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

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First draft: July 2006
This draft: July 2009

Abstract: Social norms and formal laws often create common property rights in natural resources, limiting private investment. This paper uses a randomized evaluation in Kenya to measure the health impacts of source water quality improvements achieved via spring protection, 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. We find 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-half stated preference valuations and are much smaller than levels implied by health planners' typical valuations of child mortality. Simulations indicate that private property norms would generate little additional investment, while imposing large static costs due to spring owners' local market power. 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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[*] This research is supported by the Hewlett Foundation, USDA/Foreign Agricultural Service, International Child Support (ICS), Swedish International Development Agency, Finnish Fund for Local Cooperation in Kenya, google.org, the Bill and Melinda Gates Foundation, and the Sustainability Science Initiative at the Harvard Center for International Development. We thank Alicia Bannon, Jeff Berens, Lorenzo Casaburi, Carmem Domingues, Willa Friedman, Francois Gerard, Anne Healy, Jonas Hjort, Jie Ma, Owen Ozier, Camille Pannu, Eric Van Dusen, Melanie Wasserman, Heidi Williams and especially Clair Null and Changcheng Song for excellent research assistance, and thank the field staff, especially Polycarp Waswa and Leonard Bukeke. Jack Colford, Alain de Janvry, Giacomo DiGiorgi, Esther Duflo, Liran Einav, Andrew Foster, Michael Greenstone, Avner Greif, Michael Hanemann, Danson Irungu, Ethan Ligon, Steve Luby, Enrico Moretti, Kara Nelson, Aviv Nevo, Sheila Olmstead, Ariel Pakes, Judy Peterson, Rob Quick, Mark Rosenzweig, Elisabeth Sadoulet, Sandra Spence, Duncan Thomas, Ken Train, Chris Udry, and numerous seminar participants have provided helpful comments. Sandra Spence gave key guidance on water quality testing. Opinions presented here are those of the authors not those of the Bill and Melinda Gates Foundation or its leadership. All errors are our own.

1. Introduction

Social norms and formal laws 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 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 often develop effective institutions for addressing collective action problems around common property resource use. 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 functioned effectively historically, changes in population or 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 potentially discourages private investment in water infrastructure, such as the spring protection technology we examine, which seals off the source of a spring and thus reduces water contamination. On the other hand, communal property rights in water also avoid static inefficiencies due to exploitation of local market power.

This paper makes four major contributions to the literature on environment and development. First, we provide the first randomized impact evaluation evidence on the health benefits of source water quality intervention, a significant area of government and donor investment in less developed countries. Second, we provide among the first revealed preference estimates of the value of a statistical life in a poor country. Our estimates are far below those typically used by health economists in assessing cost effectiveness of health investments and suggest that the demand for health is highly income elastic, as argued by Hall and Jones (2007). We contribute to the literature on the valuation of environmental amenities, providing evidence on the divergence between stated and revealed preference valuation for water-related interventions. Finally, we combine data from a randomized experiment with structural econometric methods used by Berry, Levinsohn, and Pakes (1995) and others to explore the implications of alternative property rights regimes in natural resources, contributing to our understanding of the role of institutions in economic development.

Policymakers have called for more investment in water infrastructure to provide cleaner water and reduce water-borne diseases including diarrhea, which accounts for 20% of deaths of children under five each year (Bryce *et al.* 2005). 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 there is controversy 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 short of piped 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 impact on child health.[1] We find that spring protection greatly improves water quality at the source, reducing fecal contamination by 66%, but is only moderately effective at improving household water quality, reducing contamination by 24%. 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 incomplete pass through of 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. There is no evidence that spring protection crowds out household water treatment measures such as boiling water or chlorination. There is also no evidence improved sanitation coverage or hygiene knowledge allows households to better translate source water quality gains into better household water.

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

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[1] Two prospective studies of source water quality interventions that find positive impacts on child health (Aziz et al. 1990 and Huttly et al. 1987), but the published articles do not mention if the treatment villages were randomly selected, generalizing to other settings is hampered by their small sample sizes (each includes only five villages), and the fact that they evaluate improved water quality and quantity simultaneously Galiani et al (2005) find that water infrastructure privatization in Argentina led to large child health gains, although a comparison with our case is complicated by their urban middle-income setting with its high degree of government regulation over water utilities.

The estimated mean annual valuation for spring protection is equivalent to 32.4 workdays. Based on households' stated preferences regarding tradeoffs between money and walking time, this corresponds to approximately US$2.96 per household per year. Under some additional assumptions this translates to an upper bound of US$0.89 on households' mean willingness to pay to avert one child diarrhea episode, and of US$769 on the mean value of averting one statistical child death, much less than the values typically used in health cost-effectiveness analyses in low-income countries. Our results are consistent with an income elasticity of demand for health greater than one .

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.[2] We find that the stated preference approaches generate much higher valuation estimates than our revealed preference approach, by a factor of two, with the contingent valuation 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. We first show that a social planner using our revealed preference valuation estimates, would only protect springs with a relatively large number of household users. Using the household water demand system derived from the revealed preference valuations, we then conduct counterfactual policy simulations and find that alternative property rights institutions have important social welfare consequences. We first find that a norm of private property rights would yield lower social welfare than existing communal rights because the static losses from spring owners pricing above marginal cost outweigh the dynamic benefits of stronger water infrastructure investment incentives, providing a rationale for why communal water norms in rural Africa have not generally been replaced with private property rights. However, an alternative norm under which

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[2] Whittington, Mu, and Roche (1990) and Mu, Whittington, and Briscoe (1990) each study water source choice in rural Africa using a contingent valuation (CV) approach. However, neither considers water quality in the source choice decision and they explicitly rule out multiple drinking water sources, which we find to be empirically important. Choe (1996) compares willingness to pay for reduced river and lake pollution in an urban Philippines setting with piped water, using both travel cost and CV methods, and finds that both are similarly low but this may not generalize to rural areas. Two other papers have compared averting 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. The shortcomings of stated preference approaches to measuring the value of non-market goods are well-known (Diamond and Hausman 1994).

spring owners can charge for protected spring water only if they also allow continued free access to unprotected water generates a Pareto improvement relative to existing communal property norms. Finally, a government-financed voucher system for spring users could approximate the solution for either a social planner who respected households' spring protection valuations or one who placed extra value on child health.

The paper is organized as follows. Section 2 describes the intervention and data. Section 3 discusses spring protection impacts on water quality and child health. Section 4 discusses the impact of protection on water source choice and estimates the willingness to pay for clean water. Section 5 discusses social welfare 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%), and surface water sources such as rivers, lakes and ponds (5%). Over 81% of all water collection trips are to sources the respondents used for drinking water in the last week.

Spring protection is widely used in non-arid regions of Africa to improve water quality at existing spring sources (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.[3]

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 spring owners to make water available "to any applicant so long as the water use

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[3] Protected springs are considered an "improved" water source by the World Health Organization and thus protecting springs counts as progress towards Millennium Development Goal targets.

of the owner of the works is not adversely affected." Later, we assess whether such orders are in fact in the public interest. In practice landowners are expected to make spring water available to neighbors for free. Spring owners thus have weak incentives to improve a water source, as they are unable to recoup the costs of any investment via the collection of user fees. In the absence of a local government with taxing authority, collective action problems mean that investments in water infrastructure (and other local public goods) often fail to occur. Historically, when it occurred, spring protection has been undertaken either by outside donors or using central government funds.

