Cognitive Development, Achievement, and Parental Investments: Evidence from a Clean Water Reform in Mexico

First Draft: December 2012

Sonia R. Bhalotra
Department of Economics
University of Bristol, UK
s.bhalotra@bristol.ac.uk

Atheendar S. Venkataramani
Department of Medicine
Massachusetts General Hospital, USA
avenkataramani@partners.org

Abstract: Mexico implemented a massive clean water reform in 1991 which led to a sharp drop in diarrhea of close to 50%. Using cohort and state variation in diarrhea reduction, we identify significant causal effects of birth year exposure to diarrhea on cognitive test scores and school achievement tests of teenage and pre-teenage girls, the corresponding estimates for boys being smaller and statistically insignificant. The gains are in the region of 0.1 standard deviations for a one standard deviation reduction in childhood diarrheal mortality rates. We find that the reform also stimulates complementary investments in girls and primarily girls, including preschool attendance and a shift in time from chores to homework, and the evidence suggests that the test score gains we identify hinge upon these investments. Parental responses are consistent with a Roy model in which a positive shock to the health endowment intensifies investments in girls' education given their comparative advantage over boys in brains relative to brawn. In support of this, we show evidence of differential sorting of women into brain-intensive occupations, and that this sorting is increasing in education and in the brawn base of the state economy.

Acknowledgements: This paper has benefited from presentation at the Paris School of Economics and the University of Bristol. We thank Damian Clark, Peter Gerrish, Claudia Herresthal, Salomo Hirvonen and Yichau Wu for research assistance.

I. Introduction

A growing literature underlines the importance of cognitive abilities and achievement in driving socioeconomic outcomes (Heckman, et al, 2006). Recent research has examined the production function for cognitive ability and the emerging consensus is that early childhood represents a critical period for the formation of cognitive skills given the rapid pace of brain development and the high degree of neuronal plasticity at this stage of life (Cunha and Heckman, 2007; Cunha, Heckman, and Schennach, 2010; Heckman, 2008). This model of skill formation suggests a role for early childhood health. In particular, severe or repeated early life infections may divert nutrients away from neurological development, particularly during infancy, when it is estimated that about 85% of calorie intake is used to build brains (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).

While recent studies establish causal effects of early childhood infections on education and labor market outcomes and cognitive development is a likely mechanism, there is little direct causal evidence of the impact of infectious disease on cognitive development.[1] Some recent studies, mostly set in developing countries, demonstrate that parents reinforce improvements in the early life cognitive and health endowments of their children with subsequent human capital investments (Almond and Mazumder, *forthcoming*, provide a survey). However, there is scarcely any research examining the extent to which these subsequent investments drive the link between better early childhood health and adult skills (see Bhalotra and Venkataramani 2012).

This paper examines the impact of waterborne infections in infancy, which manifest primarily with the symptom of diarrhea, on cognitive outcomes among a sample of Mexican teenagers. We focus on infant health given that the caloric requirements for brain development are higher during this period than at any other stage of the lifecourse[2]. To account for selectivity into infection, we exploit the introduction of the *Programa de Agua Limpia* (National Clean Water Program), a large-scale nationwide effort that led to sharp dramatic improvements in population coverage of chlorinated water and rapid declines in groundwater pollution due to legal restrictions on the use of sewage for irrigation (Gutierrez et al, 1996; Sepulveda et al, 2006). The introduction of the program is deemed exogenous as it was introduced quite suddenly in reaction to the threat of cholera arising from an epidemic spreading through the countries neighboring Mexico. We show that the introduction of the program led to rapid and sizeable drops in infant diarrheal mortality rates, with the absolute declines increasing in the pre-program diarrheal mortality rate. As in Acemoglu and Johnson (2007) and Bleakley (2007), our identification strategy exploits state*cohort variation that combines this sharp convergence pattern across the states with the timing of the nationwide reform.

We find sizeable positive effects of exposure to clean water (and thereby reduced diarrhea) in infancy on Raven test scores between the ages of 9-17 and performance on the Program for International Student Assessment (PISA) tests taken at the age of 15. These effects are only seen for

---

[1] Surveys of the literature are in Almond and Currie 2011a,b; Currie and Vogl 2012. Amongst studies of the impact of early life infectious disease on later life outcomes are Almond (1996), Bleakley (2007, 2010), Barreca (2010), Baird, et al (2011), Cutler, et al (2010), Lucas (2010), Bhalotra and Venkataramani (2012). Studies of the impact of early childhood health on cognitive outcomes and achievement include Almond and Edlund (2009), Almond and Mazumder (2011), Bharadwaj, et al (2012) Barham (2012), Maluccio et al. (2009) and Stein, et al (2005) but these studies analyse health shocks other than infection.

[2] In contrast, brain activity consumes 44% of a 5-year old child's metabolic intake and the figure for adults is estimated at 25%.

girls, and are robust to the inclusion of a rich set of controls, birth state specific trends, and a number of specification tests. A one standard deviation decrease in diarrheal mortality rates is associated with a 0.09 standard deviation increase in Raven scores and 0.08 standard deviation increases in PISA math and reading scores. These magnitudes compare favorably with the magnitude of test score gains associated with educational interventions.

In the second part of the paper we explore the gender difference in impacts. We first investigate whether the "first stage" might have operated differently for boys and girls but find no evidence of differential program effects on diarrheal mortality. We also argue that differential mortality selection cannot explain the observed gender gap in test scores. We reject the notion that girls exhibit larger gains in test scores because they are initially at a less convex point on the production function than boys since, for two of the three tests we analyze, girls do not start with lower scores. The hypothesis we defend is that the better cognitive performance of girls derives from their having received stronger parental investments that are complementary to the improved health endowment. We show that girls treated by the water reform were more likely to attend preschool, to attend school, and to have shifted time towards homework (and away from chores), and there is auxiliary evidence of the cognition-enhancing effects of these investments, in our data and in other contexts. For boys, we see a reduction in time spent on chores (smaller than the reduction for girls) but there is no significant response in the other indicators of education-related investments. We argue that gender-differentiated parental responses are consistent with a Roy-type model in which girls have a comparative advantage in brains and boys in brawn, and a health shock stimulates parental investments that play to these relative returns (Pitt, et al 2012; Rosenzweig and Zhang 2012). Using census data for Mexican adults of labor market age at the time that investments were being made in the sample cohorts, we present evidence that women disproportionately select into skill-intensive occupations, with sorting increasing in their education. Also, the differential sorting of educated women into brain-intensive occupations is greater in states with a more brawn-based economy.

Our findings suggest that early childhood infections drive cognitive skills and academic achievement, and that subsequent reinforcing investments are critical in driving this link. They contribute to the literature on the technology of skill formation which posits dynamic complementarities (Cunha and Heckman, 2007) and, in particular, provide insight into the education production function and the role of initial conditions determined in the public *vs* the private sphere (Heckman and Masterov 2007). A handful of recent studies, surveyed in Almond and Mazumder (*forthcoming*) identify reinforcing parental investments in human capital in response to improvements in the health endowment but causal estimates of the quantitative importance of these investments (which depends upon the technology) in producing later life outcomes are scarce (see Almond and Mazumder (*forthcoming*) and Almond and Currie, 2011a and b, for reviews and Bhalotra and Venkataramani 2012 for a recent attempt at this). We use the quasi-experimental conditions generated by the introduction of the water reform to identify reduced form effects of infant diarrhea on indicators of cognitive development. Using the same identification, we estimate reduced form equations for parental investments which show education-related investments increasing in girls treated by the reform, demonstrating that responsive investments are made and that they are reinforcing in nature. Then, by appeal to variation in labor market returns to investment in educational inputs for girls relative to boys, we are able to (implicitly) identify complementarity in the production of test scores and the quantitative importance of complementary investments in producing the observed cognitive gains.

A second contribution of this study is that it informs an emerging literature that seeks to understand empirical findings of gender differences in returns to public health interventions and schooling programs. The evidence suggests that labor market returns to investments in education are larger for girls in both rich and poor countries (Psacharopoulos and Patrinos 2004, Dougherty 2005), while returns to investments in health tend to be larger for boys (Thomas et al. 1997, Martorell et al 2009). Of interest here is how parental investments are differentiated by gender in response to gender differences in returns. Recent experimental studies of public health interventions find, like we do, that schooling for girls increases more than for boys (Miguel & Kremer 2004, Maluccio et al 2009). Pitt et. al. (2012) propose that this can be rationalized within a Roy model in which girls have a comparative advantage relative to boys in brain relative to brawn. They illustrate this using data from rural Bangladesh and a related paper extends the evidence to China (Rosenzweig & Zhang 2012). We contribute to this literature in the following ways. First, we use quasi-experimental changes in the availability of clean water to identify changes in the infant health endowment.[3] Second, we focus upon cognitive development rather than schooling. To the extent that early life health improvements *directly* raise cognitive development, this will tend to widen the gender difference created by differentiated parental responses (because of complementarity across inputs). Third, Mexico is a more developed country than Bangladesh or China and it is relevant to consider the extent to which the comparative advantage of men in brawn remains relevant.

Although this is central in a long-standing theoretical literature, there is relatively limited empirical evidence of investments in schooling responding to the rate of return to education, possibly because variation across individuals in the rate of return to schooling is rarely observed and sharp changes in returns seldom occur (see Abramitzky and Lavy 2012; Jensen 2010, 2012). A third contribution of this study is in this domain. Our study involves a quasi-experimental increase in the rate of return to education-related investments (in boys and girls) that flows from the water reform led improvement in infant health. We show that this raises investments in preschool and school attendance, homework. We further show that the relative return being larger for girls leads to larger investment increases in girls.

Finally, we contribute to a literature on test score gaps between boys and girls. The stylized fact is that girls under-perform in math and not in reading and recent work suggests that this may be driven by gender stereotypes (Fryer and Levitt 2010, Bharadwaj et al. 2012). We effectively draw the first link between this literature and the recent literature premised on biological differences across the sexes in brawn. We show that improvements in the health endowments of girls and boys lead to a narrowing of the gender gap in math scores, and that this is driven by relative returns on the labor market acting in a manner that, as it happens, opposes gender stereotypes. We estimate that the increase in math scores flowing from a one standard deviation decrease in diarrheal mortality rates closes 30% of the gender gap in Mexico and 8% of the gender gap between the Mexico and the United States.

This paper has policy implications for public health investments and, in particular, investments in the provision of clean water. Almost 1 billion people lack access to safe drinking water worldwide and it is estimated that each year there are more than 2 million deaths from waterborne diseases. Diarrhea is the most prevalent of waterborne diseases and is the second leading cause of child death in the world (UNICEF/WHO 2009; Lopez et al., 2006). Reducing child mortality is the fourth Millenium Development Goal and clean water forms a key part of the

---

[3] Pitt, et al (2010) compute a model of body mass index (BMI) using logged calorie consumption, activity in the chosen occupation, pregnancy, lactation status, a second order polynomial of age interacted with male gender, and water source as the main arguments. Recognising that the inputs are endogenous, they instrument them using household head age and schooling, land holdings, and a full set of food prices interacted with age, gender, and household head demographics. The health endowment is identified as a residual and identification is subject to the assumption that the instruments are valid. Rosenzweig and Zhang (2012) rely upon birth-weight differences between opposite-sex twins, with the attendant limitations of external validity.

seventh goal. Recent work highlights the immediate health-related benefits from water interventions (Ahuja, et al, 2010; Cutler and Miller, 2005; Galiani, et al, 2005; Gamper-Rabindran, et al, 2005; Watson, 2005) and we contribute what would appear to be the first evidence of long run impacts, and also the first causal evidence of impacts on cognition. Our results are topical given that the next set of targets for development is being discussed to determine priorities on a global scale. We show that the benefits of clean water reform extend beyond lowering morbidity and mortality to improving cognitive development and stimulating human capital investments, with potentially large impacts on economic growth. Since public interventions in the water domain primarily benefit poorer households, where diarrhea is more common, these interventions stand to reduce not only health inequalities but also inequalities in educational achievement and living standards. Our findings are germane in Mexico, where a lack of attention to early childhood development and subsequent schooling and growing psychosocial stress on families has been one of the factors implicated as response for the country's recent economic stagnancy (Arias, et al, 2012).

The remainder of this paper is as follows. Section II discusses the National Clean Water Program and establishes "first stage" impacts on diarrheal disease mortality. Section III discusses the empirical strategy and Section IV the data used to examine how indicators of cognition and academic achievement respond to early life exposure to diarrhea (or unclean water). These results are discussed in Section V. So as to investigate the mechanisms driving the results, Section VI analyses parental investments as a function of the water reform, and investigates alternative hypotheses for why these investments are gender differentiated. Section VII concludes.

II. Mexico's National Clean Water Program

IIa. History and Description

Through the late 1980s, infectious diseases were responsible for a significant proportion of infant and child deaths in Mexico. Diarrheal disease, which was predominantly centered in the poorer Southern states (*Figure 1*), was a particularly important scourge, accounting for nearly a quarter of under-5 deaths in this period (Gutierrez et al, 1996). Over the period of 1978-1997, public sector efforts were 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 health officials to undertake a suite of preventative measures, including 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). Of these efforts, policies related to clean water and sanitation were by far most extensive and largest in scope. Under the direction of the National Water Commission (*Comisión Nacional del Agua*, *CONAGUA*), Mexico implemented a National Clean Water Program (*Programa de Agua Limpia*) in April 1991, just a few months before the first case of cholera appeared in the country[4]. Program activities included the disinfection of previously untreated water supplies (primarily through chlorination) and the reduction of the use of wastewater for irrigation. The design and implementation of disinfection efforts was at the national level despite the fact that responsibility for clean water provision constitutionally rested with municipalities. Prevention of use of wastewater for irrigation was also

---

[4] As a preventative measure, individuals were subject to infectious disease surveillance at all ports of entry into Mexico. However, it proved impossible to monitor every route into the country: the index case was a drug smuggler who arrived via airplane from Peru.

spurred on at the national level, though the main policy instrument was the passage of legal amendments against such practices at the state level (Gutierrez et al., 1996; Instituto Mexicano de Tecnología del Agua, 2008). The total outlay for the program over the period 1991-1994 was US $1 billion[5].

