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Public wash programs, long-run child development, and intergenerational mobility: New evidence from rural People's Republic of China

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**PUBLIC WASH PROGRAMS, LONG-RUN CHILD
DEVELOPMENT, AND INTERGENERATIONAL
MOBILITY: NEW EVIDENCE FROM RURAL
PEOPLE'S REPUBLIC OF CHINA**

Dongqin Wang and Eddy (Weijian) Zou

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Abstract

Using individual-level longitudinal data from 1989 to 2015, we examine the long-run effects of nationwide water plant construction and household-level toilet subsidy programs in rural People's Republic of China (PRC) on children's education, health, and nutrition. We exploit the differential timing of these programs across rural villages and use a generalized difference-in-differences strategy to estimate their long-run effects. We find that each program independently increased years of schooling up to 20 years after its implementation. Effects on education are larger for girls than for boys, and for toilet subsidy programs. On average, toilet (water) improvement increases years of schooling by 0.608 (0.341) for girls and 0.405 (0.260) for boys. We find evidence that the toilet program had larger education effects if it was introduced after the water program. Exploiting differential program exposure by birth cohorts, we show that improving child health is an underlying mechanism toward enhanced education effects. We then investigate how the programs affected intergenerational education persistence. The toilet program significantly reduced maternal-child education persistence, while improving the upward education mobility of exposed children. Our findings suggest that improving household sanitation has larger educational benefits than expanding access to safe drinking water and that public WASH programs can be effective in reducing intergenerational inequality in socioeconomic outcomes.

Keywords: sanitation, water and toilet improvements, health and human capital, child development, education, intergenerational mobility, rural People's Republic of China

JEL Classification: I18, I24, J24, J13

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1. INTRODUCTION

In low- and middle-income countries, significant strides have been made toward achieving universal access to safe and affordable drinking water, sanitation, and hygiene, a key objective of the United Nations Sustainable Development Goals. This accomplishment can be attributed in large part to the widespread implementation of Water, Sanitation, and Hygiene (WASH) campaigns by national governments, including the Swachh Bharat Mission in India, Community-Based Total Sanitation plans in Indonesia, and the Toilet Revolution in the PRC. Prior research has demonstrated that these campaigns have contributed to a decline in child mortality rates, as well as improvements in the anthropometric and nutritional status of children (e.g., Augsburg and Rodríguez-Lesmes 2018; Alsan and Goldin 2019; Chen, Li, and Xiao 2022).

Despite this, several policy-relevant gaps remain in our understanding of the impacts of WASH programs. First, limited by the scale and duration of their implementation, most studies were only able to investigate the short- and medium-run effects of these programs on child health and educational attainment. There are good reasons to believe that their long-run impacts on child health and education may differ completely, given concerns about rigidities in sanitation behaviors (e.g., Augsburg et al. 2022). More importantly, there is little empirical evidence on whether such WASH programs can not only improve children's health and educational outcomes on average, but can also break the link between parental socioeconomic status (SES) and child development. This is theoretically ambiguous: On the one hand, public provision of better WASH services may disproportionately benefit those with lower SES status, if there is a socioeconomic gradient in private demand for sanitation; on the other hand, positive externalities from such provisions could benefit all within a given community.¹ Yet, a better understanding of how WASH programs affect intergenerational SES persistence is crucial to inform policymakers of their relative merits to reduce intergenerational inequality in lifetime socioeconomic outcomes. Finally, while public programs often improve several dimensions of WASH services in combination, whether different program components (e.g., sanitary toilets, safe drinking water, sewage connections) are complementary in improving child development remains an open question, with mixed findings in different institutional contexts (for complements, see Alsan and Goldin 2019; Bhalotra et al. 2017; for substitutes or lack of complementarity, see Bennett 2012; Orgill-Meyer and Pattanayak 2020). This question has important implications for policymakers who face budget constraints, and who are choosing from a menu of options among different WASH service provisions.

This paper provides new empirical evidence on these three questions. We analyze the long-run education and health effects of the staggered rollout of nationwide safe drinking water and sanitary latrine subsidy programs in rural PRC from 1989 to 2015. In the early 1980s, 40.75 million rural residents lacked access to safe drinking water, which prompted the PRC government to invest in the construction of water plants, subsidizing household toilet improvements, and expanding the supply of safe drinking water to rural villages. As a result of this big-push WASH campaign, the coverage of clean water supply in rural PRC reached 87% in 2022 (Ministry of Water Resources PRC 2022). Similarly, the coverage of sanitary latrines increased from 7.5% in 1993 to 76% in 2022 (National Bureau of Statistics 2018; The national People's Congress of the PRC 2022).

¹ An exception was Abramovsky et al. (2023), who show that a WASH intervention in Nigeria had greater effects on open defecation in lower-wealth communities. We also find larger positive effects on education among children from households in the bottom quintile of the household income distribution.

Exploiting the staggered introduction of both toilet and water programs across villages and years, we use individual- and household-level panel data from the China Health and Nutrition Survey (CHNS) and implement a stacked difference-in-differences (DiD) strategy to estimate the causal effects of these programs on children's health and educational attainment. Our event-study estimates show that both toilet and water programs increased children's years of schooling and the effects persisted for up to 20 years after they were first introduced. On average, for those aged 6–18 in survey years, toilet programs increase girls' (boys') years of schooling by 0.608 (0.341) years; water programs increase girls' (boys') years of schooling by 0.405 (0.260) years. We then investigate the interaction effects of these two programs by estimating heterogeneous treatment effects of toilet (water) programs in villages that had already introduced water (toilet) programs. We find that, in both cases, children in villages that had already introduced the other sanitation program saw a smaller increase in their educational attainment, compared to the average effects of each program in isolation. These suggest that the two programs did not have complementary effects on children's education. We then exploit differential exposure to both programs across birth cohorts, in a cohort difference-in-differences design, to show that one potential mechanism was indeed the health effects of sanitation campaigns, measured by reduced sickness and the probability of getting diarrhea/fever, as well as an increased height-for-age index and BMI.

Next, we investigate how these programs shaped the intergenerational transmission of education and health in rural villages. To do so, we construct measures of education rank persistence and upward rank mobility. Our stacked DiD estimates show that the toilet program significantly reduced the correlation between the mother's education rank and the children's education rank. We then show, using a "transition matrix" approach, that the reduction in average intergenerational education persistence was driven by the increased probability that children whose mothers were in the bottom two quintiles of their education distribution made it to the top quintile of their cohorts' education distribution. On average, after villages introduced toilet construction programs, the fraction of daughters born to mothers in the lowest education quintile who made it to the top quintile increased from 4.4% to 21.6%, while the fraction of sons who made it to the top quintile increased from 7.6% to 25.5%. We find a similar reduction in intergenerational health persistence, where the mother-child correlations in height have been significantly reduced after toilet programs.

Our study builds on, and contributes to, several strands of literature. We first contribute to the growing literature that exploits within-country variation in WASH interventions to study their effects on child health and educational outcomes (Gamper-Rabindran et al. 2010; Koolwal and Van de Walle 2013; Duflo et al. 2015; Spears and Lamba 2016; Adukia 2017; Augsburg and Rodríguez-Lesmes 2018; Alsan and Goldin 2019; Orgill-Meyer and Pattanayak 2020; Abramovsky et al. 2023). We contribute in several ways. First, the long horizon of the programs, combined with children's full education trajectories, allows us to estimate the causal effects of both programs up to 20 years after their implementation. The longitudinal data further allow us to include child fixed effects to remove any individual-level unobserved heterogeneity that could bias estimated program effects. The stacked DiD estimates are also robust to treatment effect heterogeneity given the staggered timing of the programs (Baker, Larcker, and Wang 2022). Further, we move beyond establishing the effects of water and toilet programs *on average* by showing that they can be effective at breaking the link between maternal education, health, and children's developmental outcomes. This adds to the body of evidence that supports the role of reducing inequality in access to sanitation as a policy tool to improve social mobility.

We also contribute to the literature evaluating the overall costs and benefits of water and sanitation programs introduced in rural PRC (Zhang and Xu 2016; Chen, Li, and Xiao 2022; Wang and Shen 2022). Previous studies found that early-life exposure to safe drinking water had improved child health, educational attainment, and cognitive test scores. However, they mostly considered safe drinking water programs in isolation, may have attributed the effects of toilet improvements to better water quality, and their OLS-TWFE estimates can be biased due to both treatment effect heterogeneity and the staggered implementation of both toilet and water programs across villages (de Chaisemartin and d'Haultfoeuille 2022). We explicitly account for the fact that the two programs not only had independent effects but could also interact with each other. By using different comparison groups, we estimate the effects of each program in isolation and investigate their interactions by assessing treatment effect heterogeneities. Our findings suggest that toilet constructions have played a quantitatively more important role in improving rural children's educational attainment and health than access to safe drinking water. We also find little evidence of program complementarities, as villages that already implemented another sanitation program saw smaller effects of the later implemented program, and the effects of prior implemented programs were attenuated.

The remainder of this paper is organized as follows. In Section 2, we describe the background to water and toilet programs in rural PRC. In Section 3, we introduce our main data sources and empirical strategy to identify causal effects of interest. In Section 4, we discuss our main findings. In Section 5, we investigate the effect of early-life exposure to water and toilet programs on child health as a potential mechanism. We perform robustness checks in Section 6 and conclude in Section 7.

2. WATER AND TOILET PROGRAMS IN RURAL PRC

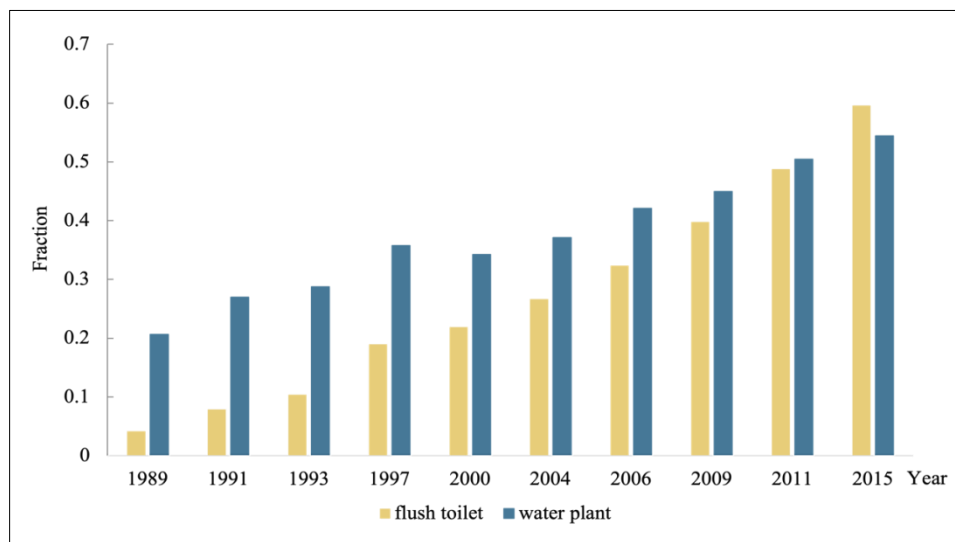
2.1 Water Program

In early rural PRC, people lacked safely managed drinking water and had to depend on contaminated water from wells, rivers, and lakes, resulting in pathogen transmission and infectious diseases. In response to this, from the 1980s, the PRC government launched a multi-stage drinking water improvement program to improve water quality across the country. The first stage of the program (1985 to 2002) primarily focused on reducing the fluoride content in water and constructing irrigation and water systems and roads connected to water projects. This was financially supported by central and local government subsidies and USD228.9 million in loans from the World Bank.² From 2000, the PRC's Ministry of Water Resources proposed to "implement projects focused on solving rural drinking water difficulties" in the Tenth Five-Year Plan, as part of the second stage of the water program (2000 to 2004). This stage was funded by a national debt of USD144 million, together with local government subsidies and villagers' self-raised funds of USD120 million. However, at the end of 2004, 34% of the rural population still faced either an insufficient or unsafe supply of water, prompting the third stage of the water program, which was aimed at upgrading the water supply and further improving water quality. From 2005 to 2015, 230,000 centralized water systems were built to ensure that water plants could be shared among villages. Furthermore, to

² The first-phase (1985–1990) loan of USD80 million had an estimated number of beneficiaries of 600 million people in 25 counties in Beijing, Liaoning, Shanxi, Zhejiang, and Sichuan provinces; the second-phase (1992–1997) loan of USD78.9 million affected 900 million people in 75 counties in Xinjiang, Guangxi, Yunnan, Gansu, Inner Mongolia, and Hunan provinces; the third-phase (1997–2002) loan of USD70 million benefited 460 million people in 40 counties in Hebei, Inner Mongolia, Jiangxi, Hubei, and Yunnan provinces.

expand remote villages' access to clean water, 680,000 decentralized water supply systems were newly constructed. It was estimated that the third-stage program would improve the issue of the lack of safe water access for 298 million rural residents and 41.33 million rural teachers and students nationwide. On average, the water program cost per capita was approximately USD30 (Zhang and Xu 2016) and the program increased the coverage of water plants from 20% in 1989 to about 55% in 2015 (Figure 1).

Figure 1: Coverage of Flush Toilets and Water Plants by Year in Rural PRC



Notes: This figure shows the fraction of CHNS sample villages that had water plants, and flush toilets, in a given survey year. There are 181 villages in the raw CHNS sample.

Data source: China Health and Nutrition Survey.

2.2 Toilet Program

In the early 1990s, the smelly and dirty sanitary environment and its related children's health problems became a serious social concern in rural PRC.³ To address this, the PRC government launched an unprecedented "Toilet Revolution" to subsidize rural households to build clean toilets. In 1992, the State Council first clearly listed rural toilet retrofitting in the government's document named "Program for Chinese Children Development in the 1990s" and set out policy guidelines. In 2004, to motivate households to improve their toilets, the state spent about USD10.6 billion on subsidizing households to install sanitary toilets, while the cost of the second round of the subsidy program was an estimated USD1.2 billion in 2009. In addition to governments, private capital and nonprofit organizations contributed to sanitation improvement. We are therefore unable to precisely estimate overall financial expenditures for toilet improvements. However, from 2005 to 2015, it is estimated that a total of USD5.53 billion was invested, including USD2.16 billion to improve toilets in nine provinces in the China Health and Nutrition Survey, and the average cost of toilet improvement was around USD81 per household (Wang and Shen 2022). With government efforts, the coverage of flush toilets increased from less than 5% in 1989 to

³ A prominent example was the widespread media coverage of Beijing's prevalent open defecation during the 1990 Asian Olympics.

over 60% in 2015 (Figure 1). Table 1 summarizes important information about toilet and water programs.⁴

Table 1: Overview of Water Improvements and Toilet Construction Programs

	Water Program	Toilet Program
Target Counties	Counties with a high incidence of disease and poor water quality	Counties with a low sanitary toilet coverage rate
Payments	Subsidies (administered to villages) to improve water quality and supply capacity	Household-level subsidy after examining toilets (Wang and Shen 2022)
Average Cost	USD30 per capita	USD81 per household
Implementation Level	Village-level water plants	Village local authorities/committees

3. DATA AND EMPIRICAL STRATEGY

3.1 Data

We primarily use an individual- and household-level panel data set from the China Health and Nutrition Survey (CHNS), which started in 1989 and had nine follow-up waves in 1991, 1993, 1997, 2000, 2004, 2006, 2009, 2011, and 2015, covering 12 out of 32 provinces in the PRC. CHNS questionnaires measured individuals' socioeconomic status, health conditions, and information on household characteristics, such as physical infrastructure, which we use to construct our treatment variables. Because toilet and water programs were implemented in rural PRC and our main interest is children's educational attainments, we restrict the sample to rural areas and to school-age children aged between six, the age of starting primary school, and 18, the age of graduating from senior high school. Our estimation sample consists of 180 villages, which have an average sample population of over 800 individuals.

To avoid household self-reported measurement errors and potential endogenous effects of the toilet and/or water improvement on children's outcomes, we define our treatment variables of exposure to these programs at the village level. This is further motivated by the logistical nature of program rollout, where county governments implement them at the village level. Specifically, we construct a binary measure of toilet/water improvement, which equals one if one of the two following conditions are met. The first condition is that the village has more than 75% coverage of flush toilets or that over 75% of the village households' water source is from plant water. The second condition is that the village had more than a 10 percentage point increase in the coverage rate for each year between two survey waves.⁵ In Section 6, we show that our results are unaffected by changing these threshold definitions, suggesting that our findings are not driven by a specific treatment definition.

⁴ A key advantage of our empirical strategy in estimating their causal effects is that we exploit differential timing of the introduction of these two programs, which we proxy using sharp changes in toilet and water source access at the village level. The choice to define our treatment exposure variable at the village level is also consistent with the fact that village boards were the de facto subsidy implementation bodies and that construction of water plants and improvements in water access was implemented at this level. This largely follows prior work that adopts similar methods (Zhang 2012; Zhang and Xu 2016; Wang and Shen 2022).

⁵ For more details about the definition of treatment variables, see Table A1 in Wang and Shen (2022).

Table 2 shows the summary statistics for our main analysis sample.⁶ We restrict the sample to years before the programs were introduced to avoid a confounding imbalance with treatment effects and then we collapse our sample of children aged six to 18 years at the village-year level. Our primary outcomes of interest are children's total years of schooling, school attendance, and the probability of completing grade for age, defined as whether a child completed primary education by 12 years old, lower-secondary by 15 years old and upper-secondary by 18 years old.

