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The Short- and Medium-Term Impacts of Household Water Supply and Sanitation on Diarrhea in Rural India

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The Short- and Medium-Term Impacts of Household Water Supply and Sanitation on Diarrhea in Rural India

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Poor water quality and sanitation lead to severe health problems in developing countries, yet there is little evidence on the effectiveness of at-scale, infrastructure-based solutions for the rural poor. This paper estimates the impact of an integrated water and sanitation improvement program in rural India that provided household-level water connections, latrines, and bathing facilities to all households in approximately 100 villages. We employ an interrupted time-series analysis with multiple units to estimate the short- and medium-term impacts of the intervention on episodes of diarrhea for which treatment was received. The estimates suggest that the intervention was effective, reducing such episodes by 30-50%. These results are evident in the short term and persist for 5 years or more. The annual cost is approximately US$60 per household, as compared to annual household consumption of approximately US$740.

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An estimated 748 million people lack access to improved sources of water, and more than 2.5 billion lack improved sanitation (WHO - UNICEF 2014). These privations are borne disproportionately by the rural poor: in developing countries, only 24% of rural populations have access to piped water from a household connection, as compared to 74% of urban populations; seven out of ten people without improved sanitation live in rural settings. Lack of safe water, inadequate sanitation, and poor hygiene practices cause an estimated 1.1 million deaths from diarrhea each year, representing 1.5% of the global burden of disease (Prüss-Ustün et al. 2014). More than a quarter of these deaths occur in India, where open defecation is still practiced by 65% of the rural population and only 14% of the rural population have piped-in water to the household (WHO - UNICEF 2014).

In response, the Government of India has recently proposed a set of initiatives aimed primarily at improving sanitation coverage. Building on his 2013 campaign statement "*pehle shauchalaya, phir devalaya*" ("toilets first, temples later"), current Prime Minister Narendra Modi has launched the *Swachh Bharat* ("Clean India") Mission, aimed at ending open defecation by 2019 (Times of India 2013, Modi 2014). *Swachh Bharat* proposes to provide toilets to all 110 million rural households that currently do not have one, at a cost of US$ 22.0 billion (Ministry of Rural Development 2014, Ministry of Drinking Water and Sanitation 2014).

However, this proposal has faced skepticism (Hueso 2014, Vyas 2014, Sreevatsan 2014), chiefly for two reasons. First, several previous toilet-building initiatives have had limited success at reducing open defecation (Barnard et al. 2013, Hueso and Bell 2013). May people in rural India prefer open defecation to using government-build toilets, which are often provided without education and built with little understanding of user preferences (Gupta et al. 2014). While information, education and communication (IEC) feature prominently in the proposal's rhetoric, only 8% of the budget is allocated to IEC, as compared to 15% in previous programs (Ministry of Drinking Water and Sanitation 2014, Hathi 2014).

Second, the evidence for the effectiveness of the types of interventions typically conducted in rural areas of developing countries is not strong. Infrastructure-based solutions have proved effective in dense populations when the correct institutional incentives are present (Cutler and Miller 2006, Galiani, Gertler, and Schargrodsky 2005). In rural areas of developing countries, concerns about cost and governance have meant that much of the effort is directed towards improving water sources (e.g. improved wells and springs, communal tapstands) rather than investing in the infrastructure necessary to deliver safe and continuous supplies of drinking water to the household (WHO - UNICEF 2014), and these interventions have not proved effective at preventing diarrhea (Wolf et al. 2014). Some household-based water treatment options such as filters are protective against diarrhea, but only if they are used correctly and consistently (Brown and Clasen 2012, Enger et al. 2013). Similarly, interventions promoting on-site sanitation of the type that can be implemented in rural and other low-density settings have not proved as effective as sewerage (Wolf et al. 2014).

Given the magnitude of the problem and the gathering momentum for large-scale spending of scarce resources in an attempt to address this problem, it is important to provide rigorous evidence on the effectiveness of potential solutions. In this paper, we study an alternative model for improving water and sanitation in rural India. Specifically, we study the effects of a program providing a communal water tank, private toilets and bathing facilities and a private water connection to all households in approximately 100 villages in rural Orissa, India. This intervention differed from currently proposed policies in two key dimensions: first, rather than rapidly building toilets in target villages, it devoted time and resources towards building consensus in the community that universal access to and use of clean water and toilets were necessary; second, it employed an infrastructure-based system not typically used in rural areas.

Three features of the program permit credible estimation of causal effects: first, in each village, piped water services were activated for all households at the same time, and sanitation coverage increased rapidly and contemporaneously, leading to a sharp improvement in environmental conditions; second, the implementer collected monthly data on outcomes beginning two years or more before the activation of services and continuing for up to five years after; third, services were activated in different months in different villages. Together, these features permit the estimation of causal effects based on the difference in outcomes before and after the sharp change in environmental conditions, while controlling for village-level fixed effects, village-specific trends in outcomes, and month-by-year fixed effects. In addition, the long follow-up period allows us to test whether the effect persists over time.

We find substantial reductions in water-related disease: episodes of severe diarrhea declined by 30-50%. These results are evident in the short term and persist for at least five years. The annual cost is approximately US$60 per household, as compared to annual household consumption of approximately US$740.