The National Clean Water Program was large in size and scope and was implemented rapidly: the percentage of the population with access to chlorinated water almost doubled between 1990 and 1992 and the number of hectares irrigated with sewage water for the purposes of vegetable cultivation declined by nearly 90% (Gutierrez et al., 1996). *Figure 2a* illustrates changes in area and population coverage of chlorinated water. As seen in the graph, the bulk of the increase in access to disinfected water occurred within the time span of a half-year (54% covered in April 1991 to 85% covered at the end of that year). Similarly, as shown in *Figure 2b*, farmland area irrigated with wastewater declined markedly between April and December 1991. Unfortunately, state level data pre-program water chlorination capacity, land irrigated by waste water, or even the population share with access to chlorinated water is not available.

IIb. Program Impacts on Diarrheal Disease Mortality

Previous studies suggest that the 1991 intervention led to substantial reductions in infant and child morbidity and mortality from diarrheal disease (see for example, Gutierrez et al., 1996; Sepulveda et al., 2007). However, to our knowledge, no study has more formally examined the extent to which this was the case. *Figure 3* shows trends in infectious disease mortality rates for infants: we cannot plot morbidity rates as there are no annual state-level series on morbidity during this period. What is immediately obvious from these graphs is that diarrheal disease mortality, which was relatively stable between 1985 and 1990, starting to drop dramatically in 1991, leveling off in 1992. This timing is consistent with the timing of the National Clean Water program, which commenced in mid-1991 (see previous subsection). Moreover, the more gradual decline in respiratory and vaccine preventable disease mortality, which are both less sensitive to water quality interventions and were not the focus of the 1991 interventions, supports the contention that the reduction in diarrheal disease mortality resulted from the National Clean Water Program.

We test this in *Table 1* and *2*. *Table 1* regresses logged diarrheal disease mortality rates at the state*year level against birth *year*, *Post*, which is equal to 1 for birth cohorts 1991 onwards, and the interaction between the two (*Post*year)*, conditional upon state fixed effects. The coefficient on the interaction term, *Post*Year* provides us with an estimate of the trend break in the mortality series as a result of the clean water interventions, while that on *Post* picks up the level break. We see strong evidence for both a level and trend break in the diarrheal disease mortality series for 1985-1995. *Table 2* takes this one step further by incorporating a control (untreated) disease and estimating the trend break using the double difference estimator. The estimated equation is:

$$(1) \qquad ln(M_{dst}) = \beta_0 + \beta_1 {*} Treated_d {*} Post_t + \beta_2 {*} Treated_d {*} Post_t {*} Year_t + \beta_3 {*} Treated_d {*} Year_t + \beta_4 {*}\ Post_t {*} Year_t + \beta_5 {*}\ Post_t + \beta_6 {*} Treated_d + \lambda_s + e_{dst}$$

where $ln(M)$ is logged mortality from disease d in state s and year t, $Treated = 1$ if the disease is diarrhea and 0 if it is respiratory. We chose respiratory mortality as our "control" disease since it was a leading cause of infant death in Mexico and, like diarrheal disease, is closely related to public health provision and general economic development. The coefficients of interest are β_1 and β_2, which indicate the level and trend break in the treated disease relative to the control disease. This specification not only differences out changes in aspects of public health policy and medical

---

[5] Personal communication with Dr. Jaime Sepulveda.

technology that may have influenced diseases other than water-borne diseases, it also accounts for pre-existing trends in diarrheal disease mortality (through the *Treated*Year* term). We find that the level and trend breaks in diarrheal disease mortality after the implementation of the National Clean Water Program are significantly larger than that in respiratory disease mortality.

Figures 4a and *b* describes another feature of the National Clean Water Program which is that the decline in diarrheal disease mortality was larger in states with higher pre-intervention rates. *Figure 4a* is a scatter plot of the absolute change in diarrheal disease mortality against the pre-program diarrhea mortality rate by state. It shows that the decline in diarrhea was greater in states with higher initial burdens, or that the reform stimulated convergence in diarrhea disease rates across the Mexican states. *Figure 4b* is an alternative representation of convergence, using a binary measure of the pre-program rates. It shows that the post-1991 decline in diarrhea mortality rates in states in the upper half of the distribution of pre-intervention rates was larger than for states in the lower half of the distribution. Here we plot gender specific disease rates since we will analyze the outcomes by gender, and they show that both the trend break and the treatment intensity variation across states is similar for boys and girls.

III. Research Strategy

IIIa. Basic Model

To assess the impacts of reduced infant diarrhea mortality on cognition and academic achievement in late childhood and adolescence we estimate:

$$(2) \qquad Y_{ijt} = a_0 + a_1 * Post_t * BaseRate_j + \boldsymbol{\alpha} * \boldsymbol{X_{jt}} + \mu_j + \delta_t + e_{ijt}$$

where Y_{ijt} is a test score for individual i born in state j in year t, $Post = 1$ if the individual was born after April 1991, *BaseRate* is the pre-intervention gender-specific child mortality rate from diarrheal disease in the individual's state of birth, X_{ijt} is a vector of individual and household-level characteristics including household age, gender, and education, housing characteristics, quality of available schooling, and fixed effects denoting urban versus rural areas, as well as the locality of residence, and μ_j and δ_t are state and year of birth fixed effects that subsume the main effects *Post* and *BaseRate*. The equations are estimated by gender.

We adopt *BaseRate* as our exposure measure given that there are no data on access to chlorinated water at the birth state*birth year level.[6] In using this measure, we follow an existing literature that effectively assumes a monotonic and predictable mapping between morbidity and mortality; see Bozzoli et al. (2009) for instance. The morbidity data available for Mexico suggest that the sharp drop in diarrheal mortality rates in response to the clean water efforts was mirrored by a similar drop in the number of diarrheal episodes per child per year (Gutierrez, et al, 1996). We use the mortality rate for children aged 0-4 as this is measured with less error than the infant mortality rate i.e. the rate for children aged 0-1.[7]

---

[6] The upside of this lack of data is that birth state*birth year program intensity is likely to be endogenous, whereas our identification approach does not rely on the precise timing of National Clean Water Program efforts in a given area.

[7] Surveillance issues leading to measurement error especially for deaths of younger children are indicated in the Data section below. We have estimated the equations using the infant rate and the impact estimates are very similar and largely of similar significance although less well determined. The estimates using infant diarrhea are available on request.

The parameter capturing the impact of the intervention is a_1. By interacting the timing of the intervention with pre-program state-specific diarrhea mortality rates, we are able to exploit state*cohort variation in program intensity. This sort of strategy has been previously used to estimate returns to health interventions (see Acemoglu and Johnson, 2007 and Bleakley, 2007, for early examples). Equation (2) may be thought of as the reduced form of a system in which test score outcomes are allowed to depend upon infant exposure to diarrheal disease, with the latter instrumented with the sharp arrival of the National Clean Water program, the impact of which is allowed to vary by the pre-intervention burden of diarrheal disease in the birth state. The parameter a_1 captures the full impact of clean-water induced disease reductions, which includes both biological impacts on mental and physical growth in infancy as well as any subsequent investments made in response to these endowment changes.

The identifying assumption is that the potential outcomes of children are uncorrelated with the timing of the reform. Administrative information about the reform supports this and we have also confirmed that treated and untreated cohorts (born either side of 1991) are balanced on relevant covariates (available on request). When comparing treatment effects across the Mexican states, we find that the effects are larger in states in which the reduction in diarrhea was greater, and this reinforces faith in the assumption that the timing of the reform is not a source of selection that can explain our results. Also any sort of sorting or selection should influence boys and girls similarly so our findings (below) that there are significantly different treatment effects on boys and girls undermine this concern. The same holds for our finding of differences in the estimated effects by socioeconomic status of the household, which we discuss in a companion paper.

The standard errors are clustered at the birth state level to avoid the tendency to over-reject the null in difference-in-difference designs if serial correlation in the outcomes across years within a given treatment unit are not taken into account (Bertrand, et al, 2004). Mexico has 32 states which is a relatively small number of clusters and this can lead to over-rejection with the usual method for cluster-correction, so we also estimate p-values using the wild cluster-T bootstrap method of Cameron, et al. (2008). As the statistical significance of the estimated coefficients is unchanged, these estimates are reported in an appendix to be made available online.

IIIb. Threats to Inference

Estimates of the parameters of equation (2) may be biased if there are state and time-varying omitted variables correlated with both Y_{ijt} and $Post_t*BaseRate_j$. One such potential confound is the measles pandemic of 1989-1990 (indicated by the blip in vaccine preventable diseases evident in *Figure 3*). Childhood measles death rates increased to a point where the disease became the second leading cause of death in 1990 (Santos, et al, 2004). The largest jumps were seen in states with high pre-intervention mortality rates. By 1991, however, measles deaths rate declined to zero on account of an emergency vaccination campaign and/or reversion to the mean. The timing of this drop in measles has implications for inferring causal effects of the National Clean Water Program. On the one hand, not controlling for region-specific intensity of the measles pandemic could bias downwards estimates of long-run returns to the 1991 interventions if exposure to measles early in life has later-life impacts of its own. On the other hand, if the measles blip led to selective attrition (deaths of children with poorer biological endowments) then the average cognitive ability of survivors in the pre-intervention period may have been otherwise higher than it would have been absent the pandemic. In this case, estimates of the long-run effects of the clean water intervention may be upward biased. To avoid bias, we include the infant death rate from vaccine preventable diseases in 1990 interacted with *Post* as a control. We also estimated a specification that included the state time series (i.e. birth state*cohort) of the infant death rate from vaccine preventable diseases; the functional form we use has no bearing on the coefficient of interest).

Other potential confounders are other public education and health efforts. We discuss and investigate whether we may be capturing the impacts of Progresa, a major cash transfer program, in the Results section. Aside from the measles vaccination effort, there were no other public health interventions within a two year band around the National Clean Water Program. However, it is possible that prior interventions could play a role in generating biased estimates of the program effect. 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; Sepulveda et al., 2007). 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. More generally, failure to account for secular changes in the disease environment or economic development more generally could bias estimates of long-run impacts. To control for these omitted variables, we include the logarithm of state per capita GDP, the infant respiratory disease mortality rate (which will proxy living standards and health environment quality) interacted with *Post*, pre-intervention state literacy rates and average years of schooling interacted with *Post*, and birth year*birth state rainfall (shown in other studies to impact adult health and socioeconomic status; e.g. Maccini and Yang, 2009). We also assess the robustness of our results to the addition of birth region*birth year fixed effects (Mexican states are grouped into four regions) and birth state specific quadratic time trends. We restrict the birth cohorts in the estimation sample to 1988-1993. A narrow window limits the role of omitted trends and this window in particular excludes the financial crises of 1986-1988 and 1994-1996, both of which have been shown to impact infant mortality (Cutler, et al, 2002).

Two factors that may bias estimated program effects downwards are mortality selection and the eventual appearance of cholera in Mexico, which reached peak incidence in 1993 (Sepulveda, et al, 2006). Mortality selection will tend to bias program impacts towards zero since the program will have kept alive children who otherwise may have died. These marginal survivors in the post-intervention period will tend to have poorer health and cognitive endowments than other members of their birth cohort (Almond 2006; Bozzoli, Deaton and Quintana-Domeque, 2009). As regards the cholera epidemic, the fact that it was worst in states with high pre-intervention diarrheal disease rates will tend to bias downwards our estimates of program impacts on cognitive test scores. This said, cholera attack rates at the epidemic peak were around 120 per 100,000 population, well below the nationwide estimate of 2.2 episodes of diarrhea per child under the age of 5 in 1993 (Gutierrez, et al, 1996). As such, we do not expect the bias from this factor, or even mortality selection for that matter, to be sizeable.

Our estimation strategy assumes that the outcomes Y only respond to program effects in the birth year. Children born in, for instance, 1988 and three years old when the program arrives, are in the pre- or untreated group of cohorts in equation (2). We relax this restriction by replacing $Post_t*BaseRate_j$ with $BirthYear_t*BaseRate_j$, so allowing flexible coefficients. This extension not only allows for program effects for children who are first exposed post-infancy, it also acts as a falsification test on the timing of the reform.

IV. Data

The outcomes we analyse are in microdata data from three sources, the Mexican Family Life Survey (MxFLS), the Program for International Student Assessments (PISA) and the Mexican Census. In addition we use state-level time series data on mortality rates by cause of death, specific to age and gender, and a vector of state-level controls for income, rainfall and education.

The MxFLS is a nationally representative panel survey covering 8,500 households in 150 communities across 16 Mexican states (Rubalcava and Teruel, 2007). The survey covers a wide variety of domains, including household demographics, expenditures, educational attainment, health and anthropometry, labor force participation, and fertility, with two waves fielded in 2001-2002 and 2005, respectively. For our purposes, the MxFLS administered a colored Raven's progressive matrices test, a non-verbal pattern matching assessment increasing in difficulty as the individual progresses onward, for all individuals above the age of 5 (an 18 item test for those aged 5-13, and a 12 item battery for those 14 and over). The Raven test is widely used as a test of general intelligence as it is thought to be an informative indicator of an individual's ability to perceive and process information accurately (Raven, et al, 1984; Stein, et al, 2005). The MxFLS records the exact birth date and birth state, which we match to *Post* and *Baserate*. For the reasons detailed in the previous section, we focus primarily on individuals born between 1988 and 1993 who are age 9-17 at the time of taking the test, depending upon the survey wave.