Table 2: Village-Level Summary Statistics, Children Aged 6–18

Panel A: Summary Statistics						
	Toilet/Water Treated			Control		
	Mean	Std. Err.	N	Mean	Std. Err.	N
Total years of schooling	5.18	1.43	250	5.38	1.74	263
School attendance	0.76	0.17	251	0.80	0.21	264
Prob(Completing grade-for-age)	0.36	0.17	251	0.33	0.22	272
Mother total years of schooling	5.10	2.35	208	6.74	2.27	241
Prob(Sickness)	0.07	0.16	239	0.07	0.17	261
Prob(Contagious diseases)	0.35	0.43	140	0.20	0.35	188
Height	136.30	12.72	236	141.33	12.99	259
Weight	33.18	8.53	239	37.19	10.75	258
BMI	17.21	1.86	236	17.77	2.39	258
Household income (RMB)	8,969.64	11,702.3	251	15,357.42	21,461.53	272
Panel B: Regressions						
	Toilet			Water		
	Coef	Std. Err.	P-val	Coef	Std. Err.	P-val
Total years of schooling	0.43	0.15	0.00	−0.12	0.17	0.49
School attendance	0.01	0.02	0.55	0.00	0.02	0.90
Prob(Completing grade-for-age)	0.06	0.02	0.00	0.00	0.02	0.96
Mother total years of schooling	0.29	0.36	0.42	−0.77	0.40	0.05
Prob(Sickness)	0.01	0.02	0.74	0.00	0.02	0.83
Prob(Contagious diseases)	0.06	0.04	0.15	−0.07	0.04	0.11
Height	1.22	0.90	0.18	−0.55	1.10	0.62
Weight	1.69	0.67	0.01	0.42	0.84	0.61
BMI	0.32	0.18	0.08	0.28	0.25	0.27
Household income	3,821.28	1,279.37	0.00	−477.07	1,418.83	0.74

Notes: Panel A shows summary statistics at the village-year level, using a sample of 6- to 18-year-olds for each survey year. The summary statistics are based on the sample of observations prior to the introduction of water/toilet programs. Household Income is measured in units of Chinese Renminbi (RMB). We compare the villages that have experienced either toilet or water programs to those that have experienced neither. Panel B reports the coefficients of toilet and water treatment from the same regression, controlling for county and year fixed effects. Heteroskedasticity-robust standard errors are clustered at the village level.

⁶ Table A1 reports the summary statistics separately for toilet and water programs in Panels A and B.

Compared to control villages, those that experienced toilet programs had lower levels of schooling and worse anthropometric outcomes (height and weight). We then run balancing tests using the village-level panel by regressing these outcomes on toilet and water treatment dummies, controlling for county and year fixed effects. We report coefficients, standard errors, and p-values from these regressions in Panel B of Table 2. Most of the coefficients are statistically insignificant, indicating that the treatment is not systematically correlated with the prior observed characteristics. To the extent that there were level differences between treated and control villages prior to the introduction of each program, our difference-in-differences strategy flexibly controls for these through the inclusion of individual (child) fixed effects.

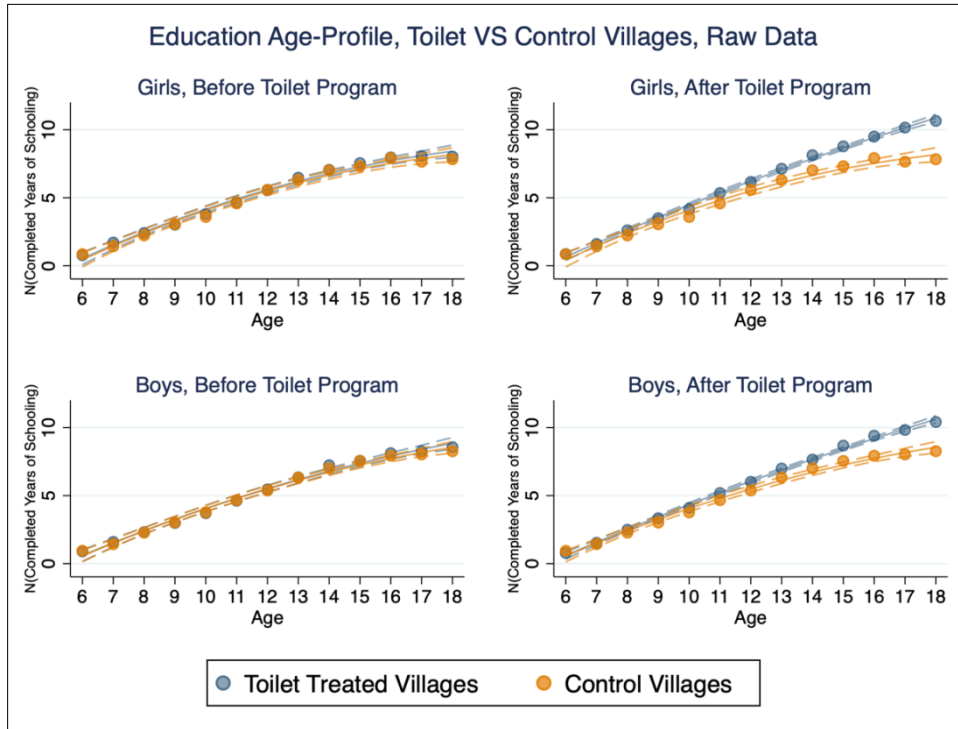
We next provide descriptive evidence that motivates our empirical strategy. We plot education and height age profiles in our sample, allowing the profiles to differ by whether a village was exposed to toilet or water programs, before and after the programs were implemented. We compare the change in age profiles in treated villages to age profiles in control villages, which we hold constant (before and after programs). Figure 2 shows that education-age profiles were similar before both programs, while there was an economically meaningful divergence after programs, with boys and girls in toilet-treated villages receiving more education from the age of 13. By the age of 18, children in toilet-treated villages had between one and one and a half more years of education than those in control villages. We find a similar, albeit much smaller, divergence in education after water programs. Moving on to height-age profiles, we again find similar age profiles prior to both programs, while the profiles became steeper for girls after toilet programs were introduced; however, we find little evidence of a similar divergence for boys or girls after water programs.

Other Data: We use data from the 2011 community survey of the China Health and Retirement Longitudinal Study (CHARLS), a nationally representative longitudinal survey of the elderly across 450 villages/towns, to check potential correlations between the introduction of water, toilet programs, and other programs that could have independent effects on health or education. The 2011 community survey records the year (based on surveying village heads/township officials) that the community (either a rural village or a near rural town) introduced water and/or sanitation, and other social programs. One limitation is that water and toilet programs are recorded as the same class in the survey, and thus we can only examine the correlations of these two programs together in this analysis. We construct an annual district-level panel data set, recording whether the community had experienced any of the social programs that may have an effect on children's education. We focus on four other programs that were rolled out around the same period as the water and sanitation programs, and that could have an impact on children's education: introduction of sewage systems, electrification, elderly pensions, and township mergers or splits.⁷

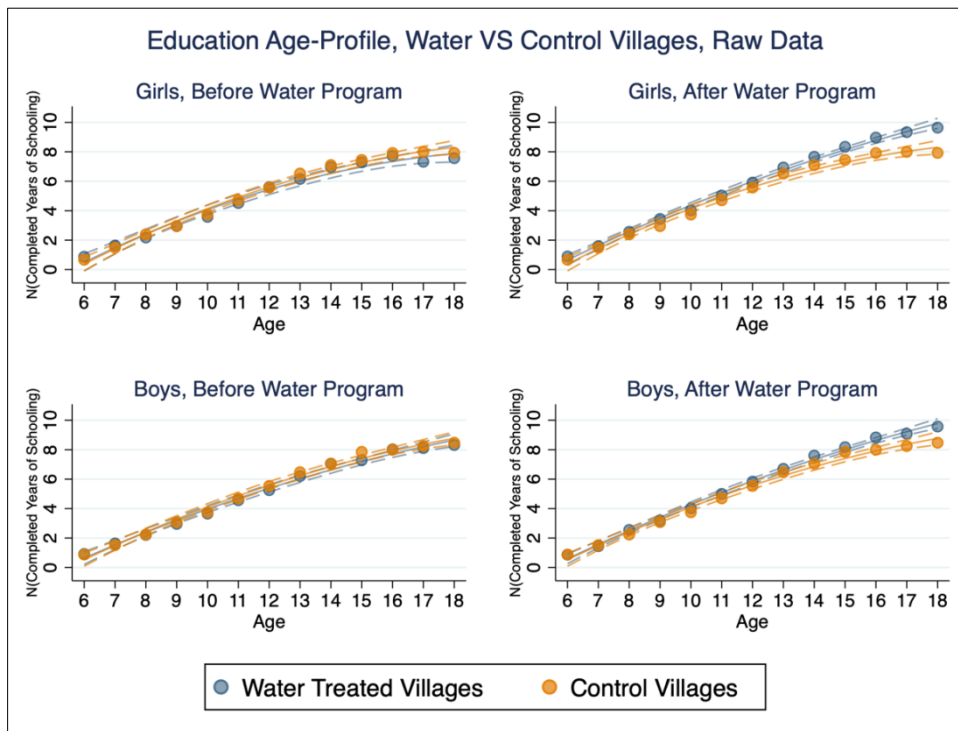
⁷ We plot the number of surveyed communities in CHARLS that have been exposed to these programs over time in Figure 11. Most of these programs started to be rolled out before the water and toilet programs.

Figure 2: Education-Age and Height-Age Profile in Villages with Toilet/Water Programs

(a) Education-Age Profile x Toilet



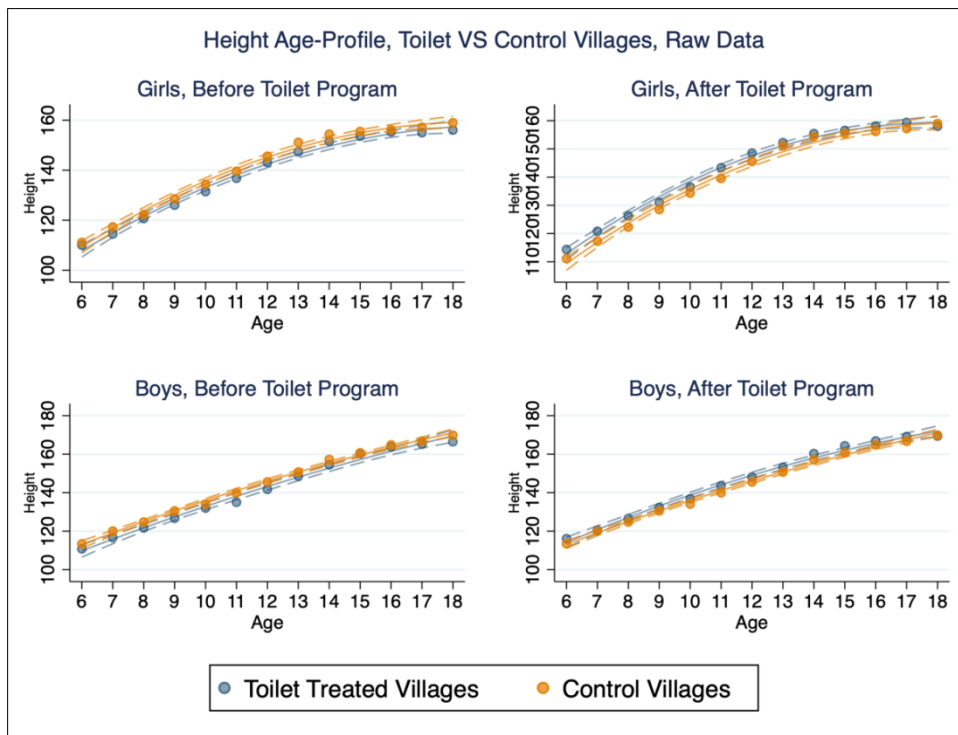
(b) Education-Age Profile x Water



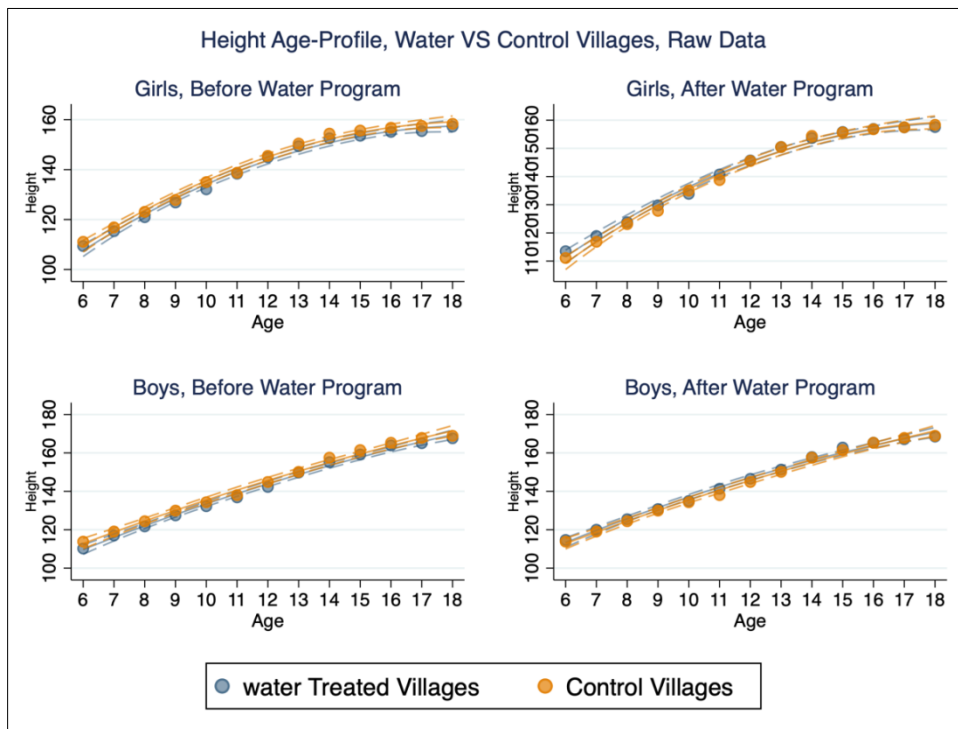
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Figure 2 continued

(c) Height-Age Profile x Toilet



(d) Height-Age Profile x Water



Notes: Panels A and B show the education-age profile, Panels C and D show the height-age profile for toilet-/water-treated villages and control villages. The sample includes children aged 6–18 years when they were surveyed. Source: China Health and Nutrition Survey.

3.2 Empirical Strategy

3.2.1 Baseline: Stacked Difference-in-Differences

We use a generalized difference-in-difference strategy to estimate the effects of sanitation and water programs. Given the staggered introduction of these two programs across villages and years, conventional OLS-TWFE estimates could be biased by treatment effect heterogeneity across villages and/or years (e.g., de Chaisemartin and d’Haultfoeuille 2022, Sun and Abraham 2021). Causal identification is further complicated due to the fact that we have two programs of interest that have been introduced across villages, with correlated timing, and we are also interested in the effects of their interactions. For instance, de Chaisemartin and d’Haultfoeuille (2022) show that in the presence of multiple treatment variables with a staggered introduction, OLS-TWFE estimators suffer from bias both from treatment effect heterogeneity and from potential confounding/contamination among treatments.

To address these empirical challenges, we use a stacked difference-in-differences model, in a similar spirit to Deshpande and Li (2017), Cengiz et al. (2019), and Baker Larcker, and Wang (2022). We construct our event-specific stacked data set as follows. First, we create a treatment vector that contains two elements, namely the years in which the village introduced toilet or water improvement programs. Each unique vector value is a combination of the two treatment years, resulting in a total of 61 treatment vectors. For each treatment vector, we use villages never exposed to either program as the control groups, forming an “event-specific” data set based on observations in villages of a given treatment vector and those villages that have never been treated. This approach differs from prior studies employing stacked DID estimators, since our “event” of interest is defined based on the year in which the village introduced **water and toilet programs**. In this way, we allow for heterogeneous treatment effects across villages, based on the specific combination of toilet and water program years. The stacked OLS-TWFE estimator is then a weighted average of these group-specific treatment effects, where the weights are based on data-specific treatment variance and sample size, and are strictly positive (Gardner 2022). We then append all the event-specific/treatment vector-specific data sets to form our estimation sample. For individual i who is resident in village v observed in year t , we estimate the following OLS event-study specification on this stacked sample:

$$y_{ivt} = \alpha_{gi} + \phi_{gt} + \sum_{k1=-B}^T \beta^{k1} Toilet_{vt} \times D_{ivt}^{k1} + \sum_{k2=-B}^T \beta^{k2} Water_{vt} \times D_{ivt}^{k2} + \epsilon_{ivt} \quad (1)$$

In this equation, we fully saturate individual and year fixed effects (α_{gi} , ϕ_{gt}) by event-source fixed effects, which are dummy variables for which treatment vector source the observation belongs to. D_{ivt}^k are dummies indicating whether individual i was observed in event time k before or after either program was introduced in the village. We include event time dummies for toilet subsidies/water plant construction. The parameters of interest are β^{k1} and β^{k2} , which, under the conditional parallel trends assumption, consistently estimate the average treatment effect on the treated k years before/after the introduction of the program. We group event times in four-year bins to increase statistical power, and we omit the dummy variables for four to one years before each program’s introduction as our reference time period. α_{gi} are individual-group fixed effects, which control for common individual-level shocks for each group; ϕ_{gt} are year-group fixed effects, which control for individuals’ time-invariant characteristics

for each group. Standard errors are clustered at the village level to allow for serial correlations over time.

We include event-time indicators for both programs in the same equation to estimate their respective effects, controlling for potential changes in outcomes induced by the other program. This helps answer the first policy question about which program had a larger impact on children's education. To study potential program complementarities, we compare the overall average effects of two programs with their effects in villages that had already introduced the other program earlier. For instance, we compare the effects of the toilet program in villages that introduced these two programs and the effects in villages that had already introduced water programs by the time the toilet programs were introduced. If there are positive complementarities, we will find larger effects in villages that had already introduced the other programs than in villages that only introduced one program.

