# 1. Introduction

Disease early in life has lasting consequences for human capital (Almond and Currie, 2011). Evidence is accumulating that poor health and inadequate nutrition in early life cause persistent deficits in cognitive development and ability (*e.g.* Case and Paxson 2010; Barham 2012). Cunha, Heckman, and Schennach (2010) advocate investing in very young children's cognitive skills, based on evidence of temporarily high returns on such investment at early ages. Although much of this literature has studied developed countries, early life missed opportunities for cognitive development may be especially important in poor countries, where disease and malnutrition are widespread.

One of the largest sources of early-life disease worldwide is unsafe disposal of human feces. 600 million people in India – 55 percent of Indian households – defecate openly, without a toilet or latrine (UNICEF and WHO 2012). This open defecation is an important cause of infant and child disease and mortality. Black, *et al*. (2003) estimate that 10 million children under 5 die every year – 2.4 million of them in India – and that a fifth to a quarter of these deaths are due to diarrhea.

Since 2001, the Indian government has been promoting the construction and use of low cost pit latrines in rural areas through a large program called the Total Sanitation Campaign (TSC). Spears (2012a) estimates that the TSC has caused a reduction in infant mortality of about 4 infant deaths per 1,000 live births, on average, and an increase in children's height for age of about 0.2 standard deviations, on average.[1] Spears (2012b) demonstrates that taller Indian children perform better on tests of cognitive achievement, on average, and that extremely poor

[1] To our knowledge, Spears (2012a) contributes the first well-identified causal estimates of the effect of rural sanitation on health.

sanitation coverage in rural India may explain why this association is so much steeper for Indian children than in other contexts studied in the literature.

Because Spears (2012a) finds an effect of the TSC on height, and because of evidence that factors that influence attained height also influence cognitive achievement (Strauss and Thomas 1998), an important next question is whether the TSC has improved children's cognitive abilities. The contribution of this paper is to estimate an effect of the TSC of Indian children's cognitive skills by matching survey data on six year olds' academic achievement with administrative data on TSC program intensity early in their life, focusing on the first three years of the TSC, 2001 to 2003. We find that, on average, during the period studied the TSC caused Indian children to be about three-tenths of a percentage point more likely to recognize letters and to recognize simple numbers.

This finding is important for three reasons. First, open defecation and poor sanitation are leading threats to global health, especially in South Asia. This paper adds evidence of a loss of cognitive ability and human capital to the health consequences of unsafe excreta disposal that are already documented in the literature. To our knowledge, this is the first paper demonstrating an econometrically well-identified cognitive cost of poor sanitation. Second, because childhood cognitive skills predict adult cognitive skills, these results imply a large, detrimental effect of widespread open defecation in developing countries on adult labor productivity (Hanushek and Woessmann, 2008).

Third, and more optimistically, unlike some other program evaluations in the literature (*cf.* Ravallion, 2012), in studying India's Total Sanitation Campaign this paper is studying large-scale implementation of a program by the Indian government. This means that the estimated average effects incorporate whatever heterogeneity, administrative costs, and losses to corruption that may exist in real implementation. Nevertheless, a positive average effect of sanitation on children's cognitive achievement is found. This suggests that low-cost rural sanitation strategies that are feasible even to limited capacity governments can importantly support children's cognitive development and improve human capital in a developing economy.

## 1.1. Early life health and cognitive skills

Grantham-McGregor, *et al* (2007) estimate that, in developing countries, "over 200 million children under 5 years are not fulfilling their developmental potential" due to malnutrition and poor health associated with poverty. This paper joins a growing literature documenting effects of early life health, disease, and nutrition on later life cognitive abilities. Like this one, many of these papers use an econometric identification strategy based on differences-in-differences, or more complicated fixed effects implementations of similar parallel trends assumptions.

For example, Barham (2012) estimates the effect on cognitive outcomes of a maternal and child health, family planning, and vaccination program in Bangladesh. Using differential timing in the implementation of the program, she finds that early life exposure to the program caused a 0.39 standard deviation in a measure of cognitive functioning when children were 8 to 14 years old. Vogl (2011), studying the Mexican labor market, finds that childhood conditions explain adult height and cognitive achievement. In turn, taller Mexican adults earn more by sorting into jobs requiring more cognitive skill. In particular, Vogl notes that children whose households had toilets or latrines at age 12 grew into taller adults than those whose did not, with more education and higher cognitive test scores (although this emphasis on a household's *own* toilet does not consider sanitation externalities, nor is causal identification of an effect of sanitation a focus of Vogl's exercise).

Perhaps the most complementary recent paper to this one is Millett and Shah's (2012) analysis of the cognitive effects of exposure to drought *in utero*. They take their dependent variable from the same ASER data on Indian children that this paper uses. Using a similar fixed effects strategy, they find a large effect: for example, they find that exposure to drought *in utero* is associated with being 2 percentage points less likely to recognize numbers in childhood.

