The Heterogeneous Effects of Treated Water on Education: The Rural Drinking Water Program in China[1]

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Abstract

Since little is known about the long-term effect of treated water, we examine the educational benefit to rural youth in China from a major drinking water treatment program started in the 1980's. By employing a data set covering two decades and encompassing more than 4,700 individuals between 18 and 25 years old, we find that, on average, the program increased the completed grades of education by 1.1 years. Moreover, the effect was highly heterogeneous across gender and age of exposure. Girls benefited from water treatment more than boys such that the water treatment program completely eliminated the gender gap in education in treated villages. Young people that had access to treated water in early childhood experienced significantly higher gains in schooling attainment (i.e., by more than a year) than those that gained access at later stages of life. Our analysis suggests that this program was highly cost-effective.

Keywords: Water treatment, education, brawn, gender, early childhood, fiscal programs.
JEL codes: I0, I1, I20, J16, O10.

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1 Introduction

An unfortunate consequence of industrialization in developing countries is the contamination of drinking water, as insufficient control of chemical impurities associated with untreated industrial waste and excessive use of agricultural fertilizers and pesticides have become major water pollutants in more and more countries. About 1.5 million cases of skin lesions and one million cases of skeletal fluorosis per year have been attributed to poor quality of drinking water (World Health Organization, 2004). Industrial contamination of drinking water has contributed to a global drinking water crisis, with 884 million people still relying on unsafe drinking water as of 2008 (World Health Organization, 2011). Despite the significant welfare consequences associated with chemical impurities in drinking water, studies on the benefits of treated water are limited. While there is positive effect of treated water on health (Zhang, 2012), little is known about its effects on *long-term* outcomes such as completed grade of education. To fill this void, in this paper we examine the longer-term benefits on education, specifically the completed grade of education by youth following the rollout of a major rural drinking water program that begun in China during the 1980s. In light of the recent literature that emphasize differential impacts of health interventions on gender due to their gender-specific impact on brawn (Pitt et al. 2012), and the critical importance of early childhood health and nutrition (Almond and Currie, 2011; Cuhna et al., 2006; Heckman, 2008), we also explore how the effect of treated water differs by gender and by age of exposure.

One of the world's most ambitious programs for improving water quality for poor people, this rural drinking water program, starting from the early 1980's, had incurred a total cost exceeding $8.8 billion U.S. dollars by 2002 (Meng et al., 2004), and had covered 300 million people by 2008 (Center for Health Statistics and Information, 2009). The program aimed to build water plants and pipelines to provide rural residents with treated drinking water. A key component of the new water treatment plants was to eliminate chemical contaminants and microorganisms by installing clean water technology and equipment. So far few studies have been conducted to evaluate this important program.[2] In this paper, by employing the longitudinal data of the China

[2] An exception is Zhang (2012), which examines the benefits of this program on health. Despite studying the same program, the major differences between ours and Zhang (2012) lie in the following: Zhang (2012) focuses on the health outcomes of adults and children. Our paper studies the long-run effect of this program on youth's completed grade of education. Moreover, we allow the effects to differ by gender and by age of exposure.

Health and Nutrition Survey (CHNS) from 1989 to 2011, we study the long-term educational benefits of the improvements in drinking water *quality* on rural populations. [3]

Our investigations suggest that young people in villages with access to treated water had better education than those without such access: the completed grade of education among youth increased by 1.1 years. The results are obtained after controlling for local educational policies and resources, household characteristics, and village characteristics such as distances to schools. These results remain robust after dealing with the endogeneity of the water treatment program by using topographic features of villages as the instrumental variable; moreover, we do not find villages in the treatment group and those in the control group differ significantly in key observable characteristics. We further check that the results remain robust after we control for village fixed effects, control for water access (i.e., whether a household has on-premise access to water), and estimate heterogeneous effects by gender and by age of exposure. The main channels through which treated water benefits youth's education are improved health of the youth themselves and early entry into brawn-type jobs of their elder brothers if any, but not time saving due to better *access* to water.

We find strong support for the brawn theory of gender division of labor in Pitt et al. (2012). In particular, boys gained more body mass than girls from using treated water; girls benefited more from water treatment than boys in terms of schooling attainment, and boys and girls with an older brother benefited more than those with an older sister. The program benefited girls much more than boys so that the gender gap in completed grade of education observed in rural China was completely eliminated by the introduction of this program in treated villages.

Young people that had access to treated water in early childhood (i.e., 0-2 years of age) experienced greater gains in education (by around a year) than those without such access until after early childhood, consistent with the recent literature on the critical importance of early childhood development for human capital investment (Almond, 2006; Cunha et al. 2006; Heckman 2008; Maccini and Yang, 2009; Cunha et al., 2010; Almond and Currie 2011). Our estimates also suggest that this program was highly cost-effective.

[3] Here by long-term effect we mean the final effect on educational attainment, and that the effect can be cumulative over decades (such as from age one to two to the educational attainment when he or she is in the early 20s in age). This is in contrast to short-term effects that show up within a couple of years of the treatment.

Our paper contributes to several strands of literature. The *first* is the literature of the effects of health-oriented government programs on youth's final educational achievement. Several studies examine the educational benefits of health programs. For instance, Bleakley (2007) finds that hookworm eradication in the 1910s improved school enrollment of children between 8 and 16 years old, but a significant increase was found only in quality of education instead of quantity of education (years of schooling) over the long term. Lucas (2010) shows that the program of malaria eradication had positive effects on female education and literacy rates of ever-married women in Paraguay and Sri Lanka, while Cutler et al. (2010) find that a similar program of malaria eradication had no such positive effects in Indonesia. Our analysis follows this line of research by studying the long-term effects of the rural water treatment program on education in China.

The *second* is the literature examining the effects of safe drinking water programs. While the literature on water programs is large (e.g., Jalan and Ravallion, 2003; Fewtrell et al. 2005; Galiani et al., 2005; Maimaitwe and Siebert, 2009; Gamper-Rabindran et al., 2010; Kremer et al., 2011), few studies rigorously examine the effect of *water treatment and water quality* on education, especially on final schooling attainment.[4] We add to this literature by focusing on a comprehensive water treatment program in which improvements in water *quality* is a key component. We also examine the long-term effects on children's final schooling attainment and how the effect differs by gender and by age of exposure.

The *third* strand is the literature of human capital investment and the gender division of labor pioneered by Pitt et al. (2012). We add to this literature by providing a dramatic example in which a health intervention results in improvements in education for rural girls to such a greater extent than for rural boys through the brawn channel that the gender gap in education is completely eliminated in treated villages.

The *fourth* contribution is to the literature on the long-term effects of early childhood conditions (see Maccini and Yang, 2009; Cunha et al., 2010; Almond and Currie, 2011). We show

[4] Kosec (2014) uses child-level data from 39 African countries and finds that private sector participation in piped water decreases diarrhea among urban under-five children, and is associated with an 8 percentage point increase in the school attendance of 7-17 year olds. Galiani et al. (2005) also find strong benefits on child mortality of private sector provision of water. Both studies do not examine the impact on final educational attainment. Moreover, the water treatment program in Kosec (2014) examines access to piped water, not the change in water quality.

that exposure to treated water at very young age (i.e., 0-2 years of age) had much more pronounced effect on a person's final schooling attainment than exposure at older ages.[5]

Finally, few studies examine the cost effectiveness of fiscal programs in China. Given the magnitude of China's fiscal pie—its fiscal spending is about 14 trillion yuan (i.e., 2.3 trillion U.S. dollars) in 2013—more evaluations of specific programs are called for, and the current study highlights a program of high cost-effectiveness.

2 The Rural Drinking Water Program in China

Before the 1980s, rural residents in China largely relied on untreated water from wells, rivers, and lakes. More than 70 percent of rural residents in the CHNS dataset drank untreated water in 1989. Sanitation was poor as human and livestock wastes were disposed of freely around dwellings within villages. These unsanitary practices routinely caused endemics of water-related diseases. While microorganisms, the major drinking water contaminants in many other developing countries, have less adverse consequences in China due to the tradition of drinking boiled water and eating cooked food, chemical impurities such as toxic metals and inorganic and organic compounds causes as much health damage in China as elsewhere. Partly the result of geography as natural soil and rock contain high levels of chemicals, chemical impurities in drinking water have been increasingly caused by the rapid industrialization of China, coupled with weak regulations. Vast discharges of industrial waste and excessive use of fertilizer and pesticides have led to widespread water pollution, causing the spread of diseases and sometimes deaths. In 2006, a total of 1,115 counties and about 81.6 million people were reported to be at the risk of fluorosis via drinking water.[6] Around 66,000 people likely die from water pollution in rural China every year (World Bank, 2007).

