The Impact of Access to Clean Water on
Cognitive and Physical Development:
Evidence from Mexico's Programa de Agua Limpia

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Abstract: More than three-quarters of a billion people live without a close source of clean water. While, the immediate impact of clean water access on infant mortality is well documented, there is very limited evidence on the long-term effect of chlorinated water. We exploit exogenous variation created by the implementation of a major clean water reform in Mexico in 1991, Programa de Agua Limpia (PAL), to investigate the impact of exposure to chlorinated water early in life on cognitive and physical development. We estimate that experiencing a one standard deviation reduction in childhood diarrhea mortality rates from PAL throughout infancy leads to ~6% increase in cognitive assessment score and .11 standard deviation increase in height in adolescence. We also confirm that the effects on human capital persist to at least early adulthood and lead to increased hourly earnings.

Keywords: clean water, diarrhea, cognitive development, height, Mexico
JEL codes: I15, I38, I12, I14, H51

I. Introduction

The United Nations and WHO have declared access to clean water a basic human right. Having water to drink is fundamental to human existence and exposure to unclean water leads to dire health consequences such as the spread of diarrhea, cholera, dysentery, typhoid and polio (WHO 2016). For example, it is estimated that each year there are more than 2 million deaths from waterborne diseases. Of these maladies, diarrhea is the most prevalent, and represents the second leading cause of child death in the world (Fischer Walker et al 2013).

This environmental reality is, on the one hand, a rather homogenous phenomenon in that it is an issue that is present throughout almost the entire developing world (UNICEF and WHO, 2017). On the other hand though, access to clean water is quite heterogeneously related to poverty as very few people that lack clean water live in a developed country. This critical difference in environmental risk factors is an important contributing cause of the significant gap that exists in the prevalence of infectious diseases in developing versus developed countries.[1]

The immediate impact that between-country variation in exposure to tainted water can have is well established. In particular there is a large literature that finds, across various contexts and time periods, increased access to clean water significantly lowers infant and child mortality (Argentina: Galiani et al., 2005; Bangladesh: Field et al., 2011; Brazil: Gamper-Rabindran et al., 2005; Mexico: Bhalotra et al., 2017 USA: Cutler and Miller, 2005 and Watson, 2005; among others). While this set of research has helped prompt large investments by governments and NGOs in safe water provision, an estimated 844 million people still do not have a readily available source of clean and protected water (UNICEF and WHO, 2017). Moreover, there is good reason to believe that by focusing only on the immediate mortality effect, researchers and policymakers may be severely underestimating the true value of clean water.

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[1] The number of healthy life years lost to infectious diseases was 15 fold higher in developing versus developed countries in 2014 (WHO, 2014).

There is a growing consensus that health shocks in the first 1000 days of life can have a persistent impact on the individual's physical and cognitive development (Currie and Vogl, 2013; Almond and Currie, 2011; Cuhna and Heckman, 2007; Heckman 2007; among others). In addition, a robust literature has demonstrated the positive association between health capital, such as stature and intelligence, and economic outcomes (Lin et al., 2018; Glewwe et al., 2017; LaFave and Thomas, 2017; Vogl, 2014; Huang et al., 2013; Case and Paxson, 2008; Heckman et al., 2006; Strauss and Thomas, 1997; among others). These two widely accepted facts suggest that increased access to clean water at birth, which will reduce the prevalence of severe or repeated infections that can divert nutrients away from physical and neurological development during the critical early life period, may have a long-lasting impact on the economy.[2]

Recognition of these mechanisms in the medical literature has generated the provocative suggestion that the much higher prevalence of infectious disease in poor countries may help explain their weaker performance on international intelligence tests and stagnated rate of economic growth. These propositions potentially have major implications for understanding human growth and economic development, standing to inform, for instance, debates in the economics literature concerning the role of innovation-led declines in infectious disease in explaining living standards (Acemoglu and Johnson 2007; Sachs and Malaney 2002; Bleakley 2010; Costa 2013). However, the evidence available to support a causal link between early life exposure to clean drinking water and markers for long-term human capital accumulation is at best suggestive.

The goal of this paper is to provide causal evidence of the contemporary relationship between improved access to clean water in infancy and physical and cognitive growth. We focus on infant exposure because the caloric requirements for brain development are higher during this period than at any other stage of the life course,

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[2] During infancy about 85% of calorie intake is used for brain development (Eppig et al 2010). In addition, the release of inflammatory molecules during an infection may directly impact the developing brain by changing the expression of genes involved in the development of neurons and the connections between them (Deverman and Patterson, 2012).

making infants especially vulnerable to diminished cognitive endowments as a result of diarrhea, a highly morbid and often recurrent symptom of waterborne infections that can severely compromise nutrition, physical growth, and mental development (Fischer Walker et al 2012a; Fischer Walker 2013).

To the extent that the clean water reform improved health and cognitive endowments in the infant period, this will tend to have raised the marginal productivity of subsequent investments in reform-treated cohorts and, by reducing morbidity and mortality from diarrhea, it will have extended the horizon over which returns flow (Heckman and Cunha 1997, Soares 2005). Any measured medium- or longer-term cognitive gains will incorporate both the stronger infant endowments and any reinforcing parental investments induced by the water reform. We attempt to identify the importance of behavioral relative to biological impacts by exploiting gender variation in labor market returns to investments determined by a biologically premised (and hence exogenously given) gender-specific comparative advantage in brain *vs* brawn-intensive tasks.

To account for selectivity into access to clean water, we leverage the introduction of the *Programa de Agua Limpia* (National Clean Water Program; PAL) in Mexico, a large-scale nationwide water treatment policy. This program provides quasi-random temporal variation, as its introduction was a sudden reaction to the unanticipated threat of cholera created by an epidemic spreading through the countries neighboring Mexico and led to rapid and sizeable drops in childhood diarrhea mortality rates (Bhalotra et al, 2017). PAL also provides rich geographic variation as its impact was systematically larger in areas with worse pre-program water quality, and thus, areas with higher pre-program childhood diarrhea mortality rates (Bhalotra et al., 2017). Our identification strategy exploits both municipality-level pre-program childhood diarrhea mortality rate and birth cohort variation, combining the sharp convergence across Mexican municipalities with the timing of the nationwide reform.

We find sizeable positive effects of exposure to clean water in infancy on performance in Raven's cognition tests and height measured at the ages of 10-16. We

estimate that experiencing the average diarrheal mortality decline over our sample period throughout the entire infancy period leads to a ~6% increase in cognitive assessment scores and a .11 standard deviation increase in height during adolescence and that similarly sized effects persist into early adulthood. Furthermore, when exploring economic outcomes in early adulthood, 17-26 years old, we find evidence that early life access to clean water resulted in significant gains to productivity, as measured by earnings per hour.

To provide faith in the causal nature of our estimates we show that the results are not driven by unobserved trends through the inclusion of municipality specific time-varying controls, municipality specific linear time trends, alternative disease mortality rates that capture the general health environment, as well as, by providing an event study analysis. Lastly, to mitigate potential bias from selective mortality, selective fertility and/or selection into live birth we conduct the analyses only using within-family variation.

Gender-differentiated parental responses are consistent with a Roy-type model in which boys have a (biologically premised) comparative advantage in brawn or, conversely, girls a comparative advantage in brain-intensive occupations, which raises the average return to education for girls relative to boys (Pitt et al 2012; Rendall 2010; Rosenzweig and Zhang 2012). Given this differential investment we also explore the gendered human capital effect of PAL exposure in infancy, and we find evidence that the gains to cognition, while present for boys, are significantly larger for girls.

Our study makes substantive contributions in several domains. We provide the first causal evidence that the benefits of clean water interventions that limit early life exposure to waterborne diseases in a developing country context extend beyond lowering morbidity and mortality to enhancing cognitive skill formation and health[3]. Recent work highlights only the immediate health-related benefits from water interventions, translated into increased disability adjusted life years (DALYs) (Ahuja et al 2010; Cutler and Miller 2005; Galiani et al

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[3] An association between the number of diarrheal episodes in early childhood and poorer cognitive development has been demonstrated in the medical literature (Fischer Walker et al 2012a; Guerrant, et al, 1999; Niehaus et al 2002) but these studies do not account for the endogeneity of diarrheal episodes beyond covariate adjustment.

2005; Gamper-Rabindran et al 2005; Watson 2005), and the allocation of global health resources is guided by these more conservative benefits. Our estimates suggest that the evidence routinely presented to policymakers, by failing to record the socioeconomic benefits that accrue in the long run to survivors, substantially under-estimates the benefits of water interventions.

Second, we extend the traditional limits of research on the long-term effect of an early-life health shock in several ways. For example, rather than providing evidence that access to clean water in infancy leads to gains in physical and cognitive development at only one arbitrary snapshot in time, we are able to test the persistence of these benefits into early adulthood. In addition, while most research on the causal impacts of early life infectious disease on human capital are restricted to a specific sub-population at one moment in time, we are able to study a nationwide change in the health environment using nationally representative data and investigate whether the benefits of this intervention persists over time. Moreover, by being able to directly test for changes in parental composition resulting from the transformation of the health environment, we show that a failure to control for selection bias can substantially alter the magnitude of the estimates.

Third, we extend an emerging literature that seeks to understand gender differences in the impact of health and education interventions in terms of gender-based comparative advantage in skills (Pitt et al 2012; Rosenzweig and Zhang 2012) by virtue of modeling an exogenous (policy-determined) change in the infant health endowment.[4] Our investigation of gender-differentiation in parental investments marks a departure from most previous work in the separate domain of studies of the long run impacts of early life interventions (see the survey in Almond and Currie 2011).

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[4] Pitt et al (2012) estimate endowments as residuals from a production function for body mass, acknowledging that this approach is sensitive to the choice of functional form and potential endogeneity of the inputs. Rosenzweig and Zhang (2012) use within-twin variation in birth weight to identify endowment differences, but this approach may lack external validity and is prone to bias if there are within-twin (or sibling) spillovers (Adhvaryu and Nyshadham 2012).

Fourth, this study provides evidence of the extent to which investments respond to rates of return to education. A vast literature measures rates of return but the elasticity of investment is not known, possibly because variation across individuals in rates of return is rarely observed and sharp changes in returns seldom occur (see Abramitzky and Lavy 2012; Jensen 2010 and 2012). In this study, the water reform creates a quasi-experimental increase in the rate of return to education-related investments by virtue of improving the infant (cognitive and health) endowment. We find that as the relative return was larger for girls these investment responses complemented that pattern.

The remainder of the paper is laid out as follows. Section II discusses the National Clean Water Program, establishes short-run (or "first-stage") impacts on diarrheal disease risk, and describes the limited current evidence regarding early-life access to clean water and human capital accumulation. Section III discusses the empirical strategy and Section IV the data. The results are discussed in Section V-VII. Section VIII concludes.

II. Background

IIa. History and Description of Mexico's National Clean Water Program

Through the late 1980s, infectious diseases were responsible for a significant proportion of infant and child deaths in Mexico. Diarrheal disease was a particularly important scourge, accounting for nearly a quarter of under-5 deaths in this period (Gutierrez et al 1996). Importantly, this public health issue in Mexico is not shared equally across the country. *Figure* 1 provides a map of diarrhea prevalence across Mexican municipalities and provides insight into its substantial heterogeneity.

Since 1978-1997, public sector efforts have been highly effective in reducing infectious disease deaths (Frenk et al 2003), with credit typically assigned to expansions in access to clean water, sanitation, vaccines and oral rehydration therapy (Sepulveda et al 2007). This paper focuses on the early 1990s, when the intensity of public health activity increased considerably due to the emergence of a cholera epidemic in other parts of Central and South America. Fears of the outbreak extending to Mexico prompted public

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health officials to proactively undertake improvements in access to potable water and sanitation and information campaigns to educate local leaders and constituents about cholera and encourage preventative behavior (Gutierrez et al 1996; Sepulveda et al 2006; Sepulveda et al 2007). In April 1991, Mexico implemented a National Clean Water Program (*Programa de Agua Limpia* or PAL). PAL did not expand existing piped water infrastructure or sewage networks, but rather increased water disinfection through existing water pipe infrastructure. To improve water quality in areas without piped water coverage, chloride tablets were disseminated to households and monitoring for commercial bottled water and ice was expanded. Lastly, there was a concerted effort to reduce the use of wastewater in irrigation. The total outlay for the program over the period 1991-1994 was 1 billion USD.[5] Implementation was rapid: within the first eight months of the program the share of the population receiving chlorinated water rose from 54% (in April 1991) to 85% (at the end of the year), see *Figure 2a*. Similarly, farmland area irrigated with wastewater declined markedly between April and December 1991, see *Figure 2b*.[6] Despite these changes, there is no evidence that PAL crowded out or in other household or public investment in water quality or health infrastructure (Bhalotra et al., 2017).

IIb. Establishing Program Impacts on Diarrheal Disease Mortality

Previous explorations of PAL's impact have hinted that the program had a substantial impact on the citizen's health. Specifically, Gutierrez et al. (1996), Velazquez et al. (2004), and Sepulveda et al. (2006) provide descriptive evidence of large declines in childhood diarrheal disease mortality rates beginning in 1991.[7] These studies, though, only

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[5] Personal communication with Dr. Jaime Sepulveda.

[6] Attention to water reform is pertinent as not all countries have seen secular progress in provision. Between 1990 & 2005, the percentage of urban households with piped water declined from 50 to 39 in 32 West African countries (World Bank).

[7] Gutierrez et al. (1996) investigates aggregate morbidity rates and finds that from 1990 to 1993 the average number of annual episodes of diarrheal disease morbidity among children decreased from 4.6 to 2.2. Velazquez et al. (2004) report that between 1990-1995 diarrheal disease morbidity in Mexico declined by over 63%.

analyze national trends over time, which may reflect other unobserved factors leading to an improved health environment unrelated to PAL. In this section we formalize and restate the evidence from Bhalotra et al. (2017) which uses *Mexican Vital Statistics* data to show there was a trend break and convergence in diarrheal disease mortality after 1991 using age, gender and disease-specific mortality rates by municipality and year.

To estimate the impact of PAL on diarrheal mortality rates among children under-5, Bhalotra et al. (2017) test for differential trend breaks in diarrheal disease mortality rates in 1991 (when PAL was implemented) relative to a control disease (respiratory infections) which should not be directly affected by PAL.[8] They choose respiratory infections because they were the second-leading cause of child mortality in Mexico prior to PAL and because they share several common risk factors with diarrheal diseases (respiratory infections are spread through oral droplets and diarrheal diseases are spread by fecal-oral contamination). Moreover, respiratory diseases are an apt comparison as they respond to water quantity, as opposed to diarrheal diseases, which respond to water quality (Fischer Walker et al. 2013b).