3.2.2 Effects on Intergenerational Education and Health Persistence

We next assess whether, and how, the two programs affected the intergenerational transmission of health and education, by looking at changes in mother-child health and education relationships. We focus on mother-child relationships given the finding from prior literature (which spans across countries and time) that maternal education has a larger impact on child health, survival, nutrition, and academic performance (Chou et al. 2010; Harding et al. 2015; Alderman and Headey 2017; Le and Nguyen 2020). To measure intergenerational educational persistence/mobility, we use two approaches: rank persistence, which measures correlations between mothers' rank and children's rank; and upward rank mobility, which measures how children's specific family backgrounds affect their probability of completing more education. To examine rank persistence, we use the same stacked sample of 6- to 18-year-olds. Using this sample, we construct percentile education ranks for children and their mothers, which vary by their birth cohorts. Importantly, mothers' education rank doesn't vary over time, but we allow the children's education ranks to vary over time, reflecting changes in their relative position in the education distribution. We then regress the child's rank on the mother's rank in the education distribution of their own birth cohorts:

$$ChildRank_{ivt} = \alpha + \beta MotherRank_{iv} + \epsilon_{ivt} \quad (2)$$

where $ChildRank_{ivt}$ and $MotherRank_{iv}$ are the child's and mother's ranks in their birth cohort education distributions. Assuming the rank-rank regression is linear, the estimated parameter β measures the relationship between the ranks of children and their mothers in the education distribution of their respective birth cohorts. α measures the expected rank of children whose mothers are at the bottom of the education distribution. Given these measures of persistence, we use a specification similar to Bütikofer, Dalla-Zuanna, and Salvanes (2018), by looking at differential change in rank-rank correlations after each program. We differ from their specification as changes in child rank allow us to control for other confounding shocks through year fixed effects.

$$ChildRank_{ivt} = \beta_0 + \beta_1 MotherRank_{iv} \times PostToilet_{vt} + \beta_2 MotherRank_{iv} \times PostWater_{vt} + \rho_{gc} + \lambda_{gv} + \phi_{gt} + \epsilon_{ivt} \quad (3)$$

$PostToilet_{vt}$ and $PostWater_{vt}$ are the indicators for children being observed after being exposed to toilet/water programs, and ϕ_{gt} are group-year fixed effects. λ_{gv} are group-village fixed effects. ϕ_{gt} are group-birth cohort fixed effects. Standard errors are clustered at the village level to allow for serial correlations over time. Our key

coefficients of interest, β_1 and β_2 , estimate the changes in intergenerational persistence in villages that experienced toilet subsidy/water plant construction. To more clearly see what drove the observed changes in children's education ranks over time, we also estimate stacked OLS-TWFE event studies, splitting the sample of children by quintiles of their mother's education rank. By assessing the relative size of the change in children's schooling based on mothers' education, we can investigate whether changes in education persistence were driven by upward or downward mobility.

Finally, we use a "transition matrix" to measure upward rank mobility. The matrix maps out the empirical probability that a child, whose mother is in a given quintile of their own cohort's education distribution, made it to each quintile of the child cohort's education distribution. We specifically examine changes in the fraction of children whose mothers were in the bottom quintiles and who made it to the top quintiles (those who "moved up the education distribution") after the introduction of each sanitation program. This shows the change in the fraction of children whose relative standing in the education distribution improved as a consequence of the programs.

3.2.3 Changes in Intergenerational Mobility across Cohorts

Our empirical strategy above allows children's education rank to vary over time. This addresses the question of whether children with less educated mothers experienced a differential increase in their relative position in the education distribution after program exposure. As an alternative strategy, we investigate how the correlation between the mother's and child's stock of human capital (measured by completed years of schooling) has changed for cohorts exposed to toilet and water programs at different ages. This empirically tests the hypothesis that early-life exposure to better sanitation has larger potential effects on child health, not only because of their vulnerability to adverse health shocks, but also because of the higher marginal returns to early-life investments. To do so, we construct a cross-sectional data set by keeping date on the last time each person was observed, in a similar spirit to Zhang and Xu (2016). We then use the sample of individuals with their highest observed education to construct time-invariant percentile education ranks, which vary by children's and mothers' birth cohorts. As our interest is now the correlation in the stock of human capital, we restrict our sample to those aged between 12 and 50 years (post-primary school completion age), 16 and 50 (post-lower-secondary completion age), and 18 and 50 (post-upper-secondary completion age). We construct a stacked sample of individuals based on the years they were exposed to water and/or toilet programs in the same way. Using the time-invariant education rank, we estimate the following specification:

$$ChildRank_{ivct} = \alpha_0 + \alpha_1 Age_i + \beta_0 MotherRank_{ivct} + \beta_{1j} MotherRank_{ivct} \times 1[AgeToilet \leq j] + \beta_{2j} MotherRank_{ivct} \times 1[AgeToilet \leq j] + \rho_{gc} + \lambda_{gv} + \epsilon_{ivct} \quad (4)$$

In this specification, $ChildRank_{ivct}$ is the percentile education rank for child i in cohort c , village v , last observed in year t . g denotes the event source, which varies based on program exposure years. We control for a second-order polynomial in individuals' age, cohort fixed effects ρ_{gc} , and village fixed effects (λ_{gv}). $MotherRank_{ivct}$ is the corresponding mother percentile rank. The parameters of interest are β_{1j} and β_{2j} , which estimate the effect of being exposed to water and toilet programs in a given age interval j , relative to those being exposed at an older age. If early-life program exposure reduced intergenerational education persistence, we should expect $\beta_{1j} < 0$. We include under-six and under-18 exposure indicators separately. The identifying assumption required for β_j to be an unbiased estimate of the differential effect of programs on intergenerational education persistence is the common trend assumption,

which requires that for cohorts sufficiently young to have their education affected (those exposed at/under 18 years old), individuals in never-treated villages experienced similar trends in education across cohorts to those in treated villages. We provide supportive evidence by showing in Section 6 that there was no change in education after programs for those exposed after the age of 18.

3.2.4 Mechanisms: Effects of Early-Life Exposure on Child Health

We next explore a potential mechanism behind the programs' effects on education: They improved children's physical health, which then led to increased formation of human capital. Specifically, we investigate whether exposure to toilet or water plant programs in early childhood had differential effects on children's health. This is motivated by the large and growing literature on the interactions between education and health (Conti, Heckman, and Urzua 2010), and the importance of early-life access to better drinking water in later-life cognitive and education outcomes (Chen, Li, and Xiao 2022; Zhang and Xu 2016).

We measure child health using parents' reported sickness over the last two weeks of each survey wave (any sickness, any infection, any fever/diarrhea symptoms). These provide measures of direct impacts on children's disease burdens and are extensively studied in prior evaluations of WASH programs (e.g., Alsan and Goldin 2019). We then look at children's standardized height for age. Child height has been identified as a predictor of cognitive skills, education (Case and Paxson 2008; Spears 2012), and earnings, and is viewed as a summary measure that reflects disease and nutrition burden in childhood (Bozzoli, Deaton, and Quintana-Domeque 2009). We also look at children's BMI index as an additional measure of a child's nutritional status (and an indicator of whether the child had a healthy weight). To estimate the heterogeneous effects of programs by age and formally test the importance of early-life exposure, we now use a cohort difference-in-differences specification, where individuals' treatment exposure is based on their birth year and village of residence. We use the Sun and Abraham (2021) estimator to estimate event studies that nonparametrically estimate average effects of exposure to programs at a given two-year age bin (where we bin to increase statistical power). This estimator is robust to treatment effect heterogeneity across villages and/or time. Although the Sun and Abraham (2021) estimator proceeds in different steps to the OLS-TWFE event-study estimator, the target estimation equation is:

$$y_{ivct} = \tau_v + \phi_{ct} + \mu_c + \sum_{k=-B}^T \beta^k Treat_{iv} \times 1[Age^{Treat} = k]_{ivct} + \epsilon_{ivct}. \quad (5)$$

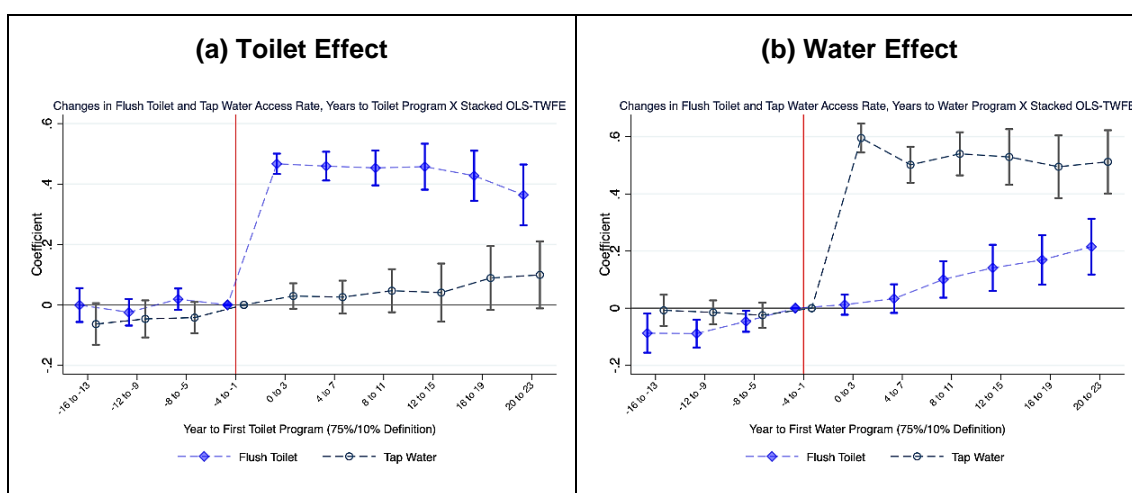
We use the same estimation sample of 6- to 18-year-olds for a child i in village v born in year c and surveyed in year t . $1[Age^{Treat} = k]_{ivct}$ is an indicator for whether the child's age at the time of the implementation of toilet/water program was in bin k . The parameter of interest is β^k . Under parallel trends and no anticipation assumption, the Sun and Abraham (2021) estimator of β^k consistently estimates the average treatment effect of being exposed to a given program in age bin k , relative to an omitted age bin. We choose the 7- to 8-year-old age bin as the omitted reference group to allow us to assess the relative effects of being exposed at six or younger (which we consider as the early childhood period). In contrast to panel DiD estimations, we estimate event studies for each program separately (and thus only include one set of event-time dummies each time), using villages never affected by either water or toilet construction as the comparison group.

4. RESULTS

4.1 Stacked Difference-in-Differences: Effects on Water and Sanitation Access

We first estimate a series of “first-stage” event studies to verify that our definition of a toilet/water improvements program did indeed reflect a sharp and economically meaningful increase in access to tap/plant water and flush toilets. Figure 3 shows event-study estimates of dynamic changes in fractions of households who had access to flush toilets and tap/plant-sourced water, before and after our defined years of water improvement/toilet subsidy programs. For both programs, we observe no pre-program trends in either flush toilet or water access, supporting our assumption that the programs were not correlated with other shocks that could have affected sanitation/water access. We observe a statistically and economically significant jump in the fraction of households with access to a flush toilet (tap water/plant water) after the defined toilet (water) program year. From zero to three years after the toilet programs, there was a 45 percentage points increase in the fraction of households with access to flush toilets, which is around 400% of the mean flush toilet access rate (10 percentage points), prior to the program. This positive effect persisted for over 20 years after the program’s implementation, alleviating concerns about reversals in toilet access over time. After the water program, there was a 60 percentage points increase in the fraction of households with tap water, which is ten times the pre-water program village mean. Importantly, we observe no significant change in the fractions of households with access to tap water after the toilet program (from Panel (a) of Figure 3). We also observe no significant change in the fraction of households with access to flush toilets up to seven years after the water program, although we observe a gradual increase in the toilet coverage rate from years after.

Figure 3: Event Study for the Effects on Households’ Toilet and Water Access Rate



Taken together, these results provide strong evidence that justifies our defined toilet and program variable capturing sharp and economically meaningful change in households' access to a flush toilet and tap water. Further, the absence of contemporaneous change in access to both toilets and water after each program provides evidence that we have enough variation to identify independent effects of each program and that our estimates are highly unlikely to capture the effects of introducing the other types of WASH services.

4.2 Stacked Difference-in-Differences: Effects on Education

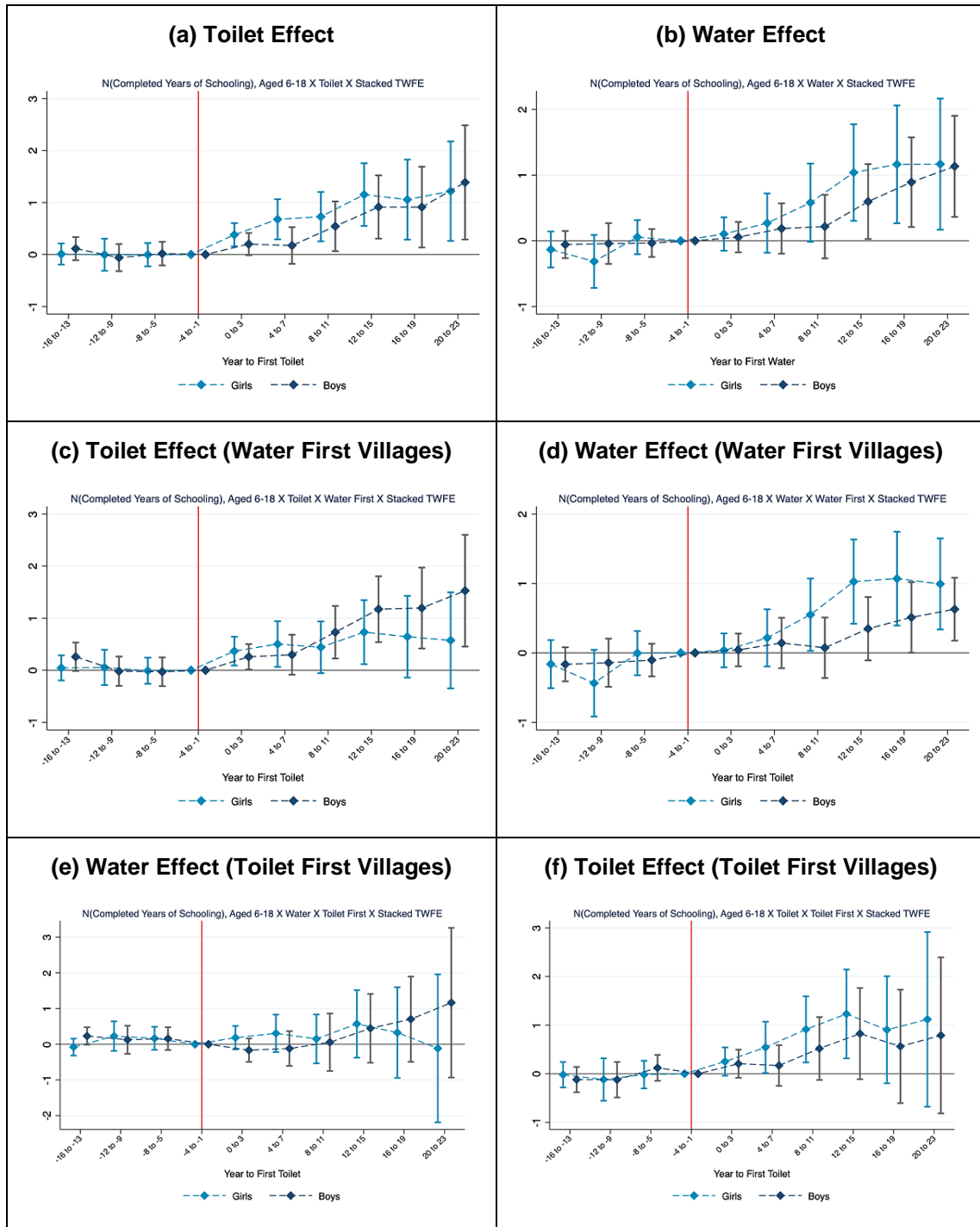
We now report results on effects of sanitation programs on children's education. Panels A and B in Figure 4 show event-study estimates of dynamic effects of toilet construction and water improvement from Equation 1.⁸ We split the sample by gender; this is motivated by the literature that shows strong son preferences and son-biased parental investment behaviors (e.g., Qian 2008; Almond, Li, and Zhang 2019), as well as empirical evidence that shows girls' health is more responsive to WASH investments than that of boys (Abramovsky et al. 2019).⁹ Panels A and B demonstrate that coefficients on pre-program years remain small and statistically insignificant, providing supportive evidence for the parallel trend assumption. Post-treatment coefficients suggest that the effects of both toilet and water programs became significantly different for both boys and girls between zero and four years after the introduction. Further, the effects increased progressively in magnitude.

We now examine the relative magnitude of the effects of each program. We find that for both boys and girls, toilet construction had a larger positive effect on years of schooling than water improvements. One potential explanation for this is that toilet construction led to greater health improvements (which we provide evidence for in Section 5.1). Further, toilet construction had led to an increase in parents' labor supply, owing to the reallocation of housework time (Wang and Shen 2022). Positive household income effects from toilet construction may be a potential channel. Examining gender heterogeneity, we find larger effects for girls than for boys. This is evident both in event-study coefficients and in coefficients in Table 3. Toilet construction increased boys' schooling by 0.260 years but raised girls' schooling by 0.608 years. Water improvements increased girls' schooling by 0.406 years and boys' by 0.338 years.

⁸ In each regression, we include villages that only experience the toilet or water program and pure control villages, which have never had either of the programs.

⁹ For instance, we believe that girls may benefit more from personal safety and privacy after having improved sanitation, or they may be more responsive to WASH improvements owing to prior underinvestments in their human capital. Prior work on water treatment programs in the PRC finds little evidence of significant heterogeneity in effects on cognitive test outcomes by gender (Chen, Li, and Xiao 2022).

Figure 4: Event Study for the Effects on Children’s Total Years of Schooling



Note: Point estimates and 95% confidence intervals of OLS-TWFE event studies of heterogeneous effects on children's total years of schooling by gender. Panels A and B show water (toilet) program effects from the same regression. Panel C shows toilet program effects in villages that already had water plants when toilet subsidies were rolled out. Panel D estimates the effects of water programs in villages that already had water plants when toilet subsidies were rolled out. Panel E estimates the effects of water programs in villages that had already introduced toilet subsidies. Panel F estimates the effects of toilet programs in villages that had already introduced toilet subsidies. Standard errors are clustered at the village level. The sample includes children who were 6- to 18 years old when they were surveyed.