After completing the urban public water system by the 1980s, the Chinese government started the rural drinking water treatment program, and has spent a significant amount of money trying to improve the *quality* of drinking water in rural China. The ultimate goal of the program has been to build water treatment plants where clean water technology and equipment can be

[5] Maccini and Yang (2009) find that Indonesian women experienced long-term gains in education (and other socioeconomic outcomes) when they had favorable weather shocks at their birth years. No such effects exist for Indonesia men. They interpreted this as evidence of gender discrimination—parents allocated limited food to boys rather than girls in time of food scarcity.
[6] Based on Chinese National Health Statistics (2007).

installed to eradicate chemical pollutants and microorganisms and where government bodies can monitor water quality precisely and regularly. The drinking water from those water treatment plants has been required to meet a variety of standards, including general chemical, toxicological, bacteriological, and radiative indices stipulated by Sanitary Standard for Drinking Water Quality (Ministry of Health, 2007). To avoid water contamination during transport, pipeline systems have also been constructed to deliver water directly from treatment plants to households. Overall, total spending for this rural drinking water program had been about 8.8 billion U.S. dollars from 1981 to 2002, and the cost for the program per capita was approximately 30 U.S. dollars (Meng et al., 2004). There is evidence that the quality of plant water for the treated villages after the implementation of this program is indeed better than that of untreated water (Zhang et al. 2009).

The rural drinking water program is far from being completed, however. Even in 2008, only 42 percent of rural residents had access to treated water, and about 300 million rural people still relied on untreated drinking water (Center for Health Statistics and Information, 2009).

3 Sample, Variables and Estimation strategy

We rely primarily on the China Health and Nutrition Survey (CHNS), which includes nine waves of survey in 1989, 1991, 1993, 1997, 2000, 2004, 2006, 2009 and 2011. The survey covers nine provinces: Guangxi, Guizhou, Heilongjiang, Henan, Hubei, Hunan, Jiangsu, Liaoning, and Shandong. The subsample in each of the provinces was selected based on a multistage, random cluster sampling process. Since the rural drinking water program has only been implemented in rural areas, we only use the rural sample of CHNS.

Our main sample consists of young people between age 18 and 25, a period in which the school-to-work transition for rural youth is largely complete. The starting age of 18 is chosen because China's Compulsory Education Law (CEL), which took effect in 1986, mandates that children must enroll in school by age six (in some areas it can go up to age seven), and thus the vast majority of rural youth would have finished high school had they chosen to do so by age 18. Indeed, based on our sample information, over 80 percent of children at age 15 were still in school, but the majority of people over 25 (87 percent) were working.[7] In our sample only 16.4 percent

[7] According to the survey, people not currently working includes being housewives, the disabled, students, retirees and those on job hunt.

of the individuals in this age group actually graduated from a high school, and less than 5 percent continued study after that. The vast majority of the rural youth clearly did not pursue more schooling beyond middle schools. In this age group, young rural residents thus almost always had already finished their schooling.[8]

We code a village in a particular year as being covered by the water improvement program when either of the two conditions hold: (1) over 80 percent of village households have a water treatment plant as their water source in the first year; (2) less than 80 percent of village households enjoy a water plant in the first wave, but plant coverage rises by more than 20 percentage points per year since the last wave.[9] Once having access, a village is assumed to have access in all subsequent years. Whether a household has access to a water treatment plant is based on a survey question that is answered by the household about its water source, which includes water plants, wells, springs, and rivers. Figure 1 shows the trend of the coverage of treatment plant water in our sample. The coverage of treated plant water in our sample started at the bottom of 20 percent of the sample in 1989, and rose to 47 percent in 2011.[10]

We choose our estimation sample as follows. Since we are interested in the final schooling attainment of rural youth, for each person between age 18 and 25, we only keep *one observation per person*. That observation represents the final year that she or he is in the sample so we know each person's final schooling attainment. Our main regression sample is thus cross-sectional. However, since the final year differs for each individual, we use *t* to indicate this time dimension. Moreover, since we link the final schooling attainment to historical information on water treatment, we do fully take advantage of the original panel data. Our final sample consists of 4,729 observations.

To gauge the effects of the rural drinking water program on youth education, we estimate the following equation:

$$Y_{ivt} = X_{ivt}\beta + T_{vt}\gamma + w_{ct} + \varepsilon_{ivt} \tag{1}$$

[8] As discussed later, our main results remain intact when restricting the sample to be young people of 19-25, or 20-25, or 21-25 years of age.

[9] For example, if over 40 percent of households in a village report that their water sources switched to plant water from 1989 to 1991, then Water Plant is set to 1.

[10] The ratio decreases slightly in 2004 as compared to 1997, from 30.7 to 29.1 percent. This is due to a slight change in the survey areas. Heilongjiang province was initially surveyed as a substitute for Liaoning in 1997, and both provinces were included since 2000. In 2004 CHNS expanded the number of their surveyed villages.

Here the subscript i indicates an individual, v a village, c a county, and t a year. Y_{ivt} is a person's educational status as represented by completed grade of education. X_{ivt} represents the characteristics of individuals and households (e.g., the individual's age, gender, and the relationship to the household head), and of villages.[11] We also control for household income in the first wave rather than the income in the current wave to avoid endogeneity.[12]

T_{vt} is the treatment variable, Water Plant, which is one as long as the village in which he or she resides has access to treated plant water in some part of his or her sample years.[13] Thus, in the base regressions, we do not let the treatment effect depend on the age of exposure to plant water. Later we shall relax this restriction and allow the treatment effect to vary with age of exposure. w_{ct} represents the county-year fixed effects. ε_{ivt} is the error term. A consistent estimate requires the following condition to be satisfied:

$$E(T_{vt}\varepsilon_{ivt}|X_{ivt}, w_{ct}) = 0 \tag{2}$$

We conjecture that the rural drinking water program benefits education in China, especially that of girls. Drinking water of higher quality improves young people's health, allowing them to improve their educational competency through reduction in absenteeism and improvement in mental focus and energy levels (Alderman et al., 2001). Moreover, access to better water quality can improve the health of other household members and, by extension, their income, which may reinforce the educational benefits of the child due to the income effect. However, while this health intervention likely benefits education of the youth, there is no guarantee. Indeed, since health is also positively correlated with individuals' labor market outcomes such as higher wage rates (Thomas and Strauss, 1997), greater labor supply, enhanced self-employment profits, and agricultural productivities (Strauss, 1986), a healthier youth may choose to join the labor market rather than to stay in school. Thus, the program could conceivably have resulted in gains in employment and income yet losses in education.

The tendency toward work instead of school likely differs by gender. Since male youth have more brawn and comparative advantage in brawn-intensive (and unskilled) work (Pitt et al.

[11] The distances to schools were not surveyed in the first wave. Therefore, we use the information in 1991 as the proxies for those in 1989.

[12] Since water treatments also benefit adult health, which further increases household income and youth education, the inclusion of the current household income as an explanatory variable may lead to an underestimation of the program's impacts on the youth—the current household income absorbs part of the benefits of the treatment on the households.

[13] Here the subscript *t* merely indicates that the observation year for each individual could differ. Our treatment variable here is essentially a cross-sectional measure.

2012), the market pull for the unskilled male would be stronger. The male, benefiting from better health by added brawn, may therefore favor brawn-intensive work and forgo schooling to a greater extent than the female. This consideration suggests that the relative educational benefits of water treatment should be smaller for the male. Furthermore, when a rural youth has an elder brother (relative to the case of having an elder sister), the elder brother may gain more brawn from higher water quality after water treatment, and thus make the transition from school to work earlier and raise household income to a greater extent. This additional income effect associated with having an elder brother may benefit the education of the youth to a greater extent than the case of having an elder sister.

