Water is Life, Clean Water Means Health: The Effect of Early-Life Exposure to City-Wide Water Filtration on Old-Age Male Mortality[*†]

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Abstract

This study examines the impact of water purification on long-run old-age mortality. We examine the effects of early-life and childhood exposure to improvements in water quality due to city-wide water filtration programs in 25 major American cities on later-life old-age longevity of male individuals. We employ data from Social Security Administration death records linked with the 1940 census. The difference-in-difference regressions suggest an improvement in male longevity of about 3.2 months. A series of balancing tests do not reveal evidence that changes in sociodemographic and socioeconomic characteristics of individuals confound the estimates. We also implement a full battery of sensitivity analyses and show that the effect is robust across specifications, subsamples, and functional form checks. Analyses using 1950-1970 censuses suggest that a portion of the long-term links can be explained by improvements in individuals' education and income as a result of early-life exposure to water filtration. We also show that treated cohorts reveal improvements in height and cognitive scores during early adulthood.

Keywords: Mortality, Longevity, Public Health, Clean Water, Water Filtration, In-Utero Exposures, Early-Life Exposures, Historical Data

JEL Codes: I18, J18, N51, N52, O13, Q28

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[†] The phrase "Water is life, clean water means health" is inspired by a quote from Audrey Hepburn, who was a passionate advocate for clean water access.

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1. Introduction

Contaminated drinking water has been the source of various diseases and a serious threat to public health for centuries. As of 2019, about 2.2 billion people worldwide still lack access to safely managed drinking water (WHO, 2019). The health impacts of poor water quality are disproportionate across age distribution, with higher impacts among infants and children. This is more evident as death rates due to waterborne diseases such as typhoid, cholera, and diarrhea have been historically higher among infants and children (Armstrong et al., 1999). In the US during the early 20th century, there have been substantial improvements in water quality through a series of state-wide and city-wide campaigns to raise expenditures toward public health infrastructures. These campaigns were successful in raising the access and quality of water and contributed to reductions in urban mortality during the first decades of the 20th century (Anderson, Charles, & Rees, 2022; Beach et al., 2016; Cutler & Miller, 2005; Troesken, 2004) Specifically, studies suggest that clean water benefits infants' health more than other age groups (Anderson, Charles, & Rees, 2022). Among many public health interventions, improvements in water technology, specifically water filtration, have affected infant mortality rates most (Costa, 2015). Studies that use more recent data from developing countries that experience rapid industrialization suggest the importance of water quality for infants' health outcomes (Greenstone & Hanna, 2014; Mettetal, 2019; Zhang & Xu, 2016).

The potential effects of water quality on infants could have long-lasting consequences. A growing literature suggests that life-cycle outcomes could partly reflect conditions in early-life (Aizer et al., 2016; Almond et al., 2012, 2018; Almond & Currie, 2011a, 2011b; Barker, 1994, 1995, 1997, 2004). A strand of this literature explores the sources of disparate longevity across individuals and documents the significant effects of early-life shocks on old-age health and mortality (Bailey et al., 2016; Hayward & Gorman, 2004; Montez et al., 2014; Lindeboom et al., 2010; Scholte et al., 2015; Smith, 2009; Van Den Berg et al., 2006, 2011). For instance, studies show that in-utero and early-life disease environment is associated with adulthood education and income and old-age cognitive functioning and longevity (Blackwell et al., 2001; Bleakley, 2007; Case & Paxson, 2009; Crimmins & Finch, 2006; Finch & Crimmins, 2004; Moore et al., 2006; Venkataramani, 2012). Therefore, the effects of water quality in early life can also be detected in life-cycle outcomes. However, while the evidence on its short-term effects is abundant, very few studies have explored the long-term effects, specifically on old-age health and mortality (Beland & Oloomi, 2019; Grossman & Slusky, 2019; Hafeman et al., 2007; He & Perloff, 2016; Jones, 2019). To fill this void in the literature, this paper examines the effects of water filtration across US cities on old-age mortality.

Water filtration involves passing water through a series of physical or chemical barriers or employing other biological mechanisms to remove contaminants, impurities, pollutants, pathogens, and other harmful microorganisms, resulting in cleaner, safer water. Such filtration and purification systems are crucial in preventing waterborne diseases, such as cholera, typhoid fever, and dysentery.[5] These diseases are caused by bacteria, viruses, and protozoa in the contaminated water. While these pathogens are harmful to the whole population, the impacts are considerably stronger in critical stages of development, especially during in-utero, early life, and childhood (Beach et al., 2016; Condran & Crimmins-Gardner, 1978; Kunitz, 1984). Moreover, these diseases have spillover impacts on survivors and make them vulnerable to other non-waterborne diseases.

[5] Several bacterial diseases (e.g., Salmonella infections, Cholera caused by Vibrio cholerae, Typhoid Fever caused by Salmonella typhi, Dysentery caused by Shigella bacteria, Legionellosis caused by Legionella pneumophila, and certain strains of E. coli infection), parasitic diseases (e.g., Giardiasis caused by Giardia lamblia, Cryptosporidiosis caused by Cryptosporidium, and amoebic dysentery caused by Entamoeba histolytica), and viral diseases (e.g., Hepatitis A, Norovirus infection, and Polio) can be transmitted through contaminated water.

For instance, based on the hypothesis of Mills–Reincke phenomenon, the prevention of typhoid fever death resulting from water filtration and purification is associated with a threefold reduction in deaths from other non-waterborne diseases (Friedrich, 1912; McGee, 1920). Moreover, the low case-fatality rate of waterborne diseases such as typhoid increases the vulnerability of survivors to later-life diseases such as tuberculosis, pneumonia, and kidney failure (Ferrie & Troesken, 2008). Therefore, one might expect to detect the benefits of exposure to water filtration during infancy and childhood for later-life and adulthood outcomes.

This paper employs data from Social Security Administration death records of male individuals over the years 1975-2005 linked with the full-count 1940 census. We exploit cross-census linkages to find individual records in historical full-count censuses 1900-1930 in order to infer their city-of-birth/childhood. We implement difference-in-difference regressions to compare the longevity of individuals who were exposed to city-specific water filtration projects across different ages. We show that city-wide improvements in water quality during in-utero and early-life is associated with about 3.2 months higher age-at-death during old ages.

The main assumption in our identification strategy is that the longevity of individuals in cities that implemented water filtration projects would have followed the same path and been influenced by the same factors in the absence of any water filtration project. We provide empirical evidence to support the exogeneity of the treatment. We show that the observed effect is not an artifact of changes in the sociodemographic composition of the final sample due to differential survival into adulthood, changes in the socioeconomic and sociodemographic composition of cities following public health reforms, changes in public spending as well as implementation of other public health interventions, changes in other city-level and state-level policies and reforms, endogenous merging across censuses and death records, and endogenous fertility. A series of sensitivity analyses suggest that the effect is robust across a wide range of alternative specifications, functional forms, and subsamples.

To explore mechanism channels, we use census data for the years 1950-1970, a period when cohorts under study are experiencing their adulthood years. We show that exposure to water quality improvements in the year-of-birth is associated with higher schooling, higher income, and increased socioeconomic scores. Several studies document the association between education/income profile and later-life longevity (Chetty et al., 2016; Cristia, 2009; Cutler et al., 2006; Fletcher et al., 2021; Fletcher, 2015; Kinge et al., 2019; Lleras-Muney, 2022). Therefore, we argue that increases in education and improvements in labor market outcomes could be potential mechanism channels.

This paper contributes to two strands of literature. First, to our knowledge, this is the first study to examine the long-run mortality effects of water quality in early-life. While several studies document the relevance of in-utero and early-life water quality exposure on education and labor market outcomes, no study has explored its effects on old-age health and mortality (Beach et al., 2016; Smith et al., 2012; Zaveri et al., 2019; Zhang & Xu, 2016). More specifically, this study is the first to examine the long-term effects of water quality improvements in the US's first decades of the 20th century on later-life health outcomes. Studies that examine health effects of public health interventions during this era have focused mostly on short-run outcomes (Anderson et al., 2019; Anderson, Charles, & Rees, 2022; Anderson, Charles, McKelligott, et al., 2022; D. Cutler & Miller, 2005). Improving drinking water quality reduces the disease burden of early-life environments. Therefore, our study also evaluates the impact of reducing early-life disease burden on later-life health. Hence, the second contribution of our paper is to add to the growing literature on the relevance of early-life disease environment and later-life mortality (Almond et al., 2012;

Bleakley, 2007; Case et al., 2005; Case & Paxson, 2009; Cormack et al., 2024; Crimmins & Finch, 2006).

The rest of the paper is organized as follows. Section 2 reviews the literature. Section 3 introduces data sources and sample selections. Section 4 discusses the econometric method. Section 5 reviews the main results. Section 6 explores potential mechanism channels. Finally, we depart some concluding remarks in section 7.

2. A Brief Literature Review

Several studies document the health benefits of public health infrastructures in American cities during the early decades of the 20th century. Cutler & Miller (2005) employ data from 13 major American cities and explore the role of improvements in clean water technologies in reducing urban mortality rates. They find significant reductions in total mortality rates. Further, they show that infant mortality drops by about 35% in the years following water disinfection. Anderson, Charles, & Rees (2022) revisit their analysis and make some corrections to their data caused by transcription errors. Their reevaluation suggests smaller but significant impacts of water filtration. They document that post-water filtration infant mortality drops by about 11%. Although they also explore the impacts of other public health interventions, including water projects, water chlorination, water filtration, and sewage treatment, they fail to find statistically significant effects of any of these interventions. Anderson et al. (2019) examine the effect of tuberculosis movements in the US between 1900-1917 on TB mortality rates. They find modest and mostly insignificant effects of various anti-TB measures on mortality. However, they show that establishing state-run sanatoriums resulted in about 4 percent reductions in pulmonary TB mortality. Anderson, Charles, McKelligott, et al. (2022) explore the effect of milk inspections in major American cities during 1880-1910 on infants' and children's mortality and find small and insignificant effects.

A strand of research employs more recent data and explores the association between water quality and infants' and children's health outcomes (Apergis et al., 2019; Currie et al., 2017; Greenstone & Hanna, 2014; Hafeman et al., 2007; He & Perloff, 2016; Hill & Ma, 2017). For instance, Clay et al. (2014) examine the effects of exposure to lead in drinking water across cities in the US between 1900-1920. They exploit the fact that lead is more likely to leach into drinking water if the water is more acidic. They document that going from the city with high exposure to the city with low exposure in their sample is associated with a 7-33 percent reduction in infant mortality rates.

Grossman & Slusky (2019) examine the impact of the Flint water crisis, in which the city of Flint, Michigan, changed its water supply resulting in sharp increases of contaminants in the water supply, on fertility and birth outcomes. They find that rises in drinking water contaminants, including lead, resulted in a 12 percent decrease in fertility rate and a 5.4 percent reduction in birth weight. Currie et al. (2013) examine the impact of water quality on birth outcomes using data from New Jersey between 1997-2007. They compare birth outcomes of infants from the same mother who were exposed to differential contaminations in drinking water. They find negative and significant effects on birth weight and the gestational length of infants of low-educated mothers. Brainerd & Menon (2014) investigate the effect of water pollution due to seasonal changes in fertilizer agrichemical use on infants' and children's health outcomes in India. They show that children exposed to higher levels of agricultural water pollution exposure reveal higher mortality rates and lower height and weight-for-age scores. Hill & Ma (2022) and Hill (2018) provide evidence that shale gas development during the recent fracking boom in the US resulted in higher water pollution and negative impacts on infants' health outcomes. Jones (2019) examines the impact of microcystin in drinking water, a potent toxin produced by cyanobacteria in freshwater algal blooms, on infants' health outcomes. He uses data from Michigan and exploits a one-time municipal attempt to improve water quality and remove algae. He finds that the intervention increased to 17 grams in birth weight and 3.2 days additional gestational age.

Therefore, one expects to observe positive impacts of improvements in water quality on infants' health. Healthier infants are more likely to experience a healthier childhood, develop more cognitive and noncognitive skills, attain higher levels of human capital, reveal better labor market outcomes, and generally have a healthier adulthood (Behrman & Rosenzweig, 2004; Black et al., 2007; Cook & Fletcher, 2015; Fletcher, 2011; Maruyama & Heinesen, 2020; Royer, 2009; Shenkin et al., 2009). A narrow strand of research examines the direct link between in-utero and early-life exposure to a change in water quality and later-life outcomes (Smith et al., 2006, 2012; Zaveri et al., 2019). For instance, Beach et al. (2016) argue that water purification technologies significantly reduced typhoid mortality rates. They proxy water quality with city-level typhoid mortality for the period 1900-1940 and examine the impacts of improvements in water quality in early-life on adulthood education and earnings. They find that water improvements resulted in an increase of about 9 percent higher adulthood income and 0.7 years additional years of schooling. Zhang & Xu (2016) examine the impact of a major water treatment program in rural China. They find that those who benefited from the program in early-life attain roughly 1 additional year of schooling during adulthood.

3. Data and Sample

The primary data source of this study comes from Death Master Files (DMF) of Social Security Administration death records extracted from the Censoc Project (Breen et al., 2023; Breen & Osborne, 2022; Goldstein et al., 2021). DMF data covers deaths that occurred among male individuals born between 1975 and 2005.[6] It contains information on dates of birth, death, limited demographic characteristics, and an identifier to link the individual with the full-count 1940 census. DMF data has two advantages that make it superior to alternative data sources in examining long-term associations. First, the raw data contains millions of observations, allowing for various types of heterogeneity analyses. Second, the data contains all familial and geographic information available in the 1940 census. Specifically, we have information on the below-state place of residence in both 1935 and 1940, as reported in the 1940 census. Moreover, the existence of cross-census linking rules allow researchers to link the full-count 1940 census to historical censuses which enables them to observe place-of-residence of individuals in earlier decades. We merge the DMF data with the 1940 census extracted from Ruggles et al. (2020).

We use city-level water filtration project data from Anderson, Charles, & Rees (2022). It provides a city-by-year panel of 25 major cities between 1900-1940 and reports whether a city has a water filtration system each year. Figure 1 shows the geographic distribution of cities in the final sample and the year each city implemented water filtration. Appendix A provides a list of these cities with the year of water filtration in each city.

Since this study focuses on assessing early-life exposures, we need to assign city-level water filtration status based on individuals' year-of-birth and city-of-birth/childhood. However, the 1940 census does not report the city-of-birth/childhood. One idea is to use the city-of-residence in 1935 and 1940 as a proxy for place-of-birth. However, individuals may migrate from their birthplace, and this migration could be a response to city-level improvements in public health,

[6] The DMF data reports deaths to male individuals only. We use the Berkeley Unified Numident Mortality Database (BUNMD) to examine the effects across both genders. The disadvantage of BUNMD data is that its death coverage is more comprehensive post-1988 years and that it is not linked to the 1940 census. Hence, we implement the main analysis of the paper using DMF data. In Appendix D, we show that using BUNMD data we observe almost identical coefficients to those of the main results if restrict the sample to male individuals only. However, among female individuals, the coefficients are considerably smaller in magnitude and statistically insignificant.

hence being endogenous. To mitigate this issue and infer city-of-birth/childhood, we start with DMF-census-linked data and merge the records with historical censuses 1900-1930 using cross-census linkage datasets extracted from Abramitzky et al. (2020). We then use the geographic information provided by the census in which a person appears for the first time in any census as the place of birth/childhood.[7] We then merge the DMF-census data with the water filtration data based on birth/childhood-city and birth-year.[8]

In our regressions, we also control for a series of city-by-year covariates. These covariates are constructed using full-count decennial censuses 1900-1940 and linearly interpolated for inter-decennial years. Section 6 also uses censuses from 1950-1970 to explore potential pathways. These census data are extracted from Ruggles et al. (2020). Finally, in section 5.3, we employ natality and mortality data at the city level extracted from Bailey, Clay, et al. (2016).

Our sample consists of a relatively long birth window (i.e., 41 years) and a relatively limited death window (i.e., 31 years). One concern in our sample selection is that our method of comparison between early versus late filtration adoption could reflect the longevity differences between earlier cohorts versus later cohorts. This problem is more evident given the sharp rises in life expectancy at birth for these cohorts (Smith & Bradshaw, 2006). To mitigate this issue, we restrict cohorts to those born 15 years before and after the city-specific year of water filtration.[9]

The final sample includes 338,758 observations born between 1900-1940 and who died between 1975-2005. Summary statistics of the final sample are reported in Table 1. The average

[7] To the extent that migration is correlated with childhood exposure to water filtration, measurement errors induced by migration in city of birth/childhood assignment may confound our estimates. In Appendix H, we argue that although between 20-40% of the linked samples (from the full count 1940 census to 1910-1930 census) moved across cities, such migration patterns do not correlate with exposure to water filtration after accounting for fixed effects and covariates.
[8] Appendix K discusses cross-census linking procedure and steps of sample construction in more detail.
[9] In Appendix E, we argue that this selection is not critical for the main findings. Removing this restriction or making a stricter balancing window of selection around water filtration reforms only slightly changes the coefficient size.

age-at-death is 867.2 months or 72.3 years but it varies between 35-104 years.[10] Roughly 98 percent of the observations are white. This overrepresentation of whites in the final sample results from a higher match rate among whites both in the DMF-1940-census match and in linking historical censuses. In section 5.3, we empirically test whether the observed match rules are endogenous, i.e., they are correlated with city-level water filtration. Moreover, Breen & Osborne (2022) argue that while certain groups are underrepresented in the Censoc-linked death records, they represent their original population of 1940 records in terms of socioeconomic and education. About 90 percent of mothers and fathers are literate. The parental information is also extracted from the earliest census each individual appears. Therefore, they reveal parental covariates during individuals' birth/childhood. Roughly 95 percent of fathers are active in the labor force at the time of the 1940 census enumeration. Our primary independent variable is the share of childhood years (ages 0-15) that the individual is exposed to water filtration. The average share of exposure is 0.16 with a standard deviation of 0.36.

