence of water infrastructure, the communities receiving the infrastructure earlier and those receiving it later would have changed similarly with respect to children's health. In this section, we assess the plausibility of this assumption in several ways.

Event study estimation

First of all, we use an event study to test if there are pre-existing different trends across communities receiving the infrastructure in different years. An event is defined as getting tap water connection at the community level. An event time, j, is the number of years away from the introduction year of tap water.[15] We generate a series of dummies, D_j, based on the event time. D_j is equal to one if a child was conceived in the event time of j.[16] The end points are open brackets (6 or more years before the year of introduction on the left, and 6 or more years after introduction on the right), which helps reduce the collinearity between event time and conception year. We replace the treatment variable, Treated water IU_{ct}, in Equation (1) with the event time dummies, D_j.[17] The Equation (1) is revised to:

$$Y_{ict} = \sum_{j=-6}^{6} \alpha_j D_j + X_{ict}\beta + g_c + \gamma_t + \varphi_p \times t + \phi Z_c \times t + \epsilon_{ict}. \quad (2)$$

The children conceived one year before the introduction of treated water ($D_{-1} = 1$) are taken as the base group. The coefficient at any other event time can be interpreted as the effect of in utero access to treated water in that event time relative to the effect if the child was conceived one year before the introduction of treated water. Since negative event times correspond to getting conceived before the introduction of tap water and having no access to tap water in utero, the coefficients on negative event time dummies actually reflect pre-trends. We follow

[15] A negative event time indicates the number of years before the introduction year of water infrastructure, a zero event time corresponds to the introduction year, and a positive event time means the number of years after the introduction.

[16] For instance, $D_0 = 1$ if the child was conceived in the year of introduction, and zero otherwise; $D_{-1} = 1$ if the child was conceived one year before introduction, and zero otherwise; $D_1 = 1$ if the child was conceived one year after introduction, and zero otherwise.

[17] Our sample children are aged 2-11. [balanced sample?]

Hoynes et al. (2016) and focus on the event study coefficients inside the unbalanced endpoints.

We are testing two hypotheses from the event study. First, if the pre-trend is flat, *i.e.* similar estimates for the negative event times, it suggests that our results are not driven by differential trends between communities getting tap water earlier and later. Second, the event study shows whether the timing of access to tap water matters. If having access to tap water during prenatal period is crucial, we would expect to observe an improvement in child health from in utero access to tap water relative to access after birth.

In Figure 2, we plot the estimates and the 90% confidence intervals from the event study. For both height-for-age z-score and the probability of at least three times of doctor visits the previous year, the pre-trends in health outcomes are relatively flat. We take it as strong evidence supporting the assumption of parallel trends. It suggests that our baseline results are unlikely to be driven by differential trends among communities getting connection in different years. Again, for both outcome variables, there is a steep improvement in health when a child gets access in utero (the event time moving from negative to positive). The exercise of event time study, therefore, reveals that in utero is the critical time period for the effect of tap water access on childhood health outcomes.

Test of parallel pre-intervention trends

In the main analysis, children from communities that first obtained tap water connection during 1999 and 2008 serve as the treatment group, and those from communities that connected before 1999 or after 2008 constitute the control group. Although we have controlled for a rich set of explanatory variables and fixed effects in the baseline regressions, our DID specification may still fail if the unobserved trends of outcome variables are different across treatment and control groups in the absence of tap water connection. While we cannot observe the counterfactual case directly, we can assess the parallel trend assumption by comparing the pre-existing characteristics across communities receiving the infrastructure in different times. Specifically, we derive from CFPS 2010 the average height by gender, average years of schooling by gender, sex-ratio, and fertility by every five adjacent cohorts born between 1955 and 1989 across two types of communities.[18] The cohorts were aged 25 to 55 in 2010, and none of them had access to tap water in utero. We do not include communities that received access before 1999, as the trends for the above mentioned cohorts in those communities may actually reflect post-intervention trends. Communities that first received tap water connection between 1999 and 2008 are considered early access, and those that did not receive before 2008 are considered late access.

In the regressions, we use the aggregated community characteristics as the dependent variables and control for the interactions between the five-year-cohort dummies and the binary variable of early access, community fixed effects, five-year-cohort fixed effects and other control variables. The coefficients on the interactions are plotted in Figure 3. Most of the coefficients on the interactions are insignificant. The only exception is that for women in the cohort of 1975, those from communities receiving connection earlier are found to be marginally higher than those from communities receiving connection later (after 2008). As shown in Panel A1 of Table 5, controlling for parents' heights and the missing indicators does not affect our findings. [19] Hence, the comparison of pre-trends provides strong evidence for parallel trends in outcome variables for pre-treatment cohorts regardless of connection time.

We re-run the regressions using children from these two types of communities to see if the choice of control group matters for our estimates. The effects of in utero access tap water exposure on childhood outcomes remain unchanged (Panel A2. of Table 5). This exercise suggests that our conclusion still holds after excluding children from communities that first obtained tap water connection before 1998.

Placebo test

We re-estimate the model using two placebo tests. In the first test, we exploit information about tap water connection year of different communities in the same county, to account for

[18]The calculation of community averages is done by five year period. For example, the value in 1955 is in fact the average for individuals born between 1955 and 1959, and the value in 1985 is the average for individuals born between 1985 and 1989.

[19] We did not control for parents' heights in our baseline regressions, as a great amount of the heights are missing. Among the 3,303 sample children, 36% of them do not have father's height, and 25% of them do not have mother's height. We attempted to replace the missing heights with the respective sample means by gender and generate missing indicators for the replacements.

the possibility that tap water connection in one community may benefit communities that do not have it, or lack of tap water connection in one community may influence communities that have it. For each community in a county, if its connection year is different from the earliest connection year among all communities in the county, we attribute the earliest year to this community. If its connection year is the earliest but different from the median connection year, we attribute the median year to this community. If its connection year is already the earliest and it is the same as the median year, we attribute the latest year to this community. "Pseudo-treated water IU" is constructed using this pseudo year of tap water connection at the community level and an individual child's conception year. We do not expect the coefficient on "Pseudo-treated water IU" to be significant, unless it captures shocks to the region that are correlated with changes in health outcomes. Panel B of Table 5 shows that indeed there is no significant effect.

The second test is a permutation test in which we randomly assign the tap water introduction year to communities (Conley and Taber, 2011). In particular, we randomly divide the 404 rural communities into 12 groups, one before 1999, ten between 1999 and 2008, and one after 2008. The number of communities in each group is consistent with the number of communities by the actual connection year. A placebo tap water exposure in utero is constructed for each child using the assigned first connection year. We estimate the placebo treatment effect on both height-for-age z-score and the probability of visiting doctor at least three times the previous year. To approximate the permutation distribution, we conduct the random assignment 5,000 times. The distributions of the 5,000 placebo estimates are plotted in Figure 4. The p-value of each permutation placebo test is the proportion of placebo estimates that are equal to or larger in absolute value than the corresponding estimate form the baseline analysis. We find that the p-values for both outcomes are smaller than 0.05. Hence, the results from permutation test further support for our identification strategy.

