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# DOES ACCESS TO IMPROVED SANITATION REDUCE CHILDHOOD DIARRHEA IN RURAL INDIA

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## ABSTRACT

Almost nine million children under 5 years of age die every year. Diarrhea is considered to be the second leading cause of under-five mortality in developing countries. About one out of five deaths is caused by diarrhea. In this paper, we use the newly available data set District Level Household Survey 3 to quantify the impact of access to improved sanitation on diarrheal morbidity for children less than 5 years of age in India. Using propensity score matching, we find that access to improved sanitation reduces the risk of contracting diarrhea by 2.2 percentage points. There is considerable heterogeneity in the impacts of improved sanitation. We find statistically insignificant treatment effects for children in low or middle socioeconomic status households and for girls; however, boys and children in high socioeconomic status households experienced economically significant treatment effects. The magnitude of the treatment effect differs largely by hygiene behavior. Copyright © 2012 John Wiley & Sons, Ltd.

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**KEY WORDS:** sanitation; diarrhea; propensity score matching; infrastructure; India

## 1. INTRODUCTION

The United Nations Millennium Development Goal 4 (MDG 4) calls for a reduction of “under-five mortality by two-third” by 2015 (United Nations, 2005). However, the level of child mortality remains high in many low and middle income countries. A recent report, Countdown to 2015 decade report, suggests that only 19 of the 68 *countdown* countries are on track to achieve MDG 4 (Bhutta *et al.*, 2010). Globally, nine million children under the age of five die every year. About one out of five of these deaths is caused by diarrhea (Bryce *et al.*, 2005). Diarrheal disease, the second leading cause of child mortality after pneumonia, kills approximately two million children every year. The number of diarrheal deaths is higher than the number of deaths caused by AIDS, malaria and measles combined (UNICEF/WHO, 2009). The global health burden of diarrheal disease is tremendous and falls disproportionately on Africa and South Asia, with more than 80% of diarrheal deaths occurring in these two regions. Within South Asia, India has the greatest burden of diarrhea. In India alone, which is the focus of this paper, about 0.4 million children die annually because of diarrhea.

Diarrheal disease is often described as water-related, but more accurately, it is an excreta-related disease because the pathogens are derived from fecal matter (UN factsheet, 2008). The principal route of diarrheal disease infection is fecal–oral cycle, and breaking this cycle, which depends primarily upon hand-washing and

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toilet use, saves children's lives. Hygiene and sanitation are considered as the most cost-effective public health interventions to reduce diarrheal morbidity and mortality.

In developed countries, the provision of piped water and improved sanitation facilities brought substantial health improvements (Cutler and Miller, 2005; Watson, 2006). However, similar evidence for developing countries is lacking, and a key question in policy circle is whether investing in the environment health sector alone will be sufficient to reduce the diarrheal disease burden. There is a lack of consensus on the effectiveness of different water, sanitation, and hygiene interventions. On one hand, children in developing countries face very high risk of mortality and morbidity from diarrheal diseases. On the other hand, these countries have also made considerable progress in extending sanitation coverage. In this context, it is an important policy question to examine whether access to improved sanitation is effective in reducing diarrheal morbidity, especially in a large developing country like India.<sup>1</sup>

Quantifying the impact of access to improved sanitation on children's morbidity is important for policy purpose for at least two reasons. First, it can serve as a guide for the allocation of scarce resources to the numerous other interventions competing for the same funds. Second, it will also help us to understand the relative importance of various factors that permit certain households in a given socioeconomic environment to achieve greater benefits from access to improved sanitation than others.

India is an ideal country to examine the aforementioned question, because it has made substantial progress in water and sanitation coverage in the last decade. The sanitation coverage in India has almost tripled from 22% in 2001 to 57% in 2008 (Figure 1). In this paper, we take advantage of a newly available nationally representative individual-level data set, District Level Household Survey 3 (DLHS-3), to rigorously quantify the effect of improved sanitation on diarrhea incidence among young children in India.<sup>2</sup>

A major challenge in program evaluation is the non-random placement of the programs, which leads to selection bias. For example, we do not know why only some households in a village have access to improved sanitation. It might be the case that households with access to improved sanitation are also more forward-looking and possess better health knowledge and behavior. Thus, the unobserved characteristics of households may be responsible for differences in outcomes across households and not the actual treatment. The best method to address this concern is to randomly assign the treatment and adopt an experimental design. Because it is nearly impossible to randomize infrastructure projects such as sanitation, we thus rely on non-experimental method, propensity score matching (PSM), to evaluate the effect of improved sanitation on diarrheal incidence.<sup>3</sup> We exploit the richness of the DLHS data to create a reasonable counterfactual group based on the propensity score and address the issue of observed selection bias.<sup>4</sup> We further check the robustness and sensitivity of our PSM results by estimating a weighted least square (WLS) with propensity score as the weights (Hirano *et al.*, 2003), and employing the bounding approach as suggested by Rosenbaum (2002).

Our main finding is that children in households with access to improved sanitation have a lower diarrheal incidence than children in households without. The incidence of diarrhea for children living in a household with *improved sanitation* is 2.2 percentage points lower than that for children living in a household without improved sanitation. Put differently, the odds of contracting diarrhea, for children in households without improved sanitation, is 24% higher than that for children in households with improved sanitation.

The treatment has heterogeneous effects. There is a very steep gradient by socioeconomic status (SES). We find a statistically significant treatment effect of 2.5 percentage points in the highest SES group, whereas the effect in the middle and low SES groups is statistically insignificant. We find a similar result when we stratify by using a

<sup>1</sup>According to the Joint Monitoring Programme for Water Supply and Sanitation by the World Health Organization and UNICEF, 'improved' sanitation includes connection to a public sewer, connection to a septic system, pour-flush latrine, access to a pit latrine, and ventilated improved pit latrine.

<sup>2</sup>DLHS has three rounds and was conducted in 1998–1999, 2002–2004, 2007–2008. See data section for more details.

<sup>3</sup>See Ravallion (2001) for methods employed in program evaluation.

<sup>4</sup>PSM assumes that there is no unobserved selection bias.



Source: Govt. of India, Dept. of Drinking Water Supply <http://ddws.nic.in> Accessed 16 Oct-00

Figure 1. Progress in coverage of rural sanitation in India

variable that can be considered a proxy for health and hygiene behavior *whether or not a household treats water before drinking it*. We find a statistically significant treatment effect of 3.3 percentage points for households that treat their water before drinking, whereas the effect is insignificant for households that do not treat their water. We also find that the treatment effect is only significant for boys and not for girls.