4. Econometric Method

To operationalize the long-run effects of water filtration on mortality, we implement a difference-in-difference framework to compare the age-at-death of individuals who were exposed to the adoption of the water filtration system to those in cities without a filtration system across

[10] We do not restrict the sample based on age at death. One concern is that individuals who were born earlier in the sample and were exposed to earlier filtration reforms must have lived into much older ages to be in the final sample than those later cohorts. Moreover, the longevity of earlier cohorts for inclusion in the final sample is beyond the life expectancy of cohorts in the early 20th century. Therefore, these cohorts could possibly contain quiet different characteristics than the later cohorts to have lived beyond life expectancy of their cohorts. Such differential longevity of earlier versus later cohorts might bring causes of concern related to endogenous survival. In Appendix F, we restrict the sample to individuals survived up to ages 50, 55, 60, 65, and 70 and replicate the main results. We observe coefficients that are about 30% smaller than the main results. This fact implies two scenarios. One, the benefits of filtration appear to be larger for younger ages at death and that expanding death window to cover deaths prior to 1975 might increase coefficient sizes. Second, possible survival selection of earlier cohorts might bias coefficients downward and that the main results underestimate the true effects.

different ages during their childhood. In other words, we examine the impacts across different ages at exposure. Specifically, we implement regressions of the following forms:

$$y_{icrb} = \alpha_0 + \sum_{j \neq [-15,-13]} \beta_j I(b^* - b = j) + \alpha_2 X_i + \alpha_3 Z_{cb} + \zeta_c + \xi_{br} + \varepsilon_{icrb} \tag{1}$$

Where y is age-at-death (in months) of individual i who was born in city c in census region r and belonged to birth-year b. $I(.)$ is a unit function that equals one if the inside argument is true. b^* represents city-specific year of water filtration. Therefore, the parameters β_j represent impacts across various age-at-exposure j. For instance, β_{-5} is the coefficient of exposure for cohorts who turn age 5 at the time of water filtration. Similarly, β_5 is the coefficient of exposure of cohorts who are born 5 years post-waterwork, hence a full in-utero and childhood exposure. We eliminate the coefficient of 13-15-year-old individuals (i.e., j=[-15,-13]) to compare all coefficients to the values of the oldest cohorts in our sample.

In matrix X, we include individual and family covariates, including indicators of race, ethnicity, maternal literacy, paternal literacy, and paternal occupational income score. In Z, we include city-level controls including share of married, labor force participation rate, share of people in different occupations, share of homeowners, share of children, and average socioeconomic score. The parameter ζ represents city fixed effects that account for time-invariant city-level confounders in longevity. Therefore, we rely on cross-cohort differences post-versus-pre water filtration to eliminate the unobserved heterogeneity across cities. The parameter ξ represents birth-cohort by birth-region fixed effects to absorb unobserved temporal heterogeneity across cohorts that are specific to each census region. Finally, ε is a disturbance term. We cluster standard errors at the city level. Since the final sample includes only 25 cities, we have very few clusters to rely on inference based on city-level clustering. Therefore, in visual representations of equation 1, we illustrate confidence intervals extracted from the wild bootstrap procedure.

We further examine the impacts in a difference-in-difference framework and assign the exposure measure based on the share of childhood years (up to age 15) that an individual was exposed to water filtration. Specifically, we implement regressions of the following forms:

$$y_{icrb} = \alpha_0 + \alpha_1 WF_{cb} + \alpha_2 X_i + \alpha_3 Z_{cb} + \zeta_c + \xi_{br} + \varepsilon_{icrb} \tag{2}$$

In this formulation, the variable WF is the share of childhood between ages 0-15 that the individual was exposed to water filtration. For instance, Baltimore, MD initiated water filtration in 1915. An individual born in 1910 is potentially exposed to water filtration for ages 6-onward. Hence, the average share of exposure is 0.6.[11] Therefore, α_1 is the parameter of interest that captures the association between full exposure to water filtration (versus no exposure) and old-age longevity. All other parameters are similar to those of equation 1. In all regressions, we report P-values based on wild bootstrap procedures.

5. Results

5.1. Age-at-Exposure Analysis

The main results of equation 1 are reported in the top panel of Figure 2. Compared to cohorts 13-15 years old at the time of water filtration, we observe very small changes for individuals 11-12 years old. For cohorts between ages 5-10, we observe a positive average effect that is small in magnitude. The effects start to rise for those 3-4 years old and younger. Four out

[11] Recall that we limit the sample to those born 15 years pre- and post-waterwork. In Appendix B, we show the results for other threshold ages. Specifically, we assign exposure measure up to age 1, 5, 10, and 14. Since all these age groups are treated in the final sample, as we limit exposure ages, we expect to observe smaller coefficients as those ages join the reference group.

of seven coefficients for the age group of 3-4-and-younger are statistically significant at the 5 percent.[12]

Difference-in-Difference Bias. The literature suggests that OLS-produced difference-in-difference estimates in a staggered adoption setting where different units receive treatments in different periods could produce biased estimates (Borusyak et al., 2021; Callaway & Sant'Anna, 2021; Goodman-Bacon, 2021; Sun & Abraham, 2021). To explore this potential bias, we implement an alternative difference-in-difference method developed by Sun & Abraham (2021) and replicate the regressions of equation 1. The results are depicted in the bottom panel of Figure 2. Compared with the OLS estimations of the top panel, we observe a very similar pattern in estimated effects, suggesting little bias in the OLS estimates.

5.2. Main Results

The main results of equation 2 for the cumulative childhood exposure are reported in Table 2. We start with a parsimonious model that only includes city fixed effects, birth-year fixed effects, and individual and family controls (column 1). Then, we add city controls in column 2 and birth-region-by-birth-year fixed effects in column 3. The estimated effects are quite stable across specifications. Based on the fully parametrized specification of column 3, exposure to filtered water throughout childhood is associated with about 3.2 months higher longevity.

This finding is in line with a narrow literature that shows removing contaminants in drinking water, e.g., arsenic, during in-utero and early-life could reduce mortality in young adults

[12] In Appendix I, we group different ages at exposure to observe and compare coefficient sizes across different ages. Specifically, we examine exposure during in utero and age 0, ages 1-4, and ages 5-9. We find a monotonic pattern: the earlier the exposure, the larger the coefficient. For instance, for in utero and early-life, exposure is associated with 3.3 months higher longevity while for exposure during ages 1-4 the coefficient implies a 2.6-month rise in longevity.

(Smith et al., 2006, 2012). This is also consistent with studies that suggest improvements in early-life water quality increase later-life human capital (Beach et al., 2016; Zhang & Xu, 2016).

We can compare this effect with the impacts of other early-life exposures to gauge its economic magnitude. Aizer et al. (2016) examine the impacts of the Mothers' Pension (MP) program, a cash transfer needs-based program to help poor families, on later-life education and longevity. They find that MP receipt during childhood is associated with 0.6 years of more schooling and almost 1 year of additional longevity. MP transfers accounted for about 12-25 percent of family income and lasted for three years, resulting in a cumulated income shock of about 50 percent of family income. Therefore, the intent-to-treat effect of Table 2 is equivalent to an increase of 13.3 percent in family income. Halpern-Manners et al. (2020) employ twin fixed effect strategy and explore the effect of education on longevity. They find that an increase of 1 year of schooling is associated with about 4 months higher longevity. Therefore, our estimated effect is equivalent to about 0.8 additional years of schooling. Noghanibehambari & Fletcher (2023b) explore the impact of in-utero exposure to state-level and federal alcohol prohibition during the early decades of the 20th century on old-age longevity. They find that prohibition is associated with a treatment-on-treated increase of 1.7 years in longevity. The estimated impact of Table 2 suggests that unfiltered water has at least about 16 percent of the effect of alcohol consumption during pregnancy. Fletcher & Noghanibehambari (2024) examine the in-utero and early-life exposure to agrichemical pesticide exposure on old-age longevity. They use the emergence of cyclical cicadas in eastern states that raises the pesticide use in tree croplands as a natural experiment. They employ DMF data and show that exposed cohorts reveal 2.2 months lower longevity. They argue that contaminating drinking water is a likely channel of exposure of infants and mothers to pesticide use. Their estimated effect is about 69 percent of the benefit of water filtration in the current study.

5.3. Endogeneity Concerns

This section discusses several potential sources of endogeneity and selection concerns. We list these concerns below and attempt to empirically test them using available data.

Balancing Tests. One concern is that the final sample is unbalanced and represents certain sociodemographic populations more than others. If this over/under-representation is correlated with water filtration even after controlling for fixed effects and covariates, then the regressions produce biased estimates. For instance, assume that following the public health improvements that resulted in water filtration, cities observe a sharp inflow of migrants from neighboring cities and counties. Also, assume that there are more whites and people of better socioeconomic conditions among these migrants. In this case, the regressions overstate the true effects and capture the higher longevity of these subpopulations of migrants rather than the effect of water filtration. Another source of an unbalanced sample is differential mortality and survival into adulthood and old age. If childhood and middle age mortality is affected by early-life water quality and the effects vary by sociodemographic characteristics, then the observed effects on longevity could reflect the differential mortality patterns of survivors rather than the direct impact of water quality. We can directly examine these sources of endogeneity by implementing a series of balancing tests. In so doing, we use individual and family characteristics as the outcomes and implement regressions of equation 1, conditional on city and region-by-cohort fixed effects. We then depict the coefficients dummies for each outcome in different panels of Figure 3 through Figure 5. To facilitate comparison across panels and figures, we standardize each outcome with respect to the mean and standard deviation of the sample. We do not observe a discernible effect on the likelihood of being white, black, or Hispanic across ages of exposure to the water filtration reforms (top-left, top-right, and bottom-left panels of Figure 3). The coefficients are economically small and statistically insignificant at 95 percent level.

However, we observe negative and significant coefficients for the outcome of mother literate for younger children (bottom-right panel of Figure 3). Since mother education is shown to have a positive impact on infants' and children's health, these reductions suggest that our results may underestimate the true effects (Huebener, 2019, 2020; Lundborg et al., 2014; Noghanibehambari et al., 2022).

We do not observe any discernible change across ages following the reform for mother labor force status, father literacy, and missing indicators of parental literacy (Figure 4). We also observe no cross-age trend in the father's socioeconomic and occupational income scores (top-right and bottom-left panels of Figure 5, respectively).[13]

A similar concern relates to the fact that water filtration may be preceded by other state-level policy changes or city-level socioeconomic and sociodemographic changes. Thus, to the extent that such place-specific policy evolution and compositional changes correlate with longevity, the impacts might pick up on these confounders rather than water filtration. In Appendix J, we implement event studies to examine the evolution of state-level and city-level outcomes in different years relative to water filtration years, conditional on city and region-by-year fixed effects. The evidence does not provide empirical support for this concern. Specifically, we do not observe any association between the water filtration and prohibition movement, suffrage

[13] We further examine potential changes in fertility following water filtration. These results are discussed in Appendix L. Although consistent with prior research in this area we find reductions in infant mortality rates, we do not observe significant changes in birth rate (Anderson, Charles, & Rees, 2022; Cutler & Miller, 2005).

movement, tax policies, birth registration laws, child labor laws, compulsory attendance laws, sociodemographic composition, and a battery of socioeconomic outcomes.[14]

Similarly, one might argue that water filtration projects are one step out a larger set of staggered piecemeal development plans that expand the general provision of public goods. In that case, the effects pick up on the benefits of other public projects and social spending. In Appendix C, we empirically investigate such concerns, and, through a series of event studies, show that water filtration projects do not correlate with spending on public education, per capita doctors as a measure of healthcare access, water chlorination projects, and several other public health intervention projects.

Endogenous Merging with Censuses. Selection from the original population to the final sample caused by data linking may generate bias in our estimations if the selection procedure is correlated with water filtration projects (see section 3 and Appendix K). To examine this source of selection-induced endogeneity, we assess the association between successful survival from the original population to the final sample with the water filtration exposure measure. In so doing, we start with the universe of cohorts born between 1900-1940 and who reside in the final sample's cities. We merge this population with those in the final sample and generate a successful merger dummy if the merging is successful. We then implement regressions that include city and cohort-by-region fixed effects, similar to equation 2. The results are reported in Table 3. We show the estimated associations between successful merger and water filtration for the full sample, sample of whites, and sample of nonwhites in columns 1-3, respectively. The coefficients suggest insignificant associations. Moreover, the magnitude of the effects is small. For instance, exposure

[14] The policies mentioned here and explored in Appendix J are documented to influence later-life mortality and longevity (Lleras-Muney, 2005; Noghanibehambari & Fletcher, 2023a, 2023b; Noghanibehambari & Noghani, 2023).

to water filtration is associated with an insignificant 2.6 basis-points increase in the probability of merging, equivalent to about a 0.8 percent change from the outcome mean. These results do not provide evidence for the endogeneity caused by cross-census and DMF-census linking and selection.

5.4. Robustness Checks

In Table 4, we explore the sensitivity of the results to alternative model specifications. Column 1 replicates the results of column 3 of Table 2 to provide a benchmark comparison. All other columns include all covariates and fixed effects used in column 1. In column 2, we interact birth-state by 1940-state fixed effects to control for the influence of early- adulthood cross-state migration on the water-longevity relationship. The estimated effect is quite similar to the main results.

In columns 3-4, we interact city fixed effects with individual and family dummies. Thus, we allow for time-invariant unobserved factors of each city to have a differential impact on health and longevity across people of different sociodemographic and socioeconomic backgrounds. The estimated effects are comparable to that of column 1.

Studies suggest that the season of birth is associated with infants' health and later-life mortality (Currie & Schwandt, 2013; Doblhammer & Vaupel, 2001). Moreover, several causes of death reveal a seasonality pattern (Falagas et al., 2009; Seretakis et al., 1997). To account for seasonality-related confounders, we add birth-month and death-month fixed effects to the regressions. The result, reported in column 5, is almost identical to that of column 1.

In column 6, we add a wide range of additional city-level controls, including share of people in different demographic groups, share of people in different age groups, average socioeconomic score, female labor force participation rate, male labor force participation rate, female literacy rate, male literacy rate, and population. The estimated effect becomes only slightly smaller than the main results and remains statistically significant.

In column 7, we implement an alternative standard error correction method. Instead of clustering, we use Huber-White robust standard error. The estimated effect remains statistically significant at 95% level.

Another concern is regarding the functional form of the regressions. In column 8, we replace the outcome with the log of age-at-death, hence estimating a semi-log specification. We observe an effect of a 0.38 percent rise in longevity. This is very similar to the 0.37 percent rise with respect to the outcome mean, implied by column 1. In column 9, we replace the outcome with a dummy variable that equals one if the individual's age-at-death is more than 75 years. Early-life water filtration is associated with a 1.5 percentage-point rise in the probability of living beyond 75 years, off a mean of 0.38.

The main regressions of the paper do not incorporate any weighting method. In column 10, we assign higher weights to more populated cities by weighting the regressions using the average city population. The estimated effect increases by roughly 19% and remains statistically significant.

Several studies suggest the long-term effects of early-life exposure to the Spanish Flu of 1918-1919 (Almond & Mazumder, 2005; Cook et al., 2019; Fletcher, 2018a, 2018b; Myrskylä et al., 2013). In column 11, we remove cohorts born between 1918-1919 who were likely affected by the pandemic. The estimated effect is quite similar to column 1.

The Great Depression induced unprecedented economic hardship among families that could affect the children's long-term outcomes (Cutler et al., 2007; Noghanibehambari et al., 2024; Van Den Berg et al., 2006, 2009). Moreover, studies point to the benefits of New Deal relief programs during this period for later-life outcomes (Noghanibehambari & Engelman, 2022). Both economic conditions and social spending could confound our estimates if they are correlated with water filtration exposure. In column 12, we remove cohorts born between 1930-1940 who were probably impacted by the Great Depression and New Deal social spending. The estimated effect is quite similar to that of column 1.

6. Mechanisms

Improvements in health accumulation during infancy and childhood could lead to higher longevity through several mediatory channels, including better human capital accumulation, better mental health, better physical health, lower obesity, higher probability of family formation, better spousal attributes, and higher socioeconomic index (Benítez-Silva & Ni, 2008; Cutler et al., 2006; Diener & Chan, 2011; Gardner & Oswald, 2004; Lleras-Muney et al., 2022; Lleras-Muney & Moreau, 2022; Noghanibehambari & Fletcher, 2023b, 2023c, 2023d; Preston, 2005; Van Den Berg et al., 2015). In this section, we examine two important channels, education and socioeconomic measures during adulthood. Since many cohorts have not yet completed their education in the 1940 census or entered the labor force, we turn to decennial censuses 1950-1970. We use information about the city of residence in these censuses to proxy for city-of-birth. We restrict the sample to male individuals born between 1900-1940 aged 25-55 and merge the data by water filtration database based on census-city and year-of-birth. We assess the association between water filtration and education/socioeconomic outcomes by implementing regressions that include individual covariates, census year fixed effects, birth-year-by-birth-region fixed effects, and city fixed effects. The results are reported in Table 5. Early-life and childhood water exposure are associated with roughly 3.5 additional months of schooling (column 1). This effect is similar to the OLS findings of Beach et al. (2016), who examine the effect of water purification in early-life on later-life education. Water filtration also leads to about 3.7 percentage-points reductions in the likelihood of less than high school education, off a mean of 0.16 (column 2).

Water quality exposure in early-life is also linked with socioeconomic measures. Exposure to water filtration during childhood results in a 1.8-unit increase in the socioeconomic index, equivalent to a 4.8 percent rise from the mean of the outcomes (column 4). We also observe positive impacts on family income although the point estimates are noisy and limit interpretation (columns 5-6).