Medical literature suggests a large effect of early life exposure to open defecation and contamination by fecal pathogens on cognitive development. For example, Humphrey (2009) details the mechanism of the profound effects of "chronic tropical enteropathy," a disorder of the small intestine above and beyond diarrhea, due to "faecal bacteria ingested in large quantities by young children living in conditions of poor sanitation and hygiene" (1032). It would be difficult to overstate the potential for such enteropathy in India, where even in the 2011 census most households reported open defecation rather than use of a toilet or latrine.

## 1.2. India's Total Sanitation Campaign

The Total Sanitation Campaign, a "flagship" program of the central Indian government, has been a large effort to improve rural sanitation. Over the first ten years of the program, it reports building one household pit latrine per 10 rural people in India and spent U.S.$1.5 billion. Pit latrines, which cost around U.S.$30-$50 to build, are an inexpensive and effective method to safely dispose of human excreta, if they are used.[2] Because this paper's identification strategy

[2] Black and Fawcett (2008) argue that earlier sanitation programs in India were unsuccessful because they focused on latrine construction, rather than promoting use. Government-built latrines in rural India are sometime repurposed for storage, as a temple, or simply taken apart for housing material.

requires comparing early life exposure to achievement several years later, we only study effects of the first three years of TSC implementation: 2001, 2002, and 2003. Spears (2012a) explains that local variation in village politics and government capacity, haphazardly delineated local boundaries, unpredictable shuffling of district-level bureaucrats, and other factors all have led to the TSC being implemented at different places and intensities in different times and places. This paper will study heterogeneity in TSC intensity at the district-year level, a large enough unit to account for the externalities of open defecation.[3]

Spears (2012a) discusses in detail why the Total Sanitation Campaign may have succeeded at reducing open defecation where earlier programs have failed. Unlike sanitation strategies focusing on latrine construction, the TSC encouraged local governments to focus on the ultimately desired outcome: latrine use. Additionally, this strategy recognizes that open defecation carries negative externalities, so it may be insufficient to build latrines for some households while ignoring their neighbors. The program makes use of the social norms and hierarchy existing in the village: local leaders have traditional authority and can motivate the community with social messages and "shaming." Pattanayak, *et al.*, (2009), in an experiment in Orissa designed explicitly to test this component of the TSC, find such social motivation effective at promoting sanitation adoption.

## 2. Empirical strategy

Are children who live in districts in which more TSC latrines had been constructed by their first year of life more likely to recognize letters and numbers when they are six years old,

[3] Although not directly relevant to this paper's analysis of household latrine use, one component of the TSC has been building latrines and sanitary complexes at village schools. Adukia (2011) estimates the effect of TSC school latrines on *contemporary* schooling outcomes.

compared with children born in the same district in different years, or in different districts in the same year? To answer this question, we matched an annual, district-level time series of TSC latrine construction to individual-level data on children's cognitive achievement. This strategy essentially extends the methods used by Spears (2012a) to new dependent variables in a new data set.

Figure 1 depicts the paper's identification strategy graphically. In order to investigate an effect of early-life sanitation coverage we must match cognitive achievement data from childhood (2007, 2008, and 2009) to data from early life (2001, 2002, and 2003). We first demonstrate evidence of an effect using district and year fixed effects, and then apply two falsification tests to verify that the results are consistent with a causal effect.

## 2.1. Data

### 2.1.1. Independent variable: Total Sanitation Campaign administrative data

Table 1 presents summary statistics from the two data sources used in this paper. As its key independent variable, this paper uses administrative records on the implementation of the TSC collected at the district level by the government of India. In the period under study, India had about 600 districts, although some had no rural population.

An annual-frequency, district-level time series of TSC household latrine construction, collected for administrative purposes, is publicly available on the program's website at http://tsc.gov.in/. These figures, in a sense, both under-report and over-report the true intensity of the TSC. The figures are under-estimates because the TSC does more than simply build latrines: in particular, it uses economic incentives and social forces to motivate people to use them. The figures are over-estimates because not every bureaucratically reported latrine was actually constructed and used.

There are several reasons why measurement error in this administrative data may not be an important concern for this paper. First, the average marginal effect of a reported TSC latrine may, in fact, be the quantity of policy relevance: this is what the Indian government is able to produce. Second, corrupt misreporting is likely to, if anything, bias our results *away from* finding an effect of the TSC: it is unlikely that the district-years where officials inflate their productivity records are those where extra investments were made in children's health and education. Third, Spears (2012a) finds similar effects of the TSC on children's health using additional measures of TSC intensity (which are not suitable for this paper's investigation) that do not make any use of these administrative records. Finally, as Spears (2012a) demonstrates in a proof, even if productivity inflation is such that the marginal coefficient estimate is incorrect, the average effect of the overall program may be correct because the inflation errors will cancel.