The PISA data record individual level results from a series of international school-based standardized mathematics and reading achievement tests. The Mexican PISA assessments were fielded in 2000, 2003, 2006, and 2009. A representative sample of school-going 15 year olds was drawn by first randomly choosing schools and then by randomly choosing students to take the assessment tests within those schools. We utilize data from the 2003, 2006 and 2009 waves which include cohorts born in 1987/1988, 1990, and 1993, the last being the post-intervention group. For each testing domain, five different estimates of the test score are provided, which represent plausible values from a posterior probability distribution delineated by PISA. We follow the OECD recommendation on combining these scores, using the *pv* command in Stata for the regressions. Unlike in the MxFLS, in the PISA data we only know the state of testing for each respondent, not the state of birth. Since over 80% of 15 year olds in the Mexican census currently reside in their state of birth, we do not expect this to introduce a great degree of bias (Venkataramani, 2009). Again, as with the MxFLS, we match our birth state proxy and the exact birth date with *Post* and *Baserate*.

Using both datasets guards against the shortcomings of either one influencing the results. A difference between the surveys is size. The main shortcoming of the MxFLS is that it is smaller (a total of 4,100 person-wave observations) than PISA (over 99,000 children across the three survey waves). Second, the Raven test measure in the MxFLS can perhaps be viewed as a measure "pure intelligence" and is administered to a representative sample of children, regardless of whether or not they are attending school. On the other hand, the PISA scores measure both cognition and what is learned in school. It is possible that there is selectivity in survival to 9th grade (age 15) given that compulsory schooling is poorly enforced. Third, both the MxFLS and the PISA contain rich, albeit different, sets of control variables. For example, in the PISA data, we can control for school quality, whereas the MxFLS has more in the way of household socioeconomic characteristics. Both data sources contain indicators of parental investments, but different ones. The PISA data allow us to look at preschool and school quality while the MxFLS contains data on school attendance and the allocation of child time to educational and household tasks.

Regarding our exposure measure, data on diarrheal disease mortality rates 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*)[8]. Diarrheal deaths

---

[8] It is estimated that the mortality registration system captured over 90% of deaths nationwide by 1990 and attempts were made to correct for the residual underreporting, which was primarily among young children and in rural areas (see Cutler et al., [2002] for details). The mortality data can be accessed at http://sinais.salud.gob.mx/basesdedatos/ or http://sigsalud.insp.mx/naais/.

for infant girls and boys were gathered separately, with the cause identified by ICD-9 code[9]. To compute *BaseRate* for each gender, we computed the average mortality rate over the period 1988-1990 for each Mexican state. Data for respiratory and vaccine preventable diseases were similarly obtained. Data on state GDP were taken from German-Soto (2005)[10], pre-intervention literacy and schooling attainment in the state are from the 1990 Mexican Census (Ruggles, et al, 2010), and rainfall data are from the National Weather Service website (http://smn.cna.gob.mx). Means and s.d. of the dependent variables are in the Tables of results.

V. The Impact of Early Life Diarrhea on Cognitive Development and Academic Achievement

Va. Main Results

Estimates of equation (2) for Raven scores are in *Table 4* and for school-based reading and math scores are in *Table 5*. For each of the three test scores we identify significant program effects for girls. The coefficients for boys are smaller and, in general, not significant. The statistical significance of the gender difference in the coefficients is confirmed by estimating models with gender interactions (p-values are in notes to the Tables). The estimates for girls are robust to controls for household socioeconomic status and community characteristics, state-level macroeconomic characteristics and mortality from control diseases (respiratory and vaccine-preventable diseases) and birth region*birth year fixed effects. In the Raven score equations we are also able to demonstrate robustness to birth state specific quadratic time trends. The PISA math and reading scores are modeled using two pre and one post cohort, making state specific trends very demanding so for those equations we stop with region*birth year effects.

Consider effect sizes in the specifications with all of the controls (column 4). A 1 standard deviation decrease in the pre-program child diarrhea mortality rate (which is a decrease of 3.3 deaths per 1,000 children aged 0-4) is associated with a 0.1 standard deviation increase in the Raven test score for girls. The coefficient for boys is insignificant in the first three specifications but is rendered significant upon controlling for quadratic state trends. Even in this specification, the estimated impact is only about half that for girls (a 0.06 standard deviation increase in scores). Estimates for the PISA math and reading tests are strikingly similar, although these are different tests done at different ages on different samples of children and in school rather than at home. Treated girls perform significantly better on math and reading scores. The PISA coefficients do decline somewhat upon including region*year fixed effects but they remain significant for math and marginally significant for reading. A 1 standard deviation decrease in child diarrheal disease mortality rates led to 0.07-0.12 and 0.06-0.10 standard deviation increases in reading and math achievement tests respectively. The estimates for boys are an order of magnitude smaller, and, as with Raven scores, the gender difference in impact is statistically significant.

Vb. Additional Robustness Checks

We have already explored and established robustness of the estimates to a rich set of controls. We now discuss additional robustness checks.[11]

---

[9] 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.

[10] Data on the share of agriculture, mining and construction in state GDP were sent to us by Ernesto Aguayo-Tellez.

[11] A Table showing the suite of robustness checks will be added to the next draft.

Placebo intervention and flexible age of exposure: First we estimated a placebo using a fake intervention in 1989, which produces a treatment effect that is not significantly different from zero. In what is effectively a generalization of this, we investigated the restriction that it is only birth year diarrhea exposure that influences cognitive attainments. Estimates are in *Appendix Tables 1a, b.* Also, *Appendix Figure 2* plots birth cohort specific coefficient estimates using the MxFLS, which we obtain by regressing, for each birth cohort, the Raven test score measure against *BaseRate* and the full set of household and birth state specific controls. Here, as before, we have split the 1991 birth cohort so that individuals born before April are in the pre-treatment group and those born after April are in the post-treatment group. We find a strong negative relationship between Raven scores and *BaseRate* for those born before the National Clean Water Program but there is no relationship between these variables for those born after, indeed, some of the coefficients are positive. So we see Raven score improvements tightly correlated with the timing of the intervention, and these estimates validate our modeling of infancy as a critical period for diarrhea and cognitive development. This is consistent with biological evidence that infancy is (a) when the risk of diarrhea infection is particularly large and (b) diarrhea infections are likely to leave particularly large scars given that this is when brain development is most rapid and most demanding of metabolic resources (which diarrhea drains). Thus confirming that impact estimates are largest for those exposed to clean water in the birth year strengthens our causal claims.

Mechanisms: By the same token, the cohort-specific estimates speak against household income being a mechanism driving the estimated relationship. It may be argued that reduced diarrhea raises adult productivity and so household income and that it is this that leads to better cognitive attainments for children. The reason this is undermined is that we see no significant impact of the water reform on children aged 2 or 3 at exposure and we know that adults are even less affected by diarrhea. Another mechanism that may be thought to be at play given evidence of the importance of *in utero* cognitive development (e.g. Almond and Currie 2011b, Almond and Edlund 2010) is that maternal diarrhea compromises fetal development or else that fetal development is compromised by the loss of earnings of adults in the household infected with diarrhea. As both of these would plausibly reduce birth weight, we examined impacts of the water reform on retrospectively reported birth size in the MxFLS. We find no association and this lends further credence to our proposed mechanism of clean water leading to better infant health and cognitive endowments and thereby improved test score performance.

Mean reversion: Since our estimates utilize the finding of post-reform convergence across the states with reference to the pre-intervention initial level of diarrhea (*BaseRate)*, a natural concern is that if the pre- level in high-burden states was high on account of a shock then the convergence we see reflects mean reversion. To investigate this we plot the rank correlation of state diarrhea rates in 1986 and 1988 and, again, in 1985 and 1990 (*Figure 5*). The ranks look fairly stable. To further check against this concern, we re-estimated the equations varying the measure of *BaseRate* from the average over 1988-90 to the average over 1987-90, 1989-90 and just 1990. The results are not sensitive to these variations.

Compositional effects and fertility: So as to investigate balance between the pre and post treatment cohorts we investigated the relationship between a set of household characteristics and *Post*BaseRate*. The pre and post samples are balanced on parental age, education and income. They are also balanced in age of mother at birth (*Appendix Figure 1*). We find no significant heterogeneity in the composition of births as a result of the reform. Models of the quantity-quality tradeoff predict that the increase in child quality and child survival occasioned by the reform may have led to a decline in fertility. This is indeed what we find and elaborate in a companion paper but it does not bias the coefficients in this paper.

***Progresa* as an overlapping intervention:** Last but not least we investigated whether any of the impacts on cognitive attainments that we attribute to *Agua Limpia*, the water reform, may in fact arise from the introduction of *Progresa*, a major cash transfer program that sought to improve preschool child health and school participation. Means-tested cash transfers were made available to poor families with children in grades 3-6 of primary school and grades 1-3 of secondary school conditional upon their attendance of school and health clinics. As discussed, *Agua Limpia* was introduced in 1991. *Progresa* was introduced in rural municipalities from 1997 and rolled out from then on, remaining largely rural till 2003. The marginal treated cohort in our sample (born 1991) was potentially exposed to *Progresa* at age 6 or more (1997 or later), and some of the 1993 cohort were exposed at age 4. Younger children may have benefited too, on account of within-household spillovers of information or scale economies in attendance of clinics or schools. Although our estimates above showed no improvements in cognitive attainments flowing from *Agua Limpia* on children exposed at ages greater than one, it remains possible that *Progresa* inputs produced cognitive gains for older children (4 upwards). To investigate this, we obtained *Progresa* rollout at the municipality level and introduced in the MxFLS Raven score models a control for *Progresa* coverage, constructed as the fraction of households in a municipality that received Progresa from the age of 6 and upwards. We did this for the sample of rural households with uneducated heads since *Progresa* was rural and means-tested. We cannot do this for reading and math since PISA does not identify municipality. The estimates in *Appendix Table 3* show that controlling for Progresa makes *no* difference to the estimated impact on Raven scores of the Clean Water Programme. We cannot investigate an interaction between the water reform and *Progresa* because *Progresa* is only available to post-water-reform cohorts.

The direct impact of *Progresa* coverage on Raven scores is significant: a one s.d. increase in Progresa coverage produces a 0.68 s.d. increase in Raven scores, which is substantially larger than the impact of a one s.d. decline in diarrhea mortality rates following *Agua Limpia* which, for the rural and uneducated sample, is 0.16. Behrman, Sengupta and Todd (2000) establish some Progresa impacts on test scores a year after its introduction but we are unaware of other causal evidence of Progresa impacts on cognitive development.

Vc. Magnitudes in Relation to the Literature

Our estimates hover around a 0.1 s.d. gain in Raven, math and reading tests flowing from a 1 s.d. reduction in diarrhea, resulting from a publicly funded clean water and sanitation program. The actual impacts were, we have seen, larger in states with initially higher diarrhea burdens (see *Figure 4*). In this section we set these in the context of estimates of (a) educational interventions and (b) other early life shocks on cognitive test scores and achievement. The costs of alternative interventions are not always available and the units of change are not always comparable so we do not attempt to compare cost-benefit ratios but instead the broad magnitude of results from interventions that today are common in developing countries.

The returns to diarrhea reduction flowing from the water reform would appear to compare with or often exceed the returns to direct interventions in schools including merit scholarships, uniforms, and conditional cash transfers assessed in various settings[12], a finding that accords with, for instance, the findings of Miguel and Kremer (2004) and Bleakley (2007). A recent analysis of hospital care for low birth weight neonates in Chile estimates that it produces a 0.1 (language) to 0.2 (mathematics) standard deviation increase in test scores among 10-16 year olds (Bharadwaj, et al 2012). Ramadan fasting among pregnant mothers has significant impacts on fetal development, evident as a 0.05 to 0.08 standard deviation decrease in age 7 test scores for Muslim children in

---

[12] See http://aidthoughts.org/?p=1279, http://www.copenhagenconsensus.com/Default.aspx?ID=1632.

British schools (Almond and Mazumder 2011). Birth year exposure to malaria eradication in the 1950s in previously endemic areas in Mexico led to a 0.1-0.2 standard deviation increase in adult Raven scores for men (Venkataramani 2012).

As discussed in the Introduction, our results are also of relevance to the literature on gender differences in test scores, particularly in math. Our estimates suggest that the male-female gap in mathematics scores fell by 30% as a result of a 1 standard deviation decrease in diarrheal disease mortality rates faced during infancy. This is striking given that in a recent examination of the cross-country PISA data available for low- and middle- income countries, Bharadwaj, et al. (2012) were unable to explain more than 10% of this gap despite using a rich set of indicators on family background (wealth, parental occupation and education) and parental investments (such as books). The authors conjecture that stereotyping forces in society may lead to differentiated parental investments in math by gender, with girls receiving fewer resources. In the next section we show that parental investments are indeed differentiated by gender but that they work in the opposite direction, favoring girls, in math as much as reading. Moreover, the evidence suggests they are motivated by the relative returns to these investments being higher for girls.

VI. Explaining the Gender Gap

VIa. Explanations that Do Not Work

There are several potential reasons why girls may benefit more from birth year exposure to clean water than boys. The first is that girls may have benefitted more from reduced diarrheal illness than boys. We can reject this outright given *Figure 4* which shows that the reform was associated with a decline in diarrhea amongst boys that was no smaller, indeed slightly larger, than the decline amongst girls.