If improvements in human capital and measures of socioeconomic status are mechanism channels, then one would expect that these pathways follow similar heterogeneous variations as those of longevity. In Appendix L, we provide evidence of significant and sizable reductions in infant mortality following water filtration. If such improvements are the results of water filtration and improvements in initial health capital, we may observe larger impacts on longevity outcomes in areas with higher initial infant mortality rates. In Appendix G, we examine this source of heterogeneity and show that the effects are considerably larger for the subsample of high infant mortality rate cities. Moreover, we show that the effects on schooling and socioeconomic index are primarily driven by the high infant mortality subsample, supporting the role of human capital as mechanism channels between exposure to water filtration and later life mortality.[15]

[15] If the population of infants that survived as a result of water filtration is weaker (who would have died for their weakness of other reasons in the absence of water filtration), then the overall benefits on longevity underestimate the true effects. On the other hand, if water filtration brings health benefits and the results illustrate the improvements in infants' health capital, then the observed impacts on infant mortality are indeed the primary mechanism channel. We can do a back-of-an-envelope calculation to examine this. Using Social Security Administration cohort life tables, we estimate that the difference between post-infancy life expectancy and life expectancy at birth increased by about 4.5 years between the years 1900-1940 (SSA, 2020). Based on aggregate vital statistics death records, infant mortality rates decreased from around 150 to 47 infant deaths per 100K births (CDC, 2015). The results of Appendix Table L-1 implies a reduction of about 6 infants per 100K. Assuming that the difference in life expectancy at age 1 and 0 can be solely attributed to reductions in infant mortality, the reduction of 6 infants per 100K imply roughly 3 months increases in life expectancy, a number that is quite similar to our main results.

The next question is to what extent improvements in these outcomes can explain the link between water filtration and longevity. Chetty et al. (2016) examine the association between household income and individual longevity in the US between 2001-2014 using individual tax returns linked to the mortality database. They find that for each 5-percentile increase in income, longevity increases by 0.7-0.9 years. For a household in the median of the sample, this means an increase of about $40K (in 2020 dollars). Therefore, an increase of $2,603 (induced by water filtration, column 5 of Table 5) is associated with about 0.62 months higher longevity. This is about 20 percent of the reduced-form effect of Table 2.

Halpern-Manners et al. (2020) and Cutler & Lleras-Muney (2006) estimate that an increase of 1 year in schooling is associated with 0.34 and 0.6 years higher longevity. Combining these estimates with the estimated effect of column 1 of Table 5, one can deduce that the water-filtration-induced rise in schooling leads to 1.2-2.1 months higher longevity. These effects are equivalent to about 37-65 percent of the effect of column 3 of Table 2. Therefore, improvements in education/income can explain about 20-65 percent of the observed long-term links.

To complement the mechanism channel analysis, we employ World War II enlistment data linked with the 1940 census and DMF, extracted from Goldstein et al. (2021). This data is a subset of DMF data in our main analysis for individuals who were enlisted for World War II. We explore the effects on two measures of human capital and health capital. First, we focus on the Army General Classification Test (AGCT) score. The AGCT was designed to capture the learning and intellectual abilities of enlistees during World War II in order to assign them to different military tasks and jobs (Potter et al., 2008). Second, we explore the effects on height as reported by enlistment enumerators. Height is an indicator of general health and is correlated with other economic and health outcomes (Bozzoli et al., 2009; Deaton, 2007; Deaton & Arora, 2009).

Specifically, some studies link height to old-age health and longevity (Jousilahti et al., 2000; Spijker et al., 2012; Wilson, 2019). We implement the same sample construction and empirical method as the main results. The results are reported in columns 7-8 of Table 5. We find a positive link between childhood exposure to water filtration and AGCT score as well as height. Full exposure to water filtration during childhood is associated with a 1.5% higher AGCT score and 1.7% increase in height. The estimated coefficients are statistically significant at 10% level.

7. Conclusion

In the early 20th century, state and local authorities initiated a series of improvements in public health infrastructure, including drinking water filtration and purification. In later decades, many state and federal laws, including the Safe Drinking Water Act, Water Pollution Control Act, and Clean Water Act, attempted to elevate drinking water quality further. Although there have been substantial improvements in the quality of drinking water, there remain communities with access to unsafe water (Mueller & Gasteyer, 2021). There are also instances of temporary water pollution with considerable negative health consequences (Grossman & Slusky, 2019; Jones, 2019). The situation is far worse in the rest of the world, specifically in poorer countries. About 1 in 4 people lack safely managed drinking water at home (WHO, 2019). According to UNICEF estimates, billions of people will lose access to safely managed drinking water by 2030 (UNICEF, 2021). Therefore, it is important and policy-relevant to document the short-run and long-run effects of water quality on human health outcomes.

This paper explored the long-run effects of in-utero and early-life exposure to water filtration on old-age longevity. We exploited city-wide public health efforts to initiate a water filtering system to purify water across 25 major American cities in the early 20th century. Our results suggested a benefit of 3.2 months of additional longevity. We implemented a wide array of tests to argue against endogeneity issues. We provided empirical evidence that changes in sociodemographic and socioeconomic characteristics do not confound the estimates. We found no evidence that these public health interventions coincide with any other city, county, or state-level policy changes. Finally, we showed that the results are robust to a wide array of specification checks, subsamples, and functional form checks.

Life expectancy at birth among male Americans increased from 46.3 to 60.8 years between 1900-1940, an increase of roughly 174 months of additional longevity. Based on our estimated effect of Table 2, exposure to cleaner water as a result of water filtration can account for about 2 percent of the overall improvements in longevity for cohorts born between 1900-1940.

In our final sample, about 15.6% of observations are fully exposed to water filtration during their childhood. Using the intent-to-treat estimate of the main results combined with the number of fully exposed individuals in the final sample, one can calculate 14.1 thousand life years gained due to childhood exposure to improvements in water quality as a result of water filtration. Further, we can monetize this number using the Value of Statistical Life (VSL) estimates. Studies suggest a VSL of about $10 million for the case of the United States (Kniesner & Viscusi, 2019; Viscusi, 2018). Given the average longevity in the final sample of 72.3 years, one can roughly calculate an annual VSL of $138.3 thousand. Therefore, the overall improvement in longevity of the exposed cohorts in the final sample due to childhood exposure to water filtration is equivalent to roughly $2 billion.

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Tables

Table 1 - Summary Statistics

Variable	Mean	Std. Dev.	Min	Max
Death Age (Months)	867.23843	123.45438	420	1249
Birth Year	1919.4808	8.43865	1900	1940
Death Year	1991.749	8.61932	1975	2005
White	.97538	.15496	0	1
Black	.02462	.15496	0	1
Hispanic	.00801	.08916	0	1
Mother Literate	.90916	.28739	0	1
Mother Literacy Missing	.01712	.12972	0	1
Mother in Labor Force	.05883	.2353	0	1
Father Literate	.90071	.29905	0	1
Father Literacy Missing	.03745	.18987	0	1
Father in Labor Force	.94566	.22668	0	1
Father Occupational Income Score	28.4077	9.17444	3	80
Father Occupational Income Score Missing	.08461	.2783	0	1
Water Filtration	.156	.36285	0	1
Chlorination of Water	.7532	.4226	0	1
Bacteriological Standard for Milk	.76789	.41507	0	1
Sewage Treatment or Diversion	.33376	.46858	0	1
Observations		338,758		
Census 1960-1980 Data:				
Year of Schooling	10.6939	3.68437	0	18
Education less than High School	.11858	.32329	0	1
Education < 12	.50174	.5	0	1
Socioeconomic Index	38.28234	23.37394	3	96
Total Family Income	72303.89	45351.124	43.71807	874361.5
Ln Total Family Income	11.00141	.66843	3.77776	13.68125
Year of birth	1920.4879	10.86928	1900	1940
White	.83326	.37275	0	1
Black	.16362	.36993	0	1
Hispanic	.00913	.09513	0	1
Observations		362,167		
WWII Enlistment Data:				
Army General Classification Test (AGCT) score	140.51797	12.91901	2	160
Log AGCT	4.94016	.11284	.69315	5.07517
Height	2.72245	.41421	.03448	4.5
Log Height	.98825	.17199	-3.3673	1.50408
Nonwhite	.01989	.13961	0	1
Observations		33,139		
Birth/Death Data:				
Infant Mortality Rate per 100,000	66.34418	21.64263	28.0595	148.97972
Log Infant Mortality Rate per	4.14187	.3283	3.33433	5.00381
Birth per 1,000 Women	37.87557	8.23834	22.90661	69.28323
Log Birth Rate	3.63908	.20451	3.17415	4.25253
Observations		559		

Table 2 - Main Results: Childhood Exposure to Water Filtration and Later Life Longevity

	Outcomes: Age at Death (Months)		
	(1)	(2)	(3)
Exposure to Water Filtration	3.68182**	3.23385**	3.24338***
	(1.76639)	(1.51732)	(1.11798)
Observations	338758	338758	338742
R-squared	.35563	.35567	.35612
Mean DV	867.238	867.238	867.242
P-Value	0.075	0.057	0.031
City FE	✓	✓	✓
Birth Year FE	✓	✓	✓
Individual Controls	✓	✓	✓
Family Controls	✓	✓	✓
City Controls		✓	✓
Region-by-Cohort FE			✓

Notes. Standard errors, clustered on city, are in parentheses. P-values are extracted from the wild bootstrap procedure with city-level clustering. Individual controls include dummies for race and ethnicity. Family controls include maternal literacy dummy, paternal literacy dummy, maternal labor force status dummy, paternal labor force status dummy, paternal socioeconomic score dummies, and a series of missing indicators for missing values of each variable. City controls include average share of homeowners, average occupational income score, share of white-collar occupation, share of farmers, share of other occupation, literacy rate, and share of married.

*** p<0.01, ** p<0.05, * p<0.1

Table 3 - Exploring Endogenous Merging

	Outcome: Successful Merging between the Final Sample and the Original Population in 1940		
	Full Sample	Whites	Nonwhites
	(1)	(2)	(3)
Exposure to Water Filtration	-.00026 (.00779)	.00016 (.00783)	.00037 (.00821)
Observations	7218487	6844592	373895
R-squared	.00571	.00587	.0045
Mean DV	0.033	0.034	0.015
P-Value	0.978	0.987	0.966

Notes. Standard errors, clustered on city, are in parentheses. P-values are extracted from the wild bootstrap procedure with city-level clustering. Regressions include city and region-by-year fixed effects. Regressions also include city covariates. City controls include average share of homeowners, average occupational income score, share of white-collar occupation, share of farmers, share of other occupation, literacy rate, and share of married.
*** p<0.01, ** p<0.05, * p<0.1

Table 4 - Robustness Checks

	Outcome: Age at Death (Months)					
	Column 3 of Table 2	Adding Birth-State by 1940-State FE	Adding City-by-Individual Covariates Interaction	Adding City-by-Parental Covariates Interaction	Adding Birth-Month and Death-Month FE	Adding more City Controls
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure to Water Filtration	3.24338*** (1.11805)	3.23791*** (1.14113)	3.20823*** (1.09942)	3.62473*** (1.25307)	3.256*** (1.10718)	2.90648** (1.31927)
Observations	338758	338758	338758	310097	338758	306759
R-squared	.35616	.35626	.35625	.35655	.35687	.22901
Mean DV	867.238	867.238	867.238	867.170	867.238	883.287
P-Value	0.024	0.033	0.028	0.027	0.013	0.086
	Using Heteroscedasticity Robust SE	Outcome: Log Age at Death	Outcome: Age at Death>75	Weighted by City Population	Dropping Cohorts of 1918-1919	Dropping Cohorts of 1930-1940
	(7)	(8)	(9)	(10)	(11)	(12)
Exposure to Water Filtration	3.24338** (1.55943)	.00385*** (.00123)	.01469** (.00692)	3.85804*** (.75699)	3.46324*** (1.06882)	3.10969** (1.19686)
Observations	338758	338758	338758	338758	307954	306185
R-squared	.35616	.36286	.18441	.38912	.37738	.2243
Mean DV	867.238	6.754	0.384	848.537	865.463	884.483
P-Value	0.038	0.019	0.056	0.006	0.013	0.048

Notes. Standard errors, clustered at the city level (except column 7), are in parentheses. P-values are extracted from the wild bootstrap procedure with city-level clustering. All regressions include city and birth-region-by-birth-year fixed effects. Individual controls include dummies for race and ethnicity. Family controls include maternal literacy dummy, paternal literacy dummy, maternal labor force status dummy, paternal labor force status dummy, paternal socioeconomic score dummies, and a series of missing indicators for missing values of each variable. City controls include average share of homeowners, average occupational income score, share of white-collar occupation, share of farmers, share of other occupation, literacy rate, and share of married.
*** p<0.01, ** p<0.05, * p<0.1

Table 5 - Exploring Mechanism Channels

	Outcomes and Samples:							
	1950-70 Census Data						**DMF-Enlistment Data**	
	Years of Schooling	Education < High School	Education ≤ 12 years	Socioeconomic Score	Total Family Income	Log Total Family Income	Log AGCT	Log Height
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exposure to Water Filtration	.2994***	-.03748***	-.02666	1.84425***	2603.0444	.03708	.01499*	.01682**
	(.10478)	(.01347)	(.01897)	(.65165)	(1580.3918)	(.02363)	(.00845)	(.00802)
Observations	245879	245879	245879	234773	235625	233920	33139	33139
R-squared	.27169	.20091	.11518	.12162	.09819	.1218	.02165	.85618
Mean DV	10.411	0.160	0.560	38.037	69308.464	10.974	4.940	0.988
P-Value	0.011	0.047	0.194	0.007	0.152	0.137	0.178	0.159

Notes. Standard errors, clustered on city, are in parentheses. P-values are extracted from the wild bootstrap procedure with city-level clustering. All regressions include city fixed effects, birth-region-by-birth-year fixed effects, and city covariates. Regressions of columns 1-6 also include census year fixed effects. Individual controls include dummies for race and ethnicity. City controls include average share of homeowners, average occupational income score, share of white-collar occupation, share of farmers, share of other occupation, literacy rate, and share of married.

*** p<0.01, ** p<0.05, * p<0.1

Figures

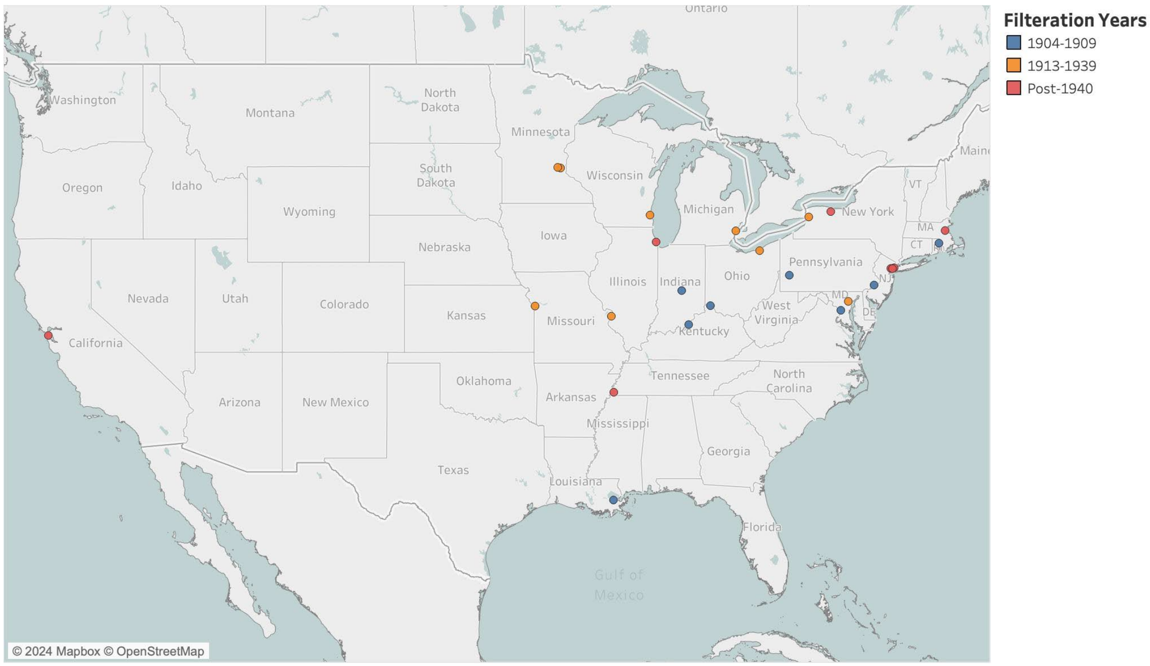


Figure 1 - Water Filtration Year across Cities in the Final Sample

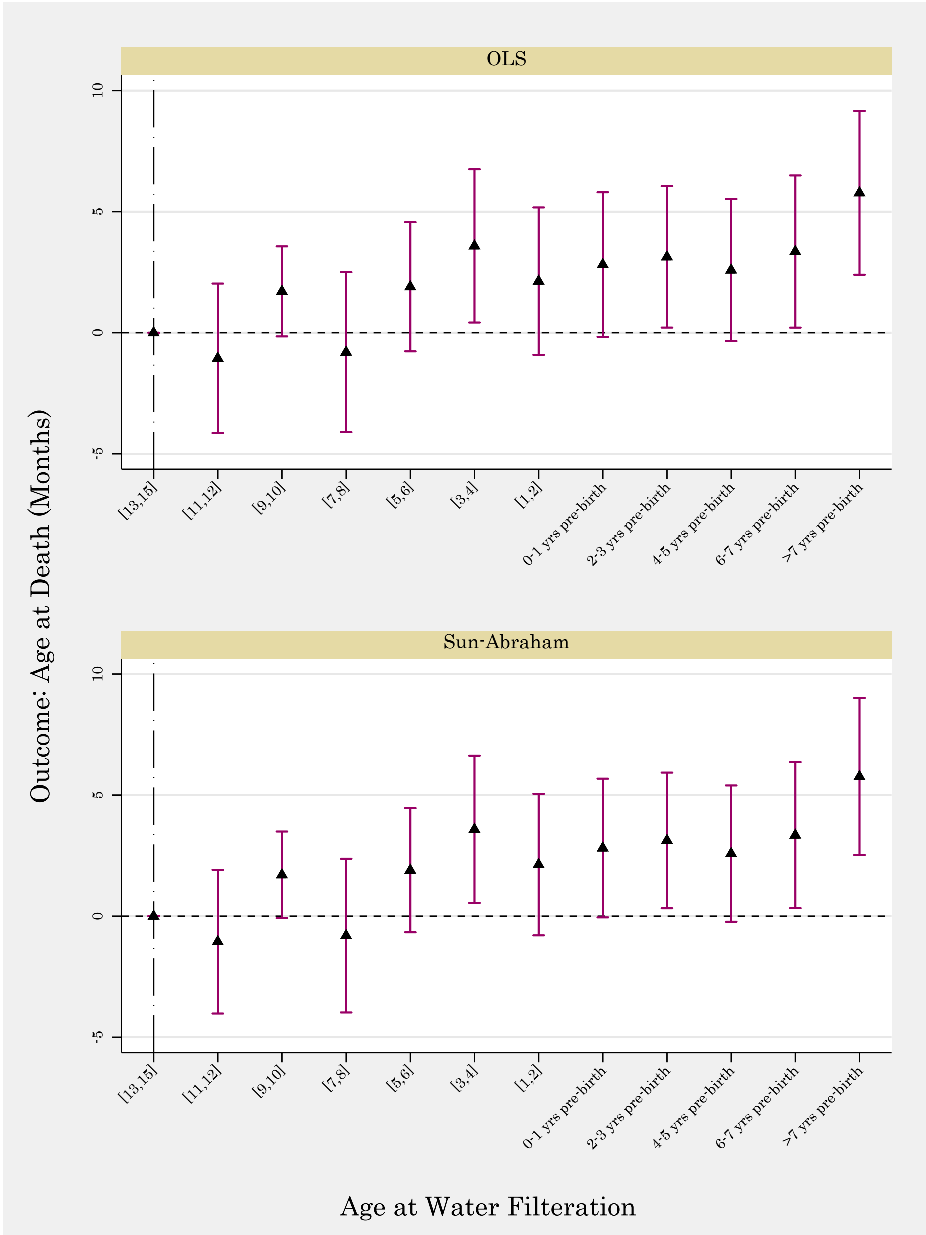


Figure 2 - Exposure To Water Filtration across Different Ages and Later-Life Longevity

Notes. Point estimates and 95 percent confidence intervals, extracted from wild bootstrap procedure with city-level clustering, are reported. All regressions include city and birth-region-by-birth-year fixed effects. Individual controls include dummies for race and ethnicity. Family controls include maternal literacy dummy, paternal literacy dummy, maternal labor force status dummy, paternal labor force status dummy, paternal socioeconomic score dummies, and a series of missing indicator for missing values of each variable. City controls include average share of homeowners, average occupational income score, share of white-collar occupation, share of farmers, share of other occupation, literacy rate, and share of married.

