Spring Cleaning:
Rural Water Impacts, Valuation, and Institutions[*]

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Abstract: Social norms and legal institutions often create common property rights in natural resources, limiting private investment. A randomized evaluation in Kenya suggests that infrastructure investments can reduce fecal contamination by 66% at naturally occurring springs and 24% in spring users' home water supply, cutting child diarrhea by one quarter. While households increase use of protected springs, travel-cost based revealed preference estimates of households' valuations are only one-third stated preference valuations and are less than one-tenth levels implied by health planners' typical valuations of child mortality. Simulations suggest that, as a result, private property norms would generate little additional investment, while imposing large static costs due to spring owners' local market power. However, social norms requiring continued open access to unimproved sources but allowing fees for protected spring water could be Pareto improving. Vouchers for improved water could closely approximate either a conventional social planner solution or one placing extra value on child health.

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1. Introduction

Social norms and associated legal institutions often create communal property rights in natural resources. In Islamic law, for example, the sale of water is generally not permitted (Faruqui, Biswas and Bino 2001), and in societies from Tsarist Russia to those in contemporary west and southern Africa, land is periodically reallocated among families based on assessments of need (e.g., Adams et al. 1999, Bartlett 1990, Fafchamps and Gavian 1996, Peters 2007). Such systems may provide social insurance and Ostrom (1990) argues that communities have often developed efficient institutions for managing common property resources. Others, however, argue that communal land tenure systems distort land use decisions (Goldstein and Udry 2005) and note that even if social norms mandating communal property rights were once appropriate, increased population or changes in technology that widen the scope for investment may make them inappropriate in contemporary settings.

In Kenya, both social norms and law make many water sources, including naturally occurring springs, common property resources (Mumma 2005). This discourages private investment in spring protection infrastructure, which seals off the source of a spring and reduces source water contamination from exposure to the environment. On the other hand, communal property rights in water also limit static inefficiencies due to exploitation of local market power.

This paper uses a randomized evaluation to measure the impact of source water quality improvements achieved via spring protection on health in Kenya, estimate the value that households place on spring protection, and simulate the welfare impacts of alternative water property rights institutions, including both communal and private property rights.

Diarrhea accounts for 20% of deaths of children under five each year (Bryce *et al.* 2005), and policy makers have called for investments in water infrastructure to provide cleaner water. Progress towards the sole quantifiable environmental Millennium Development Goal is currently measured by the percentage of the population living near improved water sources such as protected springs. Yet, among experts, there is widespread skepticism about the health value of improvements in water quality short of providing piped water to the home. In the absence of evidence from randomized trials, many argue based on non-experimental evidence that diarrhea is affected more by the quantity of water available for washing than by the quality of drinking water (Curtis, Carincross, and Yonli 2000); that improved water supplies have little impact without improved sanitation and hygiene (Esrey 1996, Esrey *et al*. 1991), and that recontamination of water in transport and storage may vitiate many of the benefits of improved source water quality (Fewtrell *et al*. 2004). These influential reviews argue that there may be little point in investing in water infrastructure that does not deliver water to households.

As the first (to our knowledge) randomized evaluation of a source water quality investment, the data used in this paper allow us to isolate the impact of a single intervention affecting the quality but not quantity of water, and to assess the claim that source water quality gains are most valuable in the presence of pre-existing access to improved sanitation and hygiene practices.[1]

We find that spring protection greatly improves water quality at the source, reducing fecal contamination by 66%. Spring protection is also moderately effective at improving household water quality, reducing contamination by 24%. The incomplete pass through from spring-level water gains into the home is due both to households' collection of water from multiple water sources and to partial recontamination of water in transport and storage. The dampened home water gains are not due to crowding out of other water treatment measures (such as boiling water or chlorination) nor does improved sanitation coverage or hygiene knowledge appear to allow households to better translate source water quality gains into better household water. We also find that spring protection improved child health: diarrhea among young children in treatment households falls by 4.7 percentage points, or nearly one quarter, on a base diarrhea prevalence of approximately 19 percent.

The second part of the paper contributes to the environmental economics literature on the valuation of environmental amenities. In our study area, most households choose from multiple local water sources. The intervention we study generates exogenous variation in the relative desirability of alternative sources, and we explore how household water source choices and other behaviors respond to water quality improvements. A discrete choice model, in which households

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[1] There are two prospective studies of source water quality interventions that find positive impacts on child health. Aziz et al. (1990) study the impact of a project in Bangladesh that simultaneously provided water pumps, hygiene education, and latrines, to two intervention villages (820 households), and compare them with three control villages (750 households) separated by about 5 km. The published article does not mention if the treatment villages were randomly selected. Huttly et al. (1987) study the impact of borehole wells with hand-pumps, pit latrines, and health education on dracunculiasis (guinea worm disease), diarrhea, and nutrition in Nigeria. Generalizing to other settings is hampered by these studies' small sample sizes (each includes only five villages not selected at random), and the fact that they evaluate improved water quality and quantity simultaneously (by providing wells).

trade off water quality against walking distance to the source, generates revealed preference estimates of household valuations of better water quality.

The estimated median valuation for spring protection is equivalent to 18.5 workdays, or approximately US$1.76 per household per year using our best measure of the time value of local water collectors. This suggests an upper bound of US$0.54 on households' median willingness to pay to avert one child diarrhea episode. Under additional assumptions, this paper provides among the first estimates of the value of a statistical life in less developed country. We estimate .an upper bound on the median value of averting one child death is US$461, less than one-tenth the values typically used in health cost-effectiveness analyses, even in the poorest countries.

We contrast our revealed preference valuation of spring protection, which exploits experimental variation in water source characteristics, with two different stated preference methodologies: stated ranking of alternative water sources, and contingent valuation (see Carson et al. 1996, Whitehead 2006). Most valuation estimates rely on stated preference data, which is relatively cheap to collect, yet few stated preference estimates have been validated against reliable revealed preference benchmarks. Revealed preference data is rarely available in less developed countries, and those studies that do exist are typically prone to omitted variable bias critiques.[3] We find that the stated preference approaches generate much higher valuation estimates than our revealed preference approach, by a factor of three, with the contingent

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[3] Whittington, Mu, and Roche (1990) and Mu, Whittington, and Briscoe (1990) each study water source choice in rural African settings using a contingent valuation (CV) approach. However, neither accounts for the role of water quality in the source choice decision (they focus on distance and price) and they explicitly rule out the use of multiple drinking water sources, which we find to be empirically important in our data. The shortcomings of CV and other stated preference approaches to measuring the value of non-market goods are well-known (Diamond and Hausman 1994). Choe (1996) compares willingness to pay for reduced river and lake pollution in an urban Philippines setting, using both travel cost and CV methods, and finds that both are low and quite similar; Choe's sample consists of households with piped connections, limiting its generality to rural areas. Two other papers have compared averting (or defensive) expenditure data to stated willingness to pay (Griffin *et al.* 1995 and Rosado *et al.* 2006 in India and Brazil, respectively), though neither exploits experimental variation in water quality as we do.

valuation survey approach yielding especially imprecise estimates, casting doubt on the reliability of stated preference methods in this setting.

Our third set of results simulates the impact of alternative social norms and property rights institutions in the rural water sector. Using our revealed preference valuation estimates, we first show that a social planner would only protect springs with a relatively large number of household users. We then conduct policy simulations under alternative property rights institutions, where estimated household water demand is derived from the revealed preference valuations. We first find that a well-enforced norm of private property rights yields lower social welfare than existing communal rights. Under private property norms, spring owners charge high positive prices, which distort static water consumption choices. However, they protect relatively few springs and the springs they protect are not necessarily those a social planner would protect. However, an alternative norm under which spring owners can charge for protected spring water only if they also provide free access to unprotected water is a Pareto improvement relative to existing communal property rights norms. A government-financed voucher system for spring users could approximate the solution for either a social planner who respected households' valuations of spring protection or who placed extra value on child health.

This paper makes four major contributions to the development, environmental, and industrial organization literatures. First, we provide the first randomized impact evaluation evidence on the health benefits of a source water quality intervention, a significant area of government and donor investment in less developed countries. Second, we provide among the first estimates of the value of a statistical life in a poor country. Third, we contribute to the literature on revealed preference valuation of environmental amenities, providing evidence on the divergence between stated and revealed preference valuation for water-related interventions. Finally, our research design allows us to explore the implications of alternative property rights regimes on investment decisions in the water sector, contributing to our understanding of the role of institutions in economic development using econometric techniques pioneered in industrial organization.

The remainder of the paper is organized as follows: Section 2 describes the intervention and data. Section 3 discusses the impacts of spring protection on water quality and the child health results. Section 4 discusses the impact of spring protection on water source choice and behavior and presents estimates of willingness to pay for clean water. Section 5 discusses the provision of clean water under alternative institutions, and the final section concludes.

2. Rural Water Project (RWP) overview and data

This section describes the intervention, randomization into treatment groups, and data collection.

2. 1 Spring protection in western Kenya

Naturally occurring springs are an important source of drinking water in rural western Kenya, where the region's topography frequently allows ground water to come to the surface. Approximately 43% of rural western Kenyan households use springs for drinking water and over 90% have access to springs (DHS 2003). Our respondents report that springs are the main source of water in the study area: 72% of all water collection trips are to springs. The next most common source are shallow wells (at 13%), followed by boreholes (7%), surface water sources such as rivers, lakes and ponds (5%), and other sources. Of all water collection trips, 81% are to sources the respondents used for drinking water in the last week. The area of Kenya in which our study site is located is poor – the daily agricultural wage for men ranges from US$1 to 2 per day depending on the task, and is typically lower for women.

Property rights to land and other natural resources are governed by a combination of traditional customary law and formal legal statutes in Kenya (Mumma 2005). Not only does custom require that private landowners allow public access to water sources on their land, but also under Kenyan law local authorities can "where, in the opinion of the Authority the public interest would be best served" order landowners to make water available "to any applicant so long as the water use of the owner of the works is not adversely affected." In practice landowners are expected to make spring water available to neighbors for free. Spring owners have very weak incentives to improve a water source, as they are unable to recoup the costs of any investment via the collection of user fees. There is no elected local government, and collective action problems mean that investments in valuable local public goods, including water infrastructure, often fail to occur. When it occurs, spring protection is generally undertaken by outside donors or the government, often in conjunction with user groups set up to collect maintenance funds, though they too lack authority to exclude free-riders.

Spring protection is widely used in non-arid regions of Africa (Mwami 1995, Lenehan and Martin 1997, UNEP 1998). Protection seals off the source of a naturally occurring spring and encases it in concrete so that water flows out from a pipe rather than seeping from the ground where it is vulnerable to contamination from runoff, improving water quality at an already existing source. Protected springs are considered an “improved” water source by the World Health Organization and thus protecting springs counts as progress towards MDG targets.

This study is based on a randomized evaluation of a spring protection project conducted by a non-governmental development organization (NGO), International Child Support (ICS). Spring protection cost an average of US$1024 (s.d. US$84.52), with some variation depending on spring characteristics. All communities contributed 10% of the project cost, mainly in the form of manual labor. The NGO conducted community meetings at which user committees responsible for maintaining the protected spring were selected. Typical maintenance needs include simple patching of concrete, keeping the catchment area clean, and clearing drainage ditches. These costs are on the order of US$35 per year, and are expected to be covered by community contributions. Free rider problems in collecting these funds are common in practice.

2.2 The study sample and assignment to treatment

Springs for this study were selected from the universe of local unprotected springs by the collaborating NGO. The NGO first obtained Kenya Ministry of Water and Irrigation lists of all local unprotected springs in the Busia and Butere-Mumias districts. NGO field and technical staff then visited each site to determine which springs were suitable for protection. Springs known to be seasonally dry in months when the water table is low were eliminated, as were sites with upstream contaminants (e.g., latrines, graves). From the remaining suitable springs, 200 were randomly selected (using a computer random number generator) to receive protection. Permission for protection was received from the spring landowner in all but two cases.

The NGO planned for the water quality improvement intervention to be phased in over four years due to their financial and administrative constraints. Although all springs were eventually protected, for our analysis the springs protected in round 1 (January-April 2005) and round 2 (August-November 2005) are called the treatment springs and those that were protected later are the comparison group. Figure 1 summarizes the project timeline. To address concerns about seasonal variation in water quality and disease burden, all springs were stratified geographically and by treatment group and then randomly assigned to an activity “wave,” and all project activities and data collection efforts were conducted by wave.

Several springs were unexpectedly found to be unsuitable for protection after the baseline data collection and randomization (discussed in Appendix A) had already occurred. These springs, which were found in both the treatment and comparison groups, were dropped from the sample, leaving 184 viable springs. Identification of the final sample of viable springs is not related to treatment assignment: when the NGO was first informed that some springs were seasonally dry, all 200 sample springs were re-visited to confirm their suitability for protection. In only 10 springs (in the final sample of 184 viable springs) did treatment assignment differ from actual treatment (for example because landowners refused to allow protection, or the government independently protected comparison springs); these springs are retained in the sample and we conduct an intention-to-treat analysis throughout. Table 1 presents baseline summary statistics for both the treatment and comparison groups. Additional details about the randomization procedures used to assign springs to treatment groups are in Appendix A.

A representative sample of households that regularly used each sample spring was selected at baseline. Survey enumerators interviewed users at each spring, asking their names as well as the names of other household users. Enumerators elicited additional information on spring users from the three to four households located nearest to the spring. Households that were named at least twice among all interviewed subjects were designated as “spring users”. The number of household spring users varied from eight to 59 with a mean of 31. Seven to eight households per spring were then selected (using a computer random number generator) from this spring user list for the household sample used in this paper. In subsequent surveys, over 98% of this spring users sample was later found to actually use the spring at least sometimes, but the few baseline non-user households were nonetheless retained in the analysis.

The spring user list is representative of all households living near sample springs. In a census of all households living within roughly a 20 minute walk at nine sample springs, nearly two thirds of these nearby households were included on the original spring users lists, with even higher rates for those households located within a 10 minute walk of the sample spring.

2.3 Data collection

Water quality was measured at all sample springs and in households using protocols based on those used at the U.S. Environmental Protection Agency. The measure of water quality used was contamination with *E. coli*, an indicator bacteria that is correlated with the presence of fecal matter. The household survey gathered baseline information about child diarrhea and anthropometrics, mothers' hygiene knowledge and behaviors (hand washing), household water collection and treatment behavior, and socioeconomic status. The target household survey respondent was the mother of the youngest child living in the home compound (where extended families often co-reside), or another woman with childcare responsibilities if the mother of the youngest child was unavailable. (Details on the data collection protocols are in Appendix B.)

The main analysis sample consists of 184 springs and 1,354 households with baseline data and at least one round of follow-up data. A first follow-up round of water quality testing at the spring and in homes, spring environment surveys, and household surveys was completed three to four months after the first round of spring protection (April-August 2005). The second round of spring protection was performed in August-November 2005, and the second follow-up survey collected one year later (August-November 2006). The third follow-up survey round took place five months later, from January-March 2007.

An intervention providing point-of-use (POU), or in-home, chlorination products was launched before the third follow-up survey (2007) in a random subset of households. Due to possible interactions with spring protection and impacts on household water quality and health, the third follow-up survey for this subset of households is excluded from the analysis. This POU intervention is studied in other research (Kremer, Miguel, Null and Zwane 2008).