I. The Rural Health and Environment Program

This paper evaluates the Rural Health and Environment Program (RHEP), a village-level intervention that promotes adoption of household latrine and bathing facilities, a community water tank, and a distribution system that supplies piped water to household taps. RHEP was developed by Gram Vikas (GV), an Indian non-governmental organization, and implemented in villages in Orissa, one of the poorest states in India. Pre-intervention water supplies consisted mainly of village ponds and open streams. Open defecation was the norm, as is typical of Orissa, where historically only 10% of the population has had access to safe water and sanitation (Government of India 2012). RHEP was first piloted in 5 villages (340 households) in 1992. It was then expanded in four phases, adding 40 villages (3,000 households) from 1995-1998, 27 villages (2,000 households) from 1999-2001, 38 villages (3,000 households) from 2001-2003 and 160 villages (8,000 households) from 2003-2006 (Gram Vikas 2001, 2005).

Because the timeline is important for the research design, we detail the typical sequence of events for implementing RHEP in a village (Gram Vikas 2001, 2004, 2005, Keirns 2007). First, GV extension workers identify a village well-suited to the program. Desirable characteristics are a strong sense of community and good village leadership. Next, GV representatives meet informally with village leadership. If there is interest in starting a program, a series of village meetings are held to build participation and obtain 100% consensus for participation in the program. This process of consensus-building usually takes 3-6 months, but can take up to 18 months. GV insists on 100% participation because of the complementarities inherent in sanitation: if even a few villagers do not participate, they can transmit disease to their neighbors, reducing the benefits of the program.

Once the village has reached 100% consensus, GV and the village enter a formal agreement. At this time, data collection begins, along with construction. Villagers begin construction of household latrine and bathing facilities, using their own labor and locally acquired materials. Once all households in the village complete the walls of their latrines and bathing houses (i.e. the external structures are complete except for the roof), GV subsidizes inputs the villagers cannot provide themselves, e.g. ceramics and piping materials. When completed, household facilities consist of a water-sealed pit latrine and a private bathing area, both enclosed by a masonry superstructure with roof and separate doors.

Construction of the water supply infrastructure runs in parallel to the construction of the latrine and bathing facilities. The water supply consists of an improved water source (usually a protected well or borehole), a pump and central water tank for the village, and a gravity distribution system (piping from the tank to individual households). Each household receives three water taps: one each for the latrine and bathing facilities and one inside the home. As with the other facilities, villagers supply unskilled labor and locally available materials while GV provides skilled labor and specialized materials. Construction of the water supply requires 3-4 months of work, but usually takes one calendar year or more to complete because work is not continuous – villagers usually supply their own labor outside of the planting and harvest seasons.

Although some households inevitably complete their sanitary houses sooner than others, GV does not activate the water supply until all sanitary facilities are complete. GV believes that households are primarily interested in obtaining running water for their homes, so turning on the water to the home before the sanitary facility is complete could lead to villagers not completing the project. Furthermore, social pressure or cross-subsidization can help push lagging households towards completion, and these forces would be muted if leading households received water service (Jenkins and Curtis 2005). Crucially for the research design, water service is activated suddenly and simultaneously for all households in the village. Sanitation coverage also increases rapidly, since access to piped water facilitates the use of the pour-flush toilets. Following construction, villagers are responsible for operational costs. GV provides some training for villagers to provide maintenance and advises village committees on governance, but does not provide further subsidies or other direct inputs such as labor or parts.

Using GV records, we estimate that the total cost of implementing RHEP in a typical village of 50 households was approximately US$60 per household per year, as compared to annual household consumption of approximately US$740. Details of this calculation, which includes the value of villagers' labor, Gram Vikas personnel time and external subsidy, are provided in the Appendix.

II. Research Design

Several reviews have identified shortcomings in the existing evidence, in particular the challenges of finding a valid counterfactual for outcomes in the absence of the intervention, measurement in the presence of externalities, small sample sizes and short follow-up periods (Fewtrell et al. 2005, Clasen et al. 2007, Zwane and Kremer 2007, Waddington and Snilstveit 2009). To address the difficulty of obtaining unbiased estimates of causal effects of water and sanitation interventions from observational data, recent work has employed matching methods to construct a plausible quasi-control group (Arnold et al. 2010, Pattanayak et al. 2010).

The design of RHEP provides an unusual and valuable opportunity for evaluation, for several reasons. First, the implementation provides a plausible quasi-experimental research design for unbiased estimates of causal effects. Cross-sectional comparisons of communities with and without access to water and sanitation are likely to be subject to omitted variables bias (e.g., due to differences in incomes). A standard event study comparing outcomes before and after an intervention may confound program effects with the evolution of other determinants of health. However, in the case of RHEP, piped water service is activated suddenly and simultaneously for all households in a village, and sanitation coverage increases rapidly in the months just before and after piped water is activated. Since it is unlikely that other determinants of health outcomes would change simultaneously and just as rapidly, an event study in this context has greater internal validity.

Second, studies that compare households with and without sanitation services within a community will overlook potential spillover benefits to non-recipient households, which may be less likely to contract communicable disease (Fink, Günther, and Hill 2011, Geruso and Spears 2014). Because RHEP is implemented at the village level, this paper's estimates capture the total effect of the intervention within the village; that is, direct benefits as well as within-village externalities.

Third, usable data from nearly 100 villages are available, which improves the precision of the estimates relative to studies of only a limited number of sites. Furthermore, because the intervention occurred in different months in different villages, it is possible to distinguish between the effect of the program and time effects, which typically is not possible in the case of an event study that examines a single event at a single time. Additionally, because data collection begins when GV and the village enter the formal agreement – usually at least a year before the water is turned on – and continues for three years or more after the water arrives, it is possible to assess the medium-run impact of the program rather than just short-run impacts.