Table 3: Stacked OLS-TWFE, Effects of Toilet, Water on Educational Attainments

	Schooling		School Attendance		Prob(Grade-for-Age)	
	Girls	Boys	Girls	Boys	Girls	Boys
Toilet Effect	0.608*** (-0.159)	0.341** (-0.154)	0.079 (-0.057)	0.08 (-0.05)	0.081** (-0.039)	0.071* (-0.038)
Control Mean Dep. Var.	5.353	5.286	0.759	0.795	0.351	0.343
N (Obs)	70,794	90,364	74,366	92,463	102,740	127,883
N (Clusters)	175	177	176	177	178	179
Water Effect	0.405* (-0.217)	0.26 (-0.175)	0.094 (-0.081)	0.053 (-0.065)	0.05 (-0.047)	0.012 (-0.043)
Control Mean Dep. Var.	5.353	5.286	0.759	0.795	0.351	0.343
N (Obs)	70,794	90,364	74,366	92,463	102,740	127,883
N (Clusters)	175	177	176	177	178	179
Toilet Effect (Water First)	0.590*** (-0.191)	0.432** (-0.181)	0.052 (-0.066)	0.057 (-0.065)	0.100** (-0.045)	0.118** (-0.046)
Control Mean Dep. Var.	5.353	5.286	0.759	0.795	0.351	0.343
N (Obs)	69,318	88,545	72,918	90,680	100,862	125,609
N (Clusters)	142	144	143	144	145	146
Water Effect (Toilet First)	0.397 (-0.246)	0.281 (-0.198)	0.094 (-0.092)	0.051 (-0.071)	0.049 (-0.053)	-0.016 (-0.049)
Control Mean Dep. Var.	5.353	5.286	0.759	0.795	0.351	0.343
N (Obs)	69,318	88,545	72,918	90,680	100,862	125,609
N (Clusters)	142	144	143	144	145	146
Water Effect (Toilet First)	0.294 (-0.255)	0.02 (-0.261)	-0.002 (-0.112)	0.012 (-0.124)	0.016 (-0.085)	0.057 (-0.089)
Control Mean Dep. Var.	5.353	5.286	0.759	0.795	0.351	0.343
N (Obs)	66,928	86,373	70,588	88,531	97,790	122,756
N (Clusters)	98	99	98	99	100	101
Toilet Effect (Toilet First)	0.647** (-0.249)	0.365 (-0.229)	0.144 (-0.093)	0.102 (-0.082)	0.076 (-0.077)	0.025 (-0.075)
Control Mean Dep. Var.	5.353	5.286	0.759	0.795	0.351	0.343
N (Obs)	66,928	86,373	70,588	88,531	97,790	122,756
N (Clusters)	98	99	98	99	100	101
Individual FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y

Note: This table shows stacked OLS-TWFE estimates of the effects of toilet/water programs on educational attainment. Regression results from Equation 3. "Schooling" means the number of completed years of schooling. "School Attendance" means the probability that the child was attending school in the survey year. "Water Only"/"Toilet Only" compares villages that were only treated by one program to those not treated by either program. "Toilet effect (Water First)" estimates the effects of the toilet program in villages that had already introduced water programs. The same is true for "Water Effect (Toilet First)," "Water Effect (Water First)," and for "Toilet Effect (Toilet First)." Standard errors are clustered at the village level.

Next, we examine whether these two programs are complements or substitutes by looking at the effects of toilet subsidies and water plant constructions on children's educational attainments in villages that already have water plants and the effects of water plant constructions and toilet subsidies on children's educational attainments in villages already provided with toilet subsidies. We present the event-study estimates of dynamic effects in Panels C to F in Figure 4 derived from Equation 1. Table 3 reports the static effects. The effects of the toilet program for girls in villages that had already introduced a safe drinking water program before (0.590 years of schooling) were very

close to the overall average effect (0.608 years). For boys, the toilet program had a larger effect in villages that had already introduced a program (0.432 years) relative to both the overall average effect (0.341 years) and villages that introduced the toilet program first (0.365 years). Similarly, we find that the toilet program had a larger positive effect on the probability of completing the grade for age in villages that had already introduced the water program, relative to the total average effect or villages that introduced the toilet program first. However, we find no evidence that the water program had larger effects if they were introduced after toilet programs. Taken together, the results suggested that the prior introduction of safe drinking water programs may have led to larger effects of toilet construction programs on education, although the water program in isolation did not have an economically significant positive effect on education.

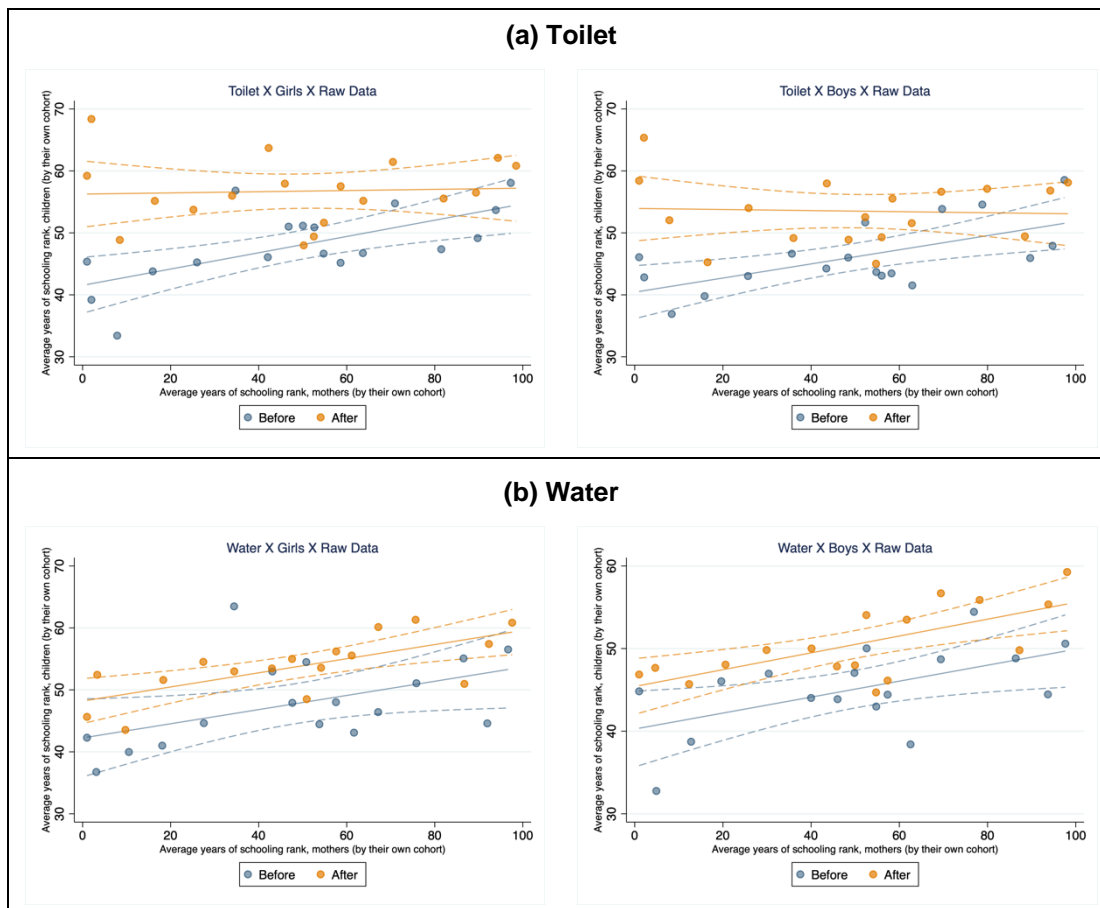
As additional evidence, we examine the other two education outcomes for children to further check the robustness of the impact of WASH programs on child education. Figures A1 and A2 in the Appendix present estimated effects on school attendance and the probability of completing grade-for-age. They show similar qualitative patterns of a progressive increase, although they tend to have larger standard errors and are less likely to be statistically significant. As we find a smaller effect on school attendance for boys and girls than the overall effects on completed schooling, this suggests that some of the increased educational attainment could be driven by a reduction in the number of dropouts. Also, for these other education outcomes, most of the graphs show a consistent pattern of gender heterogeneity, where the effects are larger on girls than on boys. Table 3 reports the static effects for school attendance and the probability of completing grade-for-age; most of the coefficients are statistically or marginally significant.

Taken together, our results show that the introduction of toilet and water programs improved boys' and girls' educational attainment, measured by completed years of schooling, school attendance, and the probability of completing grade-for-age. The increase in education was significant in the short run and persisted until 20 years after the program had been implemented. While the effects of each program in isolation are economically meaningful (from 9% to 13% of mean years of schooling in our sample), we find little evidence that the programs had positive interaction effects in villages that introduced both independently.

4.3 Effects on Intergenerational Education Persistence

Our prior results show that both toilet construction and water plant programs increased children's average educational attainment. We now investigate whether these programs also reduced the link between maternal education and children's development outcomes. We begin with rank-rank education persistence between mothers and children. Figure 5 shows binned scatterplots of the raw mean (percentile) education ranks of children against their mothers' percentile ranks. We focus on children in villages that were affected by the toilet and/or water programs, splitting the sample by whether they were observed before or after each program.

Figure 5: Associations between Children’s and Mothers’ Education Ranks by Villages



Notes: The plots present binned scatterplots of the relationship between children’s total years of schooling percentile ranks and their mothers’ total years of schooling ranks in treated villages before and after being treated. Results for girls and boys are presented in Panel A and B, respectively. Children’s and mothers’ total years of schooling are ranked in their own birth cohort’s education years distribution.

We have a few main findings. First, maternal education is positively associated with higher children’s attainment. Second, the graphs show that at all ranks of maternal education, children had a higher average education rank after being exposed to toilet and water programs.¹⁰ Combined with a higher estimated intercept of post-treated villages, this suggests that the education rank of children whose mothers are at the bottom of the education distribution was higher in villages after sanitation programs. This means that the education distribution of children in post-treated villages shifted to the right. Further, Panel A of Figure 5 shows that the relationship between a mother’s rank and the child’s rank was flattened in villages after toilet programs. The reduction in mother-child associations is larger for girls than for boys. This is consistent with our

¹⁰ One may be concerned that this difference in the slope and level of rank-rank association is due to generic effects of younger cohorts having more education. Figure A3 in the Appendix presents the binned scatterplots for younger and older cohorts (defined as being of the same age as the average age of post-treatment and pre-treatment cohorts in the treated villages) in the control villages, which provides evidence that the difference in pre- and post-treatment rank persistence is not driven by general cohort effects.

prior event-study findings that toilet construction programs increased children's educational attainment by more than water improvement programs.¹¹

Next, we present regression results from Equation 3 in Table 4, which estimates changes in mother-child rank-rank correlations before and after the introduction of each program. Our baseline specification (in Columns 1 and 3) controls for village and year fixed effects, while estimates are stable when including birth cohort fixed effects (Columns 2 and 4). The coefficients of the interaction terms of mother's education rank and *PostToilet* are negative and statistically significant, suggesting that the intergenerational persistence is significantly lower in these villages after toilet construction for both girls and boys. From Column 2, we observe that for girls, after the introduction of the toilet program, mother-child rank-rank correlation fell from 0.098 to 0.048, while the correlation fell to around zero for boys (in Column 4). However, we observe no significant changes in education persistence among children in those villages after water plant programs.

Table 4: Stacked OLS-TWFE, Education Rank Persistence (Time-Varying Rank)

	(1)	(2)	(3)	(4)
	Girls		Boys	
$MotherRank_{iv} \times PostToilet_{vt}$	-0.074*	-0.050**	-0.084**	-0.060***
	(-0.039)	(-0.024)	(-0.034)	(-0.019)
$MotherRank_{iv} \times PostWater_{vt}$	-0.006	-0.018	-0.002	0.002
	(-0.035)	(-0.025)	(-0.033)	(-0.025)
$MotherRank_{iv}$	0.039	0.098***	0.025	0.056**
	(-0.029)	(-0.026)	(-0.037)	(-0.024)
Constant	46.416***	44.252***	45.180***	43.975 ***
	(-1.014)	(-0.901)	(-1.342)	(-0.867)
N (Obs)	88,685	88,521	110,185	110,153
N (Clusters)	178	178	180	180
Village FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Cohort FE	N	Y	N	Y

Notes: Regression results from Equation 3, showing the estimated associations between the mothers' percentile education rank and the children's percentile education rank. All ranks are normalized relative to individuals' own birth cohorts. Standard errors are clustered at the village level.

4.4 Effects on Upward Education Rank Mobility

We have shown that the introduction of the toilet improvements program weakened the link between mothers' and children's education. To better understand whether children born to mothers who have lower education ranks benefited more from the toilet improvements, we use "transition matrices" to measure upward rank mobility. This is a clear and interpretable way to measure whether children were more likely to move up to higher relative positions in the education distribution after being exposed to toilet construction programs. Table 5 presents the intergenerational transition matrices for

¹¹ For completeness, Appendix Figure A4 presents the association between children's and mothers' education rank in pure control villages; the steeper slopes provide suggestive evidence that there was higher intergenerational education persistence in control villages. A simple regression shows that before treated villages have experienced toilet improvement, the correlation between the mother's and daughter's (son's) education rank is 0.181 (0.122), whereas after that treatment, the correlation is reduced to 0.120 (0.054).

children in villages affected by toilet improvements. Each element shows the *change* in the fraction of children whose mothers were in a given quintile that moved into each quintile in their birth cohort's education distribution. We find that after toilet construction programs, there was a 17.2% (17.9%) increase in the fraction of daughters (sons) whose mothers' education was in the bottom quintile, and whose education was in the top quintile. Before toilet programs, 4.4% (7.6%) of girls (boys) with mothers in the bottom quintile moved into the top quintile. After toilet improvements, the fraction increased to 21.6% (25.5%) of girls (boys).¹² This indicates increased upward education mobility, defined by the increased probability that children who were born to mothers with low levels of education completed higher levels of schooling.¹³ Table 6 presents the transition matrix for boys and girls in villages affected by the water program. We find a much smaller increase in the fraction of daughters (9.7%) and sons (7.8%) whose mothers were in the bottom quintile of the education distribution and who made it to the top quintile. This suggests that the water program also increased upward education mobility, albeit at a much smaller magnitude.

Table 5: Change in Conditional Fraction of Children in Education Quintiles Given Mother's Education in Toilet-Treated Villages

	Mother's Education in...				
	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
Δ Fraction (Daughters) with Education in...					
Quintile 1	-0.144	0.037	0.047	0.091	0.021
Quintile 2	0.013	0.001	-0.108	-0.169	-0.071
Quintile 3	-0.058	-0.085	-0.085	-0.138	-0.028
Quintile 4	0.016	-0.302	-0.003	0.11	-0.282
Quintile 5	0.172	0.349	0.149	0.106	0.361
Δ Fraction (Sons) with Education in...					
Quintile 1	-0.062	-0.038	-0.006	-0.066	0.009
Quintile 2	-0.023	-0.089	-0.109	-0.182	0.027
Quintile 3	-0.022	-0.216	0.001	-0.13	-0.116
Quintile 4	-0.072	0.118	-0.1	-0.089	-0.086
Quintile 5	0.179	0.224	0.214	0.466	0.166

To probe the robustness of our prior findings on the effects of toilet programs on intergenerational education persistence, we examine the heterogeneous effects of toilet improvement on children's total years of schooling by mother's educational attainment. This approach makes minimum functional form assumptions about the linearity of program effects based on the mother's education, while allowing us to control for child and year fixed effects. To be consistent with the measurements for upward rank mobility, we look at the heterogeneous effects by the mother's education quintile and estimate our baseline model with the sample of children in each quintile. This helps us answer whether changes in the education mobility pattern we observed can be explained by differential effects on education for children with mothers in different quintiles of the education distribution. Figure 6 plots the event-study estimates, showing a similar pattern to that in the transition matrices. For mothers in the lowest

¹² In Appendix Table A2, we show the matrices separately for the treated villages before and after toilet improvements.

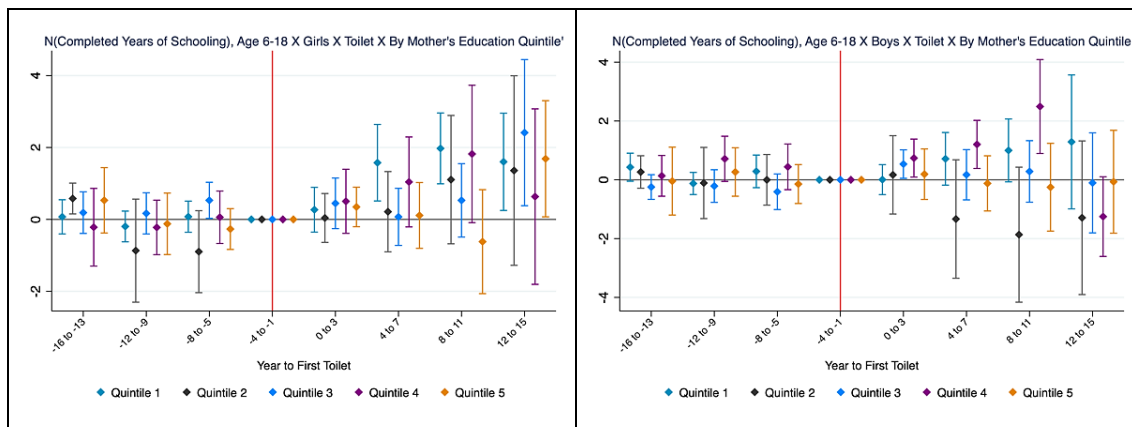
¹³ In Appendix Table A4, we show that the level of upward education mobility implied by the transition matrix for boys and girls in the pure control villages (which didn't experience toilet or water programs) was qualitatively similar to that in toilet-treated villages prior to the programs.

quintile, toilet programs increased their children’s total years of schooling and the effects are of great magnitude, compared to the effects on children born to mothers who belong to other quintiles, where the effects are not significant and are of small magnitude.