Our results survive a variety of robustness checks, including different implementation and estimation of propensity score. Additionally, we employ the bounding approach proposed by Rosenbaum (2002) to determine how strongly the unobservables must influence to make the estimated treatment effects null and void. Our PSM results remain significant even when there is substantial unobserved selection bias.

The remaining sections of the paper are organized as follows. Section 2 provides an overview of sanitation programs in India and related literature. Section 3 presents the empirical framework. Section 4 describes the data. Section 5 presents our findings. Finally, Section 6 discusses our main results and provides some concluding remarks.

## 2. CONTEXT AND PREVIOUS LITERATURE

### 2.1. Total sanitation campaign

In 1986, the Indian Ministry of Rural Development launched the first nationwide program of sanitation, the Central Rural Sanitation Programme (CRSP). CRSP was supply-driven, highly subsidized, and gave emphasis on toilet construction. The program failed to motivate and sustain high levels of sanitation coverage as there was no perceived need for sanitation among communities. It was based on the erroneous assumption that provision of sanitary facilities would lead to increased coverage and usage. Despite an investment of more than \$134 million and construction of over nine million latrines in rural areas, rural sanitation grew at just 1% annually throughout the 1990s, and the census of 2001 found that only 22% of rural households had access to a toilet (India Country Paper, 2008).

Recognizing this limitation, CRSP was restructured and renamed Total Sanitation Campaign (TSC) in 1999. The focus shifted from infrastructure to behavioral change. TSC is a demand-driven low-cost approach, advocating a shift from a high subsidy to a low subsidy regime and greater community involvement. TSC puts more emphasis on information, education, and communication (IEC), capacity building and hygiene education.<sup>5</sup> TSC places great

<sup>5</sup>One of the main objectives of TSC is to eliminate open defecation to minimize risk of contamination of drinking water sources and food. The major components of TSC are start-up activities, IEC activities, Rural Sanitary Marts and Production Centers (RSM), Individual Household Latrines, Community Sanitary Complex, School Sanitation, and Anganwadi Sanitation.

emphasis on awareness creation and demand generation for sanitary facilities in houses, schools, and in the village community environment. It is not restricted to the construction of toilet facilities.

Total Sanitation Campaign is being implemented in 590 districts of 30 states in India. As of October 2008, 57 million toilets have been constructed. In addition, 0.68 million school toilets, 14,540 sanitary complexes for women, and 222,267 anganwadi (pre-school) toilets have been constructed. Rural sanitation coverage has almost tripled from 22% in 2001 to 57% in 2008 (Figure 1). The Government of India has allocated about \$4 billion for TSC, and as of 2009, an expenditure of \$1.6 billion has been incurred. That is an average spending of \$25 per household. Since the sanitation program was launched nationwide in one go, the program did not generate a counterfactual group either at district or village level. Therefore, following Jalan and Ravallion (2003), we take advantage of household data on sanitation and perform matching at household level rather than at village level to estimate the impact of sanitation on diarrheal morbidity (see Section 5 for details on matching variables).

## 2.2. Related literature

The article by Jalan and Ravallion (2003) is one of the first papers that evaluated the impact of environmental factors, access to piped water in particular, on diarrheal morbidity in rural India. Using a household survey conducted by the National Council of Applied Economic Research (NCAER, New Delhi, India) in 1993–1994, they measured the health gains to children living in households with access to piped water in rural India. By implementing propensity score matching at the household level, they found a lower incidence and duration of diarrhea for children living in households with access to piped water.<sup>6</sup> Interestingly, the health benefits of piped water bypassed poor households and households in which mothers are poorly educated.

Khanna (2008) extends Jalan and Ravallion's work by including access to sanitation as the explanatory variable, in addition to access to piped water. Unlike Jalan and Ravallion, Khanna makes a distinction between type of sanitation and water infrastructure. She uses data from India's second National Family Health Survey (NFHS), conducted in 1998–1999. Employing PSM, the author finds an increase in diarrheal incidence in households with piped water, which is contrary to findings in [Jalan and Ravallion \(2003\)](#). While estimating the joint impact of access to water and sanitation, the study finds a decrease in diarrhea incidence for children living in households with access to well water, hand pumped water, well water and sanitation, and hand pump and sanitation.

Our study builds on these two studies. Given that national sanitation programs and investments in sanitation are increasing, it is important to quantify the effect of improved sanitation on child morbidity. We are not aware of any studies that rigorously estimate the effect of improved sanitation on diarrheal morbidity for the period that succeeds the launch of the Total Sanitation Campaign (TSC) in India. Both Khanna (2008) and [Jalan and Ravallion \(2003\)](#) use data from the pre-TSC period and do not cover the effects of recent sanitation improvements caused by TSC. Until 2009, the Government of India has spent \$700 million on TSC and has provided access to improved sanitation to five million households. Our study is the first study to estimate the impact of improved sanitation on diarrhea with a very recent data set that adequately covers the TSC period, a period that coincides with huge improvement in sanitation access in India. Another advantage of our study is that we use a large, nationally representative, and recent data set<sup>7</sup> that enables us to estimate the impact of TSC more precisely.

In addition to the aforementioned studies, there are a few more studies on the impact of access to water and sanitation on diarrheal morbidity. However, most of these studies are based on small and unrepresentative

<sup>6</sup>However, matching at village-level does not indicate lower diarrhea incidence in households with piped water.

<sup>7</sup>The DLHS-3 was conducted in 2007–2008 and covers all the districts of India with a sample of about 720,000 households.

samples. For example, Dasgupta (2004) collected her own data from 600 households in 14 localities<sup>8</sup> in New Delhi, India. The study finds that children who live in households with access to piped water are less vulnerable to diarrheal attack, but surprisingly, the study finds no effect of sanitation and the education of the household head on diarrhea incidence. Duraiswamy (2001) uses data from the India Human Development Survey (IHDS) conducted in 1993–94 by the National Council of Applied Economic Research (NCAER), New Delhi, to examine the correlates of children's vulnerability to diseases<sup>9</sup> and finds no significant effect of availability of toilet, hand-washing behavior, and sources of drinking water on children's morbidity. Interestingly, the availability of a separate kitchen turned out to be a significant correlate of morbidity among children. Using data from IHDS, Borooah (2004) finds that the safety of a village's water supply reduces the incidence of diarrhea by 5%. A lack of toilet facilities in the house increases the probability of diarrhea. The author shows a correlation between diarrhea incidence and hand washing habits of the mothers before feeding their children.<sup>10</sup> Fan and Mahal (2011) apply matching methods to estimate the effects of water supply, toilets and hand washing on child diarrhea in rural India. They find no significant and robust effect of improved sanitation on diarrheal morbidity.