4.3 Sensitivity analysis

Reporting error

As mentioned in Section 3.1, a knowledgeable individual who has access to statistical materials in each community was invited to do the community questionnaire in CFPS 2010. As part of the survey, they were asked to provide the first year that the community received tap water connection, the answer to which is used to construct the treatment variable of this study. Since receiving tap water connection is a salient and socially desirable event in the community, and the information is available in community statistical materials such as community chronicles of events, the knowledgeable respondents are likely to report accurate years (Beckett et al., 2001). However, the reporting error, if it exists and is correlated with the introduction year of tap water in the community, would bias our estimates.

We address the issue of reporting error in two ways. Firstly, Beckett et al. (2001) suggested that data quality likely deteriorates with the length of the recall period, so we exclude communities whose reported connection year is more than 16 years, 14 years, 12 years ago, or 10 years ago (reported year of first connection before 1994, 1996, 1998, or 2000). The results presented in columns (1)-(4) of Panel A of Table 6 show that the exercise of excluding suspicious observations generates similar estimates to the main results for both outcomes.

Secondly, we account for the correlation between interviewers' impressions of the quality of respondents' answer and the quality of the data (Beckett et al., 2001). On completion of each community survey, the interviewers evaluated the reliability of the respondent's answers on a scale of 1 to 7, with 1 being very unreliable and 7 very reliable. We construct a binary reliability measure and estimate our results using the subsample of children in the reliable communities only (with a score of at least 6). As indicated by column (5) of Panel A of Table 6, the estimates for both outcomes seldom change. Therefore, we are confident that the reporting error, if any, is not driving our results.

Measurement error

In our main analysis, a child was taken as having access to tap water in utero if the child was conceived in the same year of first tap water connection or after. By doing this, we assume that all the first connections occur at the beginning of a year. In reality, however, the connection time may spread out over a year. As a result, the constructed treatment status may not capture the real accessibility in utero. If the connection time is random in one year, the measurement error in the treatment variable would not bias our estimate.

To check if our estimates would be affected by non-random connection time, we rerun the regressions by considering two extreme cases. In the first case, we assume all the first connections happen in the middle of a year, and in utero access to tap water is counted as 0.5 for children conceived in the first year of connection. In the second case, we assume that all the connections happen at the end of a year, and in utero access to tap water is counted as zero for children conceived in the first year of connection and one for children conceived the year after. The results are reported in Panel C of Table 6. In both extreme cases, we get results close to our baseline estimates, suggesting that the measurement error in the treatment variable is unlikely to drive our estimation.

Sample selection

Our estimated effect would be biased if the sample of children used for analysis are not randomly selected. We discuss the possible impact of access to tap water on birth rate and the possible impact of tap water exposure in utero on sample attrition.

Birth rate

If getting access to treated water in utero tend to improve women's fertility or retain unhealthy fetuses , the population of surviving infants and children would be different from what it would otherwise have been. Estimation based on the sample of surviving sample may generate biased effects. To address the concern, we follow Ferrara et al. (2012) and estimate the probability that a woman gives birth in a given year as a function of individual and community controls and the accessibility of treated water at the community level. We construct a data set with a retrospective history of a woman's fertility during ages 15-49. We estimate the following linear probability model:

$$F_{ict} = \delta \text{Treated Water}_{c,t-1} + X_{ict}\beta + g_c + \gamma_t + \theta_s \times t + \phi Z_c \times t + \epsilon_{ict}, \quad (3)$$

where F_{it} is equal to one if a woman i living in community c gives birth to a child in year t; Treated Water$_{c,t-1}$ is a dummy variable equal to 1 if the community got tap water connection at least one year before t (to account for the length of pregnancy); δ captures the effect of having access to treated water on fertility. X_{ict} is a vector of individual controls, including age, number of existing children, years of education, and the quadratic terms of the above variables. g_c are community fixed effects, and γ_t are year fixed effects. To account for the different pre-existing trends in fertility across communities, we further include province-specific linear trends $\theta_s \times t$ and interactions between a set of exogenous community controls and linear trends, denoted by $Z_c \times t$.

In Panel B of Table 6, we report the results from the full sample of women aged 15-49 and for different age brackets (15-19, 20-34, 35-49). In all regressions, we get small and insignificant effect of having access to treated water on fertility. The findings imply that our estimated effect of in utero access to treated water on childhood health is unlikely to be affected by change in fertility rate.

Sample attrition

We next consider the potential bias caused by sample attrition. Sample attrition is of concern if the absence of children in the survey (due to household emigration, child home-leaving for study or work, or other reasons) is systematically correlated with the treatment variable. For example, if in utero access to treated water leads to higher probability of leaving home at young ages, especially for healthier children, our estimates could be under-estimated.

To address the issue of attrition bias, we first examine whether in utero access to tap water affects the likelihood of children's absence in the survey when households were surveyed. In particular, for the 2-11 years old from the surveyed households in the CFPS 2010, we investigate the probability of not being surveyed and the probabilities by reasons of absence: children not living at home and children living at home but not being surveyed. Among 3,747 children in this category, 163 were not at home at the survey time, and among the 3,584 children living at home, 285 were at home but not surveyed. For these unsurveyed children, we are able to construct the treatment variable based on parents' or other family members' reporting of their basic information. We regress the probabilities on the treatment variable with a full set of explanatory variables and fixed effects controlled for. The coefficients on the treatment variable is reported in Panel D of Table 6. In utero access to treated water has small and insignificant effect on all the probabilities in 2010.

Second, by matching children in CFPS 2010 and CFPS 2014, we can identify the children who were surveyed in 2010 but not in 2014. In particular, for a sample of 0-7 year-old children who were surveyed in CFPS 2010 and would be 4-11 years old in 2014, we estimate the overall probability of a child not being surveyed in 2014 and the probabilities by reasons of absence: household not being surveyed, children not living at home, and children living at home but not surveyed.[20] Among the 2,414 surveyed children in 2010, 206 children were absent with the households. Among the 2,208 children whose households were surveyed, 77 children had left home, and among the 2,131 children who were still living at home, 65 were not surveyed.[21] Again, we regress the probabilities on the treatment variable with a full set of explanatory variables and fixed effects controlled for. Panel D of Table 6 presents the estimation results. In utero access to treated water has small and insignificant effect on all the probabilities. Hence, we conclude that our estimates are unlikely to be biased by sample attrition.

4.4 Mechanism check: water quality or water quantity?

As briefed in Section 2.2, both toxic chemicals in drinking water and poor access to water can hamper the fetal growth and consequently, the health outcomes in adulthood. Yet, our identification strategy does not guide us to pin down whether the health effects of in utero access to treated water is sourced from improvement in access to water (water quantity), or from improvement in drinking water quality.