A second possibility is selection. Following implementation of the National Clean Water Intervention, the marginal survivor will have been relatively frail. It is well-known in the biological literature that young boys have higher early life mortality risks (Kraemer, 2000) and so selectivity will tend to be greater amongst boys. It is therefore possible that our finding of weaker (or absent) test score impacts for boys as compared with girls arises because the sample of boy survivors draws relatively heavily from the lower tail of the health distribution, where it is plausible that innate ability is also lower. This seems unlikely given that diarrhea mortality rates for boys were only slightly larger than for girls (*Table 4*). Also, boys, but not girls, exposed to the program at birth were less likely to be stunted, which is an impact at the lower end of the height (health) distribution, where selection theoretically should be strongest (Venkataramani 2009).

Selection may also operate in survival in school to age 15, the age at which the PISA tests are done. If girls are more likely to have dropped out by age 15 and if the girls who persist in school are selectively high ability girls then this, again, may be a compositional effect that accounts for our finding that program effects are larger for girls. However, using the 2010 census, we find that dropout by age 15 is small, similar across the sexes and if at all slightly larger amongst boys. Also, the Raven tests in the MxFLS were administered unconditional on schooling and they also show girls benefiting more from the program.

The differential results may reflect catch-up if girls were initially lagging boys in educational investments and test scores. This would place them on a less concave part of the production function, where marginal returns are larger. However, although boys did have an advantage in math scores, Raven scores and reading scores were very similar between the prior to the program. Schooling investments in girls may increase more rapidly than in boys in response to declines in maternal mortality (Jayachandran and Lleras-Muney 2009) but for this to explain our findings, maternal mortality in Mexico would have had to exhibit a sharp decline after 1991 and this decline will have to have been larger in states with higher pre-1991 rates of diarrheal mortality.

VIb. Parental Investments

To the extent that an increase in the infant health endowment raises the productivity of subsequent human capital investments, it will stimulate such investment. We investigate this using information on parental investments. Using different measures of investment drawn from different data sources and samples (PISA and MxFLS), we find that the water reform raises parental investments in pre-school, school and time allocated to homework as opposed to housework. These effects are consistently larger and indeed only statistically significant for girls. This lines up with the test score estimates, suggesting that reinforcing parental investments contributed to the girl advantage in test score gains. The fact that the eventual test score gains are effectively zero for boys suggests that these investments were complementary and, moreover, that they were critical in producing the longer run cognitive and educational benefits of the water reform. The rest of this section elaborates and qualifies these findings.

We looked at two further investments from the PISA files, for which we found no significant impact of the water reform for girls or boys; these are an indicator for the child attending private school and an index of resources available to the child at home including books, desk and computer (results available on request). Estimates for the set of investments for which we identify a response are in Table 5. Consider the size of the estimated effects for girls, all of which are statistically significant conditional upon fixed effects, controls and in the case of PISA models (preschool) region*year fixed effects or in the case of MxFLS outcomes (the others), state specific quadratic trends. Preschool attendance increases by 0.64 percentage points and the probability of school attendance by 0.57 percentage points. Time allocated to chores and housework falls by about 24 minutes of which 4 minutes is allocated to homework. These effects are all small but these are average effects and they are consistent with larger effects on the treated sub-population; not all children were at risk of contracting diarrhea.[13] Also, the evidence in this paper and in related studies suggests that these investments enter multiplicatively (at least in interaction with the infant health endowment and potentially in interaction with one another) in the production of cognitive attainment. Hence small changes in these investments may have large effects.

Estimates in the literature suggest causal relationships between the investments of interest and cognitive development. For instance, for preschool, see Behrman, et. al. (2004), Apps et al. (2012), for home work (in fact home teaching) see Behrman et al. (1999). In *Appendix Table 2* we report conditional and unconditional correlations of these inputs with Raven and PISA scores, in which we are restricted to looking at the inputs available in each of these files. We observe a significantly positive relationship of time spent on homework relative to chores and housework with Raven scores and, similarly, a significantly positive relationship between preschool attendance and reading and math scores at age 15.

VIc. Relative Returns and Complementary Investments

It remains to explain why parents differentiated investments by the sex of the child. We argue that responding to the infant health improvement by raising investments in the educational achievement of girls more than boys was consistent with relative returns. As indicated in the Introduction, the premise is that girls have a comparative advantage in brain and boys in brawn (Roy

---

[13] In a companion paper, we show that the average shift of children's time in favour of school attendance and homework derives from the behavior of households of low socioeconomic status, defined as those in which the head has less than secondary education.

1951, Galor and Weil 1996, Pitt, et al. 2012; also see Deolalikar 1993, Thomas and Strauss 1997 for early discussions of this).[14] Pitt et al. write down a model in which workers are bundles of brawn and skill, returns to attributes differ by occupation, and there is a continuum of occupations. Attributes are not exogenous, rather, individuals endowed with different levels of brawn optimally invest in schooling and nutrition. They then optimally sort into different occupations. Occupational sorting by comparative advantage implies that women will be disproportionately represented in skill-intensive occupations, as a result of which the average productivity of schooling is higher for women. This does not necessarily imply that their earnings are higher because boys receive greater investments in brawn.

Given a technology in which health and schooling are complements in the production of skill, an equal change in the health endowment will lead to increased investments in schooling. The Pitt et al. model predicts that these investments will be larger for girls than boys because of the higher return to skill in the occupations into which women select and the lower opportunity cost of schooling. We have already seen that this prediction of the model is born out in our data. So as to tie this in more closely with the model, we also investigated the prediction of gender-based occupational sorting and the gradients of sorting in education and the brawn-base of the local economy.

We classified occupations as brawn *vs* brain-intensive based on a recent linkage of Mexican occupational categories to job characteristics from the US Dictionary of Occupational Titles conducted by Vogl (2012). Figure 6 summarizes the classification. Using primarily census data for years in 1970-2010 (see the Data section), but cross-checking with the MxFLS data described earlier, we assign individuals of labor market age (20-50) to brain or brawn-intensive occupations. In both sources we have the individual's gender, education and state of residence so we can study gender-based occupational sorting and how it varies with education and the demand for brawn. As an indicator of the latter, we used the share of GDP that arises from agriculture, mining and construction. We first describe trends in occupation, education and the evolution of the brawn share of GDP. We the document cross-state occupational sorting, before using the individual data to estimate gender-based occupational sorting and its gradient in education and the brawn share of state GDP.

Women are persistently more likely to be in brain-intensive occupations and increasingly so. Trends for cohorts 1900-1980 show that the first gender differentiation appears around 1940 and there is a marked increase around 1960; see *Figure 7*. Women overtake men in years of education after about the mid-1960s birth cohorts (*Figure 8*).[15]

Figure 9 describes the cross-state patterns. The first confirms that the proportion of all individuals in the state who classify as being in brain-intensive occupations is decreasing in our measure of how brawn-based the state economy is. The second shows that, at the same time, the proportion of women in brain-intensive occupations is increasing in the brawn-base of the economy. This provides a compelling summary of the relevance of the Pitt et al. model.

---

[14] Pitt et al. cite evidence in support of this premise. A substantial medical literature shows that BMI translates more effectively into strength for men than for women (e.g. Round et al. 1999) and men consistently display higher grip strength than women. The similarity in the distribution of grip strength by sex between the US and Bangladesh suggests biological origins of sex differences (Mathiowetz et al. 1985, Günther et al. 2008). Think of *brawn* as the productive element of BMI.

[15] We define trends on birth cohort so it is useful to note that educational investments in, for instance, the 1960s birth cohorts are made in the 1960s and 1970s and they make occupational choices in the 1980s, which is when they arrive on the labor market. Later in this section we explain our choice of birth cohorts in the adult sample used in this section. All regression estimates are presented by birth cohort.

Using the individual data we estimate the probability that an individual is in a brain-intensive occupation as a function of their gender (*Female*), education, the brawniness of their state (*BrawnE*), their birth cohort (indicated *c* by decade and *t* by individual birth year), and interactions amongst these variables (Table 6). We display a sequence of specifications with increasingly rich controls but this is the form of the richest model:

$$(3) \qquad Brain_{ijt} = \beta_{0c} + \beta_{1c}*Female_i*Education_i*BrawnE_j + \beta_{2c}* Female_i*Education_i + \beta_{3c}* Female_i*BrawnE_j + \beta_{4c}*Education_i*BrawnE_j + \beta_{5c}*Female_i + \beta_{6c}*Education_i + \beta_{7c}*BrawnE_j + \mu_{jt} + \delta_t + e_{ijt}$$

Women are about 11% points more likely than men to be in brain-intensive occupations (column 1). An additional year of education raises the chances of a woman being in a brain-intensive occupation by about 6% points, which is significantly larger than the 4% point "occupational return" to education for men (column 2). The extent to which women's education raises their chances of sorting into brain-intensive occupations is significantly increasing in the brawniness of the labor market that they face (column 3). Overall, these estimates provide a consistent explanation of our finding that parental investments in education-enhancing inputs favour girls.

To link these findings back to the preceding analysis of cognitive attainments and parental investments, we need to estimate extensions of equation (2) that allow the impacts of the water program to vary by the brawn-base of the state economy:

$$(4) \qquad Y_{ijt} = a_0 + a_1*Post_t*BaseRate_j*BrawnE_j + a_2*Post_t*BaseRate_j + a_3*Post_t*BrawnE_j + \boldsymbol{\alpha*X_{jt}} + \mu_j + \delta_t + e_{ijt}$$

The terms $BaseRate_j*BrawnE_j$, $BaseRate_j$ and $BrawnE_j$ are all absorbed the state fixed effects μ_j and the main effect $Post_t$ is absorbed by the birth cohort fixed effects, δ_t. The vector X contains the state*year controls for pre-programme macroeconomic conditions and control diseases and the region*year fixed effects and state trends. As before Y refers to the three test scores and will be extended to include the indicators of parental investments analysed earlier. Estimates of (4) will be in the next draft of this paper.

Each of the three tendencies we describe exhibits a significant strengthening over time, across the birth cohorts of 1950 to 1980 (columns 1-3). It is noteworthy that the occupational return to education for men, in contrast to that for women, decreases slightly but significantly over time. These trends are consistent with economic development raising the return to brain relative to brawn which, in turn, raises women's relative wages (Rendall 2010, Bertocchi 2011).[16]

These estimates are obtained from census data. We ran exactly the same specifications using the MxFLS data and they are strikingly similar. As discussed in the data section, MxFLS is not representative of the country as a whole and it is small. However it has the advantages of "internal consistency", being the sample used to estimate the Raven score and time allocation results and our

---

[16] The trend can also reconcile our results with those in Venkataramani (2012) which at first glance are contrary. He finds that men who were exposed to a malaria eradication program in Mexico in the 1950s show a significant increase in cognitive performance (Raven scores) in adulthood but there are no similar gains for women. Investments in the 1950s births will have referred to a labour market populated by adults born in the 1930s or earlier at which time our data suggest occupational-based sorting had not set in (Figure 7 showing occupational trends). Consistent with this, Venkataramani finds no reform-led increase in the heights of men; further down in this section we argue that observation of investments in boys that improve their manual strength rounds off the argument. An alternative possibility is that malaria and diarrhea have different biological impacts.

finding that the patterns of occupational sorting we record hold in survey and census data with different sample frames underlines their robustness.[17]

We selected adults aged 20-50 at the time that they report their occupation because the investments of interest are made when our sample cohorts are aged between zero and about 15 and it seems likely that they are made by parents, possibly in concert with children but, in any case with reference to the parameters evident in the labour market that parents participate in. The estimates are not sensitive to small variations in this choice. We would like to analyse the completed schooling and the labour market outcomes of the children in the sample analysed in section V so as to investigate whether the expectations driving parental investments are realised but for this we need to wait a few years as they are too young at this time.

Amongst predictions of the Roy-style model of Pitt et al. is that, given complementarities across inputs, the positive infant endowment shock will have increased investments in the brawn of boys. Using the MxFLS, Venkataramani (2009) has shown that the water reform led to increases in the heights of boys at the lower end of the height distribution alongside no change for girls. Heights are measured in the teenage years and standardized by sex and age and, based upon an existing literature (Bozzoli et al. 2009, Alderman, Hoddinott and Kinsey 2006), we expect that cohort differences in height reflect nutritional investments made in the first two or three years of life. Height is correlated with strength and so this is consistent with parents directing more nutritional inputs towards boys to improve their productivity in brawn-intensive jobs. We can also directly study nutritional inputs using the Engel-Rothbarth method but the caveat with this and the height results is that nutritional inputs in early life stand to increase cognitive capacity as much as the capacity for manual work. In the model of Pitt et al., males receive more calories than females but the implicit assumption here must be that calories are more productive of brawn than of brain skills, an assumption that our main results (section V) effectively reject. A test of the converse prediction of girls receiving relatively greater investments in educational inputs does not face this problem: while schooling does causally influence health (Glied and Lleras-Muney, 2008), it seems plausible to assume that any impacts of schooling on productivity in brawn-intensive occupations are small and in any case smaller than the impacts of schooling on productivity in brain-intensive occupations.

As discussed in the Introduction, the only previous empirical tests of the hypothesis that a biologically premised comparative advantage for girls in brains influences educational investments in girls vs boys and, as a result, their occupational sorting is in Pitt et al. 2012 and Rosenzweig & Zhang 2012. Pitt et al. argue that the positive trend in girl relative to boy schooling in Bangladesh is likely to have flowed from a major initiative to reduce diarrhea introduced in Bangladesh in the early 1980s. As this is a motivating argument in their paper rather than their main analysis, it is relatively casual, resting upon observation of aggregate trends in schooling in relation to the timing of improvements in wages, the introduction of school incentive programs and microcredit. Using Mexican data and with the advantage of quasi-experimental variation generated by the water reform, we are able to verify their claim.

