1 Introduction

Health and disease early in life have lasting consequences for human capital (Currie, 2009). Evidence is accumulating that poor health and inadequate net nutrition in early life cause persistent deficits in cognitive development and skills (Case and Paxson, 2008; Barham, 2012). Cunha et al. (2010) advocate investing in very young children's cognitive skills, based on evidence of especially 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. Over 600 million people in India – 53 percent of Indian households – defecate in the open, without using a toilet or latrine (Unicef & WHO 2012). This open defecation is an important cause of infant and child disease and mortality. Spears (2013) observes that open defecation can statistically account for much of the variation across poor countries in average child height. Because the same factors that promote early-life physical development also encourage children's cognitive development (Strauss and Thomas, 1998), diseases caused by poor sanitation have been associated with cognitive achievement. Bleakley (2007) finds that the eradication of hookworm in the American South in the early 20th century led to important increases in literacy. Variation in early-life health may be particularly important for cognitive outcomes in India, where the child height-cognitive achievement gradient is particularly steep (Spears, 2012b).

From 2001 to 2012, the Indian government promoted the construction and use of low cost pit latrines in rural areas through a large program called the Total Sanitation Campaign (TSC).[1] This paper asks whether, in improving the early-life disease environment faced

[1] Spears (2012a) estimates that the TSC 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.

by rural Indian children, the TSC also improved the cognitive skills that they subsequently attain. This paper estimates an effect of the TSC on Indian children's cognitive skills by matching test score data on six year olds' academic achievement with government administrative data on TSC program intensity early in their life, focusing on the first three years of the TSC, 2001 to 2003. The contribution of the paper is to show that, during the period studied, the TSC caused Indian children to be 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, where open defecation is common and high population density contributes to negative externalities of disease. This paper adds evidence of a loss of cognitive ability to the health consequences of unsafe excreta disposal that are already documented in the literature. Second, because childhood cognitive skills predict adult cognitive skills, these results imply a detrimental effect of widespread open defecation in developing countries on adult human capital and 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 and imperfect implementation of a program by the Indian government. This means that the estimated average treatment effects incorporate whatever heterogeneity, administrative costs, and losses to corruption that exist in real implementation. Nevertheless, this analysis finds a positive average effect of sanitation on children's cognitive achievement. This suggests that low-cost rural sanitation strategies that can be implemented even by limited capacity governments can importantly support children's cognitive development.

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. Grantham-McGregor et al. (1999) put "poor sanitation" at the top of a list of "community or ecological" barriers to early childhood development in low-income countries. Our paper joins a growing literature documenting effects of early life health, disease, and nutrition on later life cognitive abilities. Many of these papers, like ours, use an econometric identification strategy based on differences-in-differences or other fixed effects implementations of 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 (2012) 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, although this approach does not consider sanitation externalities, Vogl notes that children whose households had toilets or latrines at age 12 grew into taller adults with more education and higher cognitive test scores.

Perhaps the most complementary recent paper to this one is Shah and Steinberg's (2013) analysis of the cognitive effects of exposure to drought in utero. They study the same ASER data on learning tests administered to Indian children that this paper uses. Using a similar fixed effects strategy, they find large effects: for example, they find that exposure to drought in utero is associated with being 2 percentage points less likely to recognize numbers in childhood.

Epidemiological literature suggests a large effect on early-life health and net nutrition of exposure to open defecation. One well-known mechanism is diarrhea (Guerrant et al., 1992). Checkley et al. (2008) pool data from several field sites to show an effect of diarrhea in children on subsequent physical growth. Another increasingly well understood possible mechanism is tropical or environmental enteropathy. Evidence is converging that this chronic disorder of the small intestine, due to “faecal bacteria ingested in large quantities by young children living in conditions of poor sanitation and hygiene” (Humphrey, 2009, 1032), could cause widespread malabsorption of nutrients among children living in places where open defecation is common (Petri et al., 2008; Mondal et al., 2011; Korpe and Petri, 2012). In a recent study in rural Bangladesh, Lin et al. (2013) show that children exposed to less sanitary conditions show more biologically measured markers of enteropathy and do not grow as tall (also see Kosek et al., 2013). These epidemiological and medical studies, combined with a growing literature within economics that documents effects of poor sanitation on early-life health,[2] suggest that diminished human capital and cognitive skills may be a further consequence of open defecation.

1.2 Outline

This paper continues in eight further sections. Section 2 first offers descriptive statistics from the India Human Development Survey as motivation: children in this survey who are exposed to more local open defecation perform worse on cognitive tests similar to those we study. Section 3 provides introductory context about the Total Sanitation Campaign (TSC), a government program in India. Section 4 details our difference-in-differences empirical strategy. Section 5 presents our main result, that children exposed to a sanitation environment that had been improved by the TSC by their first year of life subsequently showed greater cognitive skills at age six. Section 6 reports evidence that pre-program trends in learning outcomes and related measures were similar in districts that did and did not receive the TSC. Section 7 verifies that our findings are consistent with the hypothesized mechanism of effect, by showing that the sanitation-achievement gradient is steepest for exposure in the first year of life. Section 8 reports a simulation exercise designed to account for possible un-

[2] A growing set of studies in economics documents the health importance of sanitation. Cutler and Miller (2005) show an effect of adoption of clean water technology in U.S. cities on mortality rates in the twentieth century. Watson (2006) documents an effect of improving sanitation in U.S. Native American populations on infant mortality. Galiani et al. (2005) show that privatizing water supply in Argentina reduced child mortality.

derestimation of the effect of the program, due to endogenous effects on mortality, correlated with cognitive skills. Finally, section 9 concludes.

2 Motivation: Open defecation and cognitive achievement in the IHDS

Section 1.1 reviewed evidence in the literature that early-life health and net nutrition shape later human capital and cognitive achievement. Is exposure to open defecation indeed associated with less cognitive achievement? As a motivation for this paper's evaluation of the impact of the TSC on children's learning outcomes, this section uses the rural sample of the 2005 India Human Development Survey (IHDS) to document a robust association between sanitation externalities and children's test scores.

The IHDS interviewed about 40,000 households in India and gave children aged 8 to 11 learning tests that were modeled closely on the ASER tests that this paper primarily studies. Section 6.3 uses this data to document that TSC program intensity was not correlated with trends in test scores in children whose early life developmental period was before the program. Although the main objective of this paper is to exploit heterogeneity in program timing to identify an effect of the TSC, there are two reasons the IHDS provides additional useful motivation. First, the IHDS includes detailed socio-economic data, specifically including household consumption per capita. Second, the IHDS allows us to match children's learning outcomes to the local area open defecation to which they are exposed in their own village. Additionally, this result has the advantage of being representative of rural Indian households.

Figure 1 documents that children living in villages where most of their neighbors defecate in the open are less likely to be able to read paragraphs than children living in villages with better sanitation.[3] The sample of rural children is split into those who live in local

[3] The rest of this paper, studying younger children, will use reading words rather than reading paragraphs as a dependent variable.

areas where more than half of households defecate in the open and those who live in areas where less than half of households do. For both groups, reading outcomes are plotted conditional on consumption per capita. This ensures that the association between sanitation and achievement does not merely spuriously reflect household wealth: panel (a) shows a mean difference at all levels of household consumption per capita.

Panel (b) accounts for further dimensions of household heterogeneity that may be associated with children's cognitive outcomes. The graph plots residuals after regressing reading and consumption on age-times-sex indicators and a vector of further wealth and education controls: 15 indicators for highest female education in the household; 15 indicators for highest overall adult education in the household; 13 indicators for the language in which the test was taken; a set of indicators that the child is in school, has been in school, or has never gone to school; and the asset count reported by the IHDS. The gap remains, indicating that the gradient between cognitive achievement and sanitation externalities additionally may not merely reflect parents' education, child schooling, or these other dimensions of observed heterogeneity. The remainder of this paper uses heterogeneity in the timing of a government program to assess whether early-life exposure to fecal germs can impact later cognitive achievement.

3 Context: India's Total Sanitation Campaign

Open defecation without using a toilet or latrine is particularly widespread in India. In 2001 – the beginning of the time period this paper studies – 63.6 percent of households defecated in the open, according to the Indian census. This declined to 53.1 percent in 2011, in part due to the government's Total Sanitation Campaign. Clearly much open defecation remains; indeed more than half of all people anywhere who defecate in the open live in India.