It should be noted that Jalan and Ravallion (2003), Duraiswamy (2001), Borooah (2004), and Fan and Mahal (2011) all use the same data set (the IHDS collected in 1993–1994)- but implement different methods. The adoption of improved sanitation and water facility is not exogenous, thus the results in Duraiswamy (2001) and Borooah (2004) cannot be interpreted as causal. Jalan and Ravallion (2003) and Fan and Mahal (2011), on the other hand, correct for selection bias by employing propensity score matching, which can thus be interpreted as causal.<sup>11</sup> Our paper is an improvement over these existing studies in the sense that the sample size in this paper is at least 15 times bigger than the IHDS sample (the IHDS has sample of about 25,000 children), and that the data are more recent.

Bose (2009) analyzes the Demographic and Health Survey (DHS) data for Nepal with propensity score matching and finds a 5% reduction in diarrhea incidence for children living in households with improved sanitation. The effect is larger, about 11%, for children below 24 months of age. Besides the impact of water and sanitation on children's morbidity, some studies have also looked at other health outcomes, such as infant and child mortality, height-for-age, weight-for-age or height-for-weight (Fink *et al.*, 2011). Fink *et al.* merged 171 DHS surveys in 70 low and middle income countries over the period 1986–2007 to estimate the effect of water and sanitation on child development. Using a logit model they find that access to improved water and sanitation infrastructure at the household level results in reduction in infant mortality, diarrhea incidence, and stunting among children in low and middle income countries.

Furthermore, there is a growing impact evaluation literature that examines the effects of improved water, sanitation, and hygiene (WSH) in other developing countries. In recent years, a number of surveys have been published examining the impact of WSH interventions on diarrheal morbidity, using systematic literature reviews, meta-analysis, and/or meta-evaluation (Esrey *et al.*, 1991; Curtis and Cairncross, 2003; Fewtrell and Colford, 2004; Fewtrell *et al.*, 2005; Arnold and Colford, 2007; Snilstveit and Waddington, 2009).<sup>12</sup> These studies provide overwhelming evidence on the positive impact of hand washing, sanitation, and household and point-of-use water treatment on better health outcomes.

<sup>8</sup>These 14 localities were selected based on occurrence of five or more cases of cholera in the locality in the past 3 years before the survey, that is, 1996–1998. The occurrence of more than five cases of cholera in a locality is taken as a standard benchmark for determining the vulnerability of an area to waterborne diseases by epidemiologists at the municipal health department. The survey was conducted during the summer months of 1999.

<sup>9</sup>Diarrhea was the third most common cause of morbidity among children after cold and cough.

<sup>10</sup>In developing countries, feeding is mostly done by hand and not using cutlery, and India is no exception.

<sup>11</sup>Under the assumption that there is no unobserved selection bias.

<sup>12</sup>See Fewtrell *et al.*, 2009 for detailed discussion.



### 3. EMPIRICAL FRAMEWORK

The objective of this paper is to estimate the causal impact of 'improved sanitation infrastructure' on child morbidity, indicated by diarrhea rates. Estimating the impact of sanitation access is a major methodological challenge because we cannot observe outcomes for the same individuals in both states: treatment and counterfactual state (Heckman and Robb, 1985). For example, in this study, we can observe households with either access to improved sanitation or without, but we cannot observe outcomes for the same households in both states. The most convincing approach to solve this problem of missing data is to conduct a randomized experiment, where the counterfactual is created from a random subset of the eligible population. However, randomizing infrastructure such as roads, ports, electricity, water and sanitation is not feasible for many reasons.

Therefore, in the absence of experimental data, we rely on observational data and implement a non-experimental method, PSM, to estimate the causal impact of improved sanitation on child morbidity. The estimation of the treatment effect in observational studies may be biased owing to confounding factors because subjects are assigned to the treatment and control groups non-randomly. PSM is an alternative to correct the bias by creating treated and control groups that are not confounded by differences in observed covariate distributions (Rosenbaum and Rubin, 1983). In recent years, matching methods have become increasingly popular and widely used in the evaluation of economic policy interventions (Becker and Ichino, 2002).

The basic idea in PSM is to generate treatment and control groups that have similar characteristics such that comparisons can be made within these matched groups. In the event of a large number of observed characteristics, direct matching becomes infeasible and PSM (a single-index variable) is used. The propensity score  $p(X)$  is the estimated probability of receiving treatment given a set of background covariates. The difference in the average outcome of treated and control groups can be attributed to the program under the assumption that selection into program participation is based on observable factors alone.<sup>13</sup>

#### 3.1. Average treatment effect on the treated

Let  $Y_{1i}$  and  $Y_{0i}$  be the outcome variables for treated and control households, respectively, and  $D \in \{0, 1\}$  the indicator of treatment status. The propensity score  $p(X)$  is defined by Rosenbaum and Rubin (1983) as the conditional probability of receiving treatment given observed characteristics:

$$p(X) \equiv \Pr(D = 1|X) = E(D|X) \quad (1)$$

where  $X$  is the multidimensional vector of observed characteristics.

Given the propensity score  $p(X)$ , the average effect of treatment on the treated (ATT) can be estimated as follows:

$$\begin{aligned} \widehat{ATT} &\equiv E\{Y_{1i} - Y_{0i}|D_i = 1\} \\ &= E[E\{Y_{1i} - Y_{0i}|D_i = 1, p(X_i)\}] \\ &= E[E\{Y_{1i}|D_i = 1, p(X_i)\} - E\{Y_{0i}|D_i = 0, p(X_i)\}|D_i = 1] \end{aligned} \quad (2)$$

Equation (2) gives the average program impact under the conditional independence (CIA)<sup>14</sup> and overlap assumption.<sup>15</sup>

<sup>13</sup>See Dehijia and Wahba (1999), Heckman *et al.* (1997), Smith and Todd (2005) for an evaluation of matching estimators.

<sup>14</sup>Conditional independence means that conditional on  $X$ , the outcomes are independent of treatment, and can be written as  $Y_1, Y_0 \perp D|X$ .

<sup>15</sup>Overlap means that for each  $X$ , there are both treated and control units, that is,  $0 < \Pr[D = 1|X] < 1$ .

### 3.2. Nearest-neighbor matching method

In this paper, we employ nearest-neighbor (NN-1) matching with replacement, which is the most widely used matching algorithm.<sup>16</sup> We matched the treatment household with the nearest neighbor. Formally, the NN matching estimator with replacement within caliper is,

$$\widehat{ATT} = \frac{1}{N1} \sum_{i=1} \{Y_i - Y_j\} \quad (3)$$

For a pre-specified caliper  $\delta > 0$ ,  $j$  is chosen such that,

$$\delta > |p(X_i) - p(X_j)| = \min_{k \in I} \{|p(X_i) - p(X_j)|\}$$

If none of the non-treated units is within  $\delta$  from the treated unit  $i$ , then  $i$  is left unmatched. We use the nearest-neighbor observation within the radius of 0.01 to construct the counterfactual for each treated observation.

