ESSAYS ON THE IMPACT OF CLEAN WATER ON HUMAN CAPITAL AND PRODUCTIVITY

A Dissertation

Presented to

The Faculty of the Department

of Economics

University of Houston

In Partial Fulfillment

Of the Requirements for the Degree of

Doctor of Philosophy

By

Chon Kit Ao

May 2016

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Abstract

My dissertation investigates the effect of access to clean water on human capital. The first chapter examines the effect of municipal provisions of clean water—installation of water filtration plants—on school enrollment and child labor in American cities from 1880 to 1920. Numerous studies show that access to clean water reduces child mortality and morbidity, but little work has been done on the consequences for schooling and child labor. The effects are theoretically ambiguous because improved child health can raise schooling (e.g., better health makes education investments more productive) or lower it (e.g., the opportunity cost of attending school increases because there is a wage premium for healthier workers). Applying a difference-in-differences strategy which exploits variation of water filtration adoption across time and across cities, I find that municipal water filtration has a positive and statistically significant effect on school enrollment. Also, I find a negative effect on child labor, but it is not significant at conventional levels. These effects are most pronounced at ages 14 and 15, which map into the last years of elementary school and are beyond compulsory schooling age in some states. Additionally, I find that effects are larger for children who are exposed at an earlier age, can legally drop out of school, are from lower socioeconomic status families, or are female.

The second chapter uses a water services privatization program in Argentina during 1991 to 1999 to investigate the effect of early childhood exposure to clean water on educational attainment. By using a difference-in-differences strategy which exploits variation across regions and across cohorts, my results show that early childhood exposure to privatization has a zero effect on primary and compulsory school completion, and a small negative effect on secondary school completion. Furthermore, I find that the effect of this privatization program is heterogeneous across individuals who lived in nonpoor and poor municipalities. I find that, for primary and compulsory school completion, early childhood exposure to privatization has a zero effect in nonpoor municipalities and a negative effect

in poor municipalities. A supplemental analysis adding information on years of primary school completed among individuals who did not complete primary schooling indicates that privatization induces individuals in poor municipalities to drop out of school at 5th, 6th, and 7th grade, which correspond to the final years of primary school.

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to Chon-Man

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Chapter 1

The Effect of Municipal Water Filtration on Children's School Enrollment and Employment in American Cities, 1880–1920

1.1 Introduction

Water is important for life. However, many people in developing countries still lack access to safe drinking water. In 2015, there were still 663 million people who did not have access to safe drinking water (WHO and UNICEF 2015).[1] This poor access to clean water exposes human health to great risks of deadly waterborne diseases such as cholera, diarrhea, malaria, typhoid and paratyphoid enteric fevers, and other water-related diseases.[2] For instance, the

[1] Safe water in the report is defined by water source which is adequately protected from outside contamination, particulary faecal matter.

[2] WHO webpage: http://www.who.int/water_sanitation_health/diseases/diseasefact/en/

World Health Organization (WHO) reports that diarrhoeal disease alone causes the death of 1.5 million people every year.[3]

This study focuses on the United States from 1880 to 1920 to investigate the effect of access to clean water on human capital investment, particularly school enrollment and child labor. During the late nineteenth and early twentieth centuries, many American cities started to install water filtration plants to purify their water supply. The city-time variation in the installation of filtration plants permits me to use a difference-in-differences strategy to identify the causal impact of municipal water filtration. Intuitively, this estimate is obtained by taking the post-intervention/pre-intervention difference in child outcome in an adopting city, and using cities that never adopt or adopt it later to control for the changes in child outcome over time that would have occurred for reasons unrelated to the policy.

The effects of water access and quality on health improvement are widely discussed in the literature (Troesken 2002; Jalan and Ravallion 2003; Cutler and Miller 2005; Ferrie and Troesken 2008; Mangyo 2008; Galiani, Gertler, and Schargrodsky 2005; Galiani, Gonzalez-Rozada, and Schargrodsky 2009; Gamper-Rabindran, Khan, and Timmins 2010; Ahuja, Kremer, and Zwane 2010; Kremer et al. 2011; Zhang 2012; Alsan and Goldin 2015). One closely related study is David Cutler and Grant Miller (2005), which uses city-time variation in water purification technologies for 13 U.S. cities during the early twentieth century, and finds that water purification reduced infant mortality and child mortality by 46% and 50%, respectively.[4] Given such a large effect of clean water on child health, and in the context of a growing body of evidence indicating complementarities between childhood health and

[3] WHO webpage: http://apps.who.int/gho/data/node.main.CODWORLD?lang=en

[4] The authors look at four water purification technologies in 13 cities: water filtration, water chlorination, sewage treatment, and sewage chlorination. They conclude that water filtration and chlorination accounted for a large decline in typhoid, total, infant, and child mortality in the early twentieth century, their regression results always show statistically significant effect from water filtration, but small and not statistically significant effect from water chlorination. Furthermore, they also mention that their analysis could not examine the effect of sewage technologies since these modern technologies were not in widespread use in the U.S. until the 1930s and 1940s.

education (Glewwe, Jacoby, and King 2001; Miguel and Kremer 2004; Bobonis, Miguel, and Puri-Sharma 2006; Bleakley 2007, 2010), it is important to understand how access to clean water affects human capital investment such as school enrollment and child labor.

To date, there have been only a few studies that look at the possible effects of water on educational outcomes (Devoto et al. 2012; Bhalotra and Venkataramani 2013; Beach et al. 2014; Kosec 2014; Xu and Zhang 2014). This study adds to this under-studied topic by using a different source of variation in exposure to clean water: municipal water filtration policies in the United States. Furthermore, unlike most of the previous studies which use natural experiments that change only water access, or that change both water access and quality at the same time, I examine an intervention which changes water quality. That is, the American cities I analyze had piped water at the outset, and what the filtration policy brought to city dwellers was higher quality water. It may be that the impacts of water access and water quality are different, and I provide some of the first evidence of water quality on children's school enrollment and employment. In addition, I provide evidence on the effect of municipal water filtration for a much larger set of cities than has previously been done. I assemble filtration plants information for 74 cities during 1880–1920 from various historical sources, enabling me to evaluate these policies on a wider scale than has been done.[5] Did the U.S. municipal water filtration policies play a role in the rise in educational attainment observed for people who were children at the turn of the twentieth century? This question is important both from a historical standpoint—possibly these municipal infrastructural projects had more far-reaching effects than has been previously documented—and may be relevant for developing countries that are considering these investments by providing more information about the benefits and costs.

[5]I consulted various historical books, articles, and reports to obtain complete information on filtration plants installation for 74 U.S. cities from 1880 to 1920. The data section describes the formation of city sample.

A priori, the effect of municipal water filtration on school enrollment is unclear. For example, after municipal water filtration, children are healthier and are less likely to get sick, which allows those previously weak and sick children to go to school. At the same time, water filtration may also improve children's ability to focus and concentrate in class, so that they may perform better in school. All of these reduce the costs of going to school and increase the return to education, so parents may be more likely to send their kids to school. On the other hand, healthier children can be more productive and, therefore, be more valuable in the labor market. This increases their opportunity costs of going to school and has a negative effect on school enrollment. These two opposing stories imply that the effect of municipal water filtration on school enrollment is an open question.

There is substantial time variation in water filtration adoption in my city sample.[6] I merged this city-level water filtration adoption data with the U.S. Census microdata from 1880 to 1920 to estimate the impacts of water filtration on children's school enrollment and employment. Applying a difference-in-differences strategy which exploits variation across cities and across time, my results show that municipal water filtration increases school enrollment by 2 percentage points (compared to the sample mean of 86%). These results are not sensitive to controlling for city-specific trends or state-specific year effects, raising confidence that they reflect causal impacts of the policies rather than differential trends between earlier adopters and later/never adopters. On the other hand, water filtration has a negative effect on child labor, but the effect is not statistically significant. I further investigate which subgroups of people are more affected by the policy intervention. I find that filtration has the largest effect on children who are 14 to 15 years old, which are the ages of finishing elementary school and in some states beyond compulsory schooling age during my studied

[6]For the 74 cities which I am able to obtain complete information on water filtration from 1880 to 1920, different cities adopted the filtration at different time, and eventually 56 cities adopted the intervention by 1920. The details of the time variation of filtration adoption is discussed in Section 1.3.2.

time periods. Furthermore, I also find that when children are at their sensitive ages of finishing elementary school (14 to 15 years old), early childhood exposure to clean water has a larger impact on school enrollment and child labor. Finally, I investigate other heterogeneous effects of water filtration by legal school dropout status, by household socioeconomic status, by gender, and by race. The results suggest that the effects come from children who can legally drop out of school. The effects are also larger for children from lower socioeconomic status households. The results of gender difference indicate that, compared to males, females have stronger responses to water filtration in terms of increases in school enrollment and decreases in child labor. This is consistent with female's comparative advantage in skill acquiring activities. I also find suggestive evidence that black children in the South benefit more from the intervention.

The rest of this study is structured as follows. I review the literature in Section 2.2. Section 1.3 discusses the background of water filtration, how it affects water quality in the U.S. cities, and filtration adoption in American cities. In Sections 2.4, I present data of children's school enrollment and employment and municipal water filtration. Section 1.5 discusses the identification strategy and presents empirical results. Finally, Section 2.7 concludes this study.

1.2 Literature Review

The research on the effect of clean water on education is limited. Devoto et al. (2012) employ a randomized design in Morocco to examine the non-health effect of household water connection. They find that household water connection had no effect on children's school participation, but had a positive effect on household well-being such as increased leisure time.[7] Bhalotra and Venkataramani (2013) study a clean water program in Mexico in 1991 and find that infant exposure to clean water program increases test scores for girls by 0.1 standard deviation, and has no effect on boys' test score. Kosec (2014) examines the effect of increase in access to piped water by the private sector participation in the piped water sector in 39 African countries during 1986–2010. The author finds that increase in access to piped water reduces diarrhea of children under age five in urban area, and is associated with a 7.8 percentage points increase in school attendance. Furthermore, the positive association between access to piped water and school attendance is only statistically significant for children aged 11–13 and 17, where children are at transition points in their education. To investigate the long-term effect of clean water, Xu and Zhang (2014) employ a large drinking water treatment program in rural China and shows a positive effect on schooling attainments.[8] Similarly, Beach et al. (2014) use typhoid fatality rate as a proxy of water quality, then link males in 1900 and 1940 in the U.S. They find that eradicating early-life exposure to typhoid fever had positive effect on years of schooling and later life earnings. The present study contributes to this handful of literature on the effect of clean water on educational outcomes, which highlights this important channel in the larger literature of the benefits of clean water programs.

Numerous papers have examined the impact of clean water on health, including mortality (Troesken 2002; Jalan and Ravallion 2003; Cutler and Miller 2005; Galiani, Gertler, and Schargrodsky 2005; Ferrie and Troesken 2008; Gamper-Rabindran, Khan, and Timmins 2010; Alsan and Goldin 2015) and health conditional on survival (Mangyo 2008; Galiani, Gonzalez-Rozada, and Schargrodsky 2009; Ahuja, Kremer, and Zwane 2010; Kremer et al. 2011; Zhang 2012). These studies look at the contexts within the U.S. and across different

[7] In the paper, since all sample households have access to clean water from public tap, the randomized design of in-home water connection will not improves water quality.

[8] Education attainments are grades of education completed, binary variables of graduated from middle school and graduated from high school.

countries. Several are in the setting I examine, the U.S. in the later 1800s and early 1900s. Troesken (2002) finds that water filtration reduced both black and white typhoid mortality during 1880–1925. Troesken (2001) finds, compared to private ownership of municipal water company, public ownership had larger effect in reducing black typhoid mortality. Ferrie and Troesken (2008) show that water purification programs in Chicago from 1850–1925 decreased death rates from typhoid fever and diarrheal diseases, the program had diffused effect on other water-unrelated diseases. Cutler and Miller (2005), using data on 13 cities from 1900 to 1936, find that water purification technologies strikingly reduced typhoid mortality by 25%, total mortality by 13%, infant mortality by 46%, and child mortality by 50%. In addition, they find a larger mortality reduction on the poor. Given the dramatic health benefits of clean water found by these studies, it is of interest to examine what consequences there were for child school enrollment and labor. This study provides a more comprehensive picture of the effect of clean water on human capital investment from a broader set of cities (74 cities, see data section). This extends the knowledge of policy makers about the various benefits from the clean water program, especially the knowledge that clean water also influences human capital of survivors, which is essential to economic development. Ignoring the causal effects of water-related programs on human capital investment will underestimate the benefits on implementing water improvement programs.

More generally, this study is relevant to a large literature on the effect of early childhood environment. Given the well documented causal effect of childhood health environment on later life outcomes in the literature (see handbook chapter of Almond and Currie (2011)), the present study complements this literature by examining the effect of early childhood exposure to clean water.

My study also relates to the literature on determinants of child labor, which mostly focuses on the effect of child labor laws and changes in household incomes on child labor (see handbook chapter of Edmonds (2007)). I investigate the effect of a health intervention—municipal water filtration—on children's employment. This adds another dimension in determinants of child labor.

1.3 Background

1.3.1 Water Filtration and Water Quality

The initial purpose of water filtration was for aesthetic improvement—reduce turbidity, bad taste, and discoloration. However, before the understanding of disease transmission by microbes, public health experts acknowledged that unclean drinking water could be the source of disease epidemics (McGuire 2006). By the 1870s and 1880s, with the development of bacteriology and the understanding of how waterborne diseases could be transmitted, public health protection became the primary reason for water filtration (Journal of the American Medical Association 1903a; Logsdon and Ratzki 2007). A water filter is constructed by multiple layers of large and smaller stones, coarse sand, and gravel. When turbid raw water passes through the filter, color and particulate matter are removed, and the slimy matting organic material on the surface of the sand will entrap, digest, and break down bacteria and organic matter contained in the water (Hendricks 1991).

Water filtration has been shown to be effective at improving water quality. One type of evidence is from scientific testing of water for presence of harmful matter. A 1903 paper in the Journal of the American Medical Association shows that filtration dramatically reduces bacterial concentration. Table 1.1 shows the bacterial concentration of the water before and after filtration in the city of Lawrence, Massachusetts. After the water filtration, the average number of bacteria per cubic centimeter decreased from 10,800 to 110, a 99% reduction, in 1894. Statistics show the filter is effective in the following years, with later years' data focusing on E. coli concentration; E. coli is especially harmful to human health and it is clear that filtration reduces the prevalence of E. coli. A second type of evidence is based on studies of the impact of water filtration installation on health outcomes. For example, Cutler and Miller (2005) find a decline in child mortality using a sample of 13 U.S. cities, and Logsdon and Ratzki (2007) find that a decline in deaths due to typhoid fever.[9]

1.3.2 Water Filtration Adoption in American Cities

Many American cities started to install water filtration plants to purify their water supply in the late nineteenth and early twentieth centuries. Although there is no major city which adopted water filtration by 1880, the distinguished performance of the sand filter in the the city of Lawrence, MA marked "the opening of the modern era in water purification in America" (Journal of the American Medical Association 1903b). Historical accounts show substantial time variation in the municipal adoption of water filtration.

My empirical analysis uses 74 major cities which are identified in Census microdata and for which I was able to obtain complete information on municipal water filtration for the 1880–1920 period.[10] Figure 1.1 illustrates the 74 cities in my main sample. The cities were spread over the U.S.—in 30 different states—though concentrated more on the east coast (this is where most populous cities were located at that time). Consistent with historical accounts, Table 1.2 shows the considerable time variation of filtration plants adoption in my sample period. None of the 74 cities had installed a water filtration plant by 1880, but three-quarters installed by 1920, with 11 cities installing between 1881-1900, 29 between between 1901-1910 and 16 between 1911-1920. I use the year in which the city's filtration

[9] The cities analyzed in Logsdon and Ratzki (2007) are: Albany, NY; Cincinnati, OH; Columbus, OH; Indianapolis, IN; Lawrence, MA; Louisville, KY; McKeesport, PA; New Orleans, LA; and Washington, DC.

[10] The details of how the city sample is formed is described in the data section.

plant is first operational as year of adoption for the city.[11]

There are many reasons for the variation in timing of water filtration adoption by American cities. Bureaucratic, legal, and political factors can speed up or slow down the process of filtration adoption. Some obstacles to adoption included: lobbying state legislatives to raise debt; convincing the public that the benefits of filtration plant outweighs the costs; debates of whether the plant should be operated by city government or by private contract; and lawsuits by opponents against the major city expenditure. Even after the city government approves a filtration plant, it could take years for construction to be completed. For example, for Philadelphia, because of the political complications and length of construction time, it took more than a decade from the approval to actual operation of the plant. This example highlights that cities cannot precisely time when water filtration is operational. These various hurdles to water filtration adoption introduce a certain degree of randomness to whether, and when, a city adopts water filtration plants installation, so it is unlikely that timing of adoption is endogenous when examining children's school enrollment and employment (at least, conditional on city fixed effects).[12]

It is worth mentioning that water filtration is not the only water purification technology available during the early twentieth century. Other methods or technologies such as water chlorination or modern sewage treatment were also available to purify water. However, Cutler and Miller (2005)'s mortality reduction results show a large and statistically significant effect from water filtration, but a small and not statistically significant effect from water

[11] Note that cities usually kept expanding their water filtration system after the first operation. This implies that more households would have access to clean water after the municipal adoption of filtration. Therefore, while my estimates below will always give the effect of municipal water filtration, since not all households get access to filtered water after the first year of adoption, my estimates of the effect of water filtration in the empirical analysis could possibly be underestimated. In other words, from the perspective of obtaining causal effects of having filtered water, my estimates give intention to treat effects, and one would have to divide by the take-up rate (e.g., share of households in the city getting filtered water) to get the effect of treatment on the treated.

[12] See McCarthy (1987), Troesken (2002), Cutler and Miller (2005), Cutler and Miller (2006) for discussions.

chlorination,[13] while the modern sewage treatment was not common until the 1930s and 1940s. Note that in the context of the present study, even if the cities actually adopted water chlorination or other water purification technologies, my municipal water filtration intervention still serves as the effect of "cleaner water", given the effectiveness of water filter described in Section 1.3.1.[14]

1.4 Data

To investigate the effect of municipal water filtration on human capital investment, I require data on children's school enrollment and employment and date of city installation of water filtration plants. I obtain the former from U.S. Census microdata, and the latter from various historical sources, as I describe below.

Individual-level data on school enrollment and employment are taken from the U.S. *Integrated Public Use Micro Sample* (IPUMS) (Ruggles et al. 2015). I use four censuses: 1880, 1900, 1910, and 1920.[15] To maximize the number of cities in my analysis, I use the 1% and 10% samples in 1880, the 1% and 5% samples in 1900, and the 1% sample in both 1910 and 1920.[16] In my analysis, school enrollment and employment are binary variables.

[13]The authors claim that this could be due to two possible reasons. The first reason is because major cities generally adopted filtration before chlorination, so there is little mortality variation left to identify the effect of chlorination. The second reason is the less time variation of chlorination adoption cross cities due to its inexpensive costs of implementation; after the first full-scale application of chlorination in Jersey City, NJ in 1908, many other cities started to use chlorine in their water supple in the following decade due to its inexpensive costs (McGuire 2006).

[14]In fact, reports and articles claim that there is an increase of interest in using filtration since the application of chlorine alone cannot remove particulate matter in surface waters and chlorine-resistant pathogens (LeChevallier and Au 2004; McGuire 2006). Moveover, historical accounts show that there was objections and complaints in applying chlorine in drinking water because the unbalanced dosage causes a distinct and disagreeable taste and smell remain in the water (McGuire 2013).

[15]Microdata from the 1890 census is not available because much of the records were destroyed by fire.

[16]In some instances, different samples under one census year provide information for different cities. For example, in 1880 census, information for Austin, TX and Denver, CO is available in the 1% sample but not in the 10% sample. For my analyses I use sample weights that are adjusted for the fact that the population share of the sample varies by census year.

The school enrollment dummy equals one if the child attended or was enrolled in school in the months before the census day. The employment dummy equals one if the child reported an occupation.[17] The occupation question is only available for individuals age 10 or higher in 1880 and 1900, leading me to restrict my analysis of employment outcomes to individuals at least age 10.

To construct my data set on city water filtration policies, I began with a list of the large cities in that period (specifically, those having a population of over 30,000 in 1920). I then consulted numerous historical sources to find out each city's water filtration policy over the 1880–1920 period.[18] From these sources, I found filtration plant information for 154 cities. For 73 of these cities, I have both complete information of filtration plant installation for the 1880–1920 period, as well as Census microdata for each of the four censuses. There are 21 cities which have complete city filtration policy data, but lacks Census data from 1880.[19] I also have partial filtration policy information for 59 cities, in which they either did not have filter until 1914 (lacking information between 1915 to 1920) or had water filtered by private corporations but installation dates are not available. In my main analysis, I exclude these 80 (21 + 59) cities which are not identified in the 1880 Census or which have incomplete information on filtration policy.[20] Finally, following the same logic of Cutler and Miller (2005)

[17]Different censuses use different occupational classifications, and for constructing the employment variable I used IPUMS' harmonized occupation variable, which translates occupational classifications in different years into one classification system.

[18]My main sources were: *General Statistics of Cities: 1915* by the United States Bureau of the Census; "Filtration Plant Census, 1924" published in *Journal of the American Water Works Association*, which is compiled by C.G. Gillespie who was director of the Sanitary Engineering Bureau of the California State Board of Health; a set of three papers titled "Design and Operation Data on Large Rapid Sand Filtration Plants in the United States and Canada" published in *Journal of the American Water Works Association* (Hardin 1932, 1942; Cosens 1956). Supplementary sources included: various issues from "Water Survey Series"; "Water Supply Paper"; "The Purification of Public Water Supplies" (Johnson 1913); "Present Day Water Filtration Practice" (Johnson 1914); "Census of Municipal Water Purification Plants in the United States, 1930-1931" (Wolman 1933); "Manual of Design for Slow Sand Filtration" (Hendricks 1991).

[19]These 21 cities are not identified in the 1880 microdata. According to IPUMS, city's identity is given for households in any city with over 10,000 inhabitants in 1880, and over 25,000 inhabitants in 1900, 1910, and 1920.

[20]As I discuss below, adding in these 80 cities with incomplete data (either missing a census year or missing

who use information for smaller cities with good and readily available historical information, one city with population slightly below the 30,000 in 1920—Steubenville, Ohio—is also used in my analysis.[21] The 74 cities entering my main analysis are mapped in Figure 1.1.