Table 6: Change in Conditional Fraction of Children in Education Quintiles Given Mother’s Education in Water-Treated Villages

	Mother’s Education in...				
	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
Δ Fraction (Daughters) with Education in...					
Quintile 1	-0.062	-0.089	-0.033	-0.144	-0.13
Quintile 2	-0.082	-0.005	-0.126	0.057	0.065
Quintile 3	0.057	0.097	-0.014	-0.046	-0.013
Quintile 4	-0.01	-0.015	-0.028	0.002	-0.104
Quintile 5	0.097	0.012	0.2	0.131	0.182
Δ Fraction (Sons) with Education in...					
Quintile 1	-0.078	0.067	0.007	-0.079	-0.042
Quintile 2	-0.016	-0.087	-0.076	-0.107	-0.013
Quintile 3	-0.004	-0.031	-0.001	-0.014	-0.129
Quintile 4	0.02	-0.098	-0.016	0.071	0.091
Quintile 5	0.078	0.15	0.086	0.129	0.093

Figure 6: The Effects of Toilet Improvement on Children’s Education by Mother’s Education Quintile



Note: Event studies of heterogeneous effects on the probability of children completing grade for age by gender. Stacked TWFE estimators of Equation 1 and their 95% confidence intervals are given. Standard errors are clustered at the village level. The sample includes children who were 6–18 years old when they were surveyed.

4.5 Effects on Intergenerational Education Persistence Across Cohorts

We next report results from Specification 4, which uses a cohort DiD strategy to estimate the relative effects of being exposed to toilet and water programs at an earlier age (relative to being exposed older) on intergenerational education rank persistence. Table 7 shows the estimated effects of being exposed under the age of 18 (in Panel A) and under the age of six (Panel B) on rank-rank correlations in years of schooling, for individuals who were at least 12, 16, or 18 years old when last observed in our data.

In line with prior results using time-varying education ranks, we find that being exposed to toilet programs, aged either under 18 or under six, significantly reduced correlations between mother and child education rank. The effects are larger in magnitude for girls than for boys, and are larger in magnitude (although much less precisely estimated) for under-six exposure than for under-18 exposure. For instance, for girls aged 18 to 50, being exposed to a toilet program under the age of six reduced mother-child rank correlation from 0.446 to 0.043, while for boys the correlation reduced from 0.182 to 0.128, with the effect of a toilet program being insignificant at conventional levels.

For the safe drinking water programs, we find larger reductions in education rank persistence for boys, with the effects for girls mostly being statistically insignificant. For boys, we continue to find larger effects of under-six exposure, relative to under-18 exposure. For boys aged 18 to 50, under-six exposure to water programs reduced rank correlations from 0.182 to 0.061, while it had an insignificant effect for girls. Finally, for the toilet program, we find that the effects are largest when we use the sample of 18- to 50-year-olds who were last observed. As most individuals in rural PRC finish their schooling before or by the age of 18, this sample is most likely to yield estimates of mother-child correlations in the lifetime stock of human capital.

Table 7: Stacked OLS-TWFE, Effects of Early-Life Exposure on Intergenerational Education Persistence

Aged	(1)	(2)	(3)	(4)	(5)	(6)
	Girls			Boys		
	12–50	16–50	18–50	12–50	16–50	18–50
Panel A: Under-18 Exposure						
$MotherRank_{ivct} \times 1[AgeToilet \leq 18]$	-0.104* (-0.053)	-0.137 (-0.085)	-0.316*** (-0.104)	-0.106*** (-0.037)	-0.115** (-0.045)	-0.101* (-0.054)
$MotherRank_{ivct} \times 1[AgeWater \leq 18]$	-0.047 (-0.058)	-0.105 (-0.091)	0.089 (-0.1)	-0.060* (-0.033)	-0.037 (-0.036)	-0.02 (-0.04)
$MotherRank_{ivct}$	0.207*** (-0.072)	0.418*** (-0.116)	0.453*** (-0.131)	0.181*** (-0.038)	0.170*** (-0.047)	0.182*** (-0.051)
Panel B: Under-6 Exposure						
$MotherRank_{ivct} \times 1[AgeToilet \leq 6]$	-0.158* (-0.081)	-0.245 (-0.15)	-0.403* (-0.221)	-0.110** (-0.05)	-0.086 (-0.065)	-0.054 (-0.064)
$MotherRank_{ivct} \times 1[AgeWater \leq 6]$	-0.041 (-0.09)	0.027 (-0.105)	0.274* (-0.152)	-0.148*** (-0.047)	-0.134** (-0.054)	-0.121** (-0.058)
$MotherRank_{ivct}$	0.206*** (-0.071)	0.412*** (-0.117)	0.446*** (-0.132)	0.180*** (-0.038)	0.169*** (-0.047)	0.182*** (-0.051)
Village FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y
Age Quadratic	Y	Y	Y	Y	Y	Y
N (Obs)	12,626	8,897	7,020	30,352	25,219	22,779
N (Clusters)	157	141	128	171	168	167

Notes: Regression results from Equation 4, showing the estimated associations between the mothers' percentile education rank and the children's percentile education rank. All ranks are normalized relative to individuals' own birth cohorts. *AgeToilet* and *AgeWater* are individuals' age in the years the toilet and water programs were introduced. Standard errors are clustered at the village level.

4.6 Effects on Intergenerational Health Persistence and Upward Health Rank Mobility

We find that toilet improvement programs reduced intergenerational education persistence and increased upward education mobility. This may be because the program also affected intergenerational health persistence. A better sanitary environment may break the genetic transmission of the mother's physical health on the child, which, through the causal effect of health on education, could break the mother-child education link (Bhalotra and Rawlings 2013). We directly test this hypothesis by examining changes in intergenerational persistence of an individual's height, which is regarded as a summary measure of child nutrition and disease incidence (Bozzoli, Deaton, and Quintana-Domeque 2009). Table 8 estimates changes in rank-rank height persistence (where we use standardized height ranks for mothers' and children's own birth cohorts) using Equation 3. We find that the toilet program had weakened the intergenerational rank persistence of height for girls, as indicated by the negative and statistically significant coefficient on $MotherRank_{iv} \times PostToilet_{vt}$. We find little evidence of a similar reduction in intergenerational height persistence between mothers and children after water programs.

Table 8: Stacked OLS-TWFE, Height Rank Persistence (Time-Varying Rank)

	(1)	(2)	(3)	(4)
	Girls		Boys	
$MotherRank_{iv} \times PostToilet_{vt}$	-0.084** (-0.037)	-0.027 (-0.026)	0.010 (-0.034)	0.024 (-0.018)
$MotherRank_{iv} \times PostWater_{vt}$	0.025 (-0.031)	0.000 (-0.021)	0.024 (-0.033)	0.014 (-0.02)
$MotherRank_{iv}$	0.118*** (-0.033)	0.118*** (-0.025)	0.082*** (-0.031)	0.087*** (-0.023)
Constant	41.865*** (-1.448)	41.881*** (-1.105)	48.601*** (-1.358)	48.401*** (-1.018)
N (Obs)	85,619	85,619	102,998	102,998
N (Clusters)	180	180	181	181
Village FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Cohort FE	N	Y	N	Y

Notes: Regression results from Equation 3, showing the estimated associations between the mothers' percentile height rank and the children's percentile height rank. All ranks are normalized relative to individuals' own birth cohorts. Standard errors are clustered at the village level.

Next, we use transition matrices to study potential changes in the upward mobility of height, without imposing linearity assumptions as our regression specifications do. Table 9 presents transition matrices in villages exposed to toilet programs, which measure the change in the fraction of sons (daughters) in each quintile (given the mothers' height quintile) after the toilet program. When using this approach, we find evidence of an increase in the upward mobility of height after the program for both boys and girls. In particular, there was a 25.0% increase in the fraction of girls who made it to the top quintile of height distribution, given their mothers were in the bottom quintile;

similarly, there was a 20.9% increase in the fraction of boys whose mothers were in the bottom quintile and who made it to the top quintile.¹⁴

Table 9: Change in Conditional Fraction of Children in Education Quintiles Given Mother's Height in Toilet-Treated Villages

	Mother's Height in...				
	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
Δ Fraction (Daughters) with Height in...					
Quintile 1	-0.272	0.138	-0.121	0.093	-0.038
Quintile 2	-0.067	-0.188	-0.159	-0.045	-0.140
Quintile 3	0.031	-0.138	-0.053	-0.151	-0.135
Quintile 4	0.059	0.004	0.174	-0.163	-0.046
Quintile 5	0.250	0.184	0.159	0.266	0.358
Δ Fraction (Sons) with Height in...					
Quintile 1	-0.175	-0.137	0.018	-0.159	-0.258
Quintile 2	-0.163	-0.027	-0.158	-0.011	-0.055
Quintile 3	0.000	-0.115	-0.047	0.035	-0.024
Quintile 4	0.128	0.083	-0.021	0.014	-0.086
Quintile 5	0.209	0.196	0.208	0.121	0.423

Table 10: Change in Conditional Fraction of Children in Education Quintiles Given Mother's Height in Water-Treated Villages

	Mother's Height in...				
	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
Δ Fraction (Daughters) with Height in...					
Quintile 1	-0.008	-0.065	-0.13	-0.026	-0.187
Quintile 2	-0.113	-0.004	-0.07	-0.008	-0.074
Quintile 3	0.001	-0.033	-0.022	-0.024	-0.022
Quintile 4	0.071	0.058	0.067	-0.068	-0.018
Quintile 5	0.050	0.044	0.155	0.126	0.302
Δ Fraction (Sons) with Height in...					
Quintile 1	-0.075	-0.09	-0.185	-0.108	-0.042
Quintile 2	0.003	-0.202	0.017	0.026	-0.026
Quintile 3	-0.033	0.009	0.01	0.019	0.059
Quintile 4	0.020	0.024	0.003	0.003	-0.008
Quintile 5	0.085	0.258	0.154	0.06	0.017

Table 10 shows a similar transition matrix for changes in conditional fractions after children were exposed to water programs. In line with the education transition matrices, we find a much smaller, albeit positive, increase in the fraction of children whose mothers were in the bottom height quintile and who made it to the top height quintile. This was a 5.0% increase for girls and an 8.5% increase for boys. In summary, for

¹⁴ Appendix Table A5 presents transition matrices in villages exposed to toilet programs, before and after their introduction. Prior to the program, in treated villages, the fraction of daughters (sons) born to mothers in the lowest height quintile remaining in the lowest quintile was 43.9% (32.9%), whereas 2.1% (11.8%) could move into the top quintile. After the program, the fraction of daughters (sons) born to mothers in the lowest education quintile remaining in the lowest quintile changed to 16.7% (27.1%), and 15.4% (32.7%) had reached the top quintile.

the toilet program, we find evidence of a reduction in intergenerational height persistence, and an increase in the upward mobility of height, between mothers and daughters, although we find less conclusive evidence for boys. For the water program, we find less support for a reduction in intergenerational height persistence. Based on this, we conclude that the breaking of the link between maternal and child health is a potential mechanism through which toilet improvement programs had weakened intergenerational education persistence.

5. MECHANISMS

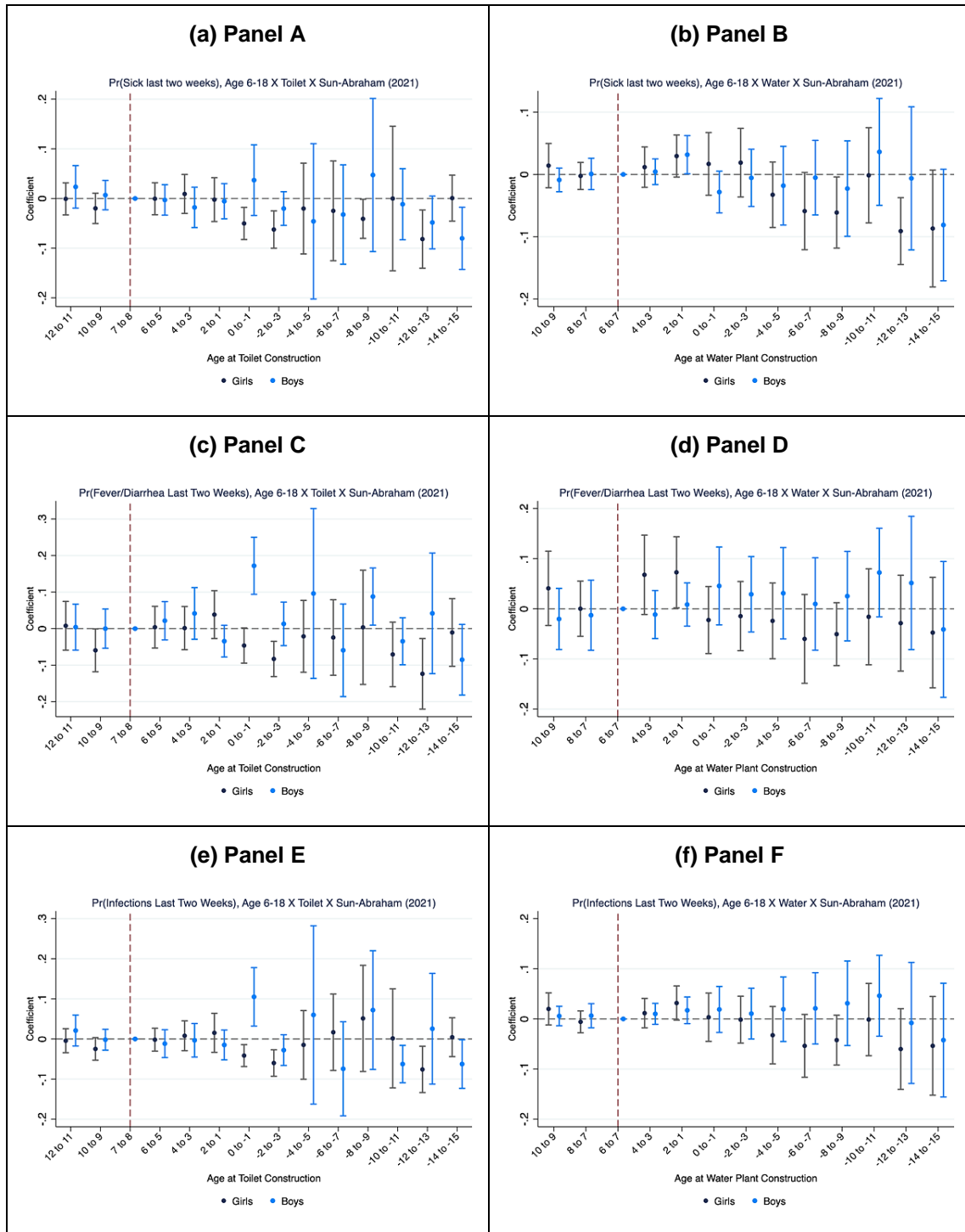
5.1 Cohort Difference-in-Differences: Effects on Health

We now examine the effects of exposure to water and/or sanitation programs at different ages on children's health outcomes to provide the underlying mechanisms through which toilet and/or water improvements increase children's educational attainment. Figure 7 shows the estimated effects of exposure to both programs on the probability that the child experienced any sickness four weeks prior to being surveyed, decomposed by whether they experienced fever/diarrhea and infection symptoms. The left panel always shows the effects of exposure to toilet improvements, while the right panel shows the effects of water improvement programs. For toilet plant programs, we find no significant changes in the probability of being sick for children who were exposed at the age of nine or older (relative to those exposed between seven and eight). We observe a significant fall in the probability of experiencing any sickness, fever/diarrhea, and infection symptoms for girls who were exposed to toilet construction at the age of nought or younger, although we find no significant effect of exposure to toilet construction at the age of one to five. For those born after toilet construction in their resident villages, we also observe falls in sickness, although we lack the statistical power to detect significant effects.

For water plant programs, we observe weaker but negative effects from early-life exposure (defined as being exposed before the age of six) on children's probability of being sick. There seemed to be a longer lag between the timing of water plant construction and its effects on sickness; children who were born around the time of water plant construction saw little significant reduction in sickness. However, we find little evidence that points to boys benefiting (in terms of reduced sickness) from exposure to water plant or toilet construction programs.

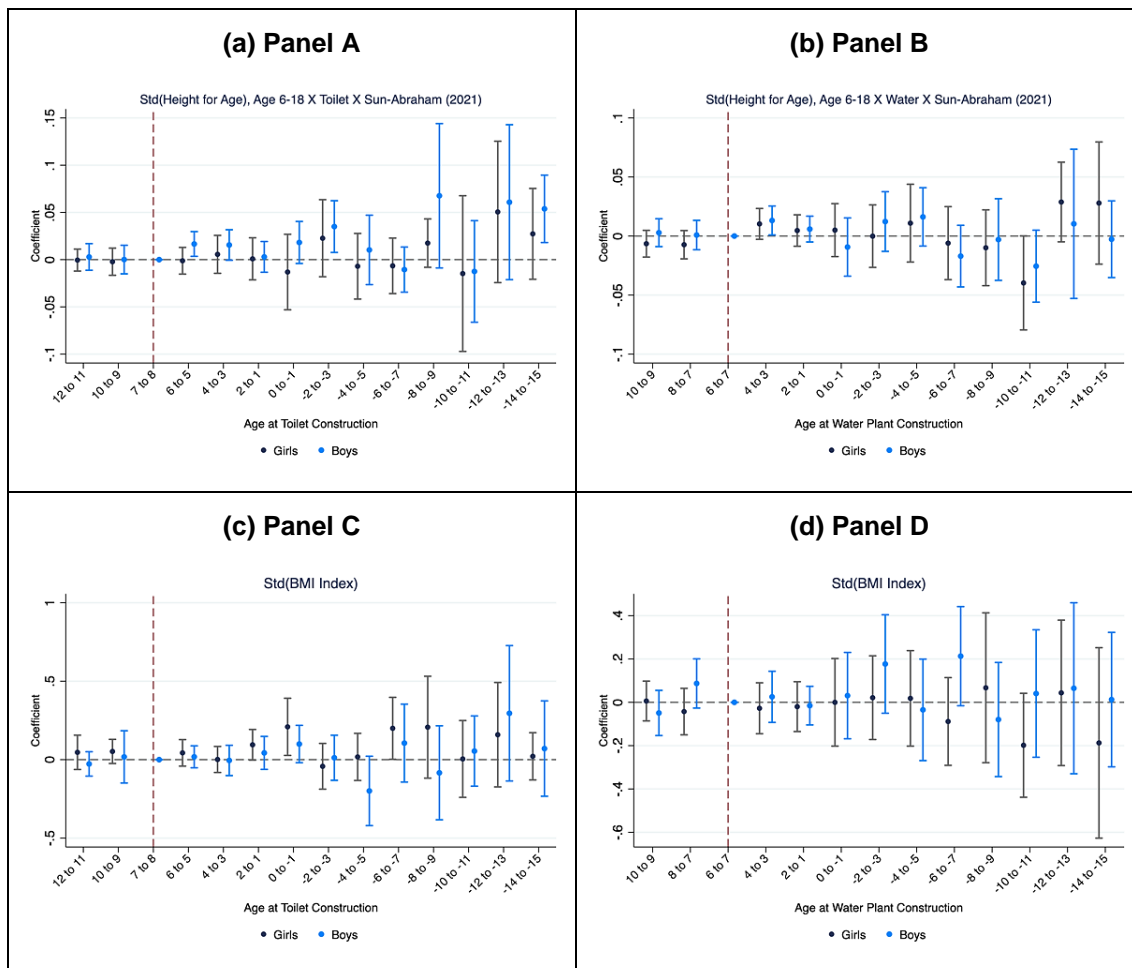
We next assess the effects of exposure on children's height (standardized height for age) and weight (standardized BMI), as shown in Figure 8. Here, we find that boys who were exposed to toilet construction at/before the age of six experienced a significant increase in their height-for-age index (0.02 to 0.04 standard deviations). Although point estimates for girls are also positive, they remain statistically insignificant. As for BMI index, we find that exposure to toilet construction before the age of six increased the BMI index for girls more than for boys. For the effects of exposure to water plant construction, we find that exposure under the age of six also had positive effects on the height-for-age and BMI index, but the coefficients are mostly statistically insignificant.