The attrition rate was modest over the course of this study. 94% of baseline households were surveyed in at least two of the three follow-ups, and 80% were surveyed in all three follow-up rounds. Attrition is not significantly related to spring protection assignment: the estimated coefficient on the treatment indicator is 0.012 (s.e. 0.018). The baseline characteristics of households lost over time are statistically indistinguishable from those that remain in the sample.

2.4 Baseline descriptive statistics

Table 1 presents baseline summary statistics for springs (Panel A), households (Panel B) and children under age three (Panel C). For completeness, we report statistics for all springs and households with baseline data (collected prior to randomization into treatment groups) even if they are dropped from the analysis because the spring was later found unsuitable for protection, although results are almost unchanged with the slightly smaller main sample (not shown).

As expected in light of the randomization design, there is no statistically significant difference between baseline water quality at treatment versus comparison springs (Table 1, Panel

A). Most spring water in our sample is of moderate quality, and only about 5-6% of samples are of high quality, and the rest are poor quality. Household water is somewhat more likely to be high quality prior to spring protection in the treatment group (and the difference in means, though relatively small, is significant at 95% confidence), but there is no significant difference in the proportion of moderate or poor quality water samples (Panel B). A Kolmogorov-Smirnov test does not reject equality of baseline home water quality distributions for the treatment and comparison groups (p-value = 0.24).

Household water quality is somewhat better than spring water quality on average at baseline: the average difference in ln *E. coli* is 0.51 (s.d. 2.63; results not shown). This likely occurs for at least two reasons. First, many households collect water from sources other than the sample spring: only half of the household sample gets all their drinking water from their local sample spring at baseline, and overall nearly one third of water collection trips are to other sources. Second, some households use point-of-use (POU) water treatment at home. Over 25% of households report boiling their drinking water at baseline though nearly all adults report drinking unboiled water on the day surveyed, and young children are commonly given water to drink directly from the household storage container, not exclusively boiled water. Moreover, the correlation between household water contamination and self-reported water boiling is low, raising the possibility of social desirability reporting bias. In the first follow-up (2005) survey, 29% of households reported chlorinating their water at least once in the last six months; these chlorination levels are higher than usually observed because the government distributed free chlorine in part of our study region following a 2005 cholera outbreak.

Water quality tests were also conducted at the two main alternative sources near each sample spring during the third follow-up survey (in 2007).[4] Protected springs have the least contaminated water with average ln *E. Coli* MPN/100 ml = 2.3, followed by unprotected springs, boreholes, shallow wells, and rivers/streams with average contamination levels of 3.6, 4.1, 5.2, and 6.0 respectively. Respondents' perceptions line up closely with objective contamination measures: the proportion of respondents stating that a source is "very" or "somewhat clean" is highest for protected springs, the cleanest source, at 92%, followed by boreholes (87%) and

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[4] Springs are often located in close proximity. Springs in the sample have an average of 1.2 other springs within 1 km and 9.2 springs within 3 km. Of these, 0.4 and 2.8 are protected within 1 and 3 km. There are no significant differences at baseline in the total number of nearby springs within 1, 3, or 6 km for the treatment and comparison groups (not shown).

unprotected springs (75%), shallow wells (73%), lakes/ponds (31%) and streams/rivers (14%). The correlation between ln *E. coli* MPN/100ml levels at individual water sources and household rankings of the sources' water quality (on a 1 to 5 scale, with 1=very clean and 5=very unclean) is strong, at 0.12 (s.e. 0.02), and this correlation is even larger when household fixed effects are included at 0.19 (s.e. 0.02).

Most other household and child characteristics are similar across the treatment and comparison groups (Table 1, Panels B and C). Average mother's education is six years, less than primary school completion. Approximately four children under age 12 reside in the average compound. Water and sanitation access is fairly high compared to many other less developed countries as about 85% of households report having a latrine, and the average walking distance (one-way) to the closest local water source is just over 8 minutes (the median one-way distance is even less at 5 minutes). A fairly high 20% of children in the comparison group had diarrhea in the past week at baseline, as did 23% in the treatment group.

3. Spring protection impacts on water quality and health

This section discusses the estimation strategy and presents the impacts of spring protection on water quality and child health and nutrition.

3.1 Estimation strategy

Equation 1 illustrates an intention-to-treat (ITT) estimator using linear regression.

$$W_{it}^{SP} = \alpha_t + \beta_1 T_{it} + X_i^{SP\prime}\beta_2 + (T_{it} * X_i^{SP})'\beta_3 + \varepsilon_{it}. \quad (1)$$

W_{it}^{SP} is the water quality measure for spring i at time t ($t \in \{0, 1, 2, 3\}$ for the four survey rounds) and T_{it} is a treatment indicator that takes on a value of one after spring protection assignment, (i.e. for treatment group 1 in all follow-up survey rounds and for treatment group 2 in the second and third follow-ups, see Figure 1). X_i^{SP} are baseline spring and community characteristics (e.g., baseline contamination) and ε_{it} is a white noise disturbance term which is allowed to be correlated across survey rounds for the same spring. Random assignment implies that β_1 is an unbiased estimate of the reduced-form ITT effect of spring protection. In some specifications we explore differential effects as a function of baseline characteristics, captured in the vector β_3. Survey round and wave fixed effects α_t are also included to control for any time-varying factors affecting all groups. Estimates of the average treatment effect on the treated

(TOT) in a two-stage procedure (Angrist, Imbens, and Rubin 1996) are very similar to the ITT estimates since assignment differed from actual treatment for few springs.

3.2 Impact of treatment on spring water quality results

Spring protection dramatically reduces contamination of source water with fecal matter. The average reduction in ln *E. coli* across all four rounds of data, is -1.07, corresponding to roughly a 66% reduction (Table 2, regression 1). These estimated effects are robust to including controls for baseline contamination (regression 2). Protection does not lead to a significantly larger proportional reduction in water contamination where initial contamination was highest (regression 3). The downward slope of the non-parametric representation of the data in Figure 2 is consistent with mean reversion, likely reflecting measurement error in water quality. The correlation in measured water contamination levels across survey rounds, including seasonal controls, is moderate, at 0.46, although some of the variation over time is likely due to actual changes in quality rather than measurement error alone. There is no statistically significant evidence of differential treatment effects by baseline hygiene knowledge (the average among local spring users), average local sanitation (latrine) coverage, or education (regression 4).

3.3 Home water quality impacts

Relying again on the randomized design, we estimate a regression analogous to equation 1 to estimate the impact of spring protection on home water quality, again measured in ln *E. coli* MPN. We control for baseline household characteristics in some specifications including sanitation access, respondent's diarrhea knowledge, water boiling, an iron roof indicator, years of education, and the number of children under age 12 at baseline. We also allow for differential treatment effects as a function of these characteristics. Regression disturbance terms are clustered at the spring level.

The average reduction in ln *E. coli* contamination at the home is -0.27, or roughly 24%, considerably smaller than the impacts on source water quality (Table 3, regression 1). For "sole source" households that were only using spring water in the pre-treatment period, home water quality should be unambiguously better after treatment since they still rely mainly on the spring and its quality improves after protection. Interpretation is more complicated for baseline "multi-source" water users in our data, who were roughly on the margin between using the sample spring and other sources. For these households, improved spring water will be combined in the home with water of unknown quality from other sources, and this together with endogenous source choice could cause home drinking water quality to increase or decrease after protection. There are slightly greater reductions in contamination for sole-source households (regression 2) though we cannot reject equal treatment effects for sole-source and multi-source users.

Random assignment of springs to protection implies that we could avoid both omitted variable bias and also reduce attenuation bias (due to measurement error in water quality) by estimating the correlation between source and home water quality in an instrumental variables framework in the sole-source users sample, with assignment to spring protection as the instrument for spring water quality. Conceptually, sole source users could be a useful sample for estimating the degree of pass through of source water quality gains to the home, if these households almost exclusively use the sample spring for drinking water in all periods. Unfortunately, water use patterns are not static across the four years of data: in the first follow-up survey, 70% of comparison group baseline sole source spring users remained sole source users but only 26% remained sole source users in all three follow-up rounds. This "churning" could be due to changes in other water options over time (as other sources improve or deteriorate), or variation in water collection costs due to evolving household composition. Regardless of the cause, baseline sole- and multi-source user status becomes less meaningful over time, making it infeasible to reliably estimate pass-through.

Using the comparison households, we also non-experimentally estimated the relationship between the use of different water source types on household water quality. Conditional upon collecting spring water, households that chose to obtain water from protected springs have significantly better home water quality: making all water collection trips to protected rather than unprotected springs is associated with a 0.44 drop in ln *E. coli* contamination (s.e. 0.18), or roughly 37% (not shown), substantially larger than the experimental estimates in Table 3 that we argue are more reliable.

We again find no evidence of differential treatment effects as a function of household sanitation, diarrhea prevention knowledge, or mother's education (Table 3, regression 3). This runs counter to claims that source water quality improvements are much more valuable when sanitation access or hygiene knowledge are also in place, although the relatively large standard errors on these interaction terms argue for caution in interpretation. Home water contamination reductions are smaller for households that report boiling their water, as expected if boiling is a substitute for spring protection (regression 3).

Externalities are possible as a result of spring protection both because of hydrological interconnections, endogenous water source choice, and the infectious nature of diarrheal diseases. We test for spring protection externalities for both spring and household water quality by considering the effect of the number of nearby treated springs (located within 1, 3, or 6 kilometers), controlling for the total of local springs (protected or not). There is no evidence for externalities in spring water quality: the coefficient estimate on the number of treated springs within 3 kilometers is -0.004 and is not statistically significant (s.e. 0.086), and similar results hold for springs at greater distances (regressions not shown). There is some weak evidence for positive household water quality externalities – the coefficient on treatment springs located within 3 kilometers is -0.090 (s.e. 0.050, not shown) – though this effect is also consistent with some households switching to use nearby protected sources, an important issue we explore at length below.

3.4 Child health and nutrition impacts

We estimate the impact of spring protection on child health and anthropometrics in equation 2.

$$Y_{ijt} = \alpha_i + \alpha_t + \beta_1 T_{ijt} + X_{ij}'\beta_2 + (T_{ijt} * X_{ij})'\beta_3 + u_{ij} + \varepsilon_{ijt.} \qquad (2)$$

The main dependent variable is diarrhea in the past week. The coefficient estimate, β_1, on the treatment indicator T captures the spring protection effect. An advantage of this experimental design over existing studies, beyond the usual benefits of addressing omitted variable bias, is the ability to avoid measurement error in the key water quality explanatory variable (through use of the treatment indicator). We include child fixed effects (α_i), survey round and month fixed effects (α_t). We also explore heterogeneous treatment effects as a function of child and household characteristics, X_{ij}.

Spring protection leads to statistically significant reductions in diarrhea for children under age 3 at baseline or born since the baseline survey. In the simplest specification taking advantage of the experimental design, diarrhea incidence falls by 4.5 percentage points (standard error 1.2, Table 4, regression 1).[6] In a probit specification the impact is similar, at -4.4 percentage points (standard error 2.0, regression 2), and similarly in a linear specification with child fixed effects and treatment group fixed effects and month of survey effects (-4.5 percentage points, standard error 2.3, p-value=0.06, regression 3). In our preferred specification with month and child fixed effects and child gender and age polynomial controls, the point estimate is -4.7 percentage points (standard error 2.3, regression 4). On a comparison group average of 19% of children with diarrhea in the past week, this is a drop of one quarter. We conclude that the moderate reductions in household water contamination caused by spring protection were sufficient to significantly reduce diarrhea incidence.

While the estimated reduction in diarrhea remains negative for boys, the effects are driven mainly by reduced diarrhea among girls (Table 4, regression 5). For girls the estimated reduction is 9.0 percentage points, and this effect is significant at 99% confidence. This finding is surprising since baseline diarrhea rates are similar for boys and girls in our sample, and differential gender impacts are rarely found in the related epidemiology literature; a decisive explanation remains elusive and calls for further investigation.

Interactions with baseline local sanitation (latrine) coverage, diarrhea prevention knowledge, and education are not significant (regression 6), in line with the lack of additional water quality gains for such households. Effects are similar in the second and third years after protection, and also across baseline sole-source versus multi-source households (not shown). Spring protection effects do not differ significantly by month of year (rainy versus dry season), nor by child age up through age five years (not shown). Spring protection effects also do not differ significantly as a function of the number of nearby treated springs (located within 1, 3, or 6 kilometers), controlling for the total of local springs (protected or not).

Despite reduced diarrhea, there are no statistically significant impacts on child weight, although impacts are positive and marginally significant for body mass index (BMI) in the three follow-up surveys (Table 4, regressions 7-10). We do not find evidence of differential effects at points along the child weight and BMI distributions using quantile regression (not shown).

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[6] Using the sample of children in comparison households, in a non-experimental analysis using the same controls as in Table 4 regressions 3 and 4, we find that households that choose to obtain water from protected springs do not have significantly lower diarrhea rates than other households: the coefficient on the fraction of water collection trips taken to protected springs is 0.007, s.e. 0.041. However, comparison households can also choose to obtain water from project springs, leading to imprecise estimates of the non-experimental effect and complicating comparisons with our experimental impact estimates.

There is some suggestive evidence that spring protection produces a small reduction in diarrhea among children ages 5-12 as well. In the basic specification equivalent to regression 1 in Table 4, the point estimate is -0.017 (standard error 0.005), on a base diarrhea rate of 4.1 percent, though the effect is no longer significant when the full set of controls is included. There is no evidence that spring protection improved school attendance in this age group, nor is there evidence of diarrhea impacts among adults after spring protection (regressions not shown).

We collected information on infant mortality from our household sample, and also from a somewhat larger sample of households with the assistance of local village elders who were asked to keep a diary of infant births and deaths in their communities. However, given the rarity of child death events and limited sample sizes, in neither sample is there sufficient statistical power to detect moderate infant mortality treatment effects at traditional confidence levels, although point estimates have the expected negative sign (not shown).

3.5 Estimating behavioral changes

The main behavioral change that resulted from spring protection is an increase in the use of the protected springs for drinking water; other changes appear to be minor. There were no significant changes in most water transportation and storage behaviors. There is a small shift in self-reported water boiling at home (Table 5, Panel A), though the effect is statistically significant with 90 percent confidence only for the subsample of sole source users. Take-up of home water chlorination is limited in our study area and we do not see large shifts in use after spring protection. There is also no evidence of changes in diarrhea knowledge or in a direct hygiene measure, fecal contamination on respondents' hands (Panel B). Enumerators collected additional information on springs' physical condition and maintenance, and find that protected springs have significantly "clearer" water, better fencing and drainage, and less fecal matter and brush in the vicinity (Panel C). In contrast, there is no effect on observed water yields, confirming that spring protection impacts water quality but not quantity.

Households do change their choice of water sources substantially in response to spring protection. We discussed earlier some of the implications of endogenous source choice for estimating household water quality impacts. The potential for differential impacts arises because protected spring use should increase more among multi-source users than sole-source users. Assignment to spring protection treatment leads to greater use of the sample spring for those households not previously using it exclusively: treated households increase the fraction of water collection trips to their sample spring by 21 percentage points if they used other sources at baseline (Table 5, Panel D, multi-source users). Underlying this increased use of protected springs were increasingly positive perceptions about the quality of drinking water from protected springs: respondents at treated springs were 22 percentage points more likely to believe the water is “very clean” during the rainy season, with somewhat smaller effects in the dry season.