An empirical issue is what level of regional fixed effects should be controlled for. This depends on what is omitted in the residual that determines the educational outcomes. We believe the key omitted determinants of local educational attainment are related to the supply side of education; after all, the demand for education could be controlled for more easily by including household and village characteristics. Chinese rural middle schools are mostly administered at the township level, and their financing is provided by township governments, which collected fees mainly from rural parents before 2001. Sometimes middle schools are also administered by county governments, which have relied on intergovernmental transfers since that time (Liu et al., 2009). High schools, in contrast, have always been managed by county governments. The key determinants of local educational attainment are thus county-level educational finance and local preferences for education, both of which are plausibly fixed at the county-year level. We therefore choose to use the full set of county-year fixed effects instead of the village fixed effects.

Another reason not to use village fixed effects in the outcome equation is that 74 percent of villages experienced no changes in the treatment status in the sample period. During the 21 years of CHNS coverage, 89 of the 174 villages had never implemented the water program, 40 of them had treated plant water in all the waves, and only 45 villages changed their treatment status. The employment of village fixed effects implies that the estimation only exploits the variations in those 45 villages in relatively shorter periods of time.[14]

[14] Since the treatment happens in the middle of the sample period, the number of post-treatment years is more limited than the sample of "always treated". This poses another challenge for identifying the long-term effect of the treatment on education.

Moreover, we believe a key source of variation is the within-county inter-village comparisons. In light of our findings presented later that the most pronounced effect of water treatment is observed for youth who had their first exposure to water treatment between age zero and two, relying exclusively on the treatment-status-changing villages has an unfortunate consequence: we rely more on youth who experienced treated plant water at ages older than 2 than in the case of keeping always-treated villages in the sample. In contrast, having the always-treated villages in the sample confers important benefits: the youth in such villages are likely to be covered by this drinking water program for a few years before they first appear in our sample. Thus, many of the youth in these villages may start to use treated plant water before age two, and their inclusion would allow us to better capture the returns to treatment at early childhood. With this consideration in mind, our main empirical strategy is to hold time-variant educational financing and county policies constant, to control for key village characteristics and key household characteristics, and then to attribute inter-village differences in educational outcomes to inter-village differences in the water treatment status. We shall later show that the treatment effects remain robust when controlling for more village characteristics including the village fixed effect, and when dealing with the endogeneity of the water treatment status. Moreover, using the same treatment-status-changing sample, we find that the effect of water treatment remain similar whether we control for county-year or village fixed effects. Finally, we apply the same estimation strategy to individuals beyond schooling age and reassuringly find no effects of the water program, which renders further support that omitted variables may not pose a serious threat to the validity of our estimation strategy.

4 Estimation Results

Table 1 presents the basic descriptive statistics for our sample individuals from age 18 to 25. The average completed grades of education is 8.7 years, slightly less than nine years as required by the Compulsory Education Law. The distances to schools are substantial. While the average distance to a primary school is only 0.6 km, that to a middle school is 2 km, and to a high school is 8.5 km.

4.1 Baseline results

Table 2 presents the county-year fixed effect (FE) regression results. The standard errors are clustered at the village level to allow the unobservable at the individual level to be correlated within a village. We report three sets of results: column (1) only controls for county-year fixed effects; column (2) add distance to various types of schools; and column (3) further add personal and household characteristics. The results are remarkably stable, with the Water Plant effect significant at 1 percent level throughout, and the magnitude ranges from 1.279 to 1.08. Using the specification of column (3), having access to treated plant water is associated with an increase in schooling attained by 1.08 years.[15]

How large is the effect of access to treated water on schooling attainment in our study compared with those of various types of health programs? Miguel and Kremer (2004) find that a less than two-year treatment with deworming drugs in Kenya led to a 0.14 year increase in schooling. Field et al. (2009) find that iodine supplements *in utero* in Tanzania increased schooling attainment by 0.35-0.56 years.[16] The treatment effect as suggested by the baseline regression—a gain of 1.08 years of schooling—thus represents a fairly pronounced effect compared with gains from other health programs. We do note that the educational gain from this program did not materialize instantaneously; rather, it represents cumulative gains from more than 9 years of having access to this program on average.

While here we define Water Plant partly based on the changes in plant water coverage in a village, we do consider alternative definitions, and the results are robust. In our own exploratory runs (see Table A1), we construct a variety of treatment variables by employing different cutoffs of coverage changes or by relying on coverage cutoffs directly. The estimates of the treatment effects remain very similar across these definitions. For example, the estimate of the gain in the completed grades of education is bound by one and 1.2 years across all of these definitions.

A legitimate concern relates to our sample composition. We have used the observation of the last year between age 18 and 25 for each individual as his or her final schooling attainment. Age 18 is generally the year of high school graduation, and the vast majority of rural youth would

[15] The coefficients of other covariates also make sense. Conditioning on Water Plant, females are not disadvantaged. Individuals with parents or grandparents in the household tend to have more education. The household size and the number of children in the household are negatively associated with youth education in general. Young people in wealthier households have more schooling than their poorer peers. The distance to a high or middle school is significantly associated with a lower schooling level.

[16] In Field et al. (2009), the increase in education is not due to improved health status of those babies but improved cognitive skills.

have finished their schooling by then. However, rural children sometimes delay their schooling, which would make graduation from high school slightly later than age 18. We thus repeat our baseline regressions, but the final sample would consist of observation of the last year between the age 19 (or 20, 21) to 25 for each individual. The new restrictions reduce the sample somewhat (to 4572, 4402, 4207), and the estimate of the effect of Water Plant on grades of education completed becomes 1.09, 1.12, and 1.15, respectively, all statistically significant at the 1 percent level.[17]

4.2 Cost effectiveness analysis

While the rural drinking water program clearly had pronounced effect on education, it is unclear whether the program was cost effective. An assessment of costs and benefits of such a program is thus important. Indeed, China's fiscal expenditures have increased dramatically over time: the total government expenditure, 706 billion Chinese Yuan in 1989, rose 14-fold by 2011 (roughly 11 trillion) (China Statistical Yearbook, 2012).[18] With such a grand scale of government spending, it is imperative to understand whether the fiscal resources are allocated cost-effectively in China, which requires knowledge about the returns to various social programs funded by fiscal means. To evaluate the current program and to help guide future fiscal allocation, we thus conduct a back-of-the-envelope analysis of the costs effectiveness of this program.

Taking into consideration that the economic return to education in rural China involves the decisions of off-farm work and migration, de Brauw and Rozelle (2006) find that the average economic return for a year of education in rural China during the 1990s was 10.5 percent for individuals younger than age 35, and the average hourly wage rate for these individuals was 2.69 Yuan. The average monthly wage for young workers was thus 448.3 Yuan in the 1990s.[19] Combined with our estimates, the monetary value of *annual* educational benefit of the rural drinking water treatment program for the youth is 610 yuan, or around 87.1 U.S. dollars.[20] Since the average cost of the program is slightly less than 30 U.S. dollars per capita (Meng et al., 2004), the *annual* return from this investment would be around 290 percent. Given the durability of the water plant for a village, and other benefits associated with the program such as those on health

[17] The table is not reported and is available upon request.
[18] All numbers here are in 2011 values.
[19] The monthly payment of 448.3 is calculated as 2.69*8(hours per day)*20.83(days per month). Here, 20.83 work days per month is stipulated by Ministry of Labor and Social Security of People's Republic of China (2008).
[20] That is, 610 = 448.3*12*10.5%*1.08. Here the exchange rate is assumed to be 7 yuan per dollar, roughly the average in our sample period.

(Zhang 2012), the total returns must be significantly larger. The construction of the program thus proves to be highly cost effective.[21] In the rest of this paper we find the estimates of the effect of Water Plant on completed grades to be between 0.44 (when village dummies are controlled for, and for the treatment-status-changing sample) and 1.69 (for individuals who were first exposed to treated water between the age of zero and two), which translates into annual returns to the program from 118% to 445%.

5 Robustness checks

In this section we conduct a number of robustness checks, including a placebo test, dealing with the endogeneity of water treatment, and considering the possibility of omitted variables.

5.1 A placebo test for older cohorts

Since the program was not randomly assigned, our baseline estimates could be inconsistent due to unobserved variables influencing both the construction of water plants and youth education. A useful falsification test to shed light on omitted variable bias is to examine whether a plant drinking water program is significantly related to the completed grades of education of older individuals whose education is unlikely to be affected by this program. If other latent factors lead to the plant water effects, we would likely see the water program to be significantly related to the education of older individuals as well.