The years in which the studied children were born – 2001 to 2003 – were the earliest years in which TSC latrines were built.[4] By 2003, the program had built about one latrine per 20 households, on average, in the districts where it was active. As the two last columns of table 1 show, over half of Indian districts had not received any TSC latrines at all by the end of 2003. The distribution of TSC intensity was not, of course, randomly assigned, and therefore it is unsurprising that there are differences between the districts that received TSC latrines in this early period and those that did not. However, there is no monotonic difference: on average the districts that received TSC latrines appear richer (with, for example, better houses), but they also

[4] Indeed, this is why this paper's investigation is limited to six-year-olds, and cannot look for an effect of the TSC at older ages, except as a falsification test.

show lower overall educational performance. These differences indicate that the use of district fixed effects – controlling for baseline differences across districts – will be a necessary part of the identification strategy.

### 2.1.2. Dependent variables: Pratham's Annual Status of Education Report

Since 2005, Pratham – a large Indian non-governmental organization – has coordinated the ASER survey. The ASER is a household survey that is implemented annually by local organizations in every rural district of India and is based on a nationally standardized set of educational achievement tests for children.[5] This data collection effort visits about 15,000 Indian villages each year. In addition to cognitive tests, the survey includes a very small set of household and child survey questions and a few questions about the child's village. More information about this data, including survey forms and summary reports, is available online at Pratham's website: http://www.pratham.org/M-20-3-ASER.aspx.

The dependent variables of this paper are children's outcomes on the educational tests in the ASER surveys. In particular, there is a reading test and a math test. From these tests, we construct three dependent variables that distinguish ability levels among Indian six year olds: ability to recognize letters, ability to recognize single-digit numbers, and ability to recognize two-digit numbers. As table 1 shows, about three-fourths of the sample can recognize letters and the simple numbers, and about one-third of the sample can recognize the larger numbers.

[5] These tests are also included in the 2005 India Human Development Survey, a nationally representative survey of 40,000 Indian households. Spears (2012b) uses this data and these tests to document that the height-cognitive achievement gradient among Indian children is steeper than that in the U.S, and that sanitation accounts for part of this slope.

The ASER test is only given to children living in rural areas. Therefore, we are unable to estimate the effect of the TSC on urban children. However, as the TSC is an exclusively rural program, we would not expect an effect in urban areas.

## 2.2. Identification: Fixed effects

We use OLS linear probability models of achieving the three binary educational outcomes: recognizing letters, recognizing the small numbers, and recognizing the larger numbers. In order to account for fixed geographic heterogeneity and for an overall time trend, we use district and year fixed effects. Therefore our regression is:

$$achivement_{idt} = \beta TSC_{dt} + \gamma girl_{idt} + X_{idt}\theta + \lambda_1 lit_{dt+6} + \lambda_2 lit^2_{dt+6} + \alpha_t + \delta_d + \varepsilon_{idt},$$

where $i$ indexes children, $d$ indexes districts, and $t$ indexes years of birth (2001, 2002, and 2003). *Achievement* is an indicator that takes on 1 if the child demonstrated the relevant ability on the test and 0 otherwise. *TSC* is reported TSC latrines per capita built in a district by a year: notice that there is no $i$ subscript on *TSC* because it is the same for all children born in the same district in the same year. $\alpha$ and $\delta$ are district and year fixed effects. As recommended by Cameron, Gelbach, and Miller (2008), standard errors are clustered at the district level (not the district-year level) and asymptotic inference is acceptable with many more than 50 clusters.

The covariate *girl* is an indicator that the child is a girl; girls are subject to a range of disadvantages and deprivations in rural India. The vector $X_{idt}$ is a set of controls varying at the household or village level that are available from the Pratham ASER survey data. Some are about the child's household or parents: housing material, household electrification in general and on the day of the survey, whether the child goes to "tuition" tutoring classes, whether the child's father and mother have been to school, and whether the child's mother is literate. Others are about the child's district, described below, or about the child's village: whether it has electricity, a road, a health worker, a school, and a government ration shop.

The district controls are $lit_{dt+6}$ and $lit^2_{dt+6}$, that is, they are district level literacy in the year of the ASER test, entered as a quadratic polynomial. District literacy data is available from the 2001 and 2011 censuses. We construct annual frequency literacy observations by linearly interpolating between these two values (the quadratic term is intended to account for any nonlinearity; the result is quite similar if interpolation in logs is used instead). We intend these controls to partially account for any unrelated district trends in literacy or education.

These two sets of controls – household/parent and village/district – are added sequentially to demonstrate that neither has an important effect, suggesting that omitted variable bias is unlikely. After estimating these main results, we present two falsification tests, intended to rule out a spurious effect of unrelated district trends.