Figure 3 provides descriptive evidence of the impact of PAL. Specifically, it shows that child mortality from diarrhea was relatively stable between 1985 and 1990, then started to drop dramatically in 1991, leveling off after 1992, while respiratory and vaccine preventable disease mortality rates show a more gradual decline. This pattern is consistent with the timing of the National Clean Water Program,

To test this formally Bhalotra et al. (2017) estimate the following event study specification over the period 1985-1995:

$$M_{djt} = \alpha_d + \sum_{t=1986}^{1995} \alpha_t (1(Diarrhea_d)*1(Year=t)) + \sum_{t=1986}^{1995} \mu_t (1(Year=t)) + \lambda_t + \varepsilon_{djt} \quad (1)$$

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[8] An equivalent strategy was used in Jayachandran, Lleras-Muney and Smith (2010).

In equation (1), M_{djt} is the inverse hyperbolic sine transformation of the mortality rate for disease, d, in municipality, j, and year, t.[9] $1(Diarrhea_d)$ is an indicator variable that equals 1 if the cause of death is a diarrheal disease versus the control disease and $1(Year=t)$ is an indicator variable which equals 1 for observations in year t. Lastly, municipality fixed effects are denoted as λj. Estimates of α_t provide the average differential percentage change in the diarrheal disease mortality rate vs. the control disease mortality rate in year t relative to the baseline year (in this specification, 1985).[10]

The estimates of α_t obtained by calculating equation (1) are found in *Figure 4*. Confirming the descriptive evidence in *Figure 3*, *Figure 4* shows that, as compared to under-5 mortality from respiratory disease, there was a discrete increase in the decline of under-5 diarrheal mortality with the implementation of PAL in 1991, which persisted throughout the post-implementation years.[11]

Subsequently to estimate the average program effects of PAL during the study period Bhalotra et al. (2017) use a specification of the following form:

$$M_{djt} = \beta_0 + \beta_1(1(Diarrhea_d)*1(Post_t)*Year_t) + \beta_2(1(Diarrhea_d)*1(Post_t))$$
$$+\beta_3(1(Diarrhea_d)*Year_t) + \beta_4(1(Diarrhea_d) + \sum_{t=1986}^{1995}\mu_t(1(Year=t)) + \lambda_t + \varepsilon_{djt} \quad (2)$$

In equation (2), *1(Post)* represents an indicator variable for post-PAL years (1991 and later), and all other variables are defined as in equation (1). β_1 and β_2, measure the trend and level breaks in 1991 relative to the control disease mortality rates. *1(Diarrhea_d)*1Year_t* controls for pre-existing trends in diarrheal disease mortality relative to respiratory disease, the year

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[9] Estimates obtained using the inverse hyperbolic sine transformation can be interpreted in the same manner as those obtained using a natural logarithm transformation of the dependent variable, with the advantage of being defined at zero (Burbridge, Magee and Robb 1988).

[10] Standard errors are clustered at the municipality level and municipality weights based on the average number of annual pre-intervention live births are used.

[11] Morbidity from diarrhea declined in line with mortality. The number of diarrheal episodes per infant per year nationwide is estimated to have declined by over 50% from the late 1980s to mid 1990s, with the bulk of that decrease occurring during 1991-1993 (Gutierrez et al 1996).[11] However, there are no region-specific yearly data for morbidity for any duration of time so we follow the convention of using mortality rates as a proxy for morbidity (Bozzoli et al 2009).

fixed effects control for municipality-invariant differences in the health environment across time, and *1(Diarrhea$_d$)** captures time-invariant differences between diarrheal and control disease mortality rates.

The results of estimating equation (2) on mortality rates among children under-5 (altogether, as well as separately for neonates (0-1 month), postneonates (1-12 months), and children ages 1-4), can be found in Table 1. In all cases there is evidence of statistically significant level and trend break in diarrheal disease mortality as a result of the introduction of PAL. Specifically, with regard to under-5 mortality rates (first column) the results imply that PAL led to a 48% decline in diarrheal disease mortality rates by 1995. Moreover, this decline was not uniform across the country. As shown in *Figure 5* the decline in diarrheal disease mortality occasioned by the Clean Water Program was larger in areas with higher pre-intervention rates.

IIc. The Long-term Impact of Early Life Access to Clean Water:
Previous Evidence in the Economics Literature

There is an established inverse relationship between access to clean water and infant and child mortality (Bhalotra et al., 2017; Cutler and Miller, 2005; Field et al., 2011; Galiani et al., 2005; Gamper-Rabindran et al., 2005; Watson, 2005; among others). With few exceptions, though, the research on access to clean water in early life has focused only on the immediate health-related benefits. Specifically, Zhang and Xu (2016) and Chen et al. (2020) represents the totality of the economics literature that claims to investigate the persistent causal impact of early life clean water exposure on later life human capital

accumulation in an environment relevant to modern day developing countries and focus on the same source of varitation.[12,13]

Zhang and Xu (2016) and Chen et al. (2020) provide estimates of the impact of early life access to clean water on educational outcomes and cognitive test scores, respectively, by studying the long-term effect of exposure to the rural drinking water program in China.[14] In particular, these two studies use temporal and geographic variation from the first 20 years of implementation of the rural drinking water program and find that the exposed attain more years of education and score higher on intelligence tests in adolescence. These analyses provided a useful first attempt at tackling the question of the persistent impact of access to clean water, but the advantage of our study is that it allows us to improve upon the two important internal and external validity limitations present in their study.

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[12] Beach et al. (2016) is another study that explores this topic by examining the impact of early-life exposure to typhoid fever, which is linked to the cleanliness of the water, on educational attainment and earnings during the early 1900s in the United States. An important distinction between their work and our own is that the heterogeneity in typhoid fever they exploit is not generated by any specific clean water intervention, and thus may be partially generated by other general health trends. In addition, given that the context of Beach et al. (2016) is the historical United States, the policy relevance of the estimates to the context of the modern developing world is, at best, speculative.

[13] However, Barham (2012) and Venkataramani (2012) provide well-identified evidence linking early life exposure to other infectious diseases to cognitive outcomes. In addition, a number of recent studies record causal impacts of early life infectious disease on education and labor market outcomes for which cognitive development may be a pathway, but they do not explicitly establish impacts on cognition; see Bleakley (2007, 2010), Barreca (2010), Baird,et al (2011), Cutler et al (2010), Lucas (2010). Papers that identify impacts of childhood health shocks other than infectious diseases on cognitive outcomes and achievement include Almond, Edlund, and Palme (2009), Almond and Mazumder (2011), Bharadwaj et al (2012), Maluccio et al (2009), and Stein et al (2005). Lastly, work by Spears and Lamba (2016) and Hammer and Spears (2016) have provided evidence that early life exposure to sanitation programs lead to improved cognitive and physical wellbeing later in childhood (i.e. at less than 6 years of age).

[14] Zhang and Xu (2016) also use height as a dependent variable but do not isolate the impact of exposure at early ages as they only provide estimates when the effect is measured as an average over all people exposed for the first time from age 0 to age 25.

With regard to causal identification, using variation in clean water determined by government provision of resources, which is likely non-random, over a 20 year period raises a concern that bias-inducing selection bias may be present. Moreover, evidence that supports the presence of bias is provided in the paper by Zhang and Xu. Specifically, Zhang and Xu find that individuals exposed for the first time to the rural drinking water program at ages as old as 21 and 22 experience a statistically significant and large, .4 years, increase in their years of education.[15] Moreover, the staggered timing differences-in-differences methodology utilized in both papers has been shown to provide average treatment on the treated estimates and event study plots that are likely biased by heterogeneity in treatment profiles across exposure timing groups (Sun and Abraham, Forthcoming; Goodman-Bacon 2021).

Studying a program like PAL that was implemented unexpectedly, rapidly, and simultaneously across the entire country of Mexico provides an environment where selection bias is more plausibly limited and which can avoid the concerns of the staggered differences and differences methodology. We exploit both the timing of exposure through differences in cohorts, as well as, heterogeneous intensity through a proxy for pre-intervention water quality at the municipality level. In addition, our estimates will be able to directly control for many forms of potential selection through the inclusion of fixed effects at the municipality, year-month of birth, year-month of interview, and state-year of birth levels, respectively, as well as, uniquely through within family comparisons using maternal fixed effects.

The second limitation faced by Zhang and Xu and Chen et al. is that, even if their findings can be interpreted as the causal effect of the rural drinking water program in China, they may not be easily generalizable to the impact of access to clean water outside of China. As Zhang and Xu note, unlike most developing countries, due to the cultural

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[15] These results are particularly troubling as they are statistically indistinguishable from the same estimated impact for individuals exposed as early as 3 years old. In addition, in rural China during the sample period the average years of education is less than 9 years, less than 9% of students are still in school at age 18, and only 5% of all rural students continue education past high school.

tradition of eating cooked food and drinking boiled water, the main impurities in the water

in China are not disease causing microorganisms. Rather the critical issue with Chinese

water is the presence of chemical contaminants from rapid industrialization and country-

specific geographical phenomena. Thus, the context and mechanisms of the rural drinking

program in China may not be ideally suited for understanding the environments faced by

most policymakers concerned about access to clean water.

III. Research Strategy

IIIa. Basic Model

To assess the impact of increased access to clean water during infancy on later life

cognition and stature, we estimate:

$$Y_{ijtmd} = \beta_0 + \beta_1 Full_t * BaseRate_j + \beta_2 Partial_t * BaseRate_j + f(\text{mom's age})_m \gamma +$$
$$\alpha_1 i.female_i + \eta_{BO} + \omega_t + \lambda_d + \nu_j + Full_t * MuniChars_j \psi + \tag{3}$$
$$Partial_t * MuniChars_j \theta + \nu_j * YOB_t + \rho_m + \varepsilon_{ijtmd}$$

Y_{imjtd} is the outcome for individual i, in municipality j, with birth year-month t, with mom,

m, and interviewed in year-month, d. $Full_i * BaseRate_j$ is an indicator equal to 1 if child, i, was

exposed to PAL since in utero (i.e. born on or after April 1991), $Full_{i,}$, interacted with the

municipality-level gender-specific child diarrheal mortality rate (per 1,000) averaged across

the pre-intervention years, 1988-1990, $BaseRate_j$. Thus, the parameter β_1 captures the

impact of exposure to increased access to clean-water throughout infancy.[16] We do not

observe which individuals are at risk of diarrhea or which benefit from the water reform,

so β_1 is interpreted as providing intent to treat (ITT) effects.

Individuals born between 4/1990-3/1991 experienced PAL for part, but not all, of

their infancy period. Rather than erroneously adding these partially exposed cohorts to the

control or full exposure groups, we estimate their effect separately using $Partial_i * BaseRate_j$,

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[16] We focus on the impact of access to clean water throughout the first year of life because
nutritional intake directed towards brain development is highest during this period (Eppig et al
2010). However, we will also use an event study design to test this restriction by estimating impacts
at other ages.

where *Partial$_t$* is an indicator equal to 1 if the child is born between 4/1990-3/1991 and *BaseRate$_j$* is defined as noted previously.

By interacting the timing of the intervention with the pre-program municipality-specific diarrhea mortality rates, we exploit municipality*cohort variation in program intensity, as in Acemoglu and Johnson (2007), Bleakley (2007), Bhalotra and Venkataramani (2012). Equation (3) may be thought of as the reduced form of a system in which later life outcomes are allowed to depend upon infant exposure to clean water, with the latter instrumented by the sharp arrival of the National Clean Water program, and whose intensity is allowed to vary by the pre-intervention burden of diarrheal disease in the municipality.

To limit the extent of bias from fixed differences between genders, birth cohorts, interview dates, municipalities, and birth order we add a number of fixed effects to our model. Specifically, the model contains an indicator value for the sex of the respondent, i.female$_i$, and fixed effects for year-month of birth, ω_t, year-month of interview, λ_d, municipality, ν_j, and birth order, η_{BO}. In addition, to account for the potential relationship between the age of the mother and program implementation a quadratic function for the mother's age at birth, f(mom's age)$_m$ is included as well.

We also want to control for other unobserved trends at the municipality level that may be correlated with the variation in our measure of program intensity, *BaseRate$_j$*. With this goal in mind we add municipality specific linear time trends, ν_j*YOB_t, to our model to control for any unobserved linear birth cohort trends that are unique to the respondent's municipality. Despite the use of region-specific linear time trends in addition to the rest of our controls, a recent critique of this type of identification strategy by Jaeger, Joyce, and Kaestner (2018) shows it may not be robust to the inclusion of other pre-intervention characteristic trends. Intuitively, if *BaseRate$_j$* is actually capturing variation in a different pre-intervention characteristic, our interpretation of β_1 as being the causal effect of access to clean water may be misguided. In order to account for this we include two control vectors, *Full$_t$*MuniChars$_j$* and *Partial$_t$*MuniChars$_j$* which represent an interaction of the

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indicators for our cohorts of interest, *Full_t* and *Partial_t*, with other municipality characteristics averaged across the pre-intervention years, 1988-1990 such as: mean education level, mean log of income, percent of population that has indigenous heritage, percent of population with access to piped water, and percent of population with access to sewage system.

Lastly, any exploration of the impact of a change in the health environment at early ages must be wary of selective mortality, selection into fertility, and selection into live birth. If certain types of families systematically select into or out of attempts at conception or are more or less likely to be able to have a live birth as a result of increased access to clean water this would generate bias in our estimates. To address the former issue of selection into fertility we, as is common in this literature, restrict the sample in an effort to limit bias. In our case we will only include children born on or before 1992. Restricting the sample, though, is only a partial solution to selection into fertility and relies on a strong assumption that the timing and location of the intervention is completely unanticipated. In addition, it does not assist in any way with regard to bias from selection into live birth. Moreover, there is an established relationship between access to clean water and infant mortality (Cutler and Miller, 2005; Field et al., 2011; Galiani et al., 2005; Gamper-Rabindran et al., 2005; Watson, 2005; among others). If, as is likely, the children saved by PAL are non-random, this would generate a bias-inducing selection into our sample. In our analysis we will confront these potential confounders by also estimating specifications that limit our analysis to within-family comparisons through the inclusion of maternal fixed effects, ρ_m.

The standard errors of our estimates of equation (3) are clustered at the municipality level to allow for serial correlation in the outcomes across years within municipalities (Bertrand et al 2004). The number of unique municipalities in each analysis ranges between 80-150, which is sufficient to avoid concern over potential over-rejection of the null due to a small number of clusters.

IIIb. Threats to Inference

Given the rich set of controls included in equation (3), there is only one first order threat to identification remaining: the potential confounding effect of similarly timed public health and education efforts. While there is no record of any government projects of this type[17,18], it is possible that prior interventions could play a role in generating biased estimates of water program impacts if they generate differential pre-trends across the municipalities.[19] To test for bias from these types of unobserved trends we add interactions of our cohorts of interest, $Full_t$ and $Partial_t$, with the levels of two alternative diseases, respiratory disease and vaccine-preventable diseases[20] averaged across the pre-intervention years, 1988-1990 as controls. These two diseases trends serve as useful

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[17]Aside from a measles vaccination effort, which is unlikely to be correlated with the temporal and geographic variation of PAL, there were no other public health interventions within a two-year band around the National Clean Water Program (Sepulveda et al 2007).