To implement the placebo test, we construct a sample of males whose age was 30 and older when their villages gained access to plant water, and of those males older than 30 whose villages never gained access to plant water in the sample period. We exclude females from the sample because their current locations likely differ from where they lived when they were in school. In contrast, males are relatively stable due to the social norms of male-biased inheritance in China. Similar to the construction of the youth sample, we only keep one observation per individual: his oldest age in the sample. In total, the placebo sample has 2,708 individuals. The outcome variable

[21] As noted, the estimated return from this program is not instantaneous, with more than 9.1 years of exposure to this program on average to reach the full benefit. In addition, we do not take into account the maintenance costs for water plants and pipelines. Such costs, however, are assumed to be smaller than the initial construction costs and taking such additional costs into account should not alter the soundness of our costs-benefit analysis of this program.

is the same as before.[22] The regression results indicate that the "treatment effect" of plant water is statistically insignificant (see Table 3). The placebo test thus supports our identifying strategy.[23]

5.2 Endogeneity of water

It remains possible that the county-year fixed effect regressions may be inconsistent when there is endogenous program placement. Consistent estimation of the causal treatment effects requires the installation of water plants to be exogenous conditional on X_{ivt} and w_{ct}. By employing the county-year fixed effects, we are able to capture the unobservable at the county-year level. However, if some unobservable that varies within counties across years affect both the timing and the location of the program and simultaneously affect education, we would still obtain inconsistent estimates of the treatment effect.

A simple way to evaluate the endogeneity of the treatment placement is to examine whether pre-treatment characteristics between the treatment and the control group are similar. We thus construct the treated group as those villages that experienced a change in the treatment status,[24] and the control group as the never-treated group. Since we control for country-year fixed effects in our base regressions, all pre-treatment characteristics in this exercise are net of the influence of these dummy variables. The results in Table A2 show that the pre-treatment characteristics are very similar between these two groups. Thus, the never-treated and the treatment-status-changing villages are similar in pre-treatment characteristics, rendering support to our county-year fixed effect specification.[25]

Though finding reasonable support for the county-year fixed effect specification, we cannot rule out the potential endogeneity of the treatment. As a way to deal with potential endogeneity, we instrument T_{vt} with a topographic characteristics of villages (i.e., whether the

[22] The average completed grade of education in this sample is 7.3 years. This placebo sample is comparable to the baseline one in their environment. For example, the distance to a middle school in the placebo sample is 2.4 kms, as compared to 2 kms in the baseline sample.

[23] The model specification for this falsification test differs slightly from the baseline specification--some individual and household characteristics are not controlled for due to the lack of data when those older cohorts were in school age. To ensure that the insignificance of the "treatment effects" in this falsification test is not caused by the change in the set of control variables, we apply the same specification here to the male youth between age 18 to 25. The regression results show that the estimated coefficients remain similar to our baseline ones. For instance, the coefficient of Water Plant on grades completed is 0.87 with the extra controls, and 0.9 without them.

[24] Since always-treated villages do not have pre-treatment characteristics, we cannot include them in the treatment group for the pre-treatment test.

[25] This test cannot evaluate whether the always-treated villages have similar pre-treatment characteristics due to the lack of data in pre-treatment periods.

village is geographically flat, hilly or mountainous), which influences the costs of the construction of water plants and pipelines. In non-flat areas, fixed costs are higher since it is more difficult to lay pipes, and high-pressure water pumps must be installed to deliver water. Similarly, in non-flat areas, variable costs are also higher as a large amount of electricity is needed to pump water from plants to villages.

Our key identifying assumption for the instrumental variable estimation is that, conditional on demographic characteristics, initial household income, accessibility of schools, and the county-year fixed effects, the topographic characteristics of the villages should affect people's education only through the water treatment program. Topography, or land gradient, has been shown in the literature as affecting agricultural productivity (Udry, 1996), crop types (Qian, 2008) and infrastructure construction (Duflo and Pande, 2007; Donaldson, 2010; Dinkelman, 2011). It is plausible that these factors may affect individuals' educational status—other than through the water treatment program—mainly through the household income. Therefore, controlling for household income and distances to various types of schools in the regressions can help satisfy the exclusion restriction when using the villages' topography as the instrument. Based on the description of the topography of a village in the CHNS survey, we construct a dummy variable of a village being non-flat as the instrument for Water Plant. The F-statistics for the excluded instrument in the first stage is 21.23 (see Table A3 for the first stage regression results); our topographic instrumental variable is thus not weak.[26] Note that the village's topography was only recorded in the survey in 1991. As a result, the sample size for IV regressions is smaller since the topography of the newly-added villages after 1991 is not available.

The IV results are reported in Table 4. We first report the baseline results. Since the IV sample differs slightly from the baseline sample, in column (2) we replicate the baseline county-year FE results with the IV sample, and the effect of water treatment remains largely identical (1.02 now versus 1.08 for the baseline).

Column (3) displays the IV estimate. The qualitative conclusion from the IV estimate is similar to the baseline estimate, although the magnitude of IV estimate doubles that of the OLS.

[26] As recommended by Baum, Schaffer and Stillman (2007), we compare the statistic to the rule of thumb (10) as well as the Stock-Yogo critical values because critical values for the heteroskedastic-robust Kleibergen-Paap Wald rk F Statistic of the test have not yet been calculated. The critical value of the Stock-Yogo weak identification test is 16.38 for 10% maximal IV size.

Water Plant has a coefficient of 2.41 while the baseline estimate is 1.02 (both are statistically significant at the one percent level).

Despite the seeming disparity of the FE and the IV results, we cannot reject the null hypothesis that the IV and the FE coefficients are identical jointly. In particular, we conduct a bootstrapped Hausman test for the null hypothesis that the FE and the IV estimates are statistically equal (Cameron and Trivedi, 2005).[27] Since the p-value from this test is 0.98, we fail to reject the null hypothesis. The IV result thus render support to our county-year FE result of a positive and statistically significant effect of access to plant water on education.[28] In light of the Hausman test result, we shall stick to the county-year FE specification in all future specifications since it is more efficient.

5.3 Controlling for additional village characteristics

In our baseline specification, we opt to control for the county-year FEs rather than the village FEs to exploit richer variations in data. With the slow rollout of the water program, only 45 out of 174 villages in the sample experienced changes in the treatment status during the sample period. When we use the village FE specification, our identification stems from the before-after changes in the outcomes for these 45 villages (after controlling for other covariates). This strategy thus entails certain risks, e.g., the variations in education and water quality for the always-treated and the never-treated villages (within a county-year cell), though quite informative, are completely ignored in identifying the treatment effects.

Nevertheless, the county-year FE specification does entail risks of omitted variable bias. For example, we may have omitted local village-level labor market conditions in our baseline regressions. Indeed, the youth's education decisions are usually made jointly with their labor supply decision, so their pursuit of education may be influenced by local (i.e., village) labor market conditions such as wage rates or job vacancies. Whether this omission can bias the estimates of

[27] The bootstrapped Hausman test is conducted as follows: (i) estimate OLS and an IV estimates from a bootstrap subsample with the village as the resampling cluster; (ii) repeat this process 1000 times to calculate the standard errors of those estimates; (iii) conduct the Hausman test by using the estimated coefficients using the whole sample and the standard errors obtained in step (ii).

[28] We have also tried using the non-flat dummy interacted with all wave dummies as instruments. The qualitative results are similar to the results with the simple non-flat dummy IV, with significant treatment effects on grades completed (2.22 years). However, the first-stage F statistics become smaller (10.11) which is over 10 but less than the Stock-Yogo weak ID test critical value (20.53) for 10% maximal IV size.

the treatment effect depends on whether these variables are correlated with Water Plant. In its community-level survey, the CHNS contains several features of the local labor market, including daily wages for male, female and construction workers, and the major occupations in which the local residents are engaged. In order to measure village-level real wages, we normalize all nominal wages by the average household daily income in a village.[29] We find that the local real wage rates do not have statistically significant correlations with the water program.[30] In separate (un-reported) regressions, we also control for travel costs (proxied by the road conditions around villages), which may affect the attractiveness of local jobs and thus affect the work-school choice. Even after we control for this proxy of travel costs, there remain no significant correlations between Water Plant and local real wages. Omitted time-varying village-level labor market conditions thus do not seem to affect our key estimate.