## 3. Results

Are children who live in districts in which more TSC latrines had been constructed by their first year of life more likely to recognize letters and numbers when they are six years old than comparable children born in the same district in different years, or in different districts in the same year? Figure 2 suggests that the answer may be yes. The figure plots the trends in mean number recognition separately for those districts that received any TSC latrines during the three years studied and those districts that did not. This is a very rough analysis because it does not account for district or year fixed effects, nor for any covariates, and ignores the substantial heterogeneity in TSC intensity and timing among those districts that were exposed. However, an initial answer is suggested by the shapes of the lines: from 2001 to 2002, before there is much

TSC intensity, the average number recognition moves in parallel across the two sets of districts. In 2003, when TSC intensity increases sharply, number recognition turns noticeably up in exposed districts, relative to unexposed districts. The rest of this section adds precision and depth to these observations while confirming the robustness of the statistical and causal inference.

## 3.1. TSC intensity associated with cognitive achievement

Table 2 presents the main result of the paper: greater TSC coverage in the first year of life is associated with greater cognitive achievement at age six, as measured by the ASER tests. The coefficients on TSC intensity must be interpreted with care: because TSC latrines are scaled *per capita*, they estimate the linear approximation to the effect of moving from 0 latrines per capita to 1 latrine per capita. The program did not nearly achieve this and would not aspire to, as members of a household can share a latrine, and some households already had latrines. Therefore, in the row below the TSC coefficients the table presents the average effect of the program: the coefficient estimate multiplied by the across-district mean of TSC intensity in 2003. Therefore, on average, the TSC increased the fraction of rural six year olds who could recognize letters by three-tenths of a percentage point. This is an important but not implausibly large effect.

The result is very similar across the three tests and levels of cognitive achievement. Adding the controls does little in any specification to change the estimated coefficients, suggesting that the result is not driven by an obvious omitted variable. Given the rural Indian context, it is reassuring of data quality that girls are consistently found to show worse performance on the tests than boys. Two districts report unusually high levels of TSC intensity in 2003 (0.16 and 0.20 TSC latrines per capita, or about one per household); including or excluding these outlier districts (the “restricted sample”) has no impact on the results.

Further respecifications are not reported in the table. Adding state-specific linear time trends does not change the slope estimate, although it does reduce precision. For example, with these trends the coefficient predicting recognizing simple numbers changes from 0.754 to 0.893, with a standard error of 0.536 and a two-sided $p$-value of 0.097. Similarly, estimation using logit rather than linear probability finds a similar result; for example, the $t$-statistic in the case of recognizing letters is 2.13.

Differencing out fixed effects produces consistent estimates because linearity is assumed. When a quadratic polynomial term in TSC intensity is introduced, it is not statistically significant for any of the three dependent variables (the $t$ statistics are 0.13, 0.81, and 1.47). $F$ statistics jointly testing the addition of a quadratic term and a cubic term are not statistically significant (the test statistics are 0.01, 0.48, and 1.70). Figure 3 plots local polynomial regressions of cognitive achievement on TSC intensity and finds little visual indication of non-linearity; these plots are designed to roughly correspond to the fixed effects regressions by using dependent and independent variables that have both been demeaned twice, first by district then by year.

As a final note in this section, it is possible that these results *underestimate* the effect of early-life sanitation coverage on cognitive achievement because of mortality selection. Spears (2012a) has demonstrated an important reduction of infant mortality due to TSC sanitation coverage. If the marginal children prevented from dying are below average in cognitive achievement then the estimated effect is lower than the true effect.

## 3.2. Falsification tests

### 3.2.1. Granger causality: No "effect" of future latrines

Two falsification tests are designed to assess the plausibility of a causal effect. The first, inspired by Granger causality, verifies that the cause (TSC intensity) precedes the effect (educational achievement), rather than the other way around (Angrist and Pischke 2009, 237-8). One threat to identification would be spurious correlation in district time trends in educational improvement and TSC implementation. If this were driving the results, rather than a causal effect of the TSC *per se*, we might expect to find similar "effects" of future latrine construction, that is, of the independent variable displaced into the future. On the other hand, very early life is known to be a critical period of cognitive development. If the TSC is indeed causing cognitive improvement, latrine coverage when a child is, for example, three or four years old would not be expected to have a large effect on cognitive outcomes. Therefore, this falsification test verifies that there is no "effect" on TSC exposure at later ages.

Table 3 reports the results. As expected, TSC exposure after the early life critical period does not predict later cognitive achievement on any of the three tests. We would not have been surprised if, in addition to an effect of exposure in the first year, TSC exposure in the second year of life mattered for cognitive achievement: Doyle, *et al.* (2003) note that before age 3 is a critical period for neural development. However, we do not find the absence of an effect in the second year problematic for two reasons. First, our year-level data is coarse (we do not know when a child was born in a year, or when exactly latrines were constructed) and might not be sufficiently powered to detect a smaller effect in the second year. Second, it is exactly in the first year of life that Spears (2012a) found an important health effect of the TSC.

