How valuable are environmental health interventions? Evidence from a quasi-experimental evaluation of community water projects[1]

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Abstract: The Millennium Development Goals reflect the world's collective hope and resolve to reverse a particularly pernicious, pervasive, and persistent set of problems in much of the world: high rates of diarrhea (the number one killer of small children), insufficient water and sanitation, and seemingly unsafe and myopic behaviors. Environmental health policies related to water and sanitation (W&S) must address the usual efficiency criteria (e.g., externalities), but also significant equity concerns. Health, time, and energy costs fall disproportionately on the poor, women and children. While there is an extensive literature on the appropriate theoretical model for evaluating welfare impacts, these have been rarely used in estimating gains from environmental health interventions both because adequate data for such evaluations are scarce and because few practitioners have explored how the textbook elegance of these models correspond to field realities. Thus, our paper uses a large-scale community-demand-driven (CDD) water supply project to (a) describe the challenges of welfare estimation using revealed preference data on multiple inputs and outputs, and (b) showcase a unique combination of propensity-score 'pre-matching' and rich panel data for estimating welfare impacts of a multi-dimensional environmental health project. Three years after project initiation, we found that the CDD project had had a significant impact on reported use of taps (13% increase) and toilets (7% increase). Diarrhea incidence fell significantly during the evaluation period in both project and control villages, suggesting weak health impacts of the project. In terms of economic welfare, we derive several empirical regularities related to illness and coping costs. Overall, our estimates of benefits indicate savings in coping costs equivalent to 5% of monthly expenditures, suggesting potentially significant gains from rural water and sanitation policies.

Keywords: MDG, India, propensity score matching, treatment effects, DID

JEL Classification: **H4** *publicly provided goods;* **I18** *public health and government policy;* **R2** *household analysis;* **O1** *economic development;* **O2** *development planning and policy*; **Q56** *environment and development*

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1. Introduction

The Millennium Development Goals reflect the world's collective hope and resolve to reverse a particularly pernicious, pervasive, and persistent set of problems in much of the world: high rates of diarrhea (the number one killer of small children), insufficient water and sanitation, and seemingly unsafe and myopic behaviors (UNDP 2006; Bartram 2008). Environmental health policies related to water and sanitation (W&S) must address the usual efficiency criteria (e.g., externalities, economies of scale), but also significant equity concerns. Health, time, and energy costs fall disproportionately on the poor, women and children. There is growing recognition that environmental health interventions such as water and sanitation (WSS) have fallen through the cracks and must be re-instated in the mainstream because of the strong linkages to malnutrition and poverty reduction (Lancet 2008; World Bank 2008a, 2008b). While there is an extensive literature on the appropriate theoretical model for evaluating welfare impacts, these have been rarely used in estimating gains from environmental health interventions both because adequate data for such evaluations are scarce and because few practitioners have explored how the textbook elegance of these models correspond to field realities. Thus, our paper uses a large-scale community-demand-driven water supply project to (a) describe the challenges of welfare estimation using revealed preference data on multiple inputs and outputs, and (b) showcase a unique combination of propensity-score 'pre-matching' and rich panel data for estimating welfare impacts of a multi-dimensional environmental health project. Our research is timely because policy analysis is being ramped up for estimating the costs & benefits of global RWSS delivery to meet MDGs. (Haller et al., Hutton et al., Whittington et al.).

This paper revisits some the challenges of welfare measurement (valuation) using revealed preference data. In general, the theory is far ahead of empirics as few practitioners have explored how the textbook elegance of these models corresponds to field realities. At the heart of these theoretical models is a household production framework that organizes a complex array of relationships among the multiple inputs (e.g., water quality, quantity, sanitation, hygiene education) and multidimensional outcomes that are of value to beneficiaries (e.g., health and non-health impacts such as aesthetics and status). However, field realities do not map neatly into the well-organized story of the household production framework. For example, water storage and treatment are complements to some types of water supply services (e.g., public wells) but substitutes for others (e.g., private taps), with these relationships changing at different thresholds of water quality and quantity. It is imperative to re-evaluate how preference restrictions such as weak complementarity and weak substitution apply to averting (defensive) and or mitigating behaviors and the expenditures associated with the behaviors, particularly when markets are incomplete and prices do not signal the relevant opportunity costs. The thin empirical literature on environmental health valuation has taken a naïve interpretation of the "Harrington-Portney" derivation of welfare changes.[2] Practitioners have either used avoided averting costs to approximate a lower bound on welfare effects or added these averting costs to costs of illness, productivity losses, and empirical surrogates of pain and

[2] There is no attempt to test for functional markets or to derive shadow prices where necessary (Pattanayak and Kramer, 2001).

suffering to estimate comprehensive welfare measures. However, the specifics of the program design, household production, and market context dictate whether exact and bounded welfare measures can be generated.

The remainder of this paper is organized as follow. The next section presents a conceptual framework of the welfare measurement using revealed preference data on W&S behaviors. In section 3, we describe a large-scale community-demand driven W&S program implemented in western India, and a rigorous impact evaluation of the program that applies a longitudinal, quasi-experimental research design with three principle research features. In section 4, we summarize empirical estimates of various components of household welfare based on the impact estimates generated by the PSM-DID techniques. Finally, in section 5, we draw on the lessons of the conceptual framework to develop empirical measures of the benefits of community environmental initiatives.

2. Conceptual Framework

In this section, we define "WSS value" as the compensating variation measure for an improvement in environmental health and discuss alternative measurement strategies. Following Pattanayak et al. (2005, 2007), we start with a household production model (Bockstael and McConnell 1983) which describes how nonmarket goods or public goods, such as community environmental conditions and WSH services, are combined with market goods to produce health. Household utility is a function of health status (H), consumption of private goods (z) and household income (y).

$$U=u(H, z, y) \tag{1}$$

Following Harrington and Portney (1987) and Bockstael and McConnell (2007), health status is a function of the negative effects of environmental conditions (b) on health as well as the household's ability to prevent those effects by consuming private goods (**x**) purchased on the market.[3]

$$H=H(x,b) \tag{2}$$

For example, to cope with poor water quality, households use purchased filters (x) to treat drinking water when community water quality (b) is poor. Alternatively (or additionally), households invest in storage drums to deal with intermittent water availability. Depending on the technology used to produce H, which determines the relationships between x and b, expenditures on privately purchased goods are known as averting, defensive, or mitigating expenditures in the literature (Freeman 2003).

$\hat{H}$ is health status without illness, and $S(x,b)$ is the number of days spent ill, then health status is:

$$H(x,b)= \hat{H} - S(x,b) \tag{3}$$

$S_x<0$ and $S_b>0$.

Households maximize utility [$U=u(H(x,b), z, y)$] by choosing their level of consumption of market goods subject to constraints on their income and time. Medical costs are the product of a fixed price, g, per day spent ill (S). As noted by Bockstael and McConnell (2007), the defensive expenditures (c) are endogenous, while the costs of illness (medical expenditures plus lost wages) are often assumed to

[3] If markets are incomplete, thin or imperfect in other ways, the challenge is to estimate the relevant shadow prices that determine household allocations such as how much time to spend on collecting water. These shadow prices will likely be household specific, which forces the empirical analysis to use strategies to account for endogenous variables such as these prices (Pattanayak et al., 2004).

be the exogenous consequence of illness. Some have contested this point and suggested that expenditures on illness are endogenous, in so much that they can mitigate the pain and suffering (and potential mortality). For example, home remedies such as oral rehydration therapy or visits to the village health post can help avert a potentially serious and bad health event. Gersovitz and Hammer (2004) contend that there is no meaningful difference between curative (or therapeutic) and preventative behaviors for infectious disease because both help reduce the overall pathogen load. As discussed in the concluding section, the endogeneity of illness costs significantly simplifies a major dilemma for welfare estimation.