A few studies have investigated the effect of water quantity and quality individually. In the setting of urban Morocco, Devoto et al. (2012) showed that improving water access while maintaining water quality did not lead to significant health improvement but increases the time available for leisure and reduces conflicts on water matters. Kremer et al. (2011) found that the spring protection, an investment that improves source water quality but not quantity in Kenya, lowered diarrhea among young children in treatment households. However, most of the other studied interventions address both water quality and quantity, hence much of the convincing

[20] Ideally we should use a sample of children aged 2-11 in 2014 yet some of them were not even born in 2010. We therefore only include those aged 0-7 in 2010.

[21] Mortality selection is caused by infant and child mortality (Currie and Vogl, 2013). If in utero access to treated water can lead to higher survival rate of the unhealthiest infants or children so that the mean health of the remaining population drops, our estimated effect would be biased. We cannot directly test this, as the child mortality is very low in China. [cite???] Among the 2,414 children, only one was dead between 2010 and 2014. Instead of examine the mortality rate, we include the child in the category of chilren not living at home.

evidence on health improvement failed to separate the impact of quality and quantity, making it unclear which route matters the most in practice (Cutler and Miller, 2005; Galiani et al., 2005; Gamper-Rabindran et al., 2010). Since the costs and benefits of water policies may vary by their emphases on water quality or quantity, identifying individual health effect of water quantity and quality is crucial for policy makers (Ahuja et al., 2010).

Using the data on childhood disease, we attempt to disentangle the roles of water quality and quantity in improving childhood health. Among 153 disease types, the CFPS 2010 recorded surveyed children's most serious disease from birth to date. Since we are more interested in the long-term effects of in utero access to treated water, we exclude children who suffered from the congenital and neonatal diseases from analysis.[22] Out of the 3,299 2-11 year-old children, we have disease information of 3,060 children. Among them, 61.9% reported a disease since birth. We use the discussion on the relationship between water and health in Section 2.2 to guide the categorization of diseases. The reported diseases are categorized into five mutually exclusive types: 1) waterborne infectious and parasitic diseases, 2) water quality sensitive diseases excluding type 1, 3) water quantity sensitive diseases, including cardiovascular diseases and kidney diseases, and 4) other diseases. The diseases type 2 contains immune-related diseases, respiratory diseases, metabolic diseases. Epidemiological evidence has suggested the link between water pollution and the digestive diseases such as liver cancer (Lin et al., 2000) or gastric cancer (Morales-Suarez-Varela et al., 1995) for adults, although to our knowledge, Ebenstein (2012) is the only study to date to show the causal association between contaminated water and digestive cancers. Considering the role of toxic chemicals in fetal development, we conjecture that the causal link between water quality and digestive diseases applies to fetuses. We include digestive diseases into type 2. A dummy indicator for ever getting the disease is generated for each type.

If it is the improvement in water quantity that has led to the rise in childhood health outcomes, we would detect the incidence of all five types of diseases to be lowered by access to treated water in utero. Alternatively, if it is the improvement in water quality that plays the sole role, we may find the incidence of diseases that are fragile to water quality (types 1 and 2) to

[22] The effect of access to treated water in utero on congenital and neonatal diseases is discussed in detail in Appendix A.2.

be lowered while the others are unaffected.

Our preferred specification as in columns (4) and (8) of Table 2 is used for estimation, and the results are reported in Table 4. The regressions show that in utero access to treated water lowered the probability of getting type 2 diseases by 9.9 percentage points, and the effect is significant at 5% level. As for the other three types, the estimates are relatively small in magnitude, and none of them is statistically significant. Although waterborne infectious and parasitic diseases are sensitive to water quality, its probability is not affected by in utero access to tap water. It is possible that the common practice of boiling water before drinking and eating cooked food in rural China may have largely killed the microorganisms and parasites in water.[23] The incidence of type 3 diseases is not affected, suggesting that it is not likely that in utero exposure to tap water improves health via improved access. We take these findings as suggestive evidence that the improvement in water quality plays a leading role in improving childhood health. Such findings echo the argument by Zhang (2012) that water quality is found to play a more important role than water quantity in health improvement in rural China.

5 Conclusion

We use the data extracted from the China Family Panel Studies (CFPS) 2010 to study the health effects of in utero access to drinking water infrastructure for a sample of children aged 2-11 in 2010. Combining the information on the first year of tap water connection in each child's birth community and the child's year of conception, we create a dummy indicator of tap water availability in utero. We focus on two health indicators as outcomes: height-for-age z-score and whether visiting doctor three or more times the previous year. As the program has gradually been rolled out among rural communities, the DID approach with community and conception year fixed effects allows us to exploit the variation in prenatal exposure across cohorts within a given community. To account for the difference in time trends across different provinces and communities with different characteristics, we include the province-specific linear trends, and the interactions of the community characteristics and the linear trends. To account for the impact of confounding factors, we control for in utero exposure to health programs and other pub-

[23] Zhang et al. (2009) find that more than 85% of rural households boil water for drinking in China.

lic facilities. We find that having access to water infrastructure in utero significantly increases children's height-for-age z-score by 0.239 standard deviations, and reduces the probability of having three or more doctor visits by 9.0 percentage points. The effects are qualitatively large, equivalent to a 3 centimeters increase in height, and a one third drop in the average probability of visiting doctor three or more times.

Results from an event study show that the beneficial impacts of water infrastructure on childhood health are concentrated in utero with limited additional impacts after birth. This finding echoes the literature by highlighting in utero as the crucial period of human capital development.

The validity of our identification strategy relies on the assumption of parallel trends among communities getting connection earlier and later. From both the event study and the comparison of pre-trends by connection timing, no pre-existing trends are detected. In addition, we find more supportive evidence of parallel trends by conducting two placebo tests. Our estimates survive a series of sensitivity analyses, including addressing the possible reporting error and measurement error in the treatment variable, and discussing the possible impact of sample selection.

To check if the welfare gains of access to water infrastructure is driven by improved water quality or improved access to water, we estimate the effects of access on the probability of getting different types of disease since birth. We find that having access to water infrastructure in utero significantly reduces the probability of having gastrointestinal disease, but exerts little impact on respiratory, infectious, or other types of disease. Such finding suggests that the health benefits of access to water infrastructure in utero may stem from improved water quality, rather than improved access.

Heterogeneity analysis suggests that children whose mother is less educated may benefit more from access to water infrastructure in utero. It suggests that constructing water infrastructure in rural China would have the potential to narrow down the gap in health stock among households of different SESs and the widening gap between urban and rural residents.

In view of the grand scale of government spending in China (roughly nine trillion Chinese Yuan in 2010), it is essential to understand whether the fiscal expenditures have been allocated cost-effectively. By comparing the health improvement with the construction cost of water infrastructure, our back-of-the-envelope calculation shows that the rural drinking water program is cost effective. Future work would explore the possibility of setting up causal link between in utero access to varied types of public infrastructure and varid dimensions of human capital.

