SPRING CLEANING: RURAL WATER IMPACTS, VALUATION, AND PROPERTY RIGHTS INSTITUTIONS*

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Using a randomized evaluation in Kenya, we measure health impacts of spring protection, an investment that improves source water quality. We also estimate households' valuation of spring protection and simulate the welfare impacts of alternatives to the current system of common property rights in water, which limits incentives for private investment. Spring infrastructure investments reduce fecal contamination by 66%, but household water quality improves less, due to recontamination. Child diarrhea falls by one quarter. Travel-cost based revealed preference estimates of households' valuations are much smaller than both stated preference valuations and health planners' valuations, and are consistent with models in which the demand for health is highly income elastic. We estimate that private property norms would generate little additional investment while imposing large static costs due to above-marginal-cost pricing, private property would function better at higher income levels or under water scarcity, and alternative institutions could yield Pareto improvements. *JEL* Codes: C93, H75, O13, Q25, Q51.

I. INTRODUCTION

Movement toward private property rights institutions has been called critical to successful economic development (De Soto 1989; North 1990). Yet social norms and formal laws often create communal property rights in natural resources. In Islamic law, for

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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., Bartlett 1990; Fafchamps and Gavian 1996; Adams, Sibanda, and Turner 1999; Peters 2007). Some argue that communities can develop effective institutions for addressing collective action problems around common property resource use (Ostrom 1990).

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 in this article. Protection seals off the source of a spring and thus reduces water contamination. On the other hand, communal property rights in water also limit static inefficiencies due to exploitation of local market power.

This article makes four main contributions. First, we provide what to our knowledge is the only evidence from a randomized impact evaluation on the health benefits of a source water quality intervention, a significant area of government and donor investment in developing countries. Second, we provide among the first revealed preference estimates of the value of child health gains and a statistical life in a poor country. Our estimates fall far below those typically used by public health planners in assessing cost effectiveness and suggest that the demand for health is highly income elastic, as argued by Hall and Jones (2007). Third, we contribute to the literature on the valuation of environmental amenities, providing evidence on the relationship between revealed and stated preference valuations for water interventions. Finally, we combine data from our randomized experiment with structural econometric methods like those used by Berry, Levinsohn, and Pakes (1995) and others[1] to explore the implications of alternative property rights regimes in natural resources, shedding light on the role of social norms and institutions in economic development.

Policy makers have called for more investment in water infrastructure in less developed countries to provide cleaner water

1. Todd and Wolpin (2006) use experimental data to validate a structural model of educational investment. We do not have sufficient nonexperimental variation in property rights institutions to conduct an analogous exercise. Instead, we combine experimental parameter estimates with a structural model of water infrastructure investment to explore the welfare implications of alternative property rights institutions.

and reduce waterborne diseases such as diarrhea, which accounts for nearly 20% of deaths of children under age five each year (Wardlaw et al. 2009). Progress toward the sole quantifiable environmental Millennium Development Goal is currently measured by the percentage of population living near improved water sources, such as protected springs. Yet there is controversy about the health value of improvements that fall short of piping treated water into the home. In the absence of evidence from randomized trials, several influential reviews argue based on nonexperimental evidence that improving source water quality through infrastructure investments may have limited health impacts because diarrhea is affected more by the quantity of water available for washing than by drinking water quality (Curtis, Cairncross, and Yonli 2000); improved water supply has little impact without good sanitation and hygiene (Esrey et al. 1991; Esrey 1996); and water is recontaminated in transport and storage (Fewtrell et al. 2005).

As the first (to our knowledge) randomized evaluation of a source water quality investment, the data used in this article allow us to isolate the impact of a single intervention affecting the quality but not quantity of water and to assess child health impacts.[2] We find that spring protection greatly improves water quality at the source and is moderately effective at improving household water quality. Diarrhea among young children in treatment households falls by nearly one quarter.

The next part of this article focuses 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 respond to these water quality improvements.[3] A discrete choice model, in which households trade off water quality against

2. Two prospective studies of source water quality interventions find positive child health impacts (Huttly et al. 1987; Aziz et al. 1990) but do not mention if treatment was randomly assigned. Samples were only five villages each. Watson (2006) finds combined water and sanitation interventions reduced mortality among Native Americans. Bennett (2009) argues that municipal water investments create private disincentives to invest in sanitation.