Next, we control for village fixed effects. The inclusion of village fixed effects has two adverse consequences: (i) it results in a dramatic drop in the useful sample for identification, and (ii) the 45 treatment-status-changing villages experience a shorter history with treated water, which makes it harder for a new water plant to influence the level of education. This is likely problematic in light of our findings presented later, that the most pronounced effect of water treatment is observed for those who were exposed to water treatment between age zero and two. Relying exclusively on the treatment-status-changing villages would force us to rely more on those who were exposed to treated water at ages older than two. The final sample would then have a smaller share of individuals exposed to treated water at early childhood, and the average treatment effect would be smaller simply because it omits (to a greater extent) the sample individuals who benefit the most from treated water.[31] As a result, we expect to find significantly smaller treatment effects when controlling for village fixed effects as compared to county-year fixed effects.

Controlling for village FEs—but still use the full sample--column (4) of Table 4 confirms our conjecture. Indeed, the magnitude of the treatment effect drops: the gain in completed grades of education changes from 1.08 to 0.44 years. Thus, even with the variation coming from a quarter of the original sample villages and only focusing on their before-after comparison, and ignoring to

[29] Since we do not have the village-level price index, using the average household daily income in a village is the best alternative. Since we have already controlled for county-year dummies, all county-level price variations have been controlled for.
[30] The table is not reported here due to space constraints and is available upon request.
[31] In particular, for data of wave 1997 and after, those individuals who got exposed to treated water at age zero to two would be excluded from the treated sample.

some extent those who began their exposure to treatment at early childhood, we still find a significant effect of Water Plant on grades attained.

The difference in the treatment effects from the village and the county-year FE specifications stems largely from two sources: omitted variables, or treatment effect heterogeneity due to sample composition. Which source is more prominent? To investigate this, in column (5) and (6) we use the treatment-status-changing sample only (i.e., 1586 observations), and control or village or county-year FEs, respectively. Now the sample is held constant, and the difference in the treatment effect can only originate from omitted variables. The coefficient of Water Plant is 0.57 when village FEs are controlled for, and 0.68 when county-year FEs are controlled for. The similarity of the estimates thus point to sample composition and treatment effect heterogeneity as the primary reasons behind the much smaller estimate of the treatment effect when village FEs are controlled for.

5.4 Distinguishing water quality and water accessibility

So far we attribute the significant positive effect of access to plant water to improved water quality. However, the water-education link associated with the program could reflect water *access* instead of water *quality*. Better water access could save a child's time spent on fetching water, thus allow him or her to benefit from more *quantity* of water. Access to plant water through pipelines also gives him or her a larger time budget for attending school and completing school tasks.

We now examine whether the link between plant water and education is due to improved water access. In CHNS, households' water access is categorized into four sources: (i) in-house tap water, (ii) in-yard tap water, (iii) in-yard well, and (iv) other places. The first three options should be considered as optimal water access (Mangyo 2008). We thus create a dummy variable of optimal water access which is one when water is accessible on the household premise.

We re-run our baseline regressions by additionally controlling for the dummy of optimal water access for a household (column (7) of Table 4). Alternatively, we limit the sample to the individuals who have optimal water access for all sample years (column (8) of Table 4). If the main channel of the Water Plant effect is water access, the coefficient of Water Plant should diminish to zero, and the optimal access dummy should be significant and positive with a large magnitude.

The coefficient of Water Plant is only slightly smaller than that in the baseline (when we ignore water access) and remains highly significant (1.01 vs. 1.08). When restricting our sample to those always with access to water in their premises, the treatment effect of 1.04 years is almost identical to the baseline results. Thus, the effect of the plant water program on education is clearly not due to improved water access, or its time-saving effect. This, coupled with the findings in Zhang (2012) that plant water improves health status for both adults and children, suggests that improved health due to plant water (and the indirect effects on household income) is more likely the explanation for the educational gains of access to plant water.

6 Heterogeneous treatment effects across exposure time and genders

In this section we explore the heterogeneous nature of the water treatment effect. In particular, we allow the treatment effect to differ by age of exposure and by gender. We show that the effect of treated water is much higher for girls (than for boys) and for individuals exposed at early childhood (than for those exposed later).

6.1 Treatment effects varying by exposure ages

It is important to explore how the effect of treated water depends on age of exposure as an emerging literature shows that health interventions at early childhood have greater potential to dramatically impact results (Heckman, 2008; Cunha et al. 2006, 2010; Almond and Currie, 2011). The nutrition literature, for instance, emphasizes that the period of early childhood is critical, and that poor nutrition during this period may create deficiencies that are difficult to reverse by better nutrition in later years (Martorell 1995, 1997; Maccini and Yang, 2009). The return on human capital investment during this early childhood period is very high because early childhood intervention fosters cognitive and non-cognitive skills when the brain is undergoing its most rapid development (Heckman, 2008). While there are studies on the long-term impacts of early childhood health or nutritional status (Glewwe and King, 2001; Glewwe et al., 2001; Case et al., 2002; Currie and Stabile, 2003; Currie et al., 2010), and of exposure to health shocks such as infections or drought (Hoddinott and Kinsey, 2001; Bleakley 2007, Chay et al. 2009, Case and Paxson 2009),[32] studies on the long-term effects of early childhood exposure to *treated water* on education are non-existent.

[32] See Almond and Currie (2011) for a summary of the literature on returns to early childhood human capital investment or environment.

To explore the effect by age of exposure, we calculate the earliest year of exposure to plant water for each individual in treated villages. Due to the lack of information, we exclude from our sample those in the always-treated villages who were born before their villages were covered in the CHNS. As a result, the sample is reduced by 665 individuals. With the smaller sample of 4,064 individuals, we explore the effects of earliest age of exposure non-parametrically on the completed grades of education (see Table 5). Column (1) displays the baseline results for comparison purpose. In Column (2), we rerun the baseline regression by using the restricted sample. Not surprisingly, the treatment effect decreases to 0.65 years since we exclude those individuals that have used plant water for the longest time in our sample and whose educational attainment may benefit the most from this water program. Nevertheless, the treatment effect stays statistically significant.

In Column (3), we include a series of dummy treatment variables to indicate individuals' earliest age of exposure to plant water, and rerun the regression. The default group is the individuals in the untreated villages. The results indicate that treated water has the greatest impact when it is available at early childhood, in particular, from birth to age two. On average, a child's completed grade of education increases by 1.69 years if he/she starts to use treated plant water from birth to age two. This impact decreases dramatically to between 0.4 and 0.6 year when the earliest age of exposure is after age 2, with the impact remaining stable at that level after age two.[33] This is apparent in Figure 2, in which we plot the coefficients and the 90 percent confidence intervals of those treatment variables indicating a child's earliest age of exposure. Our finding of the highest return occurring at the pre-school stage (i.e., birth to age 2) is consistent with the literature that suggests that the largest return on human capital investment comes at early childhood (Heckman, 2008; Almond and Currie, 2011). We add to this literature by showing that exposure to treated plant water also exhibits its highest return to education when it begins at early childhood, and that the differential returns can be quite pronounced (i.e., more than one year of schooling attainment).

6.2 Heterogeneous treatment effect across gender

During the sample period, rural Chinese girls were on average less educated than boys. In our data, the average completed grades of education was 8.54 years for girls versus 8.85 years for

[33] Although the coefficient for 3-5 years old is not statistically significant, the F-test cannot reject the hypothesis that it is the same with other coefficients except for that of those that are younger than two years old.

boys. Can the rural water treatment program significantly reduce the gender gap in education? The brawn theory of division of labor (Pitt et al., 2012) would imply so. To check if this is true, we explore the heterogeneous treatment effects by gender.

Columns (1) and (2) of Table 6 presents the results for boys and girls, respectively. We control the same set of covariates as in column (3) of Table 2, that is, with full controls. In general, the treatment effects on girls are substantially larger than those on boys. For example, when their villages gain access to treated plant water, the grades completed increase by 0.87 year for boys but 1.29 years for girls. The girl premium of the treated-water effect, 0.42 years of schooling, represents more than 100% of the girl disadvantage in schooling attainment in our sample (i.e., 0.31).[34] Our analysis thus demonstrates that the rural water treatment program completely eliminated gender disparity in schooling attainment in treated villages.