### 3.2.2. Parallel trends: No “effects” on older children

The second falsification test is also designed to help rule out the possibility of spurious district trends. If education, test-taking, or test scores were coincidentally improving in the districts that received TSC latrines first, then one might also expect test scores to be increasing in this districts for older children. Seven and eight year olds in the same district would have been exposed to similar educational trends, but would not have been exposed to TSC sanitation in their first year of life. Therefore, this falsification test verifies that there is no “effect” of the TSC intensity that six year olds experience in their first year of life on older children who took the same ASER tests at the same time.

In other words, instead of regressing the test scores of children who were 6 years old in 2009 on TSC coverage in 2003, this test regresses the test scores of children who were 7 and 8 years old in 2009 on TSC coverage in 2003, expecting not to find an “effect.” If so, this would support the parallel trends assumption that underlies fixed effects identification: that the cognitive outcomes for six year olds would not have evolved in parallel across districts if not for the introduction of differential TSC trends.

Table 4 presents the results. There is no consistent effect on older children. If anything, there is one statistically-significant negative effect on eight year olds’ reading, which is very likely a spurious result of having estimated many regressions by this point in the paper.

## 3.3. Height and cognitive achievement

One method of assessing the plausibility of this result is to compare the magnitude of the estimate effect with other estimates in the literature. Case and Paxson (2010) found that taller children in the U.S. perform better, on average, on cognitive tests. Spears (2012b) not only replicated this result among Indian children, but found that the slope in India is more than twice as steep as in the U.S. Approximately, a one standard deviation increase in height is associated with about a 5 percentage point increase in the linear probability of being able to read words among Indian eight year olds.

The results of this paper can be combined with Spears's (2012a) estimate of the effect of the TSC on children's height-for-age to verify the mutual consistency of these numbers. Analogously to using the TSC as an instrument, the ratio of the coefficients from this paper and Spears (2012a) form an estimate from different samples of the association between height and cognitive ability:

$$\beta^{IV} = \frac{0.75 \ {}^{reading}\!/_{TSC\ latrines\ per\ capita}}{8.5 \ {}^{height\ for\ age}\!/_{TSC\ latrines\ per\ capita}} \approx 0.088.$$

The numerator is this paper's estimate from Table 2. The denominator is Spears's (2012a) estimate of the comparably-scaled effect of the TSC on height for age. The ratio, an 8.8 percentage point increase in reading association with a one standard deviation increase in height, is roughly similar to the 5 percentage point slope found by Spears (2012b). It is unsurprising that this procedure's slope produces a slightly larger estimate: it has, in the TSC, a clear source of causal variation. The mutual consistency among these three results may be reassuring about all of the estimates involved.

# 4. Discussion

We find an effect of exposure India's Total Sanitation Campaign in the first year of life on cognitive skills at age six. This is consistent with evidence in the literature of critical periods for cognitive development and of a well-identified effect of the TSC on early life health.

Of course, exposure to the TSC was not randomly assigned. However, we find a consistently-sized effect across several measures of cognitive ability, an effect that is not sensitive to various respecifications. More importantly, two falsification tests are consistent with a causal effect: future latrines do not have an effect "back in time" on cognitive development, and no effect is seen on older children who took the same tests at the same time but would not have been exposed to the TSC. These tests suggest that our findings are not artifacts of spurious correlations of district trends.

These results are important because of the persistence into adulthood of childhood differences in cognitive skills. That is, there is no reason to think that the effects of the TSC end after a child is six years old. For example, Brooks-Gunn, *et al*. (2006) show that differences in cognitive ability at age 3 approximately remain through ages 5 and 8 to age 18. Knudsen, *et al*. (2006) review evidence that early life disadvantage translates into cognitive heterogeneity of the adult workforce. Vogl (2011) documents this in Mexico. Our findings suggest that even a low capacity government can implement a relatively inexpensive program that will cause an important improvement in cognitive abilities, given the context of widespread open defecation in developing countries such as India. In the meanwhile, poor sanitation may represent an important limit to human capital.

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Figure 1: Identification strategy: Timeline for children six years old at last birthday