The TSC, a "flagship" program of the central Indian government, was a large effort to improve rural sanitation. Over the approximately ten years of the program, it reported building one household pit latrine per 10 rural people in India and spent U.S.$1.5 billion. Basic pit latrines, which cost around U.S.$30-$50 to build (but can also be made much more expensively), are an inexpensive and effective method to safely dispose of human excreta, if they are used.[4] Because this paper's identification strategy 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. This paper will study heterogeneity in the timing of the implementation of the TSC at the district-year level, a large enough unit to account for the externalities of open defecation. Districts are divisions of states; there are about 600 districts in India, and we study 575 that have a rural population.

It is widely agreed that the TSC did not come close to eliminating rural open defecation in India, but it did importantly *reduce* open defecation (Barnard et al., 2013). The TSC was designed to focus on – and, in some cases, reward – the outcome required for improved health: latrine use. The TSC focused on creating demand for latrine use, in recognition of the many subsidized latrines that sit unused in rural India (Alok, 2010). One aspect of the program was an *ex post* monetary incentive to local village leaders, awarded when a village is verified to be open defecation free.[5]

In part, the TSC did this by making use of social forces, and of social hierarchy existing within villages: local leaders sometimes acted to motivate the community with social norms. Pattanayak et al. (2009) conducted a randomized, controlled trial in rural India where they demonstrated that using community meetings to shame villagers about open defecation could promote sanitation take-up; this experiment was designed to asses techniques used by the TSC. For example, Hammer et al. (2007) report that, as the TSC was implemented in an experimental collaboration of the government and the World Bank in Maharashtra, high-caste villagers were motivated to build latrines for the entire village with the observation

[4]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 abandoned or taken apart for housing material.

[5]Using a discontinuity in the size of the incentive as a function of village population, Spears (2012a) demonstrates the role of this incentive in making the TSC effective in the period after it was introduced.

that otherwise flies would carry particles of low-caste people's feces into high-caste people's food.

WaterAid (2008) documents "significant policy variations across the states": "there are variations across states and districts in terms of approaches and strategies adopted and results achieved as well" (p. 7). This heterogeneity has been "at times, not really in line with the stated TSC strategy of the program being 'community led"' (p. 9). India is a federal democracy with powerful central and state bureaucracies and several levels of elected officials with autonomous power.[6] Because the TSC depended not only upon construction but also upon information and persuasion, its implementation was heterogeneous, in large part according to the commitment of state, mid-level, and especially local government officials. Alok (2010), an Indian Administrative Service officer involved in the design and central oversight of the TSC, details the importance of these differing levels of political commitment. As an official once explained to one of the authors, "it's person driven here in [our state] – if the person in charge wants it..." Of course, a large political economy literature documents that policy outcomes can be correlated with many social and economic variables; however, in this case, section 4.3 will show that the within-district trends in TSC implementation that we exploit for identification are not correlated with within-district trends in a range of observed socioeconomic and educational variables.

4 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, compared with children born in the same district in different years, or in different districts

[6] WaterAid (2008) notes that in some states the TSC was implemented by Rural Development departments, while in others it is implemented by a Public Heath Engineering department. "In most places TSC [was] being implemented in a routine administrative fashion without any conscious and visible attempt to strategise the implementation of the program" (p. 22). States and districts have also varied in whether they have collaborated with NGOs to implement the rural information and education components of the program.

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.

Figure 2 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 on exposure to sanitation in a child's first year of life (2001, 2002, and 2003). We will verify that it is from exposure in the first year of life that the sanitation-achievement gradient is steepest, a result consistent with the mechanism of an early-life critical period Doyle et al. (2009).

4.1 Data

Table 1 presents summary statistics from the two data sources used in this paper: administrative records on TSC implementation, and Pratham's ASER survey on education outcomes, which includes learning tests.

4.1.1 Independent variable: Total Sanitation Campaign administrative records

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 data were reported by block officials to district offices, where they were aggregated into district-level figures that were input into a computer system maintained by the central government.

The TSC data report latrine *counts*, but districts vary considerably in size. The 5th percentile district in our data had a 2001 rural population of 120,000; the 95th percentile district had a rural population of 3 million. Therefore, as our measure of program intensity, we compute *TSC latrines per capita*: the number of TSC latrines reported as built in the official data by the end of that year, per rural person in the 2001 census.

The years in which the studied children were born – 2001 to 2003 – were the earliest years in which any TSC latrines were built. By 2003, the program had built about one latrine, on average, per 20 households 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 show lower overall educational performance. These differences indicate that the use of district fixed effects will be a necessary part of the identification strategy.

We interpret TSC latrines reported built per capita as a noisy measure of overall TSC intensity. It is imperfect in at least two ways. First, it measures only latrine *construction*, an easily quantifiable but secondary part of a program that would succeed only if it encouraged people to use latrines, including latrines they, in some cases, already owned (Barnard et al., 2013). Second, bureaucratic agents of distant government principals clearly had incentives to inflate latrine counts. Our strategy requires only that reported latrine construction be *correlated* with true TSC program intensity. Insofar as our independent variable is a noisy measure of overall TSC activity, our estimates of the effect will be attenuated towards zero. If administrative capacity or corruption caused administrative records to be inflated upwards – presumably in the districts where other dimensions of welfare that would be correlated with test scores are *worse* – then this would introduce *negative* omitted variable bias into our estimates.[7] Therefore, we use these administrative records cautiously as an imperfect,

[7] To see this omitted variable bias from inflation of administrative records, assume that test scores are shaped by the TSC and other uncorrelated factors, so $test = \beta TSC + \varepsilon$. We observe only $\widetilde{TSC}$, which is true program intensity plus an inflation factor ι: $\widetilde{TSC} = TSC + \iota$. Then the numerator of the OLS estimator of $\hat{\beta}$ will be $\beta \text{var}(TSC) + \beta \text{cov}(TSC, \iota) + \text{cov}(\iota, \varepsilon)$. Under the assumptions, which we find reasonable, that inflation will not be positively correlated with actual performance and service delivery, and that inflation

and perhaps conservative, signal of program intensity.

4.1.2 Dependent variables: Pratham's Annual Status of Education Report

Since 2005, Pratham – a large Indian non-governmental organization – has coordinated the Annual Status of Education Report (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. 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.

The dependent variables of this paper are children's outcomes on the educational tests in the ASER surveys. 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 larger numbers.

All districts with a rural population are included in ASER's sample (ASER, 2010). Within each district, ASER randomly selected 30 villages using probability proportionate to size sampling based on village sizes in the 2001 census. Within each village, they used a field-implemented random sampling strategy, stratifying within hamlets if necessary, to randomly sample 20 households by drawing a map. Although this strategy is representative (and self-weighting) at the district level (and districts are the level at which our independent variable varies), households in smaller districts are more likely to be selected into the national sample than households in larger districts. We attempt to estimate a causal effect, not merely describe the population, so weights may be inappropriate, as Solon et al.

will be negatively correlated with other social, political, and economic factors shaping test scores, that is ε, then the omitted variable bias due to inflation of administrative records would be *away from* the positive effect that we find.

(2013) have recently documented. However, as a robustness check we replicate our results by constructing weights according to district population, and there is no important change.[8]

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. 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`.

4.2 Identification: Difference-in-differences

We estimate OLS linear probability models of achieving the three binary educational outcomes: recognizing letters, recognizing single-digit 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, separately for each outcome, our regression is:

$$achivement_{idt} = \beta TSC_{dt} + \gamma girl_{idt} + X_{idt}\theta + \lambda_1 lit_{dt+6} + \lambda_2 lit^2_{dt+6} + \alpha_t + \delta_d + \varepsilon_{idt}, \quad (1)$$

where i indexes six-year-old 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 the end of the first year of the child's life, as in figure 2.[9] 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. δ and α are district and year fixed effects. As recommended by Cameron

[8]Indeed, for example, our main result estimate of 0.75 in column 1 of panel A of table 3 for the effect on reading slightly increases to 0.90 when weighted, with a standard error clustered at the district level of 0.39.

[9]In fact, the ASER data report the district in which the child currently lives, not the district where she lived during her first year of life. However, permanent rural-to-rural migration is rare in India, other than of women without children moving for marriage (Munshi and Rosenzweig, 2009). Demand for latrines is unfortunately sufficiently low that it is unlikely that the TSC would *cause* anyone to migrate, so migration is unlikely to be a threat to our identification.

et al. (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 (called an ASHA), a school, and a government ration shop.