The stronger effect of the water program on girls than on boys resonates well with recent papers that find stronger effects of health programs on girls.[35] For instance, Miguel and Kremer (2004) find that the spread of deworming drugs among schools in Kenya increased the school attendance instantly for both boys and girls, but this effect lasted in the second year only for girls. Maluccio et al. (2009) show a significant increase in schooling attainment for girls but not for boys in Guatemala after both were treated with nutritional supplements for three years. Maccini and Yang (2009) find that Indonesian women enjoyed long-term benefits in education and other socioeconomic outcomes when they were exposed to favorable weather shocks when they were infants, but this did not hold true for Indonesian men.

The theoretical rationale for greater responses observed in girls from health improvement is provided by Pitt et al. (2012), who emphasize the consequences of gender differences in the level of brawn. As discussed in the biomedical literature, biologically, boys have more brawn than girls, and boys' brawn grows more when their nutritional status has been improved. Thus, young men have comparative advantages over young women in brawn-intensive (as opposed to skill-intensive) jobs, and are more likely to work in such jobs. Since a health intervention generally reduces morbidity and improves an individual's nutritional status, and boys gain more brawn than

[34] The difference in the coefficients of Water plant for boys and for girls for schooling attainment is statistically significant. To see this, we ran another regression in which we used the pooled sample with additional two variables, that is, Girls, and Girls times Water Plant. The interaction term has a magnitude of 0.41, and is statistically significant at the five percent level.

[35] See Pitt et al. (2012) for a summary of other studies finding differential investment in and returns to human capital investment by males and females.

girls, the intervention increases young men's comparative advantage in working in brawn-intensive occupations, and, thus, raises the opportunity costs of their schooling. This naturally gravitates young men toward work and young women toward schooling—at least relatively—and this stronger pull for physical work is amplified by what has been going on in China's labor markets. Over the past three decades, migration from rural areas to the cities has increased dramatically, with rural males gravitating toward brawn-intensive jobs such as construction workers, laborers, and so on. The comparative advantage in brawn-intensive jobs by males thus explains why young men may choose to work more and enjoy lesser gains in education in response to health interventions.

To test the hypothesis of gender differences in the level of brawn and its responsiveness to nutrition, we relate boys' and girls' health status to Water Plant and other control variables. Here the objective is to understand the health effects of treated plant water on those young males and females, so we track their nutritional status recorded in CHNS before age 25 and explore whether access to plant water improves their health and whether this improvement differs by gender. The sample size used for this analysis is 11,169 observations.[36] The results are in Table 7. Both boys and girls gain improvements in their height and body mass (i.e., weight/height) after they have been exposed to plant water. Moreover, boys' body mass has on average increased by 0.55 kg/m, which is larger than such effect on girls (0.39 kg/m).[37] Moreover, there is evidence that the same increase in body mass translates into greater gains in strength for males than for females (Pitt et al. 2011). Thus, boys gain more brawn than girls after treated water becomes accessible. Our findings of gender-specific brawn-responsiveness to plant water therefore offer support to the brawn theory of Pitt et al. (2012).

The brawn theory of gender division of labor has implications with respect to how an elder sibling's gender identity affects education of younger siblings within a family. When treated water improves the health of young people in a household, the theory implies that the boys are more

[36] To construct the sample to examine the health effects of plant water between boys and girls, we track the measures of nutrition of our baseline individuals from age 0 to 25 recorded in CHNS data. Thus, the final sample is panel data of the individuals showing up in the sample for analyzing the effects on education, which is larger than the cross-sectional data we use in the baseline analysis.

[37] We have tested whether the difference is statistically significant by using the pooled sample but adding two new variables, e.g., girl, and girl*Water Plant. For the body mass equation, the interaction term is negative but statistically insignificant (with a t-statistic of 1.27); for the height equation, the interaction term is negative, with a large magnitude (i.e., -1.00, with the coefficient of Water Plant being 1.67) and is statistically significant at the 10 percent level.

likely to choose to work while girls more likely to continue schooling. As a result, young man are more able to contribute to their household financial resources and provide better support to the schooling pursuit of their younger siblings. We thus expect that an individual with an elder brother gains more in education from treated water due to the elder brother's stronger tendency to turn brawn into cash immediately.

Columns (1) and (2) of Table 8 test this hypothesis by presenting the treatment effects by gender of the elder sibling. For our regressions, the sample consists of the individuals who are children of household heads and who have at least an elder sibling. We divide the sample based on the gender of the elder sibling. The treatment effects in terms of grades completed for individuals with an elder brother are more than twice those for individuals with an elder sister (i.e., 1.39 vs. 0.57 years), providing strong support to the brawn theory of the gender division of labor.

6 Conclusion

The rural drinking water treatment program, perhaps the largest such program in terms of the number of people being affected, and the availability of a long panel in CHNS, provide us with a unique opportunity to investigate the impact of treated water on long-term impact on schooling attainment. We find that young people of the post-high-school ages in villages with access to treated water have significantly higher schooling attainment than those without such access--the youths' completed grades of education improved by 1.08 years. The results are obtained after controlling for local educational policies and resources, household characteristics, and village characteristics such as distances to schools. The qualitative results remain robust after addressing the endogeneity of the water treatment program, considering local labor market conditions, controlling for village dummies, controlling for optimal water access, and estimating the effects by gender and by age of exposure. The placebo test on adults' educational attainment level also supports our identification assumption.

In addition, three findings support the brawn-based theory of gender division of labor in Pitt et al. (2012). First, females benefit much more from water treatment than males in terms of schooling attainment (1.29 vs 0.87 years). Second, youth with an older brother benefit more in educational attainment than youth with an older sister. Third and finally, boys gain more in body mass than girls after water treatment. The brawn theory proves to be quantitatively important as the water treatment program completely eliminates the gender gap in education in the villages with access to treated water.

Significantly, rural people who began their exposure to treated water in early childhood (i.e., 0-2 years of age) increases their eventual schooling attainment by more than a year from the water treatment program that others, consistent with the recent literature emphasizing the critical importance of early childhood for investment in human capital and health (Cunha et al. 2006; Heckman 2008; Almond and Currie, 2011). Our back-of-envelope computation of the costs-benefits of the rural water treatment program suggests that the program is highly cost-effective even by our highly conservative estimate.

Our results suggest that basic infrastructure programs such as the provision of safe drinking water—above and beyond the access to water--can significantly increase rural educational level, a potentially critical contribution to the reduction of income inequality between rural and urban residents. Our findings also suggest that water treatment programs have the potential to dramatically reduce the educational gender gap in rural China. These results echo recent findings highlighting the critical importance of investing in young people at the early childhood stage.

Our analysis suggests that careful analyses of how Chinese fiscal resources are spent can be quite useful. The fiscal revenues and expenditures in China are growing faster than its GDP, yet we know very little about the cost-effectiveness of various types of government spending in China. For this rural drinking water program, the return on investment has been significant. What about the cost effectiveness of other programs such as those on highways or high-speed trains on which the government has spent much more than on water treatment? More careful empirical work is clearly needed to guide the allocation of fiscal resources in China and other developing countries.