VII. Conclusions

A recent body of work establishes causal effects of cognitive skills on educational attainments, health and economic growth, making it important to understand how to achieve cognitive potential and, related, to narrow gaps in cognitive skills within and across countries. This

---

[17] We re-estimated the equations conditioning upon employment and, to assess selectivity, we estimated equation (3) with employment as the dependent variable. Estimates of the occupational sorting coefficients are almost identical and this is because, in the census, occupation is reported for more than 90% of men and women.

study isolates the role of infectious disease and, in particular, diarrhea, in early life in impairing cognitive attainments, showing that any biological mechanisms are reinforced by subsequent parental investments so that the eventual effects are magnified. The marginal reform-treated cohort in our sample is 19 in the 2010 census and subsequent treated birth cohorts are younger, so we cannot analyse their labour market outcomes directly but our estimates establish the foundations for linking early life health interventions to child and adolescent cognitive development and educational achievement and hence to labour market outcomes. Our analysis of gender differences in the cognitive gains from diarrhea reduction contributes to an understanding of the evolution of gender differences in test scores (relevant to a literature on girls under-performing in math relative to boys) and wages. Our findings are relevant to developing countries, where diarrheal disease is a major cause of child death and where IQ scores on international scales fall below those in richer countries.

References

Abramitsky, R. and V. Lavy. “How Responsive is Investment in Schooling to Changes in Redistributive Policies and in Returns?”. Mimeograph, University of Warwick.

Acemoglu, Daron, and Simon Johnson. "Disease and Development: The Effect of Life Expectancy on Economic Growth." *Journal of Political Economy* 115, no. 6 (2007): 925-65.

Ahuja, A, M Kremer, and A Zwane. "Providing Safe Water: Evidence from Randomized Evaluations." *Annual Review of Resource Economics* 2 (2010): 237-56.

Aizer, Anna, and Flavio Cunha. "The Production of Human Capital in Childhood: Endowments, Investments and Fertility." *Mimeo, Brown University* (2012).

Alderman, Harold & John Hoddinott & Bill Kinsey, 2006. "Long term consequences of early childhood malnutrition," Oxford Economic Papers, Oxford University Press, vol. 58(3), pages 450-474, July.

Almond, Douglas, and Bhaskhar Mazumder. "Health Capital and the Prenatal Environment: The Effect of Ramadan Observance During Pregnancy." *American Economic Journal: Applied Economics* 3, no. 4 (2011): 56-85.

Almond, Douglas and Bhashkar Mazumder forthcoming. Fetal origins and Parental Responses. *Annual Review of Economics*, Early Life Experience and Human Development Symposium.

Almond, Douglas, and Janet Currie. "Human Capital Development before Age 5." In *Handbook of Labor Economics*, edited by Orley Ashenfelter and David E Card: Elsevier, 2011.

Almond, Douglas, and Janet Currie. "Killing Me Softly: The Fetal Origins Hypothesis." *Journal of Economic Perspectives* 25 (2011): 153-72.

Apps, Patricia, Silvia Mendolia and Ian Walker. The Impact of Pre-school on Adolescents’ Outcomes: Evidence from a Recent English Cohort. IZA DP No. 6971, October 2012

Arias, Javier, Oliver Azuara, Pedro Bernal, James J Heckman, and Cajeme Villarreal. "Policies to Promote Growth and Economic Efficiency in Mexico." *NBER Working Paper No.16554* (2012).

Baird, Sarah, Joan Hamory Hicks, Michael Kremer, and Edward Miguel. "Worms and Work: Long-Run Impacts of Child Health Gains." *mimeo, University of California, Berkeley* (2011).

Barham, Tania. "Enhancing Cognitive Function: Medium-Term Effects of a Health and Family Planning Program in Matlab." *American Economic Journal: Applied Economics* 4, no. 1 (2012): 245-73.

Barreca, Alan. "The Long-Term Economic Impact of *in Utero* and Postnatal Exposure to Malaria." *Journal of Human Resources* 45, no. 4 (2010): 865-92.

Behrman, Jere R. , Yingmei Cheng and Petra E. Todd. Evaluating Preschool Programs When Length of Exposure to the Program Varies: A Nonparametric Approach. *The Review of Economics and Statistics*. Vol. 86, No. 1 (Feb., 2004) (pp. 108-132)

Behrman, Jere, Andrew D. Foster, Mark R. Rosenzweig, Prem Vashishtha. Women's Schooling, Home Teaching, and Economic Growth, *Journal of Political Economy*, Vol. 107, No. 4 (August 1999), pp. 682-714.

Behrman, Jere R & Sengupta, Piyali & Todd, Petra, 2005. "Progressing through PROGRESA: An Impact Assessment of a School Subsidy Experiment in Rural Mexico," Economic Development and Cultural Change, University of Chicago Press, vol. 54(1), pages 237-75, October.

Bertrand, M., E. Duflo, and S. Mullainathan. "How Much Should We Trust Differences-in-Differences Estimates?" *Quarterly Journal of Economics* 119, no. 1 (2004): 249-75.

Bhalotra, Sonia, and Atheendar Venkataramani. "Shadows of the Captain of the Men of Death: Early Life Health Interventions, Human Capital Investments, and Institutions." *Mimeo, University of Bristol* (2012).

Bharadwaj, Prashant, Giacomo De Giorgi, David Hansen, and Christopher Neilson. "The Gender Gap in Mathematics: Evidence from Low and Middle-Income Countries." *Mimeo, University of California, San Diego* (2012).

Bharadwaj, P, K Loken, and C Neilson. "Early Life Health Interventions and Academic Achievement." *American Economic Review* (Forthcoming).

Black, RE, SS Morris, and J Bryce. "Where and Why Are 10 Million Children Dying Every Year." *Lancet* 361, no. 9376 (2003): 2226-34.

Bleakley, Hoyt. "Disease and Development: Evidence from Hookworm Eradication in the American South." *The Quarterly Journal of Economics* 122, no. 1 (2007): 73-117.

Bleakley, Hoyt. "Health, Human Capital and Development." *Annual Review of Economics* 2 (2010): 283-310.

Bozzoli, C, A Deaton, and C Quintana-Domeque. "Adult Height and Childhood Disease." *Demography* 46 (2009): 647-69.

Cameron, AC, J Gelbach, and D Miller. "Bootstrap-Based Improvements for Inference with Clustered Errors." *Review of Economics and Statistics* 90 (2008): 414-27.

Cunha, F, and J Heckman. "The Technology of Skill Formation." *American Economic Review* 97, no. 2 (2007): 31-47.

Cunha, Flavio, James Heckman, and Susan Schennach. "Estimating the Technology of Cognitive and Noncognitive Skill Formation." *Econometrica* 78, no. 3 (2010): 883-931.

Currie, Janet and Tom Vogl. Early-Life Health And Adult Circumstance In Developing Countries. NBER Working Paper 18371, 2012

Cutler, D, and G Miller. "The Role of Public Health Improvements in Health Advances: The Twentieth-Century United States." *Demography* 42, no. 1 (2005): 1-22.

Cutler, David, Winnie Fung, Michael Kremer, Monica Singhal, and Tom Vogl. "Early-Life Malaria Exposure and Adult Outcomes: Evidence from Malaria Eradication in India." *American Economic Journal: Applied Economics* 2 (2010): 72-94.

Cutler, DM, F Knaul, R Lozano, O Mendez, and B Zurita. "Financial Crisis, Health Outcomes and Ageing: Mexico in the 1980s and 1990s." *Journal of Public Economics* 84, no. 2 (2002): 279-303.

Deolalikar, Anil B. (1993), "Gender Differences in the Returns to Schooling and in School Enrollment Rates in Indonesia," Journal of Human Resources 28(4): 899-932.

Deverman, Benjamin E, and Paul H Patterson. "Cytokines and Cns Development." *Neuron* 64 (2009): 61-78.

Eppig, Christopher, Corey L Fincher, and Randy Thornhill. "Parasite Prevalence and the Worldwide Distribution of Cognitive Ability." *Proceedings of the Royal Society B* 277, no. 1701 (2010): 3801-8.

Frenk, J, J Sepulveda, O Gomez-Dantes, and F Knaul. "Evidence-Based Health Policy: Three Generations of Reform in Mexico." *Lancet* 362, no. 9396 (2003): 1667-71.

Galiani, S, P Gertler, and E Schargrodsky. "Water for Life: The Impact of the Privatization of Water Services on Child Mortality." *Journal of Political Economy* 113, no. 1 (2005): 83-120.

Galor, Oded and David N. Weil. "The Gender Gap, Fertility, and Growth." The American Economic Review, 86, no. 3, June (1996): 374-387.

Gamper-Rabindran, S, S Khan, and C Timmins. "The Impact of Piped Water Provision on Infant Mortality in Brazil: A Quantile Panel Data Approach." *Journal of Development Economics* 92, no. 2 (2010): 188-200.

Glied and Lleras-Muney. Health Inequality, Education and Medical Innovation. Demography, Volume 45, Number 3, August 2008, pp. 741-761.

Goldin, Claudia, and Lawrence Katz. *The Race between Education and Technology*. Cambridge: The Belknap Press of Harvard University Press, 2008.

Gutierrez, R, R Tapia-Conyer, H Guiscafre, H Reyes, H Martinez, and J Kumate. "Impact of Oral Rehydration and Selected Public Health Interventions on Reduction of Mortality from Childhood Diarrhoel Diseases in Mexico." *Bulletin of the World Health Organization* 74, no. 2 (1996): 189-97.

Heckman, James J. "The Economics, Tehcnology, and Neuroscience of Human Capability Formation." *Proceedings of the National Academy of Sciences* 104, no. 33 (2007): 13250-55.

Heckman, JJ, J Stixrud, and S Urzua. "The Effect of Cognitive and Noncognitive Abilities on Labor Market Outcomes and Social Behavior." *Journal of Labor Economics* 24 (2006): 411-82.

Instituto Mexico de Technologia del Agua. "Evaluacion De Consistencia Y Resultados 2007." edited by Comision Nacional del Agua. Mexico City, Mexico, 2008.

Jayachandran, Seema, and Adriana Lleras-Muney. "Life Expectancy and Human Capital Investments: Evidence from Maternal Mortality Declines." *The Quarterly Journal of Economics* 124, no. 1 (2009): 349-97.

Jensen, Robert (2010). "The (Perceived) Returns to Education and the Demand for Schooling,"*Quarterly Journal of Economics*, 125(2), p. 515-548.

Jensen, Robert (2012). "Do Labor Market Opportunities Affect Young Women's Work and Family Decisions? Experimental Evidence from India," *Quarterly Journal of Economics***,** 127(2), p. 753-792.

Kraemer, Sebastian. "The Fragile Male." British Medical Journal 321, no. 7276 (2000): 1609-12.

Lopez AD, Mathers CD, Ezzati M, Jamison DT, Murray CJL. Global Burden of Disease. World Bank: Washington, D.C.; 2006.

Maluccio, John, John Hoddinott, Jere R. Behrman, Reynaldo Martorell, Agnes R. Quisumbing, Aryeh D. Stein. The Impact of Improving Nutrition during Early Childhood on Education among Guatemalan Adults. The Economic Journal, Vol. 119, No. 537 (Apr., 2009), pp. 734-763

Miguel, E and M Kremer. Worms: Identifying Impacts on Education and Health in the Presence of Treatment Externalities. Econometrica, 72 (1), 159-217. 2004

Pitt, Mark M, Mark Rosenzweig, and Nazmul Mohammed Hassan. "Human Capital Investment and the Gender Devision of Labor in a Brawn-Based Economy." American Economic Review (2012).

Raven, JC, JH Court, and J Raven. Manual for Raven's Progressive Matrices and Vocabulary Scales, 1983 Ed. London: HK Lewis, 1984.

Rosenzweig, Mark, and Zhang. "Economic Growth, Comparative Advantage, and Gender Differences in Schooling Outcomes: Evidence from the Birthweight Differences of Chinese Twins." Mimeo, Yale University (2012).

Rubelcava, Luis, and Graciela Teruel. "The Mexican Family Life Survey: First Wave." http://www.mxfls.uia.mx, 2007.

Ruggles, Steven, J Trent Alexander, Katie Genadek, Ronald Goeken, Matthew B Schroeder, and Matthew Sobek. "Intergrated Public Use Microdata Series: Version 5.0 [Machine-Readable Database]." Minneapolis: University of Minnesota, 2010.

Santos, JI, MA Nakamura, MV Godoy, P Kuri, CA Lucas, and RT Conyer. "Measles in Mexico, 1941-2001: Interruption of Endemic Transmission and Lessons Learned." *Journal of Infectious Diseases* 189, no. S243-S250 (2004).

Sepulveda, J, F Bustreo, R Tapia, J Rivera, R Lozano, G Olaiz, V Partida, MD Garcia, and JL Voldespino. "Improvement of Child Survival in Mexico: The Diagonal Approach." *Salud Publica De Mexico* 49, no. S110-S125 (2007).

Sepulveda, J, JL Valdespino, and L Garcia-Garcia. "Cholera in Mexico: The Paradoxial Benefits of the Last Pandemic." *International Journal of Infectious Diseases* 10, no. 1 (2006): 4-13.

Stein, Aryeh D, Jere R Behrman, Ann DiGirolamo, Ruben Grajeda, Reynaldo Martorell, Agnes Quisumbing, and Usha Ramakrishnan. "Schooling, Educational Achievement, and Cognitive Functioning among Young Guatemalan Adults." *Food and Nutritional Bulletin* 26, no. 2 (2005): S46-S54.