The district controls are lit_{dt+6} and lit^2_{dt+6}, or district level literacy in the year of the ASER test 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 (although the quadratic term is intended to account for any non-linearity, the result is quite similar if interpolation in logs is used instead). We intend these controls to partially account for 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 on the coefficient on the TSC, suggesting that omitted variable bias is unlikely.

4.3 Balance of covariates

If TSC program intensity were associated with the observable characteristics of children and their environment, then we might doubt that variation in the TSC had an exogenous effect on children's learning. In this section, we assess whether the program predicts the covariates that are included in the ASER data. This analysis is analogous to verifying that treatment and control groups are balanced on observables in a randomized experiment. Table 2 reports results from running the main regression with district and year fixed effects in equation (1), substituting other observables as the the dependent variable. For robustness, in addition to the measure of TSC intensity, we separately regress the observed properties of children on a binary indicator that the TSC had started, meaning that any positive number of latrines had been built in that district by that year.

Table 2 demonstrates that none of the characteristics of children or households that we observe are predicted by the variation in TSC program intensity that we use to identify an effect on cognitive achievement. In particular, children exposed to more TSC latrines in their first year of life are not more likely to attend extra "tuition" classes outside of school, nor do they have better educated fathers; if anything, they have slightly less well educated mothers. Note specifically that we find no association of early-life exposure to the TSC with school enrollment among these six-year-olds. Village-level variables are also uncorrelated with TSC implementation, indicating that our results do not reflect overall trends in local government service delivery. Of course, this exercise cannot fully eliminate the possibility of omitted variable bias from another factor, but it may be unlikely that such an important factor would be uncorrelated with the variables observed here.

4.4 Evidence for parallel trends

Equation 1 will only estimate the effect of the program if we can make the parallel trends assumption that test scores would have evolved along similar trends, on average, in districts where the TSC was and was not implemented, without the program. Although we cannot observe this counterfactual case directly, we can assess this assumption by looking for differences in trends established before the program, in additional datasets. In section 6 we do this in three different ways. First, we show that the distributions of district level improvements in literacy between the 1991 and 2001 censuses are quite similar among districts that did and did not receive any reported TSC latrines by 2003. Second, we study within-district trends in literacy in the India Human Development Survey, and find no difference in trends either for children slightly older than the children we study here nor for women who are mothers of children of the age group we study. Finally, we return to the ASER-like tests in the IHDS that were introduced in section 2. Using data on children in the same districts as our main sample but a few years too old to have been importantly exposed to the TSC, we find no association between within-district trends in TSC implementation and trends in these children's cognitive test scores.

4.5 Mechanism checks

It is more plausible that the association we report reflects a causal effect if it appears to reflect mechanisms known from the literature to connect disease to cognitive achievement. In particular, there are critical early-life periods of neural development (Doyle et al., 2009), so our findings may be more plausible if the sanitation-cognition gradient we find is steepest for exposure in the first year of life. There is evidence in the literature that sanitation exposure matters for health very early in life: studying the same sanitation program as we do and using the same government records as the independent variable, Spears (2012a) finds an effect on mortality in the first year of life, and finds an effect of exposure in the first year of life on subsequent height.

Studies that examine heterogeneity in the ages at which children change environments have demonstrated that very early life is the most sensitive period for later cognitive and overall development. For example, Beckett et al. (2006) study cognitive outcomes at age 11 of 131 children adopted from Romania in the 1980s to the U.K. They find that children who moved before they were six months old were able to catch up with a sample of U.K.-born adoptees, but children who moved after they were six months old did not. In a randomized, controlled trial, Nelson et al. (2007) study abandoned children in Bucharest who were moved from institutions to foster care; children moved before they are 18 months old score better in subsequent years than children moved later, especially compared with children moved after 30 months. A similar study reflecting the Indian context of this paper is by Proos et al. (1993), who study the physical growth of Indian children adopted into Sweden early in life. Even within this group of young children, they find that children who were brought to Sweden at a younger age eventually grew taller.

In order to verify the importance of very-early life, we test for the maximum-effect timing of exposure in two complementary but distinct ways. The general approach is to replicate the estimation of our regression equation 1, but with the years of either the dependent or independent variable changed, in order to change the timing of the exposure in a child's life. First, we continue to focus only on the same sample of six-year-old children, but use the same identification strategy to identify an "effect" of TSC intensity in later years of the same children's lives. Second, we replicate our estimation strategy for children of different ages, holding constant the calendar years of TSC implementation used as the independent variable; this has the result of varying the year of the child's life in which TSC exposure is being assessed. Consistent with what we expect, we do not find an effect on cognitive achievement of later-life exposure to the TSC.

5 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? Figure 3 suggests that the answer may be yes. In a rough initial analysis, 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, alongside the trend in reported TSC latrine construction. It is clear from the figure that, although some positive number of TSC latrines were reported built in 2001 and 2002, the largest increase in program intensity during the period we study was from 2002 to 2003.

An initial answer to our question is suggested by the shapes of the lines: from 2001 to 2002, before there is much TSC intensity in any districts, test scores changed in parallel across the two sets of districts. In 2003, when TSC intensity increases sharply in some districts, test scores turn 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.

5.1 TSC intensity associated with greater cognitive achievement

Table 3 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 a 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 among rural six year olds studied in the ASER survey, during this period the TSC increased the fraction who could recognize letters by about three-tenths of a percentage point.[10] This is an important but not implausibly large effect.

The result is very similar across the three tests and levels of cognitive achievement.[11] Adding the controls does little in any specification to change the estimated coefficients, suggesting that the result is not driven by an 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 especially high levels of TSC

[10] Another way of arriving at this result is to regress test scores on an indicator for a district being reported to have received any TSC latrines by that year. As is well-known, this dichotomization will reduce power and attenuate estimates by introducing measurement error into the independent variable. This is associated with a statistically significant coefficient of about 0.015 for reading and math tests, which can be multiplied by about one-fifth of studied children being exposed to the TSC during this period.

[11] Additionally, 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.

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 tests, not reported in the table, confirm these results. First, much policy-making in India – in education, sanitation, and other sectors – is done at the state level. However, heterogeneity in state experiences cannot account for this result: adding state-specific linear time trends does not change the estimate of the sanitation-achievement gradient, 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.

Second, any convergence or divergence of test scores that is correlated with baseline levels of education or sanitation would limit the credibility of our causal identification. To account for this possibility, we replicate the main specification of column 1 with the addition of interactions of year indictors with baseline sanitation and literacy, both from the 2001 census. If districts that started out disadvantaged on one of these dimensions saw faster (or slower) subsequent growth in test scores, then this would change our results. However, the results are unchanged: compared with the estimated effects of 0.752 for recognizing letters and 0.754 for recognizing numbers, the corresponding effects and (standard errors) are 0.775 (0.339) and 0.781 (0.359) with year indicators interacted with pre-program sanitation, 0.846 (0.343) and 0.830 (0.360) with year indicators interacted with pre-program literacy, and 0.864 (0.344) and 0.848 (0.359) with both sets of controls.

Third, 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 4 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.

Fourth, the effect of the TSC on learning outcomes is not different depending on whether the child's mother is literate. The effect on reading, for example, is slightly less for children of literate mothers, but with a t statistic of 0.38 on the interaction. This is consistent with disease externalities as a mechanism of the effect. Spears (2013) has shown that exposure to local open defecation stunts the growth of rich and poor children both, and even those living in households which use a toilet or latrine.

5.2 Assessing effect magnitudes

Are these effect sizes plausible? One method of evaluating the effect size is to compare this paper's estimates, causally identified using heterogeneity in program timing, with OLS cross-sectional estimates from another data source. Section 2 reported a gradient between sanitation and cognitive achievement using the IHDS. These results are not directly comparable because they are for 8 to 11 year olds, not 6 year olds, and therefore focus on the higher achievement levels of reading paragraphs or better, which is approximately a median split in that sample. Nevertheless, regressing an indicator for reading paragraphs on local area (PSU) latrines per capita, and including all of the controls used in figure 1, finds a slope of 0.82 (clustered standard error = 0.25). Because of measurement error from the larger geographic area of districts rather than villages, and because of the general tendency of OLS to overestimate, it is unsurprising that this OLS finding is slightly larger than this paper's estimates around 0.75.