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). As implemented by ICS, spring protection included installing fencing and drainage, and organizing a user maintenance committee, in addition to the actual construction. Spring protection cost an average of US\$1024 (s.d. US\$85), with some variation depending on spring characteristics. All communities contributed 10% of project costs, mainly in the form of manual labor. The NGO also conducted community meetings at which user committees were selected. Typical protected spring 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 typically expected to be covered by local 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 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 into treatment groups are in Supplementary 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 we use. In subsequent surveys, over 98% of this sample was found to actually use the spring at least sometimes, but the few non-users were nonetheless retained in the analysis.

The spring user list is reasonably representative of all households living near sample springs. In a census of all households living near nine sample springs, 71% of households living less than a 20 minute walk from the source were included in the original spring users lists, with even higher rates of inclusion (77%) for those households less than a 10 minute walk from the sample spring.

2.3 Data collection

Water quality was measured at all sample springs and 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 Supplementary 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 to 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). Using reasonable designations of water quality drawn from U.S. EPA standards, 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 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 versus 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 households get 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, at baseline 25% of households report that they boiled their drinking water yesterday. However, even in those households both adults and children often drink some unboiled water. For instance, young children are commonly given water to drink directly from the household storage container. Moreover, the correlation between household water contamination and self-reported water boiling is low, raising the possibility of social desirability reporting bias. Finally, some households may chlorinate their water. Following a 2005 cholera outbreak the government distributed free chlorine and in the first follow-up (2005) survey, 29% of households reported chlorinating their water at least once in the last six months, though by the second follow-up survey (when more time had passed since the outbreak) fewer than 8% of households reported chlorinating their water in the last week. 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 of all source types with average ln *E. Coli* MPN/100 ml = 2.3, followed by unprotected springs, boreholes, shallow wells, lakes/ponds, and rivers/streams with 3.6, 4.1, 5.2, 6.0, and 7.0 respectively.

Respondents are well-informed about the relative desirability of different types of water infrastructure but only imperfectly about the cleanliness of individual sources. The proportion of respondents stating that a source is "very clean" or "somewhat clean" is highest for protected springs, the objectively cleanest source, at 92%, followed by boreholes (87%) and unprotected springs (75%), shallow wells (73%), lakes/ponds (31%) and streams/rivers (14%). The correlation between ln *E. coli* MPN/100ml levels at water sources and household perceptions of source water quality (on a 1 to 5 scale with 1=very clean and 5=very unclean) is just 0.12 (s.e. 0.02), though after conditioning on household fixed effects, this rises to 0.19 (s.e. 0.02). This is just under half the correlation of actual

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

E.Coli counts across successive survey rounds (0.46). The moderate correlation of *E.Coli* counts over time is presumably due both to measurement error and fluctuation in spring contamination.

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 estimation and spring protection impacts on water quality and child health.

3.1 Estimation strategy

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

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

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 a 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) (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 springs

Spring protection dramatically reduces fecal contamination of source water. 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, and protection does not lead to a significantly larger proportional reduction in water contamination where initial contamination was highest (regression 2). The downward slope of the non-parametric representation of the data in Figure 2 is consistent with mean reversion, likely reflecting measurement error and transitory water quality variation. 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 3). Protected springs are rated by enumerators as having significantly clearer water (regression 4) but no higher water yields (regression 5), consistent with spring protection improving water quality but not quantity. Protected springs also have better fencing and drainage, and less fecal matter and brush in the vicinity, as communities maintain protected springs better than unprotected springs (results not shown).

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, who used only 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 endogenous source choice could thus cause home drinking water quality to increase or decrease after protection depending on whether the alternative source is cleaner or dirtier than the protected spring. The point estimates of contamination reductions are slightly larger for sole-source households (regression 2) but 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, often by season), 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 and household water quality. Conditional upon collecting spring water[5], comparison 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 more reliable experimental estimates in Table 3. Other non-experimental approaches – including detailed controls for respondent education, boiling and at-home chlorination (as well as interaction terms), as well as employing distance to the protected source as an IV for increased use (point estimate 0.46, results not shown) – also differ substantially from the experimental estimate.

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 gains are smaller for households that report boiling their water, as expected if boiling and spring protection are substitutes.

Spring protection could potentially generate spillover benefits for other communities due to hydrological interconnections, the infectious nature of diarrheal diseases, and reductions in the number of people using alternative sources. To test for this, we consider the effect of the number of nearby treated springs (located within 1, 3, or 6 kilometers), on both spring and household water

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[5] At baseline, 15.4% of comparison households get at least some of their drinking water from protected springs. In follow-up rounds, this percentage increases to 24.5%, but most of this increase is due to the secular increase over time in spring protection funded by donors or government.

quality controlling for the total of local springs (protected or not). The coefficient estimate on the number of treated springs within 3 kilometers is small at -0.004 (s.e. 0.086), and similar results hold for springs at greater distances (not shown). There is some evidence for positive household water quality spillovers: the coefficient on treatment springs located within 3 kilometers is -0.090 (s.e. 0.050, regression not shown). Although this effect is also consistent with some households switching to use nearby protected sources, we cannot rule out that the possibility of moderate positive spillover benefits to spring protection within the local area.

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 and the associated attenuation bias 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

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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 once again find that non-experimental estimates differ sharply from experimental estimates. 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 partially complicating comparisons with our experimental impact estimates.

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, an effect 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 further investigation is warranted.

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).

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 (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 (estimated reduction -6.7 percent, s.e. 24.9 percent, not shown).

3.5 Estimating behavioral changes

Theoretically the estimated effects of spring protection on household water quality and diarrhea could reflect not only the direct impact of improved source water, but also indirect effects of spring protection on water transport, storage, or home treatment behaviors. Empirically, however, there were no significant changes in water handling or treatment behaviors aside from the increased use of protected springs for drinking water discussed below and in section four (Table 5, Panel A). There is also no evidence of changes in diarrhea knowledge or in a direct hygiene measure, fecal contamination on respondents' hands (Panel B).

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. Recall that each household in our dataset is linked to a particular spring (their "reference" spring) based on the baseline user list. The potential for differential impacts among sole-source users of this reference spring 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 reference spring for those households not previously using it exclusively: treated households increase the fraction of water collection trips to their reference spring by 21 percentage points if they used other sources at baseline (Table 5, Panel C, 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 (Panel C). There were small but statistically significant effect of spring protection on the reported average time it took to walk to their main drinking water source, with an effect of roughly one minute (average length 7.3 minutes one-way so 15 minutes round-trip). However, there was no significant 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 reference spring. Another possibility is that water collection is slightly faster at protected springs and that some respondents mistakenly assigned these time savings to reported walking times.

4. Valuing clean water

This section uses a travel cost model of water source choice to develop a revealed preference estimate of household valuation of spring protection. We then argue that the more common stated preference approaches substantially overstate households' valuation of spring protection. Finally, we argue that households' valuation of a statistical life is smaller than typically assumed in public health expenditure cost-effectiveness analysis, but consistent with models such as Hall and Jones (2007) in which income elasticity of health is greater than one.