There was no significant effect on the total number of trips made to water sources in the past week. There were small but statistically significant effects of spring protection on the reported average time it took to walk to their main drinking water source (average length was about 7.3 minutes one-way or 15 minutes round-trip), with an effect of roughly one minute. However, there was no effect on the reported time it took to walk to their assigned spring, which suggests that some of the difference in reported walking time to the main source is driven by switching to using the assigned spring. Another possibility is that water collection is slightly faster at protected springs and that some respondents mistakenly assign these time savings to reported walking times. Although queues are no more likely to form at protected springs than unprotected ones in our data, reported water collection times are shorter at protected springs by approximately one minute, even conditional upon the time of day of water collection.[7]

4. Valuing clean water

This section uses a travel cost model of water source choice to estimate household valuation of cleaner water. These revealed preference estimates are then compared to more common stated preference approaches.

4. 1 A travel cost model of household water source choice

Suppose that in choosing a water source, households trade off the cost versus the benefits. The opportunity cost of time per minute, $C > 0$, is a function of the local market wage, and we assume for simplicity that it is constant across households (in the econometric analysis below it varies across households). Thus the cost household i bears to make an additional water trip to source j is CD_i, where D_i is the round trip distance to the source. The water contamination level for water source j is $W_j > 0$, where higher values denote more contamination. The function

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[7] However, note that congestion is rare: only six percent of trips to assigned springs involved waiting in a queue.

relating water quality to health is $V(W_j)$, $V' < 0$. Any non-health benefits, for instance, the ease of water collection at a protected spring, are also captured in V. We found above that spring protection ("T") causes contamination to fall sharply, so $W_j^{\,T} < W_j$.

Households make multiple water collection trips and each trip is affected by unobserved factors, including the weather, whose turn it is to collect water, the expected queue, the direction an individual is walking for another task (i.e., to the market), or one's mood that day. Household i's indirect utility from a single water collection trip to source j at time t can be represented as:

$$U_{ijt} = V(W_{jt}) - CD_{ijt} + e_{ijt,} \tag{3}$$

where e_{ijt} is an i.i.d. error term modeled as type I extreme value. Household i chooses source j over an alternative source k if the benefits of its water quality outweigh any additional travel costs, namely when $\{V(W_j) - V(W_k)\} - C(D_{ij} - D_{ik}) \geq 0$.

Focusing on those households on the margin between using the sample spring and an alternative source conceptually allows one to estimate the value households place on spring protection. Spring water quality improvements yield potential utility benefits of $V(W_j^T) - V(W_j)$, and travel costs would have to increase by the same amount to restore households to indifference. The additional travel cost households are willing to incur is a revealed preference measure of their willingness to pay for improved water quality.

More generally, given a set of characteristics X_{ijt} for individual i and spring j at time t, where controls include both the protection status of the sample spring and the walking time to each potential local water source, as above, the probability household i chooses source j from among all water alternatives $h \in H$ at time t ($y_{ijt} = 1$) can be represented by the conditional logit formulation (McFadden 1974):

$$P(y_{ijt} \mid X) = \frac{\exp(X_{ijt}{}'\beta)}{\sum_h \exp(X_{iht}{}'\beta)} \quad . \tag{4}$$

The ratio of the coefficient estimate on the treatment (spring protection) indicator to the coefficient estimate on walking time to a source delivers the value of spring protection in terms of minutes spent walking. There is potentially heterogeneity in households' valuation of spring protection as well as in time costs. We allow the coefficient on these two terms to vary as a function of the number of children in the household and their health status, and household sanitation, hygiene knowledge, and education, by including interactions between these characteristics and the treatment indicator (and sometimes also the walking distance term).

We also explicitly estimate this heterogeneity using a mixed logit model (Train 2003, Berry, Levinsohn and Pakes 1995). Mixed logit allows for random coefficients on water source characteristics (e.g., spring protection, walking distance) in the indirect utility function. Simulation techniques are used since there is typically no closed-form solution. We estimate choice probabilities as:

$$(5) \qquad P(y_{ijt} \mid X) = \int_{\beta} \frac{\exp(X_{ijt}{}'\beta)}{\sum_{h} \exp(X_{iht}{}'\beta)} f(\beta) d\beta .$$

where y, X and β are defined as above, and $f(\cdot)$ is the mixing distribution, which we take to be the normal distribution for the spring protection coefficient and the triangular distribution (constrained to be non-negative) for the distance coefficient. Bayesian numerical methods allow us to maximize the log-likelihood to estimate the mean and standard deviation of β.

We use data from the third follow-up survey, which explicitly asked respondents about the universe of all water sources they could potentially choose and the number of trips they made to each in the last week. The median respondent used two water sources and 65% of respondents named alternatives available to them but distinct from those sources they actually used.

4.2 Estimating households' willingness to pay for cleaner water

The conditional logit analysis yields a large, negative and statistically significant effect on the round-trip walking distance to water source (measured in minutes) term, at -0.055 (standard error 0.001, Table 6, regression 1) and a positive statistically significant effect on the treatment (protected) indicator term (0.51, standard error 0.04). Other terms in the regression indicate that streams, rivers and wells are less preferred sources relative to the omitted category (non-program springs), while there are only minor differences in tastes for program (sample) springs, non-program springs and boreholes. The distance to the closest water source is only weakly correlated with a range of household characteristics, including the distance to the second closest source (not shown), alleviating some concerns about omitted variables in the estimation of how walking distance affects choice.

One issue with the interpretation of this result is possible measurement error in the reported distance walking variable. The correlation across survey rounds in the reported walking distance to the sample spring is moderate at 0.38, so attenuation bias could be important. In addition to simple recall error, the variation in reported walking time may be due to actual variation in travel time, depending on the weather, and thus the condition of the path to the spring, whether the collector is accompanied by a child, and the respondent's health or energy level that day. To approximately correct for classical measurement error in this term, we inflate its coefficient to -0.055 / 0.38 = -0.145 and use this correction in valuation calculations below.[8]

The ratio of the two main coefficient estimates in this specification implies that one round trip to a protected spring compared to an unprotected spring is valued at (0.51)/(0.145) = 3.5 minutes of walking time. Over the course of a year, using the average number of trips per week to sample springs, this is equivalent to 12.2 work days.

If spring protection yields other non-health benefits as well, as seems plausible, these estimates will be upper bounds on the willingness to pay to avoid diarrhea cases or deaths. Yet while the non-health benefits of spring protection – in terms of water appearance, taste or ease of water collection – could theoretically contribute to willingness to pay, we find no evidence that these have a major effect in practice. The inclusion of terms for measured *E. Coli* contamination available at a subset of alternative water sources, as well as the household's perception of water quality at each source, reduces the coefficient estimate on the spring protection treatment indicator to near zero (Table 6, regression 2).

One might conjecture that households have an incorrect view of the health impacts of spring protection at baseline, and that their behavior would shift over time as they learn more about true impacts. However, valuations are nearly identical for households with an additional year of experience with spring protection (not shown), so households' valuation does not appear to change with greater exposure to the protected spring.

Households with young children could potentially have both greater time costs of walking to collect water (due to the demands of child care and difficulty carrying a small child) and also greater benefits of clean water, since the epidemiological evidence suggests that young children experience the largest health gains. Empirically, households with more children under age five at baseline find additional walking distance to a source to be more costly, and this effect

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[8] The attenuation bias correction estimated in a Monte Carlo simulation is similar, at roughly 0.3 (not shown).

is especially strong for households who had young children with diarrhea at baseline: that effect is large and statistically significant at 99% confidence (Table 6, regression 3). The coefficient on the interaction between the treatment indicator and an indicator for households with child under five who had diarrhea in the past week is positive, suggesting that households with sick children also place somewhat greater value on clean water, although this effect is somewhat difficult to interpret since households where children have diarrhea at baseline may also have different underlying preferences for child health than other households.

Willingness to walk further for a protected spring does not appear to be correlated with our measures of health knowledge. Choice of protected springs is not significantly affected by diarrhea prevention knowledge (Table 6, regression 4), or by household knowledge of the link between contaminated water and diarrheal disease in particular (not shown).

Household valuations of spring protection rise with latrine ownership (perhaps reflecting underlying household taste for investing in health) and with mother's years of schooling (regression 4), yet neither asset ownership nor child gender significantly affects the taste for spring protection (not shown). The latter effect may be surprising given that health gains appear concentrated among girls.

Using the mixed logit approach, we find considerable dispersion of spring protection valuations in our sample of households: the median value of spring protection increases somewhat to 18.5 work days (with a standard deviation of 102.8 work days, Table 6, regression 5). These figures utilize the household level random coefficient estimates generated by the mixed logit, and thus take into account possible correlations in the value placed on spring protection and the disutility of walking time across households. The median and standard deviation of household willingness to pay for spring protection based on these revealed preference mixed logit results are presented in Table 7, Panel A, and then compared to the stated preference valuations discussed in the next subsection.

Combining the results from Tables 4 and 6 yields a bound on the willingness to pay to avert child diarrhea. The average number of averted diarrhea cases due to spring protection is (0.047 cases / child-week) * (1.3 children age 3 and under / household) * (52 weeks / year) = 3.2 diarrhea cases per household-year. Using our median spring protection household valuation of 18.5 work days (from the mixed logit), this would translate into 5.8 work days per case of diarrhea averted, assuming that all of spring protection's value works through child health gains, and that household put no value on improved appearance of water, ease of collection, or health gains to the household other than child diarrhea.

A different assumption, namely, that household valuation is driven entirely by reduced child mortality risk, allows us to estimate the value these households place on their children's lives using our revealed preference methodology. There are approximately 5.69 deaths per 1000 children under age five each year in sub-Saharan Africa (Lopez *et al.* 2006, Table 3B.7), and roughly 4.9 annual diarrhea episodes per African child under age five, based on Kirkwood (1991), who reviews 100 longitudinal studies of diarrheal disease in 33 African countries. If each diarrhea episode averted (by better quality water) reduced mortality risk by an equal amount, then this translates into 1.16 deaths from diarrhea averted for each 1,000 diarrhea cases eliminated. The value of averting one child diarrhea death among these households is thus 5,020 work-days, or twenty work-years (at 250 work days per year). We discuss the equivalent monetary valuations below.

4.3 Comparison of Revealed and Stated Preference Water Valuations

This subsection compares our revealed preference spring protection valuations to two different stated preference approaches, stated ranking and contingent valuation. The stated ranking approach asks respondents to rank order their potential water source options rather than relying on information on actual household water trips. This ranking is performed sequentially in the survey, with the highest ranked source eliminated from the choice set at each subsequent question. These data are then used analogously in the travel cost discrete choice analysis described above.

Estimated stated preference ranking valuation for spring protection is much higher than the revealed preference estimates. The magnitude of the coefficient estimate on distance walking falls to -0.033 while that on spring protection rises to 0.96 (Table 6, regression 6). Using the same attenuation bias correction as above, this is almost exactly three times greater than the revealed preference value (Table 7, Panel A). The willingness to pay for one year of spring protection is 57.9 work days.

Comparing the analogous columns in Table 6 (regressions 1 and 6) suggests social desirability bias may be playing a role in some respondents' stated preference rankings. The coefficient estimates on several unimproved sources many Kenyans generally think of as "bad" or unclean (e.g., streams, rivers, lakes, ponds) are far more negative in the stated preference ranking exercise than in the revealed preference case, while the estimate on spring protection is more positive.

The second stated preference method is contingent valuation (CV). Households in protected spring communities were asked how much they would be willing to pay to keep their spring protected. The CV questions were only asked of households in the treatment group since they have first-hand experience with spring protection. In the final wave of the survey, respondents were first asked if they would be willing to pay either 250 or 500 Kenyan Shillings, followed by a question that emphasized the expenditure trade-off for their assigned amount (in other words, what goods they would be giving up by spending that much on spring protection), and then were asked if they would be willing to pay the next higher amount, also with emphasis on the expenditure trade-off. This closed-end format, offering discrete value choices, is standard in the contingent valuation literature (Bateman and Willis 1999).The question wording was:

> *Now that you have seen the protected spring, suppose that somehow the spring had been split so that there was free access to an unprotected spring and restricted access to a protected spring, both at the same site. Would you be willing to pay __ Ksh for one year's access to the protected spring, assuming everyone else would also have to pay this amount too?*[9]

The main finding is that nearly all households claim to be willing to pay US$7.14 for one year of spring protection, and the majority of households are willing to pay twice that (US$14.29) even after being walked through the expenditure trade-offs by the enumerator (Table 7, Panel C). The use of the expenditure trade-off prompt reduces willing to pay substantially (by 11-14 percentage points), indicating that the CV results are sensitive to survey question framing. Valuations are also sensitive to the starting value: those respondents randomly chosen to be asked whether they valued a year of spring protection at 500 Kenyan Shillings have mean willingness to pay that is twice as high ($23.91) as those respondents first asked about a value of

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[9] The wording of the question emphasizing expenditure trade-offs was: *"So, just to be sure I understand, you would be willing to give up [price] Ksh of purchases that you currently make in order to have access to the protected part of the spring. 250 Ksh per year is about 20 Ksh every month. That's a little bit less than a half-liter of kerosene or a quarter-kilo of sugar every month. For another reference, a school uniform costs about 500 Ksh. If you had to give up something you would otherwise spend money on, would you still be willing to pay [price] Ksh for access to the protected part of the spring?"* We thank Michael Hanemann for discussions on the phrasing and framing of these questions.

250 Kenyan Shillings ($12.62), perhaps because the proposed starting value implicitly contains some information for respondents about what a "reasonable" valuation should be.

If we assume spring protection valuations are normally distributed, the distribution that best fits the CV response data (using maximum likelihood) has mean willingness to pay of US$17.64 (standard deviation US$13.09, Table 7, Panel C).

We can also estimate the willingness to pay for spring protection in monetary terms for the revealed preference and stated ranking cases by making assumptions on the value of individuals' time. Per capita income in Kenya is US$530 (World Development Indicators 2005), so with a labor share of 70% this translates into an upper bound on household willingness to pay of average annual worker earnings of US$371, or US$1.48 per work day; this falls in the middle of the range of agricultural labor wages in the study area. This implies a willingness to pay of US$17 per disability adjusted life year (DALY). Even if people could perfectly substitute time for income at the margin, which is unlikely, this is almost certainly too high a time value in our rural sample, which is relatively poor by Kenyan standards, and also since collecting water is a task for the relatively unskilled, mainly young adult women and adolescents (and sometimes even younger children; 11% of water collection trips are made by children under age 12). Because limited time-income substitution possibilities are frequently encountered (McKean, Johnson, and Walsh 1995), other authors focus on lower values, often roughly 25% of the average wage as a starting point (Train 1999).

If the water collector's time is valued at 25% of the Kenyan average wage, or US$0.35 per work day (US$0.00074 per minute), and households make our sample average of 32 water collection trips per week to the sample spring (over two thirds of their total water collection trips), 52 weeks per year, the median value to these households from protection using the mixed logit revealed preference estimates is US$6.61 per year (Table 7, Panel A). Using the same time value, the stated ranking valuation is three times as large, at US$20.64. The stated preference ranking and contingent valuation estimates are comparable under this assumption on the value of time, but both greatly exceed the revealed preference estimate.