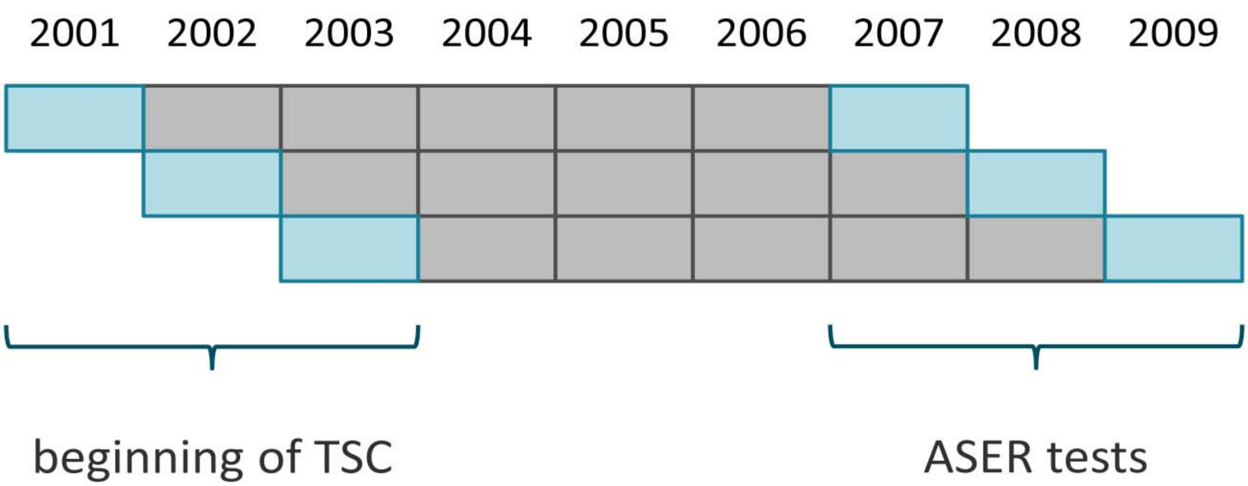
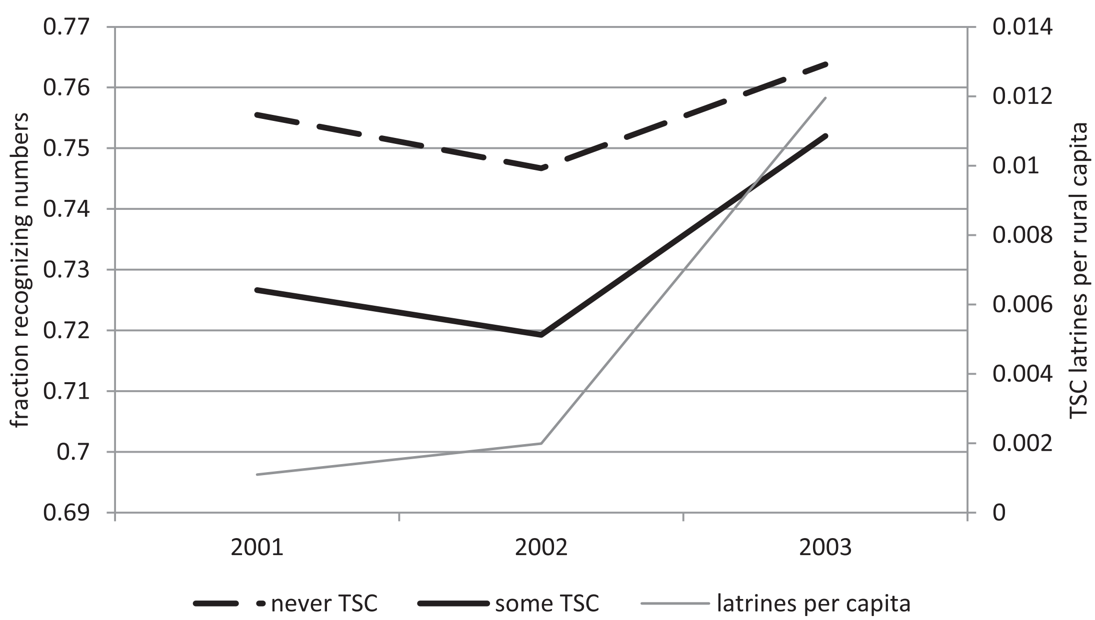
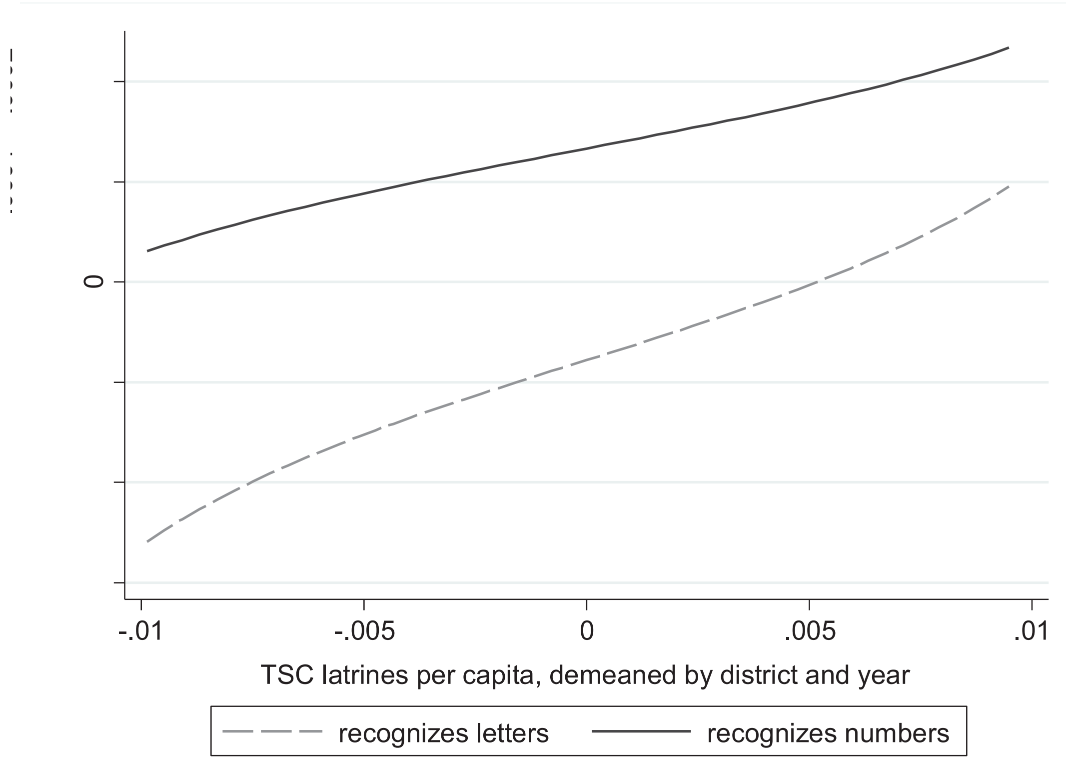