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

Denote household *i*'s cost of time per minute as, $C_i > 0$. Thus the cost household *i* bears to make an additional water trip to source *j* is C_iD_{ij}, where D_{ij} is the household's round trip distance to the source. Let the valuation of water from source *j be* Z_j, which could reflect both health and non-health attributes, such as the ease of water collection at a source. Households make multiple water collection trips and each trip is affected by unobserved factors, including the weather, which household member is collecting water, the expected queue, other errands the water collector may need to undertake, or their mood that day. Household *i*'s indirect utility from a single water collection trip to source *j* at time *t* can be represented as:

$$(3) \qquad U_{ijt} = Z_j - C_iD_{ij} + e_{ijt,} ,$$

where e_{ijt} is an i.i.d. type I extreme value error term. Household *i* chooses source *j* over an alternative source *k* if its benefits outweigh any additional travel costs, namely when $(Z_j - Z_k) - C_i(D_{ij} - D_{ik}) + (e_{ijt} - e_{ikt}) \geq 0$. Focusing on those households on the margin between using two sources conceptually allows one to estimate households' valuation for spring protection. Letting "T" denote treatment, spring water quality improvements yield utility benefits of $Z_j^T - Z_j$, and travel costs would have to increase by the same amount to restore indifference. The additional travel cost households are choose to incur is a revealed preference measure of their willingness to pay for spring protection.

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 alternatives $h \in H$ at time *t* ($y_{ijt} = 1$) can be represented in the conditional logit formulation (McFadden 1974):

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

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. We allow the households' time costs and valuation of spring protection 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).

After estimating the conditional logit, we follow Berry, Levinsohn and Pakes (1995), Train 2003) and others in explicitly estimating heterogeneity using a mixed logit model with random coefficients on water source characteristics (e.g., spring protection, walking distance) in the indirect utility function. We estimate choice probabilities as:

$$P(y_{ijt} \mid X) = \int_{\beta} \rho_{ijt} f(\beta) d\beta \qquad (5)$$

where y, X, β and ρ 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 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 that they did not use.

4.2 Estimating willingness to pay for spring protection

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 than non-program springs (the omitted category of sources), 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 bias 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 below, although the attenuation bias correction estimated in a Monte Carlo simulation is similar, at roughly 0.3 (not shown).

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.

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 indicator to near zero (Table 6, regression 2), consistent with the possibility that households greater valuation of protected springs is entirely due to the impact of protection on water quality, rather than also being influenced by other factors, such as the reduced need to bend down to collect water or faster collection times. However, a specification that includes measured *E. Coli* contamination as an explanatory variable but excludes perceived source water quality (for which respondents might give self-justifying answers that are endogenous to their actual choices) reveals that, while the coefficient estimate on the spring protection indicator falls by half, it remains positive and statistically significant at 0.27 (s.e. 0.07, regression not shown). Taking these results together, it is difficult to definitively pin down how large the positive amenity value attached to spring protection is beyond its improved water quality.

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 (due to the phase in of spring protection treatment, 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 be more costly, and the effect is especially large for households whose children had diarrhea at baseline: the estimate is large and significant at 99% confidence (Table 6, regression 3). The coefficient on the interaction between the treatment indicator and 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 result is difficult to interpret since households with sick children may also have different underlying preferences for child health.

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 (Table 6, regression 4). However, the choice of protected springs is not significantly affected by diarrhea prevention knowledge, or by household knowledge of the link between contaminated water and diarrheal disease (not shown). Asset ownership does not affect the taste for protection (not shown), and child gender does not significantly affects the taste for protection even though health gains appear concentrated among girls, nor are breastfeeding rates different by gender (not shown). The mixed logit approach suggests considerable dispersion of spring protection valuations: the mean value of spring protection is 32.4 work days with a standard deviation of 102.8 work days (Table 6, regression 5 and Table 7, Panel A).

4.3 Comparison of Revealed and Stated Preference Water Valuations

This subsection compares our revealed preference spring protection valuations from 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 analyzed in the travel cost discrete choice framework 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 two times greater than the revealed preference value, and the willingness to pay for one year of spring protection is 56.2 work days (Table 7, Panel B). Comparing the analogous columns in Table 6 (regressions 1 and 6) suggests social desirability bias may be playing a role. The coefficient estimates on several unimproved sources many Kenyans generally think of as unclean (e.g., streams, ponds) are far more negative in the stated preference case than in revealed preference, while the spring protection estimate 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, (US$7.14 or US$14.29) followed by a question that emphasized the expenditure trade-off (in other words, the 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.[7]

Nearly all households say they are willing to pay US$7.14 for one year of spring protection, and the majority of households say they are willing to pay twice that (US$14.29) even after being walked through the expenditure trade-offs (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 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 250 Kenyan Shillings ($12.62). If we assume spring protection valuations are normally distributed, and use a maximum likelihood approach to find the normal distribution that best fits the data, the mean willingness to pay is US$17.64 (standard deviation US$13.09).

To move from walking time to monetary values for the revealed preference and stated preference ranking cases, we need to know how households value water collection time. We do this in two ways, the first based on survey evidence on the time-money trade-off, and second by making assumptions using local wages. In the first approach, we asked a subset of contingent valuation subjects (surveyed after the round 3 household survey) 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 then derive water collectors' time value by dividing their stated monetary for valuation spring protection by their walking time valuation.[8] 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 detailed set of household characteristics (e.g., education, number of children, asset ownership) in this subsample and then used these estimated coefficients to predict time values for the entire sample. The resulting mean value of time is about $ 0.088 per day, or about 10% of the wage those carrying water would have earned for local agricultural labor.

We combine this household level estimated value of time with the revealed preference mixed logit in our preferred estimates of household willingness to pay for spring protection. The mean

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[7] See Supplementary Appendix B for the exact survey question wording.

[8] In computing household 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 willing to walk 10 but not 15 additional minutes to a protected spring, the median value is 12.61 minutes. The time value is then the ratio of the monetary valuation estimate to the walking time valuation estimate.

valuation for a year of access to protected spring water is only $2.96 with a standard deviation of $11.13 (Table 7, Panel A). The analogous stated preference ranking estimate is $4.96 (Panel B). The estimated distributions for the three valuation approaches (in Figure 1) indicate not only that stated preference methods exaggerate household willingness to pay for environmental amenities in a rural Kenyan setting, but that the revealed preference approach yields less variable valuations. One plausible explanation for the dispersion in stated preference methods 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.

Because limited time-income substitution possibilities are frequently encountered empirically (McKean, Johnson, and Walsh 1995), other authors also focus on a range of time values far below the wage, often at 25 to 50% of the average wage as a starting point (Train 1999). We also present revealed preference valuations using 25% of the average Kenyan wage or US$0.35 per work day (in Table 7), but while the valuation levels shift upward in this case, they remain below the contingent valuation figures and note that – by construction – the roughly 2:1 ratio of stated preference ranking to revealed preference valuations is unchanged (since both are scaled up by the value of time).

4.4 Implications for Health Valuation

Under the assumption that households are aware of the relationship between spring protection and diarrhea, combining the results from Tables 4 and 6 yields an upper bound on the willingness to pay to avert child diarrhea. The bound will be tight to the extent that households' valuation of spring protection is entirely due to its impact on real and perceived child health, rather than also being due to other spring protection amenities (impact on water appearance, ease of water collection, or health other than child diarrhea); if these other factors are important in households' valuation of spring protection, actual willingness to pay to avert child diarrhea will be lower than our estimates.