The income constraint is:

$$\bar{y} + wT_w = z + rx + gS(x,b) \tag{4}$$

where $\bar{y}$ is exogenous income, w is the wage rate, T_w is time spent working, and r is the price of x. The price of z is 1. The time constraint is:

$$T = T_w + L + S(x,b) \tag{5}$$

where T is total time available and L is leisure time. Substituting for T_w in the equation (4), we can re-write the income constraint as

$$\bar{y} = z + rx + gS(x,b) + wL + S(x,b) - wT \tag{4b}$$

Thus the dual to the utility maximization problem is described in equation (6) below, where m is the expenditure function.

$$\begin{aligned} \min m(b,r,w,g,T,u) = \quad & z + rx + wL + wS(x,b) + gS(x,b) - wT \\ & + \mu\left[u - u\left(\hat{H} - S(x,b), L, z\right)\right] \end{aligned} \tag{6}$$

Equation 7 is a general expression of compensating variation for an improvement in b (from b^0 to b^1) caused by a program or policy intervention.

$$CV = m(b^0, r, w, g, T, u) - m(b^1, r, w, g, T, u) \tag{7}$$

When conditions improve (e.g., community water sources are rehabilitated or expanded), CV is the sum of money that that the household could give up without diminishing the utility level experienced before the improvement. Thus it is their WTP for an improvement from b^0 to b^1 (Klytchnikova and Lokshin, 2007).

There is an extensive literature about how to measure CV, particularly with the help of this underlying household production model (e.g., Bockstael and McConnell 1983, Courant and Porter 1981, Bartik 1988, Bockstael and McConnell 2007). The search for exact welfare measures is essentially a search for the behaviors that will reveal welfare effects and the preference restrictions that are required to use these behaviors to estimate welfare effects (e.g., the market good is a perfect substitute for improvements in the public good, or the market good is an essential input into H and b is only used by the household in the production of H). Given that these restrictions are frequently unrealistic (Bockstael and McConnell 2007), a more popular approach to measuring welfare effects is to use averting, defensive, or mitigating expenditures, or some combination of these expenditures, as a lower bound estimate of the true WTP.

Studies frequently rely on Harrington and Portney's (1987) expression for WTP for a marginal change in the quantity or quality of a public good:

$$\frac{\partial wtp}{\partial b} = w\frac{\partial S}{\partial b} + g\frac{\partial S}{\partial b} - \mu\frac{\partial u}{\partial S}\frac{\partial S}{\partial b} + r\frac{\partial x}{\partial b} \tag{8}$$

This expression suggests that marginal WTP is the sum of four elements:

1. the sum of avoided out-of-pocket expenditures on medical care to treat illness related to environmental quality $\left(g\frac{\partial S}{\partial b}\right)$,
2. the avoided lost income or productivity due to those illnesses $\left(w\frac{\partial S}{\partial b}\right)$,
3. the avoided defensive expenditures $\left(r\ \frac{\partial x}{\partial b}\right)$, and
4. the value of the disutility of illness $\left(\mu\frac{\partial u}{\partial S}\frac{\partial S}{\partial b}\right)$.

Many studies measure one or all of the first three elements to estimate a lower bound on WTP. Bockstael and McConnell (2007) call all of these studies and their results into question as they note that, first, the inclusion of avoided defensive expenditures is incorrect. They derive the correct marginal CV:

$$\begin{aligned} \frac{\partial \mathrm{m}}{\partial \mathrm{b}} &= \mathrm{w}\frac{\partial \mathrm{S}}{\partial \mathrm{b}} + \mathrm{g}\frac{\partial \mathrm{S}}{\partial \mathrm{b}} - \mu\frac{\partial \mathrm{u}}{\partial \mathrm{S}}\frac{\partial \mathrm{S}}{\partial \mathrm{b}} \quad \text{or} \\ \frac{\partial \mathrm{m}}{\partial \mathrm{b}} &= -\mathrm{r}\frac{\partial \mathrm{S}}{\partial \mathrm{b}} \Big/ \frac{\partial \mathrm{S}}{\partial \mathrm{x}} \end{aligned} \tag{9}$$

Second, their result applies only to marginal changes, whereas nonmarginal changes are being valued in most applications.

Bartik's (1988) bounds are used to approximate the benefits of nonmarginal reductions in *b*. Bartik shows that the reduction in defensive expenditures (i.e., expenditures on *x*) is a lower bound on the compensating variation for a nonmarginal reduction in *b,* subject to two assumptions about *x*. First, *x* does not directly affect utility. Second, the damage done by *b* can be completely mitigated by private defensive expenditures. Bartik defines the savings in defensive expenditures (*DS*) as the reduction in defensive expenditures that would return the household to the original level of *H* after the improvements in *b*. His results rely on an implicit defensive expenditure function for producing *H.* The defensive expenditure function describes the expenditures on *x* (at price *r*) that are necessary given the level of *b*.

$$c(H, r, b) = \min_x \left\{ rx \mid H = \hat{H} - S(x, b) \right\} \tag{10}$$

Bartik's main result is:

$$c\left(H^0, b^0\right) - c\left(H^0, b^1\right) = DS \le CV \tag{11}$$

Bartik's bounds rely on information about the health production technology only and not on information about preferences.

Actual defensive expenditures, which are more easily observed, are based on preferences, since households' demands for *x* will respond to changes in relative price when *b* decreases. Bartik defines actual savings in defensive expenditures (*ADS*) as the savings that occur after *b* has changed and the utility maximizing household has adjusted in its consumption of *H*:

$$ADS = c\left(H^0, b^0\right) - c\left(H^1, b^1\right) \tag{12}$$

As long as *H* is a normal good and the marginal cost of producing *H* increases with *b*, $ADS \leq DS \leq CV$.

Thus, for interventions that improve environmental conditions, either ADS (which are observed) or *DS* (which are implicit) can be used as a lower bound on CV. Bockstael and McConnell (2007) show that this result holds when Bartik's model is expanded to include costs of illness.

Bockstael and McConnell (2007) evaluate two claims regarding the relationships among defensive expenditures, COI, and CV. First, they show that when defensive behavior is possible and the income effect of illness are large, the observed change in COI [$=(g+w)(S^0 - S^1)$] due to a change in *b* may be less than or greater than *CV*. The reason for the ambiguity is that observed S^1 (and H^1) are Marshallian. The Marshallian level of S^1 after a decrease in *b* is greater than the Hicksian level S^1 would be. Since this response is not constrained to maintain utility at u^0, while compensating variation is, the relationship between *COI* and CV is difficult to predict. Second, it follows that the sum of Marshallian *COI* plus *ADS* could be either greater than or less than *CV*. Thus, this result seems to cast doubt on all studies that approximate the benefits of reducing public bad as the sum of the change in COI plus ADS.

They also show that if we could estimate a Hicksian *COI*, then it would be a lower bound on *CV* and the sum of it plus *ADS* would provide a better approximation of *CV*.

In this paper, we present estimates of households' COI and defensive expenditures for safe water in rural Maharashtra. Our estimates of defensive expenditures are based on Pattanayak et al. (2005), which is among the first studies to estimate households' costs of coping with inadequate water supply using survey data collected in Kathmandu, Nepal. They estimate the monthly costs to the household of engaging in five behaviors: collecting, pumping, treating, storing, and purchasing. Like Pattanayak et al. (2005), we present the components of these defensive expenditures, but our estimates differ in two important ways.[4] First, we calculate the costs of diarrheal illness that are likely to be associated with unsafe or inadequate WSH services. Second, our estimates will make use of the difference between post-intervention coping expenditures and pre-intervention coping expenditures to measure lower bounds on households' WTP for the improvements realized through the Jalswarajya project. Throughout this paper, we use the terms coping costs, averting expenditures and defensive expenditures interchangeably.

Section 5 describes how we estimate households' expenditures (COI and ADS) and presents results. This section evaluates whether the maintained assumptions underlying Bartik's bounds and Bockstael and McConnell's analyses are applicable in our application and discuss the implications of alternative welfare estimates.

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[4] Eventually, we will also report on the costs households incurred to acquire community-level improvements in WSH services because we are using data collected from a community-demand driven intervention that relies on cost-sharing to finance the intervention.

3. Design

The program we value is an example of how the Government of India (GoI) is intending to deliver water-, sanitation-, and health-related services to reach its "national planning objectives" related to universal access to water and reduction in child mortality and morbidity. For example, the GoI has set up an ambitious target to halve the number of people without access to safe water and basic sanitation by 2015 (GoI 2002), in excess of MDG targets. The state of Maharashtra, with support from the World Bank, has embraced a cross-sectoral community-driven approach in the Jalswarajya program (JS) to provide WSS services. Before presenting the design of the evaluation study, we describe the background on the scientific knowledge base, conditions in Maharashtra and JS.