Figure 2: Parallel trends before TSC latrines, different trends after




Birth years are listed on the horizontal axis; tested years are six years later. The “never” and “some” TSC lines split the sample at the district level according to whether the district was reported to have any TSC latrines built in 2001, 2002, or 2003. Plotted points are simple means, computed without fixed effects.

Figure 3: Cognitive achievement and TSC intensity, local polynomial regressions




The independent and dependent variables have both been demeaned twice, first by district and then by year, to simulate the fixed effects regressions.

Table 1: Summary statistics

| | full sample | restricted sample | never TSC | some TSC |
|---|---|---|---|---|
| TSC household latrines per capita | | | | |
| built by 2001 | 0.00044 | 0.00043 | 0.00000 | 0.00110 |
| | (0.00009) | (0.00009) | | (0.00022) |
| built by 2002 | 0.00079 | 0.00078 | 0.00000 | 0.00199 |
| | (0.00014) | (0.00014) | | (0.00034) |
| built by 2003 | 0.00455 | 0.00455 | 0.00000 | 0.01195 |
| | (0.00048) | (0.00048) | | (0.00110) |
| ASER test scores | | | | |
| reads letters or better | 0.748 | 0.748 | 0.759 | 0.731 |
| | (0.0078) | (0.0078) | (0.0099) | (0.0126) |
| recognizes numbers 1-9 or better | 0.747 | 0.747 | 0.756 | 0.733 |
| | (0.0078) | (0.0078) | (0.0098) | (0.0123) |
| recognizes numbers 10-99 or better | 0.325 | 0.325 | 0.327 | 0.321 |
| | (0.0080) | (0.0080) | (0.0104) | (0.0123) |
| female | 0.444 | 0.444 | 0.441 | 0.448 |
| | (0.0027) | (0.0027) | (0.0035) | (0.0043) |
| pucca house | 0.293 | 0.294 | 0.275 | 0.321 |
| | (0.0091) | (0.0091) | (0.0119) | (0.0135) |
| father went to school | 0.650 | 0.650 | 0.646 | 0.656 |
| | (0.0074) | (0.0074) | (0.0095) | (0.0118) |
| mother went to school | 0.486 | 0.486 | 0.477 | 0.501 |
| | (0.0089) | (0.0089) | (0.0112) | (0.0148) |
| mother literate | 0.334 | 0.334 | 0.320 | 0.355 |
| | (0.0079) | (0.0079) | (0.0097) | (0.0136) |
| village has a government school | 0.844 | 0.844 | 0.831 | 0.864 |
| | (0.0070) | (0.0070) | (0.0091) | (0.0105) |
| *n* (six year olds) | 48,048 | 47,975 | 29, 310 | 18,738 |
| districts | 575 | 573 | 337 | 238 |

"Restricted sample" excludes two districts with very high reported TSC intensity. The "never" and "some" TSC columns split the sample at the district level according to whether the district was reported to have any TSC latrines built in 2001, 2002, or 2003. Standard errors clustered by district reported in parentheses.