Spring protection averts an average of (0.047 diarrhea cases / child-week) * (1.3 children age 3 and under / household) * (52 weeks / year) = 3.2 diarrhea cases per household-year. Using our mean spring protection household valuation of 32.4 work days (from the mixed logit), this corresponds to a willingness to pay of 10.1 work days per case of child diarrhea averted. Under the further assumption that spring protection reduces diarrhea mortality by the same proportion as diarrhea incidence, an upper bound on the valuation of a statistical life at 8,742 work-days or 35 work-years (at 250 work days per year). This bound will again be tight if households' valuation of diarrhea reduction is entirely due to its impact of mortality.[9]

Using the household time values derived from our surveys, the bound on the value of averting one case of child diarrhea is a mere US$0.89 (=US$0.088*10.1 working days), and on avoiding a child diarrhea death is just US$769 (=US$0.088*8,742 working days). These numbers correspond to an upper bound on the value of averting one DALY of about US$23.68. Using the higher time value (25% of the average Kenyan wage) translates into US$3,006 per averted child diarrhea death and US$92.56 per DALY. These value of life estimates 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, although Deaton *et al.* (2008) also find low values of life in African samples using a subjective life evaluation approach.

The revealed preference bound on the willingness to pay per DALY averted is consistent with models in which health has a high income elasticity and is far below the cost effectiveness cutoffs usually used in analyses of health projects in less developed countries. For example, the 1993 World Development Report termed health interventions that cost less than $100 or $150 per DALY as "extremely cost effective" (World Bank 1993). Jeff Sachs (2002) has argued for setting health cost effectiveness thresholds per DALY at levels corresponding to countries' GDP per capita, which for Kenya would be over $400, nearly twenty times higher than our preferred estimate. While an important source of uncertainty in our valuations is the conversion of the value of time to the value of money, it is worth noting that even if our preferred time values were tripled, the implied valuation of health and life would still fall far the below the values typically used by health planners.

In contrast, others argue that households' valuation of statistical lives in developing countries is very low. Hall and Jones (2007) use US$3 million to 6 million as benchmarks for the value of life in the United States. Applying their approach, in which the value of a year of life is roughly proportional to per capita anuual consumption raised to the CRRA utility function curvature

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[9] There are 5.69 deaths per 1000 children under age five each year in sub-Saharan Africa (Lopez *et al.* 2006, Table 3B.7). With roughly 4.9 annual diarrhea episodes per African child under age five, (see Kirkwood, 1999), 1.16 deaths from diarrhea would be averted for each 1,000 diarrhea cases eliminated under the assumption that mortality is proportional to morbidity.

parameter (which might take a value of 2), the value of a statistical life in Kenya ranges from US$1,425 to 4,054. If per capita consumption in our rural study site is only two thirds of the national Kenya average, a reasonable approximation, this range falls to just US$358 to 1,526, accommodating our revealed preference estimate of US$769.

Establishing the ideal way to conduct welfare analysis in this situation is important but beyond the scope of this paper, and thus we present a variety of approaches in section 5 below. We first present results following the conventional economic approach of valuing lives according to revealed preference measures. We then consider the case of a social planner with a higher valuation, which may be appropriate, for example, if the planner values averting child diarrhea deaths more than other types of household consumption, if children receive a lower weight in the household welfare function than in the planner's welfare function, if outsiders particularly value child welfare, or if households consider only private benefits and not the disease externalities of reducing diarrhea. Using a higher valuation might also be appropriate if households systematically underestimate the health benefits of spring protection, or if they are subject to time inconsistency problems.

5. Simulating alternative property rights norms and institutions

Under the Kenyan law referred to earlier, local authorities can determine whether or not land owners have to make water available to neighbors, and authorities typically follow local social norms in preventing landowners from charging other local residents for water. Perhaps partially as a result of these common private property rights, virtually no springs are privately protected in our study area. Social norms regarding water rights in the study region date to pre-colonial times, when there were no centralized kingdoms or chiefdoms in the area and the key local socio-political unit was the kinship clan (Were 1967, 1986). In the colonial and post-independence eras, administrative boundaries were typically set to at least roughly correspond to clan boundaries, with the region settled by a clan typically being a Kenyan administrative unit called a sublocation, and below (section 5.2) we thus consider springs within a sublocation as a natural unit for the analysis of property rights institutions. Since the colonial period, the number of clans has increased and most sublocations in western Kenya typically contain multiple clans, and for this reason our approach of examining subgroups of contiguous springs within sublocations is natural.

We use the spring protection valuation estimates derived above to determine the socially optimal level of spring protection in this region, and then estimate social welfare under alternative social norms and institutions regarding property rights. We abstract from the costs of enforcing property rights and consider the narrower question of what outcomes institutions would produce if they were fully enforced. This discussion should thus be taken as an analysis of the welfare impacts of alternative social institutions and not necessarily an exploration of short-run Kenyan government policy options, since there are may be significant costs in enforcing property rights not considered here as well as other costs in the transition to a new institutional regime.

In general, a social planner's decision whether to protect a spring will depend on whether other nearby springs are also protected, given households' ability to choose among sources within walking distance. To build intuition, we first consider the problem of a social planner deciding whether to protect an isolated spring in section 5.1, before moving on to the more realistic case of endogenous choice among multiple sources in 5.2. Throughout we treat the marginal cost of providing additional spring water as zero, since unused water simply flows away and user congestion is minimal.

5. 1 The social planner's problem for an isolated spring

Spring protection costs an average of US$1,024 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$1,405 (with a 5% annual discount rate).[10] The total benefit of protecting a spring is the sum of willingness to pay for spring protection among current users, where the mean household willingness to pay is US$2.96, as discussed above. Whether it is socially optimal to protect a particular spring depends critically on the number of users. The typical spring in our data is used by 31 households, so the total discounted net present valuation is US$909. The revealed preference valuation of protection is thus estimated to exceed the cost of protection only for springs with 48 or more baseline household users, suggesting that protection is optimal in densely populated rural areas, in towns and cities, or in areas with few springs (where each has many users), and will become increasingly attractive as local population grows. For instance, spring protection would become socially optimal at the typical spring if local population grows by 55%. While a social planner maximizing the revealed preference of households would protect only heavily used springs, a social planner who valued averting a DALY at US$125 would have a much lower threshold, at just 10 households. In other words, using a valuation often used by health planners, it would be socially optimal to protect 97% of our sample springs, whereas using the revealed preference valuations only the 15% of springs with the most household users are protected.

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[10] The local District Water Office finds that protected springs last between 10 and 15 years on average. We use the top end of the range as our cost figures correspond to the case of well-constructed and maintained springs.

5.2 Impacts of alternative property rights institutions with endogenous household water choice

We now consider the impact of alternative water property rights social norms and institutions within a unit of territory such as that of a historical clan or a modern administrative sublocation, when households endogenously choose their sources to trade off source quality, walking distance, and price, and when spring owners choose whether to invest in spring protection based on profitability. Private property rights allow spring owners to charge for access to spring water, providing an incentive to invest in protection, but also introduces a static distortion in choice of water source, since the marginal cost of using spring water is zero. Charging positive water prices can thus lead households to choose springs that would be less preferred based on walking time and water contamination, the factors that it is socially efficient for them to consider in choosing water sources.

Household demand parameters are derived from the revealed preference mixed logit results (in Table 6, column 5). Consider a partition of nearby springs into contiguous subgroups, such as the springs in the territory of a clan, which is typically a subset of a Kenyan administrative sublocation. We consider each subgroup separately and then aggregate across groups for overall welfare outcomes. The term $j \in \{1, ..., J\}$ denotes the springs within each subgroup; and $i \in \{1, ..., I\}$ refers to households that choose among these sources. The utility value of spring protection for household i from spring j is β_{ij}, γ_{ij} is the household's disutility from an additional minute of walking time, while δ_{ij} is the value of a minute of the water collector's time (from the survey method described above). This value of time allows us to convert utility into monetary values, which we mainly focus on below, although we also discuss robustness to other time value assumptions. Household preferences can be represented by $\theta_{ij} = \{\beta_{ij}, \gamma_{ij}, \delta_{ij}\}$, where $F(\theta_{ij})$ is the joint distribution in the population.