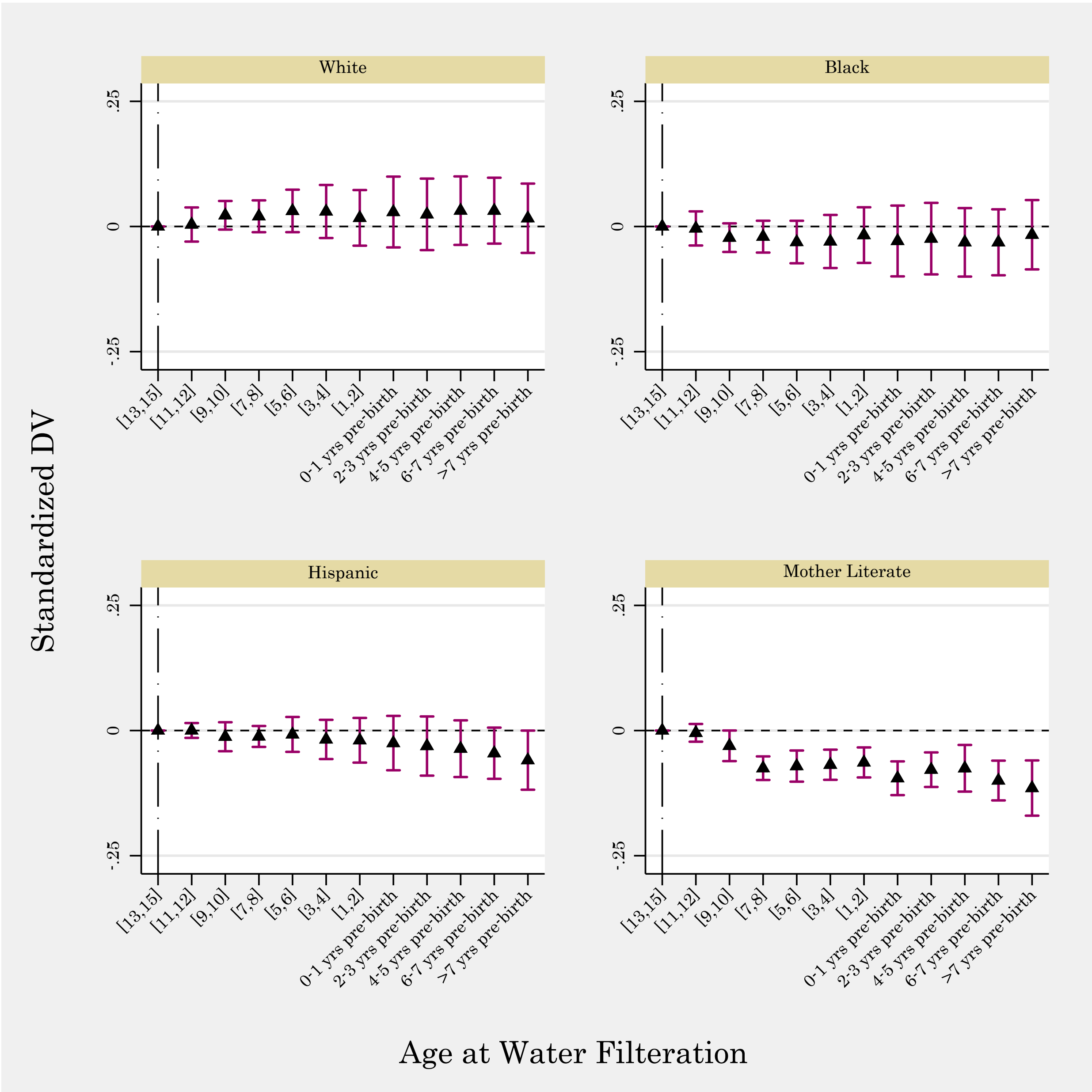


Figure 3 - Exposure To Water Filtration across Different Ages and Observable Individual/Family Characteristics

Notes. Point estimates and 95 percent confidence intervals, extracted from wild bootstrap procedure with city-level clustering, are reported. All regressions include city and birth-region-by-birth-year fixed effects.

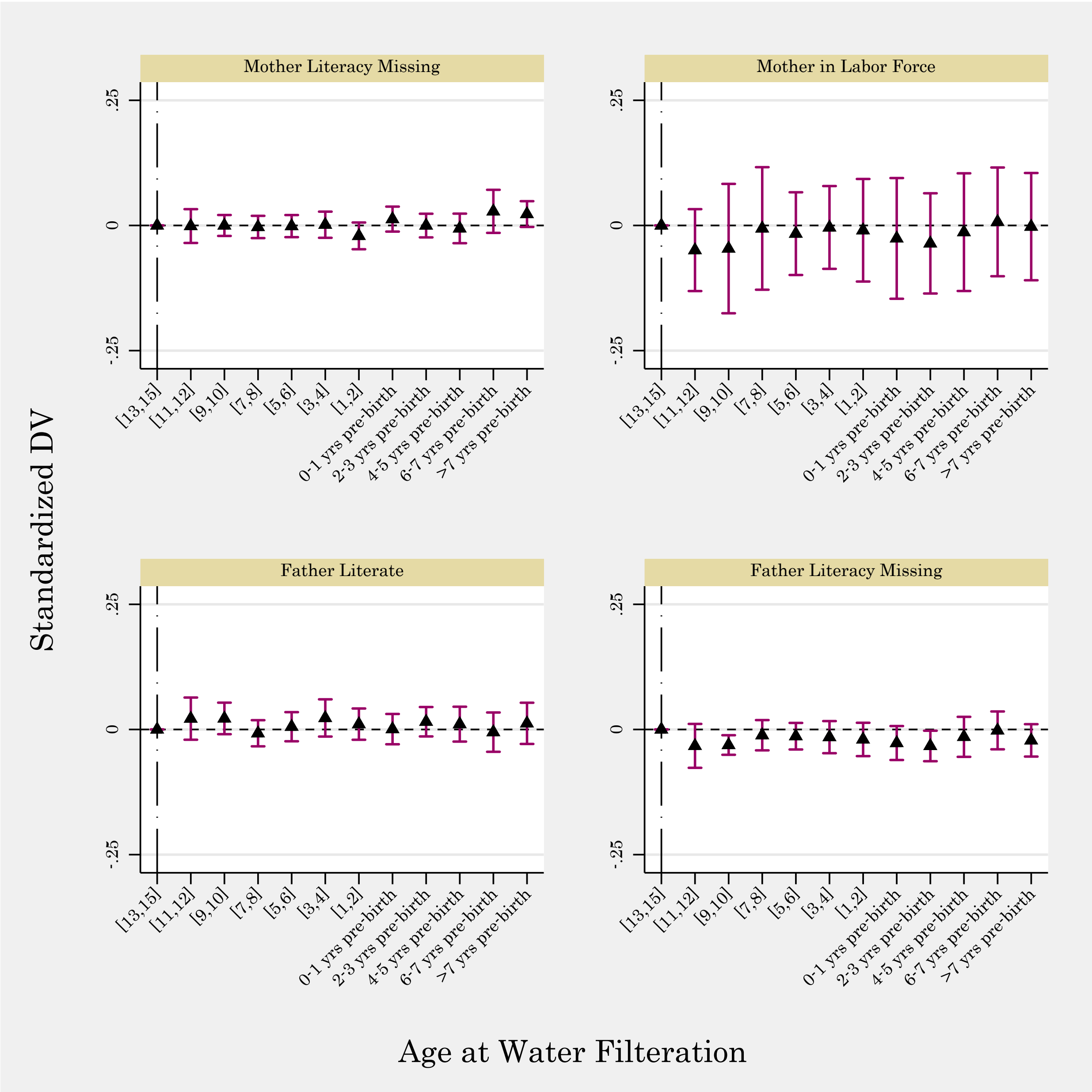


Figure 4 - Exposure To Water Filtration across Different Ages and Observable Individual/Family Characteristics

Notes. Point estimates and 95 percent confidence intervals, extracted from wild bootstrap procedure with city-level clustering, are reported. All regressions include city and birth-region-by-birth-year fixed effects.

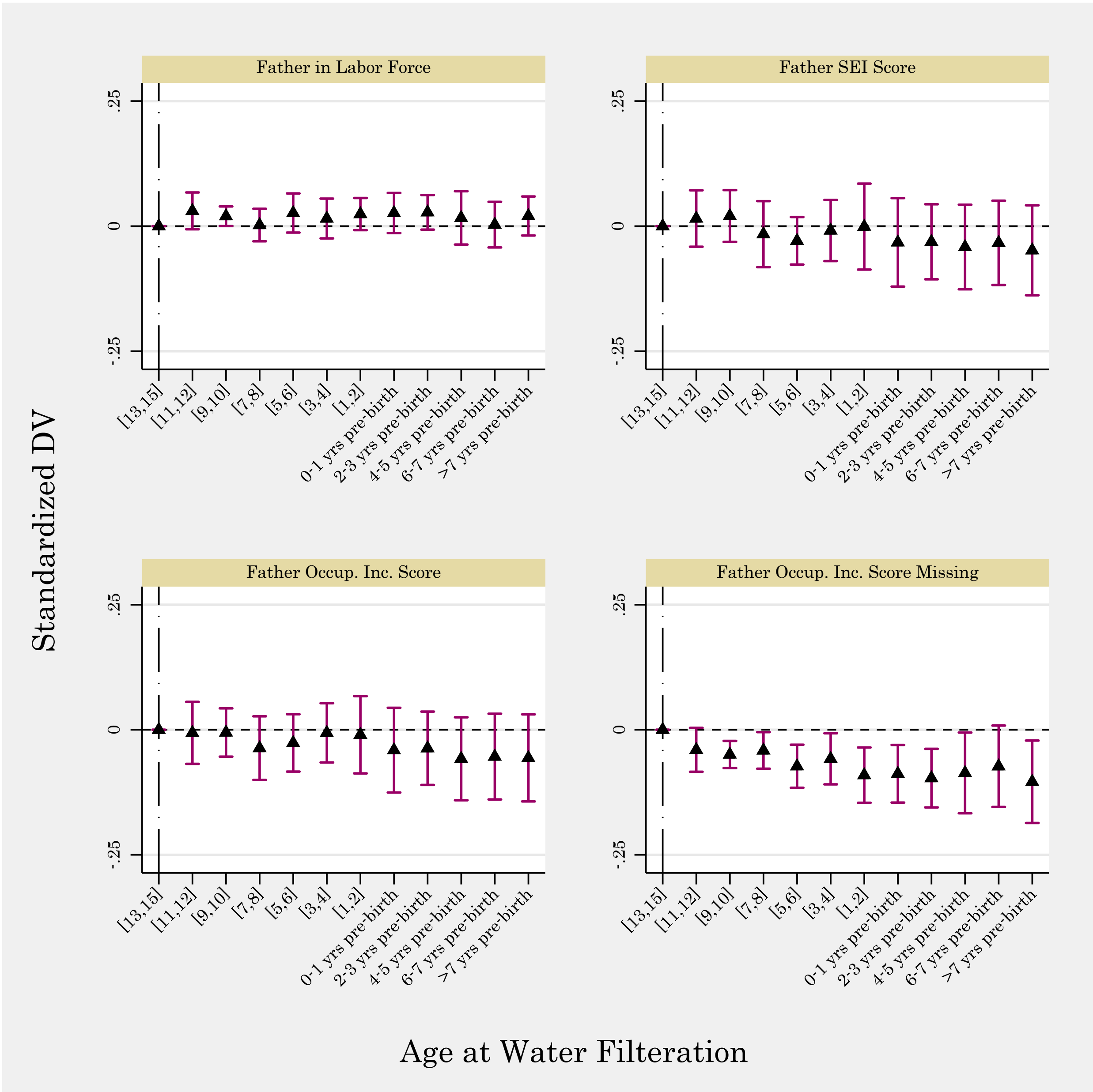


Figure 5 - Exposure To Water Filtration across Different Ages and Observable Individual/Family Characteristics

Notes. Point estimates and 95 percent confidence intervals, extracted from wild bootstrap procedure with city-level clustering, are reported. All regressions include city and birth-region-by-birth-year fixed effects.

Appendix A

Appendix Table A-1 - List of Cities and Year of Water Filtration

City	Year of Water Filtration
Providence, RI	1904
Indianapolis, IN	1904
Washington, DC	1905
Philadelphia, PA	1906
Cincinnati, OH	1907
Pittsburgh, PA	1908
Louisville, KY	1909
New Orleans, LA	1909
Minneapolis, MN	1913
Baltimore, MD	1915
St. Louis, MO	1915
Cleveland, OH	1918
St. Paul, MN	1923
Detroit, MI	1923
Buffalo, NY	1926
Kansas City, MO	1928
Milwaukee, WI	1939
Rochester, NY	Post-1940
Memphis, TN	Post-1940
Chicago, IL	Post-1940
San Francisco, CA	Post-1940
Boston, MA	Post-1940
Newark, NJ	Post-1940
New York, NY	Post-1940
Jersey City, NJ	Post-1940

Appendix B

In the main results, we evaluated the effects of the share of childhood exposure up to age 15. Recall that we limit the sample to those born 15 years pre- and post-waterwork. Therefore, our childhood ages end at 15 years. In Appendix Table B-1, we show the effects of the share of exposure between birth and age z, where $z \in \{1,5,10,14\}$. We observe increases in magnitude as we include more childhood ages. This is expected as all ages are treated although the effects are more pronounced for earlier ages of life.

Appendix Table B-1 - Exploring the Robustness across Age Thresholds for Childhood Exposure

	Outcomes: Age at Death (Months)			
	Z=1 Year	*Z*=5 Years	*Z*=10 Years	*Z*=14 Years
	(1)	(2)	(3)	(4)
Share of Childhood Up to Age *Z* Exposed to Water Filtration	1.87354***	2.35935***	3.16472***	3.45804***
	(.53569)	(.68613)	(.61429)	(.75694)
Observations	338742	338742	338742	338742
R-squared	.38911	.38911	.38911	.38911
Mean DV	848.537	848.537	848.537	848.537
P-Value	0.046	0.044	0.009	0.008

Notes. Standard errors, clustered on city, are in parentheses. P-values are extracted from the wild bootstrap procedure with city-level clustering. All regressions include city fixed effects, birth-year-by-birth-region fixed effects, individual controls, family controls, and city-level covariates. Individual controls include dummies for race and ethnicity. Family controls include maternal literacy dummy, paternal literacy dummy, maternal labor force status dummy, paternal labor force status dummy, paternal socioeconomic score dummies, and a series of missing indicators for missing values of each variable. City controls include average share of homeowners, average occupational income score, share of white-collar occupation, share of farmers, share of other occupation, literacy rate, and share of married.
*** p<0.01, ** p<0.05, * p<0.1

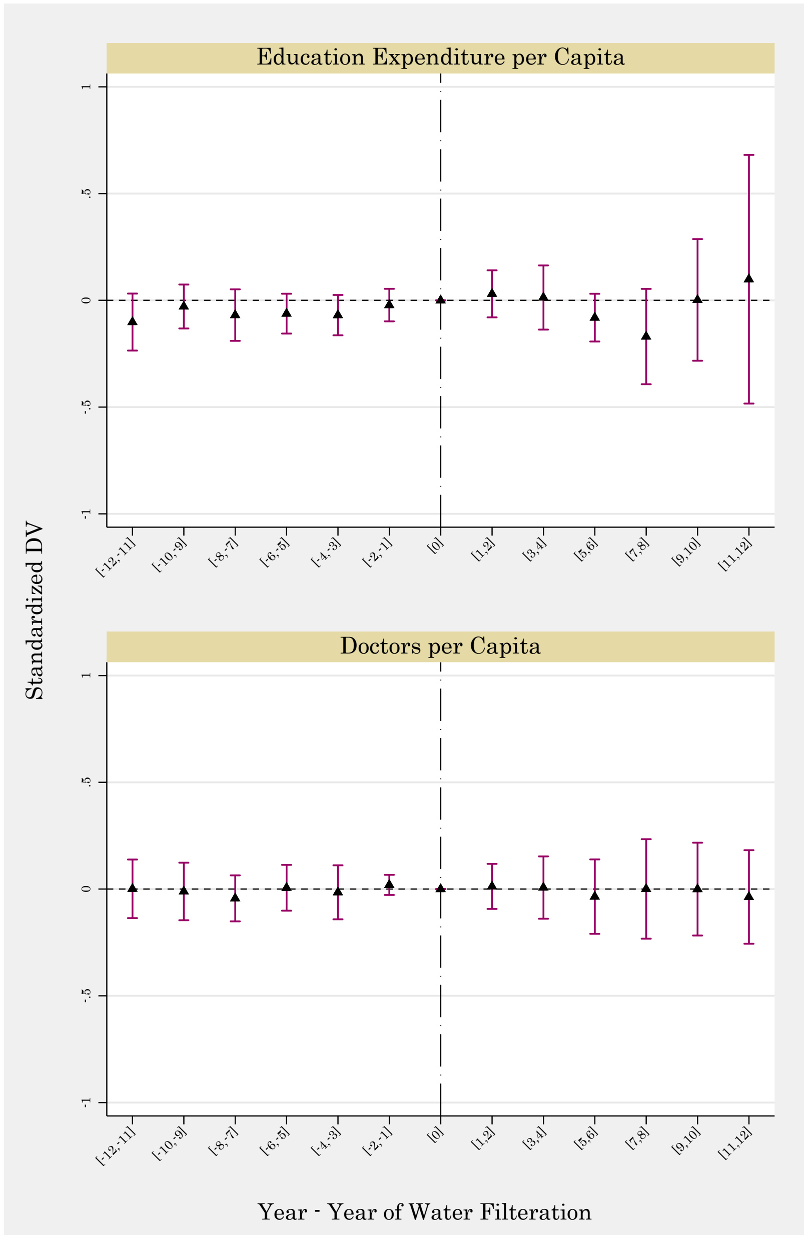
Appendix C

Ambitious public projects such as establishing water filtration stations could mirror changes in political environment and economic conditions and may be followed by changes in other public expenditures. For instance, public legislatures could allocate funds for public education in addition to establishing water treatment facilities. In that case, the results might partly incorporate the influences of improvements in public education, considering the literature that documents the education longevity relationship. Similarly, public authorities might consider public health interventions as substitutes and reallocate funds from other health expenditures toward improving water quality.