Another approach to assessing the plausibility of this result is to compare the magnitude of the estimate effect with what is implied by 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 gradient between height and cognitive ability:

$$\beta^{IV} = \frac{\frac{0.75\ reading}{TSC\ latrines\ per\ capita}}{\frac{8.5\ height\ for\ age}{TSC\ latrines\ per\ capita}} \approx 0.09. \tag{2}$$

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 approximately 9 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). The consistency of these results may be reassuring about all of the estimates involved.

6 Evidence for pre-program parallel trends

The estimates of the effect of the TSC in section 5 are only valid under the assumption of parallel trends: on average, districts exposed to more TSC intensity would have evolved similarly to districts exposed to less TSC intensity, if the program had not happened. Although this counterfactual assumption can never be verified directly, here we provide evidence of parallel trends before the program from three sources. First, we show that district-level trends in literacy between the 1991 and 2001 censuses are uncorrelated with TSC intensity in the period studied. Second, we use the India Human Development Survey to show that there was no difference in cohort-to-cohort literacy trends among children slightly too old to have been exposed to the program or among women who have children of the age studied in our sample. Third, we use data from the IHDS to show that there is no association between

ASER-like test scores and TSC trends for older children whose critical developmental periods were before the TSC.

6.1 Census literacy data, 1991-2001

Were trends in district-level literacy different before the TSC for districts that did and did not experience improvements in sanitation by 2003? If so, it may not be reasonable to assume that districts that were exposed to the TSC in the period we study would have evolved similarly to districts without the program, if the TSC had not happened. India conducts a census that includes literacy every ten years. In this section, we match district literacy rates to our administrative records on TSC implementation.

Figure 5 presents the result. Panel (a) plots the empirical cumulative distribution function of the percentage point increase in literacy from the 1991 census to the 2001 census, separately for districts that did and did not receive any TSC latrines by 2004. The two CDFs cross and touch at several points, making clear that there is no important difference between the distributions. Indeed, the average change in literacy between these two pre-program censuses is less than 0.1 percentage points smaller in the districts that received some TSC latrines by 2003, a gap with a t-statistic of 0.18.

Panel (b) shows that there is also no association between pre-program literacy trends and program intensity, measured as TSC latrines per capita reported built by 2003. If anything, a one standard deviation increase in TSC intensity is associated with a 0.2 percentage point smaller pre-program literacy trend, but again this is not statistically significant ($t = 0.97$).

6.2 Age-cohort trends in literacy, rural IHDS

A further test of parallel trends is to compare the age gradients of literacy, in districts that did and did not receive TSC latrines in the period we study. The IHDS was conducted in 2005 – after the 2001-2003 period of the TSC that we study. However, children who are six years old (the age we study in this paper) or older would have had their early life critical development before the TSC. Any difference in the age-cohort trends of these children would cast doubt on our identifying assumption of parallel trends, especially because very few latrines were built before 2003. Panel (a) of figure 6 shows that there is no statistically significant level difference in literacy between children living in these two sets of districts, and further that there is no apparent difference in age trends. In a linear regression, the interaction is not statistically significant ($t = 0.88$).

Another important predictor of children's educational achievement is their mothers' education. Because younger women are mothers of younger children, on average, differing age-cohort trends in mothers' literacy would be a threat to the validity of our strategy. Note that table 2 has already provided some evidence against this: applying this paper's difference-in-differences identification strategy, mothers' literacy and education are not associated with TSC implementation. Panel (b) of figure 6 confirms that trends in literacy are similar, focusing on adult woman aged 18 to 30 who are married mothers. As in the table of summary statistics from our main data sources, literacy is generally higher among *mothers* in TSC-exposed districts in panel (b) and lower among *children* in TSC-exposed districts in panel (a), but both of these level differences would be accounted for by district fixed effects.

6.3 Parallel trends in ASER-like tests in older children

As introduced in section 2, the IHDS conducted tests closely modeled on the ASER tests that we study. In 2005, they gave these tests to children aged 8 to 11. These children would have been several years too old to be exposed to the TSC during their early-life critical developmental period. However, if our results merely reflect district time trends unrelated to the TSC, then we might expect within-district cohort-to-cohort trends in test scores to be correlated with the unfolding of the TSC. In contrast, if we find that TSC implementation is uncorrelated with test score trends within this sample, then this would suggest that TSC implementation was not correlated with pre-existing trends in cognitive achievement.[12]

Table 4 reports results of regressions of the form:

$$\textit{test score}_{idt} = \beta TSC_{dt} + \alpha_t + \delta_d + \varepsilon_{idt}, \tag{3}$$

where i indexes individual children, d districts, and t age cohorts. Standard errors are clustered by district. Note that this data reflects a single cross section, so differences in birth cohorts are identified by comparing children of different ages, controlling for an age fixed effect. The IHDS tested children at four years of age (8 to 11), but our paper studies only TSC implementation in 2001, 2002, and 2003. Therefore, for robustness we use three different samples, matching birth cohorts to TSC years in three different ways:

- *8-10 sample*: children aged 8 in 2005 are assigned to 2003 TSC data; 9 to 2002; 10 to 2001.
- *9-11 sample*: children aged 9 in 2005 are assigned to 2003 TSC data; 10 to 2002; 11 to 2001.
- *8-11 sample*: children aged 8 in 2005 are assigned to 2004 TSC data (which is not otherwise used in our analysis); 9 to 2003; 10 to 2002; 11 to 2001.

Additionally, we replicate results with a simple indicator that in a particular district or year, the TSC had "started," by which we mean any latrines are reported built.

As table 4 shows, in no specification is the within-district trend in TSC implementation associated with the within-district cohort-to-cohort trend in test scores, in this sample of children who are too old to have been exposed to the TSC during early-life development. Indeed, only 2 of 12 point estimates are positive, and in only 4 of 12 cases does the t-statistic

[12] Although we are assuming that any effect of the TSC on cognitive scores would reflect early-life exposure (we explore this timing of the effect in detail in section 7) if we are incorrect, then any effect of later childhood exposure to the TSC would make these estimates more *positive*, biasing against our purpose in this section, which is to demonstrate a lack of association.

exceed 1 in absolute value. This sample is notably smaller than the main sample used in our analysis. However, the various negative coefficients on the TSC path in this sample suggest it is unlikely that TSC implementation was *positively* correlated with pre-program trends in cognitive achievement.

7 Mechanism check: Effect by timing of exposure

How plausible is it that the difference-in-differences effects we have estimated indeed reflect causal effects of sanitation on cognitive achievement? One further way to asses this is to analyze the apparent mechanisms of the effect, to assess whether they match what is known about the effects of early life health and disease on human capital from the literature. In this section, we examine the age at which exposure to poor sanitation shapes subsequent cognitive achievement. Section 4.5 reviewed evidence in the literature that health and net nutrition in the first year of life are particularly important for subsequent cognitive achievement. Therefore, we expect exposure to the TSC in a child's first year to have the steepest gradient with learning outcomes.

We investigate this in two ways, using the same ASER test score data and TSC administrative records as in our main analysis. First, we continue to focus only on six-year-old children, but use the same identification strategy to identify an "effect" of exposure to the TSC in other years of life. Because we have more years of TSC implementation data than of ASER test score data, we can displace the TSC exposure in time without reducing the sample. Second, we replicate our estimation strategy for children of different ages, holding constant the years of TSC implementation used as the independent variable; this has the result of varying the year of the child's life in which TSC exposure is being assessed.

Our finding that the sanitation-learning gradient is steepest for exposure in the first year of life has two implications, both of which support a causal interpretation of our result. First, and most importantly, they match the mechanism that we would expect, if the effect were through early life health.[13] Second, for a reader who was already convinced that the first year of life is a critical period for the effects of health an net nutrition on subsequent development, they make less likely that that our results reflect a spurious correlation of general sanitation trends with general educational trends: only a properly timed matching of cause and effect (in this interpretation) finds an effect on learning outcomes.