To the Census microdata I merged in city-level data on municipal water filtration (when, if at all, the city installed a water filtration plant). My main analysis focuses on native born white and black children who are 10 to 15 years old. The age 10 cutoff is motivated by my interest in analyzing both enrollment and employment, and the employment variable is available for individuals age 10 and higher in the earlier censuses. On the other hand, age 15 would typically be the end of elementary school, in a period when completing elementary school was far from universal and not many attended secondary school. Early in the twentieth century, the typical school system in the U.S. had eight years of elementary school and four years of secondary school (United States. Bureau of Education 1912). Table 1.3 describes the typical school structure in 1911. Children typically entered first grade at age 7 or 8, which means that with on-time grade progression students would be finishing elementary school at eighth grade at age 14 or 15. It can be noted that for some analysis, I use a broader age range, 7 to 19 years old, to provide evidence to support the age criteria of 10 to 15 years olds being responsive to municipal water filtration policy while younger and older children are not.

Table 2.1 shows the summary statistics for the sample used in my main analysis. The average age is 12 years old, 86% of the sample reported that they had been enrolled or attended school at least one day preceding the Censuses and 10% of the sample reported an occupation. 7% of the sample is black children and approximately 7% of the sample had an

some years of water filtration information) lead to substantially similar results (see Section 1.5.9).

[21]The city of Steubenville, Ohio has population of 28,500 according to 1920 census. My results are similar by when this city is excluded from the analysis.

illiterate household head. Around 49% of the sample is male.

1.5 Empirical Strategy and Results

The staggered installation of water filtration plants by the U.S. cities over the 1880–1920 periods permits me to use a difference-in-differences framework to investigate the effect of municipal water filtration.

1.5.1 Model Allowing for Age-Specific Effects

I begin by estimating the effect of municipal water filtration for each age (7 to 19). To allow the model to be fully flexible with age, I estimate the following equation:

$$y_{ict} = \alpha + \Sigma_{a=7}^{19} \beta_a (Filter_{ct} \cdot d_a) + \gamma_{ca} + \lambda_{ta} + \Sigma_{a=7}^{19} (\mathbf{x}_{ict} \cdot d_a) \delta_a + (\rho_c \cdot t) + \varepsilon_{ict} \quad (1.1)$$

where y_{ict} is either the school enrollment dummy or employment dummy for individual i who lives in city c in year t. $Filter_{ct}$ is an indicator variable which equals one if city c adopted water filtration in year t or zero otherwise. d_a is a dummy that equals one if the individual is age a. γ_{ca} represents vector of city-age fixed effects, which is city dummies time age dummies. λ_{ta} represents vector of year-age fixed effect, which is year dummies time age dummies. $\mathbf{x}_{ict}$ is a vector of demographic characteristics such as household head illiteracy, gender, and race dummies. $\rho_c \cdot t$ represents city-specific linear time trends which account for the possibility that cities are different in systematic and time-varying way between each other. ε_{ict} is the error term. In this unrestricted model, each coefficient β_a represents the effect of water filtration for children of a given age. The identifying assumption of equation (1.1) is that in the absence of water filtration, cities with and without water filtration have had the same changes over time in children's outcomes (this is the parallel trend assumption, however note it is made after conditioning on city-specific time trends, so it is less restrictive than the standard parallel trend assumption for basic difference-in-differences specifications).

Figure 1.2 plots the estimated coefficient $\widehat{\beta}_a$ with 95% confidence intervals from equation (1.1). The dependent variable is children's school enrollment in Panel A and employment in Panel B. In Panel A, the estimates show that water filtration has the largest positive effect on school enrollment for ages around 14 years old, in particular age 14, and the effect is close to zero for younger and older ages. The estimates in Panel B show a similar pattern in which water filtration has the largest negative effect on children's employment for ages around 14 years old, again largest effect at age 14, then the coefficients fluctuate around zero for other ages. Note that no estimated effect is shown for ages 7 to 9 in Panel B because the occupation question is asked only for children age 10 and above. In sum, Figure 1.2 shows that municipal water filtration affects school enrollment and child labor, and the effect is different at different ages.

The possible explanations for the observed pattern in Figure 1.2 are that, first, given the school structure mentioned in Section 2.4, 14 and 15 are typical ages in which children are at the last year of elementary school during my studied period (see Table 1.3). Second, most of the states allow children to legally drop out of school at around 14 years old during my sample period (see Table 1.14). These imply ages around 14 are sensitive ages for educational transition. Similar to Kosec (2014) (see Section 2.2), I observe the largest effect of water filtration when children are at their transition points of education.

In the following analysis, I pool children who are ages 10 to 15 to gain more power and investigate the average effect of water filtration on this sensitive age group of children.

1.5.2 Baseline Model

I use a difference-in-differences approach to examine the average effect of water filtration on children who are 10 to 15 years old. Specifically, I estimate the following equation:

$$y_{ict} = \alpha + \beta Filter_{ct} + \gamma_c + \lambda_t + \mathbf{x}_{ict}\tau + (\rho_c \cdot t) + \varepsilon_{ict} \tag{1.2}$$

γ_c represents vector of city dummies which control for city characteristics that do not vary over time, λ_t represents vector of year dummies which account for shocks or changes that are common to all cities. $\mathbf{x}_{ict}$ is a vector of demographic controls including household head illiteracy, gender, race, and age dummies. All other variables are defined in equation (1.1). In this equation, β is the difference-in-differences estimator of the effect of municipal water filtration. In order to interpret the difference-in-differences estimate from equation (1.2) as the causal impact of municipal water filtration, the parallel trend assumption must hold: in the absence of municipal water filtration, the change over time in enrollment (or employment) is the same between earlier-adopting cities and cities that adopt later or never adopt.

Table 1.5 shows the estimated results of equation (1.2). Each column reports the results of a separate regression with model specification denoted in the last two rows, and the dependent variable and its mean indicated on top of the columns. All regressions control for age fixed effects, city fixed effects, year fixed effects, and are weighted by sample weight. Robust standard errors are clustered at the city level (74 clusters), and they are consistent under heteroscedasticity as well as within-city serial correlation.

Columns (1) to (2) show the estimated effect of municipal water filtration on children's school enrollment with and without city-specific time effects. Without controlling for the city-linear trends in column (1), the coefficient of filter shows a positive effect of water filtration on school enrollment that is statistically significant at the 10% level. It turns out that the inclusion of city-specific time trends does not materially change the estimated policy effect, as can be seen by comparing column (2) to column (1). This lends confidence that the difference-in-differences estimate is really capturing the causal impact of municipal water filtration, as differential trends in enrollment between adopting and later/never-adopting cities do not appear to be a confounding factor (if differential trends were a problem, then the estimates in the two columns would differ). The estimate in column (2) indicates that municipal water filtration increases school enrollment by 1.9 percentage points, and this is statistically significant at the 5% level.

Columns (4) to (5) show the estimated effect of municipal water filtration on child labor. Without controlling for any city-specific time effects, the coefficient in column (4) shows that water filtration has a negative and statistically significant effect on child labor. However, the estimate becomes very small and is not statistically significant after controlling for the city-specific linear trends in column (5). The difference in results is suggestive of differential trends in child labor between adopting and later/never-adopting cities, and in subsequent analysis the preferred specification includes city-specific time trends. Based on column (5), I find there is no statistically significant effect of municipal water on child labor, although it can be noted that the point estimate is negative and meaningfully sized (as the mean employment rate is 10% in the sample).

The coefficients on the control variables are statistically significant and with expected sign. Children from lower socioeconomic status household, which is captured by household head illiteracy, are less likely to enroll and more likely to work. In addition, both blacks and males are less likely to go to school and more likely to work.

1.5.3 Assessing the Parallel Trend Assumption

A possible concern to the difference-in-differences strategy is that there could be differential trends across treated and never treated cities. To handle this issue, I employ model specifications that allow for differential trends across cities, which have been shown and discussed in Table 1.5 columns (2) and (5). However, there could still be concerns about time effects that are different between adopting and later/never-adopting cities but not of the smooth variety that could be accounted for using city-specific trends. I explore this below.

One concern might be that local economic and political conditions might affect the adoption of various amenities, not only water filtration. Perhaps the difference-in-differences estimates are also picking up the effects of other variables, such as better economic conditions, tougher compulsory schooling laws or tougher child labor laws. To assess if this is indeed a problem, I take advantage of the fact that my policy variation is at the level of city-time, which means that it is still possible to identify the difference-in-differences estimate even when I control for state-specific time effects. State-year fixed effects control for all time-varying variables that have common effects among cities within the same state, and include state economic conditions, programs and policies (note compulsory schooling laws and child labor laws tend to be set at the state level).

Table 1.5, column (3) shows the results of estimating equation (1.2) after adding state-year fixed effects, which take care of state-specific time shocks and policy changes, notably state's compulsory schooling laws and child labor laws. Even though this specification throws away all of the cross-state variation in the policy variable, and is still allowing for differential trends across cities, the estimated effect of water filtration on school enrollment is essentially unchanged. The remarkable stability of the difference-in-differences estimate of the effect of municipal water filtration on school enrollment across the specifications encapsulating different assumptions about city-specific time effects, which provides evidence that the estimate is not driven by any city-specific time patterns.[22] For child labor, it can be seen that adding state-year fixed effects yields (column (6)) yields similar results as column (5), suggesting that while there appear to be smooth trend differences in child labor between filter cities and other cities, state time-varying variables do not appear to be a source of incremental bias.

Another way to assess the validity of the parallel trend assumption is to graph the mean outcome by year and filter status, and consider whether in the pre-policy period the filter and no-filter cities do indeed have parallel trends. To preview the results, the graphical analysis indicates support for the parallel trend assumption in the case of school enrollment, but not in the case of child labor, mirroring the earlier results in which estimates of the effect on schooling are insensitive to controls for city time effects while estimates of the effect on child labor are sensitive.

In my context, different cities adopt filtration in different years, so there is no straightforward way to depict all the data used in my regression analysis on the same graph. An additional issue is that for cities adopting filtration in 1900 or earlier, I only have one pre-policy data period (the 1880; recall the 1890 census are not available because records were destroyed in a fire), which does not permit an assessment of pre-policy trends. To make progress, I compare cities which never adopted the intervention to cities which adopted water filtration in a between (1901–1910) or a later (1911–1920) period separately in each graph. Because of the lack of 1890 census, even the comparison between cities with intervention during 1901–1910 versus never-treated cities, and comparison between cities with

[22] I also try other model specifications such as state-specific linear trends or state-year fixed effects along, the estimated effect of water filtration on school enrollment are unchanged (The results are not reported and are available upon request).

intervention during 1911–1920 versus never-treated cities are not ideal since some of the pre-treatment periods have 20-year intervals and others have 10-year intervals.

Figures 1.3 and 1.4 show the mean comparison of school enrollment and child labor for treated versus never treated cities, respectively. The solid line represents the mean enrollment in each census year for cities which adopted water filtration in the mentioned time period. The dashed line represents the mean enrollment for cities which never installed a filtration plant. The dotted vertical line located in specified year indicates the census before the intervention, with left side of the line representing pre-treatment periods and right side representing post-treatment periods. Panel A compares cities which never had the intervention to cities which adopted water filtration during 1901–1910. Panel B compares cities which never had the intervention to cities which adopted water filtration in 1911–1920. Broadly speaking, the school enrollment in Figure 1.3 shows similar pre-trends between never treated and treated cities (left side of the dotted vertical line). In the post-treatment periods (right side of the dotted vertical line), treated cities started to catch up after the intervention and eventually surpassed the never-treated cities.

Doing the same comparisons in Figure 1.4, children's employment in treated cities started to catch up after the intervention (right side of the dotted vertical line) in both Panels A and B. Panel B shows a parallel pre-trends pattern, but the trends of employment in pre-treatment periods do not look parallel in Panel A. This pre-trend pattern of children's employment is consistent with the earlier finding that the estimated effect of filtration on child labor is sensitive to the inclusion of city-specific trends (columns (4)–(5) in Table 1.5).[23]

[23]Borrowing the logic from Figures 1.3 and 1.4, I implement placebo tests which compare two pre-policy years. Specifically, I compare cities which adopted water filtration in census year 1910 (or year 1920) to cities which never adopted the intervention, then I assign a fake treatment dummy to the census year right before the actual adoption year to estimate the placebo treatment effect of water filtration. Although these placebo tests are not ideal (for the same reasons mentioned for the graphical analysis), the results generally support the parallel trend assumption in the case of enrollment, and do not support it in the case of employment (The

Taken all together, the similar results on school enrollment under different assumptions about time effects in Table 1.5, plus the pre-trends comparison in Figure 1.3, give us confidence about the validity of the parallel trend assumption. However, there do appear to be differential trends for the child labor outcome. Therefore, in the following analyses, I regard model specification which controls for city-specific linear trends as the preferred specification throughout this study.

1.5.4 Early Childhood Exposure

One natural question is whether the effect of municipal water filtration varies by age at first exposure. The existing literature shows that conditions and environments at different stages of life could affect later life outcomes differently (Martorell 1997, 1999; Maccini and Yang 2009). The causal effect of early childhood exposure to negative shocks on adverse later life outcomes are well documented (see handbook chapter of Almond and Currie (2011) for details). In the context of this study, germ and bacteria in contaminated water generate great risks of exposure to infectious diseases to young children because they are more susceptible to waterborne diseases. The poor nutritional status and risks of infectious diseases during infancy or early childhood may cause nutrition deficiency which has adverse effect on various short-term and long-term outcomes, such as brain development, cognitive performance, adult health, and educational attainment, etc. (Maccini and Yang 2009; Eppig, Fincher, and Thornhill 2010; Almond and Currie 2011; Bhalotra and Venkataramani 2013). These imply that early childhood exposure to municipal water filtration, which reduces germ and bacteria, may have a greater health benefit.

results are not reported and are available upon request).

To investigate the effect of early childhood exposure to water filtration on school enrollment or child labor, I estimate the following model:

$$y_{ict} = \alpha + \Sigma_{j=0}^{15}\beta_j I(\text{1st exposed at age} = j) + \gamma_c + \lambda_t + \mathbf{x}_{ict}\tau + (\rho_c \cdot t) + \varepsilon_{ict} \qquad (1.3)$$

where $I(\text{1st exposed at age} = j)$ is an indicator variable equals to one if a child is first exposed to water filtration at age j or zero otherwise. Other variables are defined in equation (1.1). The control group in equation (1.3) is children who were never exposed to water filtration. β_j shows the effect of water filtration when a child was first exposed at age j.

In order to have a more complete picture of the effect of age at first exposure, I estimate equation (1.3) separately by each age. Figures 1.5 and 1.6 show the estimated effect of the age at first exposure to water filtration on school enrollment and child labor, respectively. Each graph represents estimation for each specific age of children, each point graphed indicates estimated $\widehat{\beta}_a$ with the 95% confidence interval from equation (1.3), with children who are never exposed to water filtration as the control group. The results in Figure 1.5 suggest that for children who are currently at ages 10 to 13, early childhood exposure to water filtration has no effect on school enrollment and child labor; all estimates for ages 10 to 13 are small and close to zero. However, for children who are currently at ages 14 to 15, although the pattern of estimated effect of age at first exposure is rather noisy and most of the estimated coefficients are not statistically significant, the bottom two graphs show a rough pattern that children who are first exposed to water filtration at ages 0 to 3 have larger positive effect on school enrollment than those who first exposed at age 4 and older, but the pattern in Figure 1.6 is rather unclear.

Guided by Figures 1.5 and 1.6, which suggest that children aged 14–15 and first exposed at ages 0-3 experience larger impacts than children first exposed later, I estimate the following model to get the average effect of early and later exposure to filtration, which is allowed to differ for age 14 and 15 years old (as suggested by Figure 1.5):

$$
\begin{aligned}
y_{ict} = \alpha &+ \beta_1 \text{1st exposed at ages 0-3} + \beta_2(\text{1st exposed at ages 0-3} \cdot \text{Ages 14-15}) \\
&+ \beta_3 \text{1st exposed at ages} \geq 4 + \beta_4(\text{1st exposed at ages} \geq 4 \cdot \text{Ages 14-15}) \\
&+ \gamma_c + \lambda_t + \mathbf{x}_{ict}\tau + (\rho_c \cdot t) + \varepsilon_{ict} \qquad (1.4)
\end{aligned}
$$

where variable 1st exposed at ages 0-3 (≥ 4) is an indicator variable equals to one if children are first exposed to water filtration at ages 0 to 3 (≥ 4) or zero otherwise. Variable Ages 14-15 is a dummy equal to one if a child is currently at age 14 or 15. The control group in equation (1.4) is children who are never exposed to water filtration. $\beta_1 + \beta_2$ indicates the total effect of early childhood exposure (first exposed at ages 0-3) to water filtration for children who are aged 14 to 15. $\beta_3 + \beta_4$ represents the total effect of first exposure to water filtration at older than age 4 for children who are aged 14 to 15.

Table 1.6 shows the regression results from equation 1.4. The total effects to children who are currently at ages 14 to 15 and first exposed to water filtration at different age ranges are shown in the second and third rows from the bottom, with the joint test p-value shown immediately below. The last row shows the differential effect for first exposed at ages 0-3 and first exposed at ages ≥ 4 on children aged 14-15 ($(\beta_1 + \beta_2) - (\beta_3 + \beta_4)$). The results clearly show that early childhood exposure to water filtration has largest impact on school enrollment and child labor for children who are aged 14 to 15. The estimate in the third row from the bottom suggests that, compared to children who are never exposed to the intervention, children's school enrollment (employment) increases (decreases) by around 15 (7.5) percentage points if first exposed to water filtration at ages 3 or younger (sample mean of school enrollment and employment for children aged 14-15 is 0.69 and 0.27, respectively). The effect dropped by more than 40% (60%) to 8.3 (2.8) percentage points if children are first exposed at ages 4 or older. The estimates in the last row indicate that, for children aged 14-15, the effect of age at first exposure to water filtration at ages 0-3 and ≥ 4 is statistically different. Finally, similar to the observed patterns of early childhood effect in Figures 1.5 and 1.6, early childhood exposure to water filtration has small and not statistically significant effect to children who are aged 10 to 13. The results here are consistent with the fact that 14 and 15 years old are ages in which children are sensitive to the intervention since they are finishing elementary school and can legally drop out of school in these ages (see discussion in Section 1.5.1).

The findings are similar when I used an alternate definition of early childhood exposure, ages 0–5, which is commonly used in the early childhood literature (Almond and Currie 2011). I re-estimate equation (1.4) by replacing variable "1st exposed at ages 0-3" with "1st exposed at ages 0-5" and "1st exposed at ages $\geq$ 4" to "1st exposed at ages $\geq$ 6". I find similar results in Table 1.13. The results again suggest that, for children aged 14 to 15, early childhood exposure (defined as first exposed at ages 0-5) to water filtration has larger effect on school enrollment and child labor. Furthermore, as indicated in the last row, the effect of early childhood exposure is statistically different from the effect of later life exposure. The point estimates in the last three rows are substantial similar to that of Table 1.6.

1.5.5 Compulsory Schooling Laws

Clean water can have differential effects on children who are bound or not bound by the compulsory schooling laws. There are likely families whose decisions about child enrollment are constrained by compulsory schooling laws, such that some parents may always send their children to school when they are below the legal school leaving age. So averaged across all families there would be less responsiveness to the health intervention which affects the health status of their children. Therefore, access to clean water may have larger effect on children who are not bound by the compulsory schooling laws and can legally drop out of school. I collect state compulsory schooling laws information from various years of *Report of the Commissioner of Education* and data appendix from Goldin and Katz (2011), then merge the state's laws to the 30 states in my sample.[24]

I construct a dummy variable—*Can drop out*—which indicates whether a child can drop out of school or not:

$$\textit{Can drop out} = 1 \text{ if}$$
$$\text{Child's age} \geq \text{Min}[(\textit{Minimum compulsory school entrance age} + \textit{yrs of schooling for exemption}), (\textit{Maximum age of compulsory schooling})]$$

where *Minimum compulsory school entrance age* denotes state's compulsory school entrance age, *Maximum age of compulsory schooling* denotes state's maximum age of compulsory education, *yrs of schooling for exemption* denotes the years of education for exemption from maximum age rule.

Note that I have data for *Minimum compulsory school entrance age* and *Maximum age of compulsory schooling* in all of my studied time periods (years 1880, 1900, 1910, 1920), but I only have data for *yrs of schooling for exemption* in years 1910 and 1920. In constructing the *Can drop out* variable, I assumed that all of the states in my sample did not have the laws of *yrs of schooling for exemption* in years 1880 and 1900. In other words, I assigned the value of zero to this variable to all states in years 1880 and 1900.[25] This *Can drop out* dummy variable indicates whether a child can drop out of school legally.

During the years 1880 to 1920, most of the states allowed children to drop out of school

[24]Goldin and Katz (2011)'s data appendix provides different state compulsory school laws and child labor laws from 1910 to 1939. Few variables also available from 1900 to 1909.

[25]This assumption sounds reasonable since the majority of states did not have the law of *yrs of schooling for exemption* in 1910, which somehow implies there would be even fewer states have this law in 1880 and 1900. The estimated results show similar intuitions if I use an alternative definition of *Can drop out* = 1 if Child's age $\geq$ *Maximum age of compulsory schooling*.

at 14. Table 1.14 shows the distribution of variable *Can drop out age* for the whole sample and in each census year.

Table 1.7 shows the estimation results of adding can drop out of school as a control variable and its interaction with water filtration. Columns (1) and (3) show estimation of equation (1.1) by adding *Can drop out* as a control variable. The coefficient of water filtration in column (1) equals to 0.020 which is very similar to the baseline results shown in Table 1.5 (the coefficient equals to 0.019 in Table 1.5). After adding the interaction term between water filtration and *Can drop out* variable in columns (2) and (4), the results show that the positive (negative) effect of water filtration on children's school enrollment (employment) is driven by children who can legally drop out of school. In order words, after the access to clean water, children who are legally allowed to drop out are more likely to continue their education and less likely to work. However, water filtration has no effects on children who are still bound by the compulsory schooling laws and cannot legally drop out of school. The finding is consistent with the municipal water filtration policy inducing children who are "compliers" to compulsory schooling laws (i.e., attend if required by law, not attend if not required by law) to attend more school.