For a subset of the sample years, we have information on state-level education expenditure per capita and the number of doctors per capita (both extracted from Kose et al. (2021)). We then examine changes in these outcomes in different years relative to the city-specific year of water filtration, conditional on fixed effects and covariates. To ease interpretation and cross-panel comparison, we standardize these outcomes with respect to their mean and standard deviation over the sample period. These results are reported in the two panels of Appendix Figure C-1. The results do not reveal any significant changes in many years prior to and after water filtration.

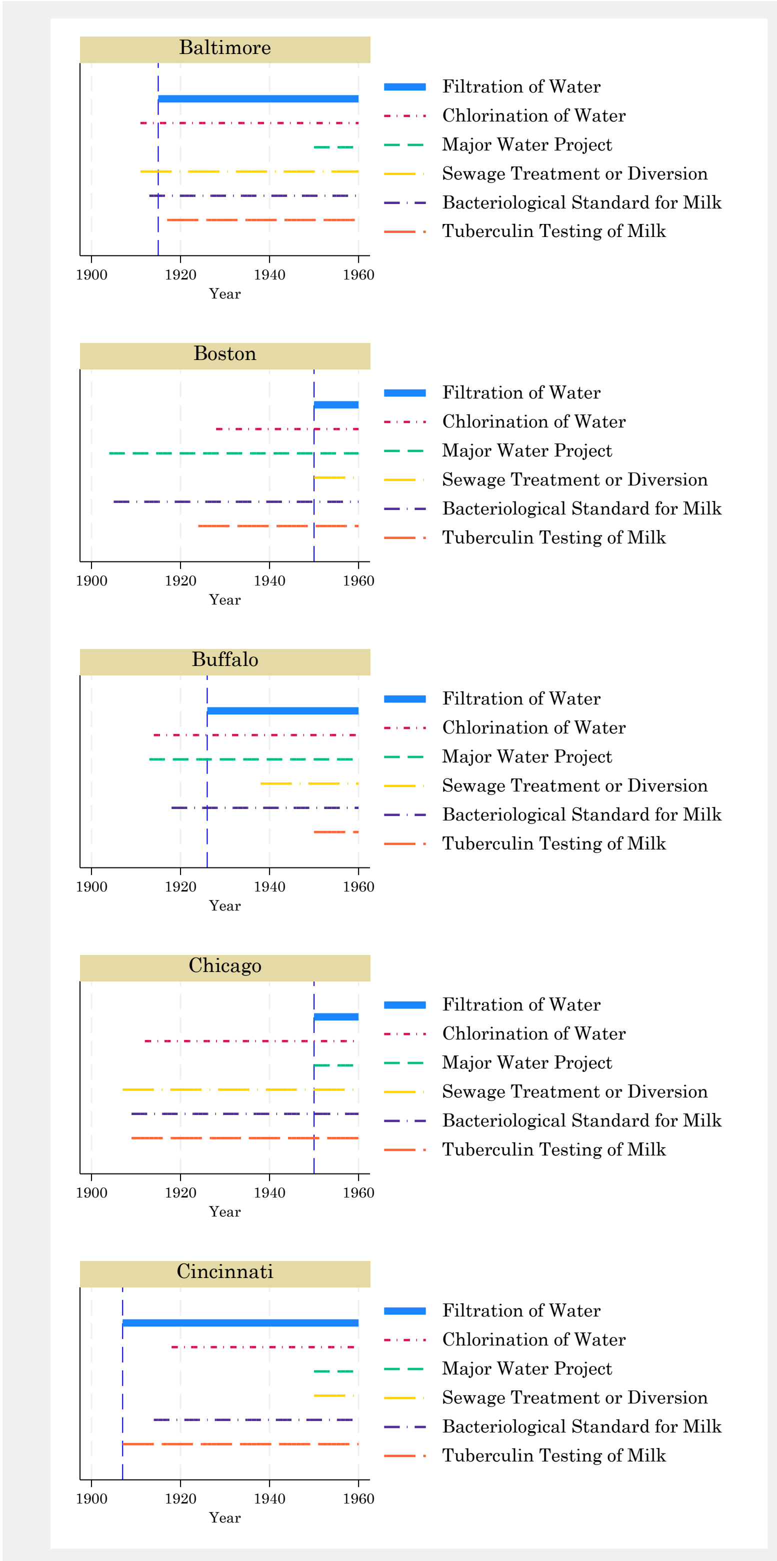
Another concern is that the public health interventions related to water quality were accompanied by other interventions, such as chlorination of water and sewage treatment. In Appendix Figure C-2 through Appendix Figure C-6, we show the year of different public health interventions relative to the year of water filtration across cities in the final sample. In most cases, water filtration occurs after water chlorination. Therefore, one argument is that the positive impacts we observe in the paper are due to the combined benefits of filtration and chlorination. However, we do not observe a significant association of water filtration status with chlorination of water once we examine their correlation using an event study framework with city and region-by-year fixed effects (top left panel of Appendix Figure C-7). This is also true for sewage treatment and implementation of bacteriological standards for milk (top right and bottom left panels of Appendix Figure C-7). We do observe a lower probability of any major water project after water filtration (bottom right panel of Appendix Figure C-7).

Further, we examine the influence of these other public health interventions on longevity. In so doing, we generate dummies that equal one if a city has initiated any of these interventions. We then merge with the final sample based on year and city of birth and implement regressions similar to equation 1. The results are reported in columns 1-3 of Appendix Table C-1. We observe a 0.9-month rise in longevity due to early-life exposure to water chlorination. However, the estimated effect is insignificant at 10 percent level (column 1). For the sewage treatment, we observe a significant increase in longevity of about 2 months (column 2). However, when we include these interventions in the presence of water filtration (column 4), we observe a larger coefficient for water filtration. The respective coefficient of other interventions becomes statistically insignificant, suggesting that the main benefits of waterworks arise from water filtration. We should note that previous studies suggest that among several public health interventions during the early 20th century in the US, water filtration was the most successful, with significant health benefits (Anderson, Charles, & Rees, 2022; Costa, 2015; Cutler & Miller, 2005). Despite the evidence in column 4, we should acknowledge that our sample covers only 25 cities in a specific timeframe in the US. The US currently has 150,000 public water systems. Therefore, our sample may not fully reveal the benefits of other interventions including chlorination of water. Specifically, other interventions such as chlorination of water have been documented to be quite beneficial for health outcomes in other settings (Nadimpalli et al., 2022).

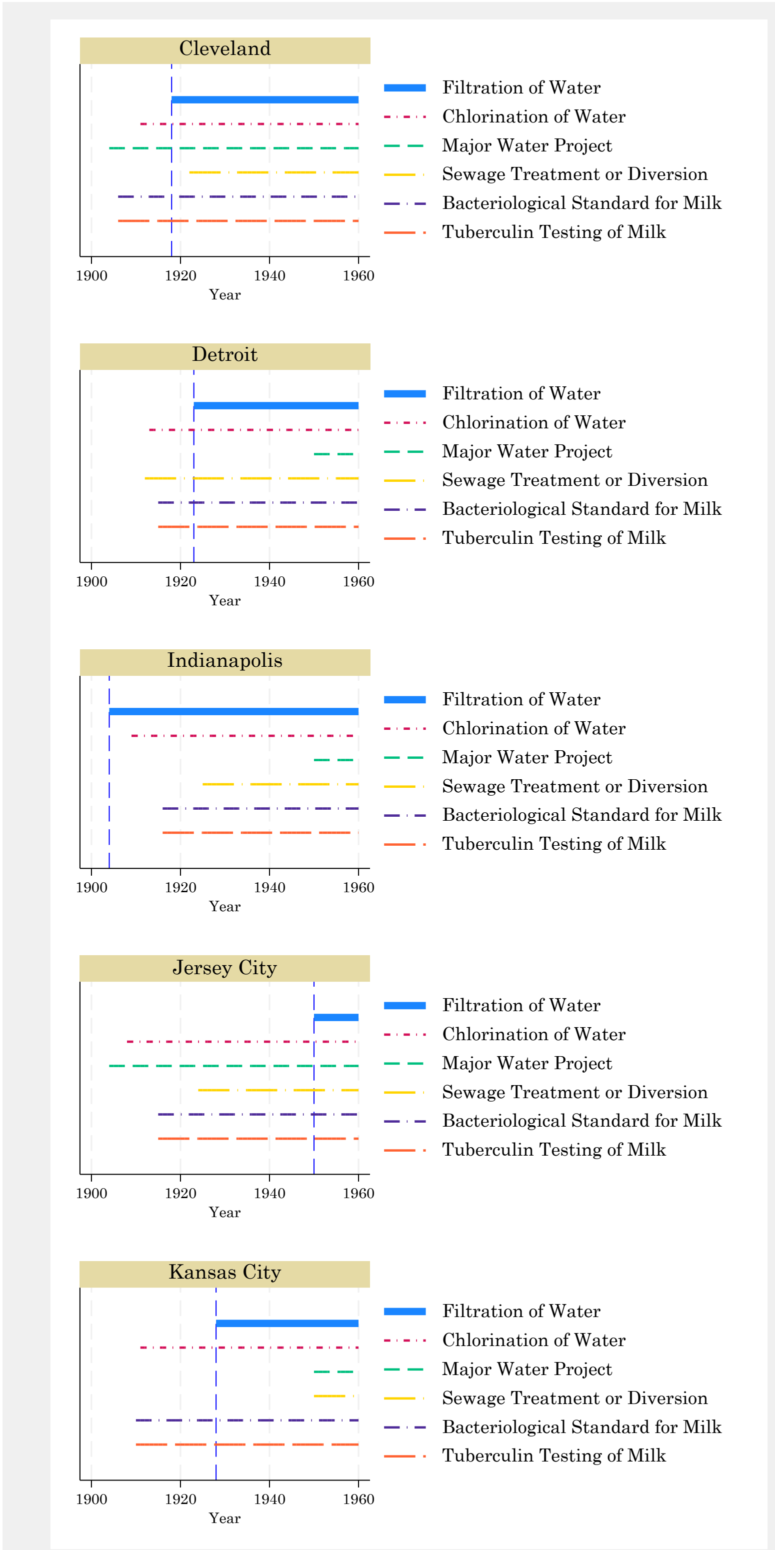


Appendix Figure C-1 - Event-Study Tests to Examine the Evolution of Public Expenditure pre/post Water Filtration

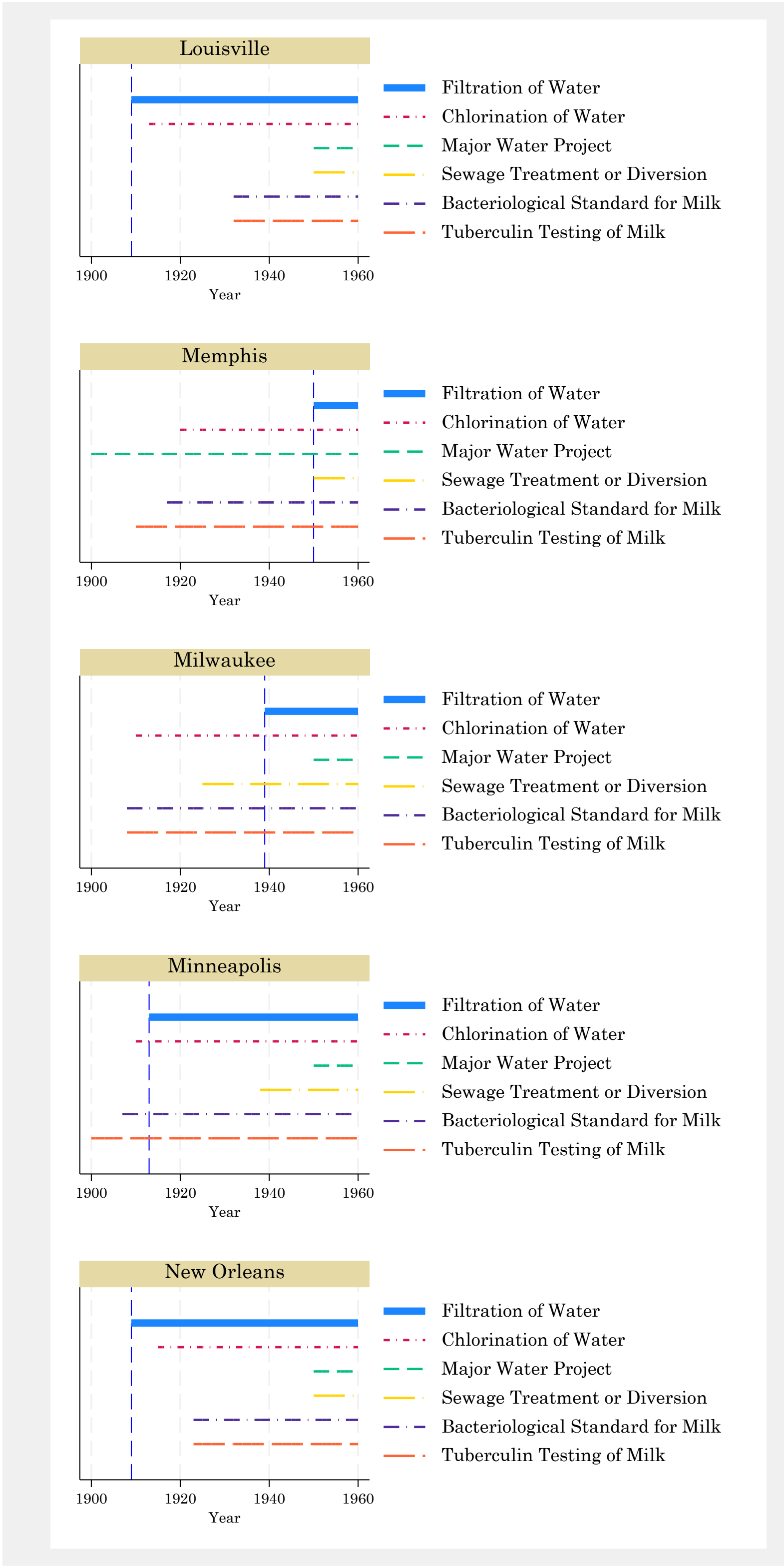
Notes. Point estimates and 95 percent confidence intervals, extracted from wild bootstrap procedure with city-level clustering, are reported. Regressions include city and region-by-year fixed effects. Regressions are weighted using city-level population.