III. Data and Sample Selection

A. Data

The implementation and outcome data were obtained from GV's internal "Monthly Progress Reports" (MPRs). MPRs were compiled on a monthly basis by GV personnel, either during monthly (or more frequent) visits or by GV staff residing on-site. MPRs contain detailed information on the status of RHEP (e.g. water tank construction, number of households with and without completed latrine and bathing facilities, number of households using their latrine), allowing exact identification of the month in which water improvement begins. Outcome data are recorded each month by the GV village supervisor, who draws on a variety of sources, including monthly or more frequent visits to individual households, consultation with the local health clinic (when one exists), and the village health committee.

It is important to note that diarrhea episodes are only recorded if a resident is "checked" or "treated." "Checked" means that the villager went for a consultation with a medical professional, i.e. a health worker or a doctor. "Treated" means that the villager received treatment for the condition, again from either a health worker or a doctor. This excludes cases for which a villager does not seek a medical consultation, or cases when a villager seeks treatment via home remedies or local healers. As a result, our measures underestimate the overall morbidity associated with diarrhea, but are likely to represent more severe cases that are more highly correlated with mortality and serious sequelae (Kotloff et al. 2013). This outcome may also be less susceptible to courtesy bias than caretaker reporting, which is the measure most frequently used in assessing the impact of environmental interventions on diarrhea (Schmidt et al. 2011). We focus on the "treated" measure in our analysis but the results using the "checked" variable are similar.[1]

B. Sample Selection

A village is included in the analysis if the month in which the water supply is activated can be identified. There are 97 such villages, with approximately 5,500 village-by-month observations total. The average village contains approximately 90 households and 500 residents. We use τ to indicate the month relative to the month in which the water switches on, so $\tau = -1$ is one month before the water turns on, $\tau = 0$ is the month in which the water turns on, $\tau = 1$ is one month after, and so on. Three samples are analyzed. Sample A includes all observations with known τ, i.e. all observations from villages in which the month of water supply activation is known. Sample B uses only observations from 2 years prior to 5 years after the activation of the water supply (i.e. $\tau \in \{-24, \ldots, +59\}$) and includes only villages for which there are at least six observations before and six observations after the activation of the water supply. Sample C uses only observations within one year of activation of the water supply (i.e. $\tau \in \{-12, \ldots, +11\}$), and includes only villages with observations in at least six of the 12 months before and six of the 12 months after the activation of the water supply. The tradeoffs among the samples are between sample size (largest in Sample A) and using a sample closer to a balanced panel (closest in Sample C). The results are generally consistent across samples. Table 1

[1] Other diseases monitored include malaria, fever, typhoid, jaundice, cold and cough, night blindness, scabies, TB and leprosy. We do not focus on these because they are either too scarce for any reasonably-sized treatment effect to be detected or are only weakly or indirectly related to water and sanitation conditions. Similarly, data are recorded on mortality and child mortality, but given the rarity of these events, we do not have statistical power to detect any reasonably-sized effect. We did detect an association between the initiation of services and a reduction in malaria, which we discuss in Section IV.C.

provides descriptive statistics for the three samples. In Figure 1, we show the rapid increase in sanitation coverage: among villages in Sample B, the median share of households using a latrine increases from less than 0.2 six months before water services are activated to over 0.9 just three months after.[2] The increase in the availability of piped water is even more dramatic: there are no documented cases of households with piped water before the water tank was activated, and with very few exceptions (e.g. maintenance problems, new household formation) all households received piped water once the tank was in use.

IV. Econometric Methods and Empirical Results

A. Econometric Methods

Our analysis follows two main strategies: an event study analysis that estimates month-by-month effects; and a panel approach that estimates average impacts across months.

The event study analysis involves estimation of the following equation:

$$y_{vt} = \sum_{s=-12}^{+11} \alpha_s \tau_{vs} + \gamma_v V_v + \delta_t T_t + \varepsilon_{vt}, \quad (1)$$

where y_{vt} denotes the outcome of interest, i.e. monthly cases of treated diarrhea, for village *v* in month *t*, τ_{vs} is an indicator for the s^{th} month after the water improvement begins (starting at zero, i.e. $\tau = 0$ is the month in which the water turns on) in village *v*, V_v is an indicator for village *v*, T_t is an indicator for year-by-month *t* and ε_{vt} is the disturbance term for village *v* in month *t*. The $\tau_{v,-1}$ indicator is omitted from the regression, so estimates of the coefficients α_s are interpreted as the mean of the outcome variable relative to the month before the water supply is activated.

A few aspects of equation (1) merit discussion. First, the village fixed effects γ_v control for any time-invariant mean differences across villages, whether from observable or unobservable confounders. Second, having multiple observations in each month from different villages allows the estimation of month-by-year fixed effects δ_t, which control for aggregate monthly shocks (e.g., due

[2] The sample in Figure 1 is additionally restricted to villages with at least 6 observations of latrine coverage both before and after the activation of services.

to weather) across villages in the sample. Third, and most important, the combination of (a) the sudden change in the availability of water in month $\tau_{v,0}$ and sanitation in the surrounding months and (b) the possibility of a rapid response of diarrhea to a change in the environment together give a sharp prediction of a swift change in the number of diarrhea cases. In terms of the econometric model, this prediction is of negative values of α_1, α_2, etc. Since there are relatively few observations for each τ and no restrictions are imposed on the month-by-month effects, these estimates are unlikely to be very precise. However, this regression will be informative about the integrity of the research design, since it provides information on pre-program trends that could cause bias. It also permits visual examination of the persistence of program impacts.