1.5.6 Household Socioeconomic Status

Existing water studies suggest that water improvement programs have a larger health impact on the poor (Cutler and Miller 2005; Galiani, Gertler, and Schargrodsky 2005; Gamper-Rabindran, Khan, and Timmins 2010). A natural question is to understand whether the effect of water filtration is heterogeneous across rich and poor households. In fact, the benefits of clean water to lower socioeconomic status households are unclear. One may think, on average, that the poor are less healthier than the rich, so they may receive greater health benefits from water quality improvement. On the other hand, the poor households could be less likely to have connections to municipal water supplies, then municipal water filtration may not benefit the poor households. Another consideration is that child labor is used more by poor families, so the improved health from filtration may pull a child into employment instead of schools. I use household head illiteracy as a measure of household socioeconomic status.

Table 1.8 shows estimated results of the baseline model when the treatment variable is interacted with the household head illiterate dummy. The total effect of water filtration to lower socioeconomic status children with joint test p-value is shown in the last row of the table. The estimated total effect in Table 1.8 show that water filtration increases (decreases) children's school enrollment (employment) of children from lower socioeconomic status household by 5.4 (2.5) percentage points at the 1% (10%) significant level. This is consistent with improvements in public health infrastructure reducing inequality in school enrollment between higher and lower socioeconomic status students. It is not possible to pinpoint why the response in school enrollment is greater for the poor. For example, it could be due to health improving more for the poor (because the rich have other mechanisms to protect health, such as better medical care, health practices, or nutrition), the poor are at a point in the health distribution where a marginal increase in health delivers greater returns from schooling, or the rich have relatively inelastic demand for schooling since they always attend schools.

1.5.7 Racial Differences

I also investigate whether the effect of water filtration is different between white and black children in the North and South.[26] The results in Table 1.15 show suggestive evidence that water filtration has a larger effect on school enrollment to black children in the South.[27]

1.5.8 Gender Differences

Recent literature shows that, compared to males, females have stronger responses to health improvement programs (Miguel and Kremer 2004; Maccini and Yang 2009; Pitt, Rosenzweig, and Hassan 2012; Bhalotra and Venkataramani 2013). In this section, I investigate the heterogeneous effects of water filtration across gender and try to explore the mechanisms behind the gender differences.

One relevant hypothesis is the difference in comparative advantage between male and female suggested by Pitt, Rosenzweig, and Hassan (2012). They construct a theoretical model which illustrates the idea that the improvement of nutritional status for everyone will increase schooling more for females relative to males and increase the gender division of labor, given that females have comparative advantage in skill intensive occupations such as education. The idea, suggested by medical literature, is that males will gain more physical strength than girls when there is an improvement of their nutritional status (Round et al. 1999). So a health intervention will increase males comparative advantage in working on physically intensive occupations, which raises their relative returns to working (given that

[26] I classify the North and South by whether the city is located in the confederate states or not. The confederate states in my sample include: Alabama, Georgia, Louisiana, North Carolina, South Carolina, Tennessee, Texas, and Virginia.

[27] It can be noted that the estimates in this table might not be precise since white and black children could have different time trends even within a city. However, given the data limitation that there are only a few black children in some cities (e.g., some cities only have 8 to 10 black children in my sample), it is not possible to control for the differential trends between white and black children in the estimation.

child labor for boys primarily requires physical stamina). This type of reasoning suggests that a health intervention increases female comparative advantage in skill intensive occupations (occupations with higher skill returns) such as education.

I estimate the effect of water filtration separately by male and female. The results are in Table 1.9. In Panel A, columns (1) and (2) show estimated effect of water filtration on school enrollment and child labor for male sample, while columns (3) and (4) show that for female sample. Both males and females are more likely to go to school (columns (1) and (3)). However, for child labor (columns (2) and (4)), the effect for females is negative and significant while it is positive and insignificant for males. These results are consistent with a story like the aforementioned one. Water filtration improves health, which in turn lowers the costs of going to school given the health-education complementarities, which has the effect of raising enrollment. However, it raises the enrollment of females more than males because an improvement in health has an offsetting effect on males of raising child labor: males have a comparative advantage in physically strenuous occupations, an improvement in health raises the relative returns to working for males. Note that the coefficients on school enrollment between male and female in columns (1) and (3) are not statistically different from each other, while the coefficients on children's employment between male and female in columns (2) and (4) are statistically different from each other at the 1% level.

There are other hypotheses which could also explain the gender differences in Table 1.9. The first possibility is that the differential results reflect catch-up if female children had initially a lower enrollment rate or higher employment rate than males. However, I can rule out this explanation because, as reported in Table 1.9, both males and females have the exact same school enrollment rate, and females have a lower employment rate compared to males.

Another possibility is the survival selection of males. After the municipal water filtration, children who are relatively weak and fragile will most likely be the marginal survivors. Since literature suggests that males are biologically more vulnerable than females (Kraemer 2000), one would expect a greater survival margin among males. This can possibly explain my findings of relatively weaker response from males. More importantly, this also implies there would be an even greater selection among male children from families of lower socioeconomic status. In other words, among males, one might expect that poor male children would have a weaker response to the health intervention compared to their nonpoor counterparts. However, the results in columns (5) and (6) in Panel B of Table 1.9 show that, compared to their counterparts from nonpoor families, males from lower socioeconomic status families have stronger responses from the clean water intervention.[28] This suggests that selection effects are not large.

1.5.9 Robustness Checks

In this section, I explore the sensitivity of the results to using alternative samples of cities and an alternative way of measuring exposure to the water filtration policy.

First, one might be concerned that cities which never adopted water filtration during my studied time periods are cities with backward and disadvantaged situations compared to the treated cities, so perhaps they are not a plausible comparison group for constructing the counterfactual change over time for the cities that do adopt filtration. To address this concern, I re-estimate equation (1.2) by restricting the sample to cities which ever adopted water filtration plants in Table 1.10. The analysis here contains 56 cities that have adopted water filtration. The sample size is reduced by around one-fourth, but the estimated effects

[28] In Panel B, the total effect of water filtration to lower socioeconomic status children shown in the last row of the table. The results here provide similar intuitions as in Panel A.

of water filtration remain the same.

Second, as mentioned in Section 2.4, in addition to the 74 cities which I have complete information of both water filtration data and Census data for 1880 to 1920, I also have partial information of two additional groups of cities: 21 cities which I have water filtration data from 1880–1920 but their city's identity is not available in the 1880 census, and 59 cities where identity is available in the 1880 census but I only have partial information of water filtration until 1914 (lacking filtration information from 1915 to 1920). For the first group of cities, I include them in my analysis to form an unbalanced city-year panel (these 21 cities lack observations from 1880). For the second group of cities, I assume that they never adopted water filtration (i.e., assign the years 1915-1920 with zero for the filter variable) and add them to my analysis. The results are in Table 1.11. Each column represents a separate regression with dependent variables and its mean reported at the top of the table. Columns (1) and (5) repeat estimation of the baseline models in Table 1.5. Columns (2) and (6) show unbalanced city-year panel estimations including 21 cities. Columns (3) and (7) show estimations by assuming the 59 cities with partial filtration information never adopted the intervention. Finally, Columns (4) and (8) show estimations including all cities. The results show that regardless of the number of cities being used in the sample, the estimated effect of water filtration on children's school enrollment and employment is largely unchanged. This provides evidence that my finding of water filtration is not sensitive to the city selection criteria.

Third, so far I have examined the effect of water filtration by using a treatment dummy specified in equation 1.2. An alternative way to estimate the effect of clean water is to calculate the number of years children are exposed to water filtration. This alternative treatment measure captures the accumulation effect of health improvement, which means that the longer the years of exposure to clean water, the larger the positive effect on health. Table

1.12 shows regression results by changing the treatment dummy variable to years of exposure to water filtration in equation (1.1) (with 3.8 mean years of exposure in the sample). The results show that, regardless of model specification, children who have longer exposure to water filtration have a larger positive effect on school enrollment and negative effect to child labor. The estimate from the main specification (model specification with city-linear trends) shows that one more year of exposure to clean water increases school enrollment by 0.8 percentage points and decreases child labor by 0.4 percentage points, these effects are statistically significant at 5% level. This finding is consistent with the results in the baseline model discussed in Section 1.5.2.

1.6 Discussion and Conclusion

This study quantifies the effect of access to clean water on school enrollment and child labor. Many American cities adopted water filtration during the late nineteenth and early twentieth centuries. From various historical sources, I am able to obtain complete filtration plant information for 74 cities from 1880–1920. By using a difference-in-differences method which exploits variation across time and across cities, I find that municipal water filtration increases school enrollment by 2 percentage points (compared to a sample mean of 86%) and has no detectable effect on child labor in the baseline analysis. These effects are more pronounced for children aged 14–15, who are in their last year of elementary school and can legally drop out of school. I explore the hetergoeneity in policy effects along several additional dimensions. I find a larger positive effects on enrollment for children who are exposed at younger ages, children who can legally drop out of school, children from lower socioeconomic status families, and female children. I also provide suggestive evidence that black children who lived in the South benefits more from water filtration.

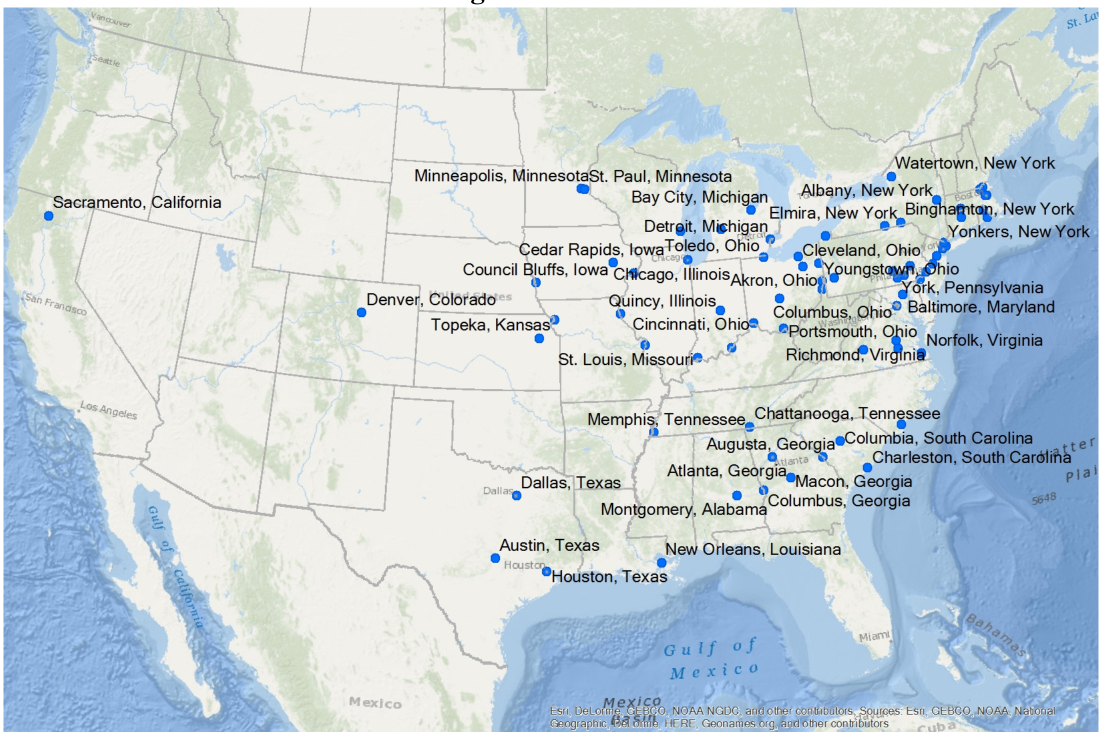
Note that since I define filtration adoption as the year of filtration plants which are operational, which does not necessarily mean that the entire city would be exposed to clean water right away. Given that more households would enjoy the benefits from filtration after the first operation of water filters, my estimation results of water filtration could be a lower bound of the true effect.

How does my estimated effect of clean water intervention compare to other studies on various types of health improvement programs? Miguel and Kremer (2004) find that a deworming program in Kenya increases school participation by 7.5 percentage points, and the effect is more pronounced for girls. Bleakley (2007) looks at a hookworm eradication program in American South happened in the early twentieth century and find that the program increases school enrollment by 3–5 percentage points. Kosec (2014) finds that private sector participation in the piped water sector in African countries is associated with a 7.8 percentage points increase in school attendance, and the effect is age specific. My estimated treatment effect in the baseline model suggests a gain of around 2 percentage points in school enrollment, which is smaller than the other health programs in the literature. Note that most of these studies examine programs in different countries which have different social and demographic contexts compared to the U.S. An article which is germane to my study is Bleakley (2007). The author looks at the American South in a similar time period and finds a little larger point estimate than my main treatment effect (3–5 vs. 2 percentage points), which could be due to the fact that the hookworm eradication program is implemented in rural areas but my municipal water filtration intervention focuses only in urban areas.

My findings suggest that some basic infrastructure investments such as municipal water filtration which provides safe water not only improves health, but also affect human capital investment (i.e., school enrollment and child labor). This provides policy makers with a better understanding that water improvement programs have benefits beyond lowering mortality and morbidity. The findings in this study are particularly important to developing countries where poor health environment may impede the development of human capital in schools.

1.7 Figures

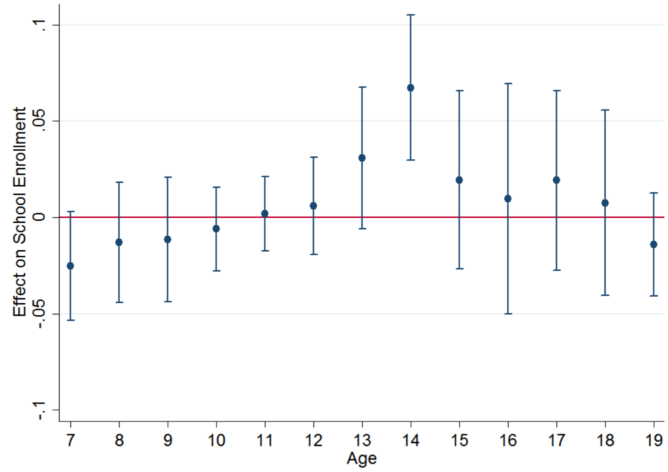
Figure 1.1: Cities



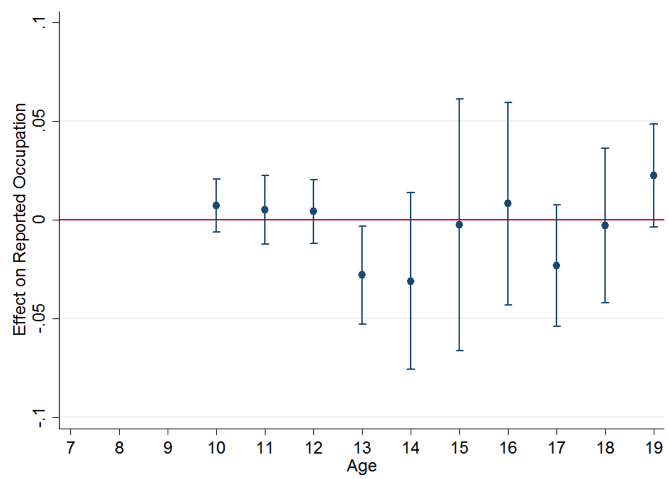
Notes: Displayed are the 74 cities (located in 30 states) in my main sample. As described in Section 2.4, these are the cities for which both complete data on filter installation and IPUMS microdata are available for census years 1880–1920.

Figure 1.2: The Effect of Municipal Filter Installation by Age

Panel A: School Enrollment



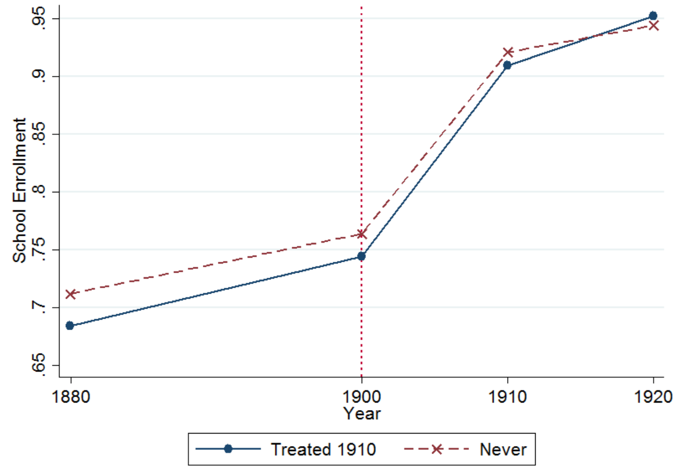
Panel B: Employment (Reported an Occupation)



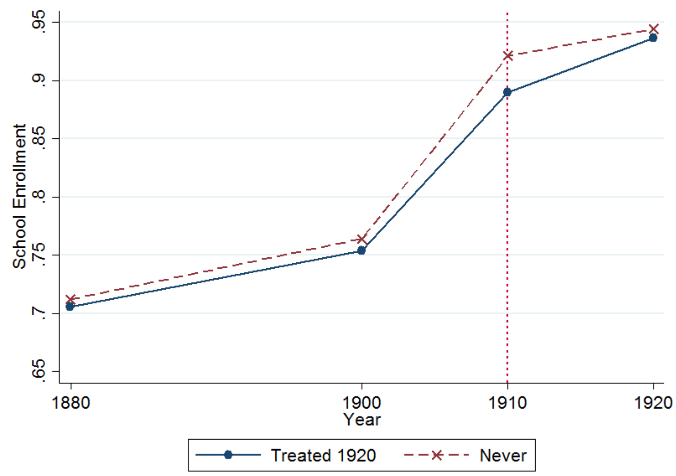
Notes: Each point shows estimated $\widehat{\beta}_a$ with 95% confidence interval from equation (1.1). Robust standard errors clustered at city level were used. The controls in this equation are: age fixed effects, year fixed effects, city fixed effects, household head illiteracy, black, male, interaction between age dummies and all aforementioned control variables, and city-specific linear time trend. In panel B, no estimated effect is shown for ages 7 to 9 because the occupation question is asked only for children age 10 and above. Sample consists of children between ages 7 to 19 in the IPUMS census 1880, 1900, 1910, 1920. Regressions are weighted by sample weight.

Figure 1.3: Assessing Parallel Trend: School Enrollment

Panel A: Cities that Adopted Water Filtration During 1901–1910 vs. Cities that Never Adopted



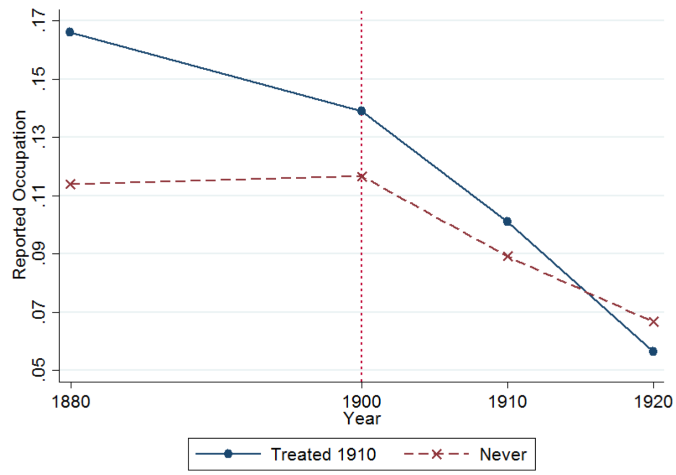
Panel B: Cities that Adopted Water Filtration During 1911–1920 vs. Cities that Never Adopted



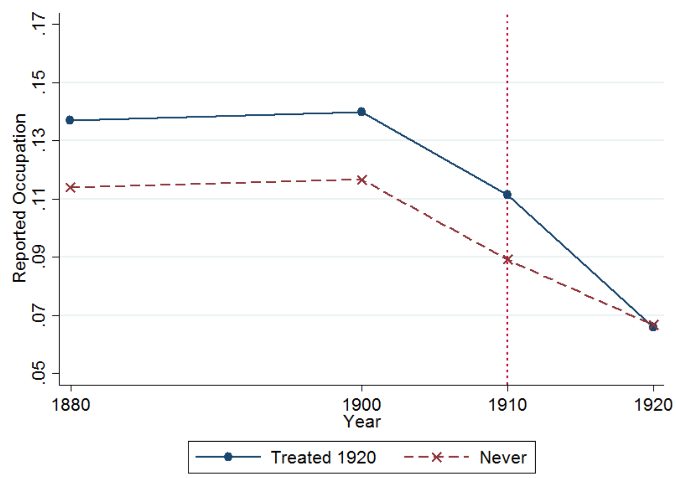
Notes: Solid line represents the mean enrollment in each year for cities which adopted Filtration Plant in mentioned time period. Dashed line represents the mean enrollment for cities which never installed a filtration plant during the entire time periods 1880-1920. The dotted vertical line located in specified year indicates the first census before the intervention. So to the left are the pre-intervention periods and to the right are the post-intervention periods. Sample consists of children between ages 10 to 15 in the IPUMS census 1880, 1900, 1910, 1920. The census of 1890 is not available because the records were destroyed by fire. Statistics are weighted by sample weight.