7.1 Effect in first year: Displacing TSC exposure for six-year-olds

In which year of life does latrine exposure have the steepest gradient with subsequent cognitive achievement at age six? Table 5 reports the results, studying the same six-year-olds as in our main analysis, but different years of TSC implementation. As expected, TSC exposure in the first year of life is most associated with subsequent cognitive achievement. Indeed, only exposure in this year is statistically significantly associated with outcomes. This is consistent with the mechanism that we hypothesize, and further suggests that our result may not be a spurious correlation of general trends.[14]

7.2 Effect in first year: No effect on older children in same data

Another approach to assessing the importance of the year of exposure is to hold constant the calendar years of TSC implementation that are used as the independent variable and examine the effect on children of different ages in the years 2007 through 2009.[15] Studying

[13] We note that other effects are possible, although outside the scope of this paper. Miguel and Kremer (2004) find an effect of deworming medicine on school attendance in Kenya, although not on test scores. Adukia (2013) shows that construction of latrines at schools (unlike the household latrines we study) promotes contemporaneous school attendance. (We reiterate from table 1 that we find no effect of early-life exposure to the TSC on school *enrollment*.) We believe that this paper's focus on early-life effects is justified by evidence of early-life critical periods, and by evidence that the TSC matters for infant mortality and child height (Spears, 2012a).

[14] Although we would not have been surprised to find an effect in the second year of life, as well, we are not concerned that we did not. First, our timing measures are noisy, as they are as coarse as calendar years, so estimates are attenuated and some children may even be matched with sanitation exposure more similar to their *third* year of life. Second, it is exactly from first year exposure that Spears (2012a) finds an effect on infant (first year) mortality and subsequent height.

[15] This is similar to the parallel trends test with the IHDS in section 6.3, but uses the same data sources as the main analysis and is more narrowly focused on isolating the early-life period.

older children has the consequence of studying the effect of the TSC later in life. 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 or 8 years old in 2009 on TSC coverage in 2003.

Table 6 presents the results, studying the same years of TSC implementation as our main analysis, but matched to older cohorts of children. Again, there is no consistent evidence of an effect on older children to TSC exposure in later years of life than the first.[16]

If education, test-taking, or ASER test scores were coincidentally improving in the districts that received TSC latrines first, then one might also expect test scores to be increasing in these 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 very much TSC sanitation in their first year of life, because the largest increase in TSC implementation during the period that we study was from 2002 to 2003. Most importantly, this test is consistent with the hypothesized mechanism of early life critical periods for development.

8 Adjusting for prevented mortality

As a final note, 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 by the TSC were below average in cognitive achievement then the estimated effect is lower than the true effect.

How large could this underestimate be? We use Spears's (2012a) main estimate of a reduction in infant mortality of 85 deaths per 1,000 live births associated with moving from 0 to 1 TSC latrines per capita to compute the expected probability that a child in a district-year would have died in the absence of the TSC, given his or her exposure to TSC in the

[16] 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.

first year of life. To put an upper bound on the effect of mortality selection, we make the extreme assumption that if a child in the sample from a district-year were prevented from dying because of the TSC, then if any child in that district-year's sample was unable to recognize letters, it would have been a child who was unable to recognize letters who died in the absence of the TSC. We re-estimate the effect of the TSC on reading scores from table 3, using 100 different samples, each excluding children who are randomly drawn to have died in the absence of the TSC. On average, we drop about 10 children from the sample in each repetition due to modeled "death."

Figure 7 presents the result. Even with the extreme assumption that the TSC always prevented the death of the sampled child with the lowest test score in a district-year, the effect on the coefficient estimate is small. The mean simulated effect is 0.805, or 107 percent of the full-sample estimate without adjusting for endogenous mortality.

9 Conclusion

We find an effect of exposure to 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 a well-identified effect of sanitation on early-life health and of the importance of early-life health for cognitive development.

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, several further tests are consistent with a causal effect of exposure to improved sanitation in the first year of life on subsequent cognitive achievement.

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 (2012) 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 skills, given the context of widespread open defecation. The program we study was imperfect: much open defecation remains even after the end of a decade of the TSC. However, open defecation may be so harmful for early-life physical and cognitive development that even an imperfect and incomplete improvement in sanitation can have important effects.

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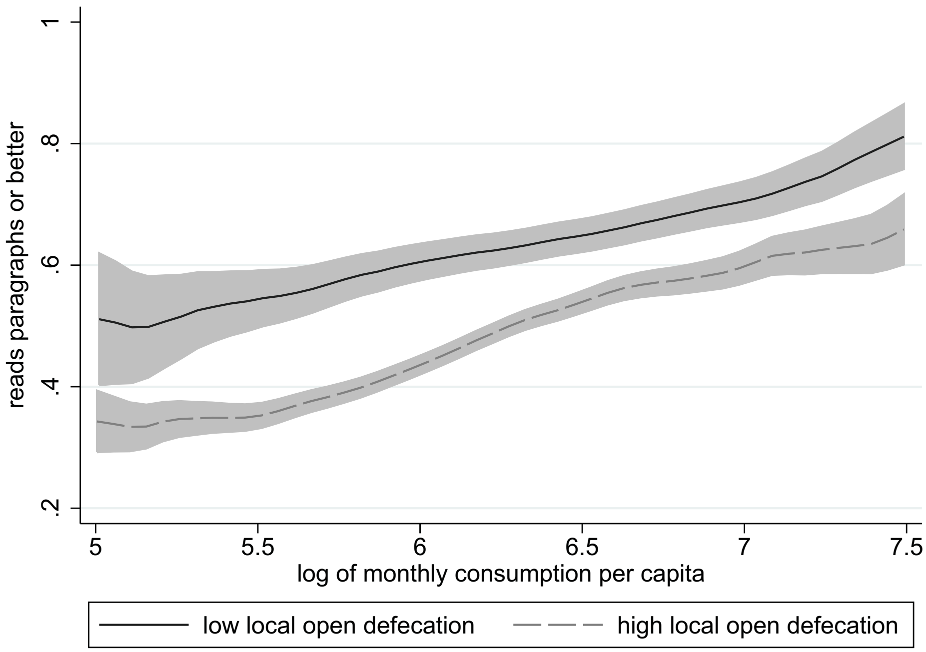
(a) reading, sanitation, and consumption, without controls



(b) reading, sanitation, and consumption, with controls

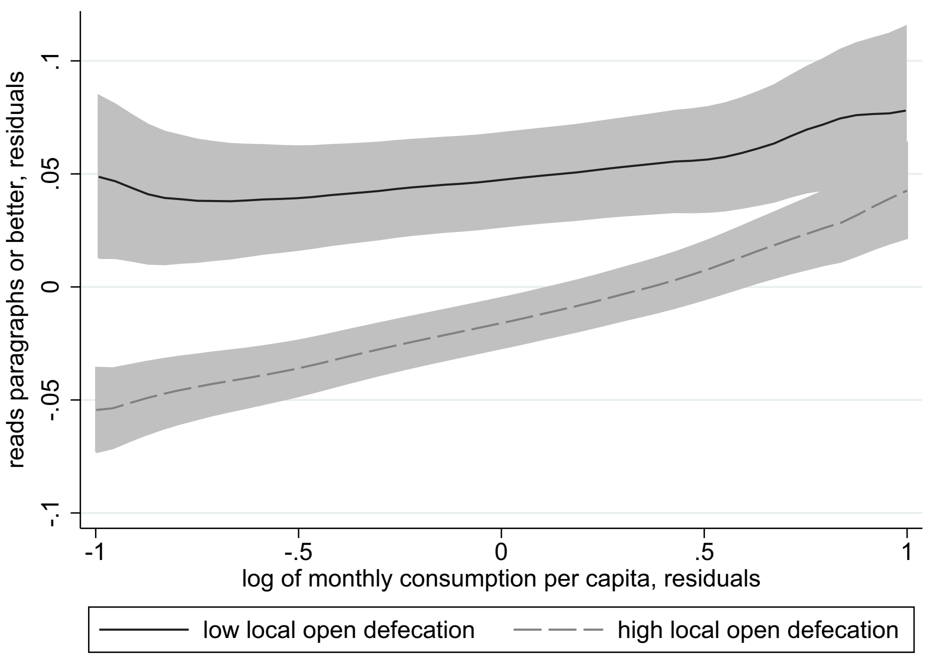


Figure 1: Local open defecation predicts reading conditional on SES, rural IHDS

Non-parametric local regressions with 95% confidence intervals. “Low” and “high” open defecation are above and below 50% of households in a child’s survey PSU. Household consumption per capita is per month, in rupees. The IHDS gave tests modeled after ASER tests to children aged 8-11. 50% of rural children can read paragraphs or stories; the rest can read letters or words, or cannot read. Residuals are after controlling for age times sex indicators; 15 indicators for highest female education in the household; 15 indicators for highest overall adult education in the household; 13 indicators for the language in which the test was taken; a set of indicators that the child is in school, has been in school, or has never gone to school; and the asset count reported by the IHDS.