These parameter estimates are available for each household from the mixed logit estimation, and allow us to compute household utility per water collection trip (as in equation 3)[11] to a particular source w, denoted u_{wij}, as well as the total trips they chose to make to each of their alternative water sources in a full year, T_{wij}. Annual household expected utility (in monetary terms) for household i from water consumption is thus $V_{ij} = \frac{\sum_w (T_{wij} * u_{wij})}{\delta_{ij}}$, minus any costs incurred purchasing the water. Empirically, recall that the total quantity of household water collected varies neither with spring protection status nor with distance to the reference spring, allowing us to simplify the problem by

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[11] In terms of the notation in equation 3, the disutility of time (in monetary terms) C_i corresponds to $\gamma_{ij} * \delta_{ij}$, and the valuation of spring protection β_{ij} corresponds to $Z_j^T - Z_j$ (which is allowed to vary across households).

assuming that the total number of household water trips is fixed ($\sum_{w} T_{wij} = T^{*}_{ij}$ for household *i* regardless of their choices among sources $w \in \{1, .., W\}$). Recall also that since households can obtain water from other sources, such as wells or streams, springs (subscripted *j*) are a subset of all sources (subscripted *w*).

We simulate the following game. At t = 0, the property rights regime is chosen. At t = 1 spring owners within a subgroup simultaneously decide whether to protect their spring, where protection is denoted by the indicator variable $protect_j$, and then at t=2 simultaneously set a price p_j per unit of water. 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 vector of protection and price decisions are denoted *protect* and *p*, respectively.[12] Households then choose water sources to maximize utility given walking times, protection decisions and prices. We solve the model backwards. We do not permit collusion among spring owners.

We find optimal prices conditional on protection status 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 a supplementary simulation appendix available upon request.) 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 groups of four springs) and search for a fixed point. 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.

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 spring water is zero), over the 15 year time horizon used above. The initial spring protection investment cost is k_0, while the recurrent annual maintenance cost is k_r, so the discounted net present cost of protection is $K \equiv \left[k_0 + k_r \sum_{\tau=1}^{15} \left(\frac{1}{1+r} \right)^{\tau-1} \right]$, which we showed is US$1,405 in our setting.

We consider the impact of changing property rights norms on spring investments and pricing outcomes, holding constant policies for other water sources. This approach is realistic in the rural

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[12] We also incorporate demand from new household users post-protection using information from a household user censuses, as described in the simulation appendix. We ignore any water consumption utility gains for spring owners since they would not necessarily live locally or consume spring water.

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, while many boreholes are sunk on public property such as schools or market centers where there is no private owner, and water from these sources is also generally free.

In our simulations, sample households can choose to obtain water from their reference spring, as well as all the other sample springs within their spring subgroup, plus any other potential alternative water sources they listed in the household survey. 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 slightly greater distance from each other were grouped together. We consider groups of up to four contiguous springs within the same administrative sublocation since this roughly corresponds to the territory of a traditional clan, and also 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 faces computational limitations. In sublocations where the number of springs is not divisible by four, we created the largest possible 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 treated as isolated. 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 and two springs, in addition to the four spring case we focus on here, and, while prices charged by private spring owners do fall as the degree of competition increases, the findings on the welfare rankings of different property rights institutions are unchanged (not shown).

The simulations were run ten times, each with independent draws of household preference parameters (from mixed logit), and the results presented here are the average of the ten runs.[13] We normalize social welfare to zero in the benchmark "status quo" case with common property rights to water and thus no spring protection (Table 8, panel A, 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 users can change with protection status, but for a rough sense of per capita gains recall that the baseline number of household spring users is 31 and households contain an average of 6.6 members, for roughly 200 persons per spring community.[14]

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[13] One full simulation run takes two days, so for logistical reasons we focus on just 10 runs (available upon request).

[14] A household census conducted at a subset of nine 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 baseline users. For a useful

We first consider the social planner's problem and then the solutions under various property rights institutions. The decision problem of a social planner maximizing revealed preference can be represented as follows, where W^S denotes social welfare:

$$(6) \qquad \underset{\{protect_g\}_g}{Max}\; W^S = \sum_{j=1}^{J}\sum_{i-1}^{I} V_{ij}(protect \mid \theta_{ij}) - K * \sum_{j=1}^{J} protect_j$$

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

A social planner maximizing household's revealed preferences protects 29% of sample springs (48 springs in all), typically these with many baseline household users (Table 8, panel A, row 2). The net social gain across all springs (protected and unprotected) is US\$351 per spring, or roughly US\$1.75 per baseline user. When the social planner exercise is carried out using the stated preference ranking mixed logit estimates of household preferences, and the value of time is set at 25% of the local wage (as in many environmental economics analyses) 97% of sample springs would be optimally protected, far above the 29% using revealed preference valuations. Similarly, using an average valuation of US\$125 per DALY averted, the social planner would protect 63% of springs.

We next consider four forms of property rights – common property (the status quo described above), full private property, Lockean private property (defined below), and modified Lockean – and then consider two non-budget balancing mechanisms, public provision and vouchers.

In the case of full private property rights, 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. As discussed above, 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 solve for the Nash equilibrium.[15] The optimization problem for the owner of spring j, where π_j denotes profits and the owner takes others' prices p_{-j}^* and protection decisions $protect_{-j}^*$ as given, is presented in equation 7:

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approximation of the welfare gains for this group, we assume in the simulation that their consumer surplus is uniformly distributed between zero and the valuation of the baseline user household that lives farthest from each spring (in our data), 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 the same.

[15] 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 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.

$$(7) \qquad \underset{protect_j}{Max}\ \pi_j = p_j^* \sum_{j'=1}^{J'} \sum_{i=1}^{I} \left\{ T_{jij'}(protect_j, protect_{-j}^*, p^*) \mid \theta_{ij} \right\} - K * protect_j$$

The double summation refers to all households at each spring j' in the spring subgroup that j belongs to, and $T_{jij'}$ is the number of trips the household makes to spring j given the equilibrium protection status and price decisions of other springs. This is solved subject to optimal non-cooperative price setting: $p_j^* = \arg\max_{p_j} \pi_j(protect_j, protect_{-j}^*, p_{-j}^*)$. The Nash equilibrium solution is a vector of protection and price decisions for each spring, and consumption decisions for all households, such that household consumption decisions are optimal given protection and prices, and protection and prices are optimal for each spring owner given other springs' prices and protection decisions.

Under "full" private property, only 5% of spring owners find it profit maximizing to protect their spring (Table 8, panel A, row 3). The net present value of profits per spring owner is US$420, and the average price charged per water collection trip in these springs is US$0.0027. While competition from local springs does dampen potential price increases – the equilibrium price spring owners charge is monotonically decreasing in the number of local competitor springs (if we compare the solution with maximum group size of one, two, and four – not shown) – this average price remains quite expensive for Kenyan households. For the typical household making 32 trips per week to such a spring, in a year this is equivalent to US$4.49, several days' wages.