3.1 Intervention

JS was launched by the Government of Maharashtra (GoM) with support from the World Bank to improve the state's current WSS conditions in rural areas. The project promotes community-led service provision and is based on the principles of the GoI's Swajaldhara approach. The western Indian state of Maharashtra is among the largest Indian states, with a population of approximately 100 million living in 44,000 villages. The state is also among the most developed and prosperous in India with a variety of economic activities, relatively high literacy and per capita income, and only about half the population engaged in agriculture.

In rural areas, the infant mortality rate is 51 per 1,000, and, for children under five, it is 68 per 1,000 (IIPS 2001). Twenty-three percent of children under three suffer from diarrhea, 85% have no sanitation, only 23% have a household water connection, and there is little or no treatment of water in the home. We hypothesize that these poor WSS conditions contribute to the high rate of water-related diseases such as diarrhea and consequent socio-economic outcomes.

JS's main objectives are to increase access to rural drinking water and sanitation services, institutionalize decentralized delivery of WSS services by local governments, and improve rural livelihoods. With resources from the state and district governments, Panchayti Raj institutions, national and local organizations, and the World Bank, village residents organize to make improvements in their WSS systems by choosing interventions that best meet their needs and capabilities. Villages apply to the state government to participate in the project and are selected based on three main criteria: (a) they have poor quality drinking water and sanitation services, and (b) a high proportion of disadvantaged groups, but (c) at the same time they have sufficient institutional capacity to organize themselves and carry out community activities, such as collecting fees for water supply.

JS is being implemented by the GoM from 2003 to 2009 in approximately 2,800 villages in 26 of the state's 33 districts. This very extensive effort has been designed to address the shortcomings of previous programs. Four sustainability principles guide the program:

- It is community-demand driven in that villages must apply to participate;
- Communities must share the cost of projects by paying 10% of capital costs and 100% of operation and maintenance costs;

- The capacity of local groups that include traditional institutions (e.g., Gram Panchayats) and new ones (e.g., Village Water and Sanitation Committees [VWSC] and Social Audit Committees) is strengthened to improve the quality of participation; and
- Decentralized decision making involves community members not government bureaucrats or sectoral technocrats (e.g., in a NGO).

JS is being implemented in five overlapping phases. The pilot phase comprised 30 villages in 3 districts; Phase I comprised 225 villages in 9 districts; the remaining 17 districts will be covered in subsequent phases and batches until the target number of 2,800 villages is met. Our study uses data from a subset of 242 villages from four districts (Buldana, Nashik, Osmanabad, and Sangli) in Phase I of the program.

The intervention begins with selection of the villages and then progresses through three additional stages. First, communities participate in a pre-planning and mobilization that culminates in the establishment of the VWSC. Second, the VWSC plans interventions, subject to review and approval, and launches implementation, with the social audit committee paying attention to procurement, construction, and financial management. Finally, VWSC establishes ongoing, continuous operation and maintenance procedures and systems. It is up to each village to customize its package of activities and outputs. In practical terms, each community is expected to make improvements in all three basic components: water, sanitation, and hygiene, with the specific goal of ending the practice of open defecation.

3.2. Evaluation Design

The hypothesis to be tested in this evaluation is whether the program outputs – which are the water-sanitation-hygiene packages – will bring about improvements in child health (measured by diarrhea prevalence and anthropometrics) and overall welfare (e.g., time savings). The evaluation must rule out any confounding influence of mediating and intervening factors. This outcome is ensured through a combination of study design, sample selection, data collection, and proposed analysis that are summarized next. The full details of the evaluation design are available in Pattanayak et al. (2007).

Identifying control communities

We used a combination of restrictions, stratification and matching to reduce sampling bias in the choice of communities for our survey. First, we eliminated from the JS districts those which were urban or coastal. One district was chosen from each of four geographically different regions: Osmanabad (Marathwada), Nashik (Mumbai), Sangli (Western Maharashtra), and Buldana (Vidarbha).
Second, from this sample of districts, each project (treatment) village was matched with two observationally similar non-project (control) villages, using PSM. Unlike previous applications of PSM in data analysis, we use the method in the design stage. The limitation of post-intervention PSM is its reliance on secondary data for baseline measures, or even the complete lack of baseline data altogether when the relevant data are not available from secondary data (Lokshin & Yemtsov 2005). Pattanayak et al. (2007) present full details of the data integration, propensity score estimation, matching, and evaluation of the match.

In summary, a logit model of project participation, estimated on a pooled sample of about 7000 project villages and all non-project villages, generate propensity scores. Our choice of pre-determined variables for propensity score estimation relied on factors that might influence (or proxy for) the three eligibility criteria for being a JS participant and what was available. These are used to find a non-JS village with the closest propensity score to each of the 90 JS villages in our sample. We enforce the region of common support and trim the score distribution to reduce bias. Furthermore, to account for district fixed effects, we also restrict a second matched control within the district. The matching strategy was checked for balance in key covariates across JS and matched non-JS villages and the reduction in average bias (or difference) across all matching variables. Table 1 confirms that these criteria were satisfied for all villages retained for further evaluation. Next, we compared all matched pairs of program and potential control villages for remaining bias in key variables (water supply and percent of socially disadvantaged groups) and eliminated pairs with statistically significant differences. This eliminated 27 villages from the pool.[5]

The bias reduction is confirmed with baseline household survey data. A comparison of means reveals no statistically significant differences between treatment and control villages in terms of a number of indicators including health outcomes, WSS conditions, personal hygiene behaviors, and perceptions of local health and environmental problems (see Table 2). However, because of baseline survey was conducted after early sensitization in treatment villages, these villages were somewhat different in three factors: (a) exposure to public health messages, (b) self-reported identification of the main problem, and (c) community participation. Thus, the double difference strategy for analysis will be critical to account for any baseline differences.

The final sample of 242 villages comprised 95 treatment villages (i.e., 2 pilot villages and 93 Phase I villages) and 147 control villages. The matching process ensures that the villages are comparable in terms of several observable criteria. Household selection in each of these villages is described in the survey implementation section below.

Sample Size

The goal of sample size calculations is to identify the minimum efficient number of observations needed to ensure adequate statistical power. Since the primary research question focuses on health outcomes – in particular, diarrhea rates in children under five years of age, we compute the size of the sample necessary to measure these health effects. Because the interventions happen at the village level and the primary outcomes are measured on individuals nested within those villages, we employ procedures for power calculations appropriate for group interventions that introduce two issues (Blitstein et al. 2005). First, there is an inverse relationship in the calculations between the number of villages and the number of households required from each village. This relationship parallels the field trade-offs between the number of villages and the number of respondents per village.[6] Second, data will be

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[5] Shadish et al. (2002) argue that the success of the matching methodology depends on two critical features: (i) the selection of control groups from a homogeneous pool, and (ii) the use of stable and reliable variables. The restriction and stratification processes enable us to meet the first criteria. Because there is a very large sample of non-project villages (>1500 in each district), we did not run into degrees-of-freedom problems in establishing matches. Further, our use of village level aggregates and multivariate matching improve the stability and reliability of the data used in matching.

[6] On the one hand, it is usually more difficult logistically and expensive to include more villages and fewer households per village than it is to sample fewer villages and more respondents per village. While maximizing the

correlated between respondents within the same village who share histories and common experiences that make them more alike to each other than they are to respondents in another village. If this intracluster correlation coefficient (ICC) is very small and the number of respondents per village is also small, the design effect (DEFF) would be close to 1, indicating little additional variation.

Sample size estimation involves a number of parameters and assumptions. These include: (a) the Type I and Type II error rates, reflecting the evaluator's willingness to reject a true null hypothesis and to accept a false null hypothesis, respectively; (b) the anticipated effect size estimate; and (c) the anticipated ICC. We set the Type I error rate at 0.10, and the Type II error rate at 0.20 to provide a test of the intervention effect with 80% power to identify statistically meaningful differences between intervention condition. Further, we intend to employ a two-tailed test when we assess the effect of the intervention. This last assumption is conservative in the sense that it places a heavier onus on the evaluation, but is appropriate in field trials where it would be important to observe intervention effects that are not in the desired direction.