Figure 1.4: Assessing Parallel Trend: Employment

Panel A: Cities that Adopted Water Filtration During 1901–1910 vs. Cities that Never Adopted

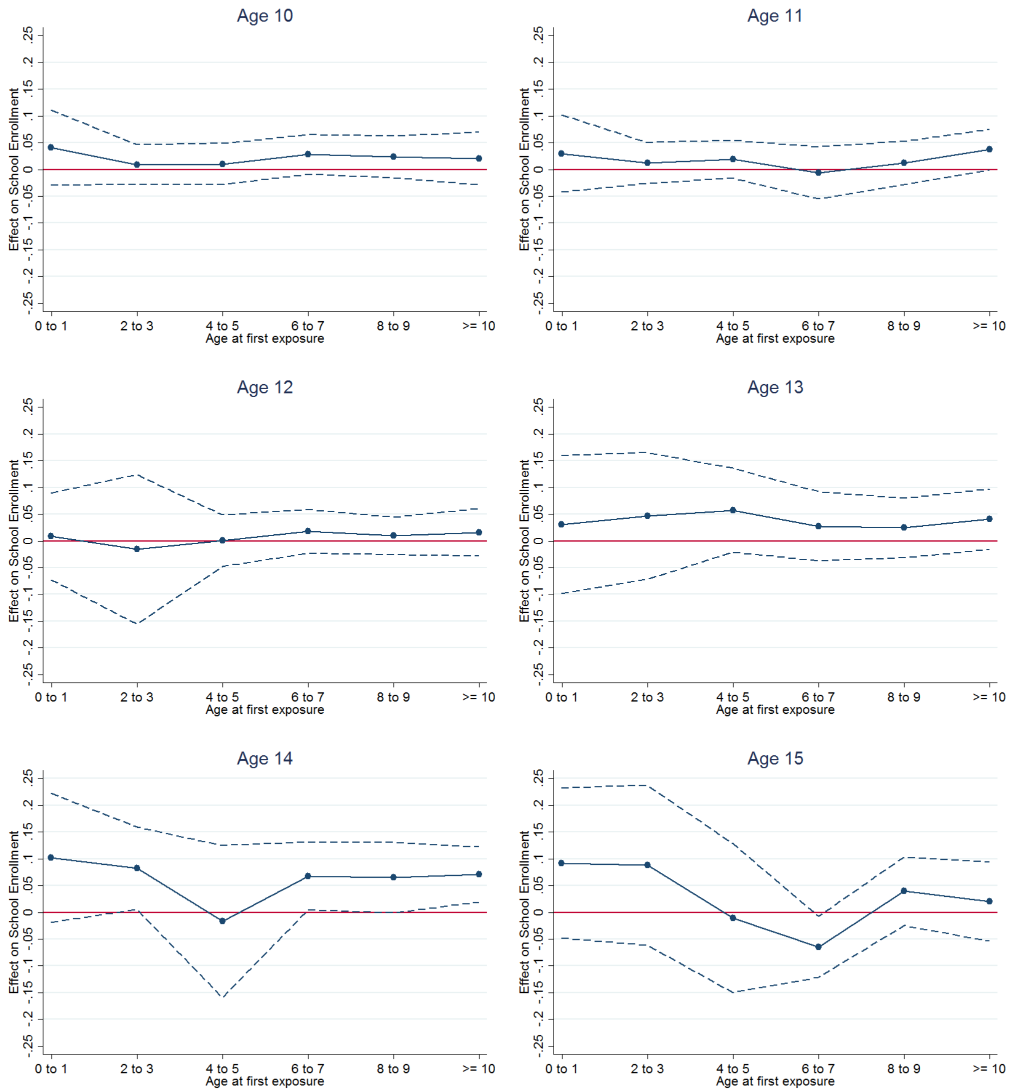


Panel B: Cities that Adopted Water Filtration During 1911-1920 vs. Cities that Never Adopted



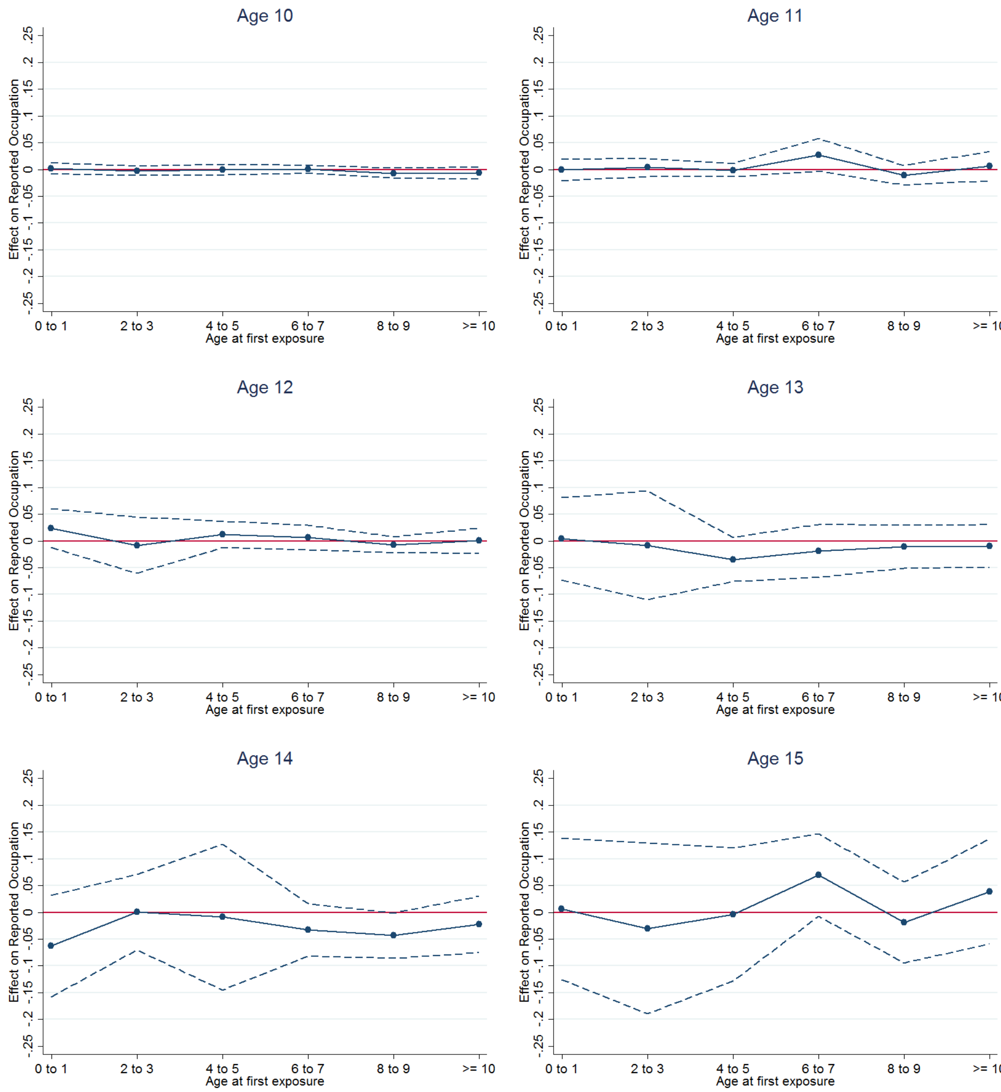
Notes: Solid line represents the mean reported occupation (child labor) in each year for cities which adopted Filtration Plant in mentioned time period. Dashed line represents the mean reported occupation (child labor) for cities which never installed a filtration plant during the entire time periods 1880-1920. The dotted vertical line located in specified year indicates the first census before the intervention. So to the left are the pre-intervention periods and to the right are the post-intervention periods. Sample consists of children between ages 10 to 15 in the IPUMS census 1880, 1900, 1910, 1920. The census of 1890 is not available because the records were destroyed by fire. Statistics are weighted by sample weight.

Figure 1.5: The Effect of Municipal Filter Installation on School Enrollment by Age and Age at First Exposure



Notes: Each point graphed shows the coefficient for filter for the given age at exposure (i.e., estimated $\widehat{\beta}_a$ with 95% confidence interval from equation (1.3)). Robust standard errors clustered at city level were used. The controls in this equation are: age fixed effects, year fixed effects, city fixed effects, household head illiteracy, black, male, and city-specific linear time trend. Control group: children never exposed to water filtration. Regressions are weighted by sample weight.

Figure 1.6: The Effect of Municipal Filter Installation on Employment by Age and Age at First Exposure



Notes: Each point graphed shows the coefficient for filter for the given age at exposure (i.e., estimated $\widehat{\beta}_a$ with 95% confidence interval from equation (1.3)). Robust standard errors clustered at city level were used. The controls in this equation are: age fixed effects, year fixed effects, city fixed effects, household head illiteracy, black, male, and city-specific linear time trend. Control group: children never exposed to water filtration. Regressions are weighted by sample weight.

1.8 Tables

Table 1.1: Average Number of Bacteria Per Cubic Centimeter in Lawrence, MA

Year	Water Before Filtration	Water After Filtration
	All Bacteria	
1894	10,800	110
1895	11,100	80
1896	7,600	85
1897	10,900	84
	B. coli communis	
1898	4,400	66
1899	5,800	42
1900	8,970	49
1901	3,017	26

Source: Journal of the American Medical Association (1903b).
Notes: Lawrence, Massachusetts installed its first water filter in 1893. In 1898, the test of water quality changed to measure only B. coli communis concentration.

Table 1.2: Municipal Filtration Plants Installation, 1880-1920

	Number of Cities	Percentage
Never	18	24.3
Installed Filter in 1881-1900	11	14.9
Installed Filter in 1901-1910	29	39.2
Installed Filter in 1911-1920	16	21.6
Total	74	100.0

Table 1.3: Typical School Structure in 1911

			Elementary								Secondary				Higher			
			First four grades				Second four grades											
Grade & year			1	2	3	4	5	6	7	8	1	2	3	4	1	2	3	4
Age	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22

Source: *Report of the Commissioner of Education, 1912*. U.S. Bureau of Education

Table 1.4: Summary Statistics For Main Analysis Sample, Children Aged 10-15

	N	Mean	Std.Dev.	Min.	Max.
School enrollment	200210	0.859	0.348	0.00	1.00
Employment (Reported an occupation)	200210	0.100	0.300	0.00	1.00
Filter	200210	0.433	0.496	0.00	1.00
Age	200210	12.417	1.703	10.00	15.00
Black	200210	0.066	0.247	0.00	1.00
Male	200210	0.494	0.500	0.00	1.00
Head of household illiterate	200210	0.069	0.254	0.00	1.00

Notes: Sample consists of children between ages 10 to 15 in the IPUMS census 1880, 1900, 1910, 1920 in the 74 cities with complete filtration information. Statistics are weighted by sample weight.

Table 1.5: The Effect of Water Filtration on School Enrollment and Employment

	Dependent variable					
	School enrollment			Employment		
	0.86			0.10		
	(1)	(2)	(3)	(4)	(5)	(6)
Filter	0.017*	0.019**	0.018*	-0.019**	-0.007	-0.001
	(0.010)	(0.010)	(0.011)	(0.007)	(0.008)	(0.007)
Head illiterate	-0.065***	-0.066***	-0.066***	0.053***	0.055***	0.054***
	(0.012)	(0.012)	(0.012)	(0.009)	(0.009)	(0.009)
Black	-0.037***	-0.037***	-0.037***	0.020***	0.021***	0.021***
	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)	(0.007)
Male	-0.006**	-0.005**	-0.005**	0.046***	0.046***	0.046***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
R-squared	0.216	0.219	0.222	0.200	0.203	0.205
N	200210	200210	200210	200210	200210	200210
Model specification:						
City-linear trends	No	Yes	Yes	No	Yes	Yes
State-year FE	No	No	Yes	No	No	Yes

Notes: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Sample consists of children between ages 10 to 15 in the IPUMS census 1880, 1900, 1910, 1920 in the 74 cities with complete filtration information. Regressions are weighted by sample weight. Robust standard errors clustered at the city (74 cities) level are in parentheses. Dependent variable school enrollment (employment) is an indicator variable equals to one if a child is enrolled (reported an occupation) and zero otherwise. Filter is an indicator variable equals to one if city installed the water filtration plant. Additional control variables not reported in the table are: age fixed effects, year fixed effect, city fixed effects.

Table 1.6: The Effect of Early Childhood Exposure to Water Filtration on School Enrollment and Employment

	Dependent variable	
	School enrollment (1)	Employment (2)
1st exposed ages 0-3, β_1	-0.019	0.019
	(0.027)	(0.018)
1st exposed ages 0-3 * Ages 14-15, β_2	0.166***	-0.094***
	(0.025)	(0.019)
1st exposed ages ≥ 4, β_3	-0.005	-0.001
	(0.016)	(0.009)
1st exposed ages ≥ 4 * Ages 14-15, β_4	0.088***	-0.027
	(0.024)	(0.018)
R-squared	0.227	0.206
N	200210	200210
City-linear trends	Yes	Yes
Effect on Ages 14-15 + Exposed 0-3	0.147***	-0.075***
Joint test [p-value][a]	[0.000]	[0.002]
Effect on Ages 14-15 + Exposed ≥ 4	0.083***	-0.028*
Joint test [p-value][b]	[0.000]	[0.093]
Difference in effect for Exposed 0-3 and ≥ 4 on Ages 14-15	0.064**	-0.047*
Joint test [p-value][c]	[0.015]	[0.060]

Notes: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Sample consists of children between ages 10 to 15 in the IPUMS census 1880, 1900, 1910, 1920 in the 74 cities with complete filtration information. Regressions are weighted by sample weight. Robust standard errors clustered at the city (74 cities) level are in parentheses. Dependent variable school enrollment (employment) is an indicator variable equals to one if a child is enrolled (reported an occupation) and zero otherwise. The sample mean of school enrollment and employment for children aged 14-15 is 0.69 and 0.27, respectively. 1st exposed at ages 0-3 ($\geq$ 4) is an indicator variable equals to one if children are first exposed to water filtration at ages 0 to 3 ($\geq$ 4). Ages 14-15 is a dummy equal to one if a child is currently at age 14 or 15. Reference group is children who never exposed to water filtration. Additional control variables not reported in the table are: age fixed effects, year fixed effect, city fixed effects, head illiterate, black, male.

[a]Joint test: $\beta_1 + \beta_2 = 0$.

[b]Joint test: $\beta_3 + \beta_4 = 0$.

[c]Joint test: $(\beta_1 + \beta_2) - (\beta_3 + \beta_4) = 0$.

Table 1.7: The Effect of Water Filtration by Legal School Dropout Status

	Dependent variable			
	School enrollment		Employment	
	(1)	(2)	(3)	(4)
Filter	0.020**	-0.000	-0.007	0.006
	(0.009)	(0.010)	(0.008)	(0.009)
Filter * Can drop out		0.089***		-0.056***
		(0.025)		(0.020)
Can drop out	-0.015	-0.039	0.000	0.016
	(0.024)	(0.028)	(0.017)	(0.019)
R-squared	0.219	0.221	0.203	0.204
N	200210	200210	200210	200210
City-linear trends	Yes	Yes	Yes	Yes

Notes: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Sample consists of children between ages 10 to 15 in the IPUMS census 1880, 1900, 1910, 1920 in the 74 cities with complete filtration information. Regressions are weighted by sample weight. Robust standard errors clustered at the city (74 cities) level are in parentheses. Dependent variable school enrollment (employment) is an indicator variable equals to one if a child is enrolled (reported an occupation) and zero otherwise. Filter is an indicator variable equals to one if city installed the water filtration plant. *Can drop out* is an indicator variable equals to one if a child can legally drop out of school. Additional control variables not reported in the table are: age fixed effects, year fixed effect, city fixed effects, head illiterate, black, male.

Table 1.8: The Effect of Water Filtration by Household Socioeconomic Status

	Dependent variable	
	School enrollment (1)	Employment (2)
Filter	0.017*	-0.006
	(0.010)	(0.008)
Filter * Head illiterate	0.037***	-0.019*
	(0.014)	(0.010)
Head illiterate	-0.083***	0.064***
	(0.008)	(0.005)
R-squared	0.219	0.203
N	200210	200210
City-linear trends	Yes	Yes
Filter + lower SES	0.054***	-0.025*
Joint test [p-value][a]	[0.000]	[0.085]

Notes: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Sample consists of children between ages 10 to 15 in the IPUMS census 1880, 1900, 1910, 1920 in the 74 cities with complete filtration information. Regressions are weighted by sample weight. Robust standard errors clustered at the city (74 cities) level are in parentheses. Dependent variable school enrollment (employment) is an indicator variable equals to one if a child is enrolled (reported an occupation) and zero otherwise. Filter is an indicator variable equals to one if city installed the water filtration plant. Additional control variables not reported in the table are: age fixed effects, year fixed effect, city fixed effects, black, male.
[a]Joint test: Filter + Interaction = 0.

Table 1.9: The Effect of Water Filtration by Gender

	Panel A: By gender			
	Male		Female	
	School Enrollment	Employment	School Enrollment	Employment
	0.86	0.12	0.86	0.08
	(1)	(2)	(3)	(4)
Filter	0.016	0.006	0.023**	-0.018*
	(0.011)	(0.010)	(0.010)	(0.009)
R-squared	0.230	0.241	0.211	0.163
N	98268	98268	101942	101942
City-linear trends	Yes	Yes	Yes	Yes

	Panel B: By Gender and household socioeconomic status			
	Male		Female	
	School Enrollment	Employment	School Enrollment	Employment
	0.86	0.12	0.86	0.08
	(5)	(6)	(7)	(8)
Filter	0.013	0.007	0.021**	-0.017*
	(0.011)	(0.010)	(0.010)	(0.009)
Filter * Head illiterate	0.050***	-0.017	0.023	-0.018*
	(0.014)	(0.015)	(0.017)	(0.009)
Head illiterate	-0.079***	0.059***	-0.086***	0.067***
	(0.009)	(0.008)	(0.008)	(0.005)
R-squared	0.230	0.241	0.211	0.163
N	98268	98268	101942	101942
City-linear trends	Yes	Yes	Yes	Yes
Filter + lower SES	0.063***	-0.010	0.044***	-0.035***
Joint test [p-value][a]	[0.000]	[0.615]	[0.007]	[0.005]

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. Sample consists of children between ages 10 to 15 in the IPUMS census 1880, 1900, 1910, 1920 in the 74 cities with complete filtration information. Regressions are weighted by sample weight. Robust standard errors clustered at the city (74 cities) level are in parentheses. Dependent variable school enrollment (employment) is an indicator variable equals to one if a child is enrolled (reported an occupation) and zero otherwise. Filter is an indicator variable equals to one if city installed the water filtration plant. Additional control variables not reported in the table are: age fixed effects, year fixed effect, city fixed effects, black, male.
[a]Joint test: Filter + Interaction = 0.

Table 1.10: The Effect of Water Filtration on School Enrollment and Employment (Restricting Sample to Cities that Ever Adopted Water Filtration during 1880–1920)

	Dependent variable	
	School enrollment (1)	Employment (2)
Filter	0.020**	-0.007
	(0.010)	(0.011)
R-squared	0.224	0.205
N	150609	150609
City-linear trends	Yes	Yes

Notes: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Regressions in this table only include sample of cities which ever adopted the water filtration plant. Sample consists of children between ages 10 to 15 in the IPUMS census 1880, 1900, 1910, 1920. Regressions are weighted by sample weight. Robust standard errors clustered at the city (56 cities) level are in parentheses. Dependent variable school enrollment (employment) is an indicator variable equals to one if a child is enrolled (reported an occupation) and zero otherwise. Filter is an indicator variable equals to one if city installed the water filtration plant. Additional control variables not reported in the table are: age fixed effects, year fixed effect, city fixed effects, head illiterate, black, male.

Table 1.11: The Effect of Water Filtration on School Enrollment and Employment, Using Cities in Main Sample and Cities with Incomplete Information

	Dependent variable							
	School enrollment				Employment			
	0.86	0.86	0.86	0.86	0.10	0.10	0.10	0.10
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Filter	0.019**	0.020**	0.027***	0.027***	-0.007	-0.007	-0.011	-0.011
	(0.010)	(0.009)	(0.010)	(0.009)	(0.008)	(0.008)	(0.007)	(0.007)
R-squared	0.219	0.216	0.217	0.214	0.203	0.200	0.201	0.199
N	200210	205524	250877	256191	200210	205524	250877	256191
No. of city/cluster	74[a]	95[b]	133[c]	154[d]	74[a]	95[b]	133[c]	154[d]
City-linear trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Sample consists of children between ages 10 to 15 in the IPUMS census 1880, 1900, 1910, 1920. Regressions are weighted by sample weight. Robust standard errors clustered at the city level are in parentheses. Dependent variable school enrollment (employment) is an indicator variable equals to one if a child is enrolled (reported an occupation) and zero otherwise. Filter is an indicator variable equals to one if city installed the water filtration plant. Additional control variables not reported in the table are: age fixed effects, year fixed effect, city fixed effects, head illiterate, black, male.

[a]74 cities in main sample.

[b]95 = 74 cities in main sample + 21 cities which city's identity is not available in IPUMS 1880 census (Unbalanced city-year panel).

[c]133 = 74 cities in main sample + 59 cities which either do not adopt water filtration until 1914 or have filtration but water filtered by private corporations.

[d]154 = 74 + 21 + 59 (Unbalanced city-year panel).

Table 1.12: Results Using Years of Exposure to Water Filtration As Treatment Measure

	Dependent variable					
	School enrollment			Employment		
		0.86			0.10	
	(1)	(2)	(3)	(4)	(5)	(6)
Yrs of exposure	0.004***	0.008**	0.014**	-0.004***	-0.004**	-0.008*
	(0.001)	(0.003)	(0.006)	(0.001)	(0.002)	(0.004)
R-squared	0.216	0.220	0.223	0.201	0.203	0.205
N	200210	200210	200210	200210	200210	200210
Specification:						
City-linear trends	No	Yes	Yes	No	Yes	Yes
State-year FE	No	No	Yes	No	No	Yes

Notes: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Each column represents separate regression. Sample consists of children between ages 10 to 15 in the IPUMS census 1880, 1900, 1910, 1920 in the 74 cities with complete filtration information. Regressions are weighted by sample weight. Robust standard errors clustered at the city (74 cities) level are in parentheses. Dependent variable school enrollment (employment) is an indicator variable equals to one if a child is enrolled (reported an occupation) and zero otherwise. Yrs of exposure measures the number of years exposed to the water filtration, with range = [0, 15] and mean = 3.79. Additional control variables not reported in the table are: age fixed effects, year fixed effect, city fixed effects, head illiterate, black, male.

1.9 Appendix

Table 1.13: The Effect of Early Childhood Exposure to Water Filtration on School Enrollment and Employment (Define Early Childhood Exposure As Ages 0-5)

	Dependent variable	
	School enrollment (1)	Employment (2)
1st exposed ages 0-5, β_1	-0.017	0.003
	(0.022)	(0.016)
1st exposed ages 0-5 * Ages 14-15, β_2	0.161***	-0.093***
	(0.026)	(0.019)
1st exposed ages ≥ 6, β_3	0.003	-0.007
	(0.012)	(0.007)
1st exposed ages ≥ 6 * Ages 14-15, β_4	0.082***	-0.024
	(0.023)	(0.018)
R-squared	0.227	0.206
N	200210	200210
City-linear trends	Yes	Yes
Effect on Ages 14-15 + Exposed 0-5	0.144***	-0.090***
Joint test [p-value][a]	[0.000]	[0.000]
Effect on Ages 14-15 + Exposed ≥ 6	0.085***	-0.031*
Joint test [p-value][b]	[0.000]	[0.076]
Difference in effect for Exposed 0-5 and ≥ 6 on Ages 14-15	0.059**	-0.058**
Joint test [p-value][c]	[0.021]	[0.023]

Notes: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Sample consists of children between ages 10 to 15 in the IPUMS census 1880, 1900, 1910, 1920 in the 74 cities with complete filtration information. Regressions are weighted by sample weight. Robust standard errors clustered at the city (74 cities) level are in parentheses. Dependent variable school enrollment (employment) is an indicator variable equals to one if a child is enrolled (reported an occupation) and zero otherwise. The sample mean of school enrollment and employment for children aged 14-15 is 0.69 and 0.27, respectively. 1st exposed at ages 0-5 (≥ 6) is an indicator variable equals to one if children are first exposed to water filtration at ages 0 to 5 (≥ 6). Ages 14-15 is a dummy equal to one if a child is currently at age 14 or 15. Reference group is children who never exposed to water filtration. Additional control variables not reported in the table are: age fixed effects, year fixed effect, city fixed effects, head illiterate, black, male.