Full private property rights substantially reduce social welfare relative to the status quo of communal property rights, with a social welfare loss of US$91 per spring, and note that nearly all households (97%) are worse off under full private property rights. This finding may be a partial explanation why communal water rights have been so durable in African settings like ours: they may simply yield higher social welfare outcomes than private property rights. The explanation is straightforward: all households pay for access to spring water in the private property rights case but few realize health benefits since nearly all springs remain unprotected. The proportion of household trips to the dirtiest local sources, rivers and streams, increases by 45% relative to the status quo case, and as a result average fecal contamination as measured by *E. Coli* contamination of collected water increases by 26 log points (from 4.66 to 4.92). Average walking time per collection trip also rises by over a minute, from 11.6 to 12.9 minutes due to the price distortion, an important change (totaling over 100 hours per year) for households making roughly 50 weekly water collection trips.

Note that many springs the social planner would protect are not protected under private property, whereas some springs the social planner would not protect are protected under private property. Among the springs that the social planner would protect, only 13% of spring owners choose protection (not shown). One reason that many fewer springs are protected under full private property than under the social planner is that spring owners are unable to capture the full consumer surplus of potential users due to heterogeneity in valuations, while some springs that the social planner would not protect will be protected by private landowners due to a rent-stealing effect, since land owners do not consider the negative impact of spring protection on owners of competing nearby springs. Another key difference is that the planner values all households equally in its decision-making while private owners with partial monopoly power focus on the marginal consumer when setting prices, and this could lead them to under- or over-protect springs depending on local demand patterns.

It is also worth considering other private property rights social norms and institutions. 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. It is also related to some historical examples from the Islamic world, in which charging for use of infrastructure that improves water quality and delivery is acceptable, while charging for water *per se* is not (Caponera 2006). A "Lockean" 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 $p_j=0$ if $protect_j=0$, increasing private investment incentives to capture rents.

There are forces for both under- and over- protection with Lockean property rights. 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 on spring protection but also part of the surplus from consuming unprotected water could lead to over-protection.

In practice, there is substantial under-protection (Table 8, panel A, row 4). Simulation suggests Lockean private property rights yield somewhat higher rates of investment in water infrastructure than full private property rights: 12% of owners find it profit maximizing to invest in spring protection under Lockean property rights. Although social welfare remains lower than the status quo, Lockean rights are marginally better for household welfare than full private property rights: the average social welfare loss per community is only US$43, average fecal contamination increases by 4 log points and average walking time rises only slightly, from 11.6 to 12.3 minutes.

A "modified" Lockean private property rights regime would permit spring owners to charge for water from spring protection infrastructure as long as they also allowed free unimpeded access to unprotected water from the spring. In our setting a system of modified Lockean property rights could be achieved simply by requiring owners who protect springs to allow some water to flow out of the pipe and away from the protected spring, where it becomes a pool of unprotected spring water exposed to the environment. In this case, the spring owner's profit maximizing condition remains unchanged from above Lockean case, although the spring owner must now take into account that households now have the choice of continuing to consume unprotected spring water at price $p_j=0$ at the source or paying p_j^* for protected spring water. This regime has several desirable properties within the set of set of budget-neutral social norms. It constitutes a Pareto improvement over the common property status quo, since the availability of free unprotected spring water shields households from the utility losses experienced under the other private property cases. Moreover, if consumers were homogenous, spring owners could exactly capture the surplus from protection and would thus have optimal spring protection investment incentives.

The analysis indicates that 2% of springs are protected under modified Lockean property rights (Table 8, panel A, row 5). At the springs the planner would protect, 6% of spring owners choose to protect their spring, while only 1% of spring owners protect springs the planner would not protect (not shown). While this form of property rights is far from the socially optimal level of protection, it does incentivize spring owners to perform some socially beneficial spring protection Some spring owners still earn moderate positive profits and the water contamination level is no worse than the status quo. Yet the social welfare gains from modified Lockean property rights are sufficiently small that it is perhaps unsurprising that there has been limited institutional innovation along these lines, given the transition costs of changing property rights norms.

There are also multiple forms that public spring protection investment could take. One could consider the impact of public funding 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 population, $F(\theta_{ij})$ not individual household preferences. This policymaker's optimization problem is the following, where DW denotes the deadweight loss of taxation per dollar of revenue raised:

$$(8) \qquad \underset{\{protect\}}{Max}\, W^P = \sum_{j=1}^{J} \sum_{i-1}^{I} \widetilde{V}_{ij}(protect \mid F(\theta_{ij})) - (1+DW)*K*\sum_{j=1}^{J} protect_j$$

Under the assumption that DW=0.3 (Ballard *et al.* 1985)[16], 22% of springs are protected (Table 8, panel A, row 6) but there is misallocation of protection: 49% of those the social planner would optimally protect get protected and 12% of those the planner would not protect (not shown).

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[16] Results are largely unchanged with the assumption that DW=0.5 (results not shown).

The welfare gain per spring is US$131, much less than the US$351 gain attained by a social planner. In addition to the tax distortion, the misallocation of protection across springs due to policymakers' limited information on households' preferences contributes to lower social welfare.

Finally, consider a regime where households are provided vouchers for access to protected spring water which they give to spring owners, and which spring owners then exchange for a fixed payment from benevolent local government authorities. The government optimally sets the voucher payment taking into account the later non-cooperative protection decisions of private 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 preferences. Spring owners are assumed to be restricted from charging top-up fees to water users. The policymaker maximizes social welfare W^V by setting the uniform voucher price p_v:

$$(9) \qquad \underset{p_v}{Max} W^V = \sum_{j=1}^{J}\sum_{i=1}^{I}\left\{\widetilde{V}_{ij}(protect^*(p_v) \mid F(\theta_{ij}))\right\} + \sum_{j=1}^{J}\left\{\pi_j^*(p_v)\right\} - DW * p_v \sum_{j=1}^{J}\sum_{i=1}^{I} T_{ij}(protect^*(p_v) \mid F(\theta_{ij}))$$

subject to spring owner protection decisions being profit maximizing:

$$(10) \qquad protect_j^* = \arg\max \pi_j^*(p_v)\,,$$

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

Again considering a 30% deadweight loss, the optimal voucher price is US$0.0010 per trip to a protected spring (much less than the price charged in the full private property rights case), social welfare gains under water vouchers are US$138 (or roughly US$0.69 per capita). The proportion of spring owners who choose to protect their springs is 12% (Table 8, panel A, row 7), which is still short of the social optimum although there is less misallocation of protection in this case: only 3% of protected springs the social planner would not protect get protected (not shown). This policy improves social welfare substantially relative to the status quo and all the private property rights cases, and is comparable to government investment. In contrast to full and Lockean private property rights, the voucher regime leaves few households worse off relative to the status quo since the vouchers are distributed for free; the only losers are the relatively small number of households estimated (in the mixed logit) to have negative valuations of spring protection, though note that the median welfare loss for these households is estimated to be quite small. Average fecal contamination in the voucher case is 10 log points lower than the status quo, and walking times only slightly longer.

5.3 Optimal institutions, valuations and income

The social welfare ordering of the public cases (water vouchers and public investment) versus private property rights cases we consider is remarkably robust to alternative assumptions about households' monetary valuation of spring protection (and equivalently, the value of water collectors' time). In results not presented in Table 8, water vouchers and public investment both yield far higher social welfare than any of the private property rights cases even when household valuation for spring protection is 2, 3, 4, or 5 times larger than the revealed preference estimates.