[18]*Opportunidades* (formerly *Progresa*) is a large and important means-tested cash transfer program in Mexico that provides families with cash conditional upon their children attending schools and health clinics. *Opportunidades* was rolled out across rural Mexican municipalities from 1997, and thus PAL was implemented much earlier, in 1991, and water-treated children, born April 1990 onwards, will have been of school entry age (age 6 or 7) when *Opportunidades* started. Pre-reform birth cohorts in our sample (born 1986-1989), will also have been exposed to *Opportunidades,* but slightly later in their school career. While the age of exposure should prevent any bias from *Opportunidades* on our height results, to generate bias in our estimates on cognition the municipality level intensity of *Opportunidades* exposure would have to be correlated with the municipality level intensity of PAL exposure and the impact of access to *Opportunidades* would have to sharply diverge for kids exposed at 6-7 versus 8-10. While this is unlikely, since *Opportunidades* at this time was restricted to rural areas we can explore whether our results differ by an urban/rural distinction. We find no evidence of this type of relationship across any of our specifications and outcomes (results available upon request).

[19] For example, starting in 1985, the Mexican Federal government introduced and promoted the use of oral rehydration therapy (ORT) for treatment of diarrheal disease (Gutierrez, et al, 1996, Frenk, et al, 2003). If these efforts were targeted to states that performed relatively poorly in terms of infant and child health, some part of the convergence in diarrhea mortality rates observed after 1991 could be attributed to pre-existing trends driven by ORT roll-out.

[20] This category is dominated by measles. As is evident from the blip in vaccine preventable disease in *Figure 2,* there was a measles pandemic in 1989-1990, so including this variable controls for this event. We also re-estimated the equation dropping these years and there was no significant change in the coefficient of interest.

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controls because infant mortality for these diseases is highly correlated with overall under age 5 mortality, they are driven by several of the same risk factors as diarrheal disease (poverty, poor nutrition, and crowding), and they are also declining through the sample period. Importantly, though, PAL should not meaningfully impact these diseases. Thus, these interactions will capture any municipality-specific trends in the living standards and health environment that are not generated by the change in access to clean water. Specifically, in order to bias our estimates, changes in the disease or general health environment would have to exhibit a trend break in 1991 and be larger in municipalities with higher pre-intervention diarrhea mortality rates. Each of the controls is therefore entered with its pre-1991 level interacted with our cohorts of interest, so as to most severely test the attribution of the 1991 break to the water reform and diarrhea reduction. Robustness of our estimates to these controls implies that omitted trends in common risk factors and interventions that may have created a break in trend in 1991 are unlikely to be biasing our inferences.

Lastly, in an attempt to fully saturate our model and purge our estimates of bias from unobserved regional trends, we assess the robustness of our results to the inclusion of state specific birth year fixed effects. While the amount of identifying variation and power to detect significant differences is reduced when including this set of fixed effects, if we observe little change in the magnitude of the estimates it provides evidence that our conclusions are not being guided by bias from geographic trends.

IV. Data

The individual level data used to estimate equation (3) comes from the Mexican Family Life Survey (*MxFLS*). The MxFLS is a nationally representative survey that collected data from approximately 8,500 households in 150 communities across 16 Mexican states (Rubalcava and Teruel 2007) at baseline in 2002. The MxFLS includes a colored Raven's progressive matrices test and an anthropometric measure of height in addition to household demographics, expenditures, educational attainment, health, labor

force participation, and fertility. We limit our analysis to the outcomes for the 1986-1992 birth cohorts who are measured in adolescence, 10-16 years old, during the baseline wave of the MxFLS and in young adulthood, 17-26 years old, during the most recent survey round, MxFLS3.

In addition to having systematic measures of cognition and stature, as well as, birth date data[21], the other key component needed to conduct this analysis is locational information that can be used to assign exposure intensity. Using the MxFLS provides two options for assignment of treatment intensity. The first option is to use the individual's municipality of residence during baseline enumeration, which is the same technique used throughout the early-life exposure to clean water literature (i.e. Zhang and Xu, 2016 and Beach et al., 2016). Municipality of residence at baseline represents the respondent's place of residence when they were between 10 to 16 years old. The advantage of using the individual's municipality of residence is that it is available for all respondents, thus maintaining the survey's national representativeness and avoiding any loss of sample/power. The drawback of this technique is that migration may lead to noise or bias inducing misassignment of exposure intensity. Bias from this type of measurement error, though, is mitigated to the extent it is related to fixed characteristics of migrating families by using within family comparisons.

Alternatively, the MxFLS contains non-comprehensive information on the respondent's municipality of birth. Specifically, municipality of birth is recorded for all respondents 15 years old or older in the migration history instrument. By collecting responses to this question across the three waves of the survey we obtain useable municipality of birth information for 70% of the individuals in our analytical sample. The remaining missing municipality of birth information is a result of interviewer notation errors (e.g. spelling or recording the state of birth instead of the municipality). For the set of respondents in our sample of interest that have municipality of birth data, we find that it matches the individual's municipality of residence at baseline 87% of the time.

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[21] The MxFLS provides birth date information at the year-month level for all respondents.

We proceed by generating our main estimates using municipality of residence at baseline for the entire sample as our exposure assignment location. We then compare the estimates of equation (3) for the restricted sample with municipality of birth information when using municipality of residence at baseline as our exposure assignment location against estimates of equation (3) using municipality of birth as the exposure assignment location. We find that the estimates are qualitatively and quantitatively equivalent, suggesting the change in exposure assignment location only impacts the representativeness of the sample and not identification. Given these results we elect to use the more inclusive municipality of residence data for all subsequent analyses.

Our study primarily focuses on two outcomes. The first is the individual's height-for-age z-score. This measure is calculated using the interviewer-measured height and self-reported age of the respondent and applying the WHO standard growth chart for developing countries. Our second outcome of interest is the percentile score (0-100) of each respondent on the Raven's colored progressive matrices test. After establishing the relationship between early life access to clean water on these measures of cognitive and physical growth we also explore the impact of PAL exposure in infancy on other human capital outcomes in adolescence such as body mass index, whether the individual still attends school, years of attained education, whether the individual had to repeat any grade levels, and whether the individual is currently employed.

The pre-intervention level of mortality (*BaseRate$_j$*) by municipality and gender is calculated using the under-5 diarrhea mortality rate over the period 1988-1990. Data on disease mortality rates by cause of death, gender and age were computed using mortality data from the Mexican Secretary of Health (*Secretaría de Salud*) and population estimates from the National Council on Population (*Consejo Nacional de Población*).[22] A one standard

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[22] The data can be accessed at http://sinais.salud.gob.mx/basesdedatos/ or http://sigsalud.insp.mx/naais/. To construct the indicators for diarrheal and respiratory mortality, we focused primarily on infectious cases. For diarrheal diseases, we used counts for ICD-9 codes A0-A9 and for respiratory diseases, codes 460-466 and 480-487. Vaccine preventable diseases are those from measles, mumps, rubella, diphtheria, and tetanus

deviation in the child diarrheal mortality rate is 10 per 1,000, which is roughly the size of the decline nationally between 1985 and 1995. This metric of change will be used as a guideline for providing relative magnitudes of our estimated effects.

V. The Impact of Early Life Diarrhea Exposure on Human Capital Accumulation

Va. Height and Cognition in Adolescence

Columns 1-4 of Table 2 presents the estimates of models that build up to equation (3) when assigning exposure based on the baseline municipality of residence. Panel A and Panel B provide results when using height-for-age and Raven's test score as the outcome of interest, respectively. Starting in column 1 the model only controls for mother's age, birth order fixed effects, year-month of birth fixed effects, and year-month of interview fixed effects. The estimates in column 1 for both panels suggest that exposure to PAL in infancy was detrimental to physical and cognitive development. This relationship though is likely driven by the fact that areas with worse pre-intervention diarrheal mortality rates, and thus high intensity of exposure, are unhealthier environments for human capital growth in general.

Once fixed differences between municipalities are controlled in column 2, through the inclusion of municipality fixed effects, this adverse relationship completely disappears. In its place we see the first evidence of a non-trivial positive impact of access to clean water in infancy on stature and cognition.

The substantial change in coefficient magnitudes between columns 1 and 2 in Panels A and B are highly suggestive that there are inherent differences between Mexican municipalities on characteristics related to our outcomes of interest. In an effort to further control these differences, Column 3 adds municipality-specific linear time trends and a vector of municipality-specific pre-intervention non-diarrhea characteristic interactions. In both Panels A and B purging the estimates of bias-inducing unobserved trends municipality-specific trends helps to uncover a larger positive relationship between access to clean water in infancy and physical and cognitive growth. Specifically, the estimates in

column 3 suggest that exposure to a one standard deviation decrease in the diarrheal mortality rate (i.e. 10 per 1,000 decline) from PAL in infancy led to .11 s.d. increase in height and a 1.7 percentile point increase in Raven's test score.

While the results in column 3 provide evidence of the persistent beneficial impact of access to clean water in infancy, there is potential that these results are biased as a result of mortality selection, fertility selection, or selection into live birth resulting from PAL. Thus, in column 4 we introduce maternal fixed effects into equation (3).[23]

The introduction of within-family comparisons in column 4 only strengthens the evidence that exposure to PAL in infancy had a long-lasting impact on physical and cognitive development. In Panel A, the effect of access to clean water in infancy is equal in size to what was found in column 3, while in Panel B, the removal of bias related to selective parentage has a much more conspicuous impact as it doubles the size of the relationship between early life exposure to clean water and cognition.

The results in column 4 indicate that there is some PAL related negative selection into the sample. We directly explore this by estimating the relationship between our treatment intensity measures and maternal characteristics. This analysis, provided in Appendix Table 2, shows that the sample of individual's exposed to greater PAL intensity in infancy are more likely to have less intelligent, poorer, and less healthy mothers, as indicated by the mother's Raven's test score, household per capita expenditure and the mother's likelihood of being obese (BMI>30) and underweight (BMI<18.5), respectively.[24]

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[23]When employing a within-family analysis it is important to confirm that the compositional change in the sample is leading to result that are generalizable or comparable to the full sample. In our case, results analogous to those found in columns 1-3 of Panels A and B but using the mother-fixed effects sample are qualitatively similar and, if anything, suggest the within-family comparisons may be providing a more conservative estimate. These results are found in Appendix Table 1.

[24] Children exposed to a 1 standard deviation increase in PAL intensity on average have mothers with 21% less household per capita expenditure and that are almost 3 times more likely to be underweight (i.e. BMI<18.5). The difference in mother's Raven's score and likelihood obesity while large in magnitude, an average decline of 3% for cognition and an average 8% increase for obesity for a 1 standard deviation increase in PAL intensity, are only marginally significant at the .13 and .15 level, respectively.

There are three primary reasons this change in composition exists. The most likely cause of this selection is the well-documented relationship between access to clean water and the reduction of infant mortality generally (Cutler and Miller, 2005; Field et al., 2011; Galiani et al., 2005; Gamper-Rabindran et al., 2005; Watson, 2005; among others) and in the specific context of PAL (Bhalotra et al., 2017). Given that the increase in survival is likely occurring at the bottom of the health distribution, this change would lead to an increase in the number of children in the survey from poorer or less healthy households in places that had the most intense exposure to PAL.

A second potential reason for this compositional change is that access to clean water may cause selection into live birth. If the positive health shock of PAL creates a healthier environment for pregnant women, some pregnancies that previously were not viable will now conclude in a live birth. As with infant mortality, this selection would be the most pronounced at the bottom of the family wealth and health distribution and lead to the type of selection found in Appendix Table 2.

Lastly, if we think PAL is lowering the cost of child quality, it could induce less well off families who were previously on the margin of family expansion due to budget constraints to select into fertility. Regardless of the exact cause(s), removing this negative bias inducing selection with the within-family comparisons is what allows the considerably larger estimate of PAL exposure's impact on cognition to come through in column 4 of Panel B as compared to column 3.

An important remaining question with regard to the results in Table 2 is whether they are driven by any unobserved trends in the general health environment correlated with the decline in diarrheal mortality. To mitigate this concern, Appendix Table 3 replicates our main within-family results in its first column and compares them in columns 2 and 3 to estimates that additionally include controls for municipality-specific trends in the general health environment and state-birth year specific unobserved factors, respectively. In each case, for both outcomes, the introduction of these controls either confirms or increases the positive relationship between access to clean water in infancy

and persistent gains in cognitive and physical growth.[25] Specifically, the model that includes both sets of controls of unobserved health trends suggests that experiencing a 1 s.d. intensity of the PAL effect (i.e. a 10 in 1,000 decline in the under 5 diarrheal mortality effect) in infancy will on average lead to a .15 standard deviation increase in both height and cognitive assessment score (3.4 percentage point increase relative to a standard deviation of 22.25) in adolescence.

As mentioned in section IV, one potential concern with interpreting and generalizing our estimates is that in order to utilize the entire analytical sample we assign treatment intensity using municipality of residence in adolescence rather than the more limited information we have available on each respondent's municipality of birth. In order to assess whether this choice is leading to misinterpretation of the impact of access to clean water in infancy, we re-estimate our coefficients of interest using only the sample with municipality of birth responses. We perform this re-estimation in two ways. In the first replication we continue to assign exposure based on municipality of residence and in the second version assign exposure based on municipality of birth. The estimates from this exercise are provided in Appendix Table 4. The results restricted to the sample with municipality of birth information that continues to use municipality of residence to assigned exposure are found in columns 1-3 of Appendix Table 4 and the estimates that use municipality of birth to assign exposure are found in columns 4-6 of Appendix Table 4.

Comparing columns 1-3 to columns 4-6 in Appendix Table 4 provides strong evidence that assigning exposure based on municipality of residence versus municipality of birth is not biasing the magnitudes of our estimates away from 0, and if anything, this

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[25] Since the addition of the alternative disease trend controls and state-year fixed effects does not substantially alter the estimates but potentially cause a reduction in efficiency, we do not include them when reporting within-family estimates of the other outcomes explored in the paper. As in Table 2, the inclusion or exclusion of these controls does not significantly change the magnitude of the estimates or conclusions drawn from the analyses.

choice is leading to more conservative estimates on Raven's scores. In addition, the magnitudes of our main estimates from Table 2 are qualitatively and quantitatively similar to the estimates using municipality of birth to assign exposure in Appendix Table 4 (columns 4-6). The critical difference between using the full sample versus only individuals with municipality of birth information is the non-trivial gains to representativeness. Comparing columns 1-3 in Appendix Table 4 with the equivalent full sample estimates, provide evidence that excluding respondents with missing municipality of birth information would bias the estimate of the impact on cognition towards 0.

Vb. Event Study Analysis

Two important questions remain when assessing the results in Table 2. First, is there any evidence of an increasing trend in human capital outcomes in areas with higher levels of pre-PAL diarrheal mortality? While we have shown that including municipality-specific linear trends, vectors of pre-intervention municipality characteristics and alternative mortality rates interacted with our cohorts of interest, as well as, state-year fixed effects in equation (3) does not change our conclusions, and if anything reveals a larger positive relationship, there is still the possibility that we have not captured all possible confounding unobserved trends.