[a] Joint test: $\beta_1 + \beta_2 = 0$.

[b] Joint test: $\beta_3 + \beta_4 = 0$.

[c] Joint test: $(\beta_1 + \beta_2) - (\beta_3 + \beta_4) = 0$.

Table 1.14: State Legal School Leaving Age by Census Year

Age	Can drop out age = Min[(Minimum compulsory school entrance age + yrs of schooling for exemption), (Maximum age of compulsory schooling)]									
			By census year							
	All sample		1880		1900		1910		1920	
	No. of state	Percent	No. of state	Percent	No. of state	Percent	No. of state	Percent	No. of state	Percent
0	37	30.8	20	66.7	12	40.0	5	16.7		
12	6	5.0			1	3.3	2	6.7	3	10.0
13	1	0.8					1	3.3		
14	41	34.2	9	30.0	13	43.3	11	36.7	8	26.7
15	11	9.2	1	3.3	1	3.3	3	10.0	6	20.0
16	24	20.0			3	10.0	8	26.7	13	43.3
Total	120	100.0	30	100.0	30	100.0	30	100.0	30	100.0

Notes: Unit of observation = state. Information of state compulsory schooling laws comes from various issues from *Report of the Commissioner of Education* and data appendix of Goldin and Katz (2011).

Table 1.15: The Effects of Water Filtration by Race in the North and South

	Dependent variable	
	School enrollment (1)	Employment (2)
Filter, β_1	0.018*	-0.008
	(0.010)	(0.009)
Filter * Black, β_2	0.003	0.005
	(0.012)	(0.010)
Filter * South, β_3	-0.008	0.023
	(0.046)	(0.020)
Filter * Black * South, β_4	0.071***	-0.035*
	(0.023)	(0.021)
Black	-0.014	-0.001
	(0.013)	(0.009)
Black * South	-0.096***	0.067***
	(0.021)	(0.016)
R-squared	0.220	0.203
N	200210	200210
City-linear trends	Yes	Yes
Effect on blacks in the South	0.084*	-0.015
Joint test [p-value][a]	[0.090]	[0.546]
Effect on blacks in the North	0.021	-0.003
Joint test [p-value][b]	[0.124]	[0.810]
Difference in effect for blacks and whites in South	0.074***	-0.030
Joint test [p-value][c]	[0.000]	[0.112]

Notes: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Sample consists of children between ages 10 to 15 in the IPUMS census 1880, 1900, 1910, 1920 in the 74 cities with complete filtration information. Regressions are weighted by sample weight. Robust standard errors clustered at the city (74 cities) level are in parentheses. Dependent variable school enrollment (employment) is an indicator variable equals to one if a child is enrolled (reported an occupation) and zero otherwise. Filter is an indicator variable equals to one if city installed the water filtration plant. Additional control variables not reported in the table are: age fixed effects, year fixed effect, city fixed effects, head illiterate, male. Southern cities in the samples are cities located in confederate states: Alabama, Georgia, Louisiana, North Carolina, South Carolina, Tennessee, Texas, and Virginia.

[a]Joint test: $\beta_1 + \beta_2 + \beta_3 + \beta_4 = 0$.

[b]Joint test: $\beta_1 + \beta_2 = 0$.

[c]Joint test: $[(\beta_1 + \beta_2 + \beta_3 + \beta_4) - (\beta_1 + \beta_3)] = \beta_2 + \beta_4 = 0$.

Table 1.16: City Installation of Water Filtration Plants

Year of installation	City, State
1892	Chattanooga, TN
1893	Lawrence, MA
1896	Cedar Rapids, IA
1897	Elmira, NY
1898	Augusta, GA; Macon, GA; Saint Joseph, MO
1899	Albany, NY; Norfolk, VA; Rock Island, IL; York, PA
1901	Petersburg, VA
1902	Binghamton, NY; Philadelphia, PA; Providence, RI; Watertown, NY
1903	Charleston, SC; Columbus, GA; Denver, CO; Indianapolis, IN; New York, NY; Reading, PA; Washington, DC; Yonkers, NY
1905	Columbia, SC; Harrisburg, PA; Youngstown, OH
1906	Lancaster, PA
1907	Portsmouth, VA
1908	Cincinnati, OH; Columbus, OH; New Orleans, LA; Pittsburgh, PA; Wilmington, DE; Wilmington, NC
1909	Louisville, KY
1910	Atlanta, GA; Newport, RI; Springfield, MA; Toledo, OH
1911	Montgomery, AL
1912	Evansville, IN; Grand Rapids, MI
1913	Minneapolis, MN
1914	Baltimore, MD; Dallas, TX; Erie, PA; Portsmouth, OH; Quincy, IL; Trenton, NJ
1915	Akron, OH; Lowell, MA; Saint Louis, MO; Steubenville, OH
1916	New Brunswick, NJ
1917	Cleveland, OH
1923	Detroit, MI
1924	Sacramento, CA; Wheeling, WV
No filter until 1920	Austin, TX; Bay City, MI; Boston, MA; Cambridge, MA; Chicago, IL; Council Bluffs, IA; Hartford, CT; Houston, TX; Jersey City, NJ; Lynchburg, VA; Memphis, TN; Milwaukee, WI; Richmond, VA; Saint Paul, MN; Topeka, KS

Notes: There are the 74 cities (located in 30 states) in my main sample. As described in Section 2.4, these are the cities for which both complete data on filter installation and IPUMS microdata are available for census years 1880–1920.

Sources: *General Statistics of Cities: 1915* by the United States Bureau of the Census; "Filtration Plant Census, 1924" published in *Journal of the American Water Works Association*, which is compiled by C.G. Gillespie who was director of the Sanitary Engineering Bureau of the California State Board of Health; a set of three papers titled "Design and Operation Data on Large Rapid Sand Filtration Plants in the United States and Canada" published in *Journal of the American Water Works Association* (Hardin 1932, 1942; Cosens 1956). Supplementary sources included: various issues from "Water Survey Series"; "Water Supply Paper"; "The Purification of Public Water Supplies" (Johnson 1913); "Present Day Water Filtration Practice" (Johnson 1914); "Census of Municipal Water Purification Plants in the United States, 1930-1931" (Wolman 1933); "Manual of Design for Slow Sand Filtration" (Hendricks 1991).

Chapter 2

The Impact of Privatization of Water Services on Educational Attainment: The Case of Argentina

2.1 Introduction

Access to safe drinking water is still a very important issue today. According to a report from the World Health Organization (WHO and UNICEF 2015), as of 2015, there were still 663 million people who did not have access to safe drinking water.[1] While the way of increasing access to safe water is debatable, one possible approach is to allow the private sector to provide municipal water infrastructure.

[1] Safe water is defined by water source which is adequately protected from outside contamination, particulary faecal matter.

This study investigates the effect of early childhood exposure to a municipal water services privatization program on educational attainment such as primary, compulsory, or secondary school completion in Argentina. During the 1990s, Argentina experienced one of the world's largest privatization campaigns which comprise municipal privatization of water services in 1991 to 1999. Different municipalities privatized their water services at different times. By 1999, municipalities which cover about 60 percent of country's population transferred their water services from public to private ownership. An influential study by Galiani, Gertler, and Schargrodsky (2005) suggests that this water services privatization program improves both access to water and water quality,[2] which reduces child mortality and the impact is most pronounced in poorest areas. Furthermore, Galiani, Gonzalez-Rozada, and Schargrodsky (2009) find that the privatization program also improves child health; the program induces a large reduction of diarrheal cases among children. The reduction in child mortality and illness documented in these two studies clearly suggest that the privatization program improves health environment in municipalities.

Establishing causal inference between clean water and later life outcomes is difficult because of the possibility of other unobserved factors which affect both human capital accumulation and access to clean water. The municipal water services privatization program provides a great opportunity to investigate the long term effect of access to clean water. To examine the causal relation between access to clean water and school completion, I exploit two sources of variation from this intervention. In particular, the municipality-cohort variation in water services privatization permits me to use a difference-in-differences strategy to identify the causal impact of access to clean water. Intuitively, this effect is estimated by taking the early/late childhood exposure difference in educational attainment in a privatized

[2]The author adopted a difference-in-differences strategy to show that the number of households connected to water networks experienced a large increase (4.2 percentage points) after privatization. The authors also provide a number of case studies to demonstrate that privatized firms significantly improved water service performance, are more efficient, and invest more on physical infrastructure.

municipality, and using cohorts never exposed to the privatization to control for the changes in educational outcomes over time that would have occurred for reasons unrelated to the privatization program.

The impacts of private sector participation in public sector are widely discussed in both empirical and theoretical studies (Shapiro and Willig 1990; Megginson, Nash, and van Randenborgh 1994; Barberis et al. 1996; Schmidt 1996; La Porta and Lopez-de-Silanes 1999; Galiani, Gertler, and Schargrodsky 2005; Galiani, Gonzalez-Rozada, and Schargrodsky 2009; Kosec 2014). The most relevant studies are Galiani, Gertler, and Schargrodsky (2005) and Galiani, Gonzalez-Rozada, and Schargrodsky (2009), which look at the same privatization program in Argentina and find that this program reduced child mortality and improve children health, which imply an improvement in health environment. Given these health benefits from the water services privatization program, and the established complementarities between childhood health and education (Glewwe, Jacoby, and King 2001; Miguel and Kremer 2004; Bobonis, Miguel, and Puri-Sharma 2006; Bleakley 2007, 2010), it is important to understand how private sector participation in public infrastructure affects human capital accumulation among survivors. This paper contributes to the literature by providing the first evidence of the effect of private sector participation in water infrastructure on educational outcomes such as primary, compulsory, and secondary school completion.

Existing literature shows that access to clean water reduces mortality and provides a healthier environment to survivors (Troesken 2002; Jalan and Ravallion 2003; Cutler and Miller 2005; Ferrie and Troesken 2008; Mangyo 2008; Galiani, Gertler, and Schargrodsky 2005; Galiani, Gonzalez-Rozada, and Schargrodsky 2009; Gamper-Rabindran, Khan, and Timmins 2010; Ahuja, Kremer, and Zwane 2010; Kremer et al. 2011; Zhang 2012; Alsan and Goldin 2015). Surprisingly, there have been only a few studies that look at the possible effects of water on educational outcomes (Devoto et al. 2012; Bhalotra and Venkataramani

2013; Beach et al. 2014; Kosec 2014; Xu and Zhang 2014). This study adds to this understudied topic by examining the impact of a municipal water services privatization program. More specifically, given the knowledge of child mortality reduction and health improvement after the private sector participation in water infrastructure (Galiani, Gertler, and Schargrodsky 2005; Galiani, Gonzalez-Rozada, and Schargrodsky 2009; Kosec 2014), the results on human capital accumulation in this study extend our knowledge about various benefits of private sector participation in water sector. This is particularly important to the developing world where under-provision of safe water may impede human capital development. Furthermore, given the well documented causal effect of childhood health environment on later life outcomes in the literature (see handbook chapter of Almond and Currie (2011) for discussion), it is reasonable to believe that early childhood exposure to contaminated water will have long-term consequences—because germ and bacteria in contaminated water create great risks of exposure to infectious diseases in young children who are more susceptible to waterborne diseases. The present study complements this literature by examining the effect of early childhood exposure to clean water.

A priori, the effect of water services privatization on educational attainment is an open question. For example, the access to clean water allows those previously weak and sick children to have the stamina go to school. At the same time, the privatization program may also improve children's ability to focus and concentrate in class, which will allow them to perform better in school. All of these reduce the costs of education and increase the return to schooling, so individuals may be more like to invest in human capital. On the other hand, healthier children can be more productive and, therefore, be more valuable in the labor market. This increases their relative return in working, in order words, increases their opportunity costs of going to school, which have a negative effect on schooling. These two opposing stories imply that the effect of municipal water services privatization program on school attainment is unclear. Furthermore, the effect of privatization can also be affected by the mortality selection effect. This means that after the access to clean water, children who are relatively weak and fragile will most likely be the marginal survivors, so they will lower the estimated average effect of clean water on educational attainment.

There is considerable time variation in municipal adoption of water services privatization. I merged this municipality-year privatization data with the individual level microdata of Argentina 2010 Census to estimate the impacts of early childhood exposure to water services privatization on educational attainment. Applying a difference-in-differences strategy which exploits variation across municipalities and across cohorts, the results suggest that privatization has a zero effect on primary and compulsory school completion, but a small negative effect on secondary school completion. I also find that the effect of water services privatization is heterogeneous for individuals who lived in nonpoor and poor municipalities. The results show that, for primary and compulsory school completion, early childhood exposure to privatization has a zero effect in nonpoor municipalities and a negative effect in poor municipalities. Moreover, the negative effect goes to zero when the first age of exposure becomes older. This is consistent with the early childhood literature that claims conditions and environments in early life has a larger effect on later life outcomes. To further understand the negative effect in poor municipalities, I implement a supplemental analysis to investigate at which grade of primary education individuals are dropping out of school. The results suggest that privatization induces individuals to drop out of school at 5th, 6th, and 7th grade among poor municipalities, which map into the final years of primary school.

The rest of this study is organized as follows. I review the literature in section 2.2. Section 2.3 discusses institutional background of privatization program in Argentina. In section 2.4, I present data of educational attainment and municipal water services privatization. Section 2.5 assesses the parallel trend assumption. Section 2.6 discusses the identification strategy and presents empirical results. Finally, Section 2.7 concludes this study.

2.2 Literature Review

This paper provides some of the first empirical evidence of how private sector participation in municipal water services affects educational outcomes. Related to this study, in the context of Argentina, Galiani, Gertler, and Schargrodsky (2005) investigate the causal effect of the privatization of water services on child mortality. Their results show that privatized municipalities experienced a significant increase in water network connections and improvement in water quality. These led to an 8 percent decrease in child mortality and the effect is largest (26 percent) in the poorest areas. Galiani, Gonzalez-Rozada, and Schargrodsky (2009) use the same privatization program and show that the expansion of water networks improved child health and induced savings in household water expenditure in urban shantytowns.

There is limited research on the effect of clean water on education. Devoto et al. (2012) use a randomized design in Morocco to examine the non-health effect of household water connection. Their results show that household water connection had no effect on children's school participation, but had a positive effect on household well-being such as increased in leisure time.[3] Bhalotra and Venkataramani (2013) examine the effect of a clean water program in Mexico in 1991 and find that infant exposure to clean water program has positive effect on girls' test scores, their test scores increased by 0.1 standard deviation, and there is no effect on boys. Kosec (2014) investigates the effect of private sector participation in the piped water sector, which increases access to piped water, in 39 African countries during 1986 to 2010. The author shows that privatization in piped water sector reduces

[3] In the paper, since all sample households have access to clean water from public tab, the randomized design of in-home water connection will not improves water quality.

diarrhea of children under age five in urban areas, and is associated with a 7.8 percentage points increase in school attendance. Furthermore, the positive association between access to piped water and school attendance is only statistically significant for children who are at transition points in their education, which is aged 11–13 and 17. Regarding the long-term effect of clean water, Xu and Zhang (2014) look at a large drinking water treatment program in rural China and find a positive effect on schooling attainments.[4] Similarly, Beach et al. (2014) link males in 1900 and 1940 in the United States and use typhoid fatality rate as a proxy of water quality. Their results show that the effect of eradicating early-life exposure to typhoid fever is positive on years of schooling and later life earnings.

My paper contributes to this limited literature on the effect of clean water on educational outcomes, which features this important channel in the larger literature of the benefits of clean water programs on mortality reduction (Troesken 2002; Jalan and Ravallion 2003; Cutler and Miller 2005; Galiani, Gertler, and Schargrodsky 2005; Ferrie and Troesken 2008; Gamper-Rabindran, Khan, and Timmins 2010; Alsan and Goldin 2015) and improvement in child health (Mangyo 2008; Galiani, Gonzalez-Rozada, and Schargrodsky 2009; Ahuja, Kremer, and Zwane 2010; Kremer et al. 2011; Zhang 2012).

Given the reduction in mortality and morbidity found by the existing literature, the present study provides policy makers a better understanding about the various benefits from the clean water program, especially the knowledge that clean water also influences educational outcomes of survivors, which is crucial to economic development. Furthermore, the benefits of water improvement programs will be understated if we neglect the causal effects of water-related programs on human capital.

[4] Education attainments are grades of education completed, binary variables of graduated from middle school and graduated from high school.

This study is also relevant to a large literature on the effect of early childhood environment. Since the causal effect of childhood health environment on later life outcomes is well documented in the literature (see handbook chapter of Almond and Currie (2011)), it is important to understand the effect of early childhood exposure to clean water through privatization of water infrastructure.

2.3 Background

2.3.1 Privatization Campaigns in Argentina

Argentina launched one of the world largest privatization campaigns in the 1990s. The privatization of municipal water services is part of this enormous privatization campaigns which transferred almost all of the country's state owned enterprizes (SOEs) to private ownership.[5] The goal of these massive privatization campaigns is to deal with the long prevailed economic depression in the past decades. Argentina experienced a hyperinflation and huge government deficits in the late 1980s. In order to save the economy, the newly elected government in 1989 decided to implement a reform program which consisted of trade and capital market liberalization, monetary reform to stabilize currency exchange rate, tax system restructuring, transforming public-run pension system to privately managed fund, and privatization of public enterprizes (Blake 1998; Galiani, Gertler, and Schargrodsky 2005; Galiani, Gonzalez-Rozada, and Schargrodsky 2009).

There are several objectives in this massive privatization. First, it is to alleviate the huge budget deficit in the late 1980s in which a nontrivial amount are created by sizable SOE

[5]These SOEs include infrastructure services such as water and sanitation, electricity, oil and gas, telecommunications, transportation, and mail services.

losses. Another intention is to induce private sector investment in infrastructural development. After two decades of stagnant economic growth in 1970s and 1980s, there was lack of investment in public utilities. To resolve this inadequate infrastructure investment, privatizing public utilities provides a positive force to improve infrastructural facilities (Chisari, Estache, and Romero 1999; Ennis and Pinto 2005).

2.3.2 Water Services Privatization in Municipalities

Before the privatization campaigns in the 1990s, municipal water services in Argentina were operated by the public sector or a number of non-profit cooperatives, most companies usually provided services on both water and sewage, but a few of them only provided water (Artana, Navajas, and Urbiztondo 2000; Galiani, Gertler, and Schargrodsky 2005). The water services privatization program started in 1991 and continued until 1999, in which municipalities that cover around 60 percent of the country's population switched its ownership from public to private control. There is considerable time variation in municipal privatization of water services during the 1990s in Argentina. Figure 2.1 plots the percentage of privatized municipalities over the water services privatization periods (1991 to 1999). The details of how the municipal privatization sample is formed is described in the data section. Within the 275 municipalities in my data, the progress of privatization is slow before 1995. There were only 8 percent of municipalities that privatized their water services in 1994. The progress sped up since 1995 and by 1999, more than 35 percent of municipalities switched their water services from private to public ownership in my sample.

There are several reasons for the variation in timing of water services privatization adoption in Argentina. Political factor is one of them. Although the privatization started in early 1990s, the privatization of water services was slow at the beginning of the decade and sped up after the newly elected government was appointed in 1995 (as shown in Figure 2.1). In addition, time-invariant factors such as municipal socioeconomic conditions can also affect the privatization decision. For example, larger and poorer municipalities with less developed public infrastructure are more likely to adopt the privatization program sooner (Galiani, Gertler, and Schargrodsky 2005).

One might be concerned that municipalities could be more likely to privatize their water services if they respond to bad economic shocks or other time-varying factors. A key threat to the difference-in-differences strategy is that the decision of privatization is driven by unobserved time-varying shocks which affect both privatization decisions and individual educational outcomes. To test the effect of time-varying shocks on privatization decisions, Galiani, Gertler, and Schargrodsky (2005) implemented a discreet-time hazard model analysis and showed that privatization decision is not correlated with observed time-varying shocks such as GDP growth, change in unemployment rate, and change in income inequality. So the authors claim that privatization decision is unlikely to be correlated with unobserved time-varying shocks, given that privatization is not associated with observed time-varying factors. The discreet-time hazard model analysis in Galiani, Gertler, and Schargrodsky (2005) provides evidence which validates the subsequence difference-in-differences analysis in the present study.

The water services privatization program has been shown to improve both water access and water quality (Alcazar, Abdala, and Shirley 2002; Artana, Navajas, and Urbiztondo 2000), which induce an improvement in health environment. For example, Galiani, Gertler, and Schargrodsky (2005) find a decline in child mortality, and Galiani, Gonzalez-Rozada, and Schargrodsky (2009) find that children are less likely to suffer from diarrhea.

2.4 Data

To investigate the effect of privatization on human capital accumulation, I require data on educational attainment and date of municipal water services privatization. I obtain the former from the 2010 Census microdata, and the latter from Galiani, Gertler, and Schargrodsky (2005) (GGS).

Individual-level data on educational attainment are taken from the *Integrated Public Use Micro Sample* (IPUMS). This study uses a single cross-sectional 2010 Census of Argentina which covers 10 percent of total population. Human capital accumulation is measured by primary school completion, compulsory school completion, and secondary school completion. It is worth mentioning that school system in Argentina experienced two reforms in last two decades. Argentinian had seven years of primary school and five years of secondary school (7-5) before 1993, in which seven years of primary school is compulsory education. Followed by an education reform in 1993 which changed the system to nine years of compulsory education and three years of higher secondary school (9-3). The nine years of compulsory education includes both primary school and lower secondary school. In 2006, the government implemented another reform which allowed education jurisdictions to choose discretionarily between a structure of 7-5 (seven years of primary and 5 years of secondary) or a 6-6 (six years of primary and secondary).[6]

The IPUMS 2010 Census provides information of individual year of schooling, which is generated by individual school attainment questions from the questionnaire. According to the instruction of the IPUMS Census, it treated more than 7 years of primary education as 7 years and coded secondary education begins at 8 years. Therefore, this years of schooling

[6] As mentioned in the following, because the youngest individual in my sample is 17 years old in 2010, the 2006 reform is not going to affect my results since my sample is old enough and cannot be affected by this latest reform.

variable is not capturing the actual years of completed schooling. In my empirical analysis, I regard individuals who have completed primary school if they have 7 years of schooling. I can also construct compulsory school completion dummy since I am able to identify whether individuals belong to 7-5 or 9-3 system in the data. Finally, secondary school completion is dummy equals one if individuals have 12 years of schooling.