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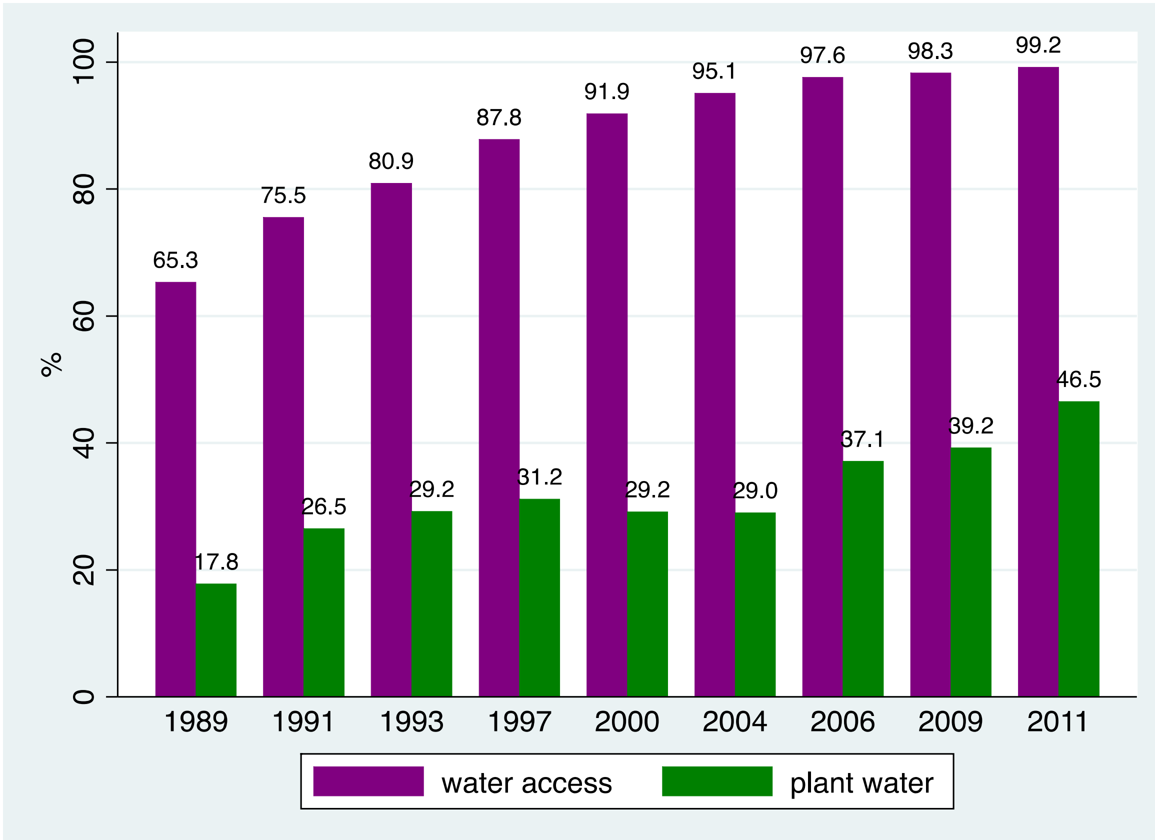
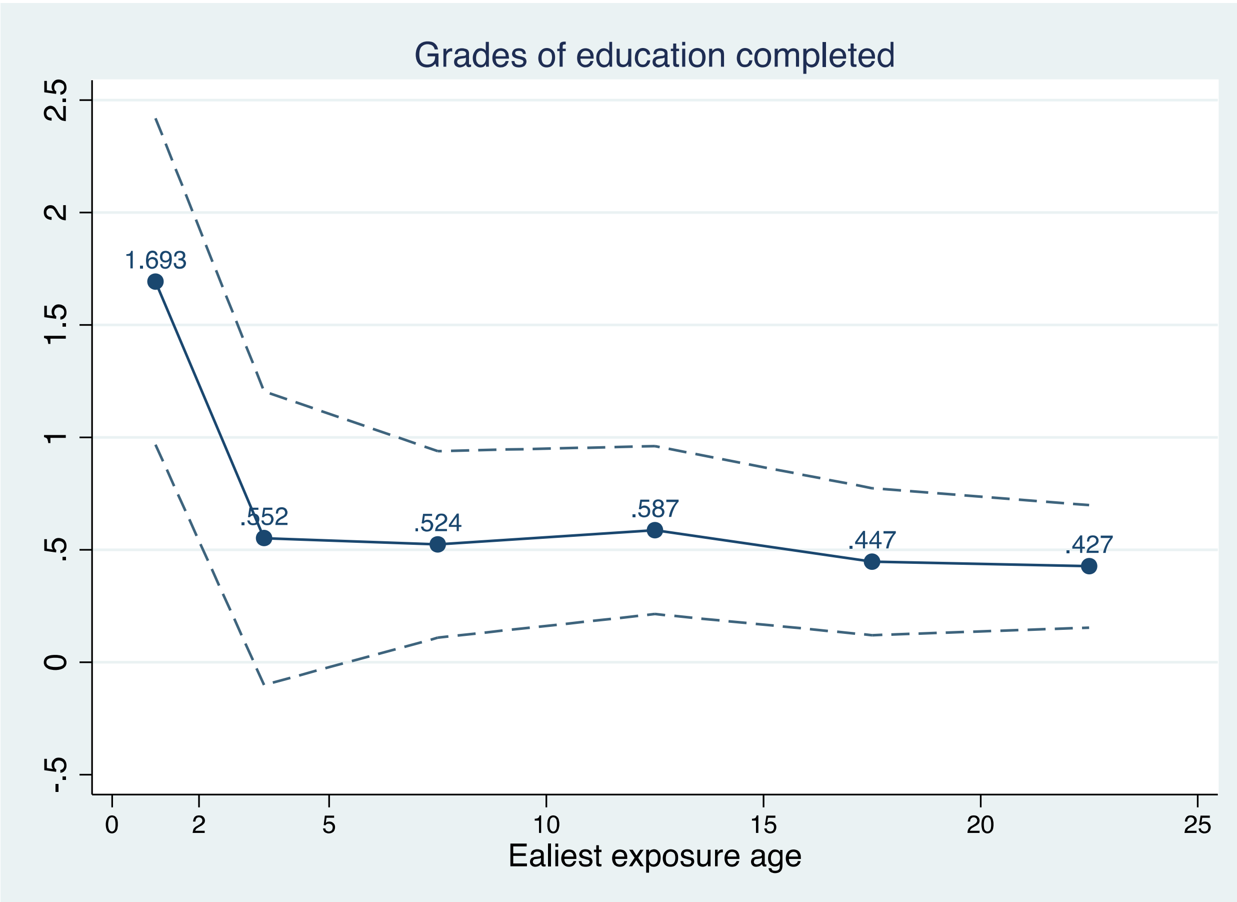
Figure 1. Coverage of Water Access and Plant Water across Waves



Data Source: China Health and Nutrition Survey (CHNS)

Figure 2. Educational Gains across Age of Exposure



Data Source: China Health and Nutrition Survey (CHNS)

Notes: The estimated coefficients of the dummy variables indicating a child's earliest exposure age and their 95 percent confidence intervals (in dashed lines).

Table 1. Descriptive Statistics

Variables	Youth (18-25) (N=4729)	Male Youth (18-25) (N=2414)	Female Youth (18-25) (N=2315)	Older cohorts (N=2708)
Grades of education completed	8.696	8.845	8.540	7.335
	(2.765)	(2.627)	(2.895)	(3.597)
Age	23.202	23.160	23.245	53.875
	(1.862)	(1.890)	(1.832)	(12.964)
Female	0.490			
	(0.500)			
Household head’s child (yes/no)	0.746	0.854	0.633	
	(0.435)	(0.353)	(0.482)	
Household head’s grandchild (yes/no)	0.051	0.052	0.051	
	(0.221)	(0.222)	(0.219)	
Household size	5.132	4.963	5.308	
	(1.776)	(1.712)	(1.825)	
Number of children in the household (age<=15)	0.167	0.140	0.197	
	(0.452)	(0.408)	(0.492)	
Log of household income in the first wave	7.228	7.266	7.187	
	(1.444)	(1.426)	(1.462)	
Distance to a primary school (Km)	0.571	0.523	0.622	1.012
	(2.969)	(2.501)	(3.389)	(2.888)
Distance to a middle school (Km)	2.025	2.083	1.964	2.355
	(5.332)	(5.492)	(5.161)	(3.768)
Distance to a high school (Km)	8.494	8.492	8.495	10.706
	(11.564)	(11.726)	(11.396)	(12.723)

Notes: Older cohorts include individuals who pass 30 years old when their villages access plant water or never treated. Standard deviations in parentheses.

Table 2. Regression Results for the Education Outcome

Dependent Variables	Completed Grades of Education (1)	Completed Grades of Education (2)	Completed Grades of Education (3)
Water plant	1.279***	1.167***	1.081***
	(0.175)	(0.171)	(0.178)
Age			0.121***
			(0.022)
Female			-0.125
			(0.094)
Head's child			0.443***
			(0.119)
Head's grandchild			0.242
			(0.224)
Household size			-0.073***
			(0.028)
Number of children			-0.284**
			(0.113)
Log of household income in the first wave			0.111***
			(0.038)
Kms to a primary school		0.006	0.002
		(0.008)	(0.009)
Kms to a middle school		-0.031***	-0.024**
		(0.010)	(0.010)
Kms to a high school		-0.024***	-0.025***
		(0.008)	(0.008)
Constant	13.900***	13.844***	3.398
	(0.616)	(0.662)	(3.285)
County-year FE	Yes	Yes	Yes
Observations	6,288	6,093	4,729
R-squared	0.217	0.228	0.255

Notes: Each column presents the results from separate regressions.