3. Related papers include Madajewicz et al. (2007), who find considerable responsiveness to information on arsenic contamination in household water source choices in Bangladesh. Whittington, Mu, and Roche (1990) and Mu, Whittington, and Briscoe (1990) each study water source choice in rural Africa. However, neither considers water quality, and they rule out multiple drinking water sources, which we find to be empirically important.

walking distance to the source, generates revealed preference estimates of household valuations of better water quality. Based on household reports on the trade-offs they face between money and walking time to collect water, estimated mean annual valuation for spring protection is US$2.96 per household. Under some stronger assumptions this translates to an upper bound of $0.89 on households' mean willingness to pay to avert one child diarrhea episode, and $769 on the mean value of averting one statistical child death, or $23.68 to avert the loss of one disability-adjusted life year (DALY). These estimates fall far below the values typically used in health cost-effectiveness analyses in low-income countries, where investments that prevent the loss of one DALY for less than $100 or $150 are often assumed to be appropriate.

We believe that the evidence in this article can be interpreted as indicative of relatively low willingness to pay for preventive health among the poor in less developed countries, consistent with other recent work, whereas the precise valuation estimates should be viewed as somewhat more speculative. The link between spring protection and child death—a relatively rare occurrence with multiple possible causes—may be quite difficult for households to discern in practice. The valuation of child health may also differ systematically from adult health (see Davis 2004; Deaton, Fortson, and Tortora 2009).

Stated preference methodologies, such as contingent valuation, are widely used but controversial (see Diamond and Hausman 1994; Carson et al. 1996; Whitehead 2006; Whittington 2010). We find that although stated preference methods also suggest fairly low valuation, they exceed the revealed preference valuation (which exploits experimental variation in water source characteristics) by a factor of two.

Finally, we simulate the impact of alternative social norms and property rights institutions. We show that a social planner maximizing welfare as captured by our revealed preference estimates would protect far fewer springs than a social planner who valued health at the levels typically used by public health policy makers. Using the household water demand system derived from the revealed preference valuations, we find that at current rural Kenyan income levels, a freehold private property rights norm 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 greater water infrastructure investment incentives, providing a rationale for why communal water norms have historically been so durable in rural Africa. However, we estimate that as demand for clean water rises—for instance, at moderately higher income levels, or if water were scarcer—private property norms would yield higher social welfare than common property, suggesting the role that underlying economic conditions might play in the evolution of institutions. Allowing spring owners to charge for protected spring water only if they provide continued free access to unprotected water generates a Pareto improvement relative to existing communal property norms. Public investment could potentially generate substantial increases in welfare.

The article is organized as follows: Section II describes the intervention and data. Section III presents spring protection impacts on water quality and child health. Section IV discusses the effect of protection on water source choice and estimates the willingness to pay for spring protection. Section V presents social welfare under alternative institutions, and the final section concludes.[4]

II. Spring Protection Intervention and Data

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

II.A. Spring Protection in Western Kenya

Spring protection is widely used in nonarid 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 when people dip vessels to scoop out water and when runoff introduces human or animal waste into the area. Because spring protection technology has no moving parts, it requires far less maintenance than other water infrastructure, such as pumps. Naturally occurring springs are an important source of drinking water in our study area in rural Busia and Butere-Mumias districts of Kenya's Western

4. A supplementary online appendix contains details on the randomization procedure (appendix I), data and measurement issues (appendix II), and the property rights simulations (appendix III), as well as additional tables and figures.

Province. Approximately 43% of rural western Kenyan households use springs for drinking water, and over 90% have access to springs (DHS 2003). Our survey respondents report that springs are their main source of water: 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 in the previous week are to sources the respondents used for drinking water (as opposed to other household needs).