Figure 7: Cohort DiD, Event Study for the Effects on Children’s Sickness



Note: Event studies of heterogeneous effects on children’s sickness (in total and by cause) by gender. Sun and Abraham (2021) event-study estimates and their 95% confidence intervals are shown. Standard errors are clustered at the village level. The sample includes children aged 6–18 in the survey year.

Figure 8: Cohort DiD, Event Study for the Effects on Children’s Height for Age and BMI

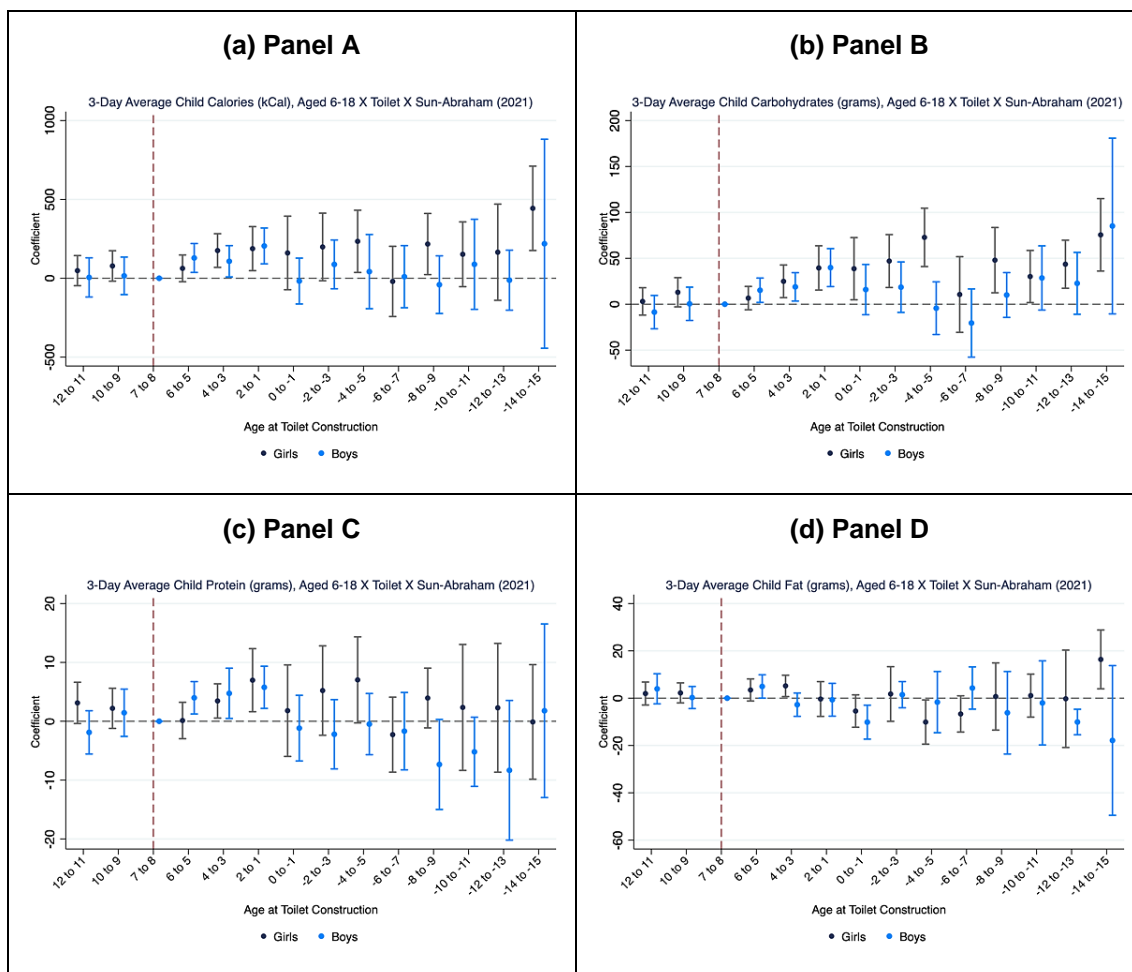


Note: Event studies of heterogeneous effects on children’s standardized height-for-age index and BMI index (weight/m²) (in total and by cause) by gender. Sun and Abraham (2021) event-study estimates and their 95% confidence intervals are shown. Standard errors are clustered at the village level. The sample includes children aged 6–18 in the survey year.

Finally, we turn to children’s nutritional status based on three-day food diaries measured at each survey wave. The effects of a toilet construction program on nutritional status are shown in Figure 9, while the effects of water programs are shown in Figure 10. We look at changes in the children’s three-day average calorie intake, and the level of protein, fat, and carbohydrate intakes (all measured in grams). The unique calorie data allow us to test indirectly whether children experienced improved health through being able to consume more calories and being less likely to suffer from malnutrition. We find that exposure to the toilet construction program under the age of six significantly increased the amount of calories consumed by both boys and girls. The size of the increase in calories is large: The average effect is around 200 kCal for girls (11% of the mean, which is 1825 kCal) and 150 kCal for boys (7.5% of the mean, which is 2000 kCal). This is mostly driven by a significant increase in both protein and carbohydrate consumption. Under-six exposure to toilet construction increased girls’ protein consumption by around 7 grams (13% of the mean) and carbohydrate consumption by 50 grams (16.7% of the mean). It increased boys’ protein consumption by 5 grams (9% of the mean), and carbohydrate consumption by 30 grams (9% of the

mean).¹⁵ By contrast, we see a very small, positive, and marginally significant effect of early-life exposure on fat consumption. This addresses concerns about children simply consuming more energy with a deterioration in nutritional balance.¹⁶

Figure 9: Cohort DiD, Event Study for the Effects of Toilet Construction on Children’s Nutritional Intake

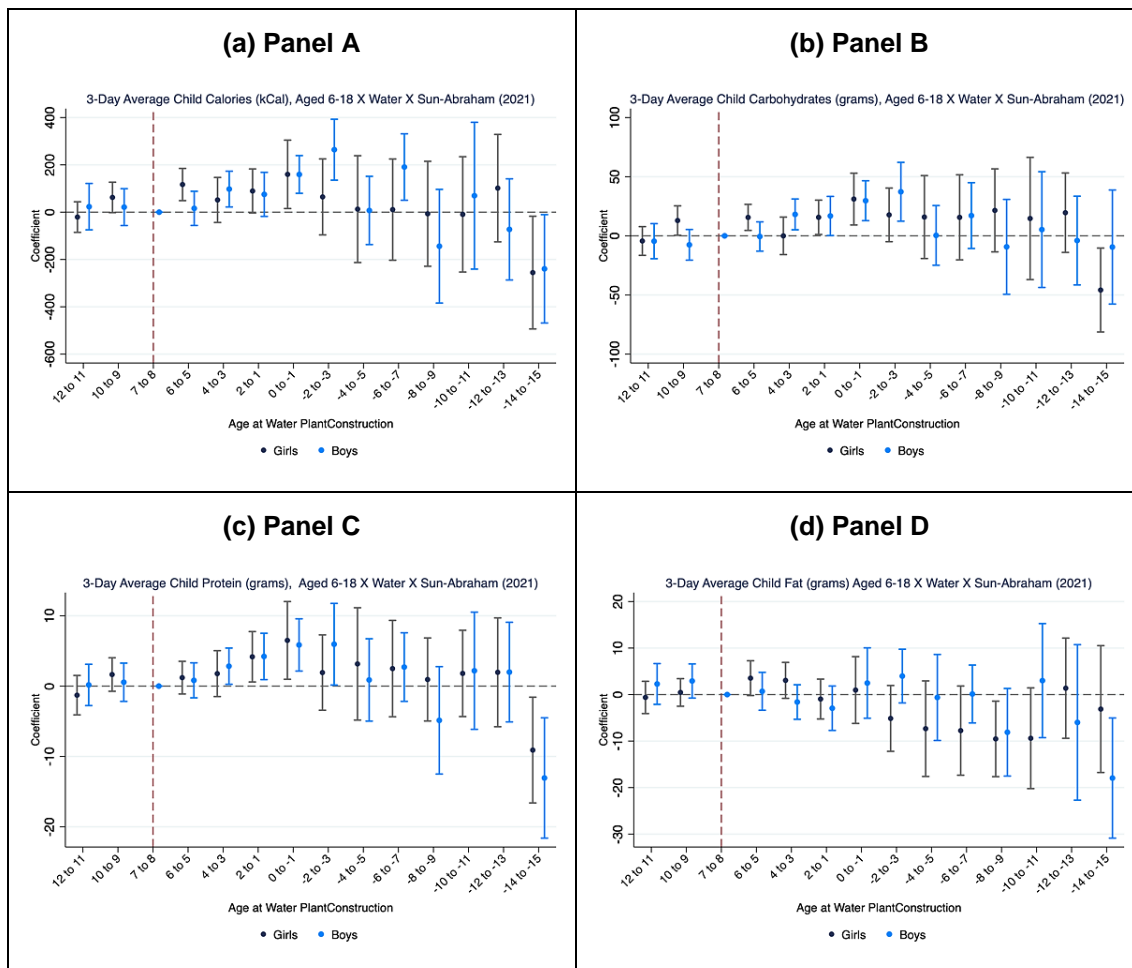


Notes: Event studies of heterogeneous effects on children’s three-day average calorie intake, and the level of protein, fat, and carbohydrate intakes by gender. Sun and Abraham (2021) event-study estimates and their 95% confidence intervals are shown. Standard errors are clustered at the village level. The sample includes children who were 6–18 years old in the survey year.

¹⁵ In our sample, girls aged 6–18 consume 1825 kCal, 54 g of protein, 295 g of carbohydrates, and 47 g of fat. Boys consume 2000 kCal, 52 g of protein, 320 g of carbohydrates, and 52 g of fat.

¹⁶ An alternative explanation is that there was an increase in parental labor supply and thus income (established in Wang and Shen (2022)), thus leading to increased nutrition. We rule out this alternative explanation by showing that there was no increase in parents’ nutritional intakes after toilet or water programs, which would be expected if the increase in children’s nutrition was driven by a general income effect.

Figure 10: Cohort DiD, Event Study for the Effects of Water Construction on Children’s Nutritional Intake



Notes: Event studies of heterogeneous effects on children’s three-day average calorie intake, and the level of protein, fat, and carbohydrate intakes by gender. Sun and Abraham (2021) event-study estimates and their 95% confidence intervals are shown. Standard errors are clustered at the village level. The sample includes children who were 6–18 years old in the survey year.

In summary, we find that under-six exposure to toilet improvements reduced girls’ probability of being sick, driven by a reduction in both fever/diarrhea and infection symptoms. Exposure to toilet improvements also increased boys’ and girls’ anthropometric outcomes, measured by standardized height-for-age and BMI index. However, we find limited evidence that early-life exposure to water improvement programs significantly enhanced children’s health; it had negative effects on sickness prevalence, but these were only significant for girls, although under-six exposure increased both girls’ and boys’ anthropometric outcomes. This is likely because most Chinese households during this time were accustomed to boiling drinking water for disinfection, potentially reducing the marginal effectiveness of clean water programs in improving child health.

These results point to the role of improving children’s health as a key channel behind the increase in educational attainment we observe. We aim to quantify the role of alternative mechanisms, including changes in parental labor supply, changes in gender-specific returns to education, and potential complementarities between better health and increased parental inputs, in future work.

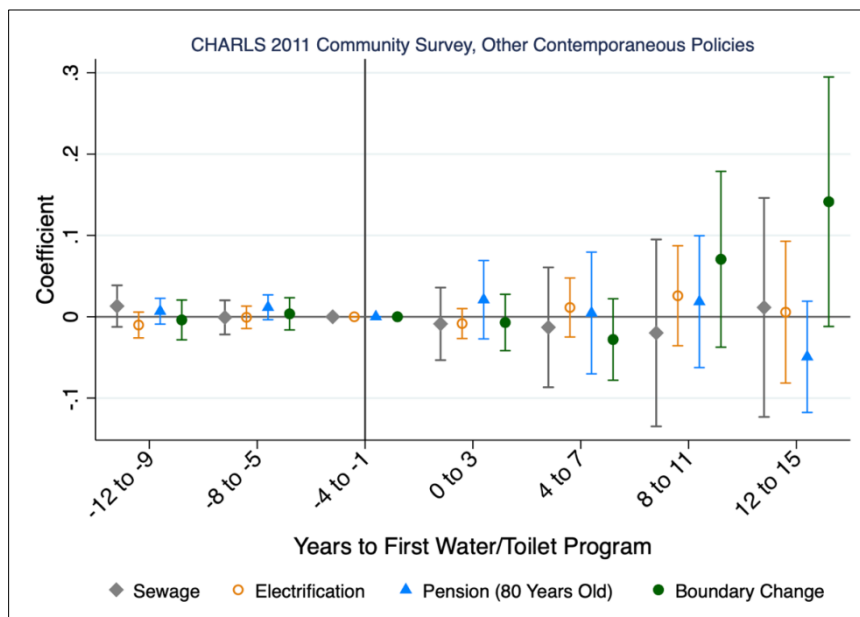
6. ROBUSTNESS CHECKS

Alternative Cutoffs: Our baseline definition of village-level program exposure is based on two conditions: Either a village reached a 75% coverage rate of sanitary toilets/safe drinking water in the first survey year or there was a 15% increase in coverage rate over a period of a year. To check the robustness of treatment definitions and confirm that our results are not driven by arbitrary cutoffs, we estimate our baseline specification (Equation 1) using alternative definitions. We test six treatment definitions, which are combinations of a 50% or 90% coverage rate in the first survey year, and a 10, 15, and 20 percentage point increase per year. Table A6 reports the corresponding results. Reassuringly, across different combinations of our alternative treatment exposure either changing the baseline coverage rate requirement, changing the size of the annual increase, or both had little effect on the size or statistical significance of our results on education.

Contemporaneous Policies: We assess the possibility that our estimated program effects are confounded by the implementation of other government policies that happened around the same time and targeted the same villages. This is a reasonable concern if the government used a multi-pronged approach to improve local sanitation. We first address the concern that the timing of water and sanitation programs may be correlated with introductions of other government policies at the village level, which will bias our estimates. Using the 2011 CHARLS community data, we estimate Sun and Abraham (2021) event studies of the changes in the probabilities that a village was exposed to other programs after it introduced water and toilet programs. The reference group is villages that never had water and toilet programs up to 2011 (survey year). Figure 11 presents the results, which clearly show no evidence of differential trends in program implementation either before or after the water and toilet programs were introduced. These provide evidence that our estimates are unlikely to be confounded by other contemporaneous policies.

We then test changes in public goods provision after the introduction of these two programs. We use annual statistical yearbooks to construct a district-level imbalanced panel data set covering 1989 to 2015. We have measures on school supply (number of preschools, primary, and secondary schools), and health infrastructures (number of hospital beds, doctors and nurses, and health centers). Given the level of statistical records, we observe these variables at the city level, which is higher than our level of program exposure definition. We thus take the first year any village in a given city was affected by the water or toilet program as the city's treatment year. We estimate the same stacked DiD specification on the city-level panel data set to test for potential changes in the supply of these public goods that could have affected children's human capital. As reported in Table 11, we find little evidence of significant changes in these two types of public goods supply in cities after they implemented the water and sanitation program. Although there was a small decline in the supply of hospital beds, this was not reflected in changes in healthcare sector staff or health centers.

Figure 11: Event-Study Estimates of Effects of Toilet and Water Programs on Other Programs



Notes: Event-study coefficients estimated using the Sun-Abraham (2021) estimator. The parameter of interest is the effect of the introduction of toilet and water programs on the probability that a village had introduced any of the four other programs. The “control”/comparison group is villages that were never affected by the toilet and water program up to the survey year. The sample includes 450 communities (rural villages or near rural towns) surveyed by the China Health and Retirement Longitudinal Study in 2011.

Source: CHARLS 2011 Community Survey.