Second, our estimates thus far have focused on the impact of access to clean water throughout infancy. This choice is motivated by the epidemiological and biological evidence that both the incidence of diarrhea (i.e. treatability by the water reform or treatment intensity)[26] and the rate of physical and cognitive development (i.e. the elasticity of the outcome to the treatment) are greatest during this period. Empirically though, we can directly explore the impact at older ages of first exposure as well.

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[26] The *MxFLS* survey queries the incidence of diarrhea in the two weeks preceding the date of interview. As many as 25% of infants in the sample had diarrhea in the two weeks preceding the survey, compared with 5 to 10% for 5 to 50 year olds.

In order to provide evidence related to both of these concerns we estimate an event study analysis. Specifically, we re-estimate the within-family difference version of equation (3) for the 1986-1992 birth cohorts, but replace *Full_t* and *Partial_t* with a vector of indicator variables for every birth cohort in the sample except 1989.[27] As is standard, our exclusion cohort is chosen as the year just prior to exposure. In our case, 1989, is the birth cohort just prior to our first exposure group, *Partial_t*. Since in our data cohort and period coincide, the results in this sub-section also implicitly confirm the absence of trend breaks in the coefficient series in years before 1991. Thus, the event study acts as a placebo test of whether we have identified impacts generated by the water reform rather than some other confounding trend.

The results of the event study analyses are provided in Figures 6 and 7 for height-for-age and Raven's score, respectively. In both cases, the coefficients for birth cohorts exposed for the first time after infancy are small and insignificant. Moreover, there is a noticeable jump in the event study coefficients for both the impact on stature and cognition for the cohorts exposed during infancy. As a whole the event study analyses provide evidence supportive of the assumptions that our estimates are not driven by unaccounted for trends correlated with the timing and intensity of PAL and that the most advantageous exposure period is in infancy.

Vc. Persistence of the PAL Effect on Height and Cognition into Early Adulthood

An additional advantage of the MxFLS that we can exploit for our study is the ability to see if the effects on height and intelligence we uncovered in adolescence at baseline persist into early adulthood (17-26 years of age) by using the most recent wave of the survey, MxFLS3. Being able to examine the persistence of gains produced by an early-

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[27] In the event study analysis all birth cohorts are redefined as containing those born from 4/1/YOB-3/31/YOB+1. This allows us to more closely match our relevant cohorts from the main analysis, *Full_t* and *Partial_t*. Thus for example, in the event study the 1991 cohort is defined as those born between 4/1/1991-3/31/1992.

life health shock by studying the same cohorts at two different points in time is a novel and unique opportunity in this literature.

The complication that arises with this type of analysis, and one of the reasons we employ the baseline survey for our main results, is that by MxFLS3 we no longer have outcome information for all of our respondents. In fact, while overall survey attrition between the baseline MxFLS survey and MxFLS3 was only slightly over 10%, non-response to Raven's and height measurement was considerably higher. Amongst the sample of interest, we are missing MxFLS3 height records for ~35% and Raven's score information for ~45% of our respondents.

To assess the way this will impact our interpretation of findings for cognition and stature using the available MxFLS3 sample, we start by comparing our main estimates in Table 2 to analogous estimates using MxFLS baseline data but only on the sample which also has MxFLS3 data. Column 1 of Table 3 reproduces results from our main analysis in Table 2 and then column 2 estimates this same model but restricted only to the respondents with MxFLS3 data for the outcome of interest. In both panels A and B we see a sizable decrease in the estimated effect size for height-for-age and cognition. This suggests that non-response for our outcomes of interest in MxFLS3 is systematically higher for respondents that experienced the largest physical and cognitive development benefits from PAL exposure. Thus, we would expect that our analysis of the relationship between access to clean water in infancy and height and intelligence in early adulthood using the MxFLS3 underestimates the true effect.

After establishing the downward bias that will be generated by moving to the MxFLS3 sample, we next re-estimate our models using data from the MxFLS3 survey in columns 3-4. In Panel A, despite using a sample we know will cause our estimates to be biased towards 0, the coefficient from our preferred specification in column 4 indicates that PAL continued to have a positive impact on height for age into early adulthood.[28]

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[28] While in MxFLS1 the age range of our cohorts allows us to use WHO child growth charts to estimate height-for-age, that is not the case in MxFLS3. Thus, in MxFLS3 we use simple height for

Similarly, while we believe the MxFLS3 sample will lead to an attenuate estimate of the impact of PAL exposure in infancy on cognition, the results in Panel B of Table 3 still provide evidence of a persistent effect of the program. As with height, our analysis of Raven's scores in young adulthood provide coefficients comparable in magnitude to the ones found in Panel B of Table 2.

Our estimates suggest that, similar to our baseline results, experiencing a one standard deviation improvement in diarrheal mortality rates from PAL in infancy leads to a .18 standard deviation increase in height for age and a 10% (5.4 points relative to an average of 55.5) increase in cognitive tests scores on average.[29,30] Overall, while the loss in power due to non-response has reduced the precision of our estimates using MxFLS3 data and likely generated bias towards 0, the results in Table 3 provide a strong indication that the gains in stature and cognition from exposure in infancy to PAL are long-lasting.[31]

Vd. Other Human Capital and Economic Outcomes in Adolescence

In addition to examining the impact of early life access to clean water on direct measures of cognitive and physical growth, the richness of the MxFLS allows us to also explore if exposure to PAL in infancy led to direct changes in their educational outcomes and economic behavior. We start by examining the impact of increased access to clean water in infancy on attained education in adolescence by estimating the within-family

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age z-scores that are calculated as (height-mean height)/standard deviation of height separately for each age and gender, where the mean and standard deviation are calculated for our cohorts of interest, the 1986-1992 birth cohorts.

[29] The within-family estimates in Panel B of Table 3 are similar or larger than those in Panel B of Table 2, but the significant (45%) loss in sample size makes them much less precisely estimated.

[30] Estimating the impact of PAL exposure in early adulthood when assigning exposure using municipality of birth information provides a similar effect size of a .15 standard deviation increase in height and a 3-10% increase in cognitive test scores using our preferred within-family specification.

[31] These results are not sensitive to the inclusion of alternative disease trends and state-year fixed effects.

version of equation (3).[32] This analysis is found in column 1 of Table 4 and provides evidence of increased attained education for children that gained access to clean water throughout infancy. The estimate suggests that a one standard deviation decrease in the under-5 diarrheal mortality rate throughout infancy led to a statistically significant 4% increase in years of attained education (.2 years compared to an average of 5.3).

This change in educational attainment is likely the result of one of two channels, either the kids exposed to PAL during infancy are staying in school longer or are progressing in school faster. To parse these two mechanisms we proceed in column 2 to test if PAL exposure led to changes in likelihood that our respondents are currently attending school. There is no evidence of any impact on this behavior. In order to explore school progression, in columns 3 and 4, we estimate our within-family version of equation (3) using whether the individual has repeated any grade and whether the individual has started secondary school as our outcomes of interest, respectively. In both cases there are large and statistically significant differences for individuals with greater PAL exposure throughout infancy, as they are found to repeat fewer grades and to be more likely to have progressed to secondary school.

Lastly, since employment even at these young ages is not uncommon in Mexico (12.5% of our sample of 10-16 year olds) we examine if the decision to join the labor market as early as adolescence is affected by access to clean water early in life. Column 5 of Table 4 provides our findings when using whether the individual is currently employed as our outcome of interest, which indicate that PAL exposure in infancy leads to a delay in labor market engagement. It is possible that this decision reflects a tradeoff between current income and the long-term gains from greater investment in education that those with greater cognitive and physical capacity are willing to make.

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[32] Results are qualitatively and quantitatively similar if the alternative disease trends and state-year fixed effects are also included in the model.

Ve. Other Human Capital and Economic Outcomes in Early Adulthood

As with our exploration of the impact of access to clean water on cognitive and physical development into early adulthood, the longitudinal nature of the MxFLS also allows to also investigate how changes in educational and economic outcomes persist later in life. As before we rely on data from the MxFLS3 to provide this analysis, but unlike when examining stature and intelligence, the loss of sample due to non-response is much less stark. Specifically, the loss in our sample of interest from MxFLS1 to MxFLS3 on education and employment outcomes is only 4 and 20%, respectively.

In column 1 of Table 5 we explore whether the boost in educational attainment we found in adolescence continues into early adulthood. While the estimated effect remains positive, it is considerably smaller in size and no longer statistically distinguishable from 0. Given our previous findings that the primary mechanism behind the difference in years of education in adolescence was not a result of school dropout but rather speed of progression, the findings in Table 5 suggest eventually the students terminate their studies at an equivalent level.

While the gains in cognition and stature do not lead to different educational attainment, we next examine if it changes an individual's labor market outcomes. In column 2 of Table 5 we see that there is no evidence that early life exposure to PAL impacts an individual's likelihood of being employed in early adulthood. With about half of our sample of interest actively working in early adulthood, the better early life health environment for the PAL exposed individuals does not seem to impact whether they are employed or not.

Despite no difference in labor market participation, the benefit to the cognitive and physical advantage possessed by those who had access to clean water during infancy may show up in their level of productivity, either through greater salary or reduced hours of work. For individuals in the labor market, in columns 3-4 we investigate these economic channels by estimating our within-family version of equation (3) on the outcomes of total

hours worked last week and earning per hour, respectively.[33] The results tell a story of increased productivity for individuals with greater access to clean water throughout infancy. While estimated on a small and selected sample, the findings suggest those exposed to a 1 standard deviation in decrease in the under-5 diarrhea mortality rate due to PAL throughout infancy are earning 37 more pesos per hour, which represents approximately a 150% increase compared to the average. Thus, while access to clean water has not led to more education or likelihood of employment, it has, through an increase in physical and cognitive growth, delivered the opportunity for higher paying positions.

VI. Potential Alternative Mechanisms

We interpret our estimates as stemming from access to clean water in infancy which decreased the likelihood of diarrheal infections that would have directly impaired physical and cognitive development by diminishing net nutrition in a critical period.[34] Here we consider the plausibility of competing explanations. First, we consider whether the primary mechanism may be a reduction in the *adult* burden of disease which impacts children's physical and cognitive development either through productivity-led improvements in household income or through improved maternal health leading to better fetal and infant health. This is unlikely given the low levels of adult diarrhea incidence. It is estimated that diarrhea morbidity rates in the developing world for adults are at most 0.3 to 0.8 episodes per year in contrast with around 3 episodes per year for children under the age of five (Fischer Walker and Black 2010, Fischer Walker et al 2012). Also, if the channel were income related, we may expect to see impacts throughout childhood,[35] but our

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[33] There is no direct earning per hour measure in the MxFLS, so this is estimated as earnings per month divided by hours worked last week times four.

[34] The positive shock of access to clean water in infancy may also be reinforced through the increased presence and productivity of subsequent parental investments.

[35] For instance, Dahl and Lochner (2012) examine income shocks and record impacts for children above the age of 2.

finding that effects fade after infancy and are hardly discernible after the age of one is consistent with what is initially a biological mechanism.[36]

VII. Gender Differences in Program Effects

VIIa. Female Schooling, Labor Force Participation, and the Gender Division of Labor in the Mexican Economy

It is common in both the experimental and quasi-experimental literature on the relationship between an improved health environment in childhood and long-term human capital accumulation to find that the impacts differ significantly with respect to the gender of the child (Almond and Mazumder, 2011; Banerjee et al., 2010; Bhalotra and Venkataramani, 2012; Cutler et al., 2010; Macinni and Yang, 2009; Miguel and Kremer, 2004; Pitt, Rosenzweig, and Hassan 2012; Rosenzweig and Zhang, 2013; among others). In our specific study context, gender differences may arise due to the dramatic shift in Mexico, since the mid-20th century, to a more skill-intensive economy.

Evidence of these employment and occupation trends by gender, are provided by census micro-data for 1960 to 2000 from IPUMS. To track calendar time trends for adult cohorts (as distinct from cohort trends), we computed statistics for individuals aged 20-50 years at enumeration. Based on linkage of Mexican occupational categories to job characteristics from the US Dictionary of Occupational Titles (Vogl 2012), we classified individuals in employment as being in brain- *vs* brawn-intensive occupations.

We estimate labor force participation and gender-based occupational sorting by estimating the equations displayed in Table 6 for each available census year in 1960-2000 (no census was conducted in 1980). Thus, looking across columns provides the trend in the coefficient across four decades. What we find is that the ratio of female to male employment

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[36] Unfortunately the MxFLS is not well suited to exploring what potential mechanism may be driving the larger cognitive effect of PAL exposure for girls as compared to boys as it lacks good measures of birth outcomes and parental investments for our sample of interest. Future exploration of this question using linked administrative and/or household surveys represents an opportunity to further contribute to the understanding the mechanisms behind PAL's benefits.

rose from about a fifth to a half between 1960 and 2000 and increases in labor force participation were greater amongst more educated women (panel A). There is also evidence of sorting: conditional on employment, women were 10 to 20 percentage points more likely to be in brain-intensive occupations than men and educated women more so. Again there is a positive trend (panel B).[37] Since education is endogenous, the estimates in the right hand column where interactions of gender and education are presented are only descriptive, but overall the evidence in Table 6 shows that women, particularly educated women, were increasingly entering the labor force and sorting into brain-intensive jobs.[38]

Figures 8 and 9 show related plots. *Figure 8a* plots the share of brawn-intensive industries (agriculture, construction, and mining) as a function of GDP between 1970 and 2000 using National Accounts data. We see a large increase (decrease) in the skill-intensity (brawniness) of the economy, which is particularly pronounced starting in the 1980s, when Mexico liberalized foreign trade and investment policies. This was reinforced in 1994 with the NAFTA agreement (Hanson 2003). *Figure 8b* shows, using the cross-section of Mexican states, that the share of brain-intensive occupations increases as the share of agriculture, construction and mining increases. Concomitant with the trends in skill intensity in *Figure 8a*, census data show that women are over-represented in brain-intensive occupations and that this tendency is growing stronger over time (*Figures 9A, 9B*). Among men, even in the most recent census data, more than 80% work in brawn-intensive occupations. Thus, the Mexican economy, while characterized by increasing skill-intensity (and, therefore, more labor force opportunities for women) continues to have a significant brawn-intensity. The proportion of women in the labor force has shown a rapid rise (from 10% in 1960 to nearly

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[37] The share of women in brain-intensive occupations shows an upward blip in the 1990 census. We examined the data but could not identify any reason that this is not genuine. We rest nothing upon this coefficient so here we simply flag it.

[38] Our evidence is consistent with findings from other work. For example, a recent study by Aguayo-Tellez et al (2010) illustrates how trade liberalization as a result of the North American Free Trade Agreement (NAFTA) led to increased employment opportunities and wages for women.

40% in 2000, *Figure 9C*), and women's schooling (*Figure 9D, 9E*) has also increased more rapidly for women than for men.