Table 2: Main result: TSC intensity and cognitive achievement

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | restricted sample | | | | full sample | |
| **Panel A: Reading (recognizes letters or better; mean = 0.748)** | | | | | | |
| TSC household | 0.752* | 0.744* | 0.689* | 0.776* | 0.752* | 0.777* |
| latrines per capita | (0.339) | (0.339) | (0.348) | (0.355) | (0.339) | (0.355) |
| effect at overall 2003 mean | 0.003 | 0.003 | 0.003 | 0.003 | 0.003 | 0.003 |
| girl | | -0.0104** | -0.00906* | -0.00922* | | -0.00913* |
| | | (0.00389) | (0.00381) | (0.00380) | | (0.00379) |
| household, parent controls | | | ✓ | ✓ | | ✓ |
| village & district controls | | | | ✓ | | ✓ |
| *n* (six-year olds) | 47612 | 47612 | 47612 | 47612 | 47684 | 47684 |
| **Panel B: Math (recognizes numbers 1 to 9 or better; mean = 0.747)** | | | | | | |
| TSC household | 0.754* | 0.743* | 0.688+ | 0.783* | 0.754* | 0.784* |
| latrines per capita | (0.359) | (0.360) | (0.373) | (0.378) | (0.359) | (0.378) |
| effect at overall 2003 mean | 0.003 | 0.003 | 0.003 | 0.003 | 0.003 | 0.003 |
| girl | | -0.0150*** | -0.0137*** | -0.0138*** | | -0.0137*** |
| | | (0.00378) | (0.00373) | (0.00372) | | (0.00372) |
| household, parent controls | | | ✓ | ✓ | | ✓ |
| village & district controls | | | | ✓ | | ✓ |
| *n* (six-year olds) | 47063 | 47063 | 47063 | 47063 | 47133 | 47133 |
| **Panel C: Math (recognizes numbers 10 to 99 or better; mean = 0.325)** | | | | | | |
| TSC household | 0.897* | 0.889* | 0.807* | 0.810* | 0.897* | 0.810* |
| latrines per capita | (0.407) | (0.408) | (0.377) | (0.375) | (0.407) | (0.375) |
| effect at overall 2003 mean | 0.004 | 0.004 | 0.004 | 0.004 | 0.004 | 0.004 |
| girl | | -0.0102* | -0.00876* | -0.00893* | | -0.00901* |
| | | (0.00433) | (0.00422) | (0.00421) | | (0.00421) |
| household, parent controls | | | ✓ | ✓ | | ✓ |
| village & district controls | | | | ✓ | | ✓ |
| *n* (six-year olds) | 47063 | 47063 | 47063 | 47063 | 47133 | 47133 |

Standard errors clustered by 573 (restricted) or 575 (full sample) clusters in parentheses. The restricted sample omits two districts with unusually high reported levels of TSC construction. Two-sided p-values: + $p < 0.1$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$. All specifications include district and year fixed effects. Scaled effects below the TSC coefficient multiply by the program means.

Table 3: Granger causality: No "effect" of future latrines

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | effect of TSC latrines in child's *y*th year of life: | | | | | |
| | 1st (age 0) | 2nd (age 1) | 3rd (age 2) | 4th (age 3) | 5th (age 4) | 6th (age 5) |
| Panel A: Reading | | | | | | |
| TSC latrines | 0.752* | -0.189 | 0.329 | 0.0206 | -0.203 | -0.142 |
| per capita | (0.339) | (0.230) | (0.235) | (0.176) | (0.203) | (0.227) |
| *n* (six-year olds) | 47684 | 47684 | 47684 | 47684 | 47684 | 47684 |
| Panel B: Math (numbers 1-9) | | | | | | |
| TSC latrines | 0.754* | -0.234 | -0.00616 | -0.236 | 0.0271 | 0.0546 |
| per capita | (0.359) | (0.209) | (0.255) | (0.180) | (0.189) | (0.219) |
| *n* (six-year olds) | 47133 | 47133 | 47133 | 47133 | 47133 | 47133 |
| Panel C: Math (numbers 10-99) | | | | | | |
| TSC latrines | 0.897* | 0.161 | 0.138 | 0.0888 | 0.199 | 0.0975 |
| per capita | (0.407) | (0.246) | (0.307) | (0.193) | (0.234) | (0.228) |
| *n* (six-year olds) | 47133 | 47133 | 47133 | 47133 | 47133 | 47133 |

Each estimate is a coefficient from a separate regression. Standard errors clustered by 575 clusters in parentheses. Two-sided p-values: + $p < 0.1$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$. All specifications include district and year fixed effects.

Table 4: No "effect" of 6 year olds' latrines on older children in the district

| | (1) | (2) | (3) |
|---|---|---|---|
| | effect of 2001-2003 latrines on: | | |
| | 6 year olds | 7 year olds | 8 year olds |
| **Panel A: Reading** | | | |
| TSC latrines | 0.752* | -0.0460 | -0.402* |
| per capita | (0.339) | (0.298) | (0.196) |
| *n* (children) | 47684 | 44056 | 59476 |
| **Panel B: Math (numbers 1-9)** | | | |
| TSC latrines | 0.754* | -0.356 | -0.121 |
| per capita | (0.359) | (0.270) | (0.186) |
| *n* (children) | 47133 | 43688 | 59019 |
| **Panel C: Math (numbers 10-99)** | | | |
| TSC latrines | 0.897* | 0.387 | -0.188 |
| per capita | (0.407) | (0.420) | (0.406) |
| *n* (children) | 47133 | 43688 | 59019 |

Each estimate is a coefficient from a separate regression. Standard errors clustered by 575 clusters in parentheses. Two-sided p-values: + $p < 0.1$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$. All specifications include district and year fixed effects.