We also develop an alternative and arguably preferable approach to valuing households' time. For a subset of contingent valuation subjects (surveyed after the round 3 household survey), we asked about their willingness to walk additional minutes to access a protected spring (versus an unprotected spring). We also implemented this approach using a closed-end format, offering respondents discrete value choices for additional minutes walked. We derive water collectors' time value by dividing the stated monetary valuation by the walking time valuation. Even if contingent valuation yields inflated valuation estimates, this is a valid measure of water collectors' time value if the degree of misreporting is equivalent for monetary value and walking time. The main advantage of this approach is that it provides data directly from household responses on the time value of water collectors themselves, which likely diverges significantly from the average adult wage.[10]

As we only had the detailed matched monetary and walking time CV data for a subset of 104 respondents, rather than the whole sample, we regressed the estimated time value on a rich set of household demographic and socioeconomic characteristics (e.g., respondent education, number of children, asset ownership) in the survey subsample and then used these estimated coefficients to predict time value for the entire sample. The resulting predicted time value in our main analysis sample ranges from 1% to 13% of the average Kenyan wage, with a mean of 6.2% (standard deviation 2%, results not shown), far below the time values that many environmental economics studies have used.

We combine this household level estimated value of time with the mixed logit random coefficient estimates for our preferred revealed preference estimates of household willingness to pay for spring protection. The median valuation for a year of access to protected spring water is only \$1.76 with a standard deviation of \$11.13 (Table 7, Panel A). The analogous median stated preference ranking estimate is \$5.00 (Panel B).

The estimated distributions for the three valuation approaches using this time value approach (in Figure 3) indicate that stated preference methods exaggerate household willingness to pay for environmental amenities in a rural Kenyan setting, and that the revealed preference approach yields more modest and less variable valuations. One plausible explanation for the dispersion is that many respondents fail to introspect carefully in hypothetical valuation exercises, and thus their resulting answers are far "noisier" than in the revealed preference case, where they face real (time) budget constraints.

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[10] In computing household level time values, we know only the bounds of valuation due to the closed-end nature of the CV questions. We address this by fitting normal distributions to both the monetary and walking time distributions, and assigning individuals the median value in the interval of the distribution defined by the bounds. For instance, among those individuals who claimed they were willing to walk 10 but not 15 additional minutes to a protected spring, the median value in the normal distribution that best fits the data is 12.61 minutes. The estimated time value is then the ratio of the monetary valuation estimate to the walking time valuation estimate.

4.4 Quantifying Health Costs and Benefits

One final set of monetary valuation results is worth mentioning. Using the household time values derived from our surveys, the value of averting one case of child diarrhea is a mere US$0.54 (0.062*1.48*5.8 working days), and to avoid a child diarrhea death just US$461 (0.062*1.48*5020 working days). Expressed in DALYs, these valuations suggest the value of averting one DALY is about US$13.6. These values are far below the estimated value of a statistical life in the U.S. and other rich countries (using hedonic labor market approaches), where the median value is approximately US$7 million (Viscusi and Aldy 2003). Studies from two poorer countries (India and Taiwan) yield estimates on the order of US$0.5-1 million per statistical life, although they are difficult to compare to our sample since they rely on data for urban factory workers in those countries, who are much richer than our poor rural respondents. We are unaware of hedonic value of statistical life estimates from the poorest less developed countries (Deaton *et al.* (2008) also find low values of life in African samples using a subjective life evaluation approach).

These figures are also much lower than the estimated costs of averting diarrhea. We begin by noting that 19% of comparison children in our sample (who were under age three at baseline or born since then) are reported having diarrhea in the past week across all survey rounds. All other things equal, and assuming constant numbers of children per household for an average of 38 spring using households post-protection (of which 80% have a child under age 3)[11], this implies 59,263cases at households that use sample springs per year over a spring's 15 year lifespan, and (291,816) * (0.047/0.19) = 291,816 cases averted as a result of spring protection. The cost per diarrhea case averted due to spring protection works out to US$0.63 and the cost per death averted to over $541.21.

We can also calculate the number of Disability Adjusted Life Years (DALYs) averted by spring protection, using the standard WHO approach.[12] This calculation is a function of the average length of a diarrhea episode, and the number of deaths that occur per 1000 diarrhea cases. An ongoing high-frequency data collection effort, where we collect morbidity diaries,

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[11] The number of households using the treatment springs increased by an average of 22% following protection, according to spring census activities we conducted, as discussed further below, however only 80% of households have a child under age three living with them

[12] This is an underestimate of spring protection's benefits if there are benefits for people over age three,but this is likely the group where mortality impacts are overwhelmingly concentrated. For more information on the DALY concept, see: http://www.who.int/healthinfo/boddaly/en/index.html.

indicates that average diarrhea episode length in our sample is 5.2 days. With 1.16 deaths per 1000 diarrhea cases (as discussed above), spring protection averts 8,241 DALYs, at a cost of US$17 per DALY averted.

Based on these estimates of the cost-effectiveness of spring protection, health planners would conclude that this is an extremely cost effective intervention. The standards for health interventions in developing countries usually use $150 as a cut-off for such calculations (World Bank 1993). The revealed preference estimates of valuations, however, do not support this conclusion. The valuation of the intervention is below its benefits, as the estimated bound of the valuation of child health is significantly below levels commonly assumed.

5. Simulating alternative property rights institutions

Current Kenyan law and custom prevent landowners from charging local spring users for water. Perhaps partially as a result of these weak private property rights, virtually no springs are privately protected in our study area. In this section we use the valuation estimates derived above to determine the socially optimal level of spring protection in this region, and then estimate welfare under alternative property rights institutions. The distributional consequences of assigning private property rights to individuals depend on who is allocated those rights; for convenience, we refer to the person allocated property rights over spring water as the spring owner throughout.

In general, optimal spring protection decisions will depend on whether other nearby springs are also protected, given households' ability to choose among nearby sources within walking distance. In the case of N springs, the social planner compares all 2^N protection versus non-protection combinations. To build intuition, we first consider the case of an isolated spring in section 5.1, before moving on to the more realistic case of an area with multiple local springs in section 5.2. In this latter case, we consider nearby springs within a Kenyan sub-location, a relevant local government authority that often also corresponds to the region dominated by a local kinship clan, an important dimension of social identity in Kenya.

In both sections 5.1 and 5.2, we treat the marginal cost of providing additional spring water as zero. Unused water at a spring simply flows away, and most springs in the region have few enough users that congestion is minimal. We also abstract from the costs of enforcing property rights and consider the narrower question of what outcomes social norms and institutions would produce if they could be fully enforced. This discussion should thus be taken as an analysis of the welfare impacts of alternative social institutions and not necessarily as an exercise in Kenyan government water policy alternatives, since there are likely to be significant costs in enforcing private property rights not considered here, as well as other costs in the transition to a new property rights regime. Finally, for the most part we take households' preferences as given, but we also discuss how the analysis would change if the social planner placed higher valuation on the health benefits of spring protection than households themselves. This case might be relevant, for instance, if households systematically underestimate social benefits due to infectious disease externalities, or if outsiders like foreign aid donors value child diarrhea reductions more highly than other consumption by these households.

5. 1 The social planner's protection decision for a single spring

We begin by considering a social planner's problem at an isolated spring given observed household willingness to pay for spring protection. Spring protection costs an average of US$1024 per spring, with maintenance costs of US$35 per year. Assuming that protected springs last for 15 years, this implies the discounted net present cost of spring protection is US$1405 (with a 5% annual discount rate).[13]

The total benefit of protecting a spring is the sum for all current users of willingness to pay for spring protection, plus any consumer surplus that generated by new users. Although the median household's willingness to pay is only US$1.76 per year, the mean WTP is somewhat higher, at US$2.96. The typical spring in our data is used by 31 households so the total discounted net present valuation of existing users for typical springs is US$1000.06.

A census at a subset of sample springs suggests that protection increases the number of spring user households by 22% when the water is free. The welfare gains to protection for these new households is presumably smaller, since they preferred an alternative source to the unprotected spring at baseline. For instance, we find in the census that many new users live a greater distance from the spring than most baseline users. For a useful first order approximation of the welfare gains for this group, we can assume that their post-protection consumer surplus is

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[13] The local District Water Office finds that protected springs last between ten and fifteen years on average. We use the top end of this range as our construction and maintenance cost figures correspond to the case of well-protected and maintained springs.

uniformly distributed between zero and US$2.96 (as might be the case if households live at a continuum of distances from the spring but their underlying taste for clean water is otherwise similar), so the average gain for a household in this group is half that for a baseline household, of US$1.48. The total benefits of spring protection to the typical spring under these assumptions are US$1110.07, still substantially below the estimated cost of protection. The benefits are estimated to exceed the cost of protection only for springs with 46 or more baseline household users (assuming the same increase in new users post-protection). This suggests that protection might be optimal in densely populated rural areas, in towns and cities, or in areas with few springs (where each has many users). In the next subsection we make these calculations more accurate by simulating demand for spring protection using the actual location of these and multiple other local water sources for our sample households, and then examining social welfare under a variety of alternative social norms and institutions.

5.2 Simulations of alternative social norms and property rights institutions

We consider the impact of alternative social norms regarding property rights to water. Private property rights would allow landowners to charge for access to spring water, providing an incentive for them to invest in protection. A downside is the static distortion introduced by pricing spring water above its marginal cost of zero, and as we discuss below this can alter both the water sources households choose as well as the walking time they incur in collection.

Household demand parameters are derived from the revealed preference mixed logit results in Table 6, column 5. The term $g \in \{1, \ldots, G\}$ denotes a partition of nearby springs into contiguous subgroups, $j \in \{1, \ldots, J\}$ are the springs within each subgroup, while $i \in \{1, \ldots, I\}$ refers to households that choose among these sources. The utility value of spring protection for household i from spring j in group g is β_{ijg}, γ_{ijg} is the household's disutility from an additional minute of walking time, while δ_{ijg} is the value of water collector's time (from the survey method described above). This value of time allows us to convert utility into monetary values, which we focus on throughout. Household preferences can be represented by $\theta_{ijg} = \{\beta_{ijg}, \gamma_{ijg}, \delta_{ijg}\}$, where $F(\theta_{ijg})$ is the joint distribution of preferences in the population as a whole.

These parameter estimates are available for each household, and allow us to compute household utility per water collection trip (as in equation 3) to a particular source w, denoted u_{wijg}, as well as the total trips they chose to make to each of their alternative water sources in a full year, T_{wijg}. Annual household utility (in monetary terms) for household *i* from water consumption is thus $V_{ijg} = \frac{\sum_{w}(T_{wijg} * u_{wijg})}{\delta_{ijg}}$, minus any costs incurred purchasing the water. In our data, recall that the total amount of household water collected varies neither with spring protection status nor with distance to the assigned spring, allowing us to simplify the problem by assuming that the total number of water trips a household collects across all sources is fixed ($\sum_{w} T_{wijg} = T_{ijg}^{*}$ for household *i* regardless of their choices among sources $w \in \{1, .., W\}$).

We simulate the following game. In t = 0, the property rights regime is chosen. Spring owners within a group *g* simultaneously decide whether or not to protect their spring at t = 1, where protection is denoted by the indicator variable $protect_{jg}$, and then simultaneously set a price per unit of water collected p_{jg} (t = 2).[14] The vector of protection decisions and price at spring group *g* are denoted $protect_{g}$ and p_{g}, respectively.[15]

We find optimal prices either through a grid search (when computationally feasible) or the numerical Nelder-Mead simplex method based on the profit functions' first order conditions described below. (Further details are in the simulation appendix, available upon request.) We assume that pricing is linear on the amount of water collected and neither spring owners nor policymakers can price discriminate because they cannot prevent resale of water. We do not allow for collusion among spring owners in price setting or investment.[16]

Spring owners maximize profits, which are equivalent to the net present value of revenues minus any spring protection construction and maintenance costs (since the marginal cost of providing a unit of spring water is zero), over the 15 year time horizon used above. The initial spring protection investment cost is denoted c_0, while the recurrent annual maintenance cost is c_r. The discounted net present cost of spring protection is $C \equiv \left[c_0 + c_r \sum_{\tau=1}^{15} \left(\frac{1}{1+r} \right)^{\tau-1} \right]$, which we showed is US$1405 in our setting.

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[14] We also incorporate demand from new household users' post-protection using information from the user censuses, as described in the simulation appendix.

[15] We ignore any water consumption utility gains for spring owners since they would not necessarily live locally or consume spring water.

[16] To determine the Nash equilibrium choice of spring protection with multiple springs, we estimate best responses to all possible protection / non-protection combinations (there are at most 2^4 = 16 in our groups of four springs) and search for a fixed point.

We consider the impact of changing property rights institutions on spring investments and pricing outcomes, holding constant policies for other water sources. This approach is realistic in the Kenyan setting, where there is typically open access through public paths to naturally-occurring rivers and lakes so people can collect water for free at these places. We assume this would continue to be possible, and most boreholes are sunk on public property such as schools or market centers where there is no private landowner, and water from these sources is generally available for free.

In our simulations, sample households can choose to obtain water from the spring they used at baseline, plus any other potential alternative water sources they themselves listed in the survey, as well as all the other sample springs within their spring group *g*. We generally considered springs located within 1 km of each other to be part of the same group, although in some cases springs at a greater distance from each other were grouped together. We analyze groups of contiguous springs within the same administrative sublocation, and consider groups of up to four nearby springs for analytical tractability; analyzing the full interdependence of spring protection decisions in the full sample of 167 sample springs with 2^{167} possible protection combinations quickly runs into computational limitations.

In sublocations where the number of springs is not divisible by four, we created the largest possible spring groups (e.g., a location with seven springs was generally divided into one group of four springs and one of three springs) unless a spring was located far from others (generally more than 1 km), in which case it was not grouped with other springs. The solution with groups of up to four nearby springs is only an approximation to the full solution but we believe it is a fairly close approximation. We have run similar analyses with groups of one spring, two springs, and three springs in addition to the four spring case we focus on here, and the main findings about the relative welfare consequences of different property rights institutions are nearly unchanged as the degree of competition increases.

We normalize total social welfare to zero in the benchmark "status quo" case with common property rights to water and thus no spring protection (Table 8, row 1). We express household utility and social welfare in U.S. dollar values on a *per spring* basis throughout. We generally do not present results on a per household basis since the number of spring users can change with protection status, but to get a rough sense of per capita welfare gains recall that the baseline number of household spring users is 31 and each household contains has an average of 6.6 members, for roughly 200 persons per spring community.

The social planner's decision problem can be represented as follows, where W^S denotes social welfare:

$$(6) \qquad \underset{\{protect_g\}_g}{Max} W^S = \sum_{g=1}^{G} \left\{ \sum_{j=1}^{J} \sum_{i-1}^{I} V_{ijg}(protect_g \mid \theta_{ijg}) - C * \sum_{j=1}^{J} protect_{jg} \right\}$$

where $V_{ijg}(protect_g \mid \theta_{ijg})$ denotes household utility given that the planner is fully informed about true household preference parameters, θ_{ijg}.