Most springs in our study area are located on private land. In Kenya, property rights to land and other natural resources are governed by a combination of traditional customary law and formal legal statutes (Mumma 2005). Not only does custom require that private landowners allow public access to water sources on their land, but under Kenyan law local authorities can "where, in the opinion of the Authority the public interest would be best served" order water source owners to make water available "to any applicant so long as the water use of the owner of the works is not adversely affected." In practice, landowners in our study area are expected to make spring water available to neighbors for free. This implies that spring owners have weak incentives to improve water sources, as they are unable to recoup the costs of any investment via the collection of user fees.

This study is based on a randomized evaluation of a spring protection project conducted by a nongovernmental organization (NGO), International Child Support (ICS). As implemented by ICS, spring protection included infrastructure construction, installing fencing and drainage, and organizing a user maintenance committee. The spring protection infrastructure cost an average of US$956 (s.d. $85), with some variation depending on land characteristics. All communities contributed 10% of project costs, mainly in the form of manual labor. After construction, the committees are expected to undertake routine maintenance, including simple patching of concrete, cleaning the catchment area, and clearing drainage ditches. These maintenance costs are roughly $32 per year, and are typically covered by local contributions, although free rider problems in collecting these funds are common.

II.B. The Study Sample and Assignment to Treatment

Springs for this study were selected from the universe of local unprotected springs. The NGO first obtained Kenya Ministry of Water and Irrigation lists of all local unprotected springs in Busia and Butere-Mumias districts. Technical staff then visited each site to determine which springs were suitable for protection. Springs known to be seasonally dry 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.

ICS planned for the water quality improvement intervention to be phased in over four years due to 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 I summarizes this timeline. To address concerns about seasonal variation in water quality and disease, all springs were stratified geographically and by treatment group and then randomly assigned to an activity "wave" grouping; all project activities and data collection were conducted by wave.[5]

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 revisited to confirm their suitability for protection. In only 10 among the final sample of 184 viable springs did treatment assignment differ from actual treatment (e.g., 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

5. One survey round took place each year from 2004 through 2007, for a total of four rounds in the panel. Within each round, the sample was randomly divided into three separate waves, yielding representative samples of communities and households surveyed during the same period of the year, to deal with any possible seasonality. Controls for survey wave and for month of year/season are thus closely related.