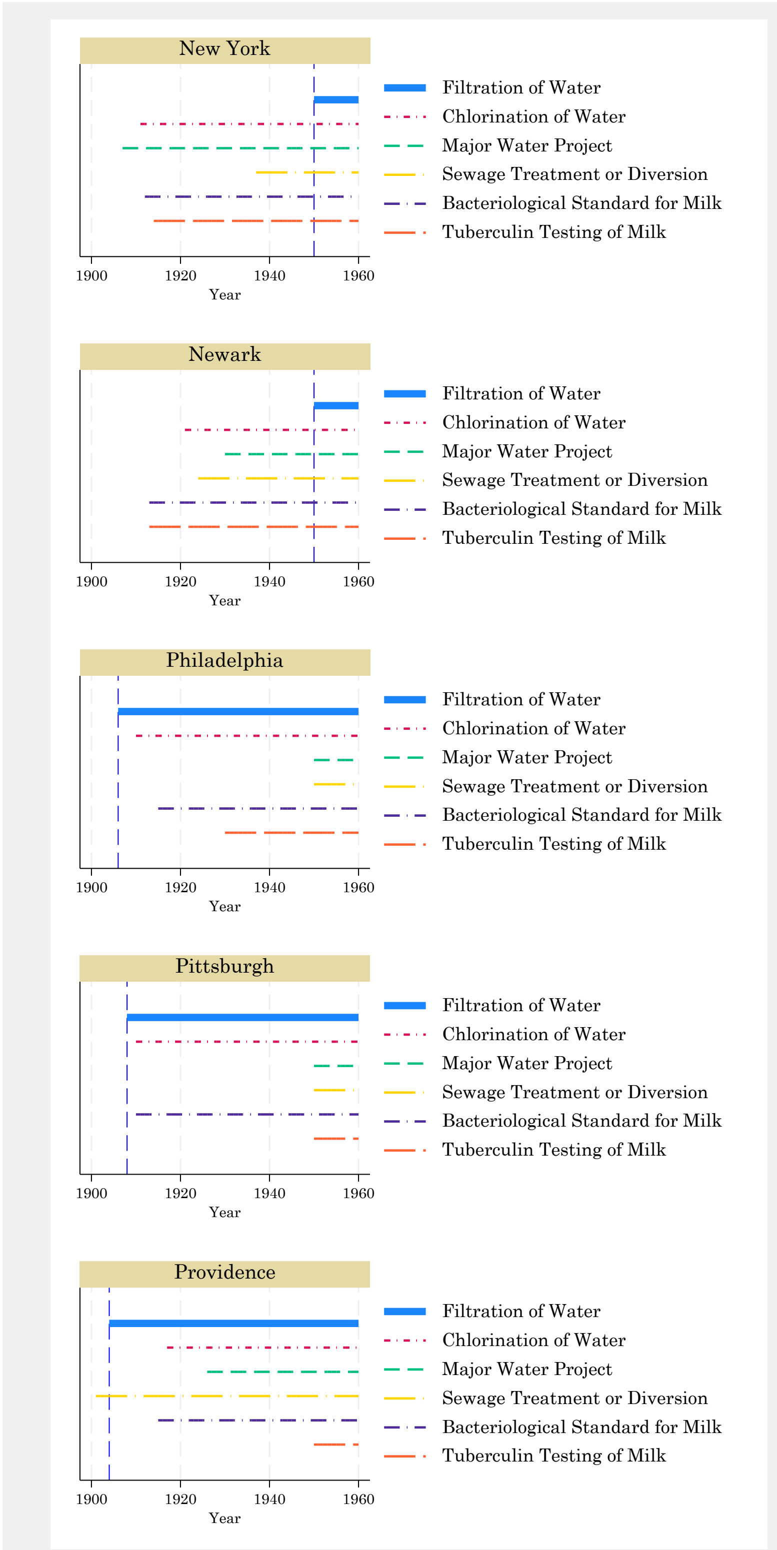
Appendix Figure C-2 - The Evolution of Water Filtration along with Other Public Health Interventions in the Cities in the Final Sample



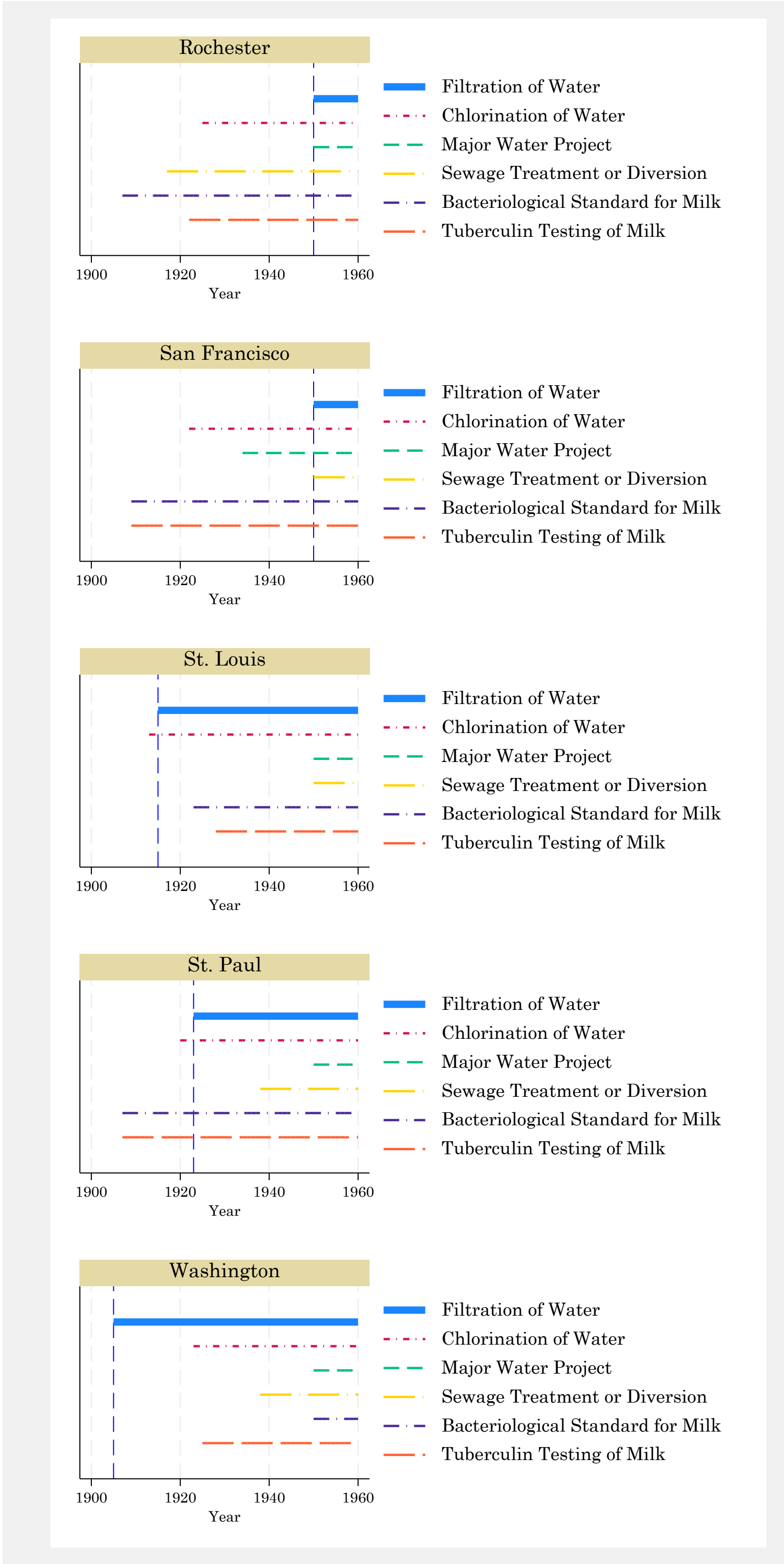
Appendix Figure C-3 - The Evolution of Water Filtration along with Other Public Health Interventions in the Cities in the Final Sample



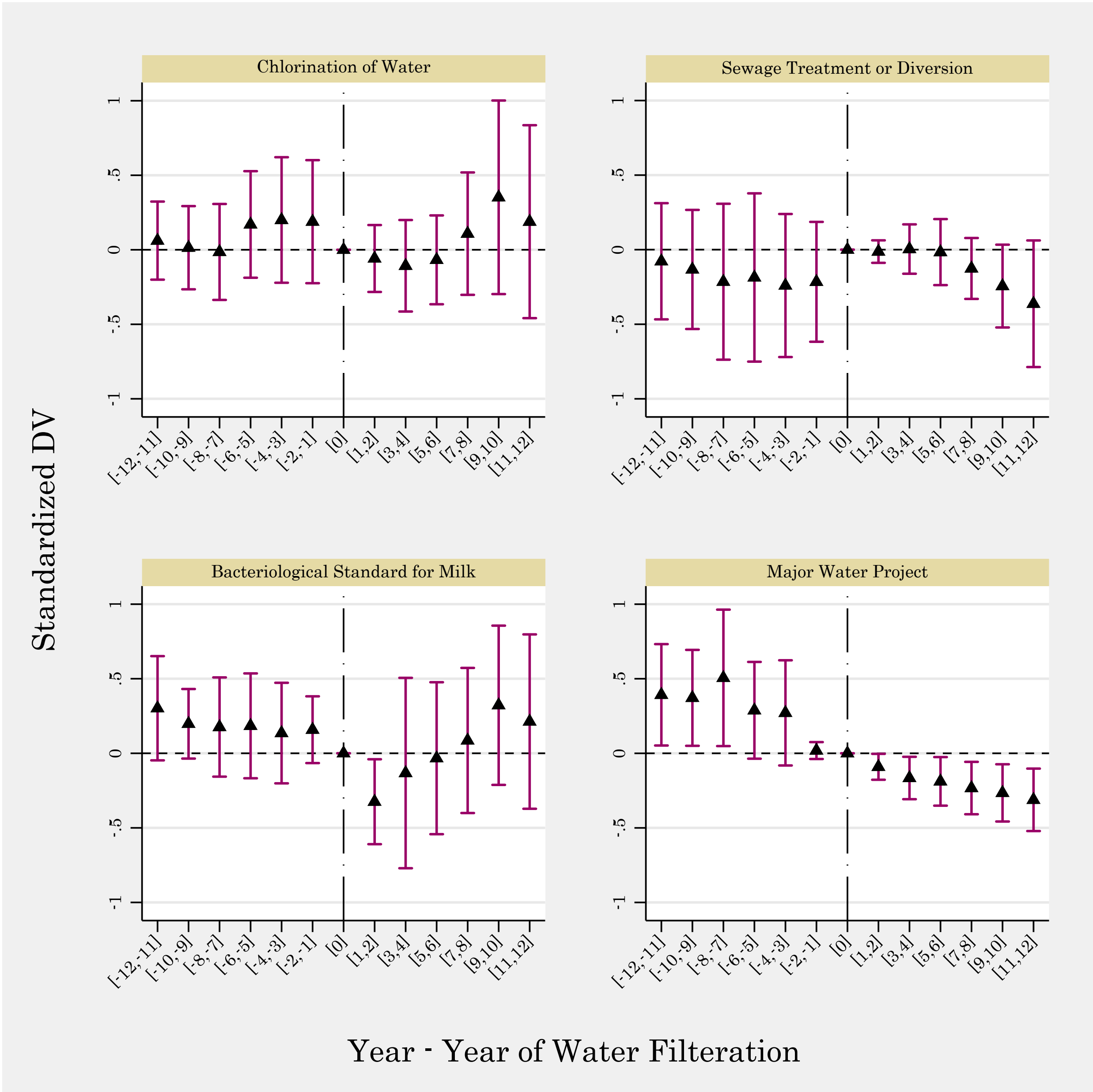
Appendix Figure C-4 - The Evolution of Water Filtration along with Other Public Health Interventions in the Cities in the Final Sample



Appendix Figure C-5 - The Evolution of Water Filtration along with Other Public Health Interventions in the Cities in the Final Sample



Appendix Figure C-6 - The Evolution of Water Filtration along with Other Public Health Interventions in the Cities in the Final Sample



Appendix Figure C-7 - Event-Study Tests to Examine the Evolution of City-Level Public Health Interventions pre/post Water Filtration

Notes. Point estimates and 95 percent confidence intervals, extracted from wild bootstrap procedure with city-level clustering, are reported. Regressions include city and region-by-year fixed effects. Regressions are weighted using city-level population.

Appendix Table C-1 - Examining Other Public Health Interventions

	Outcome: Age at Death (Months)			
	(1)	(2)	(3)	(4)
Water Filtration				2.57429** (1.22395)
Chlorination of Water	.85984 (.74369)			.23493 (.7216)
Sewage Treatment or Diversion		2.02943* (1.10875)		1.69551 (1.22897)
Bacteriological Standard for Milk			.74521 (.68676)	.56146 (.68144)
Observations	338742	338742	338742	338742
R-squared	.35611	.35612	.35611	.35613
Mean DV	867.242	867.242	867.242	867.242
P-Value	0.311	0.144	0.344	0.013

Notes. Standard errors, clustered on city, are in parentheses. P-values are extracted from the wild bootstrap procedure with city-level clustering. All regressions include city fixed effects, birth-year-by-birth-region fixed effects, individual controls, family controls, and city-level covariates. Individual controls include dummies for race and ethnicity. Family controls include maternal literacy dummy, paternal literacy dummy, maternal labor force status dummy, paternal labor force status dummy, paternal socioeconomic score dummies, and a series of missing indicators for missing values of each variable. City controls include average share of homeowners, average occupational income score, share of white-collar occupation, share of farmers, share of other occupation, literacy rate, and share of married.

*** p<0.01, ** p<0.05, * p<0.1

Appendix D

The final sample of the paper, constructed from the DMF data, exclusively focused on male individuals. In this appendix, we employ the Berkeley Unified Numident Mortality Database (BUNMD) from the Censoc project to examine the effects across both genders. The advantage of the BUNMD data is that it covers both genders. Although this data covers a small portion of pre-1975 deaths and ends in 2007 (hence more death years compared with the DMF 1975-2005), the death coverage is relatively thin and unreliable for the years prior to 1988. Moreover, the data is not linked to the 1940 census. On the other hand, the BUNMD data reports county/city-of-birth directly and relieves us from the measurement errors caused by cross-census linking. We replicate the main results using BUNMD data and report them in Appendix Table D-1. In column 1, we observe an insignificant increase in longevity of about 1.6 months. When we focus on male individuals in column 2, we observe a significant change of about 3.1 months, an effect size that is quite comparable to the main results of the paper. For the female subsample in column 3, we observe a relatively small and insignificant coefficient. Therefore, we argue that the main benefits appear to be for male individuals only.

Appendix Table D-1 - Replicating the Main Results Using BUNMD Data

	Outcomes: Age at Death (Months)		
	Full Sample	Males	Females
	(1)	(2)	(3)
Exposure to Water Filtration	1.61008 (1.47034)	3.05413* (1.652)	.68289 (1.71441)
Observations	3480529	1702463	1777755
R-squared	.40152	.32756	.44093
Mean DV	928.753	906.001	950.558
P-Value	0.351	0.181	0.741

Notes. Standard errors, clustered on city, are in parentheses. P-values are extracted from the wild bootstrap procedure with city-level clustering. All regressions include city fixed effects, birth-year-by-birth-region fixed effects, individual controls, family controls, and city-level covariates. Individual controls include dummies for race and ethnicity. Family controls include maternal literacy dummy, paternal literacy dummy, maternal labor force status dummy, paternal labor force status dummy, paternal socioeconomic score dummies, and a series of missing indicators for missing values of each variable. City controls include average share of homeowners, average occupational income score, share of white-collar occupation, share of farmers, share of other occupation, literacy rate, and share of married.

*** p<0.01, ** p<0.05, * p<0.1

Appendix E

In the main results of the paper, we restrict the sample to cohorts that were born 15 years before and 15 years after city-specific water filtration year. In panel A of Appendix Table E-1, we remove this restriction and replicate the main results. The effect size of column 3 remains quite comparable to the main results of Table 2. In panel B, we make the stricter balancing window restriction, i.e., restricting to cohorts born 12 years before and after city-specific water filtration year. The fully parameterized regression of column 3 suggests a slightly larger effect size.

Appendix Table E-1 - Sensitivity of the Results to the Balancing Window Restriction

	Outcomes: Age at Death (Months)		
	(1)	(2)	(3)
Panel A. No Balancing Window Restriction			
Exposure to Water Filtration	2.40851	2.35195*	2.92569***
	(1.45615)	(1.34966)	(.82454)
Observations	396339	396339	396330
R-squared	.35204	.35209	.35255
Mean DV	870.399	870.399	870.399
P-Value	0.136	0.107	0.004
Panel B. 12-Years Balancing Window Restriction			
Exposure to Water Filtration	3.90199**	3.50322**	4.04803***
	(1.60082)	(1.53232)	(1.1142)
Observations	318636	318636	318620
R-squared	.3514	.35145	.35195
Mean DV	869.384	869.384	869.386
P-Value	0.033	0.053	0.029
City FE	✓	✓	✓
Birth Year FE	✓	✓	✓
Individual Controls	✓	✓	✓
Family Controls	✓	✓	✓
City Controls		✓	✓
Region-by-Cohort FE			✓

Notes. Standard errors, clustered on city, are in parentheses. P-values are extracted from the wild bootstrap procedure with city-level clustering. Individual controls include dummies for race and ethnicity. Family controls include maternal literacy dummy, paternal literacy dummy, maternal labor force status dummy, paternal labor force status dummy, paternal socioeconomic score dummies, and a series of missing indicators for missing values of each variable. City controls include average share of homeowners, average occupational income score, share of white-collar occupation, share of farmers, share of other occupation, literacy rate, and share of married.
*** p<0.01, ** p<0.05, * p<0.1

Appendix F

The age profile of individuals in the final sample varies between 35 and 104. In this appendix, we replicate the analysis using the sample of individuals who survived up to ages 50, 55, 60, 65, and 70. These estimates are reported in Appendix Table F-1. Although the estimated coefficient sizes are smaller than that of the main results, they are fairly robust across different subsamples in consecutive columns. We should note that older individuals in the subsamples represent early treated cities. The fact that the inclusion of individuals who died earlier (before age 50) boosts the magnitude of the coefficients may imply that the effects are slightly larger for later cohorts and that survival of earlier cohorts beyond the life expectancy of those cohorts only pushes the coefficients downward.

Appendix Table F-1 - Replicating the Main Results Using Different Subsamples based on Survival Age

	Outcomes: Age at Death (Months), Conditional on Survival up to Age:				
	50	55	60	65	70
	(1)	(2)	(3)	(4)	(5)
Exposure to Water Filtration	2.28326* (1.11258)	2.36808** (.96606)	1.80093 (1.11282)	2.35823 (1.3886)	2.29476* (1.15803)
Observations	328859	316598	292413	249347	192493
R-squared	.28921	.25819	.21809	.17355	.17108
Mean DV	876.701	885.629	900.567	923.950	953.915
P-Value	0.106	0.071	0.181	0.052	0.115

Notes. Standard errors, clustered on city, are in parentheses. P-values are extracted from the wild bootstrap procedure with city-level clustering. All regressions include city fixed effects, birth-year-by-birth-region fixed effects, individual controls, family controls, and city-level covariates. Individual controls include dummies for race and ethnicity. Family controls include maternal literacy dummy, paternal literacy dummy, maternal labor force status dummy, paternal labor force status dummy, paternal socioeconomic score dummies, and a series of missing indicators for missing values of each variable. City controls include average share of homeowners, average occupational income score, share of white-collar occupation, share of farmers, share of other occupation, literacy rate, and share of married.