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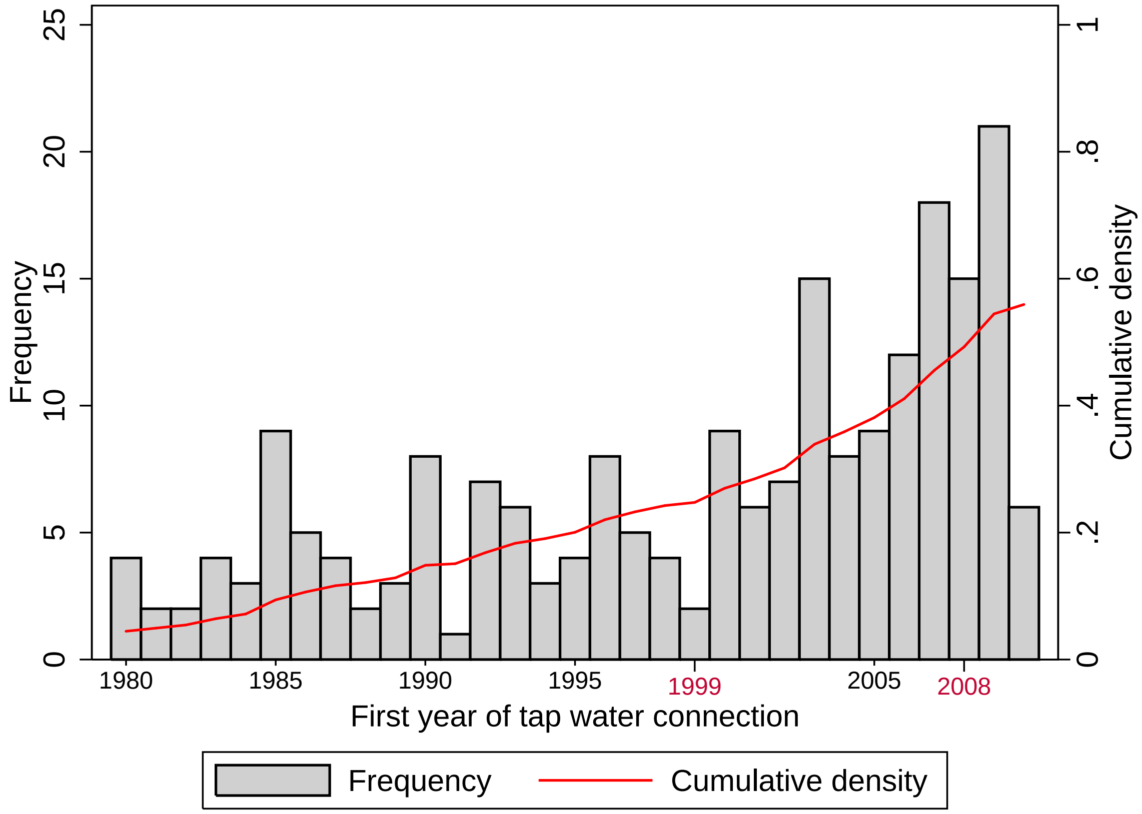
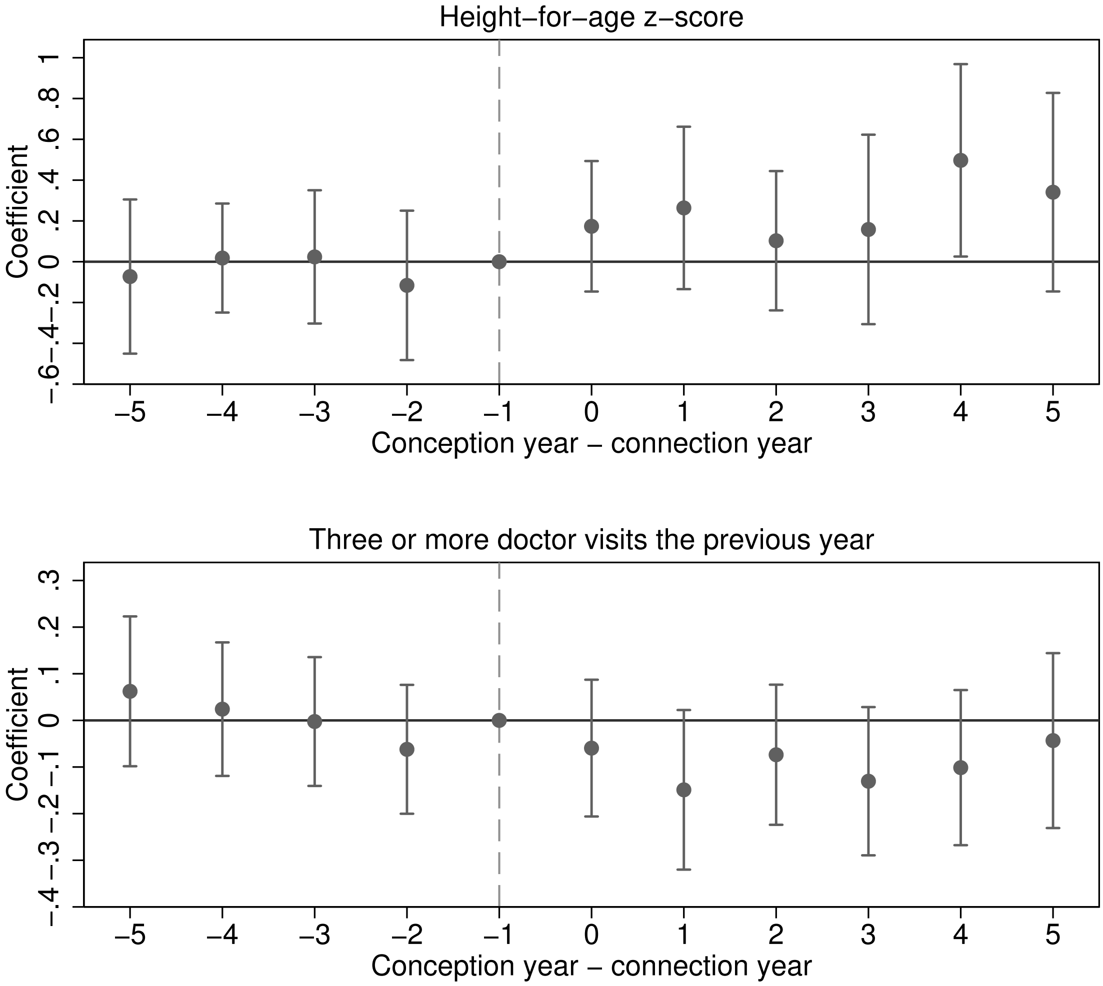