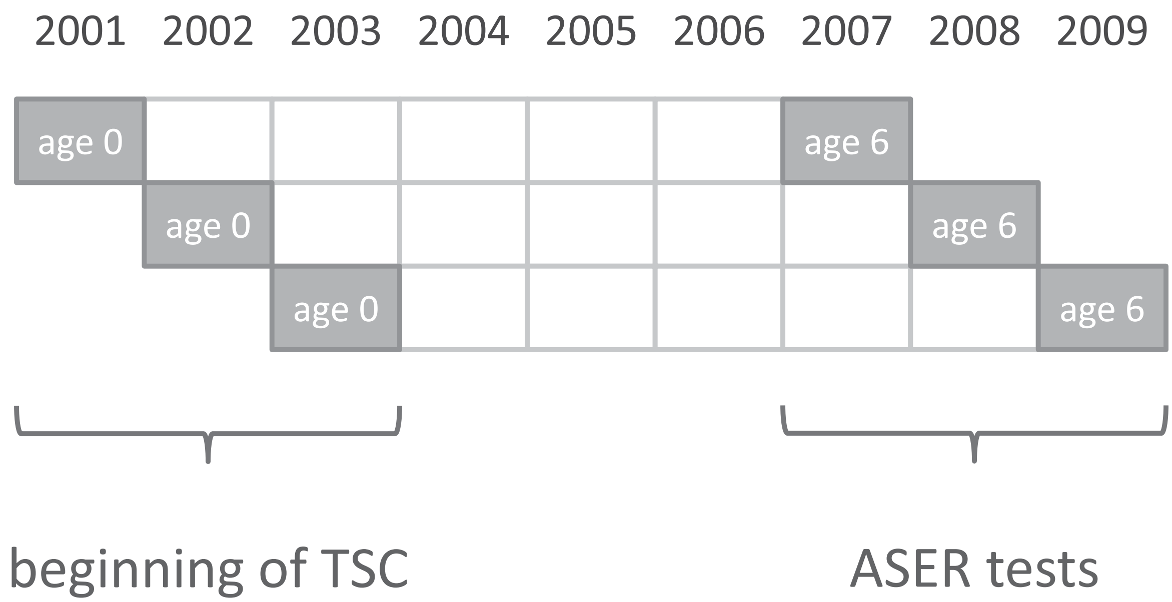


Figure 2: Empirical strategy: 2001-2003 TSC intensity matched to six-year-olds' 2007-2009 tests

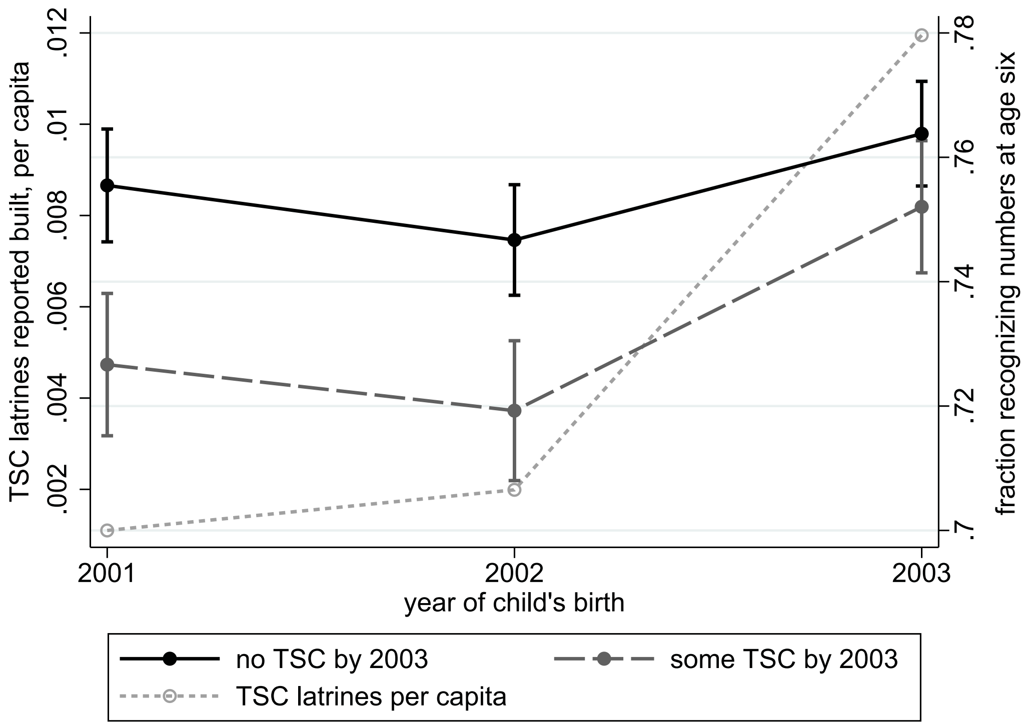


Figure 3: Difference-in-differences: Learning trends and TSC intensity

Birth years are listed on the horizontal axis; tested years are six years later. The sample is split at the district level into those which received any reported TSC latrines within 2001, 2002, or 2003 and those which did not. Error bars report 95% confidence intervals. TSC latrines per capita reports the average number of latrines per capita built in districts where any latrines were built by 2003, in order to show the timing of TSC scale-up.

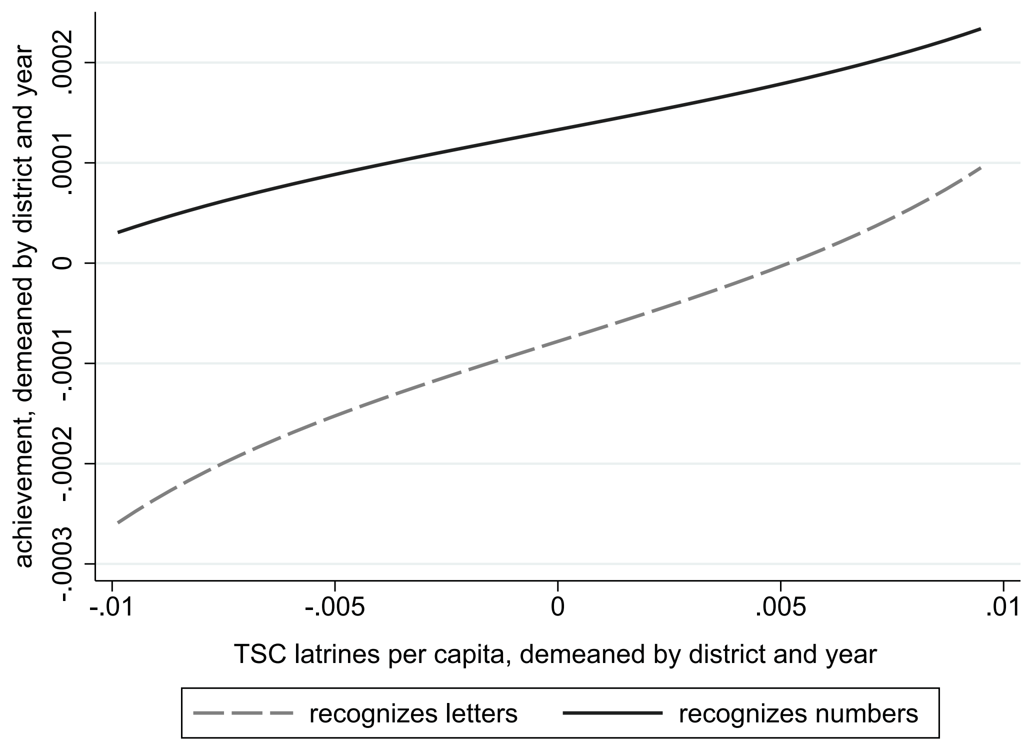
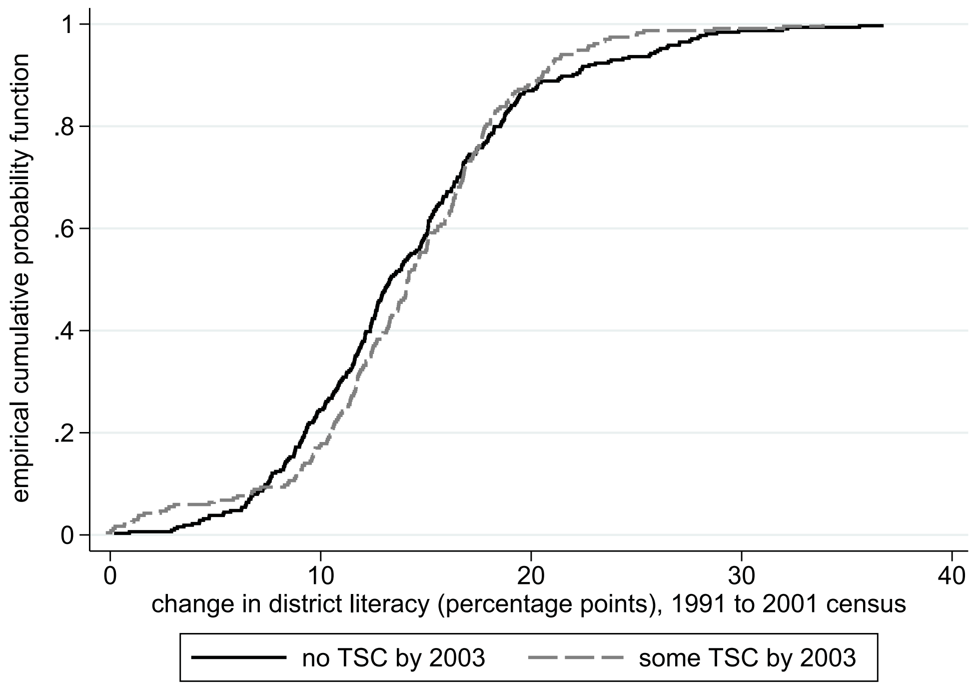