We find that the social planner would protect 27.5% of sample springs (46 springs in all), which are typically the springs with many baseline household users, and would equate the price of access to spring water to the marginal cost of provision, which is zero in this case. The net social gain, across all springs (protected and unprotected) is US$340.84 per spring, or roughly US$1.70 per capita. To illustrate the importance of using the revealed preference willingness to pay for clean water, when the social planner exercise is carried out using the stated preference ranking WTP mixed logit estimates, and the value of time set at 25% of the local wage – as in a standard environmental economics analysis – 71.3% of sample springs would be optimally protected, far above the 27.5% we find using revealed preference valuations.

We next focus on the simplest private property rights case, what we call "full" private property rights, where social norms are such that there are no restrictions on spring owners' behavior. Spring owners have some market power, given the limited alternative water choices available. They engage in uncooperative play with other nearby spring owners, recognizing that they are also setting prices and making protection decisions to maximize profits and we thus solve for the Nash equilibrium.[17] We assume that spring owners set prices with full knowledge of the water source choice situation facing each household, including the distance to each of the household's options, its disutility of walking time, and the household specific willingness to pay for spring protection. The optimization problem for the owner of spring j in group g, where π_{jg}

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[17] In a small number of spring groups (3) there are multiple Nash equilibria in the full private property rights case. Here we assume there is coordination on the equilibrium that maximizes social welfare, although given the small number of cases other equilibrium selection rules do not change the main results. Thus the social welfare outcomes in the Table 8 full private property rights case (where the multiple equilibria arise) likely represent upper bounds, yet despite this there are substantial welfare losses relative to the status quo, as described below.

denotes profits and the spring owner takes others' prices p_{-jg}^{*} and protection decisions $protect_{-jg}^{*}$ as given, is presented in equation 7:

$$(7) \qquad \underset{protect_{jg}}{Max}\ \pi_{jg} = p_{jg}^{*} \sum_{j'=1}^{J'} \sum_{i=1}^{I} \left\{ T_{jij'g}(protect_{jg}, protect_{-jg}^{*}, p_{g}^{*}) \mid \theta_{ijg} \right\} - C * protect_{jg}$$

The double summation refers to all households in the spring group that j belongs to, and $T_{jij'g}$ is the number of trips the household makes to spring j given the equilibrium protection status and price decisions of all springs in the group. This is solved subject to optimal non-cooperative price setting: $p_{jg}^{*} = \arg\max_{p_{jg}} \pi_{jg}(protect_{jg}, protect_{-jg}^{*}, p_{-jg}^{*})$.

In the "full" private property case, only 5.4% of spring owners find it profit maximizing to protect their spring given local water demand conditions and competition from other spring owners (Table 8, row 3). Among the springs that it is socially optimal to protect, only 17.4% of spring owners choose protection. The net present value of profits per spring owner is US$441, and the average price charged per water collection trip in these springs is US$0.0028. For the typical household making 32 trips per week to such a spring, in a year this is equivalent to US$4.66, several days' wages. Note that nearly all households (97%) are worse off under full private property rights compared to the status quo, and that full private property rights substantially reduce social welfare relative to communal property rights, with a social welfare loss of US$77.53 per spring. This may be a partial explanation why communal property rights to water have been so durable in rural African settings like the one we study.[18]

All households are now paying for access to spring water but few realize health benefits since the vast majority of springs remain unprotected.[19] Roughly a fifth of the total household welfare loss is due to the distortion induced by pricing about marginal cost. Moreover, the proportion of household trips to the dirtiest local sources, rivers and streams, increases by 45% relative to the status quo case (from 5.8% of trips in the status quo up to 8.4% in the full private property rights case), and the use of sample springs falls 38%, as households increasingly abandon the now expensive but relatively clean spring water. The average walking time per

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[18] These results are of course specific to time and place. Galiani et al (2005) find that the privatization of water infrastructure in Argentina led to large child health gains.

[19] In results not presented in Table 8, the social welfare consequences of full private property rights remain negative even when household valuation for spring protection is considerably higher, using time values based on the 25% of the average Kenyan wage figures (from Table 7, Panel A).

water collection trip rises by over a minute from 11.6 to 12.9 minutes, an important change for households making nearly fifty weekly collection trips to all sources on average. [A key reason is that spring owners are unable to capture the full consumer surplus of potential users due to both heterogeneity of preferences among users due to heterogeneity in valuations by users.]

It is worth considering other property rights social norms and institutions as well. Locke (1689 [2002]) argued that people acquire property rights in land when they mix their labor with it, for example by clearing land or planting a crop. This element of property rights is common in rural Africa and elsewhere. For example, in Ghana actively farming a plot is critical to securing property rights (Goldstein and Udry 2005) and clearing land is traditionally necessary for establishing rights to plots at the margin of tropical forests in the Amazon. A "conditional" private property norm would only permit spring owners to charge positive prices if they had invested in spring protection. The spring owner's profit maximizing condition remains the same but now the price is constrained to be $p_{jg}=0$ if $protect_{jg}=0$, increasing the private incentive to invest in spring protection to capture rents.

There are forces for both under- and over- protection with conditional private property rights that could potentially lead to under provision of protected water at some springs and over provision of protection at other springs. Spring owners' continued inability to price discriminate and thus capture the full consumer surplus from protection leads to under-protection, as in the full private property case. On the other hand, the possibility that protecting a spring allows them to capture not just the valuation placed on spring protection but also part of the surplus from consuming unprotected spring water could lead to over-protection. Rent-stealing effects between competing spring owners could also lead to over-protection.

In practice, there is substantial under protection (Table 8, row 4). Simulation suggests this policy yields somewhat higher rates of investment in water infrastructure: now 10.8% of spring owners find it profit maximizing to invest in spring protection under conditional private property rights. Although net social welfare remains much lower than the status quo, "conditional" private property rights is marginally better for household welfare than full private property rights: only 18.7% of households experience utility losses, the average welfare loss per spring community is only US$44.86, and the proportion of household trips to rivers and streams increases by only 10% relative to the status quo, while the increase in average walking time is 0.8 minutes.

Another alternative form of “open access” private property rights would permit spring owners to charge for water from spring protection infrastructure as long as they allowed free unimpeded access to unprotected water from the spring, an institution that is related to some historical examples from the Islamic world, in which charging for infrastructure that improve water quality and water delivery mechanisms is acceptable, while charging for water *per se* is not (Caponera 2006). A system of “open access” property rights is also desirable on theoretical grounds: if consumers were homogenous, spring owners could exactly capture the surplus from protection and thus would have optimal incentives to protect springs. In our setting a system of “open access” property rights could be achieved simply by requiring land owners to allow some water to flow away from the protected spring and pool elsewhere, where it is exposed to the environment and becomes a pool of unprotected spring water. In this case, the spring owner’s profit maximizing condition remains unchanged from the conditional private property rights problem above, although the spring owner must take into account that households now have the choice of continuing to consume unprotected spring water at price $p_{jg}=0$ at the source or paying p_{jg}^* for protected spring water.

The analysis indicates that, at the springs the planner would protect, 8.7% of land owners choose to protect their spring under “open access” property rights, while no land owners protect springs the planner would not protect (Table 8, row 5). While this form of property rights does not achieve the socially optimal level of protection, it does incentivize spring owners to perform some socially beneficial spring protection while at the same time limiting both the welfare costs to consumers and the distortions due to above marginal cost pricing. The availability of free unprotected spring water shields households from the utility losses they would experience under the other private property cases, and no household is worse off than under the status quo of common property rights. Spring owners still earn moderate positive profits and the fraction of trips to the dirtiest sources is nearly the same as in the social planner case; thus this social norm constitutes a Pareto improvement over the status quo.

There are also multiple forms that public provision of spring protection investment could take. One could consider the impact of public funding of spring protection by a hypothetical benevolent government that, unlike a social planner, had access only to distortionary taxation and knows only the distribution of preferences in the sample as a whole, $F(\theta_{ijg})$ not individual household preferences. The policymakers' optimization problem is the following, where *DW* denotes the deadweight loss of taxation per dollar of revenue raised:

$$(8) \qquad \underset{\{protect_g\}}{Max}\, W^P = \sum_{g=1}^{G} \left\{ \sum_{j=1}^{J} \sum_{i-1}^{I} \tilde{V}_{ijg}(protect_g \mid F(\theta_{ijg})) - (1+DW) * C * \sum_{j=1}^{J} protect_{jg} \right\}$$

Under the assumption that DW=0.3 (Ballard *et al.* 1985)[20], 24.3% of springs are protected but there is misallocation of protection: 56.0% of those the social planner would optimally protect get protected and 12.2% of those the planner would not protect (Table 8, row 6). The welfare gain per spring is US$115.86, much less than the US$340.84 welfare gain attained by a social planner, and high deadweight loss assumptions make the government provision option even more unattractive. In addition to the tax distortion, the misallocation of protection across springs due to policymakers' limited information on local preferences is a key driver of the lower social welfare relative to the social planner case.

Finally, consider a regime where households are provided vouchers for access to spring water which they give to spring owners, and which spring owners can then exchange for a fixed payment from benevolent local government authorities. The government optimally sets the voucher payment taking into account the later private protection decisions of spring owners. We again assume that the policymaker only knows the distribution of water preferences in the entire population, but that spring owners have perfect knowledge of local household water preferences. Spring owners are also assumed to be restricted from charging top-up fees to water users, perhaps due to social norms like those in Kenya that frown on charging for water access).

The policymaker maximizes social welfare W^V by setting the uniform voucher price p_v:

$$(9) \qquad \underset{p_v}{Max}\, W^V = \sum_{g=1}^{G} \left\{ \sum_{j=1}^{J} \sum_{i=1}^{I} \left\{ \tilde{V}_{ijg}(protect_g^*(p_v) \mid F(\theta_{ijg})) \right\} + \sum_{j=1}^{J} \left\{ \pi_{jg}^*(p_v) \right\} \right\}$$

$$- DW * p_v \sum_{j=1}^{J} \sum_{i=1}^{I} T_{ijg}(protect_g^*(p_v) \mid F(\theta_{ijg}))$$

such that spring owner protection decisions are profit maximizing:

$$(10) \qquad protect_{jg}^* = \arg\max \pi_{jg}^*(p_v)\,,$$

where spring owner profits are defined as in equation 7 above.

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[20] Results are similar with the assumption that DW=0.5 (results not shown).

When we again consider a 30% deadweight loss, the optimal voucher price is US$0.001 per trip (much less than the price charged in the full private property rights case), social welfare gain under water vouchers is US$112.90 (or roughly US$0.56 per capita). The proportion of landowners that choose to protect their springs is 11.5%, which is still short of the social optimum although there is less misallocation of protection in this case: only 5.1% of protected springs the social planner would not protect get protected (Table 8, row 7). This policy improves social welfare substantially relative to the status quo and the private property rights cases, and is comparable to government investment. In contrast to full and conditional private property, the voucher regime leaves few households worse off relative to the status quo since the government vouchers are distributed for free; the only losers are the relatively small number of households estimated to have negative valuations of spring protection, though note that these same households also lose out in the social planner's solution.

Taken together, combining common property rights in unprotected water and private property in treated water in the open access private property rights case delivers higher social welfare than the status quo, although the voucher policy and government investment still deliver the largest gains of all property rights institutions we consider. That said, the private provision cases may have some advantages over public investment that are not modeled here, especially with regards to ongoing incentives for spring maintenance over time. For instance, an inept or corrupt government's voucher program may collapse after a short time, in which case the open access private property rights case could be preferable to the voucher case, the public provision case, and to the status quo of common property.

The analysis above also abstracts from transaction costs in collecting fees, but these are likely to be large under private property institutions. In particular, it may be expensive for spring owners to monitor the amount of water that households collect, and the transaction costs involved in water sales – which have not been considered at all in our simulations – would reduce social welfare benefits. Water privatization in rural Kenya today is likely to run into extensive local resistance in practice, since it runs against strongly held traditional social norms regarding communal access to water resources. There are also likely to be non-trivial transition costs in moving from one system to another. These would likely be particularly large for non-Pareto improving approaches. A key advantage of maintaining open access to unprotected spring water is that it would be Pareto improving relative to the status quo, and thus we believe that open access private property rights would be better received in Kenya than full privatization.

The voucher regime has a number of advantages over the open access private property cases. First, spring owners might be tempted to find a way to impede access to the unimproved water at their spring under a system in which they could charge for improved water only if free access to unprotected water was maintained. Second, and perhaps more critically, it is straightforward to adjust the voucher payment level to reflect the external social value of spring protection and its associated health gains. Consider a benevolent policymaker who values spring protection twice as much as households due, for instance, to households' failure to consider the infectious disease externality benefits to other households from reducing diarrhea, to agency problems within the households that lead child health to be undervalued by some parents, or to taking into account the policymaker's or foreign aid donors' stronger preferences for better child health. In this case, the policymaker chooses to set a uniform voucher price of US$0.0017, 70% higher than the voucher price level previously chosen, and as a result 28.3% of spring owners choose protection . More generally the voucher price can be varied to attain nearly any degree of investment in water infrastructure, reflecting any level of preference for child diarrhea reduction.

Ultimately, these simulations of the impact of alternative norms are by necessity only suggestive of optimal rural water policy. In any particular context, the transactions costs, technological ease, and political acceptability of vouchers and the open access privatization policies would have to be carefully compared in detailed pilot studies in order to choose the best real-world policy.

6. Discussion and conclusion

We find that spring protection dramatically improved source water quality in a rural African setting, reducing contamination by two thirds on average and home water contamination by nearly one quarter. Child diarrhea fell by roughly one quarter. In contrast to common interpretations in the existing public health literature, we do not find evidence that spring protection led to larger home water quality gains when hygiene knowledge or latrine coverage were better. Also, spring protection did not lead to any detectable changes in water collection, transport, or storage practices, water quantity used, or to changes in any other preventive health behaviors that we measured.

There were sharp changes in water source choices among some households. By capitalizing on the observed changes in water source choice, we develop revealed preference estimates of willingness to pay for improved water quality. Because of the experimental research design, these travel cost estimates are not subject to typical econometric concerns, and can be used to validate the reliability of stated preference estimates of valuation. We find moderate median household valuation for spring protection, on the order of 18.5 work days, or US$1.76, per household annually. This translates into an upper bound on household willingness to pay approximately 3.5 work days, or US$0.54, per averted child diarrhea case, or US$13.6 valuation per DALY. These are approximately one-third of stated preference valuations and less than one-tenth of valuations typically used by health planners.

These valuation estimates suggest that either that households do not understand the causal chain between cleaner water, reduced diarrhea and lower infant mortality or that they place much lower value on improving infant and child health than typically assumed by public health planners. If one accepts these implied valuations, a social planner would protect only approximately 27.5% of springs in the area, typically those which are used by many users.

We show that different property rights institutions have substantial welfare consequences. The counterfactual policy simulations based on household revealed preference valuation suggests that, in the context we examine, existing social norms allowing communal access to naturally occurring springs are preferable to full private property norms. However, supplementing these norms either by allowing spring owners to charge for improved water while maintaining open access to unimproved sites, or by instituting vouchers through which the government pays spring owners based on the number of users served with improved water, could lead to better outcomes than either the status quo or full private property rights.