VIIb. Differential Impact of PAL exposure in Infancy by Gender

In order to explore the presence of gendered effects of PAL exposure in infancy that follow the pattern suggested by the characteristics of the Mexican economy, we re-estimate our main analysis in Table 7 but additionally interact our treatment variables, *Full$_t$*BaseRate$_j$* and *Partial$_t$*BaseRate$_j$* , with an indicator for the respondent being male.[39] In Table 7 the dependent variable being used is height-for-age in Panel A and Raven's score in Panel B.

The results of the within-family estimates in column 4 of Panel A suggest that the benefits to height from access to clean water, though slightly more evident for women, are shared and not statistically different across gender. The evidence of a gendered effect of PAL is even more pronounced for Raven's score. In column 4 of Panel B there is evidence that the scores of treated males increase significantly less than treated females. While the magnitude of the impact for males, a 2.4% increase relative to the mean from a one standard deviation improvement in diarrheal mortality rates, it is not precisely estimated and is more than three times smaller than the impact for women. Specifically, the within-family estimates in column 4 of Panel B suggest that a one standard deviation improvement in diarrheal mortality rates in from PAL in infancy generates an 8.6% increase in cognitive test scores for girls.[40]

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[39] The model also includes interactions of the indicator for the respondent being male with f(mom's age)$_m$, Full$_t$*MuniChars$_j$, and Partial$_t$*MuniChars$_j$, respectively.

[40] Importantly, it should be noted that the model used to produce these results does not utilize fixed effect control vectors that vary by gender. The relatively small sample size and added reduction in variation from the use of within-family comparisons, precludes the use of a fully interacted version of equation (3). A version of equation 3 that interacts our continuous control variables (i.e. f(mom's age)$_m$), year-month of birth fixed effects, year-month of interview fixed effects, municipality fixed effects, birth order fixed effects, and pre-intervention municipality characteristic trends (i.e. Full$_t$*MuniChars$_j$, and Partial$_t$*MuniChars$_j$) with an indicator for the

While imprecisely estimated and significantly smaller than the effect for girls, the magnitude for the impact on height and cognition for boys should not be considered economically insignicant. For instance, similarly sized estimates when using the full sample size, as in Column 2 of Table 2, are statistically different from 0. In support of the idea that males are also experiencing important cognitive benefits to early life exposure to clean water, Table 8 shows that the increased educational progress found in Table 4 are predominately a change experienced by males. Specifically, it is the exposed males that are attaining more years of education, being less likely to repeat a grade, and progressing into secondary school at a higher rate than their non-exposed comparison peers at statistically significant levels. In addition to these education gains, males are also the driving force behind the finding in Table 4 that access to clean water in utero is keeping children out of the labor market at young ages.

Lastly, in Table 9 we explore if the persistence effect of PAL exposure in early life found in Table 3 is differentially accruing to males or females. Column 1 of Table 9 looks at the impact of PAL on stature in young adulthood for males and females and finds that the effect does not appear to be gendered. Column 2 explores the longer-term impact of PAL exposure in utero and, similar to the results in Table 7, finds that, while both genders see large gains to cognition in early adulthood, the effect for females remains larger. Specifically, the estimates in column 2 of Table 9 suggest that a one standard deviation improvement in diarrheal mortality rates in from PAL in infancy generates an 15.0% increase in cognitive test scores for girls in early adulthood and a 9.5% increase in cognitive test scores for boys in early adulthood.[41] Columns 3-4 of Table 9 concludes the analysis by investigating if there is evidence of PAL exposure generating differential

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respondent being male provides results of qualitatively and quantitatively similar magnitude, which are less precise and rely on identifying variation from a less representative sample.

[41]The estimate for boys, though, is imprecise due to the reduced sample size available for this outcome in the MxFLS3 and has a p-value of .2.

impacts on educational attainment and employment by gender and finds no indication of this relationship.[42]

VIIc. Alternative Explanations for Gendered Program Impacts

In this section we consider other explanations of the identified gender differences in outcomes and investments. A natural explanation of our finding that improved cognitive performance flowing from exposure to clean water during infancy is larger for girls than boys is that diarrhea among girls fell more sharply as a result of the reform. However, estimates of the "first stage" show that the intervention was associated with a decline in diarrhea for boys that was no smaller (indeed slightly larger) than the decline for girls (Bhalotra et al., 2017).

A second possibility is greater survival selection among boys. Following the implementation of the clean water intervention, the marginal survivor will have been relatively frail and, given their higher biological vulnerability in early childhood (Kraemer 2000), one might expect greater (negative) selection among boys. If innate ability is lower amongst children in the left tail of the health distribution then it is possible that our finding of weak test score impacts for boys arises from selection. However, diarrhea mortality rates for boys were only slightly larger than for girls and, as discussed, the reform-led decline in diarrhea (in absolute and proportional terms) was similar for boys and girls, so selection effects are unlikely to be large enough to explain our results. The role of selection is also undermined by the finding that boys exposed to the program experienced gains in height that were similar to girls.

A third possibility is that the differential results reflect catch-up if girls were initially lagging boys in educational investments and test scores.[43] However, Raven's scores were

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[42] A significantly reduced sample size, and thus identifying variation, prohibits us from estimating the equivalent model for hours worked or earnings per hour in MxFLS3.

[43] Assuming a production function for cognition that shows diminishing returns to educational inputs, if girls were initially at a more concave point on the curve then the marginal returns to investments would be larger for girls.

very similar between the sexes prior to the program, as were the range of educational investments that we considered. Finally, might our results arise from brain development in girls being more sensitive to early life health shocks than boys? There is no biomedical evidence to support this, if anything, boys' brains may be more sensitive to *in utero* and early life insults (Kraemer 2000).

VIII. Conclusions

A large body of work highlights the importance of strength and cognitive skills in driving socioeconomic outcomes, making it important from a public policy standpoint to understand how to narrow gaps in physical and cognitive development. We find that access to clean water in early-life, through its reduction of infectious disease exposure, has a positive causal influence on cognition and stature. Specifically, our estimates suggest that that exposure in infancy to a one standard deviation decrease in the diarrheal mortality rate (i.e. 10 per 1,000 decline) as a result of a clean water intervention led to a .11 and .15 standard deviation increase in height and cognitive assessment score, respectively.

These magnitudes compare favorably to those from other health (or nutritional) interventions/shocks. In particular, these gains are similar to those estimated in Bharadwaj et al (2013), who find that more intensive care for low birth weight neonates resulted in 0.1 and 0.2 standard deviation increases in language and mathematics scores, respectively, in a sample of 10-16 year olds. This similarity in the benefits to these positive early life health shocks are present despite the fact that the long-term costs of national water reform are likely much smaller than the costs of freely provided advanced neonatal care. Alternatively, Almond, Mazumder and Van Ewijk (2011) show that Ramadan fasting among pregnant mothers led to 0.05 to 0.08 standard deviation decrease in test scores for 7-year old Muslim children in British schools, Venkataramani (2012) finds that birth year exposure to malaria eradication in 1950s Mexico led to a 0.1-0.2 standard deviation increase in adult Raven scores for men and Barham (2012) discovered that increased access to an early

childhood and family planning program in Bangladesh led to a 0.39 standard deviation increase in mini-Mental State exam scores.

In this paper we also uniquely provide evidence that these gains in physical and cognitive growth persist into early adulthood and lead to significant increases in labor market productivity. These results imply that clean water provision, acting through infant disease exposure, can potentially help induce economic growth and shrink the classic cross-country differences between developed and developing countries.

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Figure 1 – Pre-Intervention Under-5 Diarrheal Mortality Rates
Across Mexican Municipalities (1985-1990 average)

Legend:
- (22.43307,70.57927]
- (12.02613,22.43307]
- (6.274709,12.02613]
- [.2229654,6.274709]
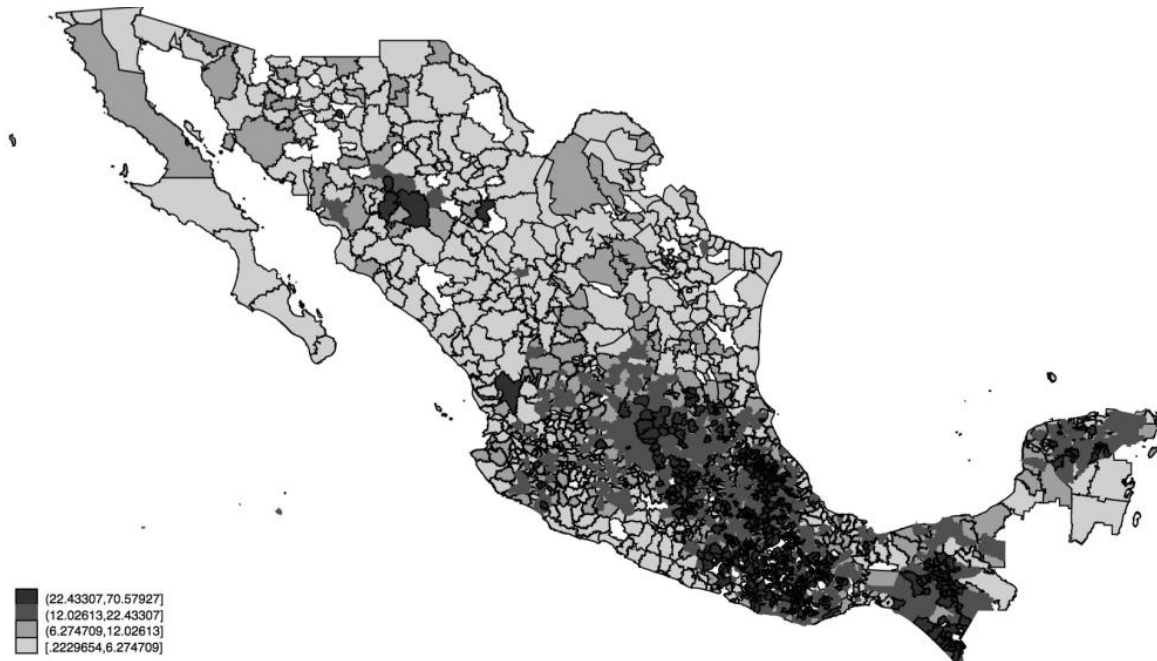
Notes: Map plots average diarrheal mortality rates per 1,000 live births over the period 1985-1990 for children under the age of 5 by municipality. Darker colors reflect higher average pre-intervention diarrheal mortality rates. Data to construct map were obtained from the Mexico Ministry of Health, Vital Statistics.

Figure 2 – Timeline and Scope of Mexico's National Clean Water Program
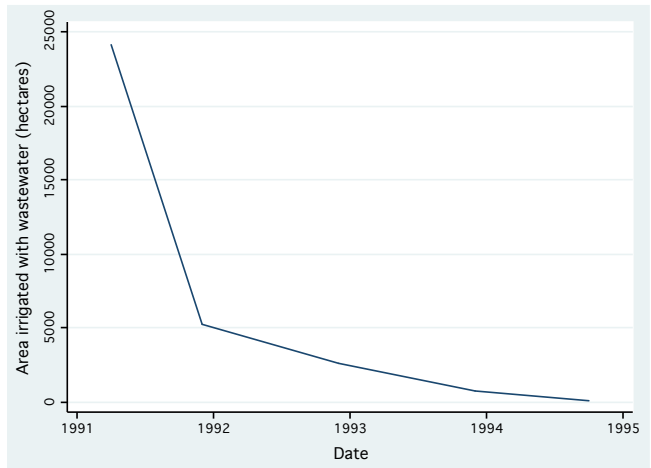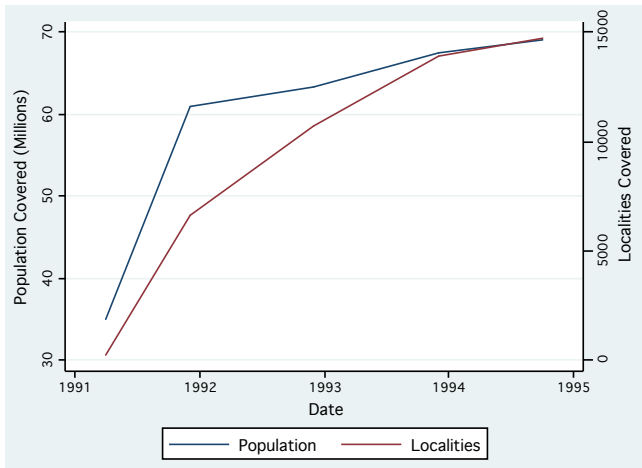
A. Population Access to Chlorinated Water

B. Land Area Irrigated with Waste Water

Source: Government of Mexico, National Water Commission

Figure 3 – Trends in Child Infectious Disease Mortality:
Diarrhea, Respiratory Infections, and Vaccine Preventable Infections
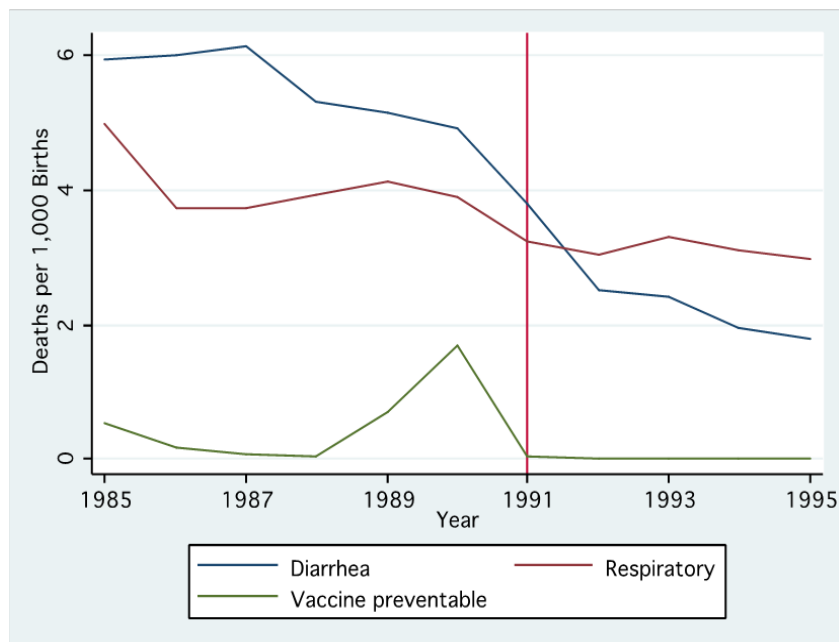
Source: See *Figure 1*.