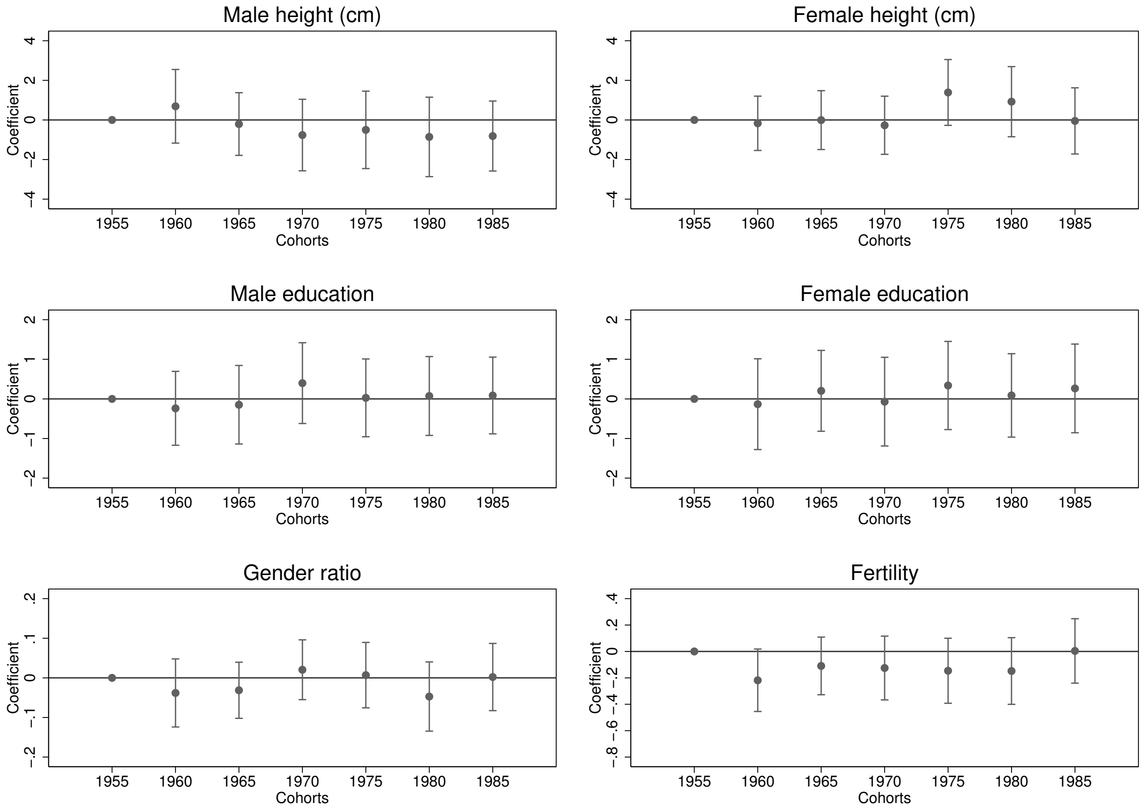
Data source: China Family Panel Studies 2010.

Figure 1: Roll-out of the rural drinking water program in China



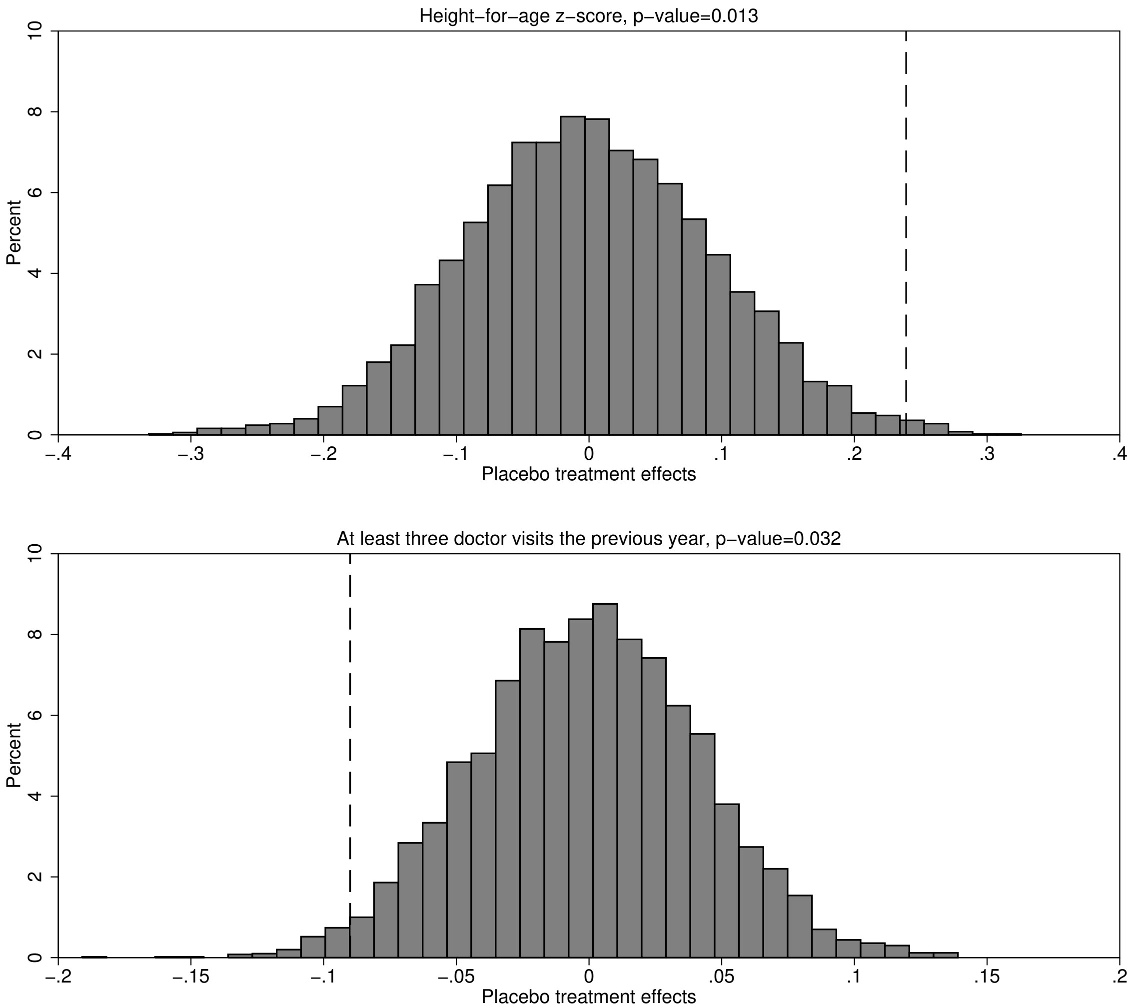
Notes: Children conceived one year before the first year of water connection are taken as the base group, so the coefficient for the year is zero.