### 3.3. Propensity-based weighted regression

Another method widely used in program evaluation literature is estimation of a multivariate regression model, using the propensity score as sampling weight. Several studies suggest that weighting the data with the propensity score balances the distribution of covariates and results in fully efficient estimates (Rosenbaum, 1987; Hirano and Imbens, 2001; Hirano *et al.*, 2003). This approach uses the propensity score ( $\hat{\lambda}$ ) to weight treatment and control groups in order to make the covariate distribution similar across both groups. The weight is defined as the inverse of the propensity score  $1/\hat{\lambda}$  for treated households and the inverse of one minus the propensity score  $1/(1-\hat{\lambda})$  for untreated households.<sup>17</sup> For comparison and robustness, we implement this approach by estimating the following multivariate regression with propensity score as weights:

$$Y_{ijs} = \beta_0 + \beta_1 \text{Sanitation}_{js} + \delta X_{js} + \gamma_s + \epsilon_{ijs} \quad (4)$$

where  $Y_{ijs}$  is the outcome for child  $i$  in household  $j$  in state  $s$ .  $\text{Sanitation}_{js}$  is the access to improved sanitation, and the equation is estimated using the weight  $\hat{\lambda}$ .<sup>18</sup>  $X_{js}$  includes household and child-level characteristics, and  $\gamma_s$  indicates state fixed effect.

## 4. DATA

We use data from the third wave of the DLHS-3, which is a health survey covering family planning, maternal and child health, reproductive health of ever-married women and adolescent girls, and use of maternal and child health-care services at the district level for all states in India. DLHS-3 was implemented during 2007–2008 in all districts of India, interviewing about 643,944 women between 15–49 years of age, from 611 districts in 34 states. Every woman is asked about her fertility history in the last 5 years preceding the survey, that is, since January 1, 2004. Finally, information on immunization and child care was collected for the two most recent births. The DLHS-3 has interviewed about 643,944 women, out of which, 504,272 (78%) resided in rural areas. Each woman was asked detailed questions on the births that took place since January 1, 2004. Furthermore, the child health outcomes, such as vaccination, diarrheal

<sup>16</sup>With nearest-neighbor matching, the individual from comparison group is chosen as a matching partner for a treated individual that is closest in terms of propensity score. We consider single-nearest-neighbor matching.

<sup>17</sup>A variation of the formula with the square root is also used. We prefer the square-rooted version because it scales down the variation in weight.

<sup>18</sup>Propensity scores ( $\hat{\lambda}$ ) are estimated from a logistic regression. We also estimate linear probability model (LPM) without reweighting the data for comparison.



episodes, fever, cough, and so on, were asked about the two recent births. In the DLHS sample, about 63% of the total sampled women were not pregnant between 2004 and 2009, thereby limiting the analytical sample to 210,000. After dropping observations with missing information and from urban areas (22%), this sample yielded a final analytical sample of 206,935 child-level observations.

Furthermore, in our analytical sample, data are not missing non-randomly, and thereby, the actual sample used in the analysis is still representative as national sample. It is not true that women with missing information purposively chose not to provide diarrheal information about their children. Had it been the case, the sample would have been biased and non-representative. Rather, information is missing because these women were not asked the diarrhea question because they did not give birth in the last 5 years preceding the survey.

*Outcome and treatment variables:* The survey collected information on diarrhea prevalence in the past 2 weeks before the survey for the children born after January 2004. We use this information to construct our outcome variable, that is, a dummy for whether a child suffered from diarrhea in the past 2 weeks. We matched households with access to improved sanitation to households without improved sanitation using the propensity score generated from the following variables: whether the household has access to piped water, mother's age, father's age, mother's years of schooling, father's years of schooling, whether house structure is pucca/kutchra, number of young children in the household (less than 5 years old), fraction of boys among young children, average age of young children, whether the panchayat head lives in the respondent's village, total number of males in the household, total number of females in the household, amount of irrigated land (in acres), whether the village has a health and sanitation committee, distance to district headquarter (in kilometers), household religion, household caste, state of residence, whether the household is below the poverty line, household electrification status, and availability of health facility (anganwadi) in the village.<sup>19</sup>

The treatment variable is access to improved sanitation.<sup>20</sup> In our sample, about 26% of all households have access to improved sanitation. The mean incidence of diarrhea is 12%. The mean diarrheal incidence in the treated households is 10%, whereas it is 13% in the control households. Younger children are more susceptible to diarrheal risk. The average diarrhea incidence among children under 2 years of age is 15%, whereas for children over 2 years of age, the average incidence is 9%.

Table I reports the pre-matching descriptive statistics of variables that were included in the propensity score estimation. Columns I & II in Table I shows the covariate means in treated and control households, respectively, while column 3 tests whether the difference-in-means are statistically significant. Nearly one-third (33%) of treated households has access to piped water, whereas only 16% of control households has access to piped water. With respect to parents' age, a majority of mothers (69–73%) are below 30 years of age, and about close to 50% of fathers are below 40 years of age. Mother's and father's education is higher in the treated households compared with non-treated households.

Moreover, Table I shows that the share of poor people is higher among the treated than the non-treated groups, with the difference ranging from 23% on the type of house structure and 12% difference in below poverty line (BPL) status across treatment groups. Table I also shows that the shares of scheduled caste (SC) and other backward caste (OBC) are higher among the treated households. Further, less than 50% of non-treated households are electrified, where the electrification rate is 77% among treated households. These differences reflect the higher likelihood of non-treated children to fall into the poor category. This means that treated households enjoy relatively higher economic and social status and are very different from control households. Results in column 3 clearly reflects that treated and control households are significantly different on most of the dimensions except land. The statistically significant differences in column 3 motivates our empirical methodology of PSM as a framework to estimate the treatment effect.

<sup>19</sup>In the absence of baseline date, we resort to ex-post matching by using variables that are presumably time-invariant and might not have changed because of treatment.

<sup>20</sup>The World Health Organization (WHO) defines improved sources of sanitation to be flush toilets connected either to a sewage system or a septic tank, ventilated pits, or composite toilets.