Based on Fewtrell et al.'s (2005) meta-analysis of the health impacts of similar interventions, we assume an estimated effect size of 30% (the approximate mid-point of their range of effectiveness). When an effect size estimate is based on a percent change, it is also important to understand and incorporate information on the current prevalence rates as this provides an estimate of the baseline diarrhea rates in the study population. The National Family Health Survey-II data (IIPS and ORC Macro 2001) indicates a child diarrhea rate of 22 percent in rural Maharashtra. We obtain our estimate of ICC from the literature. Katz et al. (1993) examined the clustering of diarrhea rates at the village-level in several developing countries to estimate the DEFF. With cluster sizes standardized to 50 households, the DEFF ranged from 1.38 to 4.73. This suggests that the ICCs in the range of 0.008 to 0.076. Hence, we used an ICC of 0.05, a conservative estimate within the range indicated by the Katz et al. meta-analysis. Our previous work in the region suggests that we can expect 10 percent loss to follow-up or non-compliance.

Given this set of parameters, our sample size calculations indicate that sampling approximately 50 households with children five years of age or younger in each village will generate a sample with 80% power to detect an intervention impact of 30% or greater in a population with a baseline diarrhea prevalence of 22%. This implies we need an overall sample of 3,000 individuals per intervention or a total of 9,000 individuals to evaluate potentially 3 interventions (as matched pairs of treatment and control) that villages may adopt under JS.

It is important to recognize that these calculations are based on best available information and buffered by a number of reasonable assumptions to help us protect the desired goals regarding statistical

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number of villages reduces the overall sample size, it also increases the costs of transportation during data collection. Another advantage of the sampling more households from fewer villages stems from the fact that, while the intervention takes place at the community level, the decision to use improved services (e.g., a toilet) is made by a household. The proportion of the population and the sample that would be using the intervention at the endline survey is uncertain. Sampling a larger number of households in each village increases the likelihood of interviewing users, which permits investigation of factors affecting usage. On the other hand, it is usually advantageous to maximize the number of villages and reduce the number of households per village to increase the amount of independent data (which boosts the power of the inference) and distribute the potential bias more evenly across intervention categories. Furthermore, the study is less vulnerable to statistical problems if projects are not completed on time and entire villages drop out of the project.

power and the planned tests of intervention effectiveness. For example, we incorporate conservative assumptions regarding the reduction in study level variation associated with taking repeated measures on respondents and villages. Further, the addition of covariates related to the outcome can further reduce random variation. These factors can improve statistical power and their place in the final evaluation will help to protect our analysis in the event that our parameters are very different from their assumed values (e.g., if the diarrhea prevalence rate is lower than the estimated 22 percent or the treatment effect is lower than 30%, the sample size would be too small).

Survey Design

The cornerstone of the study is high quality measurement of key biological, socio-economic, cultural, and environmental indicators. Quality is attained by careful design and field testing of the survey instruments, rigorous training of the field enumerators and supervisors, and checking and verification efforts in the field and at the data entry stage (Wassenich 2007). Collectively, such efforts can consume as much as 9-12 months of calendar time for a study of this scale. These indicators were measured at the individual (e.g., sex, age), household (e.g., class, caste, assets, education, and quality of community water supplies and water stored in households), and community levels (e.g., roads, clinics, schools, credit, and source water quality).

We designed the household and community survey questionnaires based on survey instruments we had developed previously, literature reviews of WSH studies, and advice from local advisors. Preliminary versions of the questionnaires were reviewed in focus group discussions with selected individuals, key informants, and households. The questionnaires were revised and pre-tested in the field before they were finalized.

The household questionnaires were designed to collect data on outputs, outcomes, and impacts. Outputs and outcomes include water, sanitation, or hygiene interventions. Impact indicators include child health as measured principally by diarrhea among children under five. A child was classified as having diarrhea if, during the two weeks prior to the survey, a household caretaker reported that the child had had three or more loose stools in a 24 hour period. Data was also collected on child growth, personal benefits, and variables to compute cost savings (e.g., time savings). Data were collected on a range of individual covariates (sex, age, class, caste, religion) and household variables (family size and composition, education, housing conditions, asset holdings, occupation and expenditures, services, sanitation practices, water storage and treatment practices).

The community questionnaire asked for information on community-level infrastructure, such as roads, electricity, environmental sanitation, water sources, employment opportunities, clinics and health care facilities, schools, credit availability and markets. Information was also gathered on key governmental and nongovernmental programs and local government size and composition. Key informants were village heads, governing council members, and members of WSS committees, if they existed.

Finally, water samples were collected from both community sources and household storage containers and tested for microbial contamination. The questionnaires were field-tested and the enumerators were trained through lectures, role-plays, and field practice. Water sample collectors received special training. A senior field manager was in charge of all survey teams and the development of field routing plans. The manager was supported by four field executives who directly supervised the enumerator teams (each consisting of seven enumerators, a person responsible for the water survey and water sample collection, and a supervisor).

Data Collection

Baseline data were collected in two phases in 2005, before the monsoon (May-June) and after (August-September). The same sample was surveyed in both seasons. Households were selected before the start of the baseline surveys in May 2005 using two steps. We listed and mapped all households in each of the selected villages. Following that, all households with at least one child under five years of age were identified. Because there were no pre-existing data on households with children under five, house-to-house visits had to be made to identify them. It took 20 teams of 4 people each 15 days to complete this labor-intensive process. In villages with 50 or fewer eligible households, all were interviewed. In villages with more than 50, a random sub-sample of 50 was interviewed. If a household was not available for an interview, then it was visited the same day or the next day at different times for up to three follow-up visits. If a household was not found or if an interview was refused, the next household was selected according to a pre-established procedure.

In total, 210 people were involved in conducting approximately 9500 household surveys and 240 community surveys. Over 6,000 water samples were taken and transported to the lab within 24 hours. The household survey took on average 120 minutes to administer; the community survey, 150 minutes; and the water quality tests, 10 minutes.

4. Impacts of Jalswarajya

Here we summarize the main results of the impacts of the *Jalswarajya* program, which are reported in details in Pattanayak et al. (2008). Overall, Jalswarajya has had a moderate, but significant impact on reported use of taps and toilets. The DID estimates show a 13 percent increase in intervention villages in dry season tap use and a 7 percent increase in toilet use (and corresponding decrease in open defecation). There are some seasonal differences in knowledge and practice, and consequently on health outcomes. In the dry season, there was some increase in safe water handling, but no corresponding improvements in the potentially more contagious rainy season. Per capita consumption of water increases in the dry season as well. In general, prevention behaviors such as hand washing and a variety of safe water handling and storage do not change with the introduction of the project. Consequently, there was some reduction in microbial contamination in the dry season in project villages. In contrast, control villages see a greater decrease in E.coli contamination in the rainy season (compared to the project villages), presumably because of reduced prevention behaviors and increased exposure due to incomplete toilet coverage in project villages.

Importantly, diarrhea incidence fell significantly during the evaluation period in both project and control villages. This general decline reflects overall socio-economic development in rural Maharashtra as well as routine and general purpose water and health programs. Thus, this general socio-economic improvement combined with limited behavioral change in project villages explains why, on average, the study found weak or no child health impacts as measured by diarrhea in these villages compared to control villages.

These changes have implications for the economic welfare of the target households. We attempt to estimate these welfare impacts through two sequential tasks: 1) calculation of coping cost (or, defensive expenditures) and cost of illness measures, and 2) the estimation of causal impacts using a difference in difference strategy (DID) on this PSM pre-matched sample. These two tasks are summarized in the next sub-sections.

4.1 Calculating Coping and Illness Costs

To estimate costs of coping costs due to inadequate WSS services and private cost of illness (COI) due to water-related illness, we basically follow Pattanayak et al. (2005) and Poulos et al. (2008). Monthly coping costs are calculated based on four types of household behaviors. These include costs of (1) time spent on collecting water, (2) time spent on defecation, (3) water treatment (i.e., filtering, boiling, and use of chemicals), and (4) water storage. Private COI (i.e., costs borne by private households rather than the public health system) due to the most recent episode of diarrhea is the sum of out-of-pocket expenditures and lost income, or economic productivity. The lost income was estimated by monetizing unproductive and lost work days using the average wage rate in each village. For patients and caregivers who did not earn wages or salaries, we use the average wage rate to value the opportunity cost of the time spent ill or caring for patients. Figures 1 and 2 present an overall picture of costs of coping and illness. However, it would be difficult to interpret these results or understand the patterns without an understanding of the calculations described below.