*** p<0.01, ** p<0.05, * p<0.1

Appendix G

One potential heterogeneity in the results is that improvements in water quality could be more beneficial in areas with a more severe disease environment. Infant mortality rates (IMR) are highly correlated with the availability of sanitation, water quality, healthcare access, and general disease environment. Indeed, several studies use IMR as a proxy for the general disease environment (Almond et al., 2012; Case & Paxson, 2009). In columns 1 and 2 of Appendix Table G-1, we examine the source of heterogeneity by replicating the main results in the subsamples based on city-cohort-specific IMR. The estimated coefficient of the high IMR subsample is about twice the size of the low IMR subsample.

In section 6, we argued that human capital and socioeconomic status are potential pathways between early life exposure to water quality and later life longevity. Therefore, one could expect to observe larger impacts on the same mediatory outcomes in high IMR versus low IMR subsamples. Using the same sample and method as in section 6, we replicate the results on years of schooling and socioeconomic index for high and low IMR subsamples and report them in columns 3-6 of Appendix Table G-1. Relative to the low IMR subsample, the high IMR subsample reveals a slightly larger and statistically significant impact on years of schooling. While we observe positive, large, and significant impacts on the socioeconomic index for the high IMR subsample, the coefficient of the low IMR subsample points to negative and insignificant effects on the socioeconomic index. Overall, the results of this appendix support the notion that improvements in human capital and socioeconomic status during adulthood are pathways of the main findings of the paper.

Appendix Table G-1 - Heterogeneity in the Results Based on Birth-City-Level Infant Mortality Rates

	Data, Outcome, and Subsample:					
	DMF Age-at-Death High IMR	DMF Age-at-Death Low IMR	Census 1950-1970 Schooling High IMR	Census 1950-1970 Schooling Low IMR	Census 1950-1970 SEI High IMR	Census 1950-1970 SEI Low IMR
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure to Water	3.76426***	1.93156	.25159***	.21655	1.85237***	-1.37909
Filtration	(.71531)	(1.27492)	(.08833)	(.3359)	(.5284)	(1.29471)
Observations	170909	167794	158445	87434	151631	83142
R-squared	.38023	.39829	.31304	.17905	.12145	.12146
Mean DV	844.027	854.217	10.400	10.431	38.487	37.217
P-Value	0.161	0.417	0.000	0.650	0.000	0.499

Notes. Standard errors, clustered on city, are in parentheses. P-values are extracted from the wild bootstrap procedure with city-level clustering. Regressions of columns 1-2 include city fixed effects, birth-year-by-birth-region fixed effects, individual controls, family controls, and city-level covariates. Individual controls include dummies for race and ethnicity. Family controls include maternal literacy dummy, paternal literacy dummy, maternal labor force status dummy, paternal labor force status dummy, paternal socioeconomic score dummies, and a series of missing indicators for missing values of each variable. City controls include average share of homeowners, average occupational income score, share of white-collar occupation, share of farmers, share of other occupation, literacy rate, and share of married. Regressions of columns 3-6 include city fixed effects, birth-year-by-birth-region fixed effects, individual controls, and city-level covariates. IMR stands for infant mortality rate and is calculated based on birth-city-birth-year-level rate of infant mortality. SEI stands for socioeconomic index. *** p<0.01, ** p<0.05, * p<0.1

Appendix H

One concern in the main results is the migration of individuals post-water filtration. For these migrations to affect our estimates, they should correlate with exposure to water filtration. We can empirically examine the association between migration and exposure to water filtration using the full count 1940 census. We link the full count census individuals in the cities covered in our final sample and who were born between 1900-1940 to 1910, 1920, and 1930 full count censuses. For the subsample of linked individuals, we can observe whether they changed city (or state) between each census year (1910-1930) and 1940. We then use the migration status as the outcome in regressions similar to equation 1. The results are reported in Appendix Table H-1. We do not observe a significant association between exposure to water filtration during childhood and the probability of migration from 1910-city, 1920-city, and 1930-city to 1940-city (columns 1-3). We do observe a significant coefficient for across-state migration between 1920 and 1940. However, this is not consistent for across states migration between 1910 to 1940 and 1930 to 1940 years.

Appendix Table H-1 - Exposure to Water Filtration and Cross-Census Migration

	Outcomes:					
	Changed city between 1910 and 1940	Changed city between 1920 and 1940	Changed city between 1930 and 1940	Changed state between 1910 and 1940	Changed state between 1920 and 1940	Changed state between 1930 and 1940
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure to Water	.05827	.0009	-.00575	.03258	.03744***	.01535
Filtration	(.04778)	(.0253)	(.02397)	(.02841)	(.01156)	(.01529)
Observations	69114	265064	393467	69114	265064	393467
R-squared	.04241	.03674	.03363	.02303	.02856	.02787
Mean DV	0.396	0.315	0.207	0.184	0.134	0.079
P-Value	0.185	0.975	0.847	0.242	0.001	0.354

Notes. Standard errors, clustered on city, are in parentheses. P-values are extracted from the wild bootstrap procedure with city-level clustering. All regressions include city fixed effects, birth-year-by-birth-region fixed effects, individual controls, family controls, and city-level covariates. Individual controls include dummies for race and ethnicity. Family controls include maternal literacy dummy, paternal literacy dummy, maternal labor force status dummy, paternal labor force status dummy, paternal socioeconomic score dummies, and a series of missing indicators for missing values of each variable. City controls include average share of homeowners, average occupational income score, share of white-collar occupation, share of farmers, share of other occupation, literacy rate, and share of married.

*** p<0.01, ** p<0.05, * p<0.1

Appendix I

The main difference-in-difference coefficient of the main results aggregates the impacts across childhood ages. The general pattern of Figure 2 suggests larger impacts for in-utero and early-life exposures. In this appendix, we disaggregate the exposure measure across different ages to be able to better isolate critical ages. Specifically, we are low for different ages at exposure to compete with each other. In so doing, we define dummy variables capturing exposure during in utero and ages 0, between ages 1-4, and between ages 5-9. The age group 10-15 (and those in treated cities) serves as the contrast group. The results are reported in Appendix Table I-1. In column 3, we observe a monotonic pattern across coefficients: the earlier in life the exposure, the higher the magnitude of the impact. Further, the effects become comparably small in magnitude and statistically insignificant for the age group 5-9.

Appendix Table I-1 - Exploring the Heterogeneity in the Effects of Exposure across Different Ages

	Outcomes: Age at Death (Months)		
	(1)	(2)	(3)
Exposure to Water Filtration in-Utero and Age 0	1.67443**	2.60686**	3.33483***
	(.80251)	(1.09392)	(.83664)
	{0.150}	{0.070}	{0.012}
Exposure to Water Filtration Ages 1-4		1.96684*	2.62821***
		(.9926)	(.73238)
		{0.116}	{0.002}
Exposure to Water Filtration Ages 5-9			1.00126
			(.83613)
			{0.422}
Observations	338742	338742	338742
R-squared	.35612	.35613	.35613

Notes. Standard errors, clustered on city, are in parentheses. P-values are extracted from the wild bootstrap procedure with city-level clustering and reported in curly bracket. All regressions include city fixed effects, birth-year-by-birth-region fixed effects, individual controls, family controls, and city-level covariates. Individual controls include dummies for race and ethnicity. Family controls include maternal literacy dummy, paternal literacy dummy, maternal labor force status dummy, paternal labor force status dummy, paternal socioeconomic score dummies, and a series of missing indicators for missing values of each variable. City controls include average share of homeowners, average occupational income score, share of white-collar occupation, share of farmers, share of other occupation, literacy rate, and share of married.

*** p<0.01, ** p<0.05, * p<0.1

Appendix J

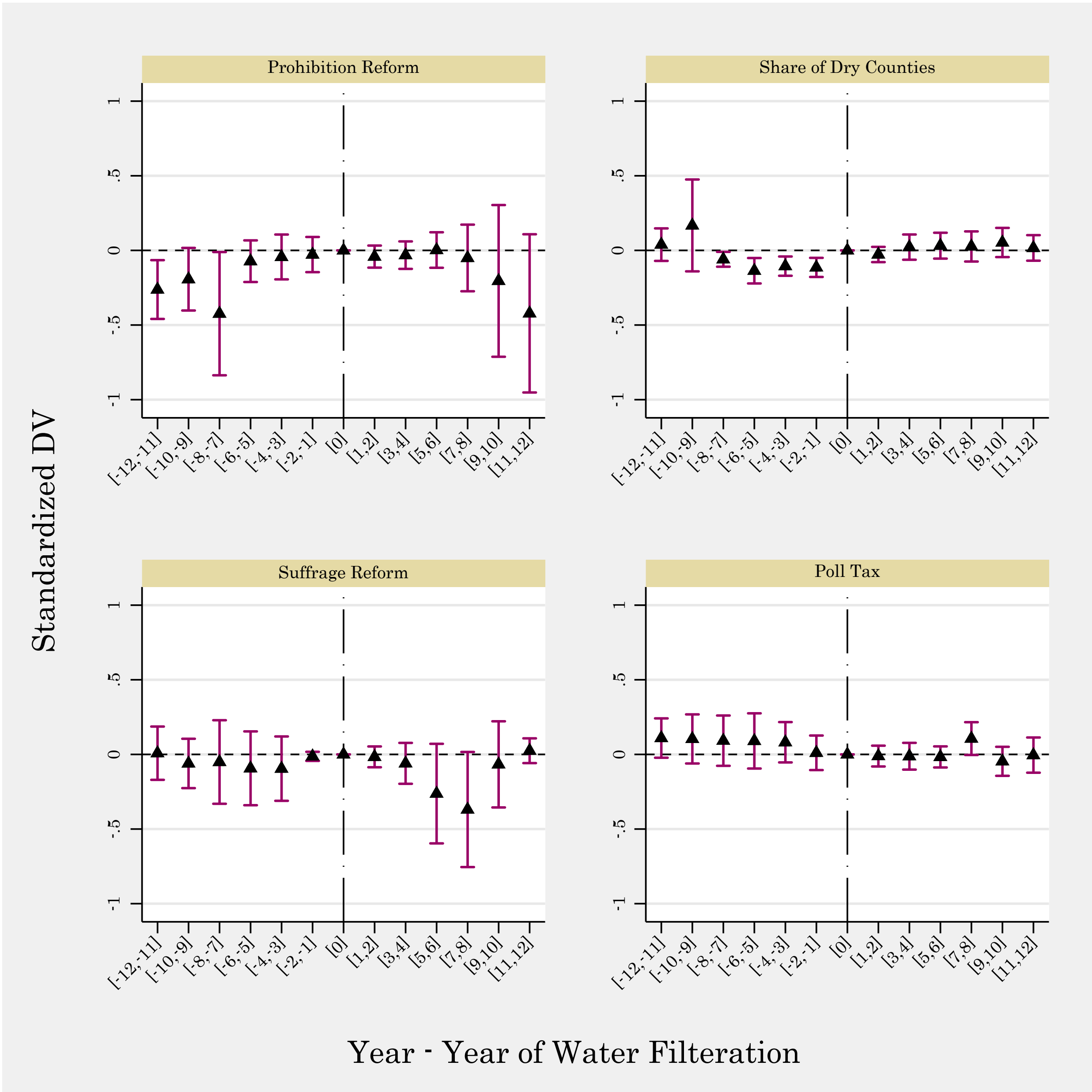
One concern in the main results is the potential endogenous association of water filtration with other state/city-level policy changes and sociodemographic transitions. In so doing, we use a panel of city-by-year covariates and use city-level characteristics and policy changes as the outcome. We execute event studies similar to equation 2 conditional on city and region-by-year fixed effects. In these regressions, we use city-level population as weights and cluster standard errors at the city level. We report confidence intervals based on wild bootstrap procedures with city-level clustering. To enable comparison across regressions and figures, we standardize all outcomes with respect to the variables' mean and standard deviations in the final sample. The results are reported in Appendix Figure J-1 through Appendix Figure J-5.

We do not find a robust and statistically significant association between water implementation and state-wide prohibition reforms, share of dry counties in each state, suffrage reform, poll tax policy change, state-level implementation of birth registration laws, state entrance into birth registration area, and the presence of child labor and compulsory attendance laws (Appendix Figure J-1 and Appendix Figure J-2). Almost all pre-trend and post-trend coefficients are small in magnitude and statistically insignificant.[16]

In the next set of figures, we explore differences in city-level sociodemographic and socioeconomic characteristics across treated-control groups and over different years relative to the public health reforms. We do not observe a discernible pre-trend and post-trend in various outcomes, including the share of whites, blacks, people of other races, and immigrants (Appendix

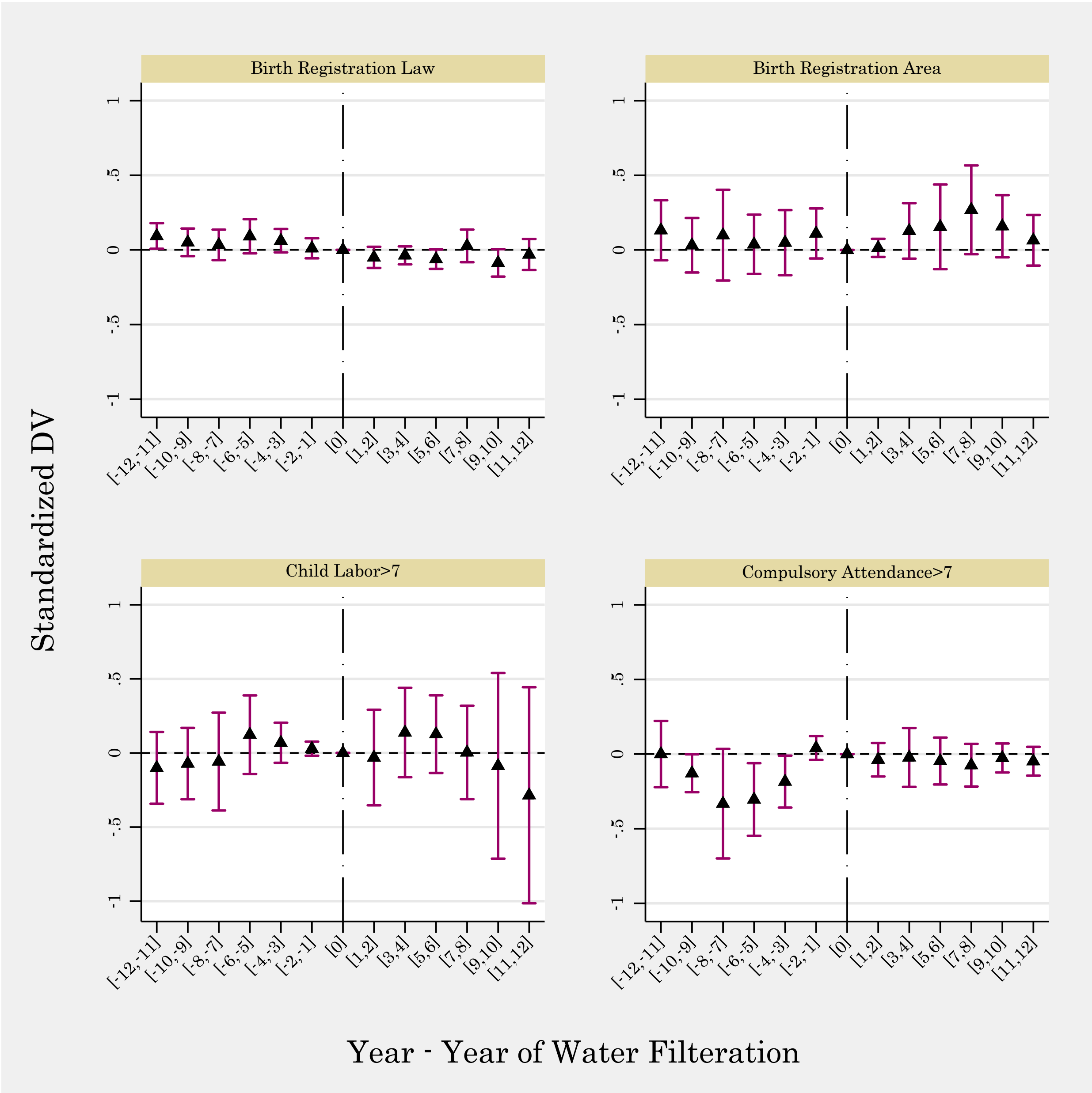
[16] Compulsory Attendance (CA) is a measure of state-imposed mandatory years of schooling and is calculated as the largest of required years of schooling before dropping out and the difference between the minimum school-leaving age and the maximum age at enrollment. Child Labor (CL) index measures the enforcement of age limitation for a work permit and is the largest of years of education required for a work permit and the difference between the minimum age for a work permit and the maximum age allowed for school enrollment. These measures are extracted from Acemoglu & Angrist (2000) and are used in Appendix Figure J-2.

Figure J-3); share of females, children less than 5 years old, married women, and literate people (Appendix Figure J-4); average occupational income score, the share of homeowners, blue-collar workers, and farmers (Appendix Figure J-5). These tests fail to provide robust, consistent, and significant evidence that changes in the demographic and socioeconomic characteristics of the cities could confound the estimates.