The second approach is to fit the following equation:

$$y_{vt} = \alpha POST_{vt} + \gamma_v V_v + \delta_t T_t + \varepsilon_{vt}, \quad (2)$$

where $POST_{vt}$ indicates that the water supply has turned on in village *v* in month *t*, and all other variables are as in equation (1). This will collapse the month-by-month estimates from equation (1) into a single summary measure of the program's impact. For example, in Sample C, $\hat{\alpha}$ represents an estimate of the program's impact in the first year, relative to the mean in the year leading up to the activation of services.

An important variant of equation (2) adds village-specific time trends, as in

$$y_{vt} = \alpha POST_{vt} + \gamma_v V_v + \delta_t T_t + \varphi_v V_v \tau_{vt} + \varepsilon_{vt}, \qquad (3)$$

Where τ_{vt}, as above, is defined as the number of months after $(\tau_{vt} > 0)$ or before $(\tau_{vt} < 0)$ the water turns on in that village. This controls for linear trends in outcomes during the period of data collection, and allows this trend to vary from village to village. The advantage of this specification is that it separates the impact of the arrival of the program from other ongoing trends in village outcomes, to the extent that these trends are roughly linear. However, this approach would not adequately control for other programs or behaviors that switch on abruptly and simultaneously with RHEP. We revisit this possibility below.

To formalize the analysis of the persistence of program effects, we estimate the following variant of equation (2), which separates the program effect into two sub-periods:

$$y_{vt} = \alpha_0 1\{0 \le \tau_{vt} < 36\} + \alpha_1 1\{\tau_{vt} \ge 36\} + \gamma_v V_v + \delta_t T_t + \varepsilon_{vt}\,. \qquad (4)$$

Here, α_0 represents the impact of the program in the first three years and α_1 represents the impact thereafter.

Several details relevant to the precision of the estimates are worth noting. First, the use of village-level aggregates as the unit of observation avoids overstating precision, as would occur if each household were treated as an independent observation (Moulton 1990). Second, because unobservable time-varying determinants of village outcomes (represented by ε_{vt} in the statistical model) are likely to be correlated over time, it is necessary to compute standard errors clustered at the village level, robust to arbitrary autocorrelation patterns within villages (Bertrand, Duflo, and Mullainathan 2004). Third, each village-by-month observation is weighted by the inverse of the number of observations on that village, so that each village receives equal weight in the estimation. The results are insensitive to alternative weighting strategies, as shown in our robustness checks.

B. Results

The first empirical test is estimation of the event-study model described by equation (1) above. The analysis consists of regressions using cases of treated diarrhea as the dependent variable. Figure 2 graphs the point estimates and 95% confidence intervals for $\alpha_{-12},\ldots,\alpha_{+11}$ for Sample C. Although the individual monthly estimates are imprecise, the downward shift after the water turns on is evident visually. There is no noticeable downward trend prior to the start of improved water, which supports the validity of the research design.

Figure 3 plots the results of a similar exercise, this time including indicators for each of the 24 months prior to and each of the 60 months following the initiation of services. This is the result of estimating

$$y_{vt} = \sum_{s=-24}^{+59} \alpha_s \tau_{vs} + \gamma_v V_v + \delta_t T_t + \varepsilon_{vt} \qquad (5)$$

using Sample B. This regression model is identical to that of equation (1) but with a longer time span. The results are similar: there is a discernable improvement after the water supply is activated and no pre-intervention trend is visible. Furthermore, this improvement appears to persist with similar magnitude, although the monthly estimates become less precise over time.

Table 2 reports results from the estimation of equations (2) and (3), which provide a summary of the program's overall impact. The upper panel presents estimates of α from equation (2). There are large and statistically significant reductions across all three specifications. Estimates of the effect on the number of cases per village-month range from -0.45, relative to a pre-intervention mean of 1.18 (Sample C), to -0.59, relative to a pre-intervention mean of 1.08 (Sample A). The lower panel reports estimates of α from equation (3), which includes village-specific linear trends. The results are robust to the inclusion of this linear trend, as can be seen from the similarity of the estimates of α with the estimates in the top panel.

Table 3 reports formal tests of the persistence of the program effect from estimation of equation (4). The evidence for persistence is strong: while the point estimates for diarrhea are somewhat smaller in absolute value in the period $\tau_{vt} \geq 36$ than in the period $0 \leq \tau_{vt} \leq 35$, the null hypothesis that the two estimated coefficients are equal is not rejected (p-value ≈ 0.3). Further, the estimate for the latter period is still statistically and practically significant.

We tested the robustness of these findings by repeating the analysis using different specifications (i.e., different weighting schemes, additional fixed effects, and different sample selection criteria), alternative definitions of key variables (i.e., activation of water supply and diarrhea checked rather than treated), and particular choices made in cleaning the data. Results of these robustness checks are summarized in Figure 4. Exact results vary, but the pattern of statistically and practically significant health improvements is robust: in 14 of 16 specifications, the estimate is significant at the 95% level. Further detail on these robustness checks are provided in Section A1 of the Appendix.

C. Threats to validity

Identification assumption.—This paper's identification assumption is that no other determinants of diarrheal disease change abruptly and at the same time as the activation of water and sanitation. One potential threat to this assumption is that GV works with RHEP villages on other projects, such as building schools, skills training, aquaculture and sustainable forestry. However, these programs are unlikely to cause bias, because their timing is not coordinated with activating the water supply and they are implemented more gradually. Furthermore, our results are robust to the inclusion of village-specific linear time trends in the regression analysis, which absorb the impact of these other programs to the extent that they arrive gradually and have approximately linear effects on outcomes over time.