The cost calculations require estimation or assumptions related to the opportunity cost of time, the operating life of consumer durables (e.g., water storage containers), and discount rates. Here, we present a summary of the preliminary findings. All costs are reported in terms of monthly household costs in 2007 Rupees.

Coping Costs

The first component of coping costs is the monetized value of household water collection. Household water collection involves traveling back and forth and waiting at water sources. Among various water sources, public wells were the most popular source, followed by private taps and public taps. On average, households took 5-6 trips to a nearby public well or tap each day, and each round trip took more than 60 (40) minutes in the 2005 dry (rainy) season. In 2007, households took an average of 6-7 of trips to a public source and each trip took about 40 (30) minutes in the dry (rainy) season. In most cases, women in the household were the primary water collectors. To monetize the time spent collecting water, we multiplied the number of hours spent walking to and from water sources by the number of trips taken by one-half of the village-level gender-specific average hourly wage (see Whittington et al. [1990] for a similar strategy). The average hourly wage for male (female) workers is about 8 (5) Rupees. The time that children spent collecting water was not monetized. Table 3 shows that the average monthly time costs are about 650 (550) Rupees in the 2005 dry (rainy) season and 290 (200) Rupees in the 2007 dry (rainy) season.

The second component of coping costs is the monetized value of the time household members spent walking to and from and waiting at the main sanitation site. In the 2005 dry (rainy) season, 86% (84%) of households traveled to fields to defecate as their main sanitation practice (open defecation), 12%

(15%) used IHL, and 2% (1%) relied on a community or neighbor's toilet. In the 2007 dry (rainy) season, 73% (73%) of households defecated in the open, 24% (24%) used IHL, and 3% (2%) still relied on community or neighbor's toilet. To estimate the travel costs, we assumed each household member made only one trip per day to the main sanitation site. We then aggregated the total sanitation-related travel and waiting time to the household level (the average household size is 6 in 2005 and 7 in 2007), and multiplied it by one-half of the village-level average hourly wage. Again, the time children spent was not monetized. The household-level mean monthly time costs related to the use of sanitation facilities are 164 (177) Rupees in the dry (rainy) season of 2005 and 163 (169) Rupees in the dry (rainy) season of 2007.

The third component of coping costs is the cost of household water treatment. In this study, filtering (e.g., with cloth or simple candle and two chamber tank) was the primary form of household water treatment. Less than 5% of the households boiled or chemically treated their drinking water. For households that used filters, we converted the one-time cost of filters into monthly costs by amortizing the reported value over the respective lifespan at an estimated annual discount rate of 15%. On average, the monthly cost of filtration is 2 Rupees. For households that boiled their water, we relied on an estimate of the cost of boiling 5 liters of water reported in Rose et al. (2006) (i.e., US$068 [27 Rupees][7]), and assumed 5 liters of boiled water per day provided enough drinking water in these households. The mean monthly cost of boiling is 30 (25) Rupees in the dry (rainy) season of 2005 and 9 (13) Rupees in the dry (rainy) season of 2007. Finally, households who chemically treated their drinking water spent no more than 2 Rupees per month.

The fourth component of coping is storage. Almost all households stored water at home. In our survey, households reported the number of storage containers they owned by type.[8] The costs and the useful life of storage containers were collected at the village level. For each type of storage containers, we first amortized the storage costs over the respective lifespan at a discount rate of 15%. In general, the mean monthly storage cost is about 230 Rupees for a pot, 200 for a bucket, 560 for a drum, and 190 for all other types of containers. In terms of the average operating life, pots and buckets last about 11 years, drums last 14 years, and all others last 7 years. We then multiplied the implied monthly costs by the number of each type of containers household owned. On average, households incurred monthly expenses of about 160 (120) Rupees in the 2005 dry (rainy) season and 140 (110) in the 2007 dry (rainy) season.

As shown in Figure 1, the average total coping costs are lower in 2007 than in 2005. Households in treatment villages experience a greater reduction in average costs between survey rounds. This can be attributed to the fact that more households were using private taps and IHLs in 2007, compared to 2005; thus, the reduction in travel and waiting time for water collection and sanitation, as well as water treatment cost savings.

Cost of Illness

Thirty percent of the households had at least one diarrhea case in the two weeks prior to their interview in 2005, compared to about 20% in 2007. The first component of the costs of these episodes is

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[7] In 2006, the exchange rate is 40 Rupees to US$1.

[8] In the next draft, we will report % households using each storage type, number of each storage type households owned, and average size of each storage type.

the out-of-pocket medical expenses. These costs included clinic/hospital fees, medicines, transportation, lodging, and meals. On average, households spent 245 (599) Rupees in the 2005 dry (rainy) season. In the rainy season, this expenditure was 181 (80) Rupees in the dry (rainy) season (Table 4).

The second component is the lost income due to diarrhea episodes by the patient's themselves. Twenty-two percent (19%) of households missed at least one day of work in the dry (rainy) season and 19% (13%) in the dry (rainy) season of 2007 due to the most recent episode of diarrhea. As shown in Table 4, these lost work days among adult patients translate into 53 (45) Rupees per household in the 2005 dry (rainy) season and 45 (32) Rupees in the 2007 dry (rainy) season. While some children missed school and/or economically productive activities while they were ill, these losses were not monetized for persons.[9]

Care-giving resulted in additional lost work days when household members cared for the sick. During the most recent episode of diarrhea, households missed an average of 1.4 (1.6) work days in the 2005 dry (rainy) season and 1 work day (about 5 hours of work) lost in the 2007 dry (rainy) season. On average, the lost work days are equivalent to 71 (74) Rupees in the dry (rainy) season of 2005 and 50 (32) Rupees in the dry (rainy) season of 2007.

As shown in Figure 2, COI fell in 2007 compared to 2005. This can probably be explained by the decrease in diarrhea cases in 2007. However, there is no clear difference in COI reductions between households in treatment and control villages, which is also confirmed by a DID analysis discussed in the next sub-section.

4.2 Estimating welfare impacts

The evidence we have presented indicates that changes are visible in some coping and illness costs. As hinted in the introduction, DID analysis forms the cornerstone of our plan to determine if the changes were caused by Jalswarajya program or just due to secular trends. The pre-post data collection plan allows us to use a DID estimator and measure the "treatment effect" by comparing the treatment and control communities before and after the intervention (Heckman et al. 1998). The DID estimate is the mean difference in the change in the outcome across the intervention and control communities. That is, we can difference the outcome values for the intervention and their matched control communities at post-intervention levels and then subtract any pre-existing differences in outcome values:

$$\mathrm{DID} = \{E[Y_{1t} \mid p(X)] - E[Y_{1c} \mid p(X)]\} - \{E[Y_{0t} \mid p(X)] - E[Y_{0c} \mid p(X)]\}$$

where Y is the outcome with subscript 1 and 0 for post-treatment and pre-treatment levels, and subscripts t and c for intervention and control unit outcomes. E is the expectations operator suggesting that this is the expected treatment effect across all treatment units. It is conditional on the propensity score of participation, p(X), which depends on all relevant covariates (X) included in the first stage estimation. DID estimators are often implemented in a regression framework by including an interaction variable for the study condition (d) and for the treatment period (T):

[9] Our definition of child includes those under 5 years of age. Thus, even a 6-year old's time is valued at 50% of market wages. In future calculations, we will calibrate the fractional wage to reflect differential wage loss. Note, children do paid work in our study area.

$$Y_{ijt} = \alpha + \beta Z_{ijt} + \gamma T_{jt} + \delta d_{jt} + \kappa T_{ijt} * d_{jt} + U_{ijt}$$

The primary coefficient of interest κ measures the pre to post change in the outcome for the affected households relative to pre to post change in the outcome for the unaffected households.
Covariates (Z) that were not balanced in the baseline are included as additional controls in the estimation. In future analysis, the regression-adjusted matching-DID estimator will be implemented. The overall strategy is to account for observable differences by matching and for time-invariant unobservable differences by differencing between treatment and control households (Heckman et al. 1998).

This analysis will generate 'intention-to-treat' (ITT) estimates of project impacts. The ITT estimates are relevant when we are interested in the broad impacts of the program, factoring the fact that only a subset of households will 'comply', for example, by installing intermediate outputs such as taps or toilets. This partial sample of households is as many as the project can hope to reach through its operation. Thus, this estimate is different from the welfare impact of a household obtaining a tap or a toilet *per se*. The latter is an important question, but not one answered in the current analysis.