Figure 4 – Event Study Coefficient Plots
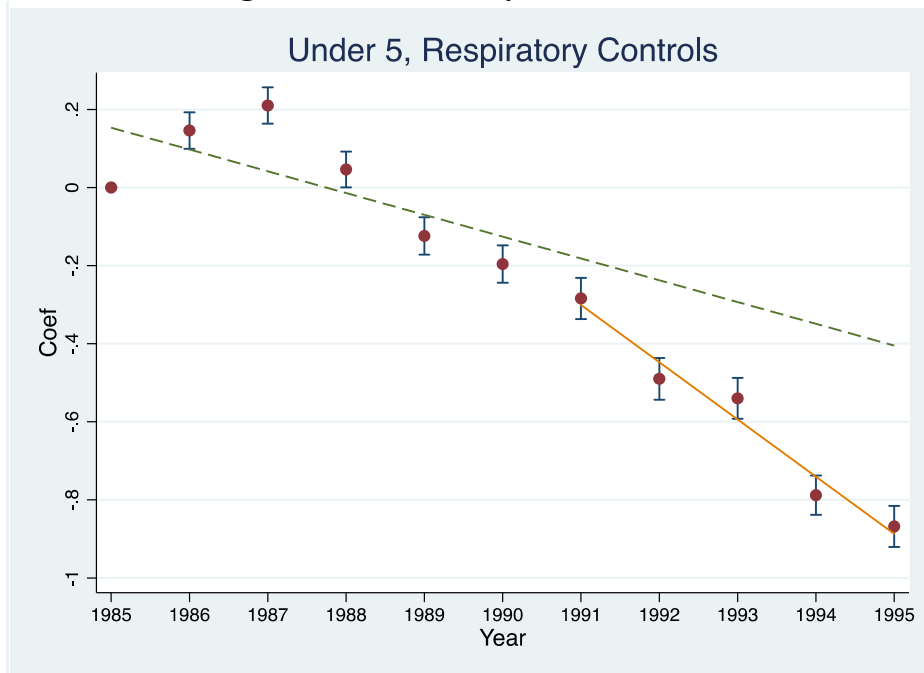
Notes: Estimates of the Diarrhea*Year coefficients from Equation 1. Coefficients are denoted by dots and the bands denote the 95% confidence interval of the estimates. The dashed green line is a predicted trend line calculated using pre-1991 coefficients, which we extend across the time series. The solid orange line is the trend line for the 1991-1995 coefficients. The gap between the two can be interpreted as the PAL treatment effect.

Figure 5 – Convergence across States in Child Diarrhea Mortality Rates

Scatter Plot of Absolute Change in Diarrheal Mortality Post-1991 and the Pre-Intervention Rate
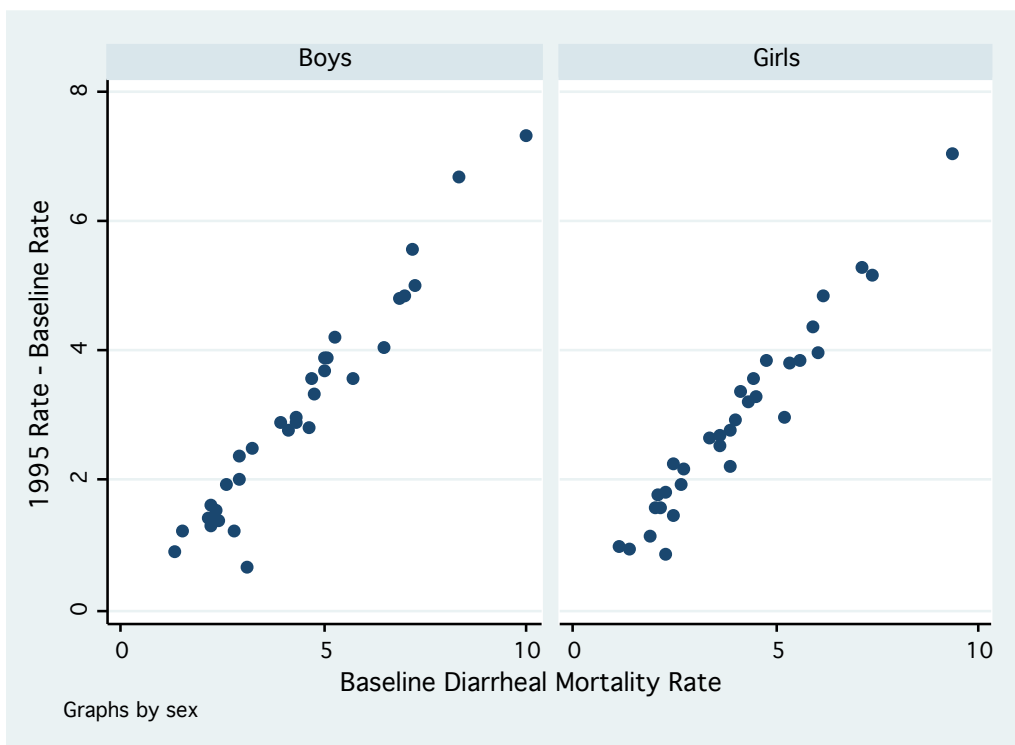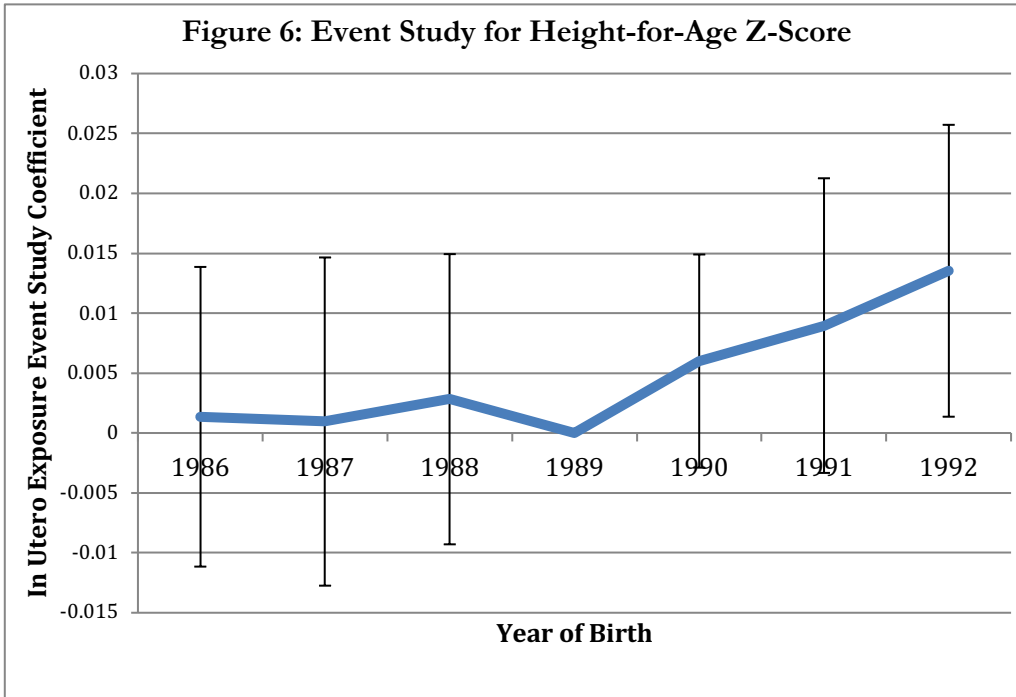
49

Figure 6: Event Study for Height-for-Age Z-Score

Notes: Standard errors are clustered at the municipality of residence level. 90% confidence intervals are provided. Birth cohorts are defined from 4/1/YOB-3/31/YOB+1.

Figure 7: Event Study for Raven's Score
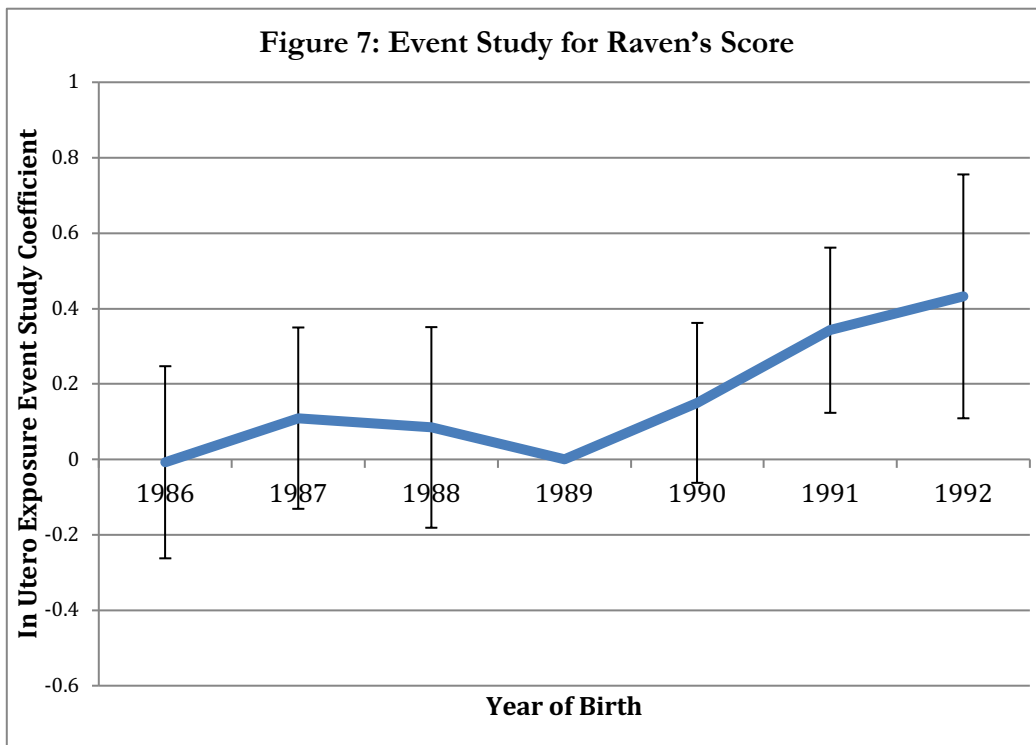
Notes: Standard errors are clustered at the municipality of residence level. 90% confidence intervals are provided. Birth cohorts are defined from 4/1/YOB-3/31/YOB+1.

Figure 8a – Trend Decline in Share of Agricultural, Mining, and Construction as % of GDP
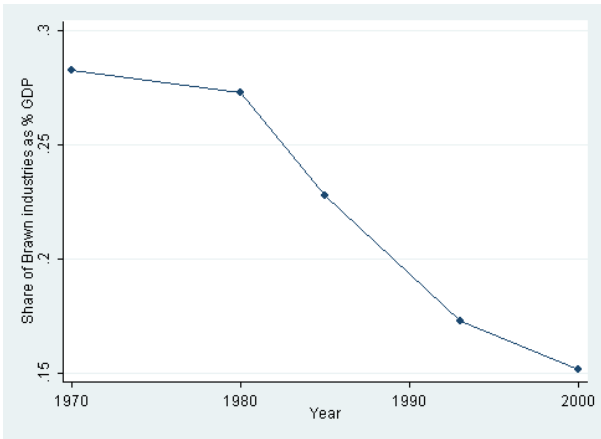
Source: National Accounts Data provided by Ernesto Aguayo-Tellez.

Figure 8b- Share of Employment in Brain-Intensive Occupations is Decreasing in Agriculture, Mining and Construction Share : State Means
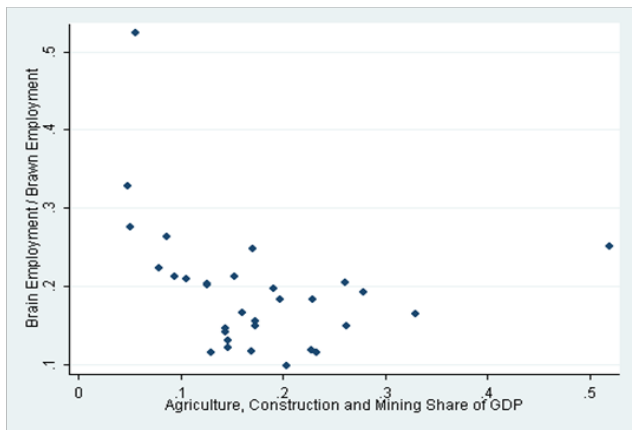
Figure 9 – Trends in Occupational Sorting, Employment and Schooling by Gender

(a) The left panel above plots the fraction of all individuals in brain-intensive occupations, showing women (the lower line) catching up with men by virtue of a stronger trend (B) the right panel plots this same fraction for individuals in employment, showing the fraction for women (the upper line) is consistently higher than for men. Source: Census microdata
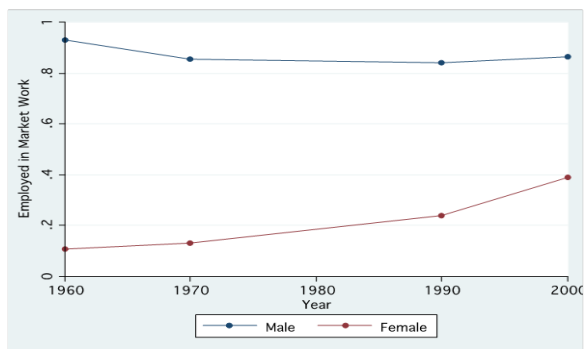
(C) The left panel above shows a stronger trend in employment for women (the lower line) than for men
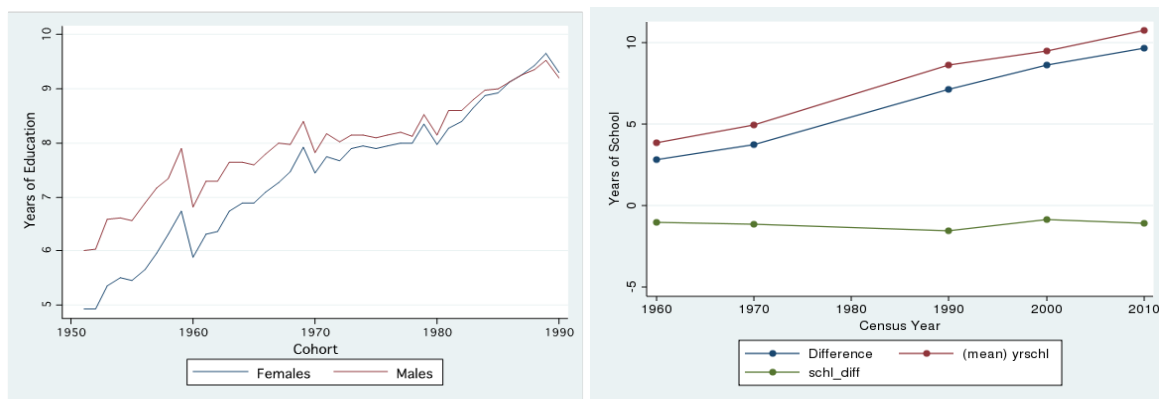
(D) the left panel shows the years of education of women (lower line) are converging towards those of men (MxFLS data) and (E) the right panel shows that in the employed sample, women (the upper line) have more education than men. The lowest line is the difference between years of school of employed men and women. Source, unless otherwise stated: Census microdata

Table 1: Diarrheal Mortality Relative to Respiratory Diseases Mortality Pre-Post PAL
From 1985 and 1995

	(1)	(2)	(3)	(4)
	Under-5 Years	*0-1 Months*	*1-12 Months*	*1-4 Years*
1(Diarrhea)*1(Post)	-0.119***	-0.0651***	-0.136***	0.008
	(0.020)	(0.020)	(0.021)	(0.020)
1(Diarrhea)*1(Post)*Year	-0.0909***	-0.0868***	-0.0422***	-0.0321***
	(0.008)	(0.008)	(0.008)	(0.008)
1(Diarrhea)*Year	-0.0558***	-0.0489***	-0.0509***	-0.0347***
	(0.005)	(0.005)	(0.005)	(0.004)
1(Diarrhea)	-0.133***	-0.361***	0.147***	0.144***
	(0.039)	(0.040)	(0.031)	(0.025)
Observations	48,906	48,906	48,906	48,906
% Decline by 1995 Due to PAL	48.2	41.2	30.5	17.1

Notes: Estimates of Equation 2 in the main text. *** - $p<0.01$, ** - $p<0.05$, * - $p<0.1$. Standard errors are clustered at the municipality level. All models include municipality and year fixed effects and are weighted by municipality baseline live births (i.e., the average for 1988-1990). Each column represents a separate regression, with the column header denoting the specific age group over which the inverse hyperbolic sine transformation of mortality was calculated for the dependent variable. 1(Diarrhea) =1 denotes diarrheal mortality rates, while 1(Diarrhea) = 0 denotes mortality from respiratory diseases. Post = 1 if the year of observation is 1991 or thereafter. The final row, % Decline by 1995 Due to PAL is calculated by adding the coefficient on the level break to four times that of the trend break (since 1995 is 4 years after PAL). The year variable is rescaled such that 1991 = 0 and increments above and below are denoted by positive and negative integers.