The Census also contains information about working status, dwelling characteristics, and other demographic variables. For geographic information, the data cannot identify respondents' place of birth. Furthermore, for household living location, it suppresses municipalities with less than 20,000 residents in one geographic category in each province due to requirement of confidentiality. There are totally 299 municipalities in the IPUMS data, 276 of them represent individual municipalities with more than or equal to 20,000 residents and 23 of them are suppressed municipalities in each province which contain municipalities with less than 20,000 residents.[7]

The data of municipal year of water services privatization are from GGS,[8] which contains 476 municipalities from 1990 to 1999.[9] Privatization is measured by a dummy variable equals one if a private water company supplies services to the largest fraction of the population in the municipality.

I merged these individual level microdata from the Census to the timing of municipal privatization of water services. Notice that the IPUMS 2010 Census of Argentina provides fewer municipality's identity than the data in GGS. To figure out the year of privatization for each municipality, I merged the municipalities in the 2010 Census with the privatization

[7] There are 23 provinces in Argentina. Each province contains suppressed municipality which represents municipalities with less than 20,000 residents.

[8] I thank Sebastian Galiani for sharing me the data and Stata code used in their paper.

[9] Note that the empirical analysis in Galiani, Gertler, and Schargrodsky (2005) does not contains all municipalities in Argentina due to no water service or missing information.

information from GGS's data.[10] There are eventually 275 municipalities in my analysis.[11] My empirical analysis focus on native born individuals who are between ages 17 to 35. The age 17 cutoff was chosen because students start their first grade (first year of schooling) at the age of 6 and will complete their secondary school at age 17 if they do not repeat in any grade.

Table 2.1 shows the summary statistics for my sample. Panel A shows the summary statistics of the overall sample. Panels B and C show statistics of subsamples for nonpoor and poor municipalities, respectively. In Panel A, on average, 92 percent of the sample completed primary school, 91 percent completed compulsory school, and around 53 percent of the sample are secondary school graduate. Around 58% of individuals were ever exposed to the water services privatization in their life. 50 percent of the samples are female. The average age is about 26 years old. About 18% of individuals were living in poor municipalities. Compared to panel A, educational attainment is higher in nonpoor municipalities in Panel B, and lower in poor municipalities in Panel C. Larger proportion of individuals were ever exposed to the privatization program in the nonpoor municipalities (63% vs. 35%). Finally, gender and age distribution are similar across all Panels.

[10] As illustrated in Figure 2.10, after merging the two datasets, section A shows that 256 municipalities in the 2010 Census are matched with year of privatization information. Among the unmatched municipalities in the 2010 Census, section B shows that 20 of them are either "no service or missing information" of water services, or they are identifiable in the 2010 Census but not exist in GGS's data. Section C shows that 23 of them are suppressed municipality in the 2010 Census. In section C, for the suppressed municipality under each province in the 2010 Census, I go back to GGS's data to figure out their year of privatization. I realized that most of these suppressed municipalities with under 20,000 residents are either never privatized or all privatized at the same year within each province. Eventually, as indicated in section D, I can keep individuals who lived in 19 suppressed municipalities since I can identify their municipal year of privatization. Overall, my empirical analysis contains 275 municipalities (sections A + D) which represent 84% of municipalities (400 out of 476 municipalities) in GGS's data. Among these 275 municipalities with identifiable year of privatization in the IPUMS, 256 of them represent 256 municipalities in GGS's data, 23 of them are suppressed municipality in each province representing 144 municipalities in GGS's data.

[11] Table 2.5 presents the year of privatization of these 275 municipalities.

2.5 Assessing the Parallel Trend Assumption

Since my empirical analysis exploits variation across municipalities and across cohorts. A possible concern to the difference-in-differences method is the differential trends on educational attainment between privatized and not-privatized municipalities in the pre-intervention period. To assess the parallel trend assumption, Figure 2.2 graphs the mean outcome by birth cohort and privatization status, and consider whether in the pre-intervention period the privatized and not-privatized municipalities do indeed have parallel trend. Panels A.1, A.2, and A.3 show the mean primary school completion, compulsory school completion, and secondary school completion, respectively. The solid line represents the mean school completion in each birth cohort for municipalities which adopted water services privatization. The dashed line represents the mean school completion for municipalities which never adopted privatization. The dotted vertical line located in 1981 indicates 10 years before the first year of the water services privatization program in 1991. In other words, in municipality privatized in 1991, a child who was born in 1981 was first exposed to the privatization at age 10, which regards as late childhood exposure to the intervention. So to the left of the dotted vertical line are the pre-intervention periods and to the right are the post-intervention periods. In general, treatment and control groups in Figure 2.2 indicate support for the parallel trend assumption in all cases of primary, compulsory, and secondary school completion; Figure 2.2 shows similar pre-trends on left side of the dotted vertical line.

Both the observed parallel pre-trends in Figure 2.2 between treatment and control groups for older cohorts, and the evidence provided by Galiani, Gertler, and Schargrodsky (2005) that the adoption of privatization is not correlated with time-varying factors provide us confidence about the validity of the parallel trend assumption. It validates my difference-in-differences strategy in empirical analysis.

2.6 Empirical Strategy and Results

The considerable time variation of water services privatization during the 1990s permits me to use a difference-in-differences method which exploits variation across municipalities and across cohorts to identify the effect of privatization program. To investigate the effect of early childhood exposure to privatization on educational attainment, I estimate the following model:

$$y_{ijc} = \alpha + \Sigma_{a=0}^{16} \beta_a I(\text{1st exposed at age} = a) + \gamma_c + \delta_j + \mathbf{x}_{ijc}\tau + \varepsilon_{ijc} \qquad (2.1)$$

where y_{ijc} is a dummy variable of primary school completion, compulsory school completion, or secondary school completion for individual i who lives in municipality j and born in year c. $I(\text{1st exposed at age} = a)$ is an indicator variable which equals one if an individual was first exposed to the privatization program at age a or zero otherwise. γ_c is a vector of year of birth dummies which control for cohort characteristics that do not vary over time. δ_j represents vector of municipality dummies which control for municipality characteristics that do not vary over time. $\mathbf{x}_{ijc}$ is a vector of demographic and region-specific variables including, gender dummy, provincial real GDP per capita and public expenditure per capita when individual was at age 6 (typical age of first year of schooling). The control groups in this regression are individuals who first exposed to the privatization at ages 17 or older, or individual who never exposed to the privatization. In this equation, β_a represents the difference-in-differences estimator of the effect of privatization when an individual was first exposed at age a. The key identifying assumption is that in the absence of the privatization program, the difference of educational attainment across cohorts would have remained the same between individuals who live in municipalities with or without the privatization program.

Figures 2.3, 2.4, and 2.5 show the estimated effect of the age of first exposure to privatization on primary school completion, compulsory school completion, and secondary school completion, respectively. Each point graphed indicates estimated $\widehat{\beta}_a$ with 95% confidence interval from equation (2.1). The control groups are individuals who never exposed to the privatization or who first exposed to the privatization at ages 17 or older. Robust standard errors in regressions are clustered at the municipality level, and they are consistent under heteroscedasticity as well as within-municipality serial correlation.

In Panel A.1 of Figures 2.3, 2.4, and 2.5, the results show that early childhood exposure to privatization has a zero effect on primary and compulsory school completion and a negative effect on secondary school completion, although the coefficients are only marginally significant. The zero effect in primary and compulsory completion and negative effect in secondary completion could be due to the reason that there may be more job opportunities for children who are older than the primary school ages. This is consistent with the story that the health intervention increases the relative return of working, which induces children to drop out from secondary school earlier (although the negative coefficients of early childhood exposure are only marginally significant), but has no effect on primary and compulsory school completion. It is worth mentioning that, especially in Figure 2.5, the effect of privatization declines to zero as the first age of exposure become older. This means that, compared to the control group, only individuals who has early childhood exposure experienced the largest effect from the intervention. There is a zero effect on educational attainment for those who experienced the intervention at late childhood. This is consistent with the early childhood literature that environments in early childhood have a larger impact on later life outcomes since young children are more sensitive to health and sanitary environments.

2.6.1 Municipal Socioeconomic Status

Galiani, Gertler, and Schargrodsky (2005) suggest that the privatization program had no impact on child mortality in nonpoor municipalities, and child mortality had the largest decrease in poorest municipalities. One interesting question is to understand whether the effect of privatization is heterogeneous across nonpoor and poor municipalities. Although empirical evidence clearly suggests that the privatization program improves municipal health environment (Galiani, Gertler, and Schargrodsky 2005; Galiani, Gonzalez-Rozada, and Schargrodsky 2009), the effect of privatization across nonpoor and poor municipalities is unclear. For example, households that lived in poor municipalities may be less likely to have household connections to water network as well as fewer health technologies (e.g., access to clinic and doctor, private water treatment) to mitigate negative health shocks before the privatization, so these households may enjoy greater health benefits from the privatization program. On the other hand, families which lived in poor municipalities may be more likely to send a healthy child to work to maintain households level of subsistence. In addition, the mortality selection effect can be more pronounced in poor municipalities, which suggests that the effect in poor municipalities can also be zero or even negative. The reason is that relatively weak and fragile children will most likely be the marginal survivors after the privatization. Depending on the cohort size of these marginal survivors, these children will attenuate my estimated effect on educational attainment or even drive the effect of clean water to negative in poor municipalities. I divide my sample into nonpoor and poor municipalities following the definition in Galiani, Gertler, and Schargrodsky (2005).[12]

[12]A municipality's socioeconomic status is classified by fraction of households that have unmet basic needs (UBN) according to 1991 census. A household has UBN if at least one of the following happened: there are more than three people per room (overcrowded housing), there are no fecal evacuation system (no toilet), household members are living in poor housing (poor housing), and there are four or more members per working member and low household head education (below subsistence). In my analysis, nonpoor municipalities are municipalities where less than 25 percent of households have UBN, poor municipalities are municipalities where at least 25 percent of households have UBN.

Each point estimate in Figures 2.3, 2.4, and 2.5 shows the difference-in-differences coefficient for individuals who lived in nonpoor municipalities (Panel A.2) and poor municipalities (Panel A.3). The primary school completion results in Figure 2.3 Panels A.2 and A.3 show that early childhood exposure to privatization has a zero effect in nonpoor municipalities and a negative effect in poor municipalities, respectively. This finding is consistent with a greater impact of privatization on poor municipalities. The compulsory school completion results in Figure 2.4 Panels A.2 and A.3 show similar patterns, except that early childhood exposure to privatization shows a slightly larger positive effect in nonpoor municipalities. In Figure 2.5, the secondary school completion results in Panel A.2 indicate a zero effect in nonpoor municipalities. Panel A.3 show that the coefficients of early childhood exposure are positive, although they are not statistically significant.

Overall, the estimated negative effect on primary and compulsory school completion and positive effect (although not statistically significant) on secondary school completion in poor municipalities are interesting. The possible explanation is that since many people in poor municipalities do not go to secondary school, the health intervention induces people to drop out of primary school earlier and go to work. However, for individuals who are likely to go on to secondary school, early childhood exposure to privatization has a small positive effect on secondary school completion, but the effect is not statistically significant at the 5% level.

2.6.2 Gender Difference

Existing literature suggests that female and male have different responses to health improvement programs (Miguel and Kremer 2004; Maccini and Yang 2009; Pitt, Rosenzweig, and Hassan 2012; Bhalotra and Venkataramani 2013). The reason is that female has competitive advantage in skill acquiring activities (such as education) while male has competitive advantage in labor intensive activities. A natural question is to understand the heterogeneous effect of privatization across gender.

I estimate the effect of privatization separately by female and male. Figure 2.6 shows the effect of privatization on primary school completion. Panel A shows the estimates for female, in which Panels A.1, A.2, and A.3 show the estimated effect for all, nonpoor, and poor municipalities, respectively. Following the same logic, Panel B shows the estimates for male in different municipalities. Figure 2.6 Panels A.2 and A.3 show that, for female, early childhood exposure to privatization has a zero effect on nonpoor municipalities and a negative effect on poor municipalities, respectively. We observed a similar pattern for male results in Panels B.2 and B.3, but most of the coefficients are not statistically significant at the 5% level. The effect on compulsory school completion in Figure 2.7 shows similar results, except that early childhood exposure to privatization show a slightly larger positive effect in nonpoor municipalities for male in Panel B.2. The findings here in Argentina is somewhat different from the existing literature which suggests that female has comparative advantage in education, and therefore, has stronger positive responses on education to health improvement programs. Finally, results in Figure 2.8 show the effect of privatization on secondary school completion. Although most of the coefficients in all panels are not statistically significant, Panel A.3 shows that individuals who are first exposed to privatization at ages 0 to 1 has positive effect on secondary school completion for female. In general, the results for female or male in Figure 2.8 is very similar to the overall results presented in Figure 2.5.

2.6.3 Effect of Years of Childhood Exposure

Guided by Figures 2.3 to 2.8, which suggest that the privatization has a larger effect on children who are first exposed at early childhood, I construct an alternative treatment measure to examine the effect of early childhood exposure to privatization. I estimate the following model to investigate the effect of years of childhood exposure to privatization:

$$y_{ijc} = \alpha + \beta \text{Yrs of childhood exposure}_{ijc} + \gamma_c + \delta_j + \mathbf{x}_{ijc}\tau + \varepsilon_{ijc} \tag{2.2}$$

where I define variable Yrs of childhood exposure as $\text{Max}((7 - \text{age of first exposure}), 0)$. The cutoff seven is chosen because Figures 2.3 to 2.8 reveal that the effect of early childhood exposure to privatization declines to zero by that age. All other variables are defined in equation (2.1). In this equation, β indicates the marginal effect of years of childhood exposure on educational attainment.

Table 2.2 shows the estimated results of equation (2.2) for primary school completion, with Panels A, B, and C present the results for female and male, female, or male sample, respectively. Each column in each panel represents a separate regression. Columns (1), (4), and (7) show estimation for all municipalities, Columns (2), (5), and (8) show estimation for nonpoor municipalities, and Columns (3), (6), and (9) show estimation for poor municipalities. Although the effect for all municipalities in Columns (1) and (4) are statistically significant, the point estimates are very small and close to zero. Consistent to the results in Figure 2.3, in poor municipalities, one more year of childhood exposure to privatization has a negative effect on primary school completion. For example, the coefficient in Column (3) shows that an additional year of childhood exposure to privatization decreases primary school completion by 0.8 percentage point. In order to understand the average effect of childhood exposure among treated people (people with any childhood exposure), I can multiple the average years of childhood exposure among treated people by the estimated coefficient. The average years of childhood exposure among people ever exposed to privatization is 3.4, 3.4, and 3.1 years for all, nonpoor, and poor municipalities, respectively. By using the coefficient in Table 2.2 Column (3) in poor municipalities, the average effect of an additional years of childhood exposure among exposed people decreases probability to complete primary school by 2.5 percentage points.[13] The estimation for compulsory school completion in Table 2.3 shows similar results as primary school completion, except for the positive but close to zero coefficient in nonpoor municipalities (Table 2.3 Columns (2), (5), and (8)). Finally, coefficients in Table 2.4 Columns (1), (4), and (7) show that an additional year of childhood exposure to privatization decreases probability of secondary school completion by around 0.9 to 1 percentage point. For the average effect of childhood exposure among treated people, the average effect of an additional years of childhood exposure among exposed people decreases probability to complete secondary school by 3.1 to 3.4 percentage points.[14] There is no statistically significantly effect on secondary school completion in nonpoor and poor municipalities. In general, the results in Tables 2.2 to 2.4 mirror the results in Figures 2.3 to 2.8.

2.6.4 Years of Primary School Completed

The results in poor municipalities in Figures 2.3 and 2.4 Panel A.3 show that early childhood exposure to privatization has a negative effect on primary and compulsory school completion. It is important to understand at what grade of primary education individuals are dropping out of school. One can imagine that, since there could be more job opportunities for older children in the labor market, individuals could be more likely to drop out of

[13] $-0.025 = -0.008 \times 3.1$ years.

[14] $-0.031 = -0.009 \times 3.4$ years. $-0.034 = -0.010 \times 3.4$ years.

school at later grades of primary education. I estimate the following equation:

$$\begin{aligned}\text{Completed } k \text{ yrs of primary school}_{ijc} = \alpha + \beta \text{Yrs of childhood exposure}_{ijc} \\ + \gamma_c + \delta_j + \mathbf{x}_{ijc}\tau + \varepsilon_{ijc}, \\ k = 1, 2, 3, ..., 7 \end{aligned} \tag{2.3}$$

where Completed k yrs of primary school is a dummy variable equals one if individual completed k years of primary school, $k = 1, 2, 3, ..., 7$ or zero otherwise. Variable Yrs of childhood exposure is defined as Max$((7 - \text{age of first exposure}), 0)$. All other variables are defined in equation (2.1). β represents the marginal effect of years of childhood exposure on probability of completed k years of primary school, $k = 1, 2, 3, ..., 7$.

The estimated results are shown in Figure 2.9. Each point represents coefficient estimated by separate regression. The horizontal axis indicates dependent variable completed k years of primary school, $k = 1, 2, 3, ..., 7$. The vertical axis indicates estimated $\widehat{\beta}$ of years of childhood exposure with 95% confidence interval from equation (2.3). Panels A.1, A.2, and A.3 show results for all, nonpoor, and poor municipalities. Results in Panel A.2 for nonpoor municipalities show that, although a few coefficients are statistically significant, the point estimates are close to zero and very small (less than 0.1 percentage point), so there is a zero effect in nonpoor municipalities. For poor municipalities in Panel A.3, the results show that the statistically significant effect appears in completed 5 years, 6 years, and 7 years of primary school. This means that an additional year of childhood exposure induces people to drop out of school in 5th, 6th, and 7th grade.[15] Finally, the results for all municipalities in Panel A.1 also show that people are more like to drop out of school in 5th, 6th, and 7th grade. This small negative effect is basically driven by the results in poor municipalities.

It is useful to think about the magnitude of the estimated coefficients and understand

[15] Note that the total effect at 7th grade composes effect of people dropping out after 6th grade, dropping out after 5th grade, and dropping out after 4th grade.

the average effect of early childhood exposure among treated people. I multiply the average years of childhood exposure among people with any exposure by the estimated coefficient.[16] In Figure 2.9 Panel A.3, the average effect of an additional year of early childhood exposure to privatization among exposed people decreases the probability to complete 5, 6, and 7 years of primary school by 1.1, 1.6, and 2.6 percentage points, respectively.[17] Taken all together, the privatization program induces individuals in poor municipalities to drop out of primary school at 5th, 6th, and 7th grade, which correspond to the final years of primary school. This finding is consistent with older children could be more likely to drop out of school because there are more job opportunities available to them.

2.7 Discussion and Conclusion

This study examines the effect of municipal water services privatization on educational attainment in Argentina. Municipalities cover about 60% of Argentina's population that privatized their water services during 1991 to 1999. Existing literature demonstrates that privatization decision is not correlated with time-varying factors such as economic shocks, and it provides clear evidence that the privatization program improves municipal health environment (Galiani, Gertler, and Schargrodsky 2005; Galiani, Gonzalez-Rozada, and Schargrodsky 2009). In this study, by using a difference-in-differences method which exploits variation across municipalities and across cohorts, the results suggest that early childhood exposure to privatization has a zero effect on primary and compulsory school completion, but a negative effect on secondary school completion. Moreover, I find that the effect of

[16] As mentioned in section 2.6.3, the average years of childhood exposure among treated people is 3.4, 3.4, and 3.1 years for all, nonpoor, and poor municipalities, respectively.

[17] $-0.011 = -0.0033 \times 3.4$ years, $-0.016 = -0.0048 \times 3.4$ years, $-0.026 = -0.0083 \times 3.1$ years.

early childhood exposure to privatization is heterogeneous across nonpoor and poor municipalities. For primary and compulsory school completion, my results suggest that early childhood exposure to privatization has no effect in nonpoor municipalities and a negative effect in poor municipalities. To get a better understanding of the negative effect in poor municipalities, I use a supplemental analysis to investigate the effect of privatization on years of primary school completed. The results indicate that the program induces individuals to drop out of primary school at 5th, 6th, and 7th grade, which map into the final years of primary school.

There are several possible explanations for the interesting negative effect on primary and compulsory school completion in poor municipalities. For example, since a lot of people in poor municipalities do not attend secondary school, after the water services privatization program, households may require a healthier child to go to work to maintain the family's subsistence. Therefore, the improvement in health status may encourage people to leave school before completing primary school, which may increase child labor. Another potential explanation is that the type of jobs and employment opportunities are different in nonpoor and poor areas. For instance, individuals who lived in poor areas are completing primary school at older ages and, therefore, have better job opportunities in the labor market, so they are more likely to drop out of primary school. Child labor is a pretty serious issue in Argentina. I found a brief report (Understanding Children's Work)[18] saying that, in 2004, there are around 11% of children aged 5-14 were working.[19] However, the report only provides me one aggregate statistic for the northern part of the country. I do not have the child labor data to test the hypothesis. Moveover, since Galiani, Gertler, and Schargrodsky (2005) suggest that poor municipalities are where child mortality reduced the most (also the areas with the larger mortality selection effect), the survival of these relatively weak and

[18] http://www.ucw-project.org/Pages/Tables.aspx?id=1255

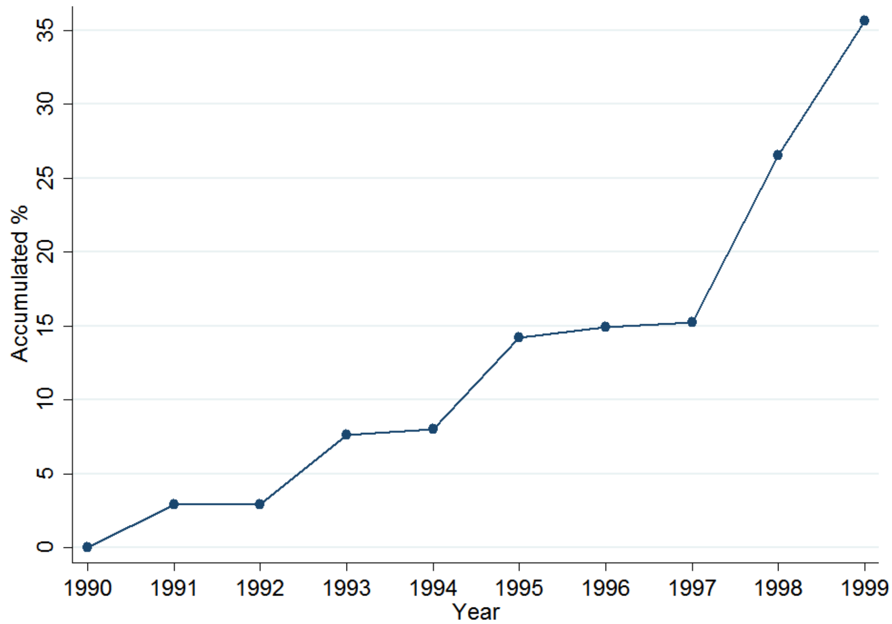
[19] This number is comparable to developing countries like Philippines.

fragile children will attenuate the effect of privatization toward zero or even negative.