A DID analysis of coping cost components and overall coping costs are reported in Table 5. The key parameter of interest is the size and significance of the interaction term. The results confirm that the average coping costs for households in the treatment villages were 180 Rupees lower than those in the control villages. Most of these savings are due to decreased travel and waiting times to water sources (about 150 Rupees savings) and the main sanitation site (about 40 Rupees savings). The results vary by season.

A DID analysis of COI components and overall COI are reported in Table 6. We see no impact of the project in the dry season. Analysis of rainy season data suggest that households in the control villages actually experience a bigger cost reduction than those in the treatment villages although both treatment and control villages incurred lower COI in 2007 than in 2005.

5. Discussion

This paper reports average monthly household-level cost savings that were caused by Jalswarajya, a community-demand-driven water, sanitation, and hygiene program in the state of Maharashtra in India. To our knowledge, there have been few previous studies papers that have measured these costs. Pattanayak et al. (2005) were the first to estimate the monthly household costs of coping with inadequate water supply using an observation cross-sectional data set from Katmandu, Nepal. Particularly, they were not able to measure the cost savings due to improvements in WSH services in their study. Whittington et al. (2008) used the estimated savings in coping costs (the value of time spent traveling and waiting) due to improved services, savings in costs of illness due to reduced illness, and the value of avoided deaths due to reduced illness to estimate the benefits of global rural WSH programs. While their study estimates how these costs have changed in relation to rural WSH programs, much of their analysis is based on assumptions about program impacts rather rigorous measures of causal effects. Indeed, there are few studies that have measured the causal effects of community-demand driven WaSH programs and none, to our knowledge, that have estimated the economic value of those causal impacts (Poulos et al. 2006). This study addresses this knowledge gap by measuring the economic value of the average treatment effect caused by a CDD WSH program. The advantage of this is that we can avoid excessive attribution of behavior change and economic benefits to the program – a conclusion that would be reached on the basis of typical multivariate regression analysis of observational data on connections to piped sewage and water.

Our rigorous quasi-experimental impact evaluation of Jalswarajya relied on several features, including: (a) seasonal collection of panel data on households, communities and water quality before the intervention (in 2005) and after the intervention (in 2007); (b) sufficient sample size (four repeat measures of approximately 9500 households in 242 communities); (c) the use of propensity score matching to match JS villages with observationally equivalent comparison villages; and (d) a panel based difference-in-difference (DID) estimation strategy controlled for pre-existing differences in project and control communities.

Three years after project initiation, we found that Jalswarajya had had a moderate, but significant impact on reported use of taps and toilets. The DID estimates show a 13 percent increase in intervention villages in dry season tap use and a 7 percent increase in toilet use (and corresponding decrease in open defecation). As described in section 4, the estimated impacts of Jalswarajya varied by season. Diarrhea incidence fell significantly during the evaluation period in both project and control villages. This general decline reflects overall socio-economic development in rural Maharashtra as well as routine and general purpose water and health programs. Thus, as a consequence of the general socio-economic improvement combined with limited behavioral change in project villages, the health impacts of JS, as measured by changes in diarrhea incidence rates, were weak.

In section 4, we present estimates of the average monthly cost savings realized by households. We derive five key empirical regularities from these findings:

(1) Both coping costs and COI declined over time. On average, coping costs declined between 364 and 384 Rs. from 2005 to 2007, depending on the season. The average COI declined 108 to 547 Rs., depending on the season.

(2) The largest component of coping costs was the value of time spent traveling to and waiting at water sources and the largest component of COI was out-of-pocket medical expenditures. The value of time spent collecting water accounted for about 65% of coping costs in 2005 and 41 to 48% of coping costs in 2007 (depending on the season). The next two largest components of coping costs were the value of time spent traveling to and waiting at sanitation sites and the costs of water storage, which accounted for 15 to 35% of coping costs depending on year and season. The out-of-pocket medical expenditures about 70 to 90% of COI. The next two largest components of COI were patients lost income and then caregivers' lost income, in that order.

(3) Cost savings differed by season. The dry season coping cost savings were a slightly larger greater than the rainy season coping cost savings, due to time savings in water collection, as well as reduced treatment (boiling) and storage costs. In contrast, the rainy season savings in COI are much (about five times) greater than the dry season savings in COI.

(4) For the most part, the savings in coping costs were larger for better-off households. Savings in coping costs and COI were greater for households above the poverty line than they were for households below the poverty line. Similarly, the cost savings were greater for open caste households than for households belonging to scheduled castes and scheduled tribes. However, COI savings were greater for BPL households in non-JS villages in the rainy season. In JS villages, COI savings were greater for BPL households and SC/ST households in the dry season.

(5) Finally, our preliminary DID analyses indicate that Jalswarajya caused a 180 Rs. savings in average monthly coping costs in the dry season and no savings in COI. These are based on analyses of the full sample, regardless of the type of intervention the villages received as part of JS. Other analyses (not presented in this version of the paper) suggest that the causal impacts vary across subpopulations and across the types of interventions introduced in JS villages.[10]

Another motivation for this paper is to revisit some of the challenges of welfare measurement (valuation) using revealed preference data. There is a thin empirical literature on revealed preference measures of the value of WSH improvements. This literature, and other revealed preference approaches to environmental health valuation, has mis-interpreted Harrington and Portney's (1987) derivation of the compensating variation for a marginal change in environmental quality. Practitioners have either used coping cost savings or COI savings to approximate a lower bound on welfare effects. Others have approximated the lower bound by adding coping costs savings to COI savings. Bockstael and McConnell (2007) are unequivocal in there assessment that COI cannot be used as a lower bound estimate of CV, when those COI are exogenous to the household. Under these conditions, theory suggests that COI may be either lower than or higher than CV, which limits its usefulness in policy analyses.

In light of their conclusions, the savings in coping costs from the JS program are a lower bound estimate of the average monthly economic of JS to beneficiary households. Given the findings to date, this implies that 180 Rs. is a lower bound estimate of the average monthly CV. But, how should the savings in COI should used, if at all, in estimating the welfare effects? The answer to this question depends on whether the COI due to diarrhea in our sample are exogenous or not. If so, they should be ignored. If the COI are endogenous, then Bockstael and McConnell (2007) argue that these may be classifies as additional defensive expenditures. Thus, if COI is endogenous, the sum of coping costs savings and COI savings would be an underestimate of CV.

The treatment of COI estimates in welfare estimation depends on the analysts' judgments about whether COI is endogenous or exogenous. The COI due to an episode depends in part on factors that are beyond the households control, such as the severity of the infection and the susceptibility of the patient (determined by age and exogenous health capital), as well as factors that the household can control, such as whether the patient receives early treatment to prevent the development of more severe illness. The relative contributions of exogenous and endogenous factors are expected to vary by disease, and household characteristics. For instance, COI may be considered to be exogenous when a household's treatment choices are limited, such as when a patient gets cholera, which is characterized by acute, rapid onset diarrhea. Without rehydration, the risk of death is relatively high, compared to other diarrheal diseases. Similarly, household income may constrain the household's ability to treat illness, or household preferences may favor curative, rather than preventive care. In the south Asian context, there may even be cultural factors where a belief in fate and destiny may interfere with rational intervention. Thus, logic is insufficient for resolving the question. Whether COI are endogenous is an empirical question that may be resolved by analyzing variation in COI by disease severity and household characteristics, and by

[10] Future analyses will examine all welfare impacts by the type of interventions adopted by the community – e.g., sanitation, water, hygiene education, and different combinations of the same

exploring the correlation between medical treatment and work loss decisions and household and individual characteristics. We plan to explore this further in future work.

There are several other unresolved issues in revealed preference valuation of environmental health improvements. First, while Courant and Porter's (1981) results for marginal environmental changes (b) are appropriate regardless of the relationship between b and the market commodity (x) in the health production function, Bartik's (1988) results apply when x and b are substitutes. Analogous results bounding CV are not available when x and b have different relationships in health production, so models with alternative preference restrictions (e.g., weak substitution – See Smith et al. 2008) should be explored. Second, all of the models used to derive welfare estimates assume that all x have the same relationship with b in the health production function (e.g., all are x are substitutes for b; or x is an essential input for H). However, there may be a vector of market goods that have different relationships with b in . None of the available analyses offer guidance on how to treat expenditures on these multiple goods when estimating welfare. Finally, Bockstael and McConnell conclude that the sum of a Hicksian COI plus ADS would be a better approximation of CV than either component alone. In ongoing work, we are considering the use of structural models to derive "Hicksian COI" and exploring the differences between welfare estimates that use these measures and those that use Marshallian COI measures.