In addition to the covariates listed in the table, each regression also controls for county-year fixed effects.

The standard errors in parentheses are clustered at the village level.

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

Table 3. Regression Results for Older Adults

Dependent Variables	Completed Grades of Education
Water plant	-0.031
	(0.369)
Age	-0.111***
	(0.006)
Kms to a primary school	-0.028
	(0.034)
Kms to a middle school	-0.054
	(0.039)
Kms to a high school	-0.036***
	(0.009)
County-year FEs	Yes
Observations	2,708
R-squared	0.335

Notes: Each column presents the results from separate OLS regressions. In addition to the covariates listed in the table, each regression also controls for county-year fixed effects. The standard errors in parentheses are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Table 4. OLS and IV estimation of Treatment Effects
(Dependent Variable = Grades of education completed)

	Baseline	Sample with IV information		Village FE	Village FE	County-year FE	Optimal access	
	OLS	OLS	IV	OLS	OLS	OLS		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Water Plant	1.081***	1.020***	2.411***	0.437**	0.568**	0.678***	1.014***	1.039***
	(0.178)	(0.186)	(0.686)	(0.214)	(0.228)	(0.210)	(0.173)	(0.171)
Water access							0.741***	
							(0.212)	
Controls in Table 2	Yes	Yes	Yes	Yes	Yes	Yes		Yes
Fixed effects	County-year	County-year	County-year	Village	Village	County-year	County-year	County-year
Sample notes	Full sample	IV sample	IV sample	Full sample	treatment-status-changing sample	treatment-status-changing sample	Full sample	optimal water access
P-values (bootstrap Hausman test)		0.982						
Observations	4,729	4,246	4,246	4,729	1,586	1,586	4,689	4,275
R-squared	0.255	0.238	0.202	0.277	0.230	0.302	0.259	0.248

Notes: Each column presents the results from separate regressions. 2SLS model when using the dummy of being non-flat as the instrument. The bootstrap Hausman test is based on 1000 bootstrap replications. The standard errors in parentheses are clustered at the village level. For the IV regression in Column (3), the **F-statistics** for the excluded instrument in the first stage is 21.23.
*** p<0.01, ** p<0.05, * p<0.1.

Table 5. OLS and IV estimation of Treatment Effects for Various Exposure Time
(Dependent Variable = Grades of education completed)

	Baseline OLS (1)		Homogeneous effects, but sample with info on exposure time (2)	Exposure time and treatment effects (3)
Water Plant	1.081***	**Water Plant**	0.652***	
	(0.178)		(0.156)	
Water access		**Exposed to** water plant when:		
		0-2 years old		1.693***
				(0.439)
		3-5 years old		0.550
				(0.395)
		6-10 years old		0.526**
				(0.251)
		11-15 years old		0.589**
				(0.226)
		16-20 years old		0.450**
				(0.198)
		21-22 years old		0.519**
				(0.209)
		23-25 years old		0.362
				(0.224)
Controls in Table 2	Yes		Yes	Yes
Fixed effects	County-year		County-year	County-year
Sample notes	Full sample		Sample with information on exposure time	Sample with information on exposure time
P for bootstrap Hausman test				
Observations	4,729		4,064	4,064
R-squared	0.255		0.255	0.259

Notes: Each column presents the results from separate regressions. In the regression in column (3), the default group of these dummy treatment variables refers to the individuals in the untreated villages.
*** p<0.01, ** p<0.05, * p<0.1.

Table6. Effects on Education by Gender

	By Gender	
	Boys	Girls
	(1)	(2)
Water Plant	0.874***	1.290***
	(0.197)	(0.210)
Other controls as in Table 2	Yes	Yes
Observations	2,414	2,315
R-squared	0.263	0.370

Notes: Each column presents the results from separate regressions. The standard errors in parentheses are clustered at the village level.

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

Table 7. Effects on Health by Gender

	Boys		Girls	
	Body mass (i.e., weight/height)	Height	Body mass (i.e., weight/height)	Height
	(1)	(2)	(3)	(4)
Water plant	0.545***	1.393***	0.386*	1.086**
	(0.170)	(0.522)	(0.211)	(0.494)
Age	1.045***	3.580***	0.956***	2.938***
	(0.011)	(0.037)	(0.011)	(0.041)
Household size	0.034	-0.554***	-0.022	-0.368**
	(0.048)	(0.146)	(0.047)	(0.166)
Log of household income in the first wave	0.125**	0.245*	0.044	0.030
	(0.058)	(0.131)	(0.046)	(0.156)
Raising livestock	-0.257*	-0.551	-0.118	-0.320
	(0.146)	(0.403)	(0.152)	(0.423)
Distance to the nearest medical facility	0.086	0.649***	0.026	0.143
	(0.054)	(0.153)	(0.057)	(0.165)
Constant	11.951***	82.046***	20.050***	113.419***
	(4.173)	(7.461)	(2.633)	(9.444)
County-year FEs	Yes	Yes	Yes	Yes
Observations	6,061	6,116	5,010	5,053
R-squared	0.827	0.872	0.815	0.828

Notes: Each column presents the results from separate regression.

The other control variables include county-year fixed effects.

The standard errors in parentheses are clustered at the village level.

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

Table 8. Effects on Education by Gender

	By Gender Identity of the Elder Sibling	
	With an elder brother	With an elder Sister
	(3)	(4)
Water Plant	1.388***	0.571**
	(0.364)	(0.239)
Other controls as in Table 2	Yes	Yes
County-year FEs	Yes	Yes
Observations	844	839
R-squared	0.416	0.398

Notes: Each column presents the results from separate regressions. The standard errors in parentheses are clustered at the village level.

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

Appendix

Table A1. Treatment Effects Based on Different Definitions of the Treatment Variable.

Dependent Variables	Grades of education completed
% of changes in plant water coverage per year between waves	
10%	1.120***
	(0.167)
15%	1.042***
	(0.172)
Water plant	1.081***
(20%)	(0.178)
25%	1.063***
	(0.181)
30%	1.151***
	(0.190)
% of plant water coverage	
60%	1.156***
	(0.165)
65%	1.113***
	(0.168)
70%	1.135***
	(0.165)
75%	1.109***
	(0.177)
80%	1.127***
	(0.165)
85%	1.178***
	(0.169)

Notes: Each cell presents the results from separate regressions for different constructions of the treatment variable. All of other covariates are the same with the ones in Table 2.
The standard errors in parentheses are clustered at the village level.
*** p<0.01, ** p<0.05, * p<0.1

Table A2. Mean Differences between Characteristics of Treated and Untreated Villages (at village level)

Variables	observations	Mean	Standard error
Age	591	-0.264	(0.204)
Female	591	0.033	(0.071)
Household head’s child (yes/no)	591	0.048	(0.062)
Household head’s grandchild (yes/no)	591	-0.003	(0.030)
Household size	591	0.239	(0.262)
Number of children in the household (age<=15)	591	-0.057	(0.075)
Log of household income	576	0.232	(0.236)
Distance to a primary school (Km)	591	-0.050	(1.044)
Distance to a middle school (Km)	591	0.193	(1.022)
Distance to a high school (Km)	591	-2.031	(3.366)

Notes: the means of the treated villages are the average of their characteristics in five years before the treatment. The mean differences are adjusted for county-year fixed effects and the standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Table A3. The first stage regression

Dependent Variable	Water plant
Non-flat	-0.445***
	(0.097)
Age	-0.003
	(0.004)
Female	0.017
	(0.011)
Head's child	0.029*
	(0.017)
Head's grandchild	0.032
	(0.037)
Household size	-0.004
	(0.006)
Number of children	-0.018
	(0.016)
Log of household income	0.005
in the first wave	(0.009)
Kms to a primary school	-0.007
	(0.005)
Kms to a middle school	-0.005
	(0.003)
Kms to a high school	-0.004**
	(0.002)
Constant	1.491***
	(0.147)
County-year FE	Yes
F-stat	21.23
Observations	4,246
R-squared	0.469

Notes: In addition to the covariates listed in the table, each regression also controls for county-year fixed effects. The standard errors in parentheses are clustered at the village level.
*** p<0.01, ** p<0.05, * p<0.1.