Thomas, Duncan and John Strauss (1997), “Health and Wages: Evidence on Men and Women in Urban Brazil,” Journal of Econometrics 77(1): 159-185

UNICEF/WHO 2009. Diarrhoea: why children are still dying and what can be done.

Venkataramani, A. "The Long-Run and Intergenerational Effects of Early Life Experiences: Evidence from Developing Countries." Yale University, 2009.

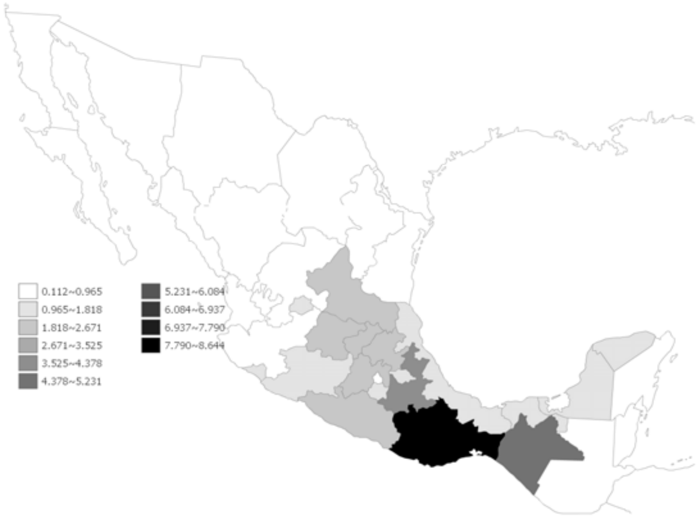
Venkataramani, AS. "Early Life Exposure to Malaria and Cognition in Adulthood: Evidence from Mexico." *Journal of Health Economics* 31 (2012): 767-80.

Vogl. T. "Height, Skills and Labor Market Outcomes In Mexico". NBER Working Paper 18318. 2012.

Watson, T. "Public Health Investments and the Infant Mortality Gap: Evidence from Federal Sanitation Interventions on Us Indian Reservations." *Journal of Public Economics* 90, no. (8-9) (2006): 1537-60.

Figures

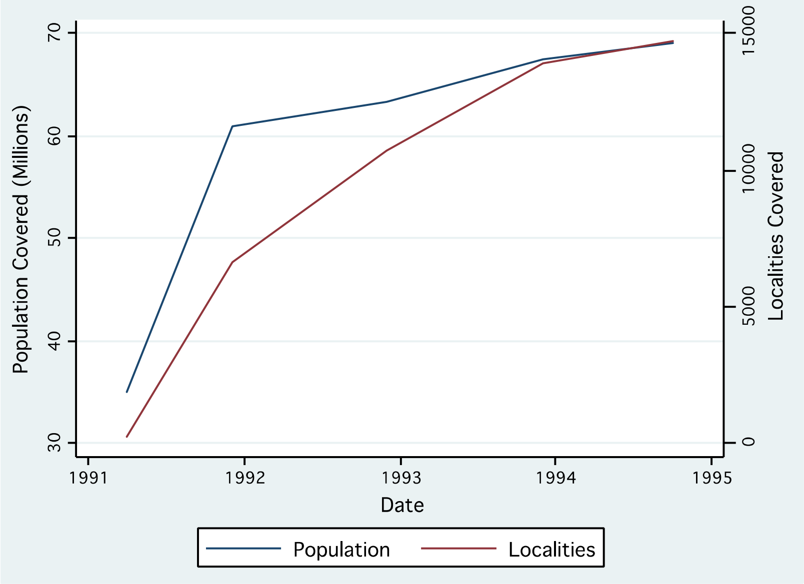
Figure 1 – Child Diarrheal Mortality Rates Across Mexican States, 1988-1990



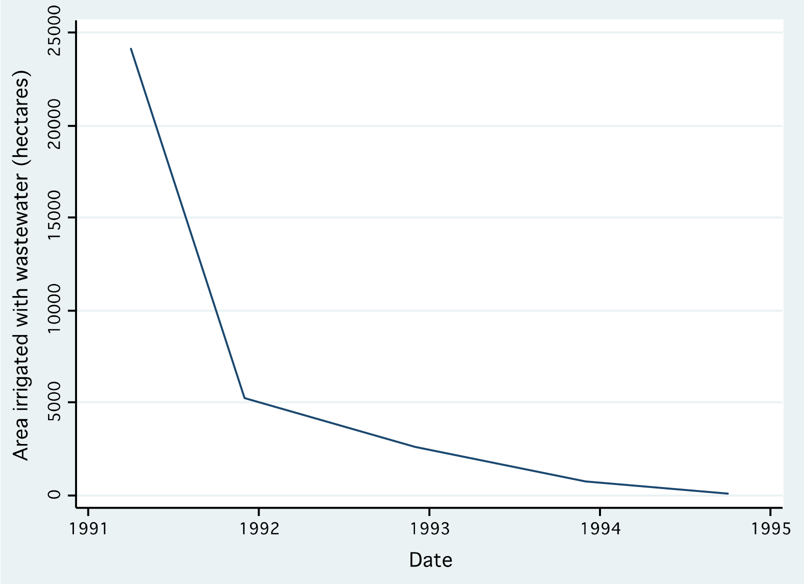
Source: Mortality data from the Mexican Secretary of Health (*Secretaría de Salud*) and population estimates for the deflator from the National Council on Population (*Consejo Nacional de Población*). Map drawn by authors.

Figure 2 – National Clean Water Program Interventions

A. Population Access to Chlorinated Water

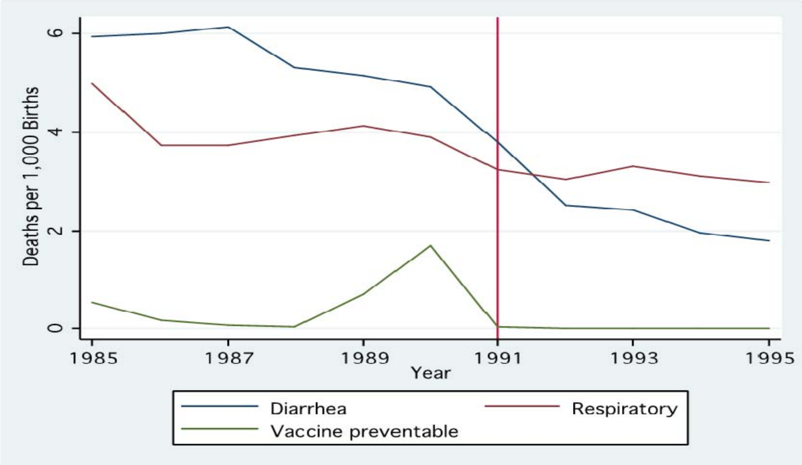


B. Land Area Irrigated with Waste Water



Data Source: CONAGUA (1994)

Figure 3 – Trends in Child Infectious Disease Mortality



Data Source: Mexican Secretary of Health

Figure 4 – Convergence across States in Child Diarrhea Mortality Rates

A. Scatterplot of Absolute Change in Diarrheal Mortality Post-1991 by Pre-Intervention Rate, State Level

Source: Mexican Secretary of Health

B. Trends by Gender and Pre-Program Diarrhea Mortality Rates

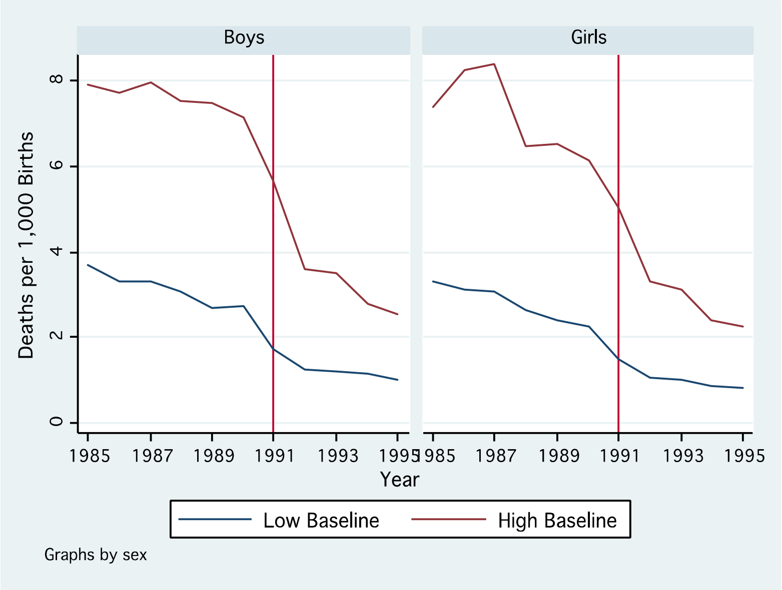
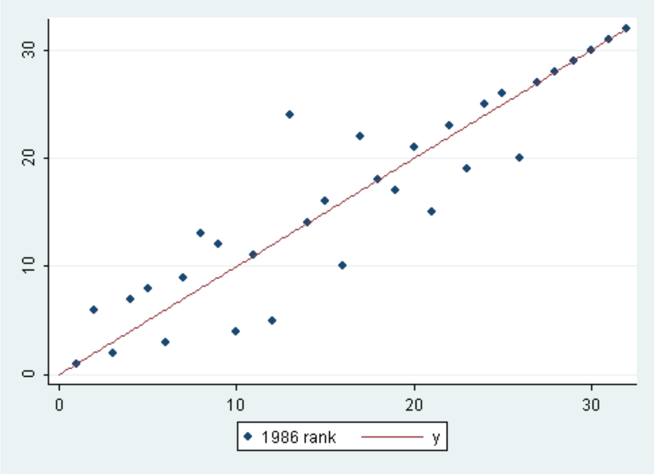


Figure 5: Rank Correlation of States by Pre-Intervention Diarrhea Mortality Rates

A. 1986 against 1988 rank



B. 1985 against 1990 rank

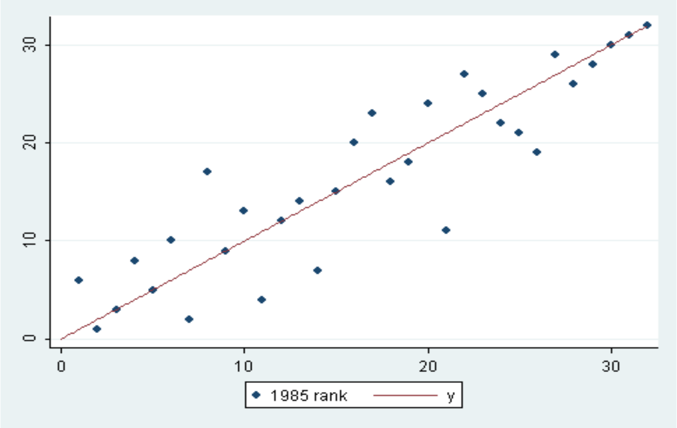
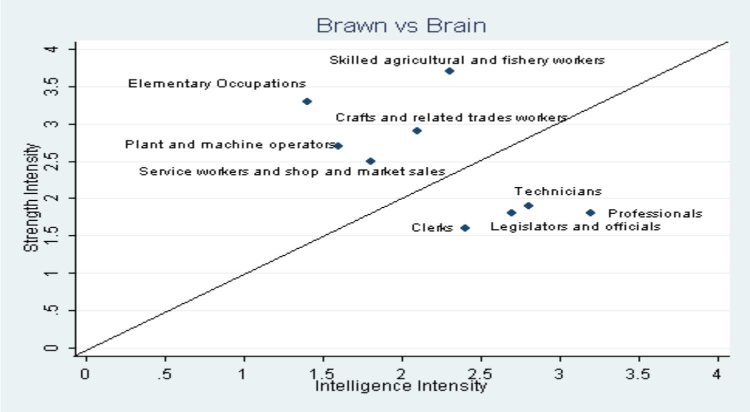


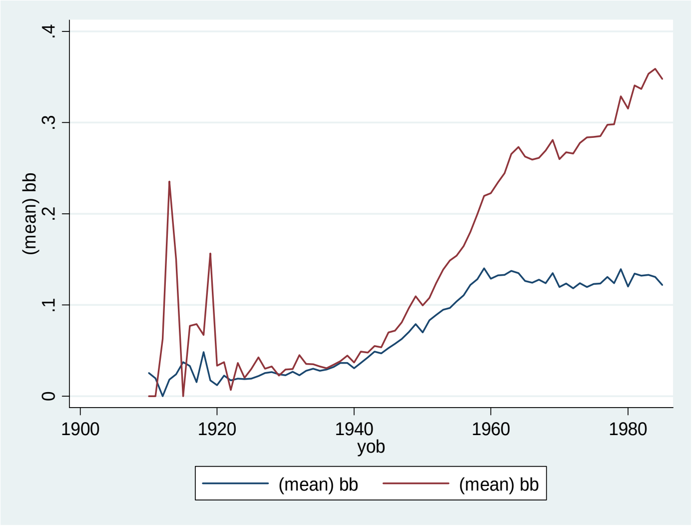
Figure 6- Classifying Occupations as Brawn V Brain



Based on new linkage of Mexican occupational categories to job characteristics from the US Dictionary of Occupational Titles (Vogl 2012)