However, the attractiveness of various cases relative to the status quo does vary: increasing households' spring protection valuation sufficiently implies that full private property rights and Lockean private property rights norms become preferable to the status quo. The level at which these private property rights become preferable to the status quo is an increase of approximately 20% in the valuation placed on spring protection. If valuations increase with income, and the income elasticity of demand for health is greater than one (as in Hall and Jones 2007), this implies that local per capita income growth of less than 20% would make private property rights more attractive than the current norm of communal access. If this finding that private property rights are optimal at higher income levels holds more broadly, it could help explain the strong cross-sectional relationship documented between institutions and income levels, and more broadly would imply that it would be risky to assume that causality always runs from institutions to income levels.

The voucher regime has several advantages over the 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 modified Lockean 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 five times as much as households, which is roughly equivalent to US$125 per DALY (Table 8, panel B), due (as discussed above) to households' failure to consider the disease externalities from reducing diarrhea, agency problems within the household, or to taking into account policymakers' or foreign aid donors' preferences. In this case, the policymaker sets a uniform voucher price of US$0.0041, four times higher than the previous price level, and as a result 65% of spring owners choose protection (row 7). More generally the voucher price can be varied to attain nearly any desired degree of water infrastructure investment, reflecting different preferences for child diarrhea reduction. In the case where the policymaker values child health more highly than most households, the voucher case yields increasingly large social welfare gains relative to the private property cases, as can be seen by comparing row 7 (the voucher case) to rows 3-5 in panel B. Government investment also yields large social welfare gains in this case.

The analysis completely abstracts from transaction costs in collecting fees, but these are likely to be large under private property norms. In particular, it may be expensive for spring owners to monitor the amount of water that households collect, and these transaction costs would reduce social welfare benefits. Water privatization in rural Kenya that runs against traditional social norms regarding communal water access is also likely to run into local resistance in practice, and more generally there are likely to be non-trivial transition costs in moving from any one system to another.

Although the voucher and government investment cases still deliver higher welfare, combining common property rights in unprotected water and private property in treated water in the modified Lockean case always delivers higher social welfare than the status quo, and the private provision cases may have some real-world 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 or investment plans may collapse after a short time, in which case the modified Lockean case could be preferable to those norms.

Ultimately, our simulations of the welfare impacts of alternative property rights norms are only suggestive of optimal real-world rural water policy, due to context-specific transactions costs and political acceptability of different property rights regimes.

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, and child diarrhea fell by one quarter. While 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, some households' water source choices changed sharply. 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 valuation estimates. We find moderate mean household valuation for spring protection, on the order of 32.4 work days, or US$2.96, per household annually. This translates into an upper bound on household willingness to pay approximately 10.1 work days, or US$0.89, per averted child diarrhea case, or equivalently US$23.68 per DALY. These are at most one-half the level of stated preference valuations and only one-fifth of the valuations typically used by public 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 29% of springs in the study area, typically those which are used by many users.

Using structural econometric methods, we carry out counterfactual simulations based on estimated household revealed preference valuations for cleaner water, and show that different property rights institutions regarding water resources have substantial welfare consequences. Existing social norms allowing communal access to naturally occurring springs yield higher social welfare than full private property norms in this setting, providing a rationale for why communal water rights have persisted in this rural African region. However, supplementing these norms either by allowing spring owners to charge for improved water while maintaining access to unimproved sites (the modified Lockean case), or through public water infrastructure investment, or by instituting vouchers through which the government pays spring owners based on the number of water users, could all lead to better outcomes than either the status quo or full private property rights. The water voucher approach appears particularly appealing in settings where a policymaker places higher valuation on child health gains than the typical household.

Some legal systems seem to have evolved in one of the directions we found promising here, towards the modified Lockean norm 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 norms to another is beyond the scope of this paper, but remains a matter of major importance in the study of natural resources and economic development.

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Table 1: Baseline descriptive statistics (2004 survey)

	Treatment (protected)		Comparison		Treatment – Comparison
	Mean (sd)	Obs	Mean (sd)	Obs	(se)
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)
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		Comparison		Treatment – Comparison
	Mean (sd)	Obs	Mean (sd)	Obs	(se)
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)
Panel C: 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). Huber-White robust standard errors are clustered at spring level when using household level data, significant at * 90% ** 95% *** 99% confidence. Assigned spring is based on spring user lists. Children in Panel C were under age 3 at baseline or were born since then.

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

	Dependent variable: ln(Spring water *E. coli* MPN)			Water clarity (observed)	Water yield (observed)
	(1)	(2)	(3)	(4)	(5)
Treatment (protected) indicator	-1.07	-1.04	-1.10	0.26	-0.06
	(0.27)***	(0.23)***	(0.24)***	(0.07)***	(0.06)
Baseline ln(Spring water *E. coli* MPN)		0.99	1.01		
		(0.07)***	(0.08)***		
Baseline ln(Spring water *E. coli* MPN)		-0.17	-0.16		
* Treatment indicator		(0.12)	(0.13)		
Baseline latrine density			-0.23		
			(0.59)		
Baseline latrine density			0.89		
* Treatment indicator			(1.75)		
Baseline diarrhea prevention score			-0.03		
			(0.07)		
Baseline diarrhea prevention score			-0.29		
*Treatment indicator			(0.24)		
Baseline boiled water yesterday density			0.48		
			(0.65)		
Baseline boiled water yesterday density			0.94		
*Treatment indicator			(1.51)		
Baseline mother's years of education density			-0.04		
			(0.05)		
Baseline mother's years of education density			0.06		
*Treatment indicator			(0.14)		
Treatment group 1 (phased in early 2005)			-0.29		
			(0.20)		
Treatment group 2 (phased in late 2005)			-0.21		
			(0.17)		
R^2	0.30	0.43	0.45	0.13	0.13
Observations	726	726	726	478	478
Mean (s.d.) of dependent variable	3.63	3.63	3.63	0.71	0.73
	(1.95)	(1.95)	(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 -1.07 effect in column 1 is equivalent to a 66% reduction in *E. Coli* fecal coliform units per 100ml.

Outcomes in columns 4 and 5 are enumerator assessments of spring water clarity and the spring's water yield.

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 -0.27 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	0.19	0.19	0.19	0.19	0.19	11.36	11.36	17.0	17.0
	(0.39)	(0. 39)	(0. 39)	(0. 39)	(0. 39)	(0. 39)	(3.50)	(3.50)	(2.2)	(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: 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.