Figure 2: Event study estimates of in utero access to treated water on childhood health



Notes: The figure plots the difference in pre-intervention characteristics by every five-year period for males and females born between 1955 and 1989 from communities receiving tap water connection between 1999 and 2008 (earlier) and communities not receiving tap water connection before 2008 (later). Each small figure is from a separate regression. Each dot on the solid line is the estimated coefficient on the interactions between five-year-cohort dummies and a binary variable indicating whether the community receiving tap water connection earlier, and the 95-percent confidence interval is plotted by solid line. The cohort of 1955 is taken as the base group. In addition to the above mentioned interactions, the model controls for community fixed effects, five-year-cohort fixed effects, province specific five-year-cohort fixed effects, and interactions between community characteristics and five-year-cohort dummies, where community characteristics include suburban village (=1 if yes, =0 if not), distance to nearest town (city), log(area), and topographic characteristics (hills, mountains, plateaus, plains, and others). Standard errors in parentheses are clustered at the community level.

Figure 3: Cohort trends in demographic characteristics by the timing of water infrastructure construction



Note: We randomly assign the tap water connection year to the communities and construct placebo treatment status for the same sample of individuals as that in the baseline analysis. The histograms display the distribution of placebo treatment effects from 5,000 random assignments. Dashed lines show the estimated treatment effects from the baseline analysis. *p*-value of each permutation placebo test is the proportion of placebo estimates that are equal to or larger in absolute value than the corresponding estimate from the baseline analysis.

Figure 4: Estimated coefficients from the permutation placebo tests

Table 1: Summary statistics

		Mean by connection years			Within-prov. mean diff.	
	Ratio of missing values	< 1999	1999-2008	> 2008	(3)-(2)	(3)-(4)
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Treatment variable						
Treated water IU^d	0	1.000	0.359	.000	-0.625*	0.341*
Panel B. Outcome variables						
Height-for-age z-score	0.078	0.132	0.034	-0.003	0.006	0.114*
Three or more doctor visits the previous year^d	0.030	0.253	0.297	0.281	0.084*	0.043*
Low birth weight^d	0.185	0.049	0.073	0.080	0.031	-0.013
Short gestation^d	0	0.025	0.032	0.041	0.017	-0.002
Congenital or neonatal diseases^d	0.067	0.002	0.005	0.007	0.003	-0.004
Three or more sicknesses in the first year of life^d	0.127	0.377	0.468	0.427	0.099*	0.053*
Three or more doctor visits in the first year of life^d	0.129	0.264	0.360	0.325	0.123*	0.059*
Weight-for-age z-score	0.064	0.051	-0.047	-0.026	0.022	0.054
Total health expenditure last year	0.032	434.500	349.300	362.900	-45.452	-66.664
Hospitalization last year^d	0.030	0.086	0.069	0.082	-0.041*	-0.027*
Waterborne infectious and parasitic diseases^d	0.072	0.035	0.033	0.031	-0.005	-0.013
Water quality sensitive diseases^d	0.072	0.526	0.492	0.486	0.037	-0.014
Water quantity sensitive diseases^d	0.072	0.008	0.011	0.017	-0.003	-0.006
Other diseases^d	0.072	0.061	0.064	0.067	-0.017	-0.005
Panel C. Individual and household controls						
Boy^d	0	0.536	0.514	0.547	-0.026	-0.032
Mother's age at birth	0.009	26.420	26.360	26.660	-0.123	0.006
Birth order	0.060	1.603	1.646	1.758	-0.024	-0.105*
Number of siblings	0.060	0.841	0.941	1.072	-0.010	-0.104*
Father's years of schooling	0.013	7.043	6.359	6.432	-0.457	0.029
Mother's years of schooling	0.018	5.957	4.832	4.825	-0.366	0.390*
NCMS IU^d	0.018	0.238	0.244	0.262	0.002	0.014
Health facility IU^d	0.011	0.515	0.595	0.415	0.144*	0.244*
Cable IU^d	0.032	0.646	0.417	0.309	-0.149*	0.093*
Electricity IU^d	0.027	0.981	0.937	0.834	-0.044*	0.090*
Road IU^d	0.017	0.850	0.660	0.615	-0.106*	0.059*
Landline phone IU^d	0.029	0.883	0.809	0.668	0.013	0.177*
Panel D. Community characteristics						
Suburban village^d	0	0.375	0.122	0.132	-0.187*	-0.019
Plains^d	0	0.335	0.291	0.316	-0.058*	0.081*
Hills^d	0	0.288	0.259	0.343	-0.010	-0.121*
Mountains^d	0	0.107	0.289	0.174	0.150*	0.065*
Plateaus^d	0	0.043	0.037	0.047	0.003	-0.036*
Other types of topology^d	0	0.226	0.124	0.121	-0.085*	0.011
Log of distance to nearest town/city (km)	0.012	2.569	3.173	3.184	0.535*	-0.122*
Average years of schooling of 25-55 year-olds	0	5.604	4.813	4.783	-0.357*	0.326*
Log of area (km^2)	0.080	0.919	1.291	0.617	0.454*	0.502*
Observations		645	804	1,854		

Notes: ^{d} denotes dummy variables. "Low birth weight" equals to one if birth weight is less than 2,500 grams and zero otherwise. "Short gestation" equals to one if gestation is less than nine months and zero otherwise. Columns (2)-(4) report raw means by connection years of tap water at the community level. Columns (5)-(6) report differences across communities with different years of connection within provinces. * denotes statistical significance at the 5% level.

Table 2: The impact of treated water on health at ages 2-11

Variables	Height-for-age z-score				Three or more doctor visits			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated water IU	0.249***	0.246***	0.247***	0.239**	-0.069	-0.081*	-0.081*	-0.090*
	(0.086)	(0.092)	(0.092)	(0.093)	(0.045)	(0.045)	(0.045)	(0.046)
Boy	0.009	0.017	0.017	0.017	0.008	0.004	0.004	0.005
	(0.034)	(0.034)	(0.034)	(0.034)	(0.016)	(0.016)	(0.016)	(0.016)
Mother's age at birth	-0.001	0.000	0.000	0.000	-0.004*	-0.005**	-0.005**	-0.005**
	(0.004)	(0.005)	(0.005)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)
Birth order	-0.072**	-0.075**	-0.074**	-0.075**	-0.019	-0.021	-0.021	-0.020
	(0.035)	(0.035)	(0.035)	(0.035)	(0.017)	(0.018)	(0.018)	(0.018)
Number of siblings	0.005	0.004	0.002	0.004	0.027	0.026	0.026	0.026
	(0.036)	(0.037)	(0.037)	(0.036)	(0.017)	(0.017)	(0.017)	(0.017)
Father's years of schooling	0.016**	0.017***	0.017***	0.018***	0.003	0.003	0.003	0.003
	(0.007)	(0.007)	(0.007)	(0.007)	(0.003)	(0.003)	(0.003)	(0.003)
Mother's years of schooling	0.009	0.008	0.008	0.008	-0.004	-0.005*	-0.005*	-0.005*
	(0.007)	(0.007)	(0.007)	(0.007)	(0.003)	(0.003)	(0.003)	(0.003)
NCMS IU			-0.001	-0.001			0.010	0.011
			(0.063)	(0.063)			(0.034)	(0.034)
Health facility IU			-0.173	-0.183			0.021	0.024
			(0.153)	(0.154)			(0.067)	(0.067)
Cable IU				0.060				-0.006
				(0.078)				(0.035)
Electricity IU				0.130				0.175
				(0.151)				(0.113)
Road IU				0.050				0.030
				(0.094)				(0.039)
Landline phone IU				0.104				-0.031
				(0.117)				(0.039)
Constant	-0.166	-152.067*	-142.191*	-145.443*	0.079	-0.966	-2.339	-0.288
	(0.118)	(81.555)	(83.117)	(84.508)	(0.055)	(36.815)	(37.658)	(37.163)
Observations	3,043	3,043	3,043	3,043	3,200	3,200	3,200	3,200
R^2	0.330	0.341	0.341	0.342	0.358	0.368	0.368	0.368
Number of clusters	387	387	387	387	387	387	387	387
Community FE & cohort FE	Y	Y	Y	Y	Y	Y	Y	Y
Province linear trends, community controls × linear trends	N	Y	Y	Y	N	Y	Y	Y