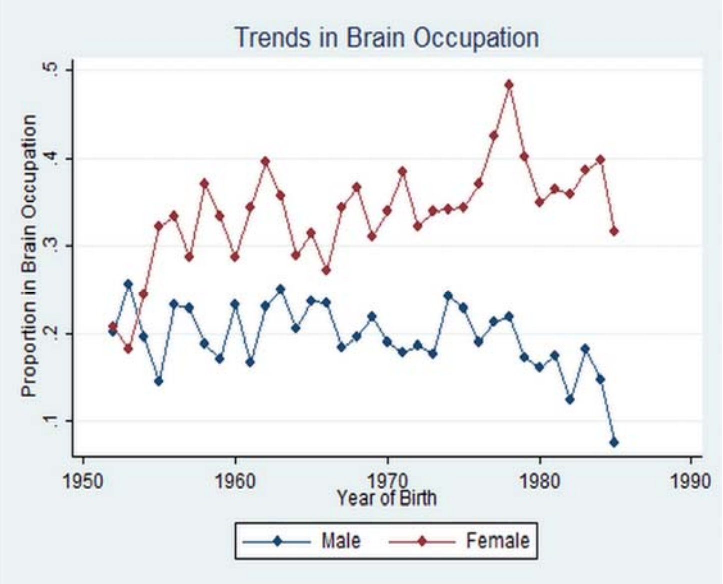
Figure 7 – Share Employed in Brainy versus Brawny Occupations

A. Long Cohort Series



Source: Census

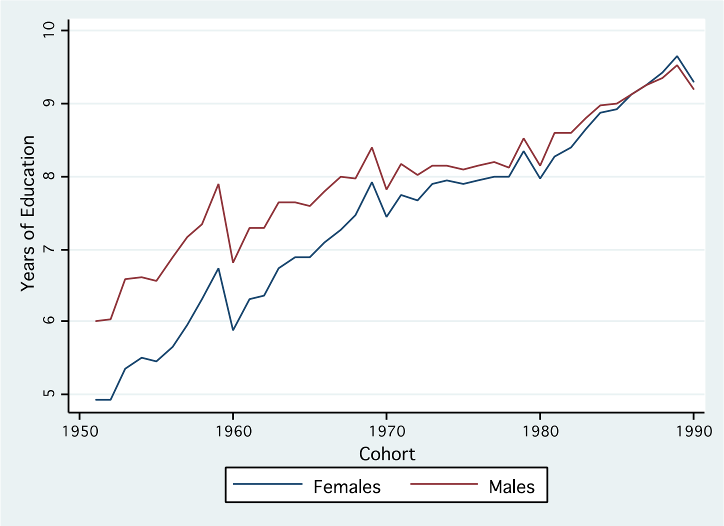
B. Shorter Cohort Series



Source: MxFLS

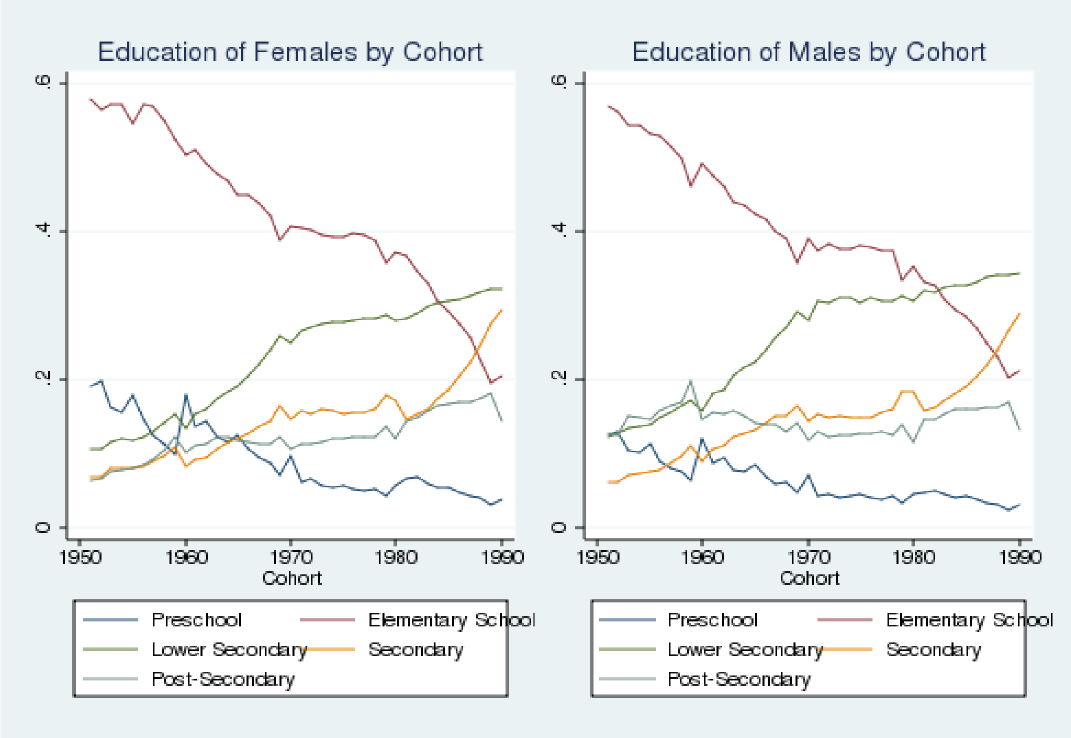
Figure 8 - Education Trends by Gender

A. Years of Education



Source: MxFLS

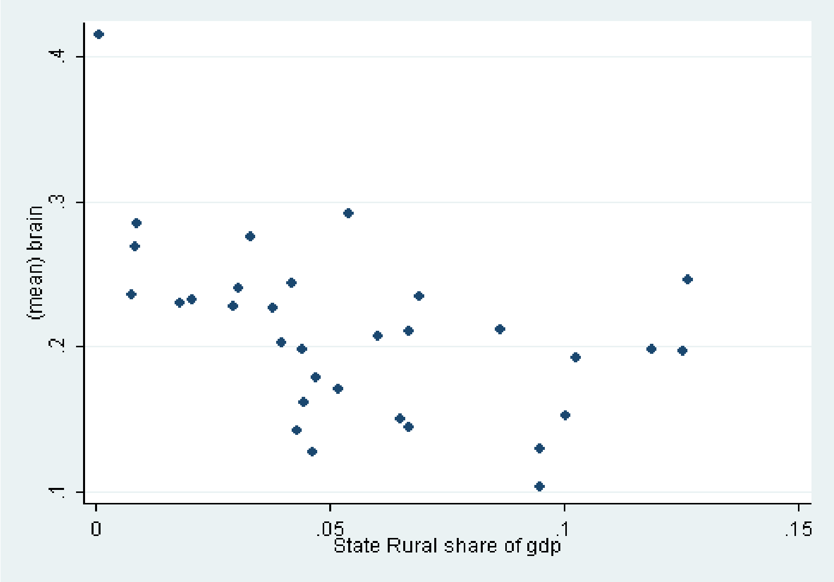
B. Education Levels



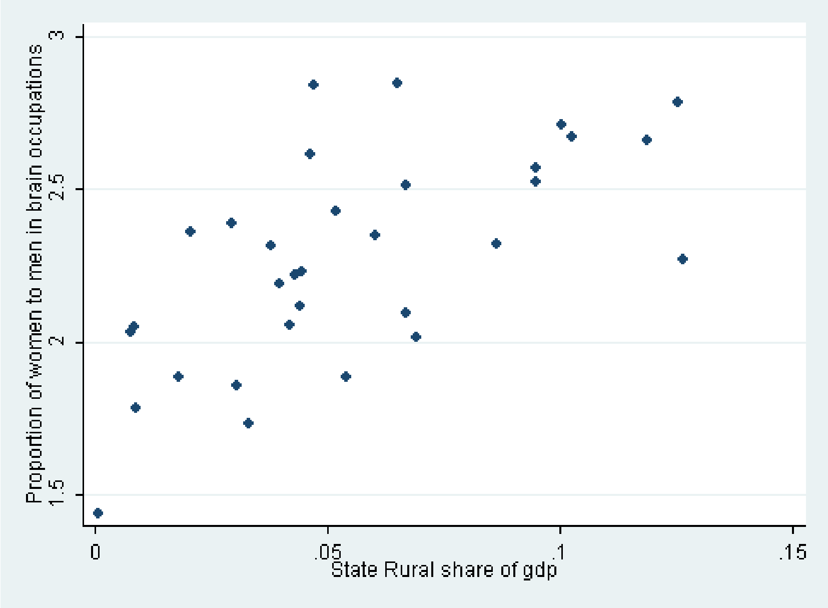
Source: Census

Figure 9 – Gender-Based Occupational Sorting by Brawn-Base of State

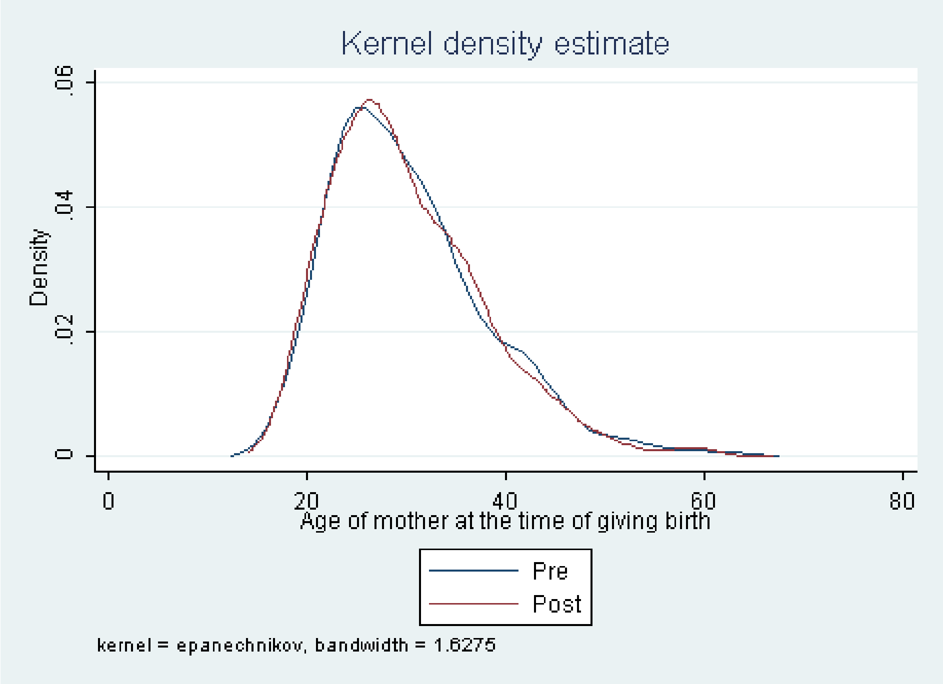
A. Ratio of Brain to Brawn Intensive Occupations in the State



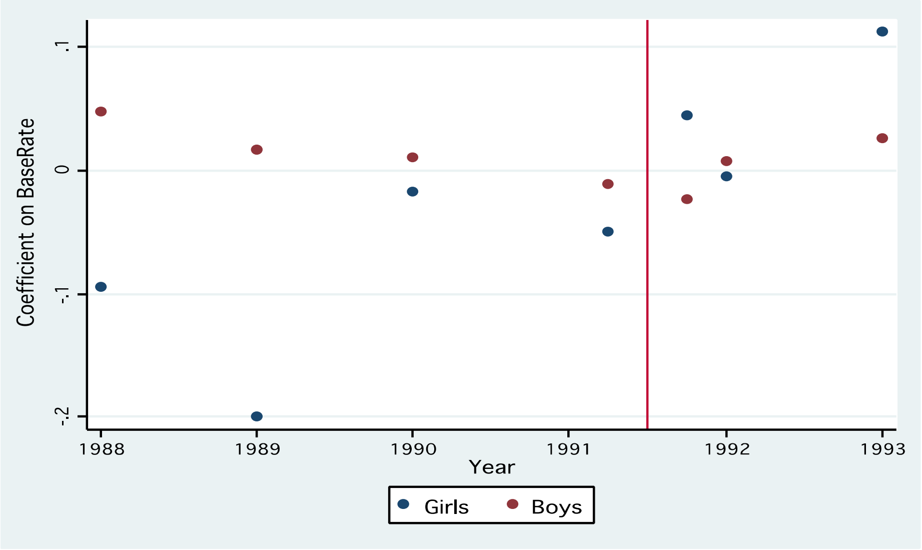
B. Share of Women in Brain Intensive Occupations in the State



Appendix Figure 1. Pre and Post Program Distribution of Age of Mother at Birth



Appendix Figure 2. – Birth Cohort Specific Impact Estimates for Raven Scores



Notes: Coefficients obtained by regressing Raven test scores on BaseRate and other household and state level controls for each MxFLS birth cohort.

Tables

Table 1 –Trend Breaks in Infant and Child Diarrhea Mortality

	(1)	(2)
	Child	Infant
Post	-0.381***	-0.365***
	(0.0417)	(0.0399)
Post*Year	-0.0994***	-0.115***
	(0.0183)	(0.0211)
Year	-0.0638***	-0.0600***
	(0.0126)	(0.0162)
N	704	704

Notes: Robust standard errors, clustered at the state level, in parenthesis. *** - p<0.01. Dependent variable is logged diarrhea mortality. Child refers to under-5 year olds net and infant those 12 months and under. -Sample includes all 32 Mexican states over the period, 1985-1995. Post = 1 if Year = 1991 or greater. All models include state and year FE. See main text for details.