It is worth noting that some legal systems seem to have evolved in one of the directions we found promising here, that of maintaining open access to unimproved water sources while allowing private agents to charge for the services involved in improving water quality. For instance, despite the fact that access to water is considered a human right and water sales are strongly discouraged by several *hadith* (see the discussion in Caponera 2006), in certain later Islamic legal traditions, the builders of wells and irrigation canals were permitted to charge others for access to the water made available by their investments (Mawardi 1901: 316, Wanasharisi 1909: 285). Determining the most effective way to transition from one set of property rights institutions and social norms to another is beyond the scope of this paper, but remains a matter of major importance in the study of economic development.

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Appendix A: Sample randomization procedures

The randomization procedure for the 200 springs was designed for balance on observable variables. For each of three data collection "waves" of springs in the study (containing 68, 70, and 62 springs), we found a randomized, balanced assignment to treatment and control groups using the following procedure.

First, all spring communities were stratified by both water quality (into three groups, low, medium and high contamination levels) and by geographic region (the three main administrative divisions in our sample: Mumias, Butula, and Busia/Nambale). This resulted in nine distinct strata of springs. Within each stratum, springs were randomly allocated (using a computer random number generator in the STATA statistical program) to either treatment (protection) in 2004, treatment in 2005, or the comparison group (at ratios of approximately 1:1:2). Because the stratum sizes were not all multiples of four, there is still some imbalance in sample sizes across groups despite stratification.

Second, we next carried out 100 randomizations (for wave 1) and 200 randomizations (for waves 2 and 3) and identified the most "balanced" randomization along the following five observed baseline characteristics: total coliform bacteria level at the spring; *E. coli* bacteria level at the spring; approximate distance to nearest tarmac road (in meters); approximate slope of ground near the spring; number of household spring users. For each randomization, we checked for balance by regressing each of these five observables on indicator variables for treatment and comparison groups. We considered the t-statistics from these regressions and chose the one that minimized the largest t-statistic (in absolute value) across all five variables. If this value was above 1, we drew another 100-200 randomizations, repeating this procedure until the largest t-statistic was below 1. Finally, this balance test was applied to all three waves considered together, and a randomization satisfying the maximum t-statistic requirement overall was chosen.

Bruhn and McKenzie (2008) argue correctly that this process of re-randomization to achieve balance on observables may lead standard errors to be either under- or over-estimated. They show that correct inference can be achieved by including the "balancing" observables in the regression analysis as control variables, and we do this throughout the paper. In this case, the treatment effect estimates are interpreted as impacts conditional on these observables. It is worth noting, however, that coefficient estimates and standard errors are nearly unchanged if these controls are excluded from the analysis. (The one exception where the baseline "balancing" controls are not included is in the conditional and mixed logit analysis in Table 6. Here the controls do not affect inference since they are constant across all alternative water sources in a community and effectively drop out of the estimation equation.)

We also performed a randomization inference exercise that required generation of 10,000 placebo treatment groups using exactly the same re-randomization procedure described above. For each of the treatment coefficients estimated in the regressions in our tables, we computed the randomization inference p-values, using a Monte Carlo extension to Fisher's exact test; see Imbens and Wooldridge (2008). The p-value is the number of regressions (including the real assignment regression) with a coefficient at least as large in absolute value as the coefficient from the real assignment regression. The results are again largely consistent with the standard errors presented in the tables, and in fact in many cases the randomization inference procedure produced slightly smaller p-values than those we present, suggesting that our results are somewhat conservative. Overall, the re-randomization procedure to achieve balance on baseline observables does not appear to have substantially affected statistical inference. We thank Lorenzo Casaburi and Owen Ozier for their excellent work on this issue.

Appendix B: Measuring water quality, diarrhea, anthropometrics, and hygiene knowledge

Water quality

Water samples were collected in sterile bottles by field staff trained in aseptic sampling techniques. At springs, the protocol was as follows. The cap of a 250 ml bottle is removed aseptically. Samples are taken from the middle of standing water and the bottle is dragged through the water so the sample is taken from several locations at unprotected springs, while bottles are filled from the water outflow pipe at protected springs. About one inch of space is left at the top of full bottles. The cap is replaced aseptically. In homes, following informed consent procedures, respondents are asked to bring a sample from their main drinking water storage container (usually a clay pot). The water is poured into a sterile 250 ml bottle using a household's own dipper (often a plastic cup).

Samples were then packed in coolers with ice and transported to water testing laboratories for same-day analysis. A substantial fraction of water samples were held for longer than six hours, the recommended holding time limit of the U.S. EPA, but baseline water quality measures are balanced across treatment and comparison groups when attention is restricted to those water samples incubated within six hours of collection, yielding the most reliable estimates (results not shown). Extended holding time increases the noise in the *E. coli* estimate, but there is no definitive direction of bias as bacteria both grow and die prior to incubation.

The labs use Colilert, a method which provides an easy-to-use, error-resistant test for *E. coli*, an indicator bacteria present in fecal matter. Our lab procedures were adapted from EPA Colilert Quantitray 2000 Standard Operating Procedures. A continuous quantitative measure of fecal contamination is available after 18-24 hours of incubation. *E. coli* MPN CFU measurements provided by Colilert can take values from <1 to >2419. In the analysis, we treat values of <1 as one and values of >2419 as 2419, although in practice, there are very few censored observations. We categorize water samples with *E. coli* CFU/100 ml ≤ 1 as "high quality" those with counts between 1 and 126 "moderate quality" those and with counts > 126 as "poor quality". For reference, the U.S. EPA and WHO standard for clean drinking water is zero *E. coli* CFU/100 ml, and the EPA standard for swimming/recreational water is less than 126.)

Quality control procedures used to ensure the validity of the water testing procedures included monthly positive and negative controls, and duplicate samples (blind to the analyst), as well as occasional inter-laboratory controls. There are several potential sources of measurement error. First, Colilert generates a "most probable number" of *E. coli* coliform forming units (CFU) per 100 ml in a given sample, with an estimated 95% confidence interval. Second, samples that are held for more than six hours prior to incubation may be vulnerable to some bacterial re-growth/death, making tested samples less representative of the original source. Third, sampling variation is an issue given the small size of the collection bottle (at 250 ml).

It is common to use *E. coli* to quantify microbacteriological water contamination in semi-arid regions like our study site. The bacteria *E. coli* is not itself necessarily a pathogen, but testing for specific pathogens is costly and can be difficult. Dose-response functions for *E. coli* have been estimated for gastroenteritis following swimming in fresh water (Kay *et al*. 1994), but such functions are location-specific because fecal pathogen characteristics and loads vary over space and time. In a district near our study site, a U.S. Centers for Disease Control study finds that the most common bacterial pathogens are Shigella and non-typhoidal Salmonella.

We thank Sandra Spence for her guidance on these procedures.

Child diarrhea

For all children in the compound under age five the respondent is asked about as the incidence of "three or more loose or watery stools in a 24 hour period," over the period of the past day and the past week. This definition of diarrhea is identical to that used by Aziz *et al.* (1990) and Huttly *et al.* (1987). Additional information about measuring diarrhea in this sample is in Kremer, Miguel, Null, Van Dusen, and Zwane (2009).

Child anthropometrics

Enumerators used a board and tape measure to measure height for children older than two years of age, and digital scales for weight. The height of children under two was measured as their recumbent length using a measuring board, and a digital infant scale measured their weight.

Hygiene knowledge and behaviors

A baseline "diarrhea prevention knowledge score", was constructed based on the number of correct responses to an unprompted question on methods to prevent diarrhea; provided. The set of plausible answers include "boil drinking water", "eat clean/protected/washed food", "drink only clean water", "use latrine", "cook food fully", "do not eat spoiled food", "wash hands", "have good hygiene", "medication", or "clean dishes/utensils". Hygiene behavior was explored by measuring contamination of people's hands. To measure fingertip contamination, respondents pressed their hands into KF Streptococcal media (agar plates), and the lab isolated *fecal streptococci* bacteria colonies. Fingertip contamination was measured in only one round of follow-up data collection, so the reported coefficient gives the difference between the treatment and comparison groups rather than the difference-in-difference estimate.

Table 1: Baseline descriptive statistics (2004 survey)

	Treatment (protected) Mean (s.d.)	Obs.	Comparison Mean (s.d)	Obs.	Treatment – Comparison (s.e)
Panel A: Spring level data					
Ln. *E. coli* MPN (CFU/ 100 ml)	3.90	98	3.79	95	0.11
	(1.95)		(1.97)		(0.28)
Water is high quality (*E. coli* MPN ≤ 1)	0.05	98	0.06	95	-0.01
	(0.22)		(0.24)		(0.03)
Water is high or moderate quality (*E. coli* MPN <126)	0.70	98	0.69	95	0.01
	(0.46)		(0.46)		(0.07)
Water is poor quality (*E. coli* MPN 126-1000)	0.19	98	0.23	95	-0.04
	(0.40)		(0.42)		(0.06)
Water is very poor quality (*E. coli* MPN ≥ 1000)	0.10	98	0.07	95	0.03
	(0.30)		(0.26)		(0.04)
Latrine density (fraction of homes with latrines)	0.85	98	0.88	95	-0.02
	(0.16)		(0.15)		(0.02)
Average diarrhea prevention knowledge score	3.06	98	3.19	95	-0.13
	(0.87)		(1.17)		(0.15)
Iron roof density (fraction of compounds with iron roof)	0.70	98	0.68	95	0.03
	(0.21)		(0.23)		(0.03)
Other variables used for balancing:					
Distance of spring from paved road (meters)	3005	98	3028	95	-23
	(2101)		(2198)		(310)
Slope of catchment area (1=flat, 5=very steep)	3.56	98	3.59	95	-0.03
	(0.69)		(0.63)		(0.09)
Number of households that use the spring	29.90	98	29.60	95	0.30
	(13.99)		(14.33)		(2.04)
Butere district indicator	0.34	98	0.32	95	0.02
	(0.48)		(0.47)		(0.07)
Mumias district indicator	0.41	98	0.40	95	0.01
	(0.49)		(0.49)		(0.07)
Total coliform MPN (CFU/ 100 ml)	2170	98	2152	95	17
	(622)		(624)		(90)
E. coli MPN (CFU/ 100 ml)	265	98	248	95	17
	(548)		(552)		(79)
Water is poor or moderate quality (*E. coli* MPN 100-1000)	0.23	98	0.26	95	(0.03)
	(0.43)		(0.44)		(0.06)
Panel B: Household summary statistics					
Ln. *E. coli* MPN (CFU/ 100 ml)	3.22	733	3.33	712	-0.11
	(2.22)		(2.13)		(0.14)
Water is high quality (*E. coli* MPN ≤ 1)	0.15	733	0.12	712	0.04
	(0.36)		(0.32)		(0.02)**
Water is high or moderate quality (*E. coli* MPN <126)	0.76	733	0.76	712	0.00
	(0.43)		(0.43)		(0.03)
Water is poor quality (*E. coli* MPN 126-1000)	0.17	733	0.16	712	0.01
	(0.37)		(0.37)		(0.02)

	Treatment (protected)		Comparison		Treatment – Comparison
	Mean (s.d.)	Obs.	Mean (s.d)	Obs.	(s.e)
Water is very poor quality (*E. coli* ≥ 1000)	0.07	733	0.08	712	-0.01
	(0.25)		(0.26)		(0.01)
Respondent years of education	5.71	731	5.66	717	-0.05
	(3.61)		(3.60)		(0.23)
Children under age 12 in the compound	4.04	736	3.93	719	0.11
	(2.48)		(2.46)		(0.14)
Iron roof indicator	0.70	735	0.68	717	0.03
	(0.46)		(0.47)		(0.03)
Walking distance to closest water source (minutes)	8.74	725	8.03	714	0.71
	(8.40)		(6.82)		(0.49)
Water collection trips per week by household	48.03	733	47.99	716	0.04
	(36.51)		(38.48)		(2.51)
Ever collects drinking water at “assigned” spring indicator	0.82	661	0.80	668	0.02
	(0.38)		(0.40)		(0.03)
Multi source user (uses sources other than assigned spring)	0.45	732	0.44	715	0.00
	(0.50)		(0.50)		(0.04)
Fraction of respondent water trips to “assigned” spring	0.72	655	0.71	663	0.01
	(0.41)		(0.42)		(0.04)
Rates water at the spring “very clean” – rainy season	0.33	736	0.33	719	0.00
	(0.47)		(0.47)		(0.04)
Rates water at the spring “very clean” – dry season	0.74	736	0.74	719	-0.01
	(0.44)		(0.44)		(0.03)
Fraction of water trips by those under age 12	0.10	727	0.10	711	-0.00
	(0.20)		(0.20)		(0.01)
Water storage container in home was covered	0.90	673	0.93	656	-0.03
	(0.30)		(0.26)		(0.02)**
Yesterday's drinking water was boiled indicator	0.25	731	0.29	711	-0.03
	(0.43)		(0.45)		(0.02)
Respondent diarrhea prevention knowledge score	3.06	736	3.19	719	-0.13
	(2.14)		(2.26)		(0.15)
Respondent said “dirty water” causes diarrhea	0.68	736	0.67	719	0.01
	(0.47)		(0.47)		(0.03)
Household has soap in the home	0.91	733	0.91	717	0.00
	(0.28)		(0.29)		(0.02)
<u>Panel C:</u> Child demographics and health					
Child age (years)	1.70	1047	1.72	995	-0.02
	(0.95)		(0.97)		(0.04)
Child male (=1)	0.52	1047	0.53	995	-0.01
	(0.50)		(0.50)		(0.02)
Child had diarrhea in past week indicator	0.23	996	0.20	961	0.03
	(0.42)		(0.40)		(0.02)
Child height (cm)	76.10	870	76.13	835	-0.03
	(11.67)		(12.16)		(0.57)
Child weight (kg)	9.98	864	10.02	810	-0.05
	(3.04)		(3.09)		(0.16)

Notes: The treatment springs were later protected (in 2005). In the final column, Huber-White robust standard errors are presented (clustered at the spring level when using household level data), significantly different than zero at * 90% ** 95% *** 99% confidence.
Diarrhea is defined as three or more “looser than normal” stools per day.
Assigned spring is the project sample spring that we believed households used at baseline, based on spring user lists. Household survey respondent is the mother of the youngest child in the compound (or the youngest adult woman available).
All children in Panel C were reported to be under age 3 at baseline or have been born since then.