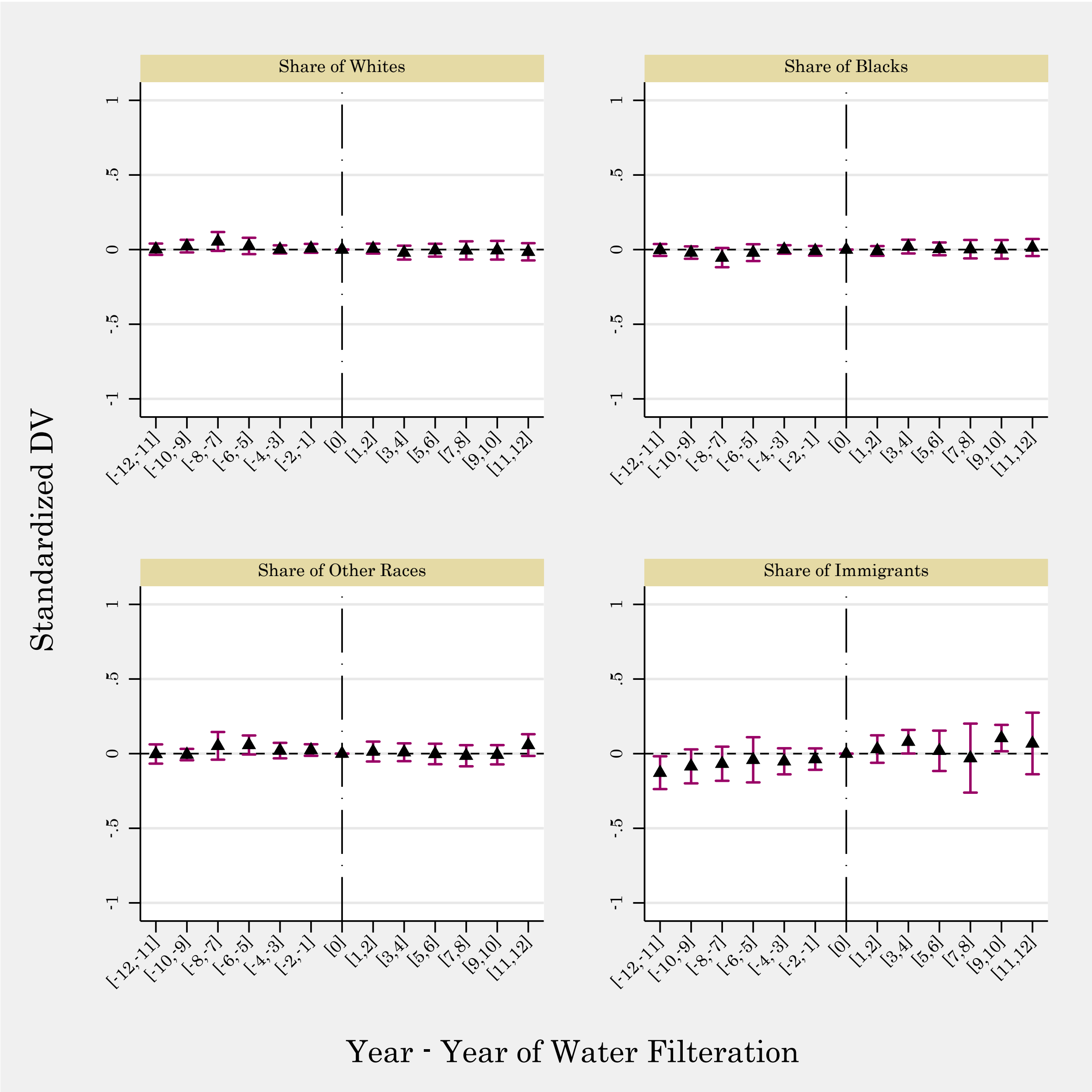
Appendix Figure J-1 - Event-Study Tests to Examine the Evolution of City Observables pre/post Water Filtration

Notes. Point estimates and 95 percent confidence intervals, extracted from wild bootstrap procedure with city-level clustering, are reported. Regressions include city and region-by-year fixed effects. Regressions are weighted using city-level population.



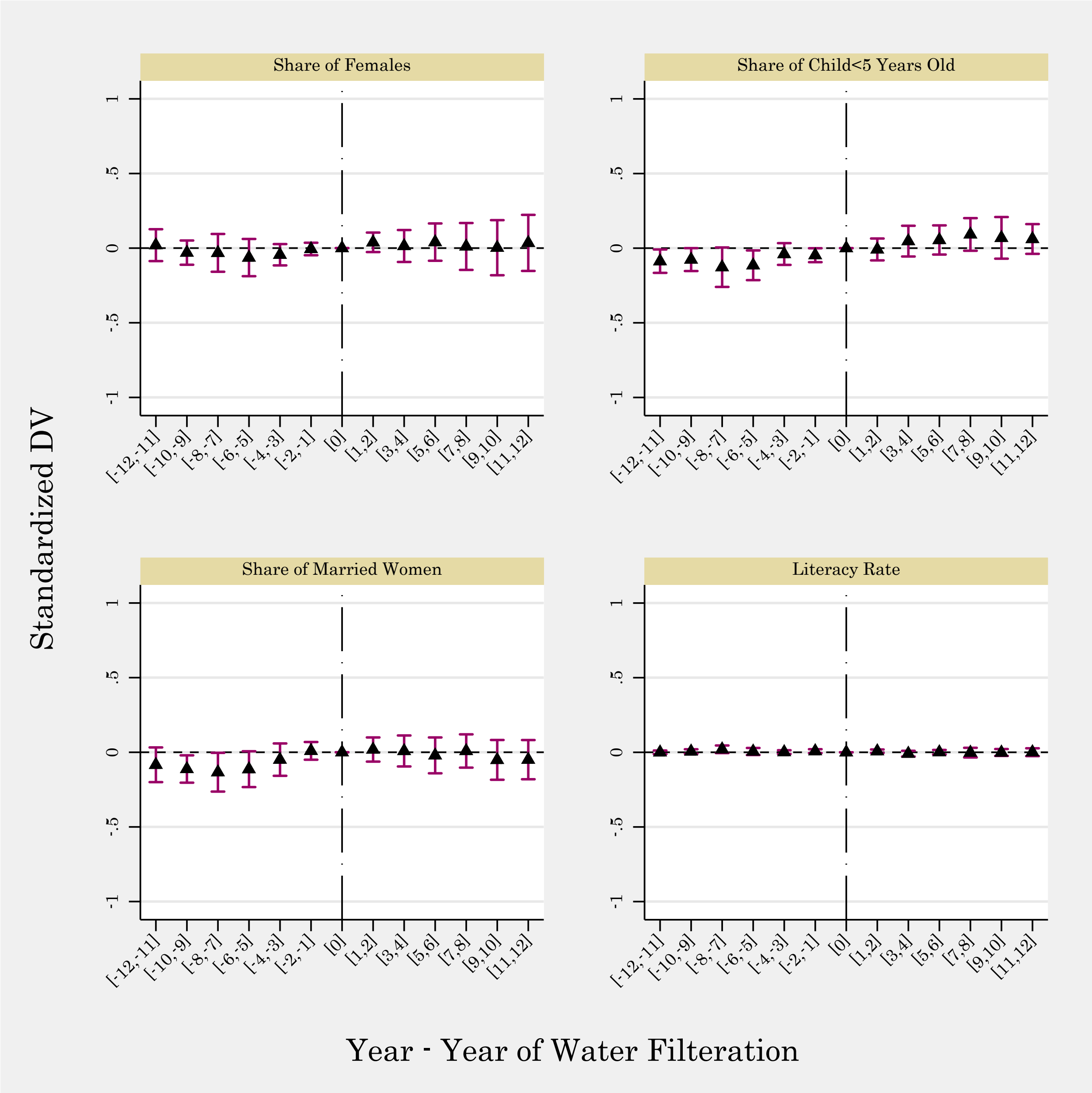
Appendix Figure J-2 - Event-Study Tests to Examine the Evolution of City Observables pre/post Water Filtration

Notes. Point estimates and 95 percent confidence intervals, extracted from wild bootstrap procedure with city-level clustering, are reported. Regressions include city and region-by-year fixed effects. Regressions are weighted using city-level population.



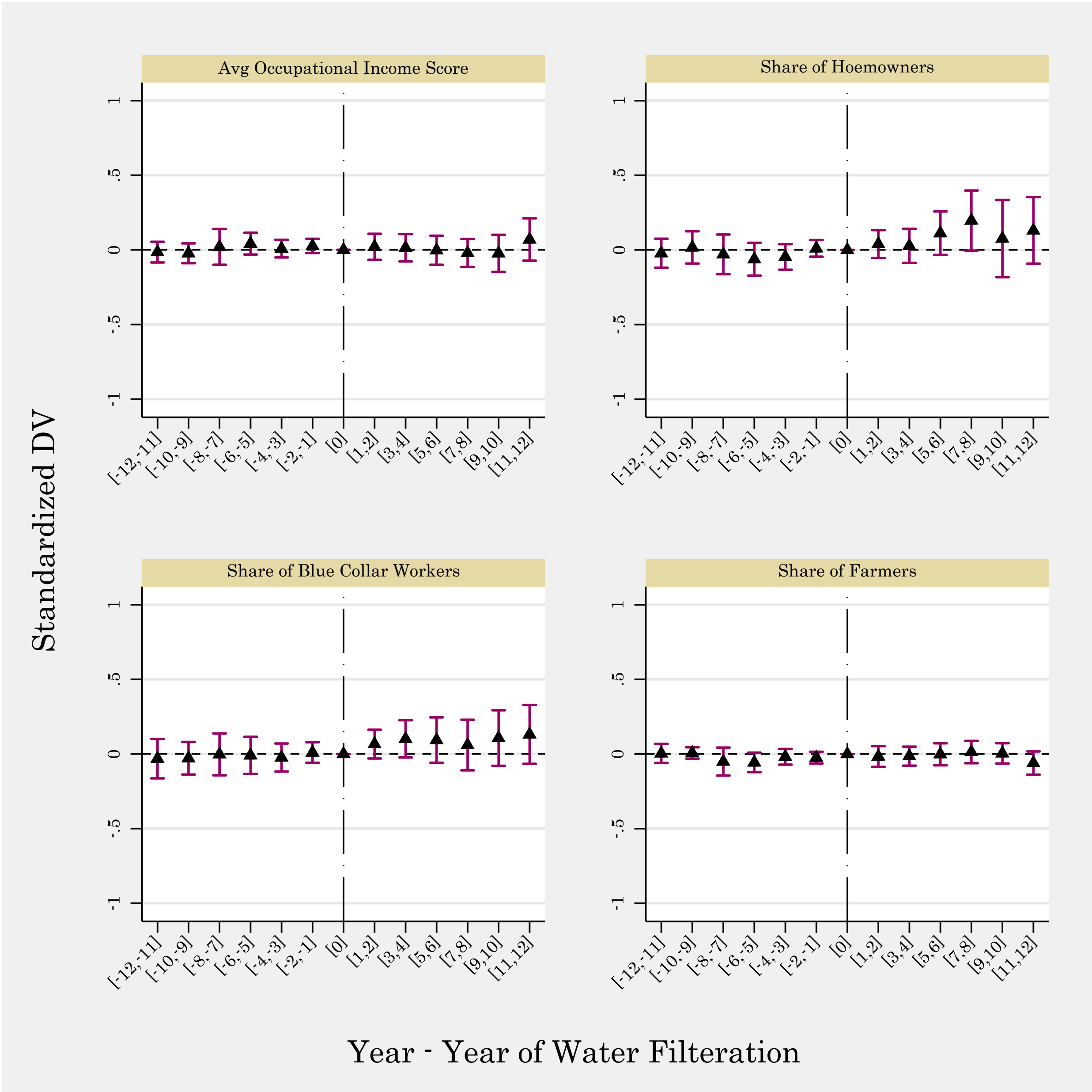
Appendix Figure J-3 - Event-Study Tests to Examine the Evolution of City Observables pre/post Water Filtration

Notes. Point estimates and 95 percent confidence intervals, extracted from wild bootstrap procedure with city-level clustering, are reported. Regressions include city and region-by-year fixed effects. Regressions are weighted using city-level population.



Appendix Figure J-4 - Event-Study Tests to Examine the Evolution of City Observables pre/post Water Filtration

Notes. Point estimates and 95 percent confidence intervals, extracted from wild bootstrap procedure with city-level clustering, are reported. Regressions include city and region-by-year fixed effects. Regressions are weighted using city-level population.



Appendix Figure J-5 - Event-Study Tests to Examine the Evolution of City Observables pre/post Water Filtration

Notes. Point estimates and 95 percent confidence intervals, extracted from wild bootstrap procedure with city-level clustering, are reported. Regressions include city and region-by-year fixed effects. Regressions are weighted using city-level population.

Appendix K

In this appendix, we explain the procedure of cross-census linking and construction of the final sample. We start with the full count 1940 census and link the records to DMF death records. This leaves us with an initial linked sample size of roughly 7.7 million observations. We then restrict the sample to individuals born between 1900-1940, reducing the sample to about 6.6 million observations. Next, we use cross-census linking rules to link individuals across historical censuses 1900-1930. For cohorts born between 1900-1905, we use the following information from the 1900 census: city, state, and parental information. Similarly, for cohorts born between 1906-1910, 1911-1920, and 1921-1930 we use information from 1910, 1920, and 1930 censuses. In the 1940 census, we have information on county of residence in 1935. If the household did not move from 1935 to 1940, the 1935 county is the same as the 1940 county. For cohorts born between 1931-1935, we use the information of 1935 county (and state) to assign the city of birth/childhood. This is possible because for 24 cities out of 25 cities of the final sample, there is a 1-to-1 link between city and county. Further, several counties within New York City can be mapped only to New York City (the 25th city). Finally, for cohorts born between 1936-1940, we use information from the 1940 census. We drop all individuals who are not linked and for whom we cannot infer the city of birth/childhood as well as parental information.

These selections leave us with a sample size of about 2.4 million observations. The sample contains 1,037 cities. Restricting the sample to 25 cities in the final sample for which we have information on water filtration reduces its size by about 84%. We further restrict the sample to cohorts born 15 years before and after the city-specific water filtration year (only for treated cities). The final sample size covers roughly 338 thousand observations.

Although the cross-census linking considerably reduces the sample size, the process arguably limits measurement error in assigning the city of birth/childhood. In Appendix Table K-1, we use the 1940 city as the city of birth/childhood and replicate the main results. We observe point estimates that are 30 percent smaller in size than the main results, suggesting that measurement errors likely result in coefficients that underestimate the true impacts.

Appendix Table K-1 - Replicating the Main Results Using the 1940 City as City of Birth/Childhood

	Outcomes: Age at Death (Months)		
	(1)	(2)	(3)
Exposure to Water Filtration	1.44747 (1.22579)	1.40238 (1.30313)	2.18849 (1.70999)
Observations	996533	996533	996533
R-squared	.36391	.36395	.36428
Mean DV	887.055	887.055	887.055
P-Value	0.249	0.336	0.280
City FE	✓	✓	✓
Birth Year FE	✓	✓	✓
Individual Controls	✓	✓	✓
Family Controls	✓	✓	✓
City Controls		✓	✓
Region-by-Cohort FE			✓

Notes. Standard errors, clustered on city, are in parentheses. P-values are extracted from the wild bootstrap procedure with city-level clustering. Individual controls include dummies for race and ethnicity. Family controls include maternal literacy dummy, paternal literacy dummy, maternal labor force status dummy, paternal labor force status dummy, paternal socioeconomic score dummies, and a series of missing indicators for missing values of each variable. City controls include average share of homeowners, average occupational income score, share of white-collar occupation, share of farmers, share of other occupation, literacy rate, and share of married.
*** p<0.01, ** p<0.05, * p<0.1

Appendix L

One concern regarding the exogeneity assumption pertains to parental fertility response to city-wide public health infrastructure improvements. For instance, if water filtration had an evident impact on infant mortality, parents may observe improvements in their children's survival and be encouraged to increase fertility. Similarly, they may reduce their fertility as the target number of children could be attained by fewer births in the presence of lower child mortality rates (Palloni & Rafalimanana, 1999; Sandberg, 2016). Moreover, the fertility decision could also be correlated with other sociodemographic characteristics of parents, which are among the determinants of their children's later-life longevity.

To address these concerns, we use limited natality and mortality data available for a subset of cities and counties in the data for the years 1915-1940 extracted from Bailey et al. (2016). The data reports the infant mortality and birth rates at the city-year level. We merge this data with water filtration data and implement regressions that include city and year fixed effects. First, we explore the effects on infant mortality rates. These results are reported in column 1 of Appendix Table L-1. Water filtration results in roughly 6.3 fewer infant deaths per 100,000 live births, equivalent to a 9.8 percent reduction from the outcome's mean.[17] However, this finding is sensitive to the functional form and becomes insignificant when we use the log infant mortality rate as the outcome (column 2). Next, we assess the associations with the birth rate per 1,000 women and log birth rate (columns 3-4). The estimated coefficient suggests small and statistically insignificant reductions

[17] There are two reasons that our findings on mortality rate is different than those reported by Anderson, Charles, & Rees (2022), i.e., roughly 11% reduction. First, they include a city-specific time trend while we do not. Including unit-specific trends may over-control for time-varying treatment effects and has been a controversial issue in the literature (Chou et al., 2006; Goodman-Bacon, 2021; Gruber & Frakes, 2006; Lee & Solon, 2011; Meer & West, 2016; Neumark et al., 2014). Second, since the main purpose of this section is to evaluate fertility response, we use natality records from a data source that contains birth rate information starting from 1915. To have a similar panel with natality records, we also use mortality during the same period (i.e., 1915-1940). Indeed, when we use the replication data of Anderson, Charles, & Rees (2022), limit the sample to 1915-1940 years, and remove the city-trend, we reach almost identical effects as columns 1-2 of Appendix Table L-1, both for level and log of infant mortality rate.

in the birth rate, which limits further interpretations. Overall, while we find some evidence for improvements in infants' health, we fail to observe a discernible fertility response.

Appendix Table L-1 - Effects on Infant Mortality and Fertility Rates

	Outcomes:			
	Infant Mortality Rate	Log Infant Mortality Rate	Birth Rate	Log Birth Rate
	(1)	(2)	(3)	(4)
Exposure to Water	-6.38942**	-.03222	-2.53902	-.02475
Filtration	(2.41462)	(.04141)	(1.85609)	(.03295)
Observations	559	559	559	559
R-squared	.97621	.97731	.96937	.97948
Mean DV	63.997	4.103	37.580	3.630
P-Value	0.024	0.396	0.154	0.447

Notes. Standard errors, clustered on city, are in parentheses. P-values are extracted from the wild bootstrap procedure with city-level clustering. Regressions include city and region-by-year fixed effects. Regressions also include city covariates. City controls include average share of homeowners, average occupational income score, share of white-collar occupation, share of farmers, share of other occupation, literacy rate, and share of married.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$