Figure 4: Cognitive achievement and TSC intensity, non-parametric local regressions
The independent and dependent variables have both been demeaned twice, first by district and then by year, to graph the equivalent of fixed effects regressions. Lines are moved apart vertically for visibility.

(a) CDF of pre-program change in literacy, by whether TSC started by 2003



(b) pre-program change in literacy and TSC latrines per capita in 2003

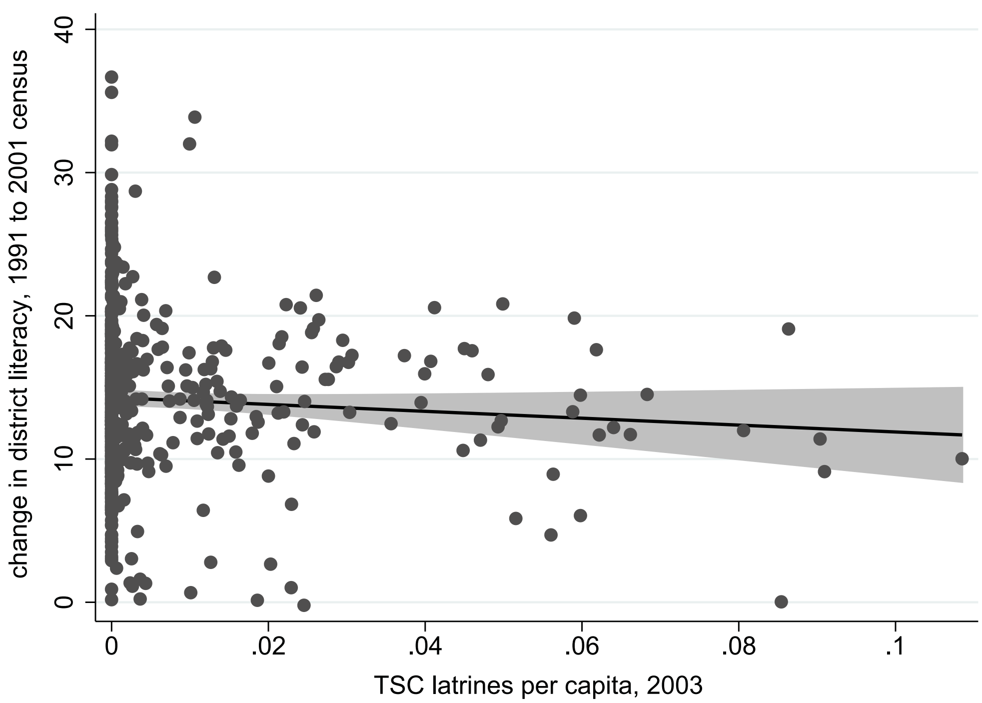


Figure 5: TSC implementation uncorrelated with pre-program literacy trends, 1991 & 2001 census

Observations are Indian districts, to match the district-level variation in the TSC. Panel (b) uses the restricted sample that omits two districts with implausibly high reported TSC intensity, to avoid stretching the horizontal axis.

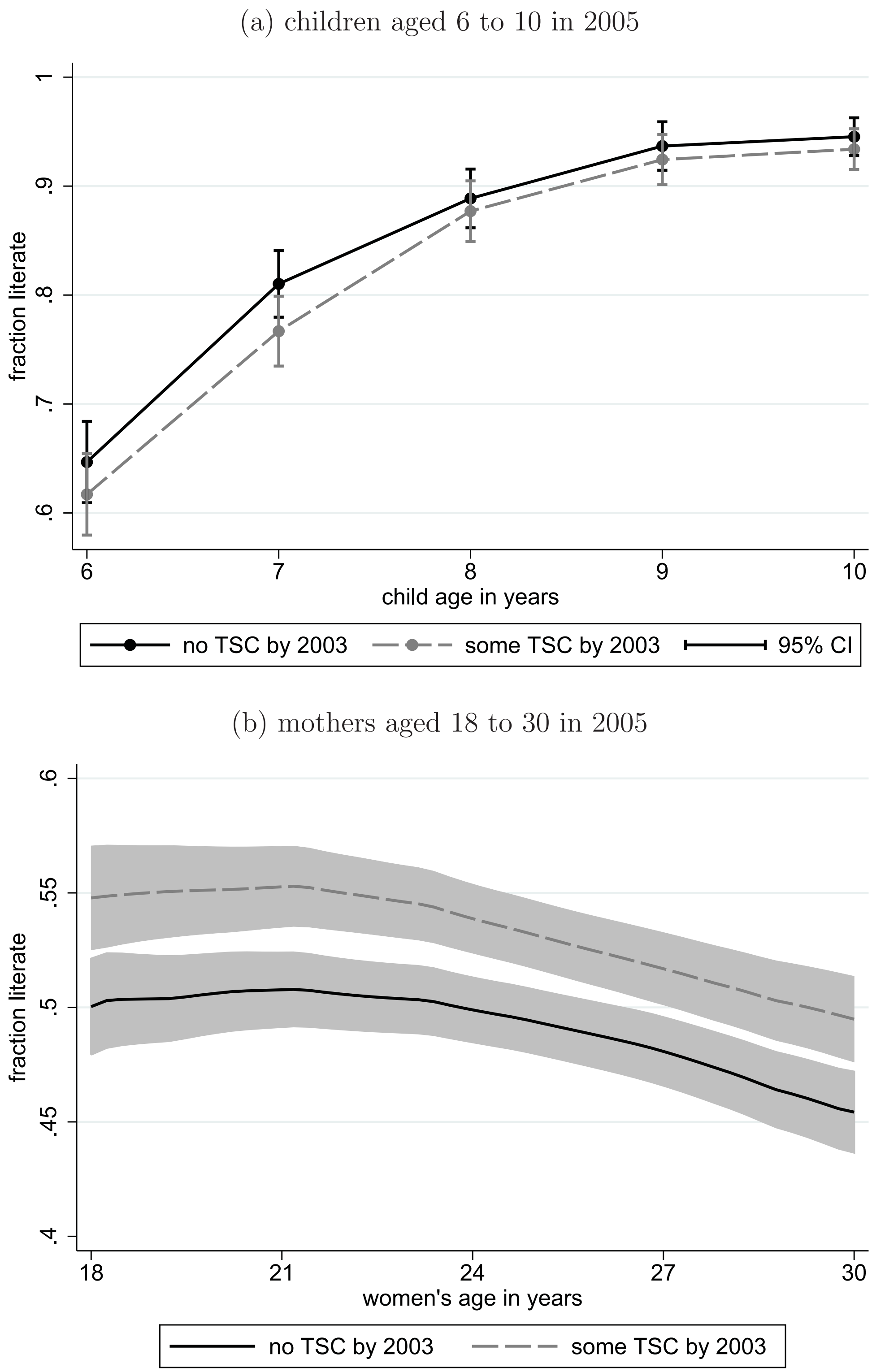


Figure 6: Age cohort literacy trends by TSC exposure, rural IHDS

Observations are individual rural children or adult women, matched to whether their district of residence was reported to receive any TSC latrines by 2003. Panel (b) limits the "individual" sample of the IHDS to married rural mothers. The shaded areas in panel (b) are 95% confidence intervals.