Table 2: Spring protection source water quality impacts (2004-2007)

	Dependent variable: ln(Spring water *E. coli* MPN)			
	(1)	(2)	(3)	(4)
Treatment (protected) indicator	-1.07	-1.08	-1.04	-1.10
	(0.27)***	(0.27)***	(0.23)***	(0.24)***
Baseline ln(Spring water *E. coli* MPN)		0.47	0.99	1.01
		(0.06)***	(0.07)***	(0.08)***
Baseline ln(Spring water *E. coli* MPN)				
* Treatment indicator			-0.17	-0.16
Baseline latrine density			(0.12)	(0.13)
				-0.07
Baseline latrine density * Treatment indicator				(0.58)
				0.90
Baseline diarrhea prevention score				(1.76)
Baseline diarrhea prevention score				-0.04
*Treatment indicator				(0.07)
Baseline boiled water yesterday density				-0.29
				(0.25)
Baseline boiled water yesterday density				
*Treatment indicator				0.59
Baseline mother's years of education density				(0.68)
Baseline mother's years of education density				0.92
*Treatment indicator				(1.52)
Treatment group 1 (phased in early 2005)				-0.06
				(0.05)
Treatment group 2 (phased in late 2005)				0.06
				(0.14)
R^2	0.30	0.34	0.43	0.45
Observations	726	726	726	726
Mean (s.d.) of dependent variable	3.63 (1.95)	3.63 (1.95)	3.63 (1.95)	3.63 (1.95)

Notes: Estimated using OLS. Huber-White robust standard errors are presented (clustered at the spring level), significantly different than zero at * 90% ** 95% *** 99% confidence.
There are 184 spring clusters with data for some of the four survey rounds (2004, 2005, 2006, 2007). MPN stands for "most probable number" coliform forming units (CFU) per 100ml.
Average diarrhea prevention knowledge calculated as average of demeaned sum of number of correct responses given to the open ended question "to your knowledge, what can be done to prevent diarrhea?"
All variables that are interacted with the treatment indicator are de-meaned.
Time (survey round and wave) fixed effects are included in all regressions but not reported, as are all variables used to balance the initial randomization into treatment and comparison groups. When interactions included, baseline variables are interacted with time indicators and treatment group indicators in addition to the treatment indicator. These coefficients not reported.
Baseline iron roof density and its interaction with the treatment indicator are included as additional control variables (not shown in the table).
The -107 log point effect in column 1 is equivalent to a 66% reduction in *E. Coli* fecal coliform units per 100ml.

Table 3: Spring protection household water quality impacts (2004-2007)

	Dependent variable: ln(Home water *E. coli* MPN)		
	(1)	(2)	(3)
Treatment (protected) indicator	-0.27	-0.29	-0.67
	(0.15)*	(0.19)	(0.27)**
Baseline ln(Spring water *E. coli* MPN)	0.01	0.03	0.035
	(0.05)	(0.05)	(0.05)
Baseline multi-source user		-0.29	-0.274
		(0.16)*	(0.17)
Baseline multi-source user * Treatment indicator		0.04	0.061
		(0.25)	(0.26)
Baseline latrine density	-0.73	-0.73	-0.023
	(0.32)**	(0.31)**	(0.60)
Baseline latrine density * Treatment indicator			1.423
			(1.01)
Baseline diarrhea prevention score	-0.02	-0.03	-0.054
	(0.02)	(0.02)	(0.04)
Baseline diarrhea prevention score * Treatment indicator			-0.053
			(0.06)
Baseline boiled water yesterday indicator	0.17	0.16	0.29
	(0.08)**	(0.08)**	(0.15)*
Baseline boiled water yesterday indicator * Treatment indicator			0.52
			(0.28)*
Baseline mother's years of education	0.00	0.00	0.017
	(0.01)	(0.01)	(0.02)
Baseline mother's years of education * Treatment indicator			0.017
			(0.04)
Treatment group 1 (phased in early 2005)	0.00	-0.14	-0.011
	(0.14)	(0.18)	(0.27)
Treatment group 2 (phased in late 2005)	-0.10	-0.12	-0.162
	(0.12)	(0.15)	(0.27)
R^2	0.04	0.04	0.05
Observations (spring clusters)	4343 (184)	4343 (184)	4343 (184)
Mean (s.d.) of dependent variable in comparison group	3.00 (2.27)	3.00 (2.27)	3.00 (2.27)

Notes: Estimated using OLS. Huber-White robust standard errors (clustered at the spring level) are presented, significantly different than zero at * 90% ** 95% *** 99% confidence. MPN stands for "most probable number" coliform forming units (CFU) per 100ml.

Additional control variables included are: season fixed effects, number of children under 12 living in the home, home has iron roof indicator, iron roof density within spring community. When differential treatment effects are reported in column 3, we also include interactions with all of these control variables and the treatment indicator (not shown in the table). Baseline spring water quality, latrine density, and diarrhea prevention score are de-meaned.

Time (survey round and wave) fixed effects included in all regressions but not reported, as are all variables used to balance the initial randomization into treatment and comparison groups. When interactions are included, baseline variables are interacted with time effects and treatment group indicators, in addition to interactions with treatment (protected) indicator. These coefficients not reported in the table.

The -27 log point effect in column 1 is equivalent to a 24% reduction in *E. Coli* fecal coliform units per 100ml.

Table 4: Health outcomes for children under age three at baseline or born since 2004 (2004-2007 data)

	------------Dependent variable: Diarrhea in past week ------------						Dependent variable Weight (kg)		Dependent variable Body mass index, BMI (kg/m^2)	
	(1)	(2) Probit	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment (protected) indicator	-0.045***	-0.044**	-0.045*	-0.047**	-0.090***	-0.032	0.065	0.093	0.21	0.27
	(0.012)	(0.020)	(0.023)	(0.023)	(0.029)	(0.039)	(0.076)	(0.100)	(0.13)*	(0.17)
Treatment (protected) indicator * Male					0.083**			-0.054		-0.12
					(0.040)			(0.120)		(0.19)
Treatment (protected) indicator * Baseline latrine density						0.105 (0.119)				
Treatment (protected) indicator * Baseline diarrhea prevention score						-0.0084 (0.0073)				
Treatment (protected) indicator * Baseline mother's years of education						0.0023 (0.0044)				
Child fixed effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treatment group fixed effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month of year controls	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Gender-age controls	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.00	-	0.53	0.53	0.53	0.53	0.96	0.96	0.69	0.69
Child-year observations	6750	6749	6749	6660	6660	6601	5736	5736	5646	5646
Mean (s.d.) of the dependent variable in the comparison group	0.19 (0.39)	0.19 (0. 39)	0.19 (0. 39)	0.19 (0. 39)	0.19 (0. 39)	0.19 (0. 39)	11.36 (3.50)	11.36 (3.50)	17.0 (2.2)	17.0 (2.2)

Notes: Column 2 estimated using probit (marginal effects presented), columns 1 and 3-10 estimated using OLS. Huber-White robust standard errors (clustered at the spring level) are presented, significantly different than zero at * 90% ** 95% *** 99% confidence. Data from all four survey rounds (2004, 2005, 2006, 2007), sample restricted to children under age three at baseline (in 2004) and children born since 2004. Diarrhea defined as three or more "looser than normal" stools within 24 hours at any time in the past week. The gender-age controls include linear and quadratic current age (by month), and these terms interacted with a gender indicator. Columns 2-10 also contain survey round controls. In column 6, additional control variables are number of children under 12 living in the home, home has iron roof indicator, iron roof density within spring community, and the boiled water yesterday indicator (all measured at baseline), all interacted with the treatment indicator.

Table 5: Treatment effects on household water source choice and health behaviors (2004-2007)

Dependent variable	Coefficient (s.e.) on treatment indicator Full sample		Coefficient (s.e.) on treatment indicator Sole-source users		Coefficient (s.e.) on treatment indicator Multi-source users		Mean (s.d.) comparison group in 2006, 2007 surveys	
Panel A: Water transportation and storage	(1)		(2)		(3)		(4)	
Fraction of water trips by those under age 12(a)	0.00	(0.01)	0.00	(0.02)	0.00	(0.02)	0.09	(0.19)
Water storage container in home covered indicator	0.00	(0.01)	-0.01	(0.02)	0.01	(0.02)	0.98	(0.15)
Ever treated water with chlorine indicator(b)	0.02	(0.03)	0.03	(0.05)	0.01	(0.04)	0.45	(0.50)
Yesterday's drinking water boiled indicator(c)	0.03	(0.02)	0.05	(0.03)*	0.01	(0.03)	0.25	(0.44)
Panel B: Sanitation and hygiene behaviors								
Diarrhea prevention knowledge score	0.14	(0.14)	0.21	(0.18)	0.04	(0.19)	3.92	(2.07)
Respondent says drinking clean water is a way to prevent diarrhea	-0.03	(0.03)	-0.03	(0.04)	-0.04	(0.04)	0.73	(0.44)
Household has soap in the home indicator	-0.01	(0.02)	-0.02	(0.02)	0.01	(0.03)	0.89	(0.31)
Fingers with bacterial contamination (fecal *Streptococci* colonies) (d)	0.10	(0.12)	0.41	(0.23)*	0.11	(0.21)	0.71	(1.26)
Panel C: Spring amenities (recorded by enumerators) (e)								
Reported quality (1=very poor, 5=very good)	0.52	(0.14)***	-		-		3.22	(0.92)
Spring has "clear" water	0.26	(0.07)***	-		-		0.71	(0.45)
Fence around spring	0.94	(0.03)***	-		-		0.00	0.00
Spring has "high" water yield	-0.06	(0.06)	-		-		0.73	(0.45)
Fecal matter around spring	-0.15	(0.06)**	-		-		0.27	(0.44)
Trench for spring water cleared in last month	0.29	(0.11)***	-		-		0.59	(0.49)
Vegetation near spring cleared in last month	0.17	(0.10)*	-		-		0.36	(0.48)
Panel D: Water collection and source choice								
Fraction of trips to assigned spring	0.09	(0.03)***	0.03	(0.02)*	0.21	(0.05)***	0.76	(0.40)
Perceive water at assigned spring to be very clean – rainy season	0.22	(0.04)***	0.22	(0.05)***	0.22	(0.04)***	0.18	(0.38)
Perceive water at assigned spring to be very clean – dry season	0.11	(0.04)***	0.07	(0.03)**	0.15	(0.06)***	0.76	(0.43)
Trips made to get water (all uses, members, sources) past week	-2.38	(2.15)	-0.71	(2.41)	-4.41	(3.51)	31.77	(24.42)
Self-reported distance to nearest water source (min.)	-0.95	(0.34)***	-1.22	(0.47)**	-0.6	(0.50)	7.29	(6.68)
Self-reported distance to assigned spring (min.)	-0.07	(0.44)	-0.96	(0.51)*	1.10	(0.73)	8.39	(7.00)
Calculated distance (GPS) to assigned spring (km)	0.03	(0.03)	0.05	(0.04)	0.01	(0.01)	0.36	(2.52)

Notes: N=1354 households at 184 springs (full sample), 755 of whom are baseline sole source users. Each cell reports the differences-in-differences treatment effect estimate from a separate regression, where the dependent variable is reported in the first column. Huber-White robust standard errors (clustered at the spring level) are presented, significantly different than zero at * 90% ** 95% *** 99% confidence. Reported means of the dependent variables are in the comparison group 2006 and 2007 (rounds 2 & 3 post-treatment) surveys. Assigned spring is the sample spring that we believed households used at baseline based on spring user lists. The fingertip contamination results are for the respondent's main hand (so values range from 0-5).
(a): Because of changes in survey design, responses to this question are not available for the third (2006) round of data collection.
(b): Because of changes in survey design, responses to this question are not available for the first (2004) round of data collection.
(c): Because of changes in survey design, responses to this question are not available for the fourth (2007) round of data collection.
(d): Because information on fingertip contamination was collected only in the third (2006) round of data collection, this cell reports the difference between the treatment and comparison groups rather than the differences-in-differences treatment effect.
(e): Panel C contains spring level information, so the breakdown into sole source and multi-source households is not possible.

Table 6: Discrete choice models (conditional and mixed logit) of water source choice (2007 surveys)

	---------------- Revealed Preference ----------------					--- Stated Ranking ---	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment (protected) indicator	0.51***	-0.02	0.34***	0.68***		0.96***	
	(0.04)	(0.08)	(0.08)	(0.09)		(0.24)	
Mixed logit – Mean (normal):					2.95***		1.46**
					(0.25)		(0.60)
Mixed logit – Std. dev. (normal):					5.73***		1.22
					(0.33)		(0.75)
ln (source water E. coli MPN)		-0.14***					
		(0.01)					
Water quality at source perceived to be above average		1.14***					
		(0.07)					
Distance to water source (minutes walking)	-0.055***	-0.059***	-0.031***	-0.053***		-0.033***	
	(0.001)	(0.002)	(0.003)	(0.002)		(0.010)	
Mixed logit – Mean (restricted triangular):					-0.21***		-0.03***
					(0.01)		(0.01)
Mixed logit – Std. dev (restricted triangular)					0.09		0.001
Distance * Children aged 0-5 with diarrhea last week			-0.006***				
			(0.001)				
Treatment indicator * Children aged 0-5 with diarrhea last week			0.09***				
			(0.03)				
Treatment indicator * Baseline latrine ownership				1.80***			
				(0.25)			
Treatment indicator * Baseline diarrhea prevention score				0.023			
				(0.020)			
Treatment indicator * Baseline mother's years of education				0.057***			
				(0.011)			
Source type: Borehole/piped	-0.08		-0.05	-0.13*	-1.02***	0.07	0.04
	(0.05)		(0.07)	(0.08)	(0.14)	(0.25)	(0.27)
Source type: Well	-0.28***		-0.35***	-0.31***	-1.87***	-0.43*	-0.47*
	(0.05)		(0.07)	(0.07)	(0.13)	(0.24)	(0.25)
Source type: Stream/river	-0.77***		-0.71***	-0.63***	-1.46***	-2.19***	-2.25***
	(0.06)		(0.09)	(0.09)	(0.15)	(0.52)	(0.53)
Source type: Lake/pond	-0.20		-0.20	-0.18	-0.32	-2.82	-2.85
	(0.14)		(0.20)	(0.19)	(0.35)	(1.86)	(1.87)
Log likelihood at convergence	-11743	-2626	-5416	-5392	-3980	-363	-363
Number of observations	53427	29068	50988	50024	53427	2114	2114

Number of households	452	329	428	422	452	483	483

Notes: Conditional logit model in columns 1-4 and 6 and mixed logit model in columns 5 and 7 (grouped by trip or choice situation, and weighting each household equally). Significantly different than zero at * 90 ** 95 *** 99% confidence. In columns 1-5 each observation represents a unique household-water source pair in a given water collection trip. In columns 6-7, each observation represents a household-water source pair from a series of questions in which the respondent is asked to choose their favorite source from among alternatives. The data are from the final round of household surveys (2007). The dependent variable is an indicator equaling 1 if the household chose the source represented in that household-water source pair in that collection trip. The omitted water source category is “non-program spring”. The coefficient estimate on the indicator for being the assigned program sample spring, the spring that we believe households used at baseline based on spring user lists, is not shown. In column 3, additional controls are included for children aged 0-5 and 5-12 at baseline, and the distance to water source term, directly and interacted with the treatment indicator (not shown). In column 4, additional control variables are number of children under 12 living in the home, home has iron roof indicator, iron roof density within spring community, and the boiled water yesterday indicator (all measured at baseline), directly and interacted with the treatment indicator.

Table 7: Valuation of one year of spring protection (2007 survey)

Panel A: Revealed preference valuation				
(from mixed logit – Table 6, column 5)	One year of spring protection			
	Median	Std. dev.		
Work days (8 hour days)	18.5 days	102.8 days		
Assume value of time is 25% Kenyan worker average wage	$6.61	$36.69		
Time value from survey questions (time and monetary value)	$1.76	$11.13		
Panel B: Stated preference ranking valuation				
(from mixed logit – Table 6, column 7)				
Work days (8 hour days)	57.9 days	12.3 days		
Assume value of time is 25% Kenyan worker average wage	$20.64	$4.38		
Time value from survey questions (time and monetary value)	$5.00	$1.97		
Panel C: Contingent Valuation			Full Round	Final Wave, emphasizing trade-offs
Proportion willing to pay this for spring protection:				
US$3.57 (250 Kenya Shillings)			0.94 [308]	0.80 [98]
US$7.14 (500 Kenya Shillings)			0.90 [316]	0.79 [204]
US$14.29 (1000 Kenya Shillings)			-	0.60 [204]
	One year of spring protection			
	Median	Std. dev.		
Sample: Final Wave, emphasizing trade-offs	$17.64	$13.09		
Subsample with 250 KSH starting value	$12.62	$11.06		
Subsample with 500 KSH starting value	$23.91	$14.28		

Notes: The results in Panels A and B all correct for attenuation bias in the coefficient estimate on distance walking to water source, assuming a correction for classical measurement error (the correlation between reported distance walking to the sample spring across survey rounds is 0.38.)