Table 11: Stacked OLS-TWFE, Changes in Other City-Level Public Goods

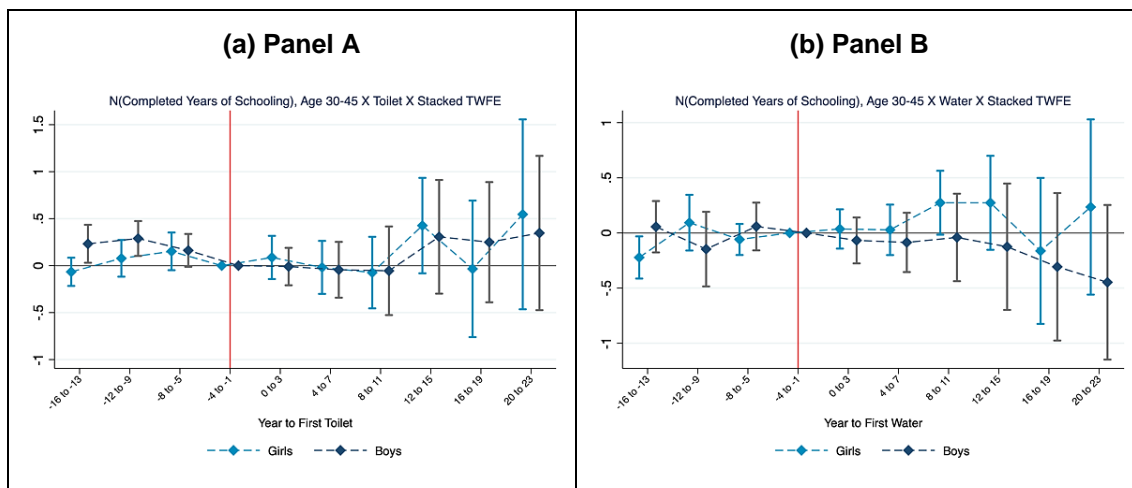
Outcome (in Logs)	Hospital Beds	Health Centers	Health Workers	Doctors	Preschools	Primary Schools	Secondary Schools
Toilet	-0.040*	0.066	0.01	0.029	-0.512	-0.094	0.013
	-0.022	-0.065	-0.028	-0.033	-0.586	-0.078	-0.088
Water	-0.031	-0.011	-0.025	0.025	0.428	0.024	-0.009
	-0.025	-0.097	-0.035	-0.019	-0.404	-0.063	-0.135
N (Obs)	1,669	2,949	1,238	1,697	2,659	2,423	8,999
N (Clusters)	26	26	20	24	30	24	42
Adjusted R ²	0.995	0.878	0.997	0.997	0.742	0.988	0.91
City FE	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y

Notes: The table shows stacked OLS-TWFE DiD regression results on changes in the supply of other city-level public goods following the implementation of toilet or water programs. The sample is a city-year panel data set, where we define city-level program exposure based on the first year any village in the city had introduced water/toilet programs. Heteroskedasticity-robust standard errors are clustered at the city level.

Placebo Tests for Older Cohorts: Our generalized difference-in-differences strategy accounts for individual- and village-level time-invariant heterogeneity (through the inclusion of individual fixed effects) and common shocks (through year fixed effects). Key threats to our identification come from other time-varying shocks correlated with both the timing of the introduction of sanitation programs and children’s long-run outcomes. We perform a placebo test to check for the presence of other time-varying unobservables. Specifically, we use a sample of individuals aged 30 to 45 in each survey year, who were too old (at the time of the programs’ introduction) to have their educational attainment affected. This makes them a plausible placebo sample, as we

can see whether the treated villages exhibited significant differences in *education trends*, which would inform us of the presence of unobservable time-varying shocks likely biasing our estimates. We rerun our baseline event-study models using this placebo sample, and the results are reported in Figure 12. Event-study graphs show no evidence of significant effects of toilet and/or water programs on the educational attainment of this sample of older individuals, providing evidence against potential unobservable shocks that bias our estimates.¹⁷

Figure 12: Event Study for the Effects on Years of Schooling for Cohorts Aged 30–45



Notes: Event studies of heterogeneous effects on the probability of children’s years of schooling. Stacked TWFE estimates from Equation 1 and their 95% confidence intervals are given. Panels A and B plot for coefficients for the toilet program and water program, respectively. Standard errors are clustered at the village source. The sample includes children who were 30–45 years old when they were surveyed.

We conduct a second placebo test by estimating the same stacked DiD specifications for two cohorts too old to be affected: all individuals who were exposed to both programs after they turned 18, and those exposed to programs between the ages of 18 and 22. If there are other time-varying unobservable shocks affecting treated villages, we would expect this to be reflected in changes in educational outcomes for those cohorts too old to have their education affected. While those exposed between the ages of 18 and 22 could still be at university (and thus continue to experience increased education effects), we believe this is unlikely in the rural Chinese setting. This placebo test is also useful for testing potential changes in migration following the two programs: If there were significant migration flows, we should expect this to be reflected in changes in the composition of the village-level population, such as significant changes in the education of village members. Our results, reported in Table 12, show there were small and statistically insignificant changes in educational outcomes for both placebo cohorts (all individuals exposed post-18, and those exposed between the ages of 18 and 22). This further helps address concerns regarding unobservable time-varying shocks and migration responses.

¹⁷ We do not examine school attendance and the probability of completing grade for age with a placebo sample, because these two variables are not available for nonschool-age individuals.

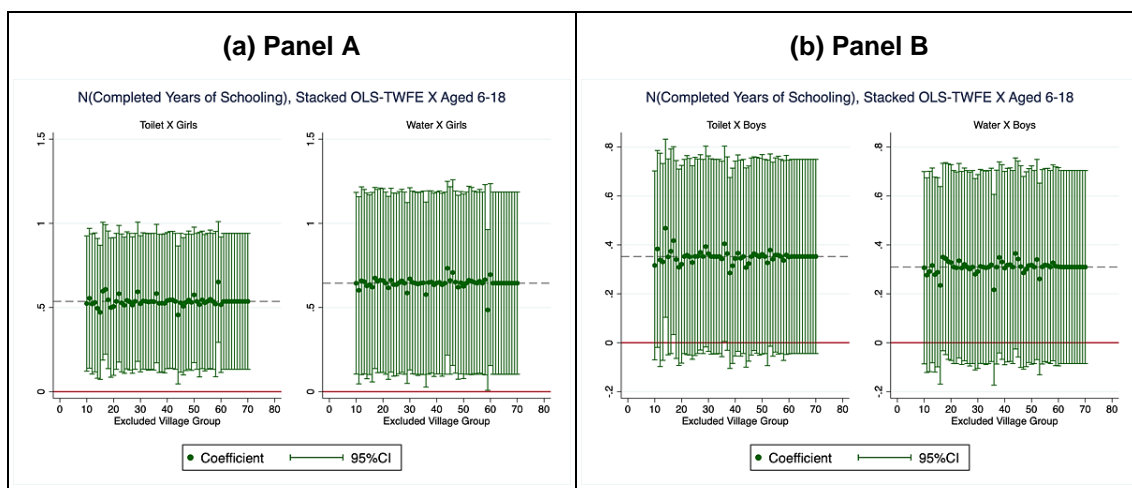
Table 12: Stacked OLS-TWFE, Effects on Education for Placebo Cohorts

	Toilet		Water	
	Girls	Boys	Girls	Boys
Placebo 1: Exposed Post-18				
Post	0.027	0.011	-0.071	0.02
	-0.086	-0.073	-0.086	-0.069
Placebo 2: Exposed 18-22				
Post	0.616	0.436	0.141	-0.149
	-0.436	-0.433	-0.407	-0.526
N (Obs)	457,210	453,740	457,210	453,740
Individual FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y

Notes: The table shows stacked OLS-TWFE DiD regression results (from Equation 1), using a sample of individuals who were (1) exposed to either program after the age of 18, and (2) exposed between the ages of 18 and 22. Heteroskedasticity-robust standard errors are clustered at the village level.

Leave-One-Out Estimates: We next show that our estimates are not dependent on any group of villages that implemented their water and sanitation programs in a given year. We estimate our baseline stacked OLS-TWFE specifications, each time dropping one group (defined by “event source”) of villages based on when they introduced their water/toilet construction programs (there are 61 groups in total). Figure 13 plots estimated coefficients on the toilet and water programs and their 95% confidence intervals, which provides evidence that our estimates are highly robust to dropping any single village group.

Figure 13: Stacked OLS-TWFE Estimates, Dropping One Village Group at a Time



Notes: Average treatment effect estimates (formed by summing up event-study coefficients) from stacked OLS-TWFE specifications. Each panel compares boys/girls in villages that implemented only water/toilet programs with those in villages that had neither program. Each coefficient is from a sample dropping one group of villages that introduced water/toilet programs in a given year. There are 61 “event-source” groups in total. The black dashed line is the average point estimate across all leave-out samples. The sample includes children aged 6 to 18 at the time they were surveyed.

7. CONCLUSION

In rural PRC, children are at risk of not reaching their full potential due to malnutrition and adverse health conditions. As a result, more than a third of children under the age of three are at risk of developmental delay (UNICEF 2018). Among the causes of adverse health, unimproved water and sanitation systems are important factors. In this paper, we leverage the staggered introduction of nationwide public programs to subsidize household sanitary toilets and increase access to safe drinking water (primarily through building water plants), to estimate the long-run effects of improved sanitation and safe drinking water access on children's long-run human capital, which we measure through educational attainment, physical health, anthropometric outcomes, and nutritional status. Our novel contribution is to establish that the programs not only benefited a child on average, but had strong distributional effects by weakening the intergenerational education and height persistence between mothers and children. Our results indicate that both toilet and water programs increased children's educational attainments, with larger effects for girls than for boys. Moreover, toilet improvements reduced intergenerational education persistence for children born to mothers in villages that have experienced toilet construction subsidies. We provide supportive evidence that this was potentially driven by improved child health, irrespective of the mother's health, which weakened intergenerational health persistence, and by differential improvements in child health after being exposed to the program below the age of six.

Putting our results into perspective, our finding that a sanitation program can change intergenerational mobility corresponds well with the existing literature. For instance, Bütikofer, Dalla-Zuanna, and Salvanes (2018) find that the Norwegian oil boom shock increased intergenerational mobility for cohorts entering the labor market at the beginning of the boom in the most affected labor markets, and Feigenbaum (2015) finds that the Great Depression lowered intergenerational mobility for the sons that grew up in the cities, with severe downturns in the US. A key difference in our context is that we show the importance of public sanitation programs, which have largely been advocated on the grounds of improving child health and as a kind of fundamental necessity, and can be quantitatively important in shaping intergenerational mobility in socioeconomic outcomes. While the programs we study are unique in that they were gradually rolled out across almost 20 years, an important question we wish to address in ongoing work is whether more recent sanitation programs implemented in other countries had similar effects on intergenerational mobility. This will contribute to building a body of empirical evidence that improves our understanding of the net benefits of WASH programs across generations.

In 2020, 74% of the global population still lacked safely managed drinking water services and 46% of the world's population lacked safe sanitation. On a global scale, there is a collaborative effort among governments and organizations to ameliorate the WASH infrastructure, with the ultimate goal of ensuring universal access to safe drinking water and adequate sanitation. Our finding that WASH programs can have long-term health and educational benefits can serve as additional motivation for policymakers to subsidize WASH programs. Furthermore, our study reveals that WASH programs can have a positive impact on intergenerational mobility, implying that once up and running, public WASH programs can significantly level the playing field in socioeconomic outcomes across generations.

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APPENDIX: ADDITIONAL FIGURES AND TABLES

Table A1: Village-Level Summary Statistics, Children Aged 6–18

	Treated				Control				Regression		
	Mean	Std. Dev.	N	N	Mean	Std. Dev.	N	N	Mean	Std. Dev.	N
Panel A: Villages with Only Toilet Programs VS Never-Treated Villages											
Total years of schooling	5.23	1.3	114	114	5.38	1.74	263	263	0.06	0.25	0.82
School attendance	0.77	0.19	114	114	0.8	0.21	264	264	-0.02	0.03	0.5
Prob(Completing grade-for-age)	0.35	0.17	114	114	0.33	0.22	272	272	0.02	0.03	0.51
Mother total years of schooling	5.63	2.1	99	99	6.74	2.27	241	241	-0.66	0.6	0.28
Prob(Sickness)	0.09	0.17	111	111	0.07	0.17	261	261	-0.04	0.02	0.03
Prob(Contagious diseases)	0.39	0.43	79	79	0.2	0.35	188	188	-0.01	0.05	0.86
Height	136.18	11.76	108	108	141.33	12.99	259	259	1.39	1.34	0.3
Weight	32.05	8.56	110	110	37.19	10.75	258	258	3.2	1.03	0
BMI	16.67	1.77	108	108	17.77	2.39	258	258	1.04	0.2	0
Household income	11,861.11	14,231.28	114	114	15,357.42	21,461.53	272	272	8,906.59	2,612.44	0
Panel B: Villages with Only Water Programs VS Never-Treated Villages											
Total years of schooling	5.13	1.54	136	136	5.38	1.74	263	263	-0.49	0.28	0.09
School attendance	0.76	0.16	137	137	0.8	0.21	264	264	-0.01	0.02	0.62
Prob(Completing grade-for-age)	0.36	0.17	137	137	0.33	0.22	272	272	-0.03	0.03	0.36
Mother total years of schooling	4.62	2.48	109	109	6.74	2.27	241	241	-1.31	0.54	0.02
Prob(Sickness)	0.05	0.14	128	128	0.07	0.17	261	261	-0.02	0.03	0.39
Prob(Contagious diseases)	0.3	0.42	61	61	0.2	0.35	188	188	-0.16	0.06	0.01
Height	136.39	13.52	128	128	141.33	12.99	259	259	0.44	1.86	0.81
Weight	34.15	8.41	129	129	37.19	10.75	258	258	1.36	1.4	0.34
BMI	17.67	1.82	128	128	17.77	2.39	258	258	0.64	0.43	0.14
Household income	6,563.6	8,403.42	137	137	15,357.42	21,461.53	272	272	1,322.79	2,311.81	0.57

Notes: The table shows summary statistics at the village-year level, using a sample of 6- to 18-year-olds in each survey year. The summary statistics are based on the sample of observations prior to the introduction of water/toilet programs. In Panel A, we compare toilet-only to never-either villages; in Panel B, we compare water-only to never-either villages. Regression coefficients, standard errors, and p-values on the right are from regressions of outcomes on treatment indicators, controlling for county and year fixed effects. Heteroskedasticity-robust standard errors are clustered at the village level.

**Table A2: Conditional Fraction of Children in Education Quintiles
Given Mother's Education in Toilet-Treated Villages**

Panel A: Treated Villages Prior to Treatment					
	Mother's Education in...				
	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
Fraction (Daughters) with Education in...					
Quintile 1	0.306	0.224	0.224	0.225	0.137
Quintile 2	0.203	0.172	0.244	0.169	0.157
Quintile 3	0.247	0.216	0.220	0.191	0.235
Quintile 4	0.200	0.302	0.201	0.258	0.392
Quintile 5	0.044	0.086	0.110	0.157	0.078
Fraction (Sons) with Education in...					
Quintile 1	0.254	0.278	0.253	0.166	0.222
Quintile 2	0.193	0.209	0.232	0.182	0.127
Quintile 3	0.256	0.296	0.232	0.230	0.254
Quintile 4	0.221	0.122	0.196	0.289	0.286
Quintile 5	0.076	0.096	0.088	0.134	0.111
Panel B: Treated Villages Post-Treatment					
	Mother's Education in...				
	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
Fraction (Daughters) with Education in...					
Quintile 1	0.162	0.261	0.272	0.316	0.159
Quintile 2	0.216	0.174	0.136	0.000	0.085
Quintile 3	0.189	0.130	0.136	0.053	0.207
Quintile 4	0.216	0.000	0.198	0.368	0.110
Quintile 5	0.216	0.435	0.259	0.263	0.439
Fraction (Sons) with Education in...					
Quintile 1	0.191	0.240	0.247	0.100	0.231
Quintile 2	0.170	0.120	0.123	0.000	0.154
Quintile 3	0.234	0.080	0.233	0.100	0.138
Quintile 4	0.149	0.240	0.096	0.200	0.200
Quintile 5	0.255	0.320	0.301	0.600	0.277

Table A3: Conditional Fraction of Children in Education Quintiles Given Mother’s Education in Water-Treated Villages

Panel A: Treated Villages Prior to Treatment					
	Mother’s Education in...				
	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
Fraction (Daughters) with Education in...					
Quintile 1	0.311	0.225	0.218	0.223	0.208
Quintile 2	0.267	0.174	0.265	0.107	0.221
Quintile 3	0.177	0.208	0.184	0.289	0.195
Quintile 4	0.180	0.270	0.286	0.215	0.260
Quintile 5	0.066	0.124	0.047	0.165	0.117
Fraction (Sons) with Education in...					
Quintile 1	0.314	0.231	0.277	0.243	0.158
Quintile 2	0.229	0.231	0.251	0.189	0.263
Quintile 3	0.189	0.185	0.199	0.288	0.289
Quintile 4	0.189	0.269	0.210	0.189	0.105
Quintile 5	0.080	0.085	0.063	0.090	0.184
Panel B: Treated Villages Post-Treatment					
	Mother’s Education in...				
	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
Fraction (Daughters) with Education in...					
Quintile 1	0.249	0.136	0.185	0.079	0.078
Quintile 2	0.185	0.169	0.139	0.164	0.286
Quintile 3	0.234	0.305	0.170	0.243	0.182
Quintile 4	0.170	0.254	0.259	0.217	0.156
Quintile 5	0.162	0.136	0.247	0.296	0.299
Fraction (Sons) with Education in...					
Quintile 1	0.236	0.297	0.284	0.164	0.116
Quintile 2	0.213	0.144	0.175	0.082	0.250
Quintile 3	0.185	0.153	0.198	0.274	0.161
Quintile 4	0.209	0.171	0.195	0.260	0.196
Quintile 5	0.157	0.234	0.149	0.219	0.277

Table A4: Conditional Fraction of Children in Education Quintiles Given Mother’s Education in Pure Control Villages

	Mother’s Education in...				
	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
Fraction (Daughters) with Education in...					
Quintile 1	0.254	0.199	0.235	0.124	0.117
Quintile 2	0.237	0.204	0.224	0.152	0.105
Quintile 3	0.192	0.230	0.205	0.215	0.175
Quintile 4	0.229	0.231	0.211	0.313	0.314
Quintile 5	0.088	0.137	0.124	0.195	0.288
Fraction (Sons) with Education in...					
Quintile 1	0.268	0.259	0.249	0.127	0.205
Quintile 2	0.194	0.195	0.208	0.186	0.176
Quintile 3	0.214	0.227	0.222	0.225	0.186
Quintile 4	0.228	0.204	0.218	0.310	0.216
Quintile 5	0.095	0.114	0.103	0.152	0.217