Table I. Descriptive statistics of household characteristics (pre-matching)

	Improved sanitation (Mean)	Unimproved sanitation (Mean)	Differences (1)–(2) ( <i>t</i> -values)	Propensity score logit	
				Coeffs.	Std. error
Household characteristics	(1)	(2)	(3)	(4)	(5)
Household-level variables					
Piped water (1 = yes)	0.33	0.16	−0.17***	0.263	0.183
Mother's age 20–29	0.73	0.69	−0.04***	0.087	0.119
Mother's age 30–39	0.22	0.22	0.009***	0.003	0.188
Mother's age 40–49	0.02	0.03	0.008***	−0.265	0.269
Father's age 30–39	0.50	0.47	−0.03***	0.233*	0.087
Father's age 40–49	0.11	0.11	0.005***	0.554**	0.189
Father's age 50–60	0.01	0.01	0.000	0.803**	0.264
Mother's education: less than primary	0.21	0.38	0.17***	−0.371***	0.095
Mother's education: more than primary	0.70	0.32	−0.38***	0.781***	0.141
Father's education: less than primary	0.08	0.09	0.006***	1.015***	0.112
Father's education: more than primary	0.80	0.58	−0.22***	0.882***	0.098
House structure (1 = pucca)	0.35	0.12	−0.24***	0.881***	0.170
Fraction of young boys	0.53	0.52	−0.009***	−0.014	0.017
Average age of young children (months)	24.74	23.59	−1.15***	0.004***	0.001
Number of males in the household	3.48	3.44	−0.045***	0.020*	0.010
Number of females in the household	3.64	3.65	0.017*	−0.001	0.018
Land	0.59	0.64	0.06	−0.455***	0.132
Below poverty line status	0.25	0.37	0.120***	−0.339***	0.092
Hindu	0.59	0.83	0.234***	−1.69***	0.328
Muslim	0.18	0.11	−0.063***	−0.759*	0.436
Schedule caste (SC)	0.13	0.22	0.081***	−0.849***	0.140
Schedule tribe (ST)	0.21	0.19	−0.017***	−0.515***	0.197
Other backward caste (OBC)	0.31	0.43	0.123***	−0.726***	0.151
Electrified	0.77	0.45	−0.316***	0.673***	0.214
Village-level variables					
Health and sanitation committee in the village	0.34	0.22	−0.122***	0.295**	0.157
Distance to district	43.11	46.07	2.96***	−0.002	0.001
Panchayat head lives in respondent's village	0.68	0.56	−0.122***	0.121	0.148
Anganwadi in the village	0.95	0.91	−0.039***	0.235**	0.074
Observations	54629	152306			
Wald			2205.04		
<i>p</i> -value			0.00		
McFadden's pseudo-R			0.36		

Robust standard errors are presented. Parent's education is years of schooling. Baseline categories are 'mother's age: less than 20'; 'father's age: less than 30'; 'mother's education: illiterate'; 'father's education: illiterate',

\*\*\**p* < 0.01; \*\**p* < 0.05; \**p* < 0.10.

## 5. RESULTS

### 5.1. Propensity score estimation

As mentioned before, the main objective of our empirical strategy is to estimate the causal treatment effects in the presence of selection bias, which is exclusively based on observable characteristics. We are confident that by comprising extensive information on child, household, and village characteristics, in particular on several aspects of SES, education, religion, caste, the survey data cover the complete range of observables necessary to make this empirical strategy viable. We strongly believe that household and village characteristics adequately capture the observables that led the selection into treatment. For example, high SES and more educated households are more likely to have access to sanitation. The survey data also shows heterogeneity in the access

to sanitation within a village, suggesting that a household's characteristics may be correlated with the household's decision to have sanitation access.

We take a closer look at the household's decision to gain access to improved sanitation, as the sanitation program by itself does not ensure the access of sanitation to the every household in the village. Once the sanitation program reaches a village, a household has to make a conscious choice to have access to sanitation as the toilet construction cost is substantial. Only a portion of the construction cost is subsidized by the program, and a substantial part of the cost is borne by the households. The construction cost is probably the most significant factor influencing a household's decision to have access to sanitation. Because of this, we include household's wealth status as a proxy for their ability to pay for the construction of the toilet. Furthermore, education level of household members could also influence the selection into treatment, as more educated households may possess better health knowledge and know the importance of sanitation in reducing diarrhea. Village infrastructure, such as health and sanitation committee, could also provide important health inputs and motivate households to build toilets in their houses. Considering the importance of these variables, we include several household- and village-level variables in the first-stage PSM model to calculate the propensity score.

We use propensity score for matching to avoid the curse of dimensionality (Rosenbaum and Rubin, 1983). The propensity score is the probability of receiving treatment (here, access to improved sanitation) conditional on the observed characteristics at the household and village level. We estimate the propensity score with a logit regression model, which has a treatment dummy as the dependent variable, and a number of covariates as independent variables (see data section for a full list of variables; also, see Table I for the list of covariates used in PSM model).

Columns 4 and 5 in Table I report the first stage of the PSM estimation. Almost all variables that entered into the regression significantly predict the treatment. For example, education level of mother and father positively predicts the treatment, whereas the BPL status of the households negatively predicts the treatment. Electrified households are more likely to have better sanitation, and village infrastructure positively predicts the treatment.

*Covariate balance:* The difference in mean values of variables across treated and non-treated groups in Table I shows a statistically significant difference (column 3), suggesting that matching would improve the precision of the estimates, and purges the observed bias from the estimates. To what extent the matching method has been successful in making the treated and non-treated groups comparable and similar can be confirmed by examining the *t*-values of variable differences in the post-matching sample. The *t*-values on post-matching sample are presented in Table II (column 4). Column 3 reports the percent reduction in bias. Results show that to a very large extent, the matching was successful. With the exceptions of three variables (mother's education: more than primary; number of females in the household; and OBC), none of the other variables in the treated households are statistically different from the non-treated households.

A further test of quality of the matching can be a comparison of the pseudo R-squared in pre- and post-matching as suggested by Sianesi (2004). Sianesi (2004) suggests that the lower value of pseudo R-squared in post-matching sample compared with pre-matching pseudo R-squared indicates a higher quality of matching. Table III presents this result and shows that the post-matching pseudo R-squared is much lower (0.01) than pre-matching pseudo R-squared (0.26). This large reduction in the pseudo R-squared indicates that treated and non-treated households are quite similar. A likelihood ratio test of the joint significance is insignificant, again suggesting that the matching quality is good. To sum up, it is evident from Table III that matching has achieved a significant reduction in observed selection bias.

Finally, we also examine if there is sufficient overlap in the propensity score across the treated and non-treated groups, as only observations on common support are included in the matching process. The off-support observations are discarded from the analysis. The visual analysis of the density distribution of the propensity score in Figure 2 indicates sufficient overlap between treated and control households and thus satisfies the overlap condition of the PSM matching.