Table 2: Impact of Clean Water Program Exposure in Infancy on Height and Raven's Cognitive Score Measured in MxFLS1 For Individuals Born Between 1986 and 1992

	(1)	(2)	(3)	(4)
Panel A: Height for Age Z-Score				
Full*Base Rate	-0.016***	0.009***	0.011*	0.011*
	[0.003]	[0.003]	[0.006]	[0.006]
Partial*Base Rate	-0.022***	0.001	0.002	0.006
	[0.004]	[0.004]	[0.005]	[0.005]
Mean dep. variable	-0.43	-0.43	-0.43	-0.56
Observations	4,687	4,687	4,687	2,726
Number of mothers	-	-	-	1,172
Panel B: Raven's % Score (0-100)				
Full*Base Rate	-0.203***	0.091**	0.171*	0.340***
	[0.059]	[0.044]	[0.103]	[0.113]
Partial*Base Rate	-0.278***	0.036	0.063	0.082
	[0.065]	[0.043]	[0.078]	[0.112]
Mean dep. variable	61.74	61.74	61.74	60.40
Observations	4,973	4,973	4,973	2,909
Number of mothers	-	-	-	1,254
Municipality Fixed Effects	NO	YES	YES	YES
Municipality Specific Trends	NO	NO	YES	YES
Maternal Fixed Effects	NO	NO	NO	YES

Notes: Standard errors are clustered at the municipality of residence level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. All specifications include an indicator for gender, date of birth fixed effects, date of interview fixed effects, birth order fixed effects, mother's age at birth, and mother's age at birth squared. Raven Progressive Matrix test scores are defined as the fraction of questions answered correctly, range 0 to 100. Height for age Z-Score calculated using the STATA command "zanthro" and utilizing the WHO child growth charts. "Full"=1 if the individual was born on or after April 1991, "Partial"=1 if the individual was born between April 1990 and March 1991, and "Base Rate" is the gender-specific child diarrheal mortality rate in the municipality of residence averaged across the pre-intervention years, 1988-1990. "Municipality Specific Trends" includes municipality-specific linear time trends and two vectors of pre-intervention municipality of residence characteristics interacted with "Full" and "Partial", respectively. Pre-intervention municipality characteristics include: percent of population with access to piped water, percent of population with access to sewage system, percent of population has indigenous heritage, mean education level, and mean log of income.

Table 3: Impact of Clean Water Program Exposure in Infancy on Height and Raven's Cognitive Score Measured in MxFLS3 For Individuals Born Between 1986 and 1992

	MxFLS1 Survey		MxFLS3 Survey	
	Full Sample	MxFLS3 Sample		
	(1)	(2)	(3)	(4)
Panel A: Height for Age Z-Score				
Full*Base Rate	0.011*	0.006	0.010	0.018**
	[0.006]	[0.007]	[0.006]	[0.008]
Partial*Base Rate	0.002	0.004	0.006	0.005
	[0.005]	[0.006]	[0.004]	[0.006]
Mean dep. variable	-0.433	-0.41	0.02	-0.03
Observations	4,687	3,057	3,057	1,580
Number of mothers	-	-	-	708
Panel B: Raven's % Score (0-100)				
Full*Base Rate	0.171*	0.089	0.104	0.536*
	[0.103]	[0.124]	[0.106]	[0.309]
Partial*Base Rate	0.063	-0.017	-0.181	0.136
	[0.078]	[0.080]	[0.120]	[0.215]
Mean dep. variable	61.74	63.38	57.50	55.54
Observations	4,973	2,726	2,726	1,224
Number of mothers	-	-	-	558
Municipality Fixed Effects	YES	YES	YES	YES
Municipality Specific Trends	YES	YES	YES	YES
Maternal Fixed Effects	NO	NO	NO	YES

Notes: Standard errors are clustered at the municipality of residence level. *** p<0.01, ** p<0.05, * p<0.1. All specifications include an indicator for gender, date of birth fixed effects, date of interview fixed effects, birth order fixed effects, mother's age at birth, and mother's age at birth squared. Raven Progressive Matrix test scores are defined as the fraction of questions answered correctly, range 0 to 100. Height-for-age z-Score in MxFLS1 calculated using the STATA command "zanthro" and utilizing the WHO child growth charts. Height for age z-scores in MxFLS3 are calculated as (height-mean height)/standard deviation of height separately for each age and gender. "Full"=1 if the individual was born on or after April 1991, "Partial"=1 if the individual was born between April 1990 and March 1991, and "Base Rate" is the gender-specific child diarrheal mortality rate in the municipality of residence averaged across the pre-intervention years, 1988-1990. "Municipality Specific Trends" includes municipality-specific linear time trends and two vectors of pre-intervention municipality of residence characteristics interacted with "Full" and "Partial", respectively. Pre-intervention municipality characteristics include: percent of population with access to piped water, percent of population with access to sewage system, percent of population has indigenous heritage, mean education level, and mean log of income.

Table 4: Impact of Clean Water Program Exposure in Infancy on Other Human Capital Outcomes Measured in MxFLS1
For Individuals Born Between 1986 and 1992

	Years of Education	Attends School	Repeated Grade	Started Secondary School	Employed
	(1)	(2)	(3)	(4)	(5)
Full*Base Rate	0.020**	-0.001	-0.008*	0.005**	-0.005***
	[0.009]	[0.002]	[0.004]	[0.002]	[0.002]
Partial*Base Rate	0.007	0.000	-0.002	0.003	-0.004**
	[0.009]	[0.002]	[0.003]	[0.002]	[0.002]
Mean dep. variable	5.31	0.895	0.274	0.316	0.125
Observations	3,033	2,971	2,926	3,033	3,015
Number of mothers	1,298	1,278	1,262	1,298	1,292
Municipality Fixed Effects	YES	YES	YES	YES	YES
Municipality Specific Trends	YES	YES	YES	YES	YES
Maternal Fixed Effects	YES	YES	YES	YES	YES

Notes: Standard errors are clustered at the municipality of residence level. *** p<0.01, ** p<0.05, * p<0.1. All specifications include date of birth fixed effects, date of interview fixed effects, birth order fixed effects, mother's age at birth, and mother's age at birth squared. "Full"=1 if the individual was born on or after April 1991, "Partial"=1 if the individual was born between April 1990 and March 1991, and "Base Rate" is the gender-specific child diarrheal mortality rate in the municipality of residence averaged across the pre-intervention years, 1988-1990. "Municipality Specific Trends" includes municipality-specific linear time trends and two vectors of pre-intervention municipality of residence characteristics interacted with "Full" and "Partial", respectively. Pre-intervention municipality characteristics include: percent of population with access to piped water, percent of population with access to sewage system, percent of population has indigenous heritage, mean education level, and mean log of income.

Table 5: Impact of Clean Water Program Exposure in Infancy on Economic Outcomes Measured in MxFLS3
For Individuals Born Between 1986 and 1992

	Years of Education	Employed	Hours Worked Last Week Conditional on Working	Earnings per Hour
	(1)	(2)	(3)	(4)
Full*Base Rate	0.009	0.002	-0.602	3.693*
	[0.015]	[0.006]	[1.069]	[2.151]
Partial*Base Rate	0.020	-0.001	-1.280*	3.259**
	[0.015]	[0.004]	[0.720]	[1.357]
Mean dep. variable	9.76	0.46	42.76	22.04
Observations	2,406	2,036	526	455
Number of mothers	1,036	897	249	217
Municipality Fixed Effects	YES	YES	YES	YES
Municipality Specific Trends	YES	YES	YES	YES
Maternal Fixed Effects	YES	YES	YES	YES

Notes: Standard errors are clustered at the municipality of residence level. *** p<0.01, ** p<0.05, * p<0.1. All specifications include date of birth fixed effects, date of interview fixed effects, birth order fixed effects, mother's age at birth, and mother's age at birth squared. "Full"=1 if the individual was born on or after April 1991, "Partial"=1 if the individual was born between April 1990 and March 1991, and "Base Rate" is the gender-specific child diarrheal mortality rate in the municipality of residence averaged across the pre-intervention years, 1988-1990. "Municipality Specific Trends" includes municipality-specific linear time trends and two vectors of pre-intervention municipality of residence characteristics interacted with "Full" and "Partial", respectively. Pre-intervention municipality characteristics include: percent of population with access to piped water, percent of population with access to sewage system, percent of population has indigenous heritage, mean education level, and mean log of income.

Table 6 - Employment and Occupational Sorting by Gender and Education, Mexican Census 1960-2000

Panel A: Labor Force Participation

Census Year	(1) 1960	(2) 1970	(3) 1990	(4) 2000	(5) 1960	(6) 1970	(7) 1990	(8) 2000
Female	-0.798***	-0.704***	-0.580***	-0.501***	-0.846***	-0.796***	-0.797***	-0.704***
	(0.020)	(0.025)	(0.021)	(0.017)	(0.009)	(0.011)	(0.011)	(0.012)
Education	0.0110***	0.00722***	0.0154***	0.0172***	0.00233	-0.00438***	-9.09E-05	0.00333**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
Female*Education					0.0193***	0.0272***	0.0331***	0.0269***
					(0.002)	(0.002)	(0.001)	(0.001)
N	135,920	154,610	2,873,475	3,702,327	135,920	154,610	2,873,475	3,702,327

Panel B: Employment in Brain-Intensive Occupations

Census Year	-1 1960	-2 1970	-3 1990	-4 2000	-5 1960	-6 1970	-7 1990	-8 2000
Female	0.106***	0.138***	0.234***	0.129***	0.0234**	0.0332***	0.0658***	-0.0493***
	(0.011)	(0.006)	(0.003)	(0.003)	(0.010)	(0.005)	(0.014)	(0.009)
Education	0.0513***	0.0508***	0.0513***	0.0533***	0.0472***	0.0465***	0.0465***	0.0463***
	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)
Female*Education					0.0223***	0.0223***	0.0202***	0.0206***
					(0.003)	(0.002)	(0.002)	(0.001)
N	70,081	76,285	1,511,215	2,188,066	70,081	76,285	1,511,215	2,188,066

Note: Robust standard errors corrected for clustering at the state level in parenthesis. *** - $p<0.01$, ** - $p<0.05$, * - $p<0.10$. Census microdata, people aged from 20 to 50 years old at the time of enumeration, census year is in the column header. Dependent variables: Employment = 1 if the individual reports working, 0 otherwise. Employed in a Brain-Intensive Occupation = 1 if the individual is working in an occupation considered brain intensive as per Vogl (2012), 0 otherwise. Education is years as schooling. Every equation includes birth year, census year and enumeration state fixed effects.

Table 7: Impact of Clean Water Program Exposure in Infancy on Height and Raven's Cognitive Score Measured in MxFLS1 by Gender For Individuals Born Between 1986 and 1992

	(1)	(2)	(3)	(4)
Panel A: Height for Age Z-Score				
Full*Base Rate	-0.005	0.023***	0.026***	0.024***
	[0.005]	[0.004]	[0.006]	[0.007]
Full*Base Rate*I(Male=1)	-0.017***	-0.021***	-0.012**	-0.009
	[0.006]	[0.005]	[0.006]	[0.008]
Partial*Base Rate	-0.017***	0.009**	0.010	0.017*
	[0.006]	[0.004]	[0.007]	[0.009]
Partial*Base Rate*I(Male=1)	-0.011	-0.015***	-0.005	-0.010
	[0.007]	[0.005]	[0.008]	[0.011]
p-value for H0: Total "Full" Male Effect=0	0.00	0.72	0.09	0.01
p-value for H0: Total "Partial" Male Effect=0	0.00	0.11	0.76	0.38
Mean dep. variable	-0.43	-0.43	-0.43	-0.56
Observations	4,687	4,687	4,687	2,726
Number of mothers	-	-	-	1,172
Panel B: Raven's % Score (0-100)				
Full*Base Rate	-0.217**	0.101	0.306**	0.522***
	[0.088]	[0.070]	[0.135]	[0.134]
Full*Base Rate*I(Male=1)	0.023	-0.017	-0.214*	-0.376**
	[0.092]	[0.070]	[0.123]	[0.163]
Partial*Base Rate	-0.287***	0.070	0.152	0.059
	[0.079]	[0.060]	[0.098]	[0.138]
Partial*Base Rate*I(Male=1)	0.022	-0.072	-0.167	-0.025
	[0.105]	[0.076]	[0.133]	[0.227]
p-value for H0: Total "Full" Male Effect=0	0.00	0.07	0.60	0.38
p-value for H0: Total "Partial" Male Effect=0	0.00	0.97	0.44	0.86
Mean dep. variable	61.74	61.74	61.74	60.40
Observations	4,973	4,973	4,973	2,909
Number of mothers	-	-	-	1,254
Municipality Fixed Effects	NO	YES	YES	YES
Municipality Specific Trends	NO	NO	YES	YES
Maternal Fixed Effects	NO	NO	NO	YES

Notes: Standard errors are clustered at the municipality of residence level. *** p<0.01, ** p<0.05, * p<0.1. All specifications include an indicator for gender, date of birth fixed effects, date of interview fixed effects, birth order fixed effects, mother's age at birth, and mother's age at birth squared. Raven Progressive Matrix test scores are defined as the fraction of questions answered correctly, range 0 to 100. Height for age Z-Score calculated using the STATA command "zanthro" and utilizing the WHO child growth charts. "Full"=1 if the individual was born on or after April 1991, "Partial"=1 if the individual was born between April 1990 and March 1991, and "Base Rate" is the gender-specific child diarrheal mortality rate in the municipality of residence averaged across the pre-intervention years, 1988-1990. "Municipality Specific Trends" includes municipality-specific linear time trends and two vectors of pre-intervention municipality of residence characteristics interacted with "Full" and "Partial" for each gender, respectively. Pre-intervention municipality characteristics include: percent of population with access to piped water, percent of population with access to sewage system, percent of population has indigenous heritage, mean education level, and mean log of income.