Data collection.—This study relies on outcome data collected by the implementer and not independently verified. The implementer may have incentives to exaggerate the beneficial impact of the project. While we cannot rule out bias entirely, several considerations mitigate this possibility. First, the primary purpose of the data was to provide information on the project's implementation and conditions in the village to help GV's programming decisions. GV did not collect these data for an evaluation, as evidenced by the fact that they had not previously been used in this way, despite having been collected for many years. In fact, the paper forms were locked in a closet when they were uncovered by the research team during a visit to discuss an unrelated evaluation (Hanna, Duflo, and Greenstone 2012). Second, GV personnel were not compensated on the basis of the outcome, and were subject to sanction for misreporting. Third, the focus on cases for which treatment was sought or received may reduce recall or courtesy bias relative to standard measures, since such occasions were unusual as compared to "usual" cases of diarrhea.

Observed reduction in malaria.—Our analysis found an association between the initiation of services and a reduction in reported cases of malaria. Although this is not a robust relationship (significant at the 95% level in just 5 of 15 specifications, as seen in Figure A1), it deserves further discussion: since there is no direct biological mechanism linking clean water and malaria, this finding could indicate reporting bias that would in turn raise doubts about the data on diarrhea.

Alternatively, the improved sanitation program may have reduced the incidence of malaria through an indirect channel. First, in-home water and on-site toilets plausibly reduce exposure to malaria vectors, since such exposure can occur during trips outside the vicinity of the home for open defecation or to collect water (Keiser, Singer, and Utzinger 2005). Second, a reduction in diarrheal disease reduces insults to immune function, which could in turn reduce co-morbidity from other infectious diseases (Schmidt et al. 2009, Walker and Black 2009). A related possibility is that the intervention may have reduced cases of fever misreported as malaria: since malaria is not often diagnosed rigorously, many reported cases could in fact be fever caused by water-borne infections (Cohen, Dupas, and Schaner 2014). There is some support in our data for these last two hypotheses: there is a weak signal of an association of a reduced number of cases of fever with the initiation of program services, although this association is typically not statistically significant (see Figure A2). Further discussion and empirical results are provided in Section A2 of the Appendix.

Other limitations.—This study has several additional limitations. First, the data under-report the total number of cases of diarrhea since they cover only treated cases. However, since the data likely include the most severe cases, it is plausible that the majority of the diarrhea related welfare improvements are captured. Second, RHEP is a package of a communal water tank, piped water to the home, and household latrines and bathing facilities. This evaluation therefore does not provide information on the effectiveness of the individual components. Third, GV partners with villages it believes capable of building the institutional capacity to complete the project with 100% participation and to sustain the facilities after construction is finished. These are important and perhaps unusual characteristics of a village, and they may limit external validity: a program technically similar to RHEP but targeted at villages lacking these characteristics may be less successful. Fourth, a complete cost-benefit assessment of RHEP would require a monetization of the full array of the program's benefits, for which data are not available. These other benefits may include reductions in the incidence of other morbidities such as stunting or soil-transmitted helminth infections (Spears, Ghosh, and Cumming 2013, Strunz et al. 2014), decreases in time devoted to water collection (Pattanayak et al. 2010, Kremer et al. 2011), and the amenity value of private sanitary facilities.

V. Conclusion

This study exploits detailed knowledge of features of a community-level, infrastructure-based water and sanitation intervention to obtain quasi-experimental estimates of the program's causal effects. We find that the program reduced severe cases of diarrhea by approximately 30% to 50%, which is generally consistent with the pooled estimates reported in systematic reviews (Fewtrell et al. 2005, Waddington and Snilstveit 2009, Engell and Lim 2013, Wolf et al. 2014). Further, the intervention is relatively affordable, with all costs, inclusive of external subsidy, implementer staff time and non-monetary inputs from the beneficiaries totaling less than 10% of the value of annual household consumption.

These positive results differ from the null findings of two recent cluster-randomized trials evaluating sanitation-only interventions in rural India, one in Orissa (Clasen et al. 2014) and the other in Madhya Pradesh (Patil et al. 2014). We do not view our results as contradictory, for two reasons. First, RHEP provides water and sanitation together, so the underlying intervention is different. Second, while these two recent interventions did increase latrine *coverage*, they were less successful in increasing latrine *use* and reducing open defecation, which are necessary for health benefits (Brown and Clasen 2012). Understanding the factors that motivate or impede the use of sanitation facilities remains a key subject for further research (Coffey et al. 2014, Gertler et al. 2015).

On methodological grounds, this study shows that quasi-experimental evaluation can be used with retrospective, observational data to obtain plausible estimates of causal effects of environmental interventions in developing countries (Arnold et al. 2010, Chen et al. 2013, Pattanayak et al. 2010). Such evidence is critical in developing country settings, where the allocation of health resources can have immediate mortality consequences.