My findings suggest that privatization of water services in Argentina not only affects health, but also affects other outcomes such as education. This provides policy makers with a better understanding that water improvement programs affect other outcomes beyond mortality and morbidity. The findings in this study are particularly important in understanding the effect of clean water programs since I find that water services privatization may reduce primary school completion in poor areas.

2.8 Figures

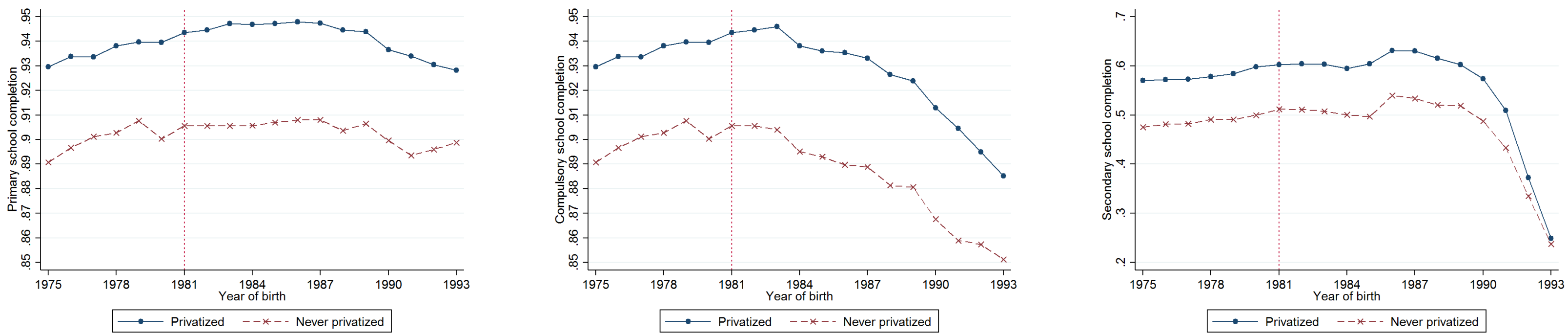
Figure 2.1: Percentage of Municipalities with Privatized water systems



Notes: There are 275 municipalities in my sample. The municipal water services privatization program happened between 1991 to 1999.

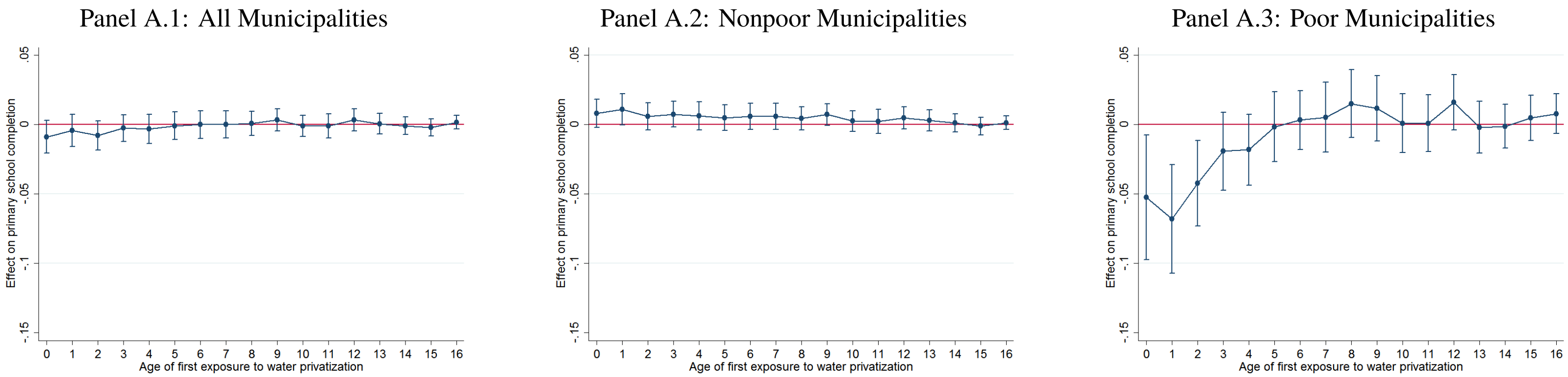
Figure 2.2: Assessing Parallel Trend

Panel A.1: Primary School Completion (Privatized vs. Never Privatized) | Panel A.2: Compulsory School Completion (Privatized vs. Never Privatized) | Panel A.3: Secondary School Completion (Privatized vs. Never Privatized)



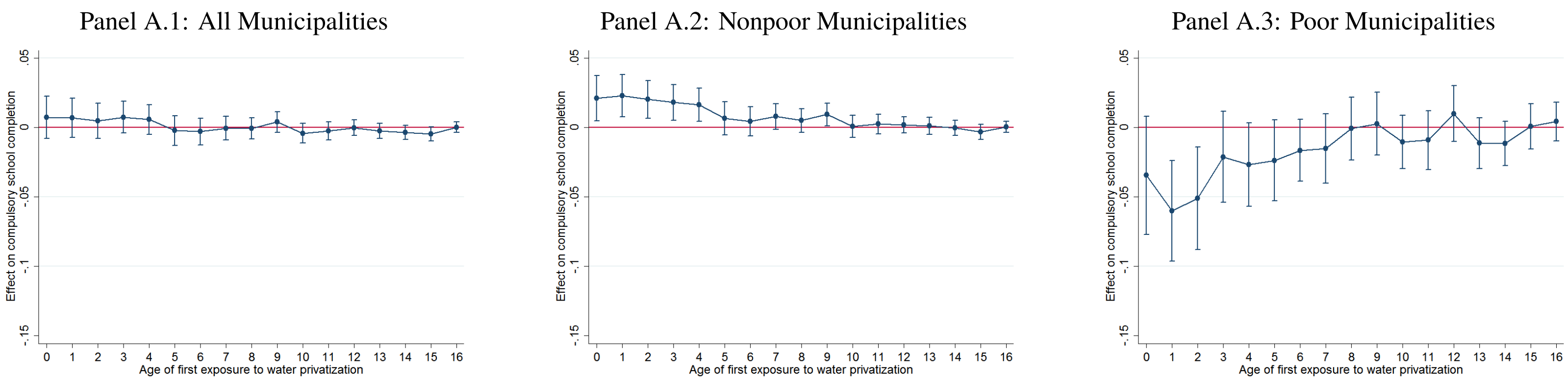
Notes: Sample consists of individuals between ages 17 to 35 in the IPUMS 2010 Census. Panels A.1, A.2, and A.3 show the mean primary school completion, compulsory school completion, and secondary school completion, respectively. Solid line represents the mean school completion in each birth cohort for municipalities which adopted water services privatization. Dashed line represents the mean school completion for municipalities which never adopted privatization. The dotted vertical line located in 1981 indicates 10 years before the first year of the water services privatization program in 1991. In other words, in municipality privatized in 1991, a child who was born in 1981 was first exposed to the privatization at age 10, which regards as late childhood exposure to the intervention. So to the left of the dotted vertical line are the pre-intervention periods and to the right are the post-intervention periods.

Figure 2.3: The Effect of Privatization on Primary School Completion



Notes: Sample consists of individuals between ages 17 to 35 in the IPUMS 2010 Census. Dependent variable primary school completion is an indicator variable equals one if an individual completed primary school or zero otherwise. Each point shows estimated $\widehat{\beta}_a$ with 95% confidence interval from equation (2.1). In Panel A.1, A.2, and A.3 show estimated $\widehat{\beta}_a$ for individuals who live in all, nonpoor, and poor municipalities. The control groups are: never treated or first treated at age 17+. Robust standard errors were clustered at municipality level. Control variables are: year of birth fixed effects, municipality fixed effects, gender dummy, provincial real GDP per capita and public expenditure per capita when individual was at age 6.

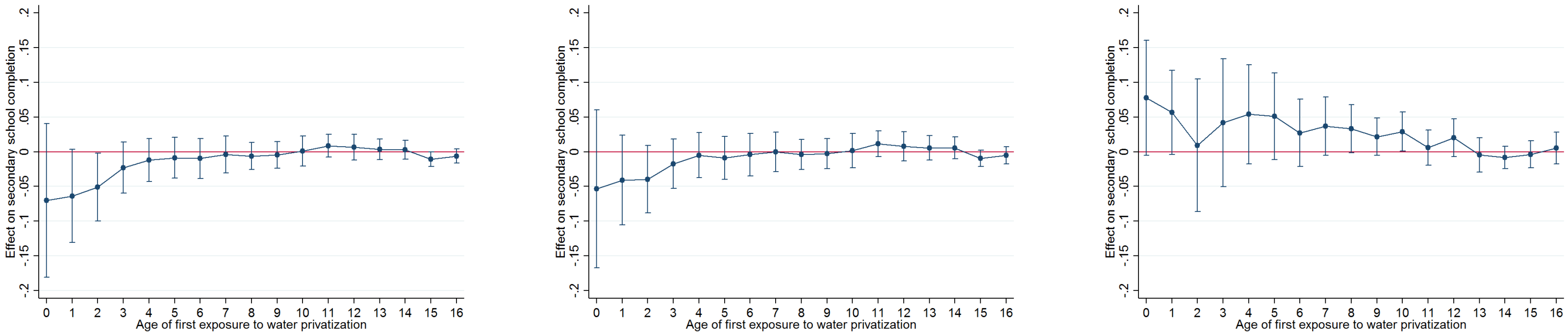
Figure 2.4: The Effect of Privatization on Compulsory School Completion



Notes: Sample consists of individuals between ages 17 to 35 in the IPUMS 2010 Census. Dependent variable compulsory school completion is an indicator variable equals one if an individual completed compulsory school or zero otherwise. Each point shows estimated $\widehat{\beta}_a$ with 95% confidence interval from equation (2.1). In Panel A.1, A.2, and A.3 show estimated $\widehat{\beta}_a$ for individuals who live in all, nonpoor, and poor municipalities. The control groups are: never treated or first treated at age 17+. Robust standard errors were clustered at municipality level. Control variables are: year of birth fixed effects, municipality fixed effects, gender dummy, provincial real GDP per capita and public expenditure per capita when individual was at age 6.

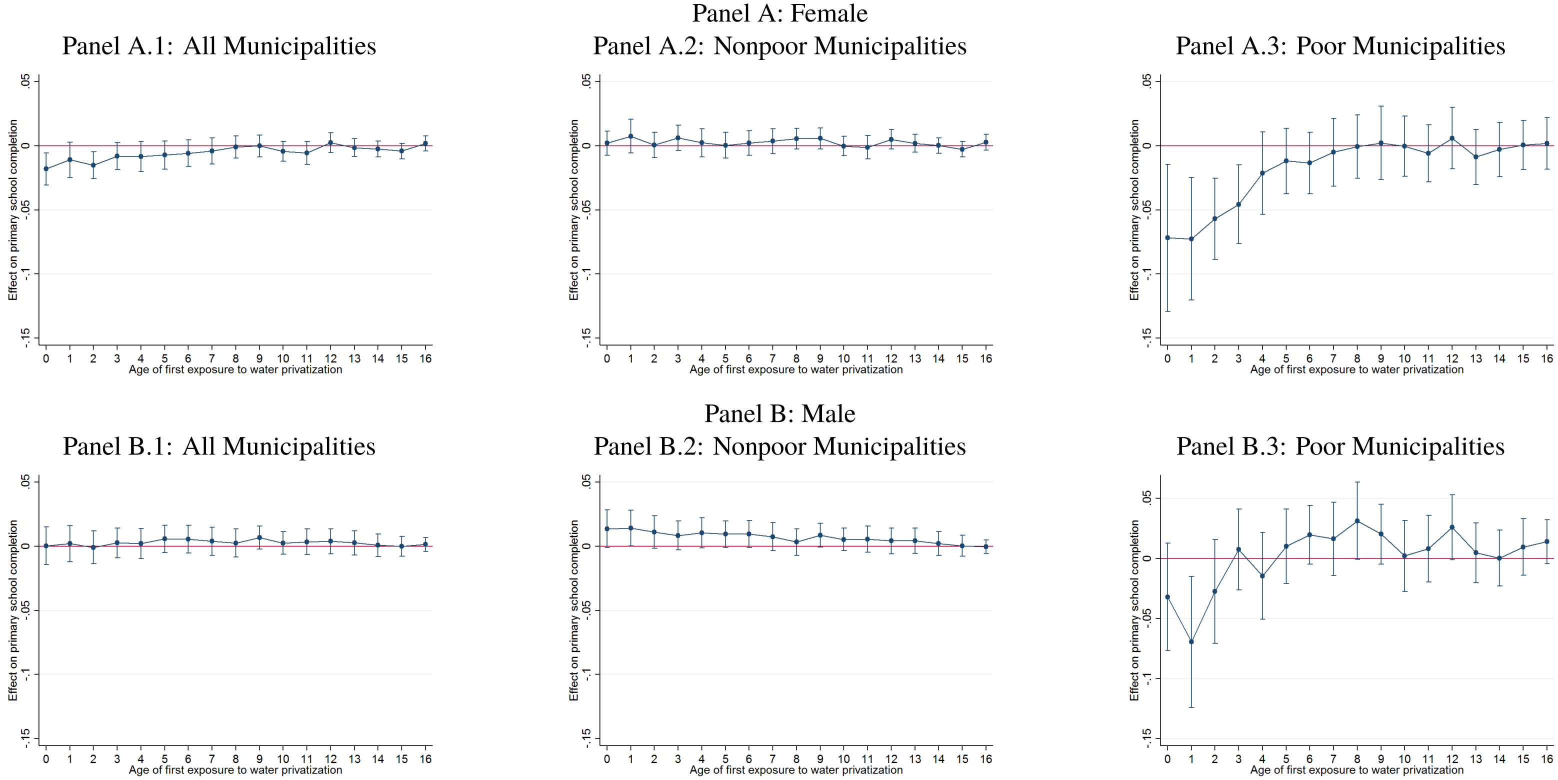
Figure 2.5: The Effect of Privatization on Secondary School Completion

Panel A.1: All Municipalities　　Panel A.2: Nonpoor Municipalities　　Panel A.3: Poor Municipalities



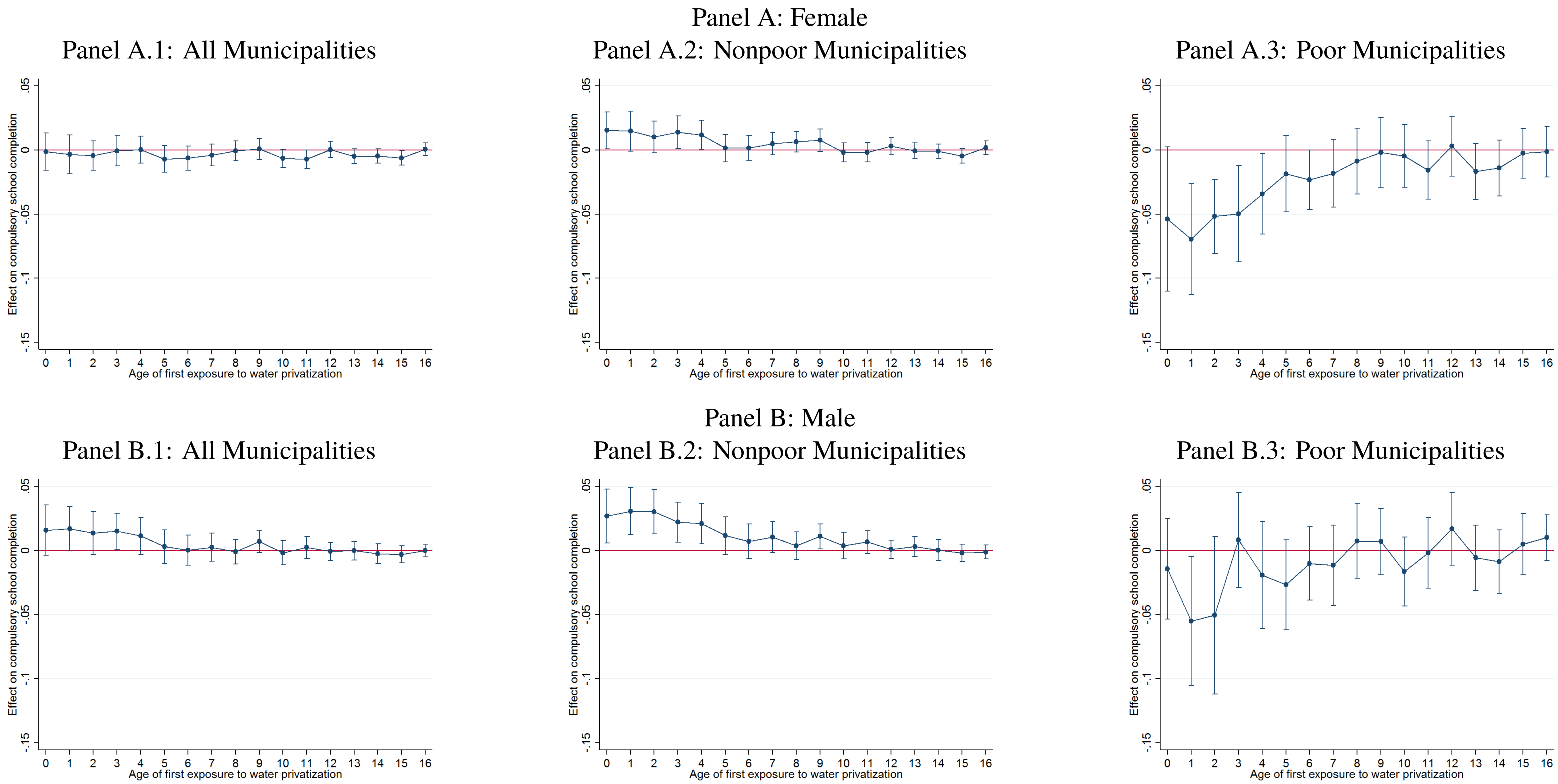
Notes: Sample consists of individuals between ages 17 to 35 in the IPUMS 2010 Census. Dependent variable secondary school completion is an indicator variable equals one if an individual completed secondary school or zero otherwise. Each point shows estimated $\widehat{\beta}_a$ with 95% confidence interval from equation (2.1). In Panel A.1, A.2, and A.3 show estimated $\widehat{\beta}_a$ for individuals who live in all, nonpoor, and poor municipalities. The control groups are: never treated or first treated at age 17+. Robust standard errors were clustered at municipality level. Control variables are: year of birth fixed effects, municipality fixed effects, gender dummy, provincial real GDP per capita and public expenditure per capita when individual was at age 6.

Figure 2.6: The Effect of Privatization on Primary School Completion by Gender and Poverty Level



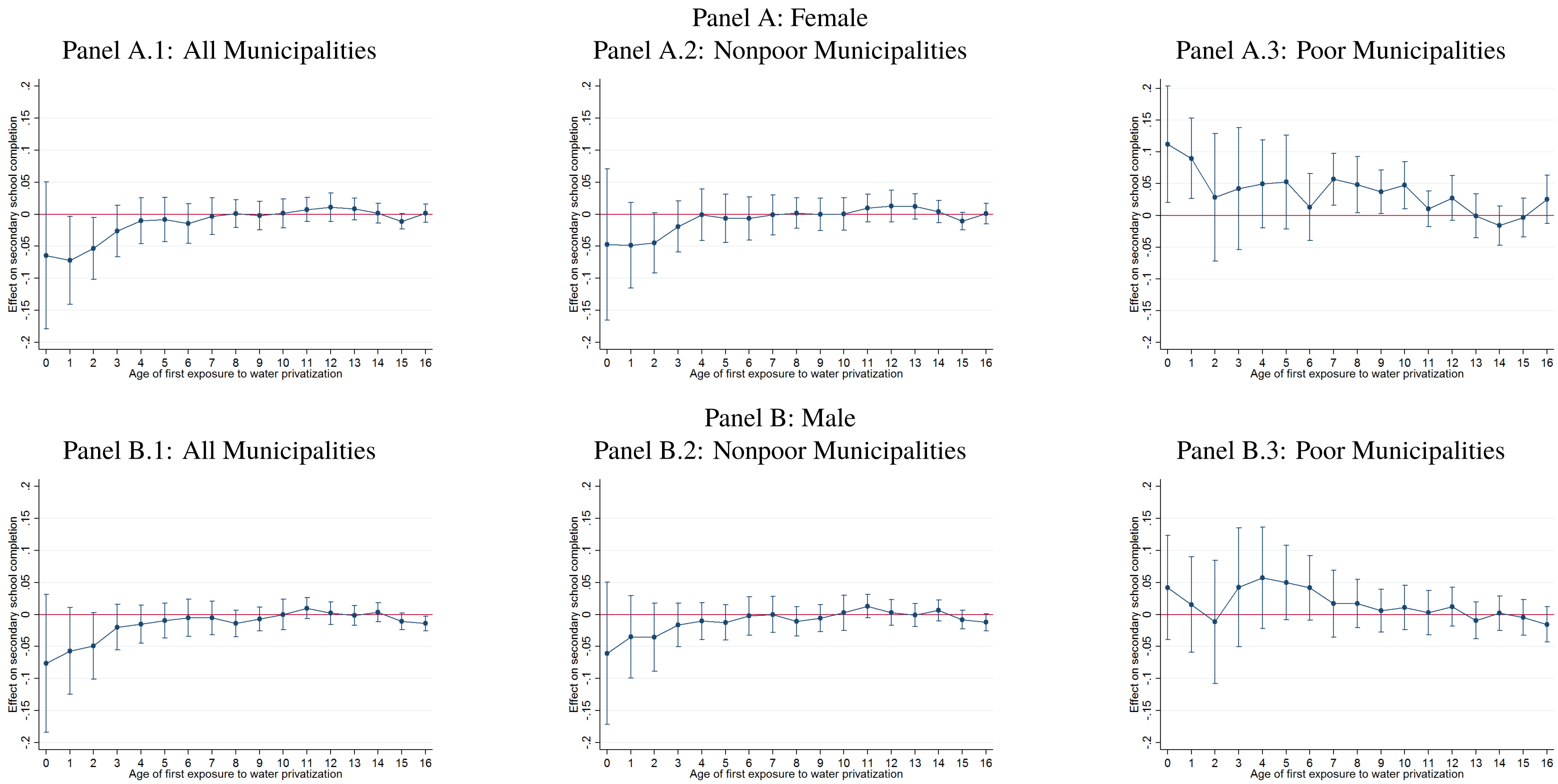
Notes: Sample consists of individuals between ages 17 to 35 in the IPUMS 2010 Census. Dependent variable primary school completion is an indicator variable equals one if an individual completed primary school or zero otherwise. Each point shows estimated $\widehat{\beta}_a$ with 95% confidence interval from equation (2.1). Panel A shows estimated results for female sample, in which Panel A.1 shows estimated $\widehat{\beta}_a$ for all municipalities, Panel A.2 shows estimates for individuals who live in nonpoor municipalities, and Panel A.3 shows estimates for individuals who live in nonpoor municipalities. Following the same logic, Panel B shows estimated results for male sample. The control groups are: never treated or first treated at age 17+. Robust standard errors were clustered at municipality level. Control variables are: year of birth fixed effects, municipality fixed effects, gender dummy, provincial real GDP per capita and public expenditure per capita when individual was at age 6.