Despite the fact that these issues remain unresolved, this paper makes an important contribution to what we know about the economic value of WSH improvements. These preliminary results indicate that the average monthly economic value of JS to beneficiary households is 180 Rs. This is equivalent to about 4.5 US$ (in 2007 terms). Survey data indicate that the average monthly expenditures in our sample (a proxy for income) were 4,000 Rs. Thus, our benefit estimates represent about 5% of monthly income, suggesting a potentially big gain from rural water and sanitation policies.

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Figure 1. Monthly Household Coping Costs by Intervention, Season and Survey Year

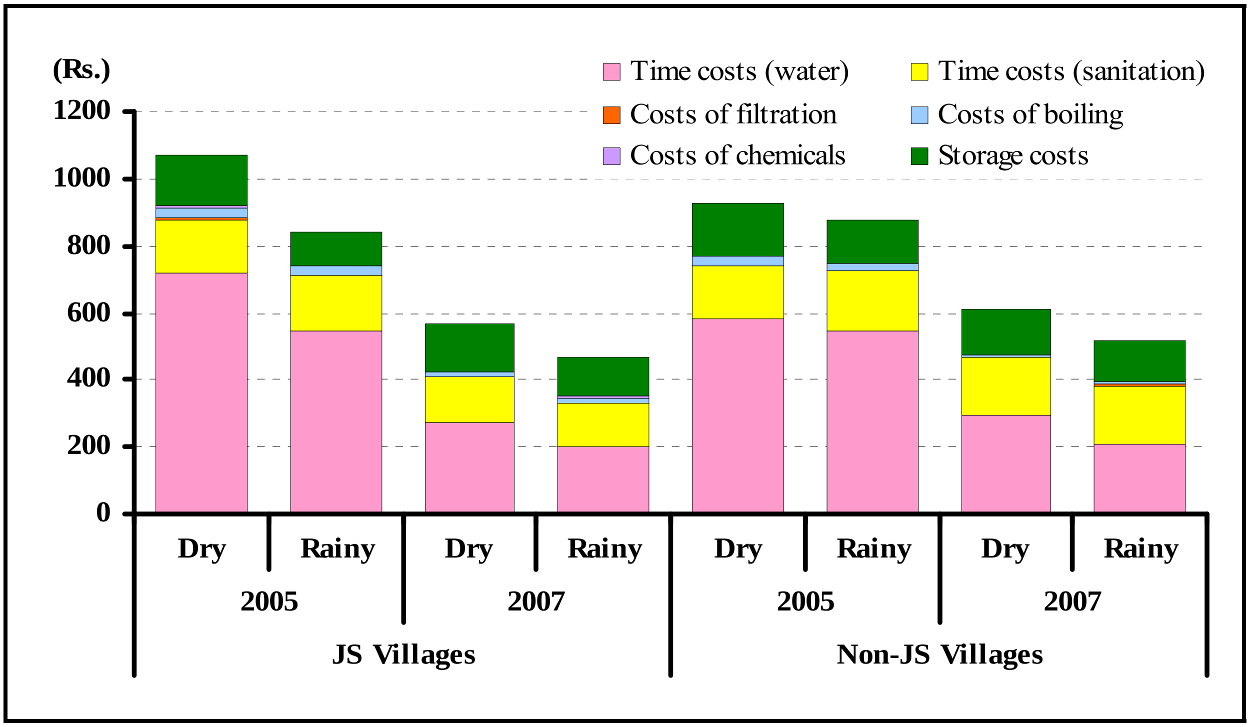

Figure 2. Household Cost of Illness by Intervention, Season and Survey Year

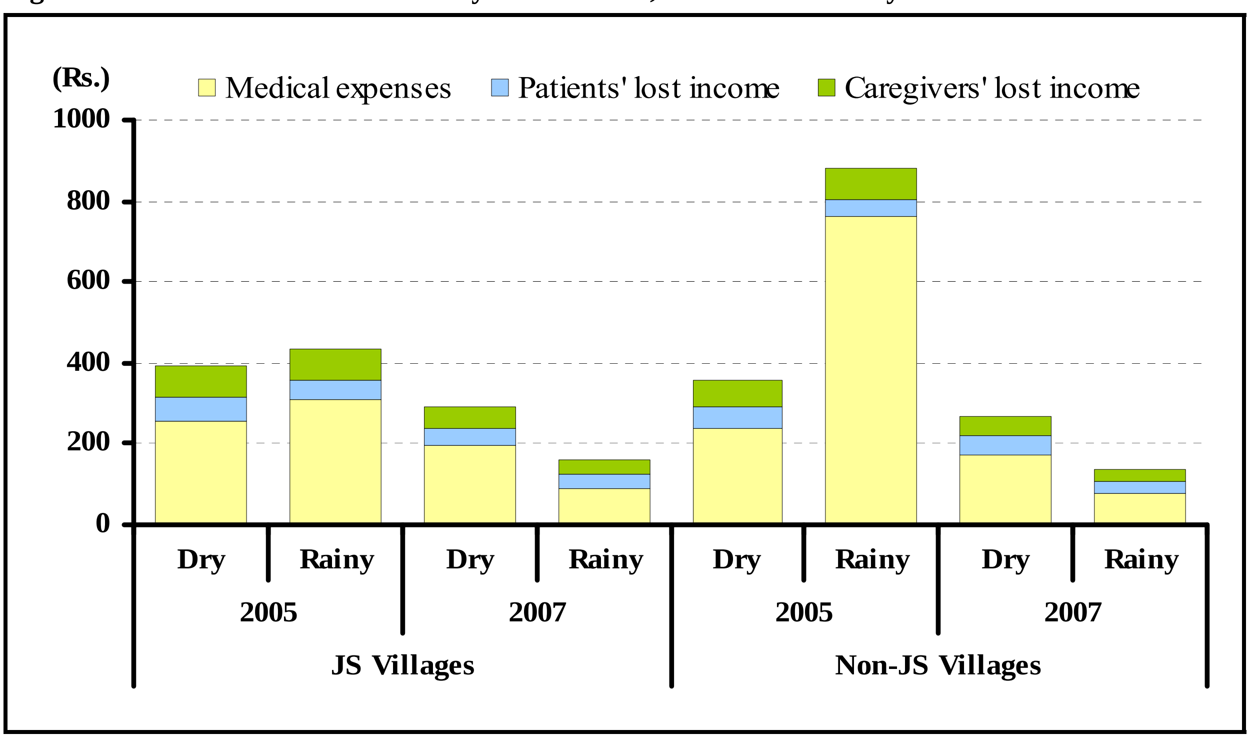

Table 1. Testing covariate balance across treatment and 'matched' control villages using secondary data†

Variables	**% bias reduced†**	**t-statistic♣**
% males in village (2001)	20	0.29
% children in village (2001)	68	0.74
% scheduled castes in village (2001)	-749	1.78
% scheduled tribes in village (2001)	79	-0.36
% female workers in village (2001)	83	-0.05
% cultivators in village (2001)	9	-1.18
% agricultural labors in village (2001)	37	1.55
% marginal workers in village (2001)	49	-0.37
Households in village (2001)	98	-0.03
Average household size in village (2001)	99	0.03
Female literacy rate in village (2001)	55	-1.05
% permanent houses in block (2001)	56	-0.57
% households with private tap in block (2001)	98	0.07
% households without toilets in block (2001)	66	-0.49
% households with electricity in block (2001)	77	0.82
% households who use firewood / crop residue / cowdung as cooking fuel in block (2001)	85	0.4
***Water supply level (lpcd) in village** (1999)*	7	2.33

† Reduction in bias when comparing mean difference between treatment and unmatched controls to mean difference between treatment and matched controls. Bias is the difference in standardized means between JS and control (non-JS) villages.