Notes: * means significant at 10%, ** significant at 5%, and *** significant at 1%.

Each column of each panel represents a separate regression. The individual and household characteristics controlled for in all specifications are gender, mother's age at birth, birth order, number of siblings, and parents' years of schooling. The community characteristics used to construct interactions with linear time trends are an indicator for suburban village, distance to nearest town/city in kilometers, area in square kilometers, average years of schooling for 25-55 year-old residents, and topographic characteristics. Health programs IU refers to in utero access to health facilities and access to health insurance. Other governmental programs IU refers to a series of dummy variables measuring in utero access to cable/satellite TV, electricity, road, and landline phone. Standard errors in parentheses are clustered at community level.

Table 3: Heterogeneous effects of treated water on childhood well-being

	Gender			Mother's years of schooling			Household income		
Subsamples	Boys	Girls	p-values	<median	≥median	p-values	<median	≥median	p-values
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Dep.var.: Height-for-age z-scores									
Treated water IU	0.369**	0.124	0.298	0.323**	-0.159	0.034	0.408**	0.174	0.232
	(0.170)	(0.186)		(0.130)	(0.230)		(0.179)	(0.147)	
Observations	1,629	1,414		2,101	890		1,293	1,622	
R^2	0.426	0.449		0.386	0.577		0.483	0.437	
Panel B. Dep.var.:Three or more doctor visits the previous year at ages 2-11									
Treated water IU	-0.094	-0.020	0.326	-0.051	-0.172	0.271	-0.095	-0.046	0.520
	(0.062)	(0.078)		(0.061)	(0.114)		(0.062)	(0.071)	
Observations	1,716	1,484		2,234	915		1,364	1,698	
R^2	0.472	0.462		0.427	0.550		0.509	0.478	

Notes: * means significant at 10%, ** significant at 5%, and *** significant at 1%.

Table 4: The impact of treated water on childhood diseases

	The type of most serious disease since birth			
	Waterborne infectious and parasitic	Water quality sensitive	Water quantity sensitive	Other
VARIABLES	(1)	(2)	(3)	(4)
Treated water IU	-0.006	-0.099**	0.002	0.035
	(0.019)	(0.047)	(0.011)	(0.027)
Observations	3,060	3,060	3,060	3,060
R^2	0.355	0.492	0.167	0.245

Notes: * means significant at 10%, ** significant at 5%, and *** significant at 1%.

Table 5: Validity tests

VARIABLES	(1) Height-for-age z-score	(2) Three or more doctor visits the previous year
Panel A1. Additionally controlling for parents' heights and missing indicators		
Treated water IU	0.243***	-0.089*
	(0.092)	(0.046)
Observations	3,043	3,200
R^2	0.347	0.369
Panel A2. Excluding communities that pre-trends cannot be plotted		
Treated water IU	0.246**	-0.091*
	(0.097)	(0.048)
Observations	2,449	2,572
R^2	0.339	0.363
Panel B. Placebo test		
Pseudo-treated water IU	0.037	-0.046
	(0.074)	(0.047)
Observations	3,043	3,200
R^2	0.340	0.368

Notes: * means significant at 10%, ** significant at 5%, and *** significant at 1%.

Table 6: Robustness tests

	(1)	(2)	(3)	(4)	(5)
Panel A. Reporting error					
	≥1994	≥1996	≥1998	≥2000	Reliable answers
	Dep. var.: Height-for-age z-score				
Treated water IU	0.230**	0.230**	0.238**	0.247**	0.242**
	(0.094)	(0.096)	(0.098)	(0.097)	(0.110)
Observations	2,647	2,594	2,475	2,436	1,845
R^2	0.337	0.340	0.340	0.340	0.347
	Dep. var.: At least three doctor visits last year				
Treated water IU	-0.085*	-0.086*	-0.091*	-0.092*	-0.102*
	(0.047)	(0.048)	(0.048)	(0.049)	(0.059)
Observations	2,793	2,734	2,606	2,559	1,960
R^2	0.377	0.375	0.364	0.364	0.395
Panel B. Measurement error (Dep. var. shown as column name)					
	Height-for-age z-score	At least three doctor visits	Height-for-age z-score	At least three doctor visits	
Treated water IU (Mid-year connection)	0.262**	-0.107**			
	(0.110)	(0.051)			
Treated water IU (Year end connection)			0.188*	-0.084*	
			(0.105)	(0.044)	
Observations	3,043	3,200	3,043	3,200	
R^2	0.342	0.369	0.341	0.368	
Panel C. Fertility					
	Dep. var.: whether giving birth in year t				
	Full sample	Age 15-19	Age 20-34	Age 35-49	
Treated water $_{t-1}$	-0.001	0.003	-0.010	0.002	
	(0.004)	(0.007)	(0.008)	(0.003)	
Observations	86,297	13,724	35,188	37,385	
R^2	0.063	0.047	0.072	0.035	
Panel D. Sample attrition (Dep. var. shown as column name)					
	Sample attrition in 2010				
	Children not in the final sample	a. Children not living at home	b. Children at home but not surveyed		
Treated water IU	0.019	-0.022	-0.003		
	(0.027)	(0.019)	(0.021)		
Observations	3,747	3,747	3,584		
Observations with dep.var.=1	448	163	285		
Observations with dep.var.=0	3,299	3,584	3,299		
R^2	0.356	0.258	0.397		
	Sample attrition in 2014				
	Children not surveyed in 2014	a. Household not surveyed	b. Children not living at home	c. Children at home but not surveyed	
Treated water IU	0.015	-0.002	-0.011	0.019	
	(0.044)	(0.034)	(0.029)	(0.029)	
Observations	2,414	2,414	2,208	2,131	
Observations with dep.var.=1	348	206	77	65	
Observations with dep.var.=0	2,066	2,208	2,131	2,066	
R^2	0.279	0.240	0.319	0.290	

Notes: * means significant at 10%, ** significant at 5%, and *** significant at 1%. The same specification as in column (4) and (8) of Table 2 is applied for estimation. Each block under each column in each panel contains the coefficient estimate of Treated water IU, standard errors (in parentheses) clustered at the community level, number of observations, and R^2, respectively. In Panel D, each dependent variable is a dummy variable. Among the 3747 children aged 2-11 in 2010, 163 were not at home, 285 children were at home but did not participate in the survey.