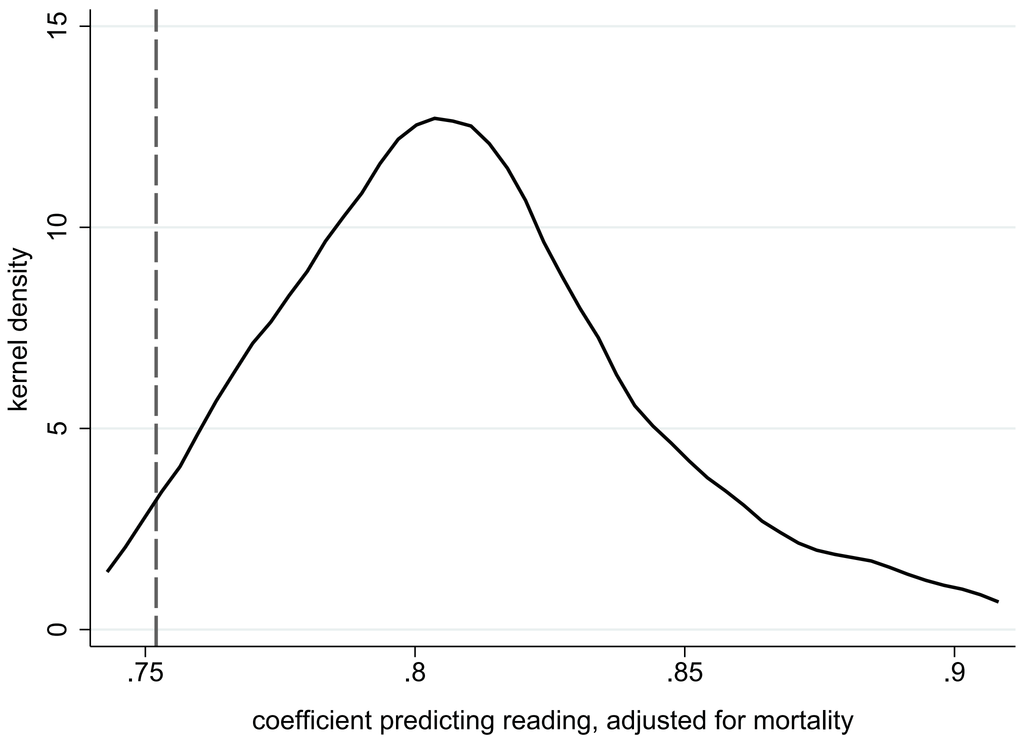


Figure 7: Simulated distribution of effect on reading, adjusting for mortality
The figure plots a kernal density of coefficient estimates from 100 simulations of estimating the baseline specification effect of the TSC on recognizing letters, dropping from the sample low-performing children who are modeled to have died in the absence of the TSC. The vertical, dashed line is the full-sample estimate from column 1 of panel A of table 3.

Table 1: Summary statistics

	full sample	restricted sample	never TSC	some TSC
TSC government administrative data:				
latrines per capita by 2001	0.00044	0.00043	0.00000	0.00110
	(0.00009)	(0.00009)		(0.00022)
latrines per capita by 2002	0.00079	0.00078	0.00000	0.00199
	(0.00014)	(0.00014)		(0.00034)
latrines per capita 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)
ASER survey data:				
female	0.444	0.444	0.441	0.448
	(0.0027)	(0.0027)	(0.0035)	(0.0043)
formal 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: Balance of covariates: Observed properties uncorrelated with program timing

independent variable:	TSC latrines per capita		TSC started	
mother literate	-0.46	(0.38)	-0.01	(0.01)
mother went to school	-0.30	(0.44)	0.00	(0.01)
father went to school	0.12	(0.43)	0.00	(0.01)
child receives extra tuition	0.26	(0.46)	0.00	(0.01)
school enrollment	-0.24	(0.32)	0.00	(0.00)
formal house	-0.45	(0.50)	-0.01	(0.01)
electrification	0.20	(0.50)	0.00	(0.01)
village has road	0.18	(0.61)	-0.03†	(0.02)
village has ASHA	0.18	(0.64)	-0.01	(0.02)
village has school	-0.12	(0.61)	0.00	(0.01)
village has ration shop	-0.25	(0.69)	-0.02	(0.02)

Observations are the same sample of six-year-olds used in our main analysis. Each estimate presents a coefficient and a clustered standard error in parentheses from a separate regression (of the form of equation 1) of the variables listed as the dependent variable, on either latrines per capita or an indicator for a positive number of latrines per capita ("TSC started"), with district and year fixed effects. Two sided p-value: † $p < 0.1$.

Table 3: Main result: Difference-in-differences in TSC intensity predicts ASER test scores

	(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: † $p < 0.10$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$. All specifications include district and year fixed effects. Scaled effects below the TSC coefficient multiply by the mean program intensity. Household and parent controls are 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. Village controls are indicators for the village having electricity, a road, a health worker, a school, and a government ration shop; district controls are a quadratic polynomial of interpolated census literacy.

Table 4: Parallel trends: displaced TSC intensity uncorrelated with test scores in older children, IHDS

	(1)	(2)	(3)	(4)	(5)	(6)
sample:	8-10 year olds		9-11 year olds		8-11 year olds	
Panel A: Reading score						
TSC latrines per capita	2.418		-1.619		-0.523	
	(2.323)		(2.088)		(1.455)	
TSC started		-0.0270		-0.0161		-0.00395
		(0.0797)		(0.0942)		(0.0710)
n (children)	6,580	6,580	6,169	6,169	8,213	8,213
Panel B: Math score						
TSC latrines per capita	1.590		-2.297		-0.470	
	(1.685)		(2.178)		(1.422)	
TSC started		-0.0631		-0.0436		-0.0217
		(0.0577)		(0.0645)		(0.0469)
n (children)	6,566	6,566	6,149	6,149	8,189	8,189

Standard errors clustered by survey PSU in parentheses. No coefficient is statistically significantly different from 0 at a two-sized 0.10 level. Only the rural subsample of the IHDS is used. Each regression includes fixed effects for districts and for age, and an indicator for being female. "TSC started" is an indicator that in that district, by that year, a positive number of TSC latrines had been built. TSC implementation is displaced in various samples (see section 6.3) in order to demonstrate that the within-district program trend is uncorrelated with the trend in these children's test scores.

Table 5: Mechanism: Sanitation-cognition gradient is steepest for early-life exposure

	(1)	(2)	(3)	(4)	(5)	(6)
	effect of TSC latrines in child's 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)	47,684	47,684	47,684	47,684	47,684	47,684
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)	47,133	47,133	47,133	47,133	47,133	47,133
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)	47,133	47,133	47,133	47,133	47,133	47,133

Standard errors clustered by districts in parentheses. Two-sided p-values: † $p < 0.10$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$. The sample is the same set of six-year-olds used in table 3. Within a panel, each column reports a separate regression where the sample and dependent variables are held constant and the TSC administrative data for the dependent variable is moved one year later. Each regression includes district and year fixed effects.

Table 6: Mechanism: Sanitation-cognition gradient is not found for older children

	(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)	47,684	44,056	59,476
Panel B: Math (numbers 1-9)			
TSC latrines	0.754*	-0.356	-0.121
per capita	(0.359)	(0.270)	(0.186)
n (children)	47,133	43,688	59,019
Panel C: Math (numbers 10-99)			
TSC latrines	0.897*	0.387	-0.188
per capita	(0.407)	(0.420)	(0.406)
n (children)	47,133	43,688	59,019

Standard errors clustered by districts in parentheses. Two-sided p-values: † $p < 0.10$; * $p < 0.05$. The ASER survey rounds are the same from 2007 to 2009 as used in table 3, so in column 1, the sample is the same set of six-year-olds used there. Within a panel, each column reports a separate regression where the TSC administrative data from 2001 to 2003 is held constant, while the age cohorts from whose 2007 to 2009 ASER test score is taken (for the dependent variable) change; thus column 1 uses the same sample as tables 3 and 5, but columns 2 and 3 do not. Each regression includes district and year fixed effects.