♣ For mean difference between treatment & matched control villages

Table 2. Testing balance across treatment and control villages using baseline survey data†

Covariate of Interest	Treatment Mean	Control Mean	z-value‡
% under 5 children with diarrhea	11%	10%	1.62
% under 5 children with ARI	21%	22%	-0.71
% households using private tap	18%	24%	-1.55
% households using private toilet	13%	10%	0.96
# of critical times a caregiver washes hands	2.3	2.4	-0.51
# of critical times a child washes hands	1.1	1.2	-0.44
% households treating drinking water	64%	63%	0.11
% households stating roads are 'main' problem	19%	21%	-0.84
% households stating water supply is 'main problem' ***	54%	42%	3.27
% households stating sanitation is 'main problem' ***	11%	14%	-1.72
% households stating public well water quality is bad *	19%	24%	-1.77
% households stating public tap water quality is bad	24%	22%	0.44
% households stating village water-sanitation committee (VWSC) is active***	20%	12%	2.71
% households participating in VWSC***	5%	3%	3.35

† Differences are noted *** if statistically significant at 1%; ** if significant at 5%; * if significant at 10%.

‡ For mean differences after adjusting standard errors to account for clustering at the village level.

Table 3. Average Monthly Coping Costs by Intervention, Season and Survey Year

Type of Coping Costs	JS Status	2005						2007					
		Dry Season			Rainy Season			Dry Season			Rainy Season		
		N	mean	sd	N	mean	sd	N	mean	sd	N	mean	sd
Time costs (water)	Non-JS	6375	593	930	5882	554	576	5990	296	315	5936	208	234
	JS	3635	725	1071	3326	549	603	3383	272	291	3372	198	226
	Overall	10010	641	986	9208	552	586	9373	288	307	9308	204	231
Time costs (sanitation)	Non-JS	6407	164	158	5905	180	165	5723	178	170	5627	185	177
	JS	3612	165	189	3330	170	178	3325	138	171	3236	139	174
	Overall	10019	164	170	9235	177	170	9048	163	171	8863	169	178
Filter costs	Non-JS	6177	2	14	4328	2	13	5595	2	10	5875	2	16
	JS	3516	3	23	2523	2	25	3227	4	103	3333	2	9
	Overall	9693	2	18	6851	2	18	8822	3	63	9208	2	14
Costs of boiling	Non-JS	6441	29	170	5959	22	150	5995	5	66	5941	11	95
	JS	3650	31	177	3366	29	171	3389	14	106	3374	15	110
	Overall	10091	30	172	9325	25	158	9384	9	83	9315	13	100
Chemical costs	Non-JS	6323	1	8.2	5958	0	0	5941	0.9	33	5833	0.6	4
	JS	3581	5	215	3366	0.007	0.4	3352	0.7	5	3285	0.4	3
	Overall	9904	2	130	9324	0.003	0.3	9293	0.8	26	9118	0.5	4
Storage costs	Non-JS	6526	159	114	5960	128	124	5997	138	96	5941	123	107
	JS	3679	152	79	3366	100	29	3390	142	124	3374	116	83
	Overall	10205	156	103	9326	118	102	9387	140	107	9315	121	99
Monthly household coping costs	Non-JS	6526	930	966	5960	877	651	5997	612	405	5941	521	350
	JS	3679	1068	1110	3366	842	674	3390	567	399	3374	465	323
	Overall	10205	980	1022	9326	864	660	9387	596	403	9315	500	342

Table 4. Average Monthly Cost of Illness by Intervention, Season and Survey Year

Type of COI	JS Status	2005						2007					
		Dry Season			Rainy Season			Dry Season			Rainy Season		
		N	mean	sd	N	mean	sd	N	mean	sd	N	mean	sd
Out-of-pocket medical expenses	Non-JS	6418	238	1464	5960	762	10675	5921	173	1172	5930	76	451
	JS	3612	258	1235	3366	310	5068	3337	194	2050	3367	88	484
	Overall	10030	245	1386	9326	599	9063	9258	181	1547	9297	80	464
Patients' lost income	Non-JS	6418	51	179	5960	43	138	5921	46	154	5930	29	120
	JS	3612	57	160	3366	49	151	3337	45	154	3367	36	152
	Overall	10030	53	173	9326	45	143	9258	45	154	9297	32	132
Caregivers' lost income	Non-JS	6418	50	326	5960	24	110	5921	15	77	5930	9	78
	JS	3612	55	445	3366	22	113	3337	16	75	3367	6	52
	Overall	10030	52	373	9326	23	111	9258	16	76	9297	8	70
Household cost of illness	Non-JS	6418	339	1551	5960	829	10698	5921	234	1218	5930	114	529
	JS	3612	370	1434	3366	381	5091	3337	255	2084	3367	130	577
	Overall	10030	350	1510	9326	667	9085	9258	242	1586	9297	120	547

Table 5. DID Analysis of Coping Costs[†]

	Dry Season Analysis						
	Monthly household coping costs	Time costs (water)	Time costs (sanitation)	Filter costs	Costs of boiling	Chemical costs	Storage costs
JSpost	-183.39	-155.68	-41.91	1.72	5.98	-4.13	10.49
	0.044**	0.095*	0.001***	0.358	0.368	0.253	0.603
JS	138.25	131.73	1.68	0.21	2.54	3.94	-6.91
	0.129	0.157	0.862	0.717	0.69	0.271	0.585
post	-317.87	-296.86	14.31	-0.37	-23.33	0.26	-20.50
	0.000***	0.000***	0.045**	0.234	0.000***	0.565	0.066*
Constant	930.06	593.21	163.50	2.34	28.78	0.61	158.86
	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***
Observations	19592	19383	19067	18515	19475	19197	19592
R-squared	0.06	0.058	0.006	0	0.006	0	0.007
	Rainy Season Analysis						
	Monthly household coping costs	Time costs (water)	Time costs (sanitation)	Filter costs	Costs of boiling	Chemical costs	Storage costs
JSpost	-20.95	-5.27	-35.74	-0.49	-3.21	-0.13	20.24
	0.737	0.917	0.009***	0.447	0.704	0.247	0.262
JS	-35.05	-4.98	-10.20	0.37	7.08	0.01	-27.85
	0.526	0.917	0.352	0.565	0.408	0.316	0.028**
post	-356.30	-346.27	5.19	0.57	-11.17	0.56	-4.55
	0.000***	0.000***	0.483	0.136	0.009***	0.000***	0.76
Constant	876.98	554.24	180.20	1.67	22.29	0.00	127.96
	0.000***	0.000***	0.000***	0.000***	0.000***	1	0.000***
Observations	18641	18516	18098	16059	18640	18442	18641
R-squared	0.109	0.133	0.009	0	0.003	0.01	0.01

[†] Robust p value is reported below each coefficient. We also ran other model specifications that include household level covariates, which did not affect the overall significance of the DID estimates but affected the size of the DID estimates slightly.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 6. DID Analysis of Cost of Illness[†]

	Dry Season Analysis			
	Household cost of illness	Out-of-pocket medical expenses	Patients' lost income	Caregivers' lost income
JSpost	-13.76	1.40	-6.35	-8.81
	0.825	0.979	0.381	0.361
JS	35.02	19.76	5.44	9.82
	0.368	0.548	0.291	0.241
post	-88.42	-64.74	-5.65	-18.02
	0.002***	0.009***	0.162	0.001***
Constant	356.22	237.78	51.28	67.17
	0.000***	0.000***	0.000***	0.000***
Observations	19288	19288	19288	19288
R-squared	0.001	0.001	0.001	0.002
	Rainy Season Analysis			
	Household cost of illness	Out-of-pocket medical expenses	Patients' lost income	Caregivers' lost income
JSpost	468.69	464.71	-0.14	4.12
	0.011**	0.011**	0.983	0.54
JS	-444.44	-452.35	6.79	1.12
	0.016**	0.013**	0.134	0.881
post	-743.53	-686.70	-13.24	-43.60
	0.000***	0.000***	0.000***	0.000***
Constant	878.98	762.45	42.60	73.93
	0.000***	0.000***	0.000***	0.000***
Observations	18623	18623	18623	18623
R-squared	0.003	0.002	0.003	0.014

[†] Robust p value is reported below each coefficient. We also ran other model specifications that include household level covariates, which did not affect the overall significance of the DID estimates but affected the size of the DID estimates slightly.

* significant at 10%; ** significant at 5%; *** significant at 1%