Appendix

A.1 Determinants of water infrastructure availability

While we are identifying the effect of treated water using variation in community-level access to water infrastructure over time, we acknowledge that this variation is certainly non-random. Because local government and residents take partial responsibility in financing, planning, constructing and managing the infrastructure, the timing of water infrastructure construction could be related to the location, wealth, and other characteristics of the community. To get a sense of what might have driven the introduction of water infrastructure, we estimate the correlation between community characteristics in 2010 and the probability and timing of water infrastructure construction. Population and area are proxies for the scale of a community, and average years of schooling is a proxy for the economic development of a community. Topographic characteristics capture the difficulty of constructing water infrastructure in the community.

Table A.1 presents the regression results. We find that location of a community is among the most important determinants for both the availability of water infrastructure in 2010 as well as the timing of construction.[24] A suburban community or a community being closer to a town or city is more likely to have access to water infrastructure as of 2010 and would receive infrastructure earlier than other communities located in the same province. We do not find any significant correlation for population, size, and average years of schooling of the 25-55 year-old, and topographic characteristics. In our preferred specifications, we control for interactions of all the community characteristics and linear time trends, to account for the difference time trends across communities with different characteristics.

A.2 Other health outcomes

The height in the baseline analysis is standardized to account for the difference by age and gender, and the dummy variable of at least three doctor visits the previous year is a measure of frequent doctor visit. To check if our estimates are sensitive to the transformations, we run the regressions using the outcomes in the original forms. As shown in Panel A of Table A.2,

[24] Among the 404 rural communities, 90 are located in suburban area, and the rest are located in rural area.

we find that having access to tap water in utero leads to a three centimeter increase in height for ages 2-11, and a 0.6 time decrease in doctor visits the previous year. Both estimates remain significant.

In addition, we examine the impact of access to water infrastructure in utero on birth outcomes and other childhood health outcomes. Birth outcomes include dummy indicators of low birth weight ($< 2,500$ g), short gestation (≤ 8 months), any congenital or neonatal diseases, at least three sicknesses in the first year of life, and at least three doctor visits in the first year of life. Since birth outcomes are reported by parents years after child birth, they are likely to be subject to measurement error. As long as the measurement error is not correlated with explanatory variable, our estimates are unbiased and consistent, but the standard errors of the estimates could be larger. In Panel B of Tabel A.2, we show that in utero access to tap water improves all birth outcomes, although the estimates for the former two are insignificant. Collecting better data may enhance our results.

Other childhood health outcomes include weight-for-age z-score, health expenditure the previous year (yuan), and dummy indicators of any hospitalization the previous year and any sickness the previous month, where weight-for-age z-score is obtained in the same way as height-for-age z-score as shown in Section 3.1. Again, all the health outcomes are improved with in utero access to tap water, although only the previous year health expenditure is significantly lowered.

To summarize, the results from regressions using original measures of childhood health indicators, birth outcomes, and other childhood health outcomes as dependent variables render support to our baseline findings that in utero access to tap water improves childhood health.

Table A.1: Regression analysis of community-level water infrastructure availability

VARIABLES	Having the infrastructure in 2010 (1)	Year first receiving the infrastructure (2)
Suburban village (0/1)	0.109*	-3.319***
	(0.064)	(1.105)
Log (distance to nearest town or city (hours))	-0.077**	1.732***
	(0.033)	(0.541)
Log (population)	-0.023	-0.909
	(0.041)	(0.632)
Log (area)	0.007	-0.018
	(0.012)	(0.197)
Average years of schooling of 25-55 year-old	0.018	-0.191
	(0.015)	(0.258)
Hills	-0.079	0.446
	(0.066)	(1.075)
Mountains	0.041	-0.478
	(0.097)	(1.460)
Plateaus	-0.054	-0.530
	(0.135)	(1.843)
Others	-0.012	-0.534
	(0.078)	(1.321)
Observations	404	404
R^2	0.198	

Notes: * means significant at 10%, ** significant at 5%, and *** significant at 1%.
We categorize the communities into five types by topographic characteristics: hills, mountains, plateaus, plains, and others. The type of plains is taken as the omitted group. Column (1) is estimated with OLS regressions. Column (2) is the marginal effects from Tobit regressions, conditional on community first receiving the infrastructure no later than 2010. Robust standard errors are in parentheses.

Table A.2: The impact of treated water on other health outcomes

	(1)	(2)	(3)	(4)	(5)
Panel A. Key outcomes in original forms					
	Height (m)	Number of doctor visits the previous year			
Treated water IU	0.030**	-0.598*			
	(0.013)	(0.313)			
Observations	3,043	3,200			
R^2	0.770	0.333			
Panel B. Birth outcomes					
	Low birth weight	Short gestation	Congenital or neonatal diseases	At least three sicknesses in the first year of life	At least three doctor visits in the first year of life
Treated water IU	-0.011	-0.023	-0.016**	-0.116**	-0.099*
	(0.038)	(0.017)	(0.007)	(0.055)	(0.058)
Observations	2,689	3,299	3,077	2,881	2,873
R^2	0.211	0.168	0.127	0.341	0.373
Panel C. Other childhood health outcomes					
	Weight-for-age z-score	Health expenditure the previous year	Whether hospitalized the previous year	Whether getting sick last month	
Treated water IU	0.015	-256.095***	-0.035	-0.055	
	(0.102)	(89.778)	(0.033)	(0.054)	
Observations	3,089	3,195	3,200	3,282	
R^2	0.298	0.281	0.198	0.250	

Notes: * means significant at 10%, ** significant at 5%, and *** significant at 1%.
Since community fixed effects are controlled for in the regression, we have accounted for the variation in health costs across regions.

Table A.3: Constructing treatment variable in alternative ways

	Height-for-age z-score	Three or more doctor visits	Height-for-age z-score	Three or more doctor visits	Height-for-age z-score	Three or more doctor visits
	(1)	(2)	(3)	(4)	(5)	(6)
Treated water IU (2^{nd} trimester)	0.190*	-0.087*				
	(0.100)	(0.045)				
Treated water IU (3^{rd} trimester)			0.181*	-0.077*		
			(0.095)	(0.044)		
Treated water IU (birth year)					0.157*	-0.056
					(0.085)	(0.039)
Observations	3,043	3,200	3,043	3,200	3,043	3,200
R^2	0.341	0.368	0.341	0.368	0.341	0.368

Notes: * means significant at 10%, ** significant at 5%, and *** significant at 1%.