Table II. Covariate balance—individual *t*-test

	Improved sanitation (Mean)	Unimproved sanitation (Mean)	% reduction bias	Differences (1)–(2) <i>t</i> -values
	(1)	(2)	(3)	(4)
Household characteristics				
Household-level variables				
Piped water (1 = yes)	0.29	0.30	98.2	−0.93
Mother's age 20–29	0.73	0.73	96.3	0.42
Mother's age 30–39	0.21	0.21	94.1	0.20
Mother's age 40–49	0.02	0.02	96.6	−0.28
Father's age 30–39	0.48	0.48	97.7	0.20
Father's age 40–49	0.11	0.11	98.7	−0.03
Father's age 50–60	0.01	0.01	79.4	0.18
Mother's education: less than primary	0.26	0.26	97.9	1.19
Mother's education: more than primary	0.61	0.62	97.8	−2.43**
Father's education: less than primary	0.09	0.09	63.7	−1.06
Father's education: more than primary	0.76	0.76	99.5	−0.35
House structure (1 = pucca)	0.27	0.27	99.1	−0.67
Fraction of young boys	0.53	0.53	63.0	−1.08
Average age of young children (months)	24.30	24.33	97.6	−0.32
No of males in the household	3.49	3.48	66.2	1.12
No of females in the household	3.66	3.63	−88.7	2.38**
Land	0.61	0.61	97.4	0.44
Below poverty line status	0.28	0.28	96.0	−1.55
Hindu	0.69	0.69	97.9	1.51
Muslim	0.17	0.17	97.6	−0.58
Schedule caste (SC)	0.16	0.16	96.5	−1.13
Schedule tribe (ST)	0.19	0.19	86.4	−0.82
Other backward caste (OBC)	0.35	0.34	93.0	2.58**
Electrified	0.70	0.70	99.3	0.71
Village-level variables				
Health and sanitation committee in the village	0.30	0.30	99.6	0.16
Distance to district	43.76	43.83	97.7	−0.27
Panchayat head lives in respondent's village	0.64	0.64	98.9	0.39
Anganwadi in the village	0.94	0.94	97.9	−0.48

Robust standard errors are presented. Parent's education is years of schooling. Baseline categories are 'mother's age: less than 20'; 'father's age: less than 30'; 'mother's education: illiterate'; 'father's education: illiterate',  
 \*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.10$ .

Table III. Absolute bias, pseudo- $R^2$  and LR  $\chi^2$ 

	Pseudo- $R^2$	LR $\chi^2$	$p > \chi^2$
Unmatched	0.245	58505.81	0.000
Matched	0.000	32.95	0.237

## 5.2. Main results

Table IV reports the ATT for the diarrhea outcome. The ATT is the difference in mean prevalence of diarrhea for children in households with access to improved sanitation and children in households without access to improved sanitation. Access to sanitation leads to a statistically significant reduction in diarrhea for children under the age of five. Column 1 first reports the results from Linear Probability Model (LPM). LPM results suggest that children in treated

<sup>21</sup>The odds for the treated group is 0.101/0.899 = 0.112, and the odds for the control group is 0.123/0.877 = 0.140, which results in an odd ratio of 1.25 (0.140/0.112).

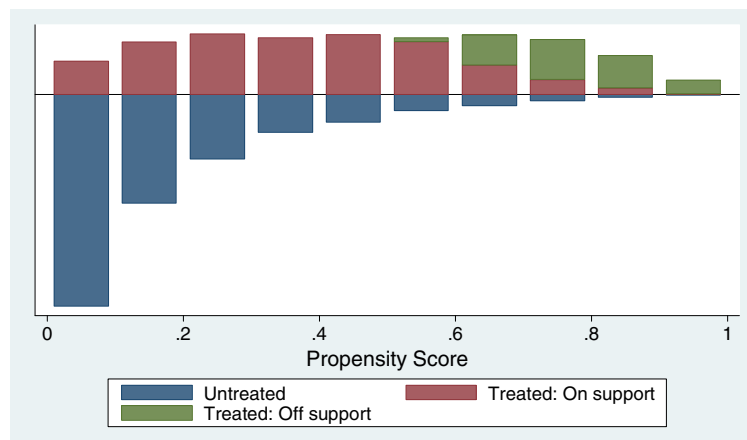


Figure 2. Distribution of propensity score

Table IV. Average treatment effect of improved sanitation

	LPM	PSM (NN 1)	WLS
	(1)	(2)	(3)
Improved sanitation	−0.008** (0.004)	−0.022*** (0.002)	−0.010*** (0.003)
N	206,935	109,258	206,935
R-square	0.03		0.03

LPM, linear probability model; PSM, propensity score matching; NN, nearest-neighbor; WLS, weighted least square.

Standard errors are clustered by state and are robust to heteroskedasticity. Household characteristics include mother's age, mother's education, father's age, father's education, poverty status, caste, electrification status, house type (pucca), and religion. Village-level variables include distance to district headquarter, whether panchayat head lives in the village, health and sanitation committee in the village, and whether village has health worker (anganwadi).

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.10$ .

households are 0.8 percentage points less likely to be exposed to diarrhea risk than the children in the counterfactual households. Since LPM result in column 1 suffers from selection bias, we estimate the PSM model in column 2.

Results in column 2 mean that the mean incidence of diarrhea in households with access to improved sanitation is 2.2 percentage points lower than in households in the control group. To put this in context, the odds of a child living in a household without access to improved sanitation of having diarrhea is 25% larger than for a child in the treated group.<sup>21</sup> Additionally, for a comparison group average of 13% of children with diarrhea in the past week, a 2.2 percentage point decline means a reduction of 17% ( $2.2 * 100/13$ ).

For comparison, we also estimate WLS regression (Table IV, column 3).<sup>22</sup> The treatment effects are consistent in sign, and they are also statistically significant at 1% level of significance. As per the result in column 3, treated children are 1.0 percentage points less likely to suffer from diarrheal risk than the untreated children.

To sum up, the point estimates in LPM and WLS model are 0.8 and 1.0 percentage points, respectively. It should be noted that point estimates in both LPM and WLS are slightly lower than PSM estimates. However, as we discussed before, the estimates from either LPM or WLS cannot be interpreted as causal and suffers from selection bias because sources of variation in the treatment are not plausibly exogenous, we focus on PSM results in column 2, and this is our preferred specification for the rest of the paper.

<sup>22</sup>The weight is defined as the inverse of the propensity score  $1/\lambda$  for treated households and the inverse of one minus the propensity score  $1/(1-\lambda)$  for untreated households (see Hirano *et al.* (2003) for more details).