Figure 2.7: The Effect of Privatization on Compulsory School Completion by Gender and Poverty Level



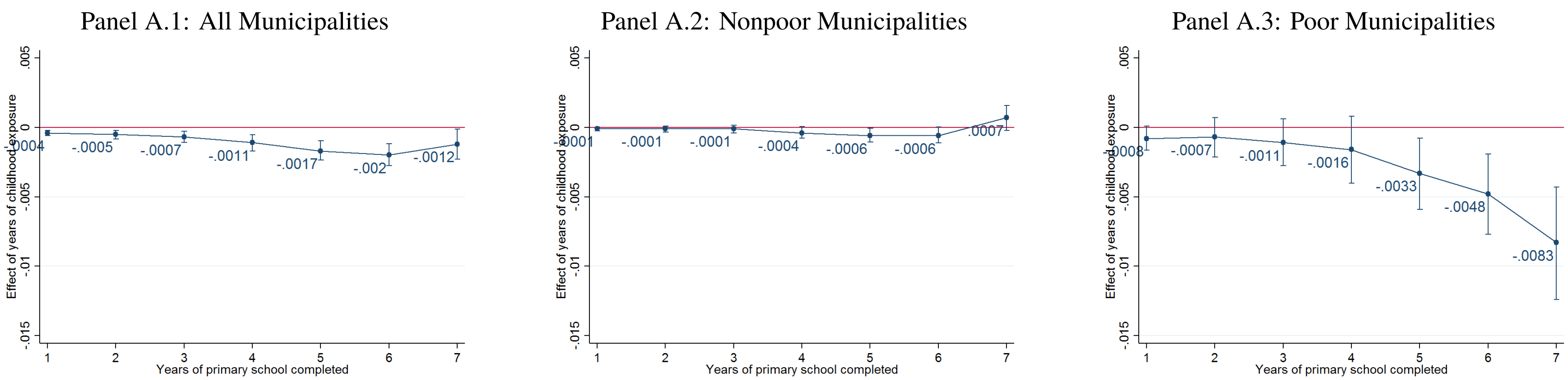
Notes: Sample consists of individuals between ages 17 to 35 in the IPUMS 2010 Census. Dependent variable compulsory school completion is an indicator variable equals one if an individual completed compulsory school or zero otherwise. Each point shows estimated $\widehat{\beta}_a$ with 95% confidence interval from equation (2.1). Panel A shows estimated results for female sample, in which Panel A.1 shows estimated $\widehat{\beta}_a$ for all municipalities, Panel A.2 shows estimates for individuals who live in nonpoor municipalities, and Panel A.3 shows estimates for individuals who live in nonpoor municipalities. Following the same logic, Panel B shows estimated results for male sample. The control groups are: never treated or first treated at age 17+. Robust standard errors were clustered at municipality level. Control variables are: year of birth fixed effects, municipality fixed effects, gender dummy, provincial real GDP per capita and public expenditure per capita when individual was at age 6.

Figure 2.8: The Effect of Privatization on Secondary School Completion by Gender and Poverty Level



Notes: Sample consists of individuals between ages 17 to 35 in the IPUMS 2010 Census. Dependent variable secondary school completion is an indicator variable equals one if an individual completed secondary school or zero otherwise. Each point shows estimated $\widehat{\beta}_a$ with 95% confidence interval from equation (2.1). Panel A shows estimated results for female sample, in which Panel A.1 shows estimated $\widehat{\beta}_a$ for all municipalities, Panel A.2 shows estimates for individuals who live in nonpoor municipalities, and Panel A.3 shows estimates for individuals who live in nonpoor municipalities. Following the same logic, Panel B shows estimated results for male sample. The control groups are: never treated or first treated at age 17+. Robust standard errors were clustered at municipality level. Control variables are: year of birth fixed effects, municipality fixed effects, gender dummy, provincial real GDP per capita and public expenditure per capita when individual was at age 6.

Figure 2.9: The Effect of Years of Childhood Exposure on Years of Primary School Completed



Notes: Sample consists of individuals between ages 17 to 35 in the IPUMS 2010 Census. Each point represents coefficient estimated by separate regression. Horizontal axis indicates dependent variable completed k yrs of primary school equals one if individual completed k years of primary school, $k = 1, 2, 3, ..., 7$ or zero otherwise. Vertical axis indicated estimated $\widehat{\beta}$ of years of childhood exposure with 95% confidence interval from equation (2.3). Panels A.1, A.2, and A.3 show estimated $\widehat{\beta}$ for individuals who live in all, nonpoor, and poor municipalities. Robust standard errors were clustered at municipality level. Control variables are: year of birth fixed effects, municipality fixed effects, gender dummy, provincial real GDP per capita and public expenditure per capita when individual was at age 6.

2.9 Tables

Table 2.1: Summary Statistics

	Panel A: All municipalities				
	N	Mean	Std.Dev.	Min.	Max.
Primary school completion	856914	0.924	0.265	0.00	1.00
Compulsory school completion	856914	0.911	0.284	0.00	1.00
Secondary school completion	856914	0.526	0.499	0.00	1.00
Exposed to privatization	856914	0.579	0.494	0.00	1.00
Yrs of childhood exposure	856914	0.480	1.392	0.00	7.00
Female	856914	0.504	0.500	0.00	1.00
Age	856914	25.694	5.459	17.00	35.00
Poor municipalities	856914	0.179	0.383	0.00	1.00
	Panel B: Nonpoor municipalities				
	N	Mean	Std.Dev.	Min.	Max.
Primary school completion	703458	0.941	0.236	0.00	1.00
Compulsory school completion	703458	0.929	0.257	0.00	1.00
Secondary school completion	703458	0.556	0.497	0.00	1.00
Exposed to privatization	703458	0.629	0.483	0.00	1.00
Yrs of childhood exposure	703458	0.530	1.458	0.00	7.00
Female	703458	0.503	0.500	0.00	1.00
Age	703458	25.766	5.436	17.00	35.00
	Panel C: Poor municipalities				
	N	Mean	Std.Dev.	Min.	Max.
Primary school completion	153456	0.848	0.359	0.00	1.00
Compulsory school completion	153456	0.829	0.376	0.00	1.00
Secondary school completion	153456	0.385	0.487	0.00	1.00
Exposed to privatization	153456	0.350	0.477	0.00	1.00
Yrs of childhood exposure	153456	0.253	1.007	0.00	7.00
Female	153456	0.506	0.500	0.00	1.00
Age	153456	25.363	5.554	17.00	35.00

Notes: Sample consists of individuals between ages 17 to 35 in the IPUMS 2010 Census in 275 municipalities. Yrs of childhood exposure = $\text{Max}((7 - \text{age of first exposure}), 0)$.

Table 2.2: The Effect of Privatization on Primary School Completion

	Dependent Variable Primary school completion		
	Panel A: Female and Male		
	All municipalities 0.92	Nonpoor municipalities 0.94	Poor municipalities 0.85
	(1)	(2)	(3)
Yrs of childhood exposure	-0.001**	0.001	-0.008***
	(0.001)	(0.000)	(0.002)
R-squared	0.047	0.018	0.056
N	856914	703458	153456
	Panel B: Female		
	All municipalities 0.94	Nonpoor municipalities 0.95	Poor municipalities 0.86
	(4)	(5)	(6)
Yrs of childhood exposure	-0.002***	0.000	-0.010***
	(0.001)	(0.001)	(0.002)
R-squared	0.046	0.013	0.059
N	431708	354080	77628
	Panel C: Male		
	All municipalities 0.91	Nonpoor municipalities 0.93	Poor municipalities 0.83
	(7)	(8)	(9)
Yrs of childhood exposure	-0.000	0.001*	-0.007***
	(0.001)	(0.001)	(0.002)
R-squared	0.048	0.020	0.054
N	425206	349378	75828

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. Sample consists of individuals between ages 17 to 35 in the IPUMS 2010 Census in the 275 municipalities. Robust standard errors were clustered at the municipalities level. Dependent variable primary school completion is an indicator variable equals one if an individual completed primary school or zero otherwise. Yrs of childhood exposure = $\text{Max}((7-\text{age of first exposure}), 0)$. Panels A, B, and C show the results for sample female and male, female, and male, respectively. Each column in each panel represents a separate regression. Columns (1), (4), and (7) show estimation for all municipalities, Columns (2), (5), and (8) show estimation for nonpoor municipalities, and Columns (3), (6), and (9) show estimation for poor municipalities. Control variables are: year of birth fixed effects, municipality fixed effects, gender dummy, provincial real GDP per capita and public expenditure per capita when individual was at age 6.

Table 2.3: The Effect of Privatization on Compulsory School Completion

	Dependent Variable Compulsory school completion		
	Panel A: Female and Male		
	All municipalities 0.91	Nonpoor municipalities 0.93	Poor municipalities 0.83
	(1)	(2)	(3)
Yrs of childhood exposure	0.001*	0.003***	-0.007***
	(0.001)	(0.001)	(0.002)
R-squared	0.050	0.021	0.057
N	856914	703458	153456
	Panel B: Female		
	All municipalities 0.92	Nonpoor municipalities 0.94	Poor municipalities 0.85
	(4)	(5)	(6)
Yrs of childhood exposure	0.000	0.002***	-0.008***
	(0.001)	(0.001)	(0.002)
R-squared	0.046	0.015	0.058
N	431708	354080	77628
	Panel C: Male		
	All municipalities 0.90	Nonpoor municipalities 0.92	Poor municipalities 0.81
	(7)	(8)	(9)
Yrs of childhood exposure	0.003**	0.004***	-0.005
	(0.001)	(0.001)	(0.003)
R-squared	0.051	0.024	0.055
N	425206	349378	75828

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. Sample consists of individuals between ages 17 to 35 in the IPUMS 2010 Census in the 275 municipalities. Robust standard errors were clustered at the municipalities level. Dependent variable Compulsory school completion is an indicator variable equals one if an individual completed compulsory school or zero otherwise. Yrs of childhood exposure = Max$((7 -$ age of first exposure$), 0)$. Panels A, B, and C show the results for sample female and male, female, and male, respectively. Each column in each panel represents a separate regression. Columns (1), (4), and (7) show estimation for all municipalities, Columns (2), (5), and (8) show estimation for nonpoor municipalities, and Columns (3), (6), and (9) show estimation for poor municipalities. Control variables are: year of birth fixed effects, municipality fixed effects, gender dummy, provincial real GDP per capita and public expenditure per capita when individual was at age 6.

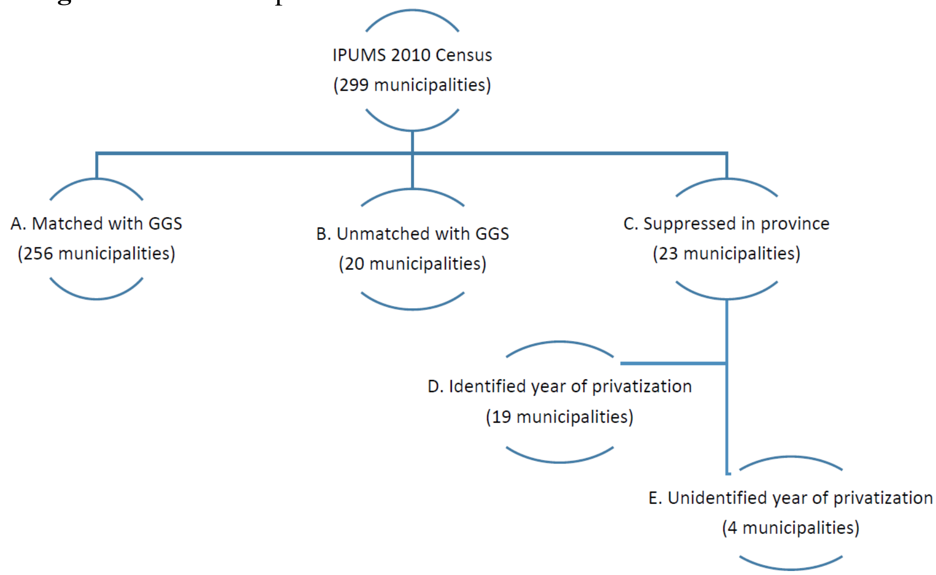
Table 2.4: The Effect of Privatization on Secondary School Completion

	Dependent Variable Secondary school completion		
	Panel A: Female and Male		
	All municipalities 0.53	Nonpoor municipalities 0.56	Poor municipalities 0.39
	(1)	(2)	(3)
Yrs of childhood exposure	-0.009**	-0.007	0.006
	(0.005)	(0.005)	(0.007)
R-squared	0.095	0.086	0.052
N	856914	703458	153456
	Panel B: Female		
	All municipalities 0.57	Nonpoor municipalities 0.60	Poor municipalities 0.43
	(4)	(5)	(6)
Yrs of childhood exposure	-0.010**	-0.008*	0.007
	(0.005)	(0.005)	(0.007)
R-squared	0.092	0.082	0.052
N	431708	354080	77628
	Panel C: Male		
	All municipalities 0.48	Nonpoor municipalities 0.51	Poor municipalities 0.34
	(7)	(8)	(9)
Yrs of childhood exposure	-0.009*	-0.007	0.005
	(0.005)	(0.005)	(0.007)
R-squared	0.084	0.076	0.038
N	425206	349378	75828

Notes: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Sample consists of individuals between ages 17 to 35 in the IPUMS 2010 Census in the 275 municipalities. Robust standard errors were clustered at the municipalities level. Dependent variable secondary school completion is an indicator variable equals one if an individual completed secondary school or zero otherwise. Yrs of childhood exposure = Max$((7 -$ age of first exposure$), 0)$. Panels A, B, and C show the results for sample female and male, female, and male, respectively. Each column in each panel represents a separate regression. Columns (1), (4), and (7) show estimation for all municipalities, Columns (2), (5), and (8) show estimation for nonpoor municipalities, and Columns (3), (6), and (9) show estimation for poor municipalities. Control variables are: year of birth fixed effects, municipality fixed effects, gender dummy, provincial real GDP per capita and public expenditure per capita when individual was at age 6.

2.10 Appendix

Figure 2.10: Municipalities in IPUMS with Identified Year of Privatization



Notes: There are 23 provinces and 299 municipalities in the IPUMS 2010 Census, in which 256 municipalities are matched with Galiani, Gertler, and Schargrodsky (2005) (GGS)'s data (section A). 20 of them are either no service or missing information of water services, or they are identifiable in the 2010 Census but not exist in Galiani, Gertler, and Schargrodsky (2005) (section B). 23 of them are suppressed municipalities in each province (section C). For section C, I go back to Galiani, Gertler, and Schargrodsky (2005) to figure out their year of privatization. I realized that most of these suppressed municipalities with under 20,000 residents are either never privatized or all privatized at the same year under each province. Eventually, I am able to identify year of privatization of 19 municipalities (section D). Overall, my empirical analysis contains 275 municipalities (sections A + D) which represent 84% of municipalities (400 municipalities) in Galiani, Gertler, and Schargrodsky (2005).

Table 2.5: Water Services Privatization in Municipalities

Privatization year	Municipality
1991	Bella Vista; Capital - Corrientes; Curuzú Cuatiá; Esquina; Goya; Mercedes; Monte Caseros; Paso de los Libres
1992	N/A
1993	City of Buenos Aires; Almirante Brown; Avellaneda; General San Martín; La Matanza; Lanús; Lomas de Zamora; Quilmes; San Fernando; San Isidro; Tigre; Tres de Febrero; Vicente López
1994	Balcarce
1995	Caseros; Castellanos; General Obligado; Iriondo; La Capital - Santa Fe; Rosario; Cruz Alta; Chicligasta; Juan Bautista Alberdi; Leales; Lules; Monteros; Rio Chico; Capital - Tucumán ; Simoca; Tafí Viejo; Yerba Buena
1996	Formosa; Pilcomayo
1997	Capital - Córdoba
1998	Campana; Capital - Mendoza; General Alvear; Godoy Cruz; Guaymallén; Junín; Las Heras; Lavalle; Malargüe; Rivadavia; San Carlos; San Martín; San Rafael; Tunuyán; Departments under 20,000 in Mendoza province; Anta; Capital - Salta; Cerrillos; General Guemes; General José de San Martín; Metán; Orán; Rivadavia; Rosario de la Frontera; Rosario de Lerma; Departments under 20,000 in Salta province; Banda; Capital - Santiago del Estero; General Taboada; Moreno; Río Hondo
1999	Ayacucho; Bahía Blanca; Berisso; Bragado; Carlos Casares; Coronel de Marine L. Rosales; Chivilcoy; Dolores; Ensenada; Escobar; General Juan Madariaga; General Rodríguez; General Viamonte; General Villegas; La Plata; Las Flores; Lincoln; Mar Chiquita; Merlo; Monte; Moreno; 9 de Julio; Patagones; Pehuajó; Villarino
Never privatized	Adolfo Alsina; Azul; Baradero; Arrecifes; Benito Juárez; Berazategui; Bolívar; Brandsen; Colon; Coronel Pringles; Coronel Suárez; Chacabuco; Chascomús; Exaltación de la Cruz; General Alvarado; General Pueyrredon; Junín; La Costa; Lobos; Luján; Marcos Paz; Mercedes; Necochea; Olavarría; Pergamino; Pinamar; Puan; Ramallo; Rojas; Saavedra; Saladillo; Salto; San Andrés de Giles; San Antonio de Areco; San Nicolás; San Pedro; Tandil; Trenque Lauquen; Tres Arroyos; 25 de Mayo; Villa Gesell; Zárate; Belén; Capital - Catamarca; Valle Viejo; Departments under 20,000 in Catamarca province; Calamuchita; Colón; Cruz del Eje; General Roca; General San Martín; Ischilín; Juárez Celman; Marcos Juárez; Presidente Roque Sáenz Peña; Punilla; Río Cuarto; Río Primero; Río Segundo; San Alberto; San Javier; San Justo; Santa María; Tercero Arriba; Unión; Departments under 20,000 in Cordoba province; Ituzaingó; Lavalle; Santo Tomé; Almirante Brown; Bermejo; Comandante Fernández; General Güemes; Libertador General San Martín; Maipú; Mayor Luis J. Fontana; 9 de Julio; Quitilipi; San Fernando; 25 de Mayo; Departments under 20,000 in Chaco province; Biedma; Cushamen; Escalante; Futaleufú; Rawson; Departments under 20,000 in Chubut province; Diamante; Federación; Federal; Gualeguay; Gualeguaychú; La Paz; Nogoyá; Paraná; Tala; Uruguay; Victoria; Departments under 20,000 in Entre Rios province; Patiño; Pirané; Departments under 20,000 in Formosa province; El Carmen; Dr. Manuel Belgrano; Ledesma; Palpala; San Pedro; Departments under 20,000 in Jujuy province; Capital - La Pampa; Maracó; Departments under 20,000 in La Pampa province; Capital - La Rioja; Chilecito; Departments under 20,000 in La Rioja province; Luján de Cuyo; Maipú; Tupungato; Apóstoles; Cainguás; Candelaria; Capital - Misiones; El Dorado; General Manuel Belgrano; Guaraní; Iguazú; Leandro N. Alem; Libertador General San Martín; Montecarlo; Oberá; San Ignacio; San Pedro; 25 de Mayo; Departments under 20,000 in Misiones province; Confluencia; Lácar; Pehuenches; Zapala; Departments under 20,000 in Neuquen province; Adolfo Alsina; Avellaneda; Bariloche; General Roca; San Antonio; Departments under 20,000 in Rio Negro province; Capital - San Juan; Caucete; Chimbas; Pocito; Departments under 20,000 in San Juan province; Chacabuco; General Pedernera; Junín; La Capital - San Luis; Departments under 20,000 in San Luis province; Deseado; Güer Aike; Departments under 20,000 in Santa Cruz province; Belgrano; Constitución; General López; Las Colonias; 9 de Julio; San Cristóbal; San Javier; San Jerónimo; San Justo; San Lorenzo; San Martín; Vera; Departments under 20,000 in Santa Fe province; Copo; Choya; Robles; Burruyacú; Famailla; Río Grande; Departments under 20,000 in Tierra del Fuego province

Notes: There are 275 municipalities. As mentioned in Section 2.4, these are the municipalities after I merged the municipalities in the 2010 Census with the privatization information from Galiani, Gertler, and Schargrodsky (2005)'s data.
Sources: Galiani, Gertler, and Schargrodsky (2005).

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