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)
Number of observations	53427	29068	50988	50024	53427	2114	2114
Number of households	452	329	428	422	452	483	483

Notes: The data are from the final round of household surveys (2007). Conditional logit in columns 1-4 and 6, and mixed logit in columns 5 and 7 (grouped by choice and weighing households equally). Significant at * 90 ** 95 *** 99% confidence. In columns 1-5 each observation is a unique household-water source pair in one water collection trip. In columns 6-7, each observation is a household-water source pair from questions where the respondent chooses their preferred source. The dependent variable is an indicator equaling 1 if the household chose the water source represented in the household-source pair. The omitted water source category is "non-program spring". The coefficient estimate on the indicator for the household's reference sample spring is included in the analysis but not shown in the table. In column 3, additional controls are children aged 0-5 and 5-12 at baseline, and the distance to the water source, directly and interacted with the treatment indicator (not shown). In column 4, additional controls are the number of children under 12, home has iron roof indicator, iron roof density in the 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	
	Mean	Std. dev.
Work days (8 hour days)	32.4 days	102.8 days
Assume value of time is 25% Kenyan worker average wage	$11.57	$36.69
Time value from survey questions (time and monetary value)	$2.96	$11.13
Panel B: Stated preference ranking valuation (from mixed logit – Table 6, column 7)		
Work days (8 hour days)	56.2 days	12.3 days
Assume value of time is 25% Kenyan worker average wage	$20.06	$4.38
Time value from survey questions (time and monetary value)	$4.96	$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	
	Mean	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

	% springs protected	Average price per water trip (USD) | price>0	Spring owner profits (USD), per spring	Household utility (USD) per spring	Average walking time (min)	Average fecal contamination, ln(avg *E coli*)	Social welfare (USD) per spring
Panel A: Revealed preference valuation							
(1) Communal property rights (status quo)	0	0	0	0	11.6	4.66	0
(2) Social planner	29	0	0	726	12.5	4.45	351
(3) Full private property rights	5	0.0027	420	-511	12.9	4.92	-91
(4) Lockean private property rights	12	0.0069	84	-127	12.3	4.70	-43
(5) Modified Lockean property rights	2	0.0058	8	26	11.7	4.66	35
(6) Public investment	22	0	0	510	12.4	4.48	131
(7) Vouchers	12	0.0010	39	348	12.1	4.56	138
Panel B: Revealed preference valuation for households, $125/DALY for planner							
(1) Communal property rights (status quo)	0	0	0	0	11.6	4.66	0
(2) Social planner	67	0	0	3,586	12.9	4.32	5,138
(3) Full private property rights	5	0.0027	420	-508	12.9	4.92	644
(4) Lockean private property rights	12	0.0069	84	-139	12.3	4.70	775
(5) Modified Lockean property rights	2	0.0058	8	28	11.7	4.66	366
(6) Public investment	67	0	0	944	12.8	4.33	4,724
(7) Vouchers	65	0.0041	1,160	891	12.4	4.40	3,915

Notes: In the communal property rights (status quo) case, social welfare is normalized to zero. Profits, utility and welfare are net present values calculated using a 5% annual discount rate over 15 years. Household spring protection valuations are from Table 6, column 5, and utility is converted into USD using households' predicted value of time (as discussed in the text). A summary of key assumptions in each case is as follows:

(1) Communal property rights: The price of spring water is zero (by custom and/or law). Spring protection by spring owners, the government and donors is zero.

(2) Social planner: Planner maximizes social welfare. The price of spring water is zero, its marginal cost. There is no deadweight loss to raising funds for spring protection. The planner knows preferences θ_{ij} (protection valuation, disutility of walking time) for each household.

(3) Full private property rights: Spring owners make profit-maximizing protection and pricing decisions in simultaneous non-cooperative play with other local springs (in groups of up to four). Any water price that can be charged but pricing must be linear. Spring owners know preferences θ_{ij} for each household.

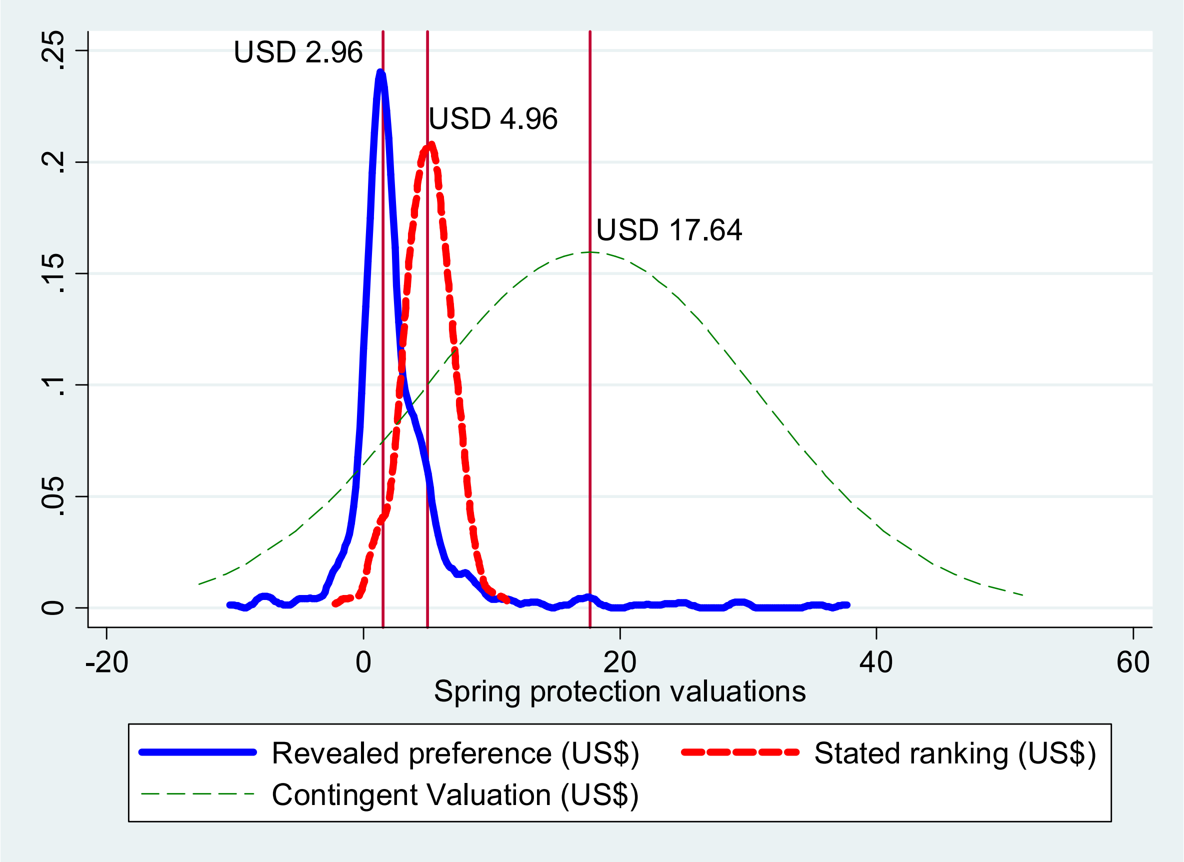
(4) Lockean private property rights: Same as the full private case except the price of unprotected spring water is constrained to be zero.

(5) Modified Lockean property rights: Same as the Lockean private case except the spring owner must always provide access to free unprotected water.

(6) Public investment: Policymaker maximizes social welfare. The price of spring water is zero. There is 30% deadweight loss to raising funds for spring protection. The policymaker knows the distribution of preferences $F(\theta_{ij})$ in the population but not for each household.

(7) Vouchers: Policymaker maximizes social welfare. The policymaker sets the voucher price (per trip for protected spring water) taking into account spring owners' subsequent investment. Spring owners then make profit-maximizing protection decisions in simultaneous non-cooperative play. There is 30% deadweight loss to funding the vouchers. The policymaker knows the distribution of preferences $F(\theta_{ij})$ in the population; spring owners know preferences θ_{ij} for each household.

Figure 1: 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.

Supplemental Online Appendix (not intended for publication)

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 as follows.

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 the treatment 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. In this case, 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 all analysis in this paper. The treatment effect estimates are thus 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 nearly unchanged from 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 exercise.

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 remain 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.

Contingent valuation surveys

The exact contingent valuation 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?*

This closed-end format, offering discrete value choices, is standard in the contingent valuation literature (Bateman and Willis 1999).

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.

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