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Tables

Table 1: Descriptive Statistics

	Sample A (1)	Sample B (2)	Sample C (3)
Village characteristics:			
Number of villages	96	75	73
Number of households per village	88.4	91.6	102.2
	(60.0)	(61.4)	(70.4)
Village population	493.3	509.0	527.0
	(327.2)	(324.2)	(327.6)
Observation counts:			
Num. of months betw. first obs. and initiation of service	16.4	17.6	11.5
	(9.7)	(5.4)	(1.2)
Num. of obs. before initiation of service	17.4	17.0	11.3
	(8.3)	(5.3)	(1.3)
Num. of months betw. initiation of service and last obs.	48.3	42.7	11.9
	(33.2)	(16.8)	(0.7)
Num. of obs. after initiation of service	45.4	41.2	11.8
	(31.5)	(16.5)	(0.8)
Health outcomes (per village-month observation):			
Number of cases of diarrhea	0.64	0.68	0.87
	(1.44)	(1.45)	(1.59)
Number of cases of malaria	0.96	0.99	0.90
	(2.11)	(2.12)	(1.94)
Number of cases of fever	2.70	2.66	2.58
	(3.68)	(3.76)	(3.69)

Note: This table presents summary statistics on the number of villages and their characteristics, observation counts and timing, and health outcomes. The sample used varies by column. Column (1) ("Sample A") includes all observations from villages for which the month of water activation is known. Column (2) ("Sample B") uses only observations from up to 24 months before to up to 59 months after the water supply is activated, and only villages which have at least 6 observations before and after. Column (3) ("Sample C") uses only observations in a one-year window around the activation of the water supply (12 months before through 11 months after), and restricts the sample to villages with at least 6 observations within this window on both sides. Standard deviations in parentheses.

Table 2: The Effect of Water Improvement on Diarrhea

	Sample A (1)	Sample B (2)	Sample C (3)
Panel A: Mean shift (equation 2):			
Post-intervention	-0.585***	-0.587***	-0.433**
	(0.125)	(0.138)	(0.174)
Num. observations	5,408	4,077	1,605
Num. villages	95	74	71
Mean outcome pre-treatment	1.076	1.091	1.180
Panel B: Mean shift with village-specific trend (equation 3):			
Post-intervention	-0.571***	-0.550***	-0.407**
	(0.129)	(0.158)	(0.183)
Num. observations	5,408	4,077	1,605
Num. villages	95	74	71
Mean outcome pre-treatment	1.076	1.091	1.180

Note: This table presents estimates of Equations (2) (Panel A) and (3) (Panel B) in the text. The dependent variable is the number of cases of diarrhea reported as treated in a month. "Post-intervention" denotes the average effect of the intervention, corresponding to α in Equations (2) and (3). Each observation is one village-month-year. Estimation is by GLS regression. Each observation from village v is weighted by $1/N_v$, where N_v is the total number of observations of village v. The sample used varies by column. Column (1) includes all observations from villages for which the month of water activation is known (Sample A in the text). Column (2) uses only observations from up to 24 months before to up to 59 months after the water supply is activated, and only villages which have at least 6 observations before and after (Sample B in the text). Column (3) only uses observations in a one-year window around the activation of the water supply (12 months before through 11 months after), and restricts the sample to villages with at least 6 observations within this window on both sides (Sample C in the text). All regressions include village and month-by-year fixed effects. Standard errors clustered by village are reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

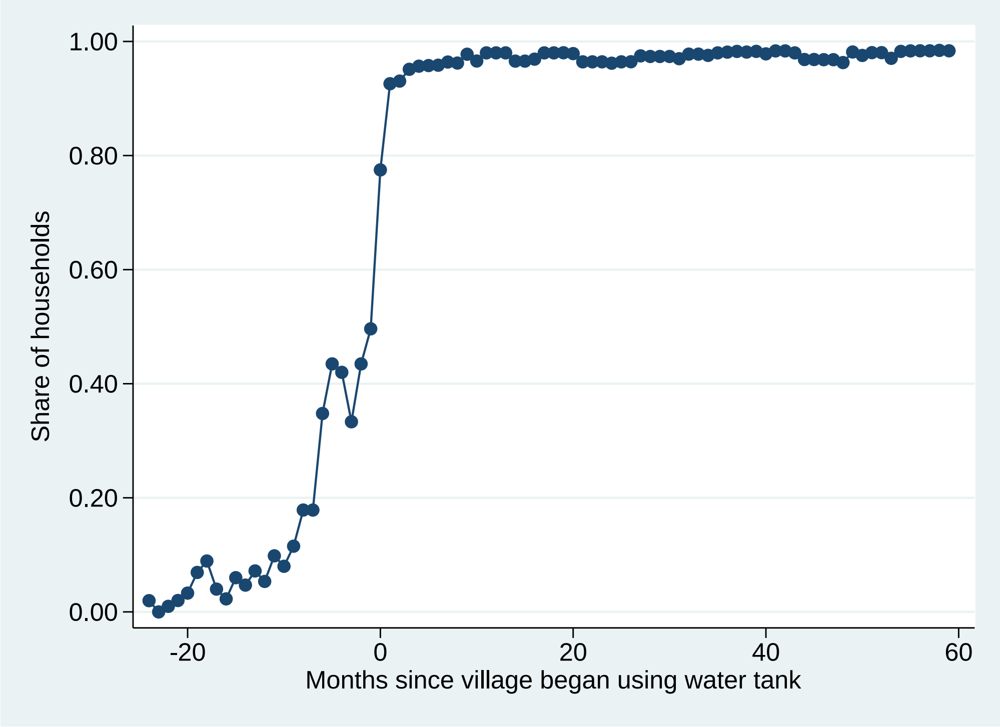
Table 3: Effect of Program by Post-Intervention Period

	Sample A (1)	Sample B (2)
Months 0-35	-0.552***	-0.539***
	(0.133)	(0.147)
Months 36+	-0.423**	-0.406*
	(0.211)	(0.229)
Num. observations	5,408	4,077
Num. villages	95	74
Mean outcome pre-treatment	1.076	1.091
p-value: coeffs. are equal	0.293	0.316