Table 8: Impact of Clean Water Program Exposure in Infancy on Other Human Capital Outcomes Measured in MxFLS1 by Gender For Individuals Born Between 1986 and 1992

	Years of Education	Attends School	Repeated Grade	Started Secondary School	Employed
	(1)	(2)	(3)	(4)	(5)
Full*Base Rate	0.007	0.000	-0.006	0.004	-0.004
	[0.012]	[0.002]	[0.005]	[0.003]	[0.003]
Full*Base Rate*I(Male=1)	0.027***	-0.001	-0.004	0.003	-0.002
	[0.010]	[0.001]	[0.004]	[0.002]	[0.002]
Partial*Base Rate	-0.003	0.000	-0.003	0.002	-0.004
	[0.012]	[0.003]	[0.004]	[0.003]	[0.003]
Partial*Base Rate*I(Male=1)	0.020	0.002	0.001	0.002	0.002
	[0.017]	[0.003]	[0.005]	[0.003]	[0.004]
p-value for H0: Total "Full" Male Effect=0	0.00	0.60	0.01	0.00	0.00
p-value for H0: Total "Partial" Male Effect=0	0.11	0.42	0.74	0.06	0.26
Mean dep. variable	5.31	0.895	0.274	0.316	0.126
Observations	3,033	2,971	2,926	3,033	3,015
Number of mothers	1,298	1,278	1,262	1,298	1,292
Municipality Fixed Effects	YES	YES	YES	YES	YES
Municipality Specific Trends	YES	YES	YES	YES	YES
Maternal Fixed Effects	YES	YES	YES	YES	YES

Notes: Standard errors are clustered at the municipality of residence level. *** p<0.01, ** p<0.05, * p<0.1. All specifications include an indicator for gender, date of birth fixed effects, date of interview fixed effects, birth order fixed effects, mother's age at birth, and mother's age at birth squared. "Full"=1 if the individual was born on or after April 1991, "Partial"=1 if the individual was born between April 1990 and March 1991, and "Base Rate" is the gender-specific child diarrheal mortality rate in the municipality of residence averaged across the pre-intervention years, 1988-1990. "Municipality Specific Trends" includes municipality-specific linear time trends and two vectors of pre-intervention municipality of residence characteristics interacted with "Full" and "Partial" for each gender, respectively. Pre-intervention municipality characteristics include: percent of population with access to piped water, percent of population with access to sewage system, percent of population has indigenous heritage, mean education level, and mean log of income.

Table 9: Impact of Clean Water Program Exposure in Infancy on Outcomes Measured in MxFLS3 by Gender
For Individuals Born Between 1986 and 1992

	Height for Age Z-Score	Raven's % Score (0-100)	Years of Education	Employed
	(1)	(2)	(3)	(4)
Full*Base Rate	0.014	0.829**	0.022	0.009
	[0.009]	[0.342]	[0.023]	[0.008]
Full*Base Rate*I(Male=1)	0.005	-0.302	-0.015	-0.001
	[0.011]	[0.387]	[0.021]	[0.008]
Partial*Base Rate	0.009	-0.181	0.031	0.005
	[0.011]	[0.332]	[0.026]	[0.005]
Partial*Base Rate*I(Male=1)	-0.007	0.611	0.004	-0.007
	[0.012]	[0.371]	[0.038]	[0.009]
p-value for H0: Total "Full" Male Effect=0	0.10	0.20	0.71	0.16
p-value for H0: Total "Partial" Male Effect=0	0.78	0.19	0.18	0.78
Mean dep. variable	-0.03	55.54	9.76	0.46
Observations	1,580	1,224	2,406	2,036
Number of mothers	708	558	1,036	897
Municipality Fixed Effects	YES	YES	YES	YES
Municipality Specific Trends	YES	YES	YES	YES
Maternal Fixed Effects	YES	YES	YES	YES

Notes: Standard errors are clustered at the municipality of residence level. *** p<0.01, ** p<0.05, * p<0.1. All specifications include an indicator for gender, date of birth fixed effects, date of interview fixed effects, birth order fixed effects, mother's age at birth, and mother's age at birth squared. The model also includes interactions of the indicator for the respondent being male with f(mom's age)m, Full*MuniChars, and Partial*MuniChars, respectively. "Full"=1 if the individual was born on or after April 1991, "Partial"=1 if the individual was born between April 1990 and March 1991, and "Base Rate" is the gender-specific child diarrheal mortality rate in the municipality of residence averaged across the pre-intervention years, 1988-1990. "Municipality Specific Trends" includes municipality-specific linear time trends and two vectors of pre-intervention municipality of residence characteristics interacted with "Full" and "Partial" for each gender, respectively. Pre-intervention municipality characteristics include: percent of population with access to piped water, percent of population with access to sewage system, percent of population has indigenous heritage, mean education level, and mean log of income.

**Appendix Table 1: Impact of Clean Water Program Exposure in Infancy
on Height and Raven's Cognitive Score Measured in MxFLS1
For Individuals Born Between 1986 and 1992
Using Maternal Fixed Effects Sample**

	(1)	(2)	(3)
Panel A: Height for Age Z-Score			
Full*Base Rate	-0.013***	0.011**	0.016*
	[0.004]	[0.004]	[0.008]
Partial*Base Rate	-0.020***	0.001	0.010*
	[0.004]	[0.003]	[0.006]
Mean dep. variable	-0.56	-0.56	-0.56
Observations	2,726	2,726	2,726
Panel B: Raven's % Score (0-100)			
Full*Base Rate	-0.154**	0.101	0.108
	[0.067]	[0.067]	[0.105]
Partial*Base Rate	-0.224***	0.044	0.121
	[0.078]	[0.072]	[0.088]
Mean dep. variable	60.40	60.40	60.40
Observations	2,909	2,909	2,909
Municipality Fixed Effects	NO	YES	YES
Municipality Specific Trends	NO	NO	YES

Notes: Standard errors are clustered at the municipality of residence level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. All specifications include an indicator for gender, date of birth fixed effects, date of interview fixed effects, birth order fixed effects, mother's age at birth, and mother's age at birth squared. Raven Progressive Matrix test scores are defined as the fraction of questions answered correctly, range 0 to 100. Height for age Z-Score calculated using the STATA command "zanthro" and utilizing the WHO child growth charts. "Full"=1 if the individual was born on or after April 1991, "Partial"=1 if the individual was born between April 1990 and March 1991, and "Base Rate" is the gender-specific child diarrheal mortality rate in the municipality of residence averaged across the pre-intervention years, 1988-1990. "Municipality Specific Trends" includes municipality-specific linear time trends and two vectors of pre-intervention municipality of residence characteristics interacted with "Full" and "Partial", respectively. Pre-intervention municipality characteristics include: percent of population with access to piped water, percent of population with access to sewage system, percent of population has indigenous heritage, mean education level, and mean log of income.

Appendix Table 2: Impact of Clean Water Program Exposure in Infancy on Mother's Characteristics Measured in MxFLS1
For Individuals Born Between 1986 and 1992

Mother's Characteristics:

	Raven's Score	Height for Age Z-Score	Household Per Capita Expenditure	Underweight (BMI<18.5)	Obese (BMI>30)	Finished Compulsory Schooling
	(1)	(2)	(3)	(4)	(5)	(6)
Full*Base Rate	-0.148	0.001	-24.317*	0.002*	0.003	0.001
	[0.096]	[0.006]	[13.241]	[0.001]	[0.002]	[0.002]
Partial*Base Rate	-0.074	0.010***	-6.179	0.001***	-0.001	0.000
	[0.097]	[0.003]	[10.999]	[0.001]	[0.002]	[0.001]
Mean dep. variable	43.54	-0.03	1,165.10	0.007	0.374	0.317
Observations	4,579	4,465	5,066	4,425	4,425	4,814
Municipality Fixed Effects	YES	YES	YES	YES	YES	YES
Municipality Specific Trends	YES	YES	YES	YES	YES	YES
Pre-Intervention Non-Diarrheal Child Mortality Rates	YES	YES	YES	YES	YES	YES
State of Residence-by-Year of Birth Fixed Effects	YES	YES	YES	YES	YES	YES

Notes: Standard errors are clustered at the municipality of residence level. *** p<0.01, ** p<0.05, * p<0.1. All specifications include date of birth fixed effects, date of interview fixed effects, birth order fixed effects, mother's age at birth, and mother's age at birth squared. Raven Progressive Matrix test scores are defined as the fraction of questions answered correctly, range 0 to 100. Height for age z-scores are calculated as (height-mean height)/standard deviation of height separately for each age. "Full"=1 if the individual was born on or after April 1991, "Partial"=1 if the individual was born between April 1990 and March 1991, and "Base Rate" is the gender-specific child diarrheal mortality rate in the municipality of residence averaged across the pre-intervention years, 1988-1990. "Municipality Specific Trends" includes municipality-specific linear time trends and two vectors of pre-intervention municipality of residence characteristics interacted with "Full" and "Partial", respectively. Pre-intervention municipality characteristics include: percent of population with access to piped water, percent of population with access to sewage system, percent of population has indigenous heritage, mean education level, and mean log of income. Pre-intervention non-diarrheal child mortality rates interactes "Full" and "Partial" with respiratory diseases and vaccine preventable diseases.

Appendix Table 3: Impact of Clean Water Program Exposure in Infancy on Height and Raven's Cognitive Score Measured in MxFLS1
For Individuals Born Between 1986 and 1992

	(1)	(2)	(3)
Panel A: Height for Age Z-Score			
Full*Base Rate	0.011*	0.017***	0.015**
	[0.006]	[0.006]	[0.006]
Partial*Base Rate	0.006	0.009	0.006
	[0.005]	[0.006]	[0.007]
Mean dep. variable	-0.56	-0.56	-0.56
Observations	2,726	2,726	2,724
Number of mothers	1,172	1,172	1,171
Panel B: Raven's % Score (0-100)			
Full*Base Rate	0.340***	0.313***	0.343***
	[0.113]	[0.104]	[0.120]
Partial*Base Rate	0.082	0.211*	0.324***
	[0.112]	[0.117]	[0.121]
Mean dep. variable	60.40	60.40	60.39
Observations	2,909	2,909	2,907
Number of mothers	1,254	1,254	1,253
Municipality Fixed Effects	YES	YES	YES
Municipality Specific Trends	YES	YES	YES
Maternal Fixed Effects	YES	YES	YES
Pre-Intervention Non-Diarrheal Child Mortality Rates	NO	YES	YES
State of Residence-by-Year of Birth Fixed Effects	NO	NO	YES

Notes: Standard errors are clustered at the municipality of residence level. *** p<0.01, ** p<0.05, * p<0.1. All specifications include an indicator for gender, date of birth fixed effects, date of interview fixed effects, birth order fixed effects, mother's age at birth, and mother's age at birth squared. Raven Progressive Matrix test scores are defined as the fraction of questions answered correctly, range 0 to 100. Height for age Z-Score calculated using the STATA command "zanthro" and utilizing the WHO child growth charts. "Full"=1 if the individual was born on or after April 1991, "Partial"=1 if the individual was born between April 1990 and March 1991, and "Base Rate" is the gender-specific child diarrheal mortality rate in the municipality of residence averaged across the pre-intervention years, 1988-1990. "Municipality Specific Trends" includes municipality-specific linear time trends and two vectors of pre-intervention municipality of residence characteristics interacted with "Full" and "Partial", respectively. Pre-intervention municipality characteristics include: percent of population with access to piped water, percent of population with access to sewage system, percent of population has indigenous heritage, mean education level, and mean log of income. Pre-intervention non-diarrheal child mortality rates interactes "Full" and "Partial" with respiratory diseases and vaccine preventable diseases.

Appendix Table 4: Impact of Clean Water Program Exposure in Infancy on Height and Raven's Cognitive Score Measured in MxFLS1
For Individuals Born Between 1986 and 1992
Using Only Respondents with Municipality of Birth Information

	Exposure Assigned Using Municipality of Residence			Exposure Assigned Using Municipality of Birth		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Height for Age Z-Score						
Full*Base Rate	0.015*	0.015**	0.017**	0.014	0.017	0.017
	[0.008]	[0.007]	[0.007]	[0.011]	[0.011]	[0.012]
Partial*Base Rate	0.013	0.011	0.010	0.015	0.010	0.007
	[0.009]	[0.013]	[0.015]	[0.014]	[0.018]	[0.021]
Mean dep. variable	-0.58	-0.58	-0.59	-0.58	-0.58	-0.59
Observations	1865	1865	1856	1865	1865	1858
Number of mothers	829	829	825	829	829	826
Panel B: Raven's % Score (0-100)						
Full*Base Rate	0.208	0.180	0.182	0.249	0.254	0.320
	[0.153]	[0.185]	[0.195]	[0.207]	[0.259]	[0.268]
Partial*Base Rate	-0.127	0.042	0.143	-0.020	0.088	0.262
	[0.239]	[0.325]	[0.294]	[0.366]	[0.476]	[0.466]
Mean dep. variable	60.21	60.21	60.20	60.21	60.21	60.20
Observations	1961	1961	1952	1961	1961	1952
Number of mothers	872	872	868	872	872	868
Municipality Fixed Effects	YES	YES	YES	YES	YES	YES
Municipality Specific Trends	YES	YES	YES	YES	YES	YES
Maternal Fixed Effects	YES	YES	YES	YES	YES	YES
Pre-Intervention Non-Diarrheal Child Mortality Rates	NO	YES	YES	NO	YES	YES
State of Residence-by-Year of Birth Fixed Effects	NO	NO	YES	NO	NO	YES

Notes: Standard errors are clustered at the municipality of assigned exposure level. *** p<0.01, ** p<0.05, * p<0.1. All specifications include an indicator for gender, date of birth fixed effects, date of interview fixed effects, birth order fixed effects, mother's age at birth, and mother's age at birth squared. Raven Progressive Matrix test scores are defined as the fraction of questions answered correctly, range 0 to 100. Height for age Z-Score calculated using the STATA command "zanthro" and utilizing the WHO child growth charts. "Full"=1 if the individual was born on or after April 1991, "Partial"=1 if the individual was born between April 1990 and March 1991, and "Base Rate" is the gender-specific child diarrheal mortality rate in the municipality of assigned exposure averaged across the pre-intervention years, 1988-1990. "Municipality Specific Trends" includes municipality-specific linear time trends and two vectors of pre-intervention municipality of birth characteristics interacted with "Full" and "Partial", respectively. Pre-intervention municipality characteristics include: percent of population with access to piped water, percent of population with access to sewage system, percent of population has indigenous heritage, mean education level, and mean log of income. Pre-intervention non-diarrheal child mortality rates interactes "Full" and "Partial" with respiratory diseases and vaccine preventable diseases.