Table V. Heterogeneous effects of improved sanitation

	Nearest-neighbor model	Std. error
Panel A: Stratified by gender of children		
Boy	−0.020**	0.009
Girl	−0.007	0.008
Panel B: Stratified by wealth index quintiles		
Low SES	0.001	0.009
Middle SES	−0.008	0.006
High SES	−0.025***	0.008
Panel C: Whether households treat water		
Treat water (yes)	−0.033***	0.011
Treat water (no)	0.0005	0.005

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ .

### 5.3. Heterogeneous treatment effects

So far, we are focused on the average impact of improved sanitation on diarrhea incidence. However, it is quite likely that the impact of the treatment varies by subgroups such as age and gender of the child, SES, and health behavior of the household. The results are shown in Table V.

First, we stratify our sample by characteristics of the child. It is plausible that young children could benefit more from the treatment because they are more susceptible to diarrheal attack owing to their weaker and less-developed immune system. However, we do not find any age gradient in the treatment effect. The effects are quite similar for children who are less than 2 years old and children who are between 2 and 5 years old.<sup>23</sup> However, we do find differences in the treatment effect by gender of the child. The mean incidence of diarrhea for boys in households with access to improved sanitation is 2.0 percentage points lower than for boys in households in the control group, and this effect is significant at 95% confidence level. For girls, the treatment effect is 0.7 percentage points; however, it is statistically insignificant. We cannot think of any medical or biological reason why the treatment effect should be higher for boys than for girls; thus the remaining explanation is that parents favor boys over girls.

There is a very steep gradient in the treatment effect by SES. We find a statistically significant effect of 2.5 percentage points in the highest SES group. In the middle SES group, the effect is much smaller (0.8 percentage points) and statistically insignificant. For the low SES group, the treatment effect even carries the opposite sign, but it is statistically insignificant. There are a few plausible explanations for this gradient in the treatment effect by SES. First, SES could be correlated with better health and hygiene behavior of the household and thus strengthen (in case of good behavior) or void (in case of bad behavior) the overall positive effect of improved sanitation. We will turn to this explanation in the next paragraph and explicitly test this hypothesis. Second, it could be that the quality of what is considered improved sanitation differs across households. Let us assume that a low SES household has a flush toilet, but the flush only works for 2 hours per day because of restrictions in the water supply. In this case, having a flush toilet is no advantage over not having a flush toilet. Possibly, it is even a disadvantage if household members use the toilet at times when the flush does not work. However, this explanation is only supported by anecdotal evidence. Unfortunately, we cannot directly test this hypothesis because the data only contains information on the type of sanitation, but not about its quality. Third, low SES households plausibly face stronger negative spillovers from neighboring household that do not have improved sanitation (since they are more likely to live in a neighborhood with low coverage of improved sanitation).

<sup>23</sup>The results are not shown here, but they are available upon request.

<sup>24</sup>Please note that we do not adjust for multiple testing in Table V. However, the  $p$ -values—in particular for SES and water treatment—are so small that the adjustment would not change the conclusions.

<sup>25</sup>The literature does not suggest which model should be preferred, and logit and probit are both commonly used.



We now turn to the hypothesis that the treatment effect is strengthened or voided depending on the household's health or hygiene behavior. A good proxy for this is whether or not a household treats water before drinking it. About 27% of all households reported treating their water before drinking. Boiling was the most common method of treating the water. We find a statistically significant treatment effect of 3.3 percentage points for households that treat their water before drinking. For households that do not treat their water before drinking, the treatment effect has the opposite sign, but it is statistically insignificant. In line with the gradient by SES, this clearly shows that improved sanitation alone does not necessarily lead to better outcomes. Bad health or hygiene behavior can void the positive effect of improved sanitation.<sup>24</sup>

#### 5.4. Robustness checks

In Table VI, we perform a couple of robustness checks on the estimated results from the PSM technique. The standard PSM estimate in the first row of Table VI uses a logit model to estimate the propensity score. The first alternative uses a probit model to estimate the propensity score.<sup>25</sup> The second alternative extends the propensity score model to include additional covariates such as dummies for different types of household assets. The ownerships of a motorcycle, refrigerator, TV, and mobile phone to produce the propensity score. The extended *p*-score model in the second alternative uses a logit model, whereas the third alternative uses a probit model to estimate the extended *p*-score model.

Results in Table VI are robust to a wide range of variations used in the propensity score specification. The treatment effects do not vary much across different specification of the *p*-score. Diarrheal incidence reduces by 2.2 percentage points, 2.1 percentage points, 2.1 percentage points, and 2.2 percentage points owing to improved sanitation in standard PSM, first alternative, second alternative, and third alternative, respectively. Overall, we see that different implementations of PSM lead to very similar results, both in direction and magnitude of the treatment effect.

*Limitations:* Finally, even though we are confident of the quality of matching, our results should be interpreted with a caveat that propensity score matching only provides causal interpretation if the selection into treatment is observed and adequately controlled in the model. The PSM assumptions are violated if the selection into treatment is based on some unobserved covariates. We could not combine matching with differences in estimates to remove bias from selection on time invariant unobserved variables because of unavailability of suitable baseline data. We therefore used ex-post matching. Second, our estimates may underestimate the true health effect of sanitation caused by spillover effects/externalities. Diarrhea is an infectious disease. Thus, it is quite likely that children in counterfactual households might have benefitted indirectly from the treatment of other households in their neighborhood. In this scenario, the estimated results will be biased towards zero. On the other hand, negative spillover effects might have reduced the treatment effect for households in neighborhoods that mostly do not have access to improved sanitation.

Table VI. Robustness check

	Nearest-neighbor model	Std. error
Base model	0.022***	0.002
Alternate implementation of <i>p</i> -score		
First alternate estimation of <i>p</i> -score	0.021***	0.002
Second alternate estimation of <i>p</i> -score	0.021***	0.002
Third alternate estimation of <i>p</i> -score	0.022***	0.002

The first alternate estimation of *p*-score uses a probit model to estimate the propensity score. The second alternate estimation of *p*-score uses a logit model and includes additional covariates such as household assets in the *p*-score model. The third specification re-estimates the second model in a probit framework.

\*\*\* $p < 0.01$ .

Table VII. Sensitivity analysis: Rosenbaum bounds

$\Gamma$	Mantel and Haenszel (1959) bounds for variable diarrhea			
	$Q_{mh}^+$	$Q_{mh}^-$	$p_{mh}^+$	$p_{mh}^-$
	(1)	(2)	(3)	(4)
1	11.62	11.62	0.00	0.00
1.2	21.23	2.07	0.00	0.02
1.4	29.46	5.97	0.00	0.00
1.6	36.68	12.98	0.00	0.00
1.8	43.15	19.19	0.00	0.00
2.0	49.03	24.78	0.00	0.00
2.2	54.44	29.89	0.00	0.00
2.4	59.45	34.60	0.00	0.00

Gamma: odds of differential assignment caused by unobserved factors.