Notes: This table reports estimates of Equation (4) in the text. The dependent variable is the number of cases of diarrhea reported as treated in a month. "Months 0-35" denotes the average effect over months 0-35, corresponding to α_0 in Equation (4). "Months 36+" denotes the average effect beyond month 36, corresponding to α_1 in Equation (4). Each observation is one village-month-year. Estimation is by GLS regression. Each observation from village v is weighted by $1/N_v$, where N_v is the total number of observations of village v. The sample used varies by column. Column (1) includes all observations from villages for which the month of water activation is known (Sample A in the text). Column (2) uses only observations from up to 24 months before to up to 59 months after the water supply is activated, and only villages which have at least 6 observations before and after (Sample B in the text). All regressions include village and month-by-year fixed effects. Standard errors clustered by village are reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

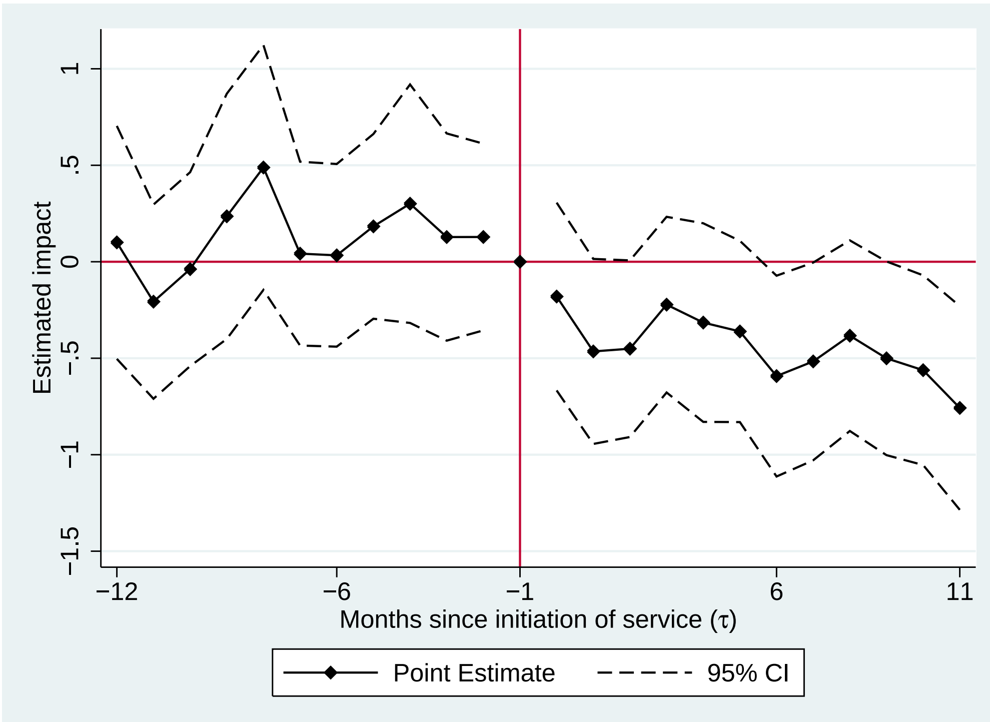
Figures

Figure 1: Share of households using latrine



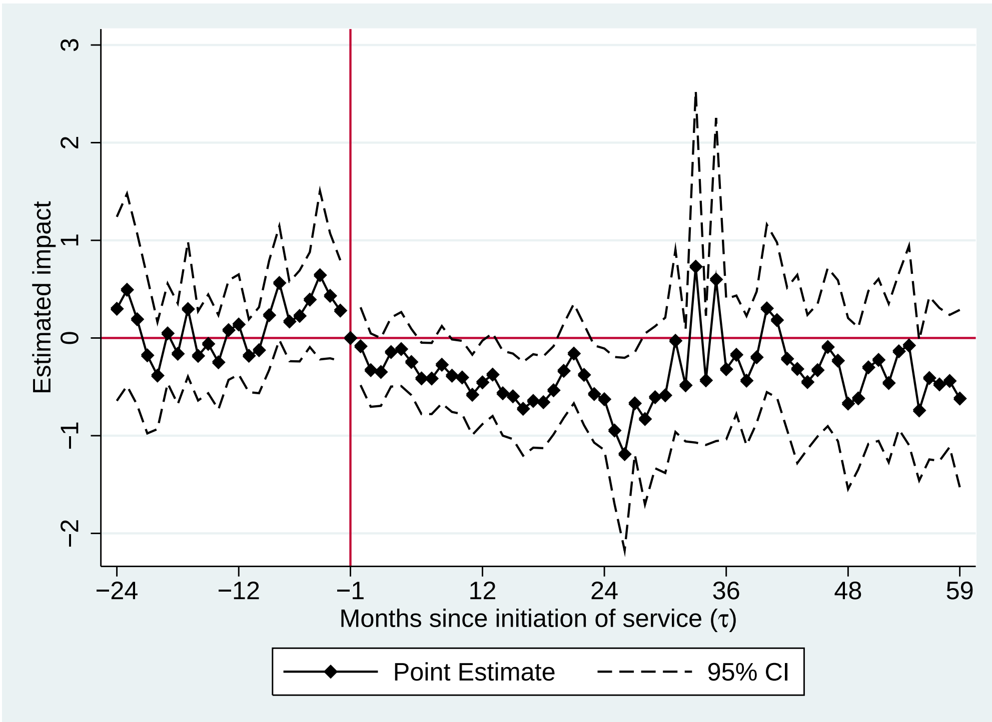
Notes: this figure shows the evolution of village-level latrine coverage around the time at which water services are activated. The x-axis represents time in months, relative to the month in which the village's water tank is activated. The water tank is activated in month $\tau = 0$. The y-axis represents the village-level share of households that own and use a latrine. The figure plots the median village-level coverage rate among villages in “Sample B”, as defined in the text, with the additional restriction that there be at least 6 observations of latrine coverage both before and after the activation of services in the window $-24 \leq \tau \leq 59$. This sample consists of 54 villages.