Table 2 – Differences-in-Differences Model Examining Trend Breaks in Child Diarrheal Disease Mortality Rates Relative to Child Respiratory Mortality Rates

	(1)	(2)
	Child	Infant
Treated*Post	0.0226	-0.102
	(0.100)	(0.0994)
Treated*Post*Year	-0.0675***	-0.0862***
	(0.0156)	(0.0159)
Post	-0.284**	-0.363***
	(0.102)	(0.0662)
Year	-0.0165	-0.0304**
	(0.0135)	(0.0131)
Post*Year	-0.00614	-0.00944
	(0.0136)	(0.0142)
Treated*Year	-0.0731***	-0.0492**
	(0.0177)	(0.0180)

Notes: Robust standard errors, clustered at the state level, in parenthesis. *** - p<0.01. Dependent variable is logged mortality. Treated = 1 if the disease in question is diarrhea and 0 if respiratory diseases. This model is a differences-in-differences setup that examines the differential trend break in infant and child death rates from diarrheal verus respiratory diseases. See main text (model 1) for details. All models include FE for state, year, and disease type.

Table 3 –Raven Test Scores

	Girls				**Boys**			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Post*BaseRate	**0.00582*** **	**0.00492*** **	**0.00449** **	**0.00629*** **	**-0.00206**	**-0.000526**	**3.64e-06**	**0.00364*** **
	(0.00123)	(0.00114)	(0.00195)	(0.00203)	(0.00208)	(0.00151)	(0.00185)	(0.00124)
N	4,285	4,270	4,270	4,270	4,120	4,105	4,105	4,105
Pre Intervention Raven Mean	0.617	0.618	0.618	0.618	0.627	0.627	0.627	0.627
	(0.222)	(0.222)	(0.222)	(0.222)	0.225	0.224	0.224	0.224
Controls								
Birth State FE, Birth Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Characteristics	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Birth Region X Birth Year FE	No	No	Yes	Yes	No	No	Yes	Yes
Birth State Specific Quadratic Trends	No	No	No	Yes	No	No	No	Yes

Data: MxFLS 2002 and 2005 Waves. Dependent variable is fraction of questions answered correctly on Raven PM Test Sample comprises of all individuals born between 1988 and 1993 (inclusive). Post = 1 if the child was born after April 1991. BaseRate is child diarrheal mortality rate in the birth state between 1988 and 1990.Robust standard errors clustered at birth state level in parentheses. *** p<0.01, ** - p<0.05, * - p<0.10 Household Controls include household level of education, and gender, as well as housing characteristics (type of wall, floor), logged consumption expenditures, and the size of the community the child lives in. State Characteristics include Post interacted with pre-intervention birth state child mortality from respiratory disease, mortality from vaccine preventable diseases, logged GPD per capita, literacy and average grade attainment.. The p-value of the difference between the coefficient on Post*BaseRate for girls and boys in Columns (3)-(7) and (4)-(8) is <0.01. This was computed in a pooled gender model where all variables were interacted with a dummy for female (full results available upon request).

Table 4 – Reading and Math Scores

	Girls			Boys		
	(1)	(2)	(3)	(4)	(5)	(6)
Reading Scores						
Post*BaseRate	**2.012****	**2.656*****	**1.591**	**0.15**	**0.876**	**0.0405**
	(1.025)	(0.862)	(1.002)	(1.107)	(1.022)	(1.094)
N	46211	46211	46211	46211	46211	46211
Pre Intervention Raven Mean	434.88	434.88	434.88	412.49	412.49	412.49
	76.64	76.64	76.64	81.43	81.43	81.43
Mathematics Scores						
Post*BaseRate	**2.055****	**3.022*****	**1.840****	**0.692**	**1.518***	**0.364**
	(0.847)	(0.774)	(0.826)	(0.98)	(0.901)	(0.954)
N	52991	52991	52991	52991	52991	52991
Pre Intervention Raven Mean	406.32	406.32	406.32	421.24	421.24	421.24
	70.87	70.87	70.87	75.97	75.97	75.97
Controls						
Birth State FE, Birth Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Household Controls	Yes	Yes	Yes	Yes	Yes	Yes
State Characteristics	No	Yes	Yes	No	Yes	Yes
Birth Region X Birth Year FE	No	No	Yes	No	No	Yes

Notes: Data from PISA 2003, 2006, 2009 Surveys. Dependent variable is the internationally normalized score on the PISA reading or mathematics test. The sample consists of school going individuals aged 15 at the time of the test. Robust standard errors clustered at birth state level in parentheses. *** - p<0.01, ** - p<0.05, * - p<0.10. Post = 1 for individuals born on or after 1991. This includes a fraction of the 2006 PISA cohort and the entirity of the 2009 cohort. BaseRate is as defined in Table 3. In terms of controls, the variables are similar to those in Table 3, except the household controls are now comprised of indicators for maternal and paternal schooling attainment, occupational qualification, school size, measures of available educational materials, whether or not the attended school is public or private, and whether the school is situated in an urban or rural area. Differences between coefficients on Post*BaseRate are statistically significant across gender, once again calculated using a pooled model where gender was interacted with all of the regressors (p<0.05 on the interaction between Post*BaseRate*Gender)

Table 5. Parental Investments as a Function of the Clean Water Program

	Attend Preschool	**Attend School**	**Time Homework**	**Time Housework**	**Time Chores**
Girls					
Post*BaseRate	**0.00635*** **	**0.00571** **	**0.0695**	**-0.142** **	**-0.261*** **
	(0.00162)	(0.00264)	(0.0782)	(0.0646)	(0.0771)
N	29,135	4,307	3,263	3,456	3,446
Pre-Intervention Mean	0.915	0.811	7.344	7.074	7.553
	(0.280)	(0.391)	(5.942)	(6.361)	(6.867)
Boys					
Post*BaseRate	**0.00393**	**-0.00102**	**-0.0671**	**0.0419***	**-0.0882** **
	(0.00344)	-0.00315	(0.0615)	(0.0238)	(0.0404)
N	25,761	4171	3,193	3,399	3,393
Pre-Intervention Mean	0.895	0.822	6.623	2.816	3.932
	(0.307)	(0.383)	(5.662)	4.629	(5.787)

Notes: Robust standard errors, correcting for clustering at the birth state level, in parenthesis. *** - p<0.01, ** - p<0.05
Preschool is 1 if the child reported attending preschool (PISA), controls are as in column 3 of Table 4.
We also find significant effects using a categorical indicator of duration of preschool.
School attendance, breastfeeding duration and time allocation are from MxFLS, controls from column (4) of Table 3.
Time allocation is hours per week and chores refers to washing, cleaning, gathering water, taking care of household members.

Table 6 – Gender -Based Occupational Sorting

	(1) female, education				(2) female*education				(3) female*education*brawny state			
	(1a)	(1b)	(1c)	(1d)	(2a)	(2b)	(2c)	(2d)	(3a)	(3b)	(3c)	(3d)
Birth cohorts:	1950s	1960s	1970s	1980s	1950s	1960s	1970s	1980s	1950s	1960s	1970s	1980s
education*BrawnE*female									0.0141***	0.0159***	0.0213***	0.0258***
									(0.00237)	(0.00275)	(0.00352)	(0.00290)
female*education					0.0150***	0.0182***	0.0232***	0.0276***	0.00267	0.00866	0.00767	0.00672
					(0.000691)	(0.000917)	(0.00114)	(0.000935)	(0.00771)	(0.00927)	(0.0119)	(0.00950)
education*BrawnE									-0.0276*	-0.0337*	-0.0421**	-0.0441***
									(0.0153)	(0.0185)	(0.0197)	(0.0152)
female*BrawnE									-0.0186	-0.00760	0.0620	0.126
									(0.0835)	(0.0928)	(0.100)	(0.0990)
female	0.0926***	0.114***	0.119***	0.128***	-0.0195***	-0.0425***	-0.0920***	-0.146***	-0.0119	-0.0380	-0.106***	-0.178***
	(0.00636)	(0.00411)	(0.00339)	(0.00390)	(0.00608)	(0.00763)	(0.00941)	(0.0110)	(0.0252)	(0.0274)	(0.0290)	(0.0277)
education	0.0508***	0.0488***	0.0487***	0.0444***	0.0462***	0.0425***	0.0405***	0.0349***	0.0529***	0.0511***	0.0513***	0.0469***
	(0.00170)	(0.00188)	(0.00224)	(0.00230)	(0.00172)	(0.00202)	(0.00236)	(0.00225)	(0.00454)	(0.00559)	(0.00613)	(0.00529)
observations	615,782	1,561,546	1,924,440	989,706	615,782	1,561,546	1,924,440	989,706	615,782	1,561,546	1,924,440	989,706

Dependent variable: 1 if individual is in a brain-intensive occupation. Census microdata for 1970, 2000 and 2010, sample cohorts 1951-1990 (age 20-50 at time of a survey). Education defined as years of education, BrawnE is mean share of agriculture, mining and construction of GDP in state in the years 1970 and 1980. Every equation includes state, birth cohort and census year fixed effects.

Appendix Tables

Appendix Table 1b. Flexible Coefficients: Reading and Math Scores

	MATHS		READING	
	GIRLS	BOYS	GIRLS	BOYS
byr1988*diarq04	-3.471	-1.972	-0.0438	-3.096
	(2.755)	(2.637)	(2.721)	(3.072)
byr1990*diarq04	1.698	1.283	2.162	0.555
	(1.24)	(1.48)	(1.437)	(1.841)
byr1993*diarq04	**2.805****	**0.954**	**3.063***	**0.138**
	(1.349)	(1.485)	(1.584)	(1.544)
N=99202				

Pisa 2003, 2006 and 2009. Scores standardised such that (weighted) mean of OECD countries is 500 and standard deviation 100. Controls as in the final column of Table 2.

Appendix Table 1b. Flexible Coefficients: Raven Scores

Raven Flexible coefficients model

	Cohort	1988	1989	1990	1991 pre Apr	1991 post Apr	1992	1993	Obs
Boys	Cohort*Base diarq04	0.00488	0.00746**	0.00712*	0.0112	**0.00798**	**0.00107**	**0.00340**	4,105
		(0.00289)	(0.00357)	(0.00392)	(0.0210)	(0.0207)	(0.00148)	(0.00442)	
Girls	Cohort*Base diarq04	-0.00939*	-0.000342	-0.00376	-0.0215*	**-0.0195***	**0.00436**	**0.00120**	4,270
		(0.00486)	(0.00366)	(0.00313)	(0.0106)	(0.0107)	(0.00514)	(0.00583)	

MFxLS 2002 and 2005. Range for Raven is [0,1]. FE refers to births state and birth year fixed effects. Controls are for household (female head, indicators of wealth) and state characteristics (log income, log rainfall, literacy and schooling rates).

Appendix Table 2a. Raven Test Scores correlated with MxFLS Time Allocation

GIRLS

Indp. Variable	Attends School	Time on homework	Time on chores	Time on
Coefficient	**0.104*** **	**0.00319*** **	**-0.00195***	**-0.0**
	(0.0187)	(0.000578)	(0.000981)	(0.0
Observations	4,275	3,238	3,413	3,4
Pre Y mean	**0.618**	**0.639**	**0.625**	**0.6**
Pre Y Sd	0.222	0.214	0.221	0.2
BOYS				
Coefficient	**0.104*** **	**0.00256*** **	**-0.00227*** **	**0.001**
	(0.0151)	(0.000651)	(0.000766)	(0.00
Observations	4,112	3,155	3,337	3,3
Pre Y mean	**0.627**	**0.642**	**0.630**	**0.6**
Pre Y Sd	0.224	0.215	0.221	0.2

Like previous table, but now one input at a time since the inputs are correlated. These are the raw correlations of Raven scores and inputs.

Appendix Table 2b. PISA Test Scores correlated with PISA Parental Investment Variables

	Girls		**Boys**	
	Maths	Reading	Maths	Reading
Attended Preschool	19.63***	21.50***	31.62***	24.34***
	(4.716)	(4.186)	(5.216)	(6.089)
Private School	-2.462	-1.362	0.133	-0.820
	(1.810)	(2.177)	(2.257)	(1.882)
Ability-streaming school	28.47***	31.68***	36.21***	35.23***
	(3.273)	(3.271)	(3.660)	(2.558)
Parental Inv. Index	8.747***	9.779***	9.363***	8.718***
	(0.470)	(0.538)	(0.606)	(0.571)

Notes: PISA data. Test scores regressed on available parental investments conditional on birth year and state fixed effects.

Appendix Table 3. Controlling for Progresa Coverage

	Girls					
Sample	ALL	ALL	ALL	Low SES	Low SES	Low SES
	FE	Reg*Yr	State trends	FE	Reg*Yr	State trends
Progresa intensity	**-0.0145**	**0.00361**	**-0.00561**	**0.439***	**0.463****	**0.612****
	-0.0664	-0.0646	-0.0723	-0.213	-0.168	-0.255
Post*Diarq04	0.00624***	0.00722**	0.00488**	0.0213***	0.0278***	0.0123*
	-0.00206	-0.0028	-0.00187	-0.00652	-0.0051	-0.00662
Base Diarq04	-0.013	0.00743*	-9.175	-0.000176	0.00535	-2.459
	-0.00862	-0.0035	-11.53	-0.00993	-0.00791	-5.02
Observations	2,306	2,306	2,306	416	416	416
	Boys					
Progresa intensity				-0.636**	-0.763***	-0.697**
				-0.268	-0.251	-0.327
Post*Diarq04				0.0140**	0.0133**	0.024
				-0.00563	-0.00472	-0.02
Base Diarq04				0.0226**	0.0136	10.56
				-0.0091	-0.00814	-11.72
Observations				418	418	418

MxFLS 2002 data. Progresa was largely rural until 2001. Low SES is defined as rural and uneducated. Progresa intensity has mean 0.31 (s.d. 0.24). The s.d. of base_diarrhea is 2.97. Progresa intensity is the fraction of households in a municipality that receive Progresa from the age of 6 and upwards. Controls include birth municipality and birth year fixed effects, parental education, indicators for household wealth, macro and control