Number of observations in brackets in Panel C. The contingent valuation questions were only asked of households in the treatment group, since they have a first-hand sense of what spring protection is worth. In the final wave of the survey, respondents were first asked if they would be willing to pay either 250 or 500 Kenya Shillings, followed by the question that emphasized the expenditure trade-off for their assigned amount, and then were asked if they would be willing to pay the next higher amounts also with emphasis on the expenditure trade-off.

Table 8: Property Rights Institutions: Counterfactual Policy Simulations

	Proportion of springs protected	Average price per water trip, conditional on price>0 (USD)	NPV profits, per land owner (USD)	NPV household welfare, per spring (USD)	Proportion households worse off than status quo	Social welfare, per spring (USD)
(1) Status quo	0	0	0	0	0	0
(2) Social planner	0.275	0.0	0.0	700.0	0.198	340.8
(3) “Full” private property rights	0.054	0.0028	441	-518	0.972	-77.5
Springs social planner does not protect	0.008	0.0026	306	-411	0.978	-105.0
Springs social planner protects	0.174	0.0033	796	-802	0.960	-5.2
(4) “Conditional” private property rights	0.108	0.0006	90	-135	0.187	-44.9
Springs social planner does not protect	0.041	0.0002	24	-103	0.134	-78.9
Springs social planner protects	0.283	0.0017	262	-217	0.287	45.0
(5) “Open access” private property rights	0.024	0.0085	16	28	0.000	43.4
Springs social planner does not protect	0.000	0.0000	0	18	0.000	18.2
Springs social planner protects	0.087	0.0085	57	52	0.000	109.8
(6) Public investment (with 30% tax deadweight loss)	0.243	0.0000	0	528	0.196	115.9
Springs social planner does not protect	0.122	0.0000	0	366	0.136	158.9
Springs social planner protects	0.560	0.0000	0	953	0.329	2.6
(7) Voucher (with 30% tax deadweight loss)	0.115	0.0	38	319	0.101	112.9
Springs social planner does not protect	0.051	0.0	19	233	0.082	140.6
Springs social planner protects	0.283	0.0	90	546	0.138	40.1

Notes: In the status quo, all water prices are zero and all springs are unprotected, and household utility is normalized to zero. The “full” private property rights case places no constraints on landowners’ decision to charge water prices or protect their spring. The “conditional” private property rights case allows landowners to charge positive prices only at protected springs. The “open access” private property rights case prohibits land owners from charging for the unprotected water at their spring. Net present values are calculated using a 5% annual discount rate over 15 years. Household utility values are converted into USD using each household’s predicted value of time (derived from the contingent valuation survey exercise discussed in the text.).

Figure 1: Rural Water Project (RWP) Timeline 2004-2007

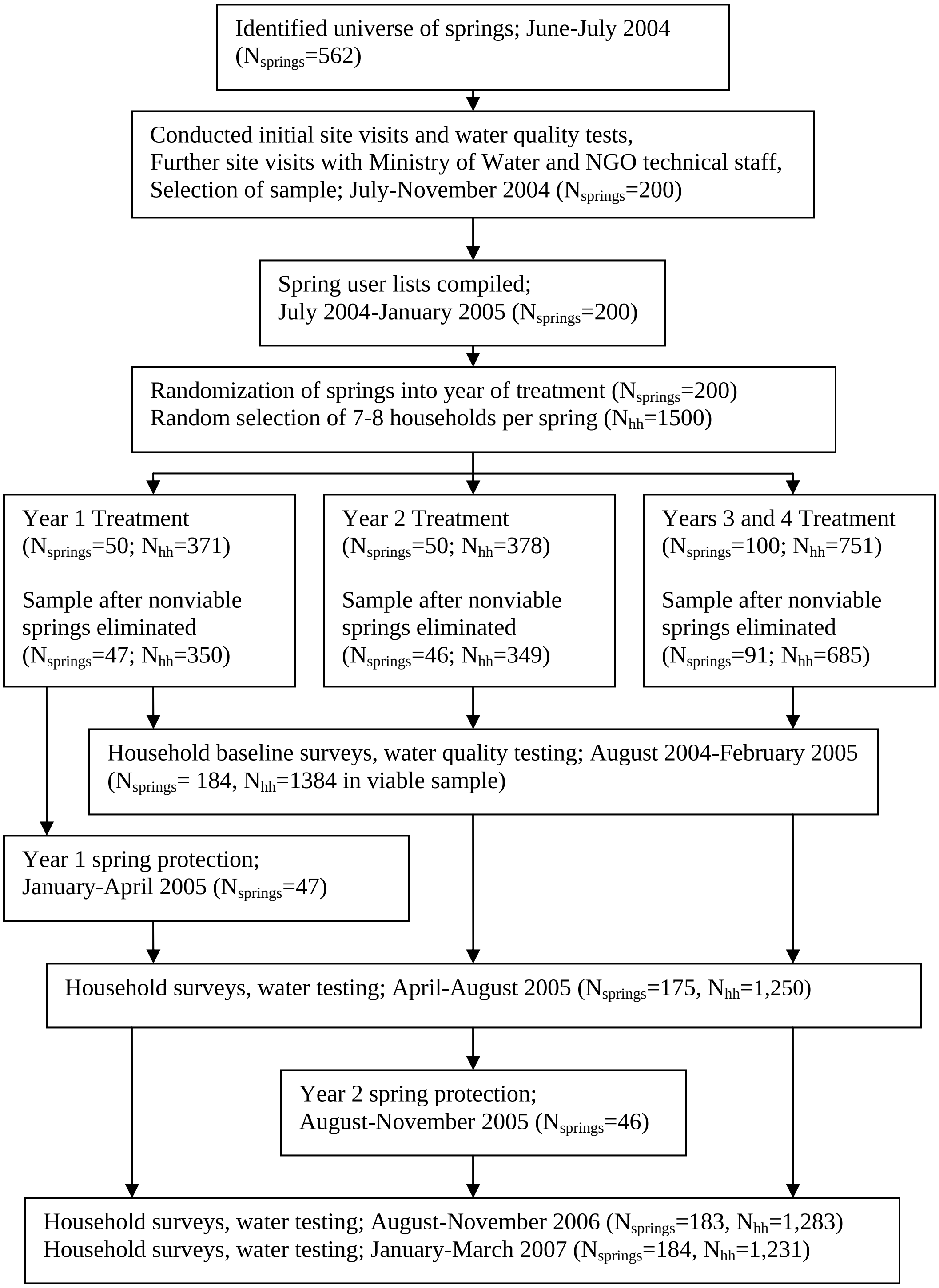
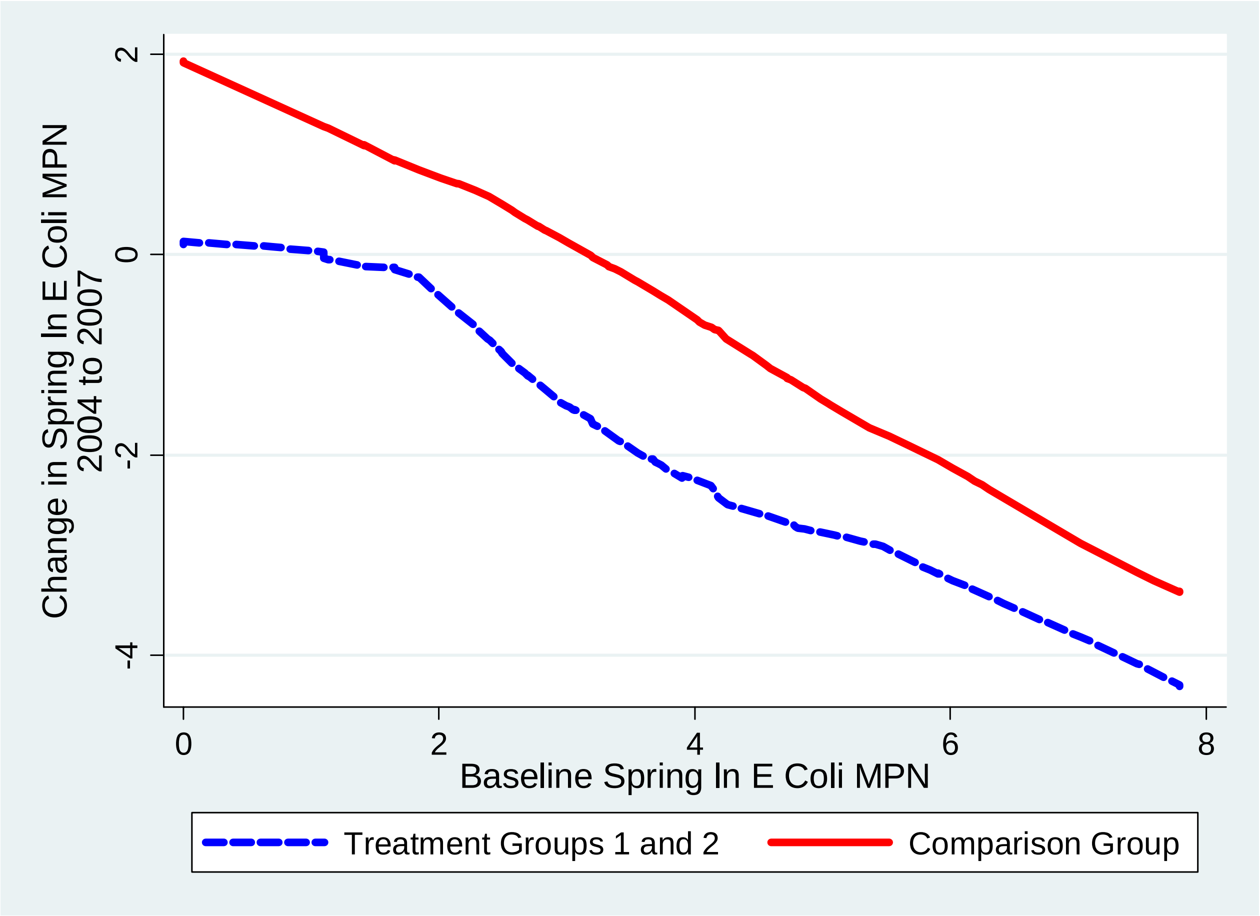
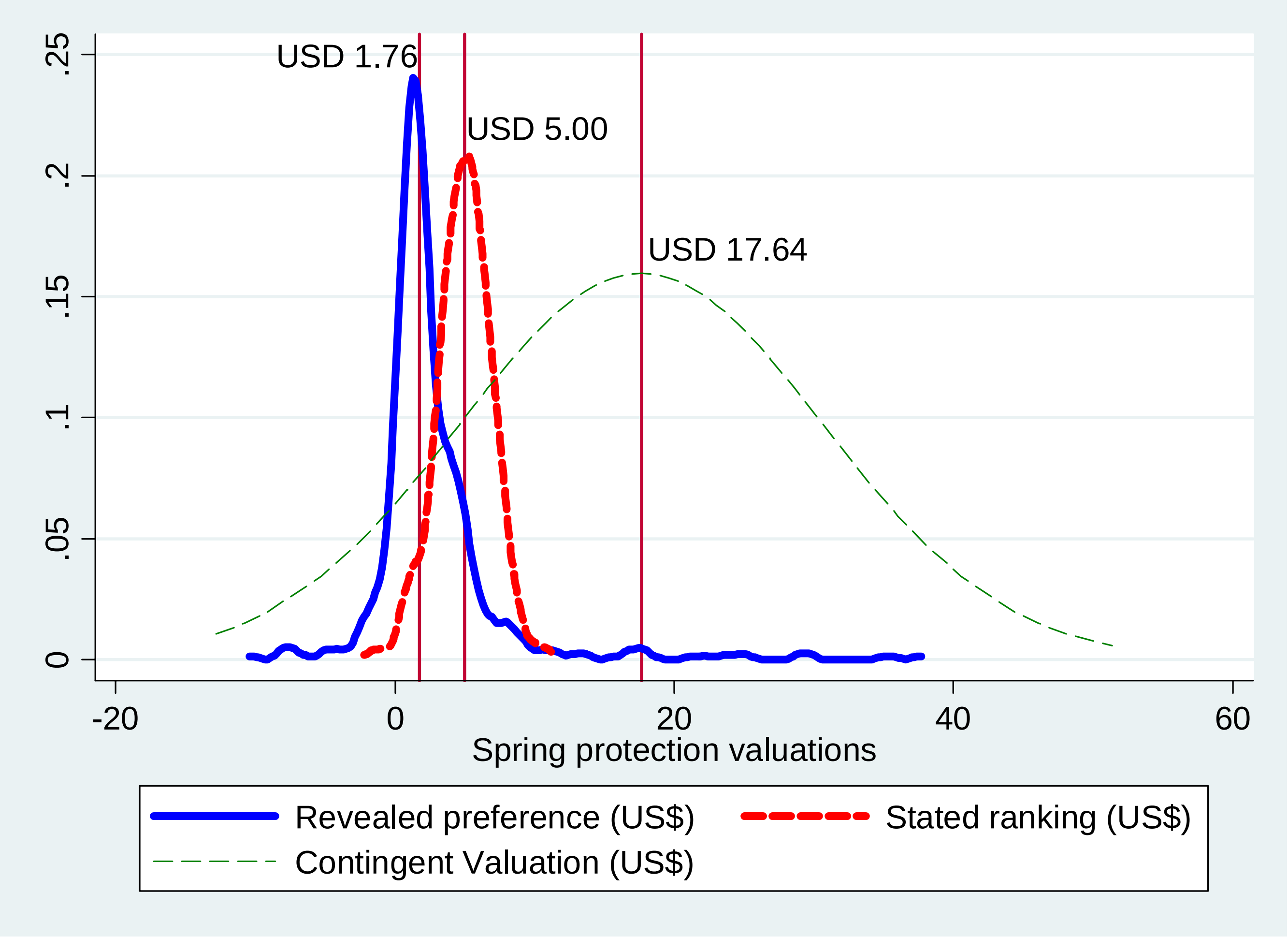


Figure 2: Change in spring water contamination from 2004 to 2007
versus baseline (2004) water contamination



Notes: To 10-90 range in Baseline ln (*E Coli* MPN) is [1.1, 6.3]. MPN stands for "most probable number" coliform forming units (CFU) per 100ml.

Figure 3: Household revealed preference and stated preference valuations
of one year of spring protection (2007)



Notes: The revealed preference estimates are from the mixed logit results in Table 6, regression 5, and the stated preference ranking results are from the mixed logit results in Table 6, regression 7. The contingent valuation data are presented in Table 7, Panel C.