Figure 2: Event Analysis Within Twelve Months of Beginning Water Improvement
Impact on Cases of Diarrhea Treated Per Month



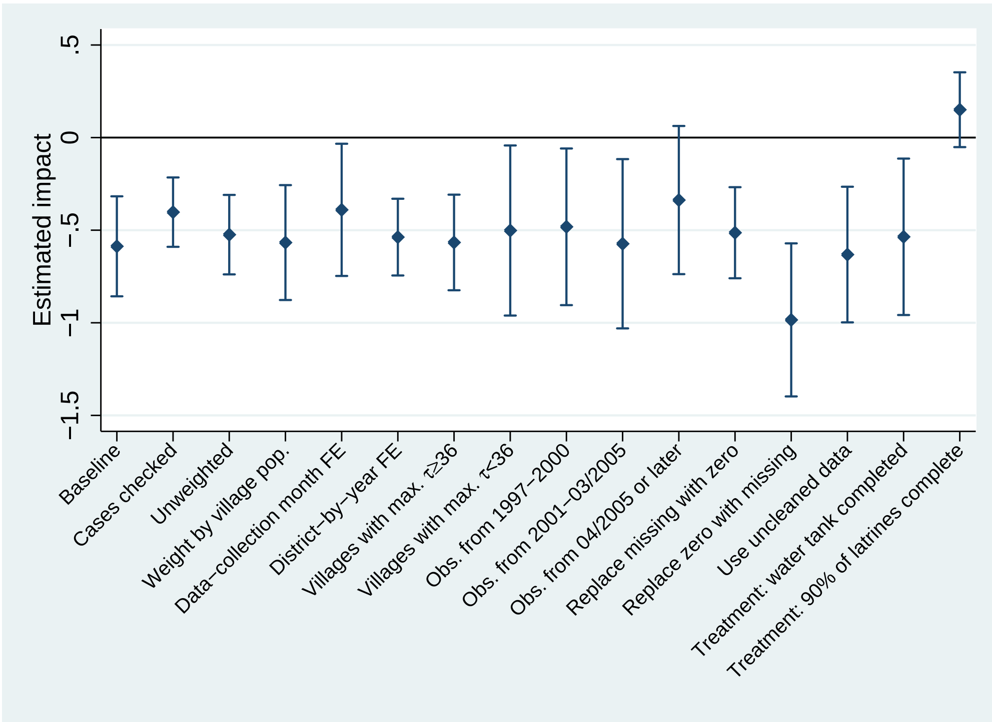
Notes: this figure describes the month-by-month estimated impact of the intervention by plotting parameter estimates for each value of τ in Equation (1) in the text. The dependent variable is the number of cases of diarrhea treated in a given month. τ represents the month relative to initiation of service, with τ_0 the month in which service began. All estimates are relative to the month prior to the initiation of service, so the estimate on τ_{-1} is normalized to zero. The regression includes village fixed effects and month-by-year fixed effects. Each observation is one village-month-year. Estimation is by GLS regression. Each observation from village v is weighted by $1/N_v$, where N_v is the total number of observations of village v. The dashed lines represent 95% confidence intervals for the individual estimates. The sample is "Sample B": only observations from up to 24 months before to up to 59 months after the water supply is activated, and only villages with at least 6 observations before and 6 observations after the activation of services. Standard errors are clustered at the village level.

Figure 3: Event Analysis from Two Years Before to Five Years After Water Improvement
Impact on Cases of Diarrhea Treated Per Month



Notes: this figure describes the month-by-month estimated impact of the intervention by plotting parameter estimates for each value of τ in Equation (1) in the text. The dependent variable is the number of cases of diarrhea treated in a given month. τ represents the month relative to initiation of service, with τ_0 the month in which service began. All estimates are relative to the month prior to the initiation of service, so the estimate on τ_{-1} is normalized to zero. The regression includes village fixed effects and month-by-year fixed effects. Each observation is one village-month-year. Estimation is by GLS regression. Each observation from village v is weighted by $1/N_v$, where N_v is the total number of observations of village v. The dashed lines represent 95% confidence intervals for the individual estimates. The sample is "Sample B": only observations from up to 24 months before to up to 59 months after the water supply is activated, and only villages with at least 6 observations before and 6 observations after the activation of services. Standard errors are clustered at the village level.

Figure 4: Estimated Impact on Cases of Diarrhea
Robustness Checks



Notes: this figure reports estimated treatment effects, with 95% confidence intervals, for each of a series of variants on Equation (2) in the main text. The variant for each specification is described briefly by the label on the x axis, and in greater detail in the text of the Supporting Information. In all cases, the sample corresponds to Sample B in the main text, consisting of observations from two years before to five years after the initiation of services ($-24 \leq \tau \leq 59$), and including only villages with at least six observations before and six observations after the initiation of services. All regressions include village and month-by-year fixed effects. Standard errors are clustered at the village level.