$Q_{mh}^+$ : Mantel–Haenszel statistic (ass: overestimation of treatment effect).

$Q_{mh}^-$ : Mantel–Haenszel statistic (ass: underestimation of treatment effect).

$p_{mh}^+$ : significance level (ass: overestimation of treatment effect).

$p_{mh}^-$ : significance level (ass: underestimation of treatment effect).

Source: MH bounds using STATA 11.

### 5.5. Hidden bias and sensitivity analysis

The PSM method addresses the biases that are due to observed characteristics. It does not correct biases that are due to unobserved characteristics. There could still be hidden bias that remains unobserved. For example, it might be possible that households with access to improved sanitation are more forward-looking and more motivated, and this may cause them to invest more in improved sanitation. The extent of this motivation and forward-looking behavior is not observed, but it can still bias estimates obtained through the PSM method.

The estimation of treatment effects with matching methods rests on the assumption of conditional independence, meaning that treatment and control groups do not differ on *unobservables* that influence the selection into treatment and outcome variables. If both groups differ on unobserved variables, there may be a *hidden bias* to which matching estimators are not robust (Rosenbaum, 2002). Since estimating the magnitude of selection bias is nearly impossible with non-experimental data, we employ the bounding approach proposed by Rosenbaum (2002) to determine how strongly the unobservables must influence to make the estimated treatment effects null and void.<sup>26</sup>

The results from the sensitivity analysis are presented in Table VII. The results suggest that the estimated treatment effect with the matching method remains significant even in the presence of large hidden bias. The  $Q_{mh}$  statistic implies a significant treatment effect of access to improved sanitation on diarrheal prevalence. In the event of negative (unobserved) selection, when those most likely to participate tend to have lower diarrhea rates even without treatment, the estimated treatment effects are underestimated and should be adjusted upward. After carefully examining the values of  $Q_{mh}^-$  and  $p_{mh}^-$  in Table VII, it seems that the PSM results are insensitive to selection bias emanating from unobserved variables. In the case of positive (unobserved) selection bias, the relevant statistics to examine are  $Q_{mh}^+$  and  $p_{mh}^+$ . Positive selection bias occurs when households with improved sanitation may have higher diarrheal prevalence rate. This type of selection bias leads to upward bias in the PSM results and needs to be adjusted downward. The  $Q_{mh}^+$  and  $p_{mh}^+$  also provides no evidence of *hidden bias*, and the PSM results would remain significant even in the presence of substantial amount of positive (unobserved) selection bias. It is comforting that the PSM results survive even in the presence of large

<sup>26</sup>The sensitivity of the PSM results is checked by using the *MHBounds* package in Stata. *MHBounds* with a focus on binary outcomes was developed by Becker and Caliendo (2007).

<sup>27</sup>(0.022 diarrhea case per child in 2 weeks)\*(1.40 children under five per household)\*(26 two-week episodes per year) = (0.8 diarrhea cases per household-year).

hidden bias. Even allowing for a significant amount of selection on unobservables, the PSM results remain significant irrespective of whether there is a positive or negative selection bias. The inference from the test statistic does not change even at the higher values of  $T$ .

## 6. CONCLUSIONS AND POLICY IMPLICATIONS

This study employs PSM to quantify the health gains to children from access to improved sanitation in rural India. We find reduced diarrhea incidence for children in households with access to improved sanitation. Access to improved sanitation roughly averts 0.8 diarrhea episodes per household-year.<sup>27</sup> The 0.8 cases per household-year might not seem like a big effect, but given that diarrhea is the second leading cause of death and that there are, on average, 3.1 diarrhea cases per child-year (or 3.9 diarrhea cases per household-year), this is an important improvement from a public health perspective.

There is considerable heterogeneity in the impact of improved sanitation on diarrhea. We find a very steep gradient by SES with strong treatment effects for high SES households and no effects for middle and low SES households. We only find a significant treatment effect for households that treat their water before drinking it, but not for those that do not treat their water before drinking it. And we only find a significant treatment effect for boys and not for girls. This indicates that the average treatment effect may not be the best measure to assess gains from improved sanitation. It is particularly troubling that the treatment effect completely bypassed girls and children from low and middle SES households.

India made huge progress in sanitation coverage in rural areas. However, it still has the highest burden of child mortality and morbidity related to diarrhea in South Asia. Our results suggest that benefits from the access to improved sanitation<sup>28</sup> have not been fully realized yet. How can this problem be addressed, and how can policy help to fully realize the benefits from improvements in sanitation that have been achieved in recent years?

From a perspective of justice, it would be particularly important to address gender bias. Son preference is deeply rooted in Indian society and not easy to change. We are certain that all families love their daughters as much as their sons, but there are strong cultural and economic forces that make them give a preferential treatment to their sons. It is important to work on changing these underlying forces, but it would be overly optimistic to expect quick outcomes.

Health and hygiene behavior seems like a more promising target for intervention. The difference in treatment effect between good (treating water) and bad (not treating water) health and hygiene behavior is even larger than the difference in treatment effect between high and low SES households. Treating water is intrinsically important; however, it is certainly also a proxy for other behavior. This proxy for behavior makes the difference between a completely voided treatment effect and a very strong treatment effect.

Further research is needed to understand the SES gradient in the treatment effect. Certainly, the SES effect is partly also a behavioral effect. But at this point, we cannot rule out additional or alternative explanations. For example, improved sanitation (even though coded the same way in the data) might have very different meanings for a high SES household with 24/7 water supply and a low SES household with 2-hour per day water supply. Also spillover effects might play a greater role for low SES households than for high SES households. First, it would be important to have data that allow to explicitly test this hypothesis. And second, if it really turns out that quality is an important driver behind the SES effect, policy should make sure that improved sanitation facilities are complemented with a reliable water supply (which is one prerequisite for the proper functioning of the facilities).

To conclude, improved sanitation is an important public health intervention to reduce burden from diarrheal morbidity and consequently mortality for children in India. The potential benefits of the current sanitation

<sup>28</sup>Possibly also from other infrastructure such as piped water.

infrastructure have not been fully realized. Continuing improvements in sanitation infrastructure in hand with complementing policies for behavior change through community participation, education, awareness, and health promotion activities may go a long way in reducing diarrheal burden in India and elsewhere.

## CONFLICT OF INTEREST

No conflict of interest exists among the authors.

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