Table A5: Conditional Fraction of Children in Height Quintiles Given Mother's Height in Toilet-Treated Villages

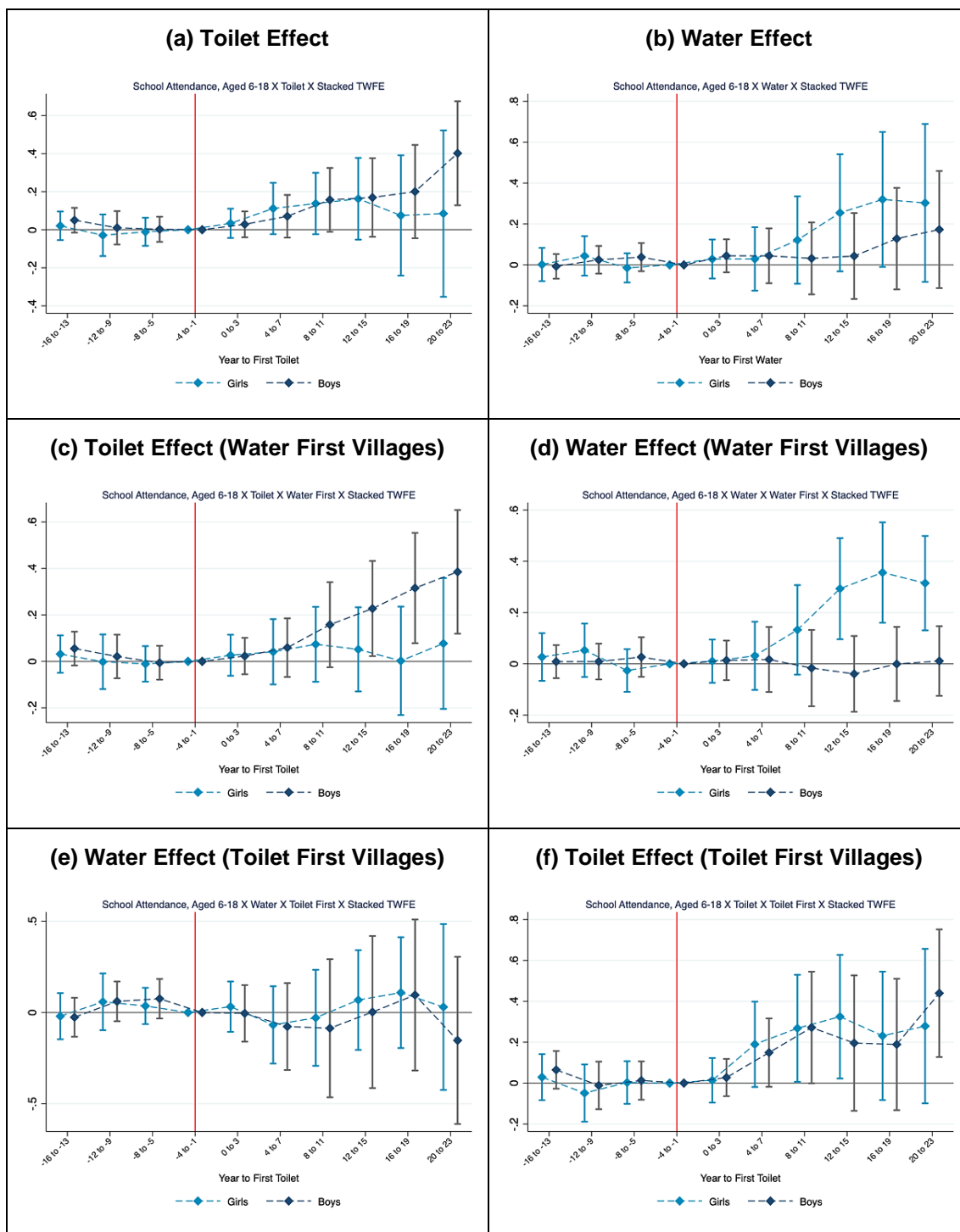
Panel A: Treated Villages Prior to Treatment					
	Mother's Education in...				
	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
Fraction (Daughters) with Education in...					
Quintile 1	0.439	0.206	0.280	0.193	0.102
Quintile 2	0.213	0.250	0.227	0.217	0.203
Quintile 3	0.199	0.294	0.235	0.265	0.390
Quintile 4	0.129	0.184	0.189	0.277	0.237
Quintile 5	0.021	0.066	0.068	0.048	0.068
Fraction (Sons) with Education in...					
Quintile 1	0.329	0.286	0.205	0.266	0.258
Quintile 2	0.259	0.176	0.241	0.190	0.180
Quintile 3	0.153	0.221	0.186	0.215	0.180
Quintile 4	0.141	0.151	0.132	0.165	0.180
Quintile 5	0.118	0.166	0.236	0.165	0.202
Panel B: Treated Villages Post-Treatment					
	Mother's Education in...				
	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
Fraction (Daughters) with Education in...					
Quintile 1	0.167	0.344	0.159	0.286	0.064
Quintile 2	0.146	0.062	0.068	0.171	0.064
Quintile 3	0.229	0.156	0.182	0.114	0.255
Quintile 4	0.188	0.188	0.364	0.114	0.191
Quintile 5	0.271	0.25	0.227	0.314	0.426
Fraction (Sons) with Education in...					
Quintile 1	0.154	0.149	0.222	0.107	0.000
Quintile 2	0.096	0.149	0.083	0.179	0.125
Quintile 3	0.154	0.106	0.139	0.250	0.156
Quintile 4	0.269	0.234	0.111	0.179	0.094
Quintile 5	0.327	0.362	0.444	0.286	0.625

Table A6: Effects of Toilet and Water on Educational Attainments with Alternative Cutoffs

	Percentage Point	50%		90%	
		Girls	Boys	Girls	Boys
Panel A: Schooling					
Toilet	10	0.559*** (-0.177)	0.426** (-0.182)	0.593*** (-0.173)	0.333** (-0.161)
	15	0.383* (-0.207)	0.536*** (-0.185)	0.514** (-0.198)	0.481** (-0.186)
	20	0.471* (-0.270)	0.681*** (-0.191)	0.588** (-0.251)	0.787*** (-0.182)
Water	10	0.465* (-0.248)	0.136 (-0.194)	0.335 (-0.217)	0.261 (-0.170)
	15	0.689** (-0.269)	0.418** (-0.186)	0.436* (-0.242)	0.335** (-0.164)
	20	0.675*** (-0.242)	0.262 (-0.175)	0.361 (-0.224)	0.151 (-0.159)
Panel B: School Attendance					
Toilet	10	0.049 (-0.058)	0.091* (-0.052)	0.064 (-0.060)	0.103** (-0.051)
	15	0.138** (-0.059)	0.163*** (-0.048)	0.170*** (-0.054)	0.173*** (-0.053)
	20	0.164** (-0.067)	0.121 (-0.075)	0.195*** (-0.062)	0.169** (-0.078)
Water	10	0.140 (-0.091)	0.042 (-0.071)	0.084 (-0.080)	0.035 (-0.060)
	15	0.091 (-0.068)	0.082 (-0.060)	0.053 (-0.064)	0.068 (-0.050)
	20	0.146** (-0.066)	0.064 (-0.059)	0.103* (-0.058)	0.072 (-0.054)
Panel C: Prob (Grade-for-Age)					
Toilet	10	0.086** (-0.039)	0.075* (-0.040)	0.100** (-0.041)	0.069* (-0.037)
	15	0.113** (-0.051)	0.099** (-0.044)	0.102** (-0.043)	0.071* (-0.042)
	20	0.121* (-0.070)	0.211*** (-0.058)	0.140** (-0.062)	0.198*** (-0.045)
Water	10	0.062 (-0.052)	0.004 (-0.047)	0.026 (-0.045)	-0.001 (-0.041)
	15	0.102* (-0.052)	0.024 (-0.039)	0.051 (-0.046)	0.034 (-0.038)
	20	0.122** (-0.055)	0.036 (-0.050)	0.097** (-0.047)	0.039 (-0.041)

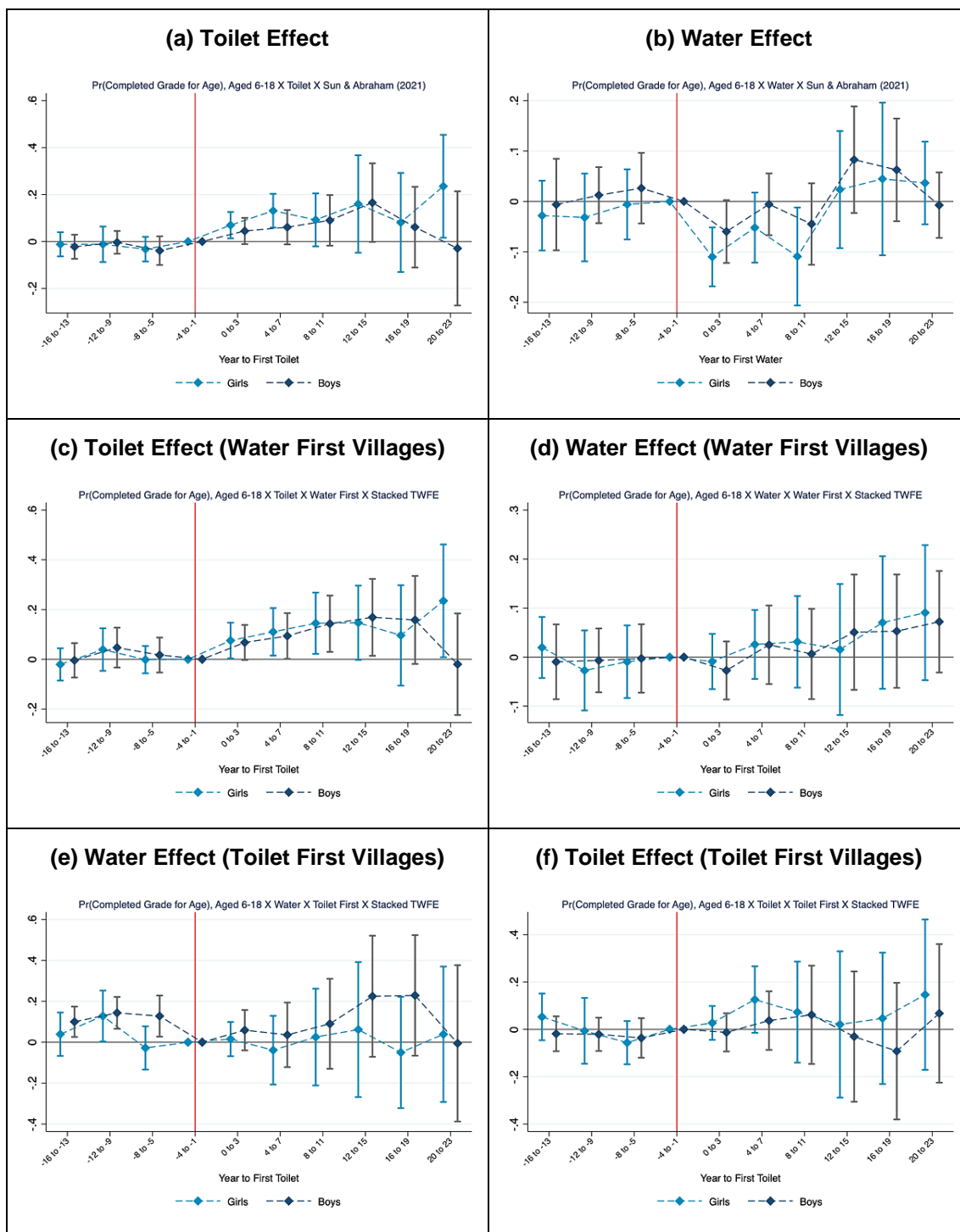
Notes: 50% and 90% indicate that the village has more than 50% and 90% coverage rate of flush toilet/water plant in the first survey wave; 10, 15, and 20 percentage points indicate a 10, 15, and 20 percentage points increase in flush toilet/water plant for each year between two survey waves. "Schooling" means the number of completed years of schooling. "School Attendance" means the probability that the child was attending school in a survey year. Standard errors are clustered at the village level.

Figure A1: Event Study for the Effects on Children’s School Attendance



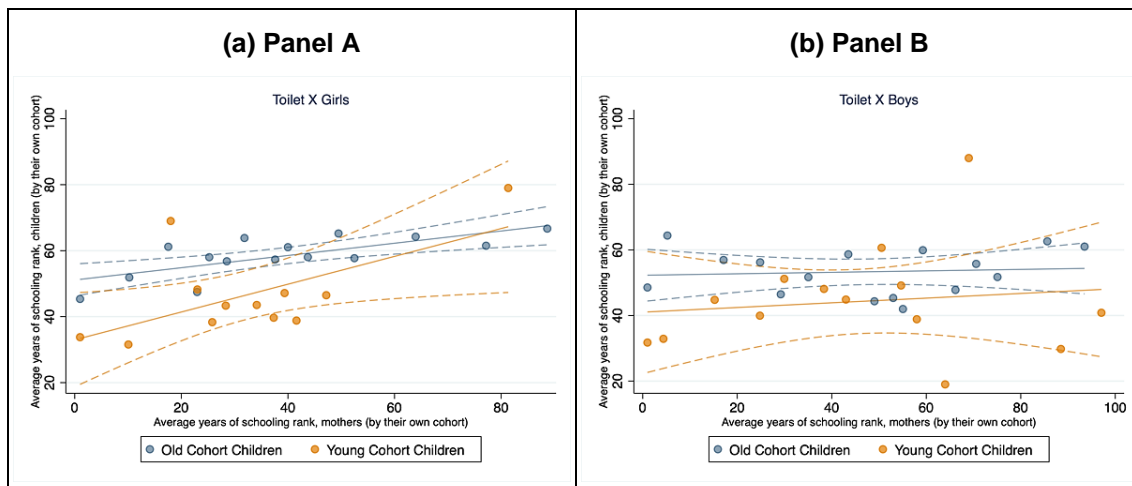
Notes: Point estimates and 95% confidence intervals of OLS-TWFE event studies of heterogeneous effects on children’s school attendance by gender. Panels A and B show water (toilet) program effects from the same regression. Panel C shows toilet program effects in villages that already had water plants when toilet subsidies were rolled out. Panel D estimates the effects of water programs in villages that already had water plants when toilet subsidies were rolled out. Panel E estimates the effects of water programs in villages that had already introduced toilet subsidies. Panel F estimates the effects of toilet programs in villages that had already introduced toilet subsidies. Standard errors are clustered at the village level. The sample includes children who were 6–18 years old when they were surveyed.

Figure A2: Event Study for the Effects on Probability of Children Completing Grade-for-Age



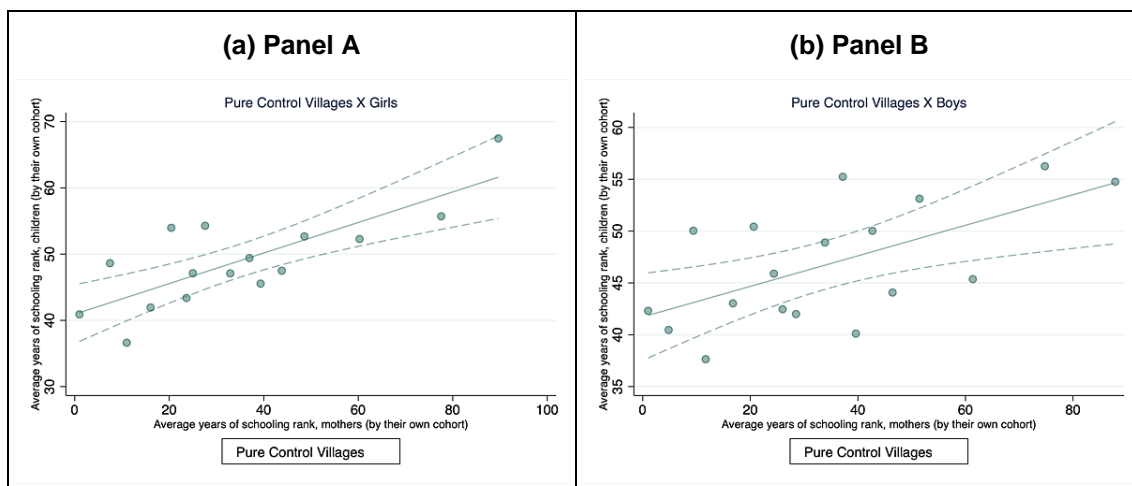
Notes: Point estimates and 95% confidence intervals of OLS-TWFE event studies of heterogeneous effects on probability of children completing grade-for-age by gender. Panels A and B show water (toilet) program effects from the same regression. Panel C shows toilet program effects in villages that already had water plants when toilet subsidies were rolled out. Panel D estimates the effects of water programs in villages that already had water plants when toilet subsidies were rolled out. Panel E estimates the effects of water programs in villages that had already introduced toilet subsidies. Panel F estimates the effects of toilet programs in villages that already introduced toilet subsidies. Standard errors are clustered at the village level. The sample includes children who were 6–18 years old when they were surveyed.

Figure A3: Association between Children’s and Mothers’ Education Ranks in Toilet-Treated Villages for Younger and Older Cohorts



Notes: The plots present binned scatterplots of the relationship between children’s total years of schooling percentile ranks and their mothers’ total years of schooling ranks in treated villages before and after being treated. Children’s and mothers’ total years of schooling are ranked in their own birth cohort’s education years distribution.

Figure A4: Association between Children’s and Mothers’ Education Ranks in Pure Control Villages



Notes: The plots present binned scatterplots of the relationship between children’s total years of schooling percentile ranks and their mothers’ total years of schooling ranks in pure control villages, i.e., neither have toilet nor water programs. Children’s and mothers’ total years of schooling are ranked in their own birth cohort’s education years distribution.