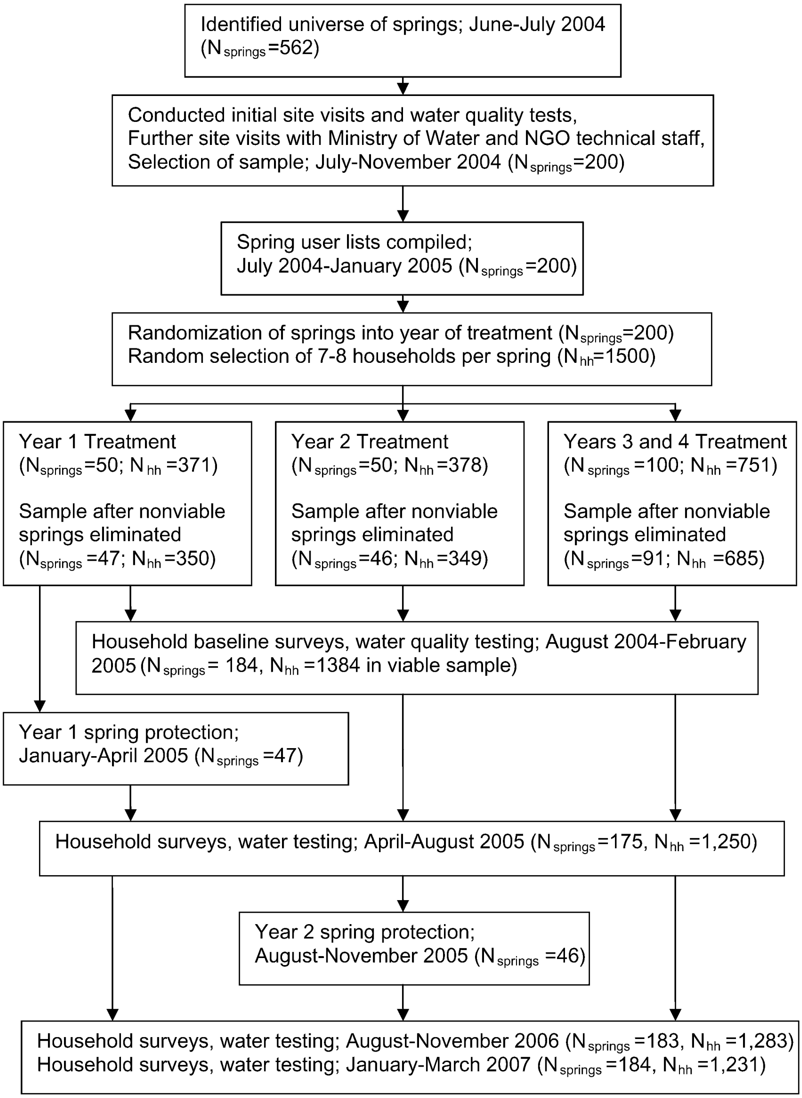

FIGURE I
Rural Water Project Timeline 2004–2007

analysis throughout. Table I presents baseline summary statistics for the treatment and comparison groups.

A representative sample of households that regularly used each sample spring was selected at baseline. Survey enumerators

TABLE I
BASELINE DESCRIPTIVE STATISTICS (2004 SURVEY)

	Treatment (protected)		comparison		Treatment – comparison
	Mean (s.d.)	Obs.	Mean (s.d.)	Obs.	(s.e.)
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 randomization balancing					
Distance of spring from paved road (m)	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)

TABLE I
(CONTINUED)

	Treatment (protected)		comparison		Treatment – comparison
	Mean (s.d.)	Obs.	Mean (s.d.)	Obs.	(s.e.)
	(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)
B: Household-level data					
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)

TABLE I
(CONTINUED)

	Treatment (protected)		comparison		Treatment – comparison
	Mean (s.d.)	Obs.	Mean (s.d.)	Obs.	(s.e.)
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	0.10
	(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 "reference" spring indicator	0.82	661	0.80	668	0.02
	(0.38)		(0.40)		(0.03)
Multisource user (uses sources other than reference spring)	0.45	732	0.44	715	0.00
	(0.50)		(0.50)		(0.04)
Fraction of respondent water trips to "reference" 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)

TABLE I
(CONTINUED)

	Treatment (protected)		comparison		Treatment – comparison
	Mean (s.d.)	Obs.	Mean (s.d.)	Obs.	(s.e.)
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)
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	0.50	995	0.02
	(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. Reference spring is based on spring user lists. Children in panel C were under age three at baseline or were born since then.

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," and the spring they use is denoted their "reference spring." The number of household spring users varied from 8 to 59 with a mean of 31. Seven to eight households per spring were randomly selected 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; the few nonusers 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 within 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 within a 10-minute walk from the spring.

II.C. 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 (EPA). The water quality measure we use is contamination with *E. coli*, an indicator bacterium that is correlated with the presence of fecal matter, as measured by the natural log of the most probable number (MPN) of colony-forming bacteria per 100 ml of water. The household survey gathered baseline information about child diarrhea and anthropometrics, mothers' hygiene knowledge and behaviors (handwashing), household water collection and treatment behavior, and socioeconomic status. The target survey respondent was the mother of the youngest child living in the home compound (where extended families often co-reside), or another woman with child-care responsibilities if the youngest child's mother was unavailable.

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 in August–November 2005, and the second follow-up survey one year later (August–November 2006). The third follow-up survey took place five months later

(January–March 2007). The main analysis sample consists of 184 springs and 1,354 households with baseline data and at least one round of follow-up data. Attrition was modest: 95% 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 treatment is 0.012 (s.e. 0.018). The characteristics of households lost over time are statistically indistinguishable from those that remain.

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, the third follow-up survey for this subset of households is excluded from the analysis. The POU intervention is studied in Kremer et al. (2008).

II.D. Baseline Descriptive Statistics

Table I 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 were dropped from the analysis because the spring was later found unsuitable for protection, although results are almost unchanged with the slightly smaller main sample (see Appendix Table A.I). There is no statistically significant difference between baseline water quality at treatment versus comparison springs (panel A). Using water quality designations drawn from EPA standards, most spring water is of moderate quality and only about 5–6% of samples are of high quality. Household water is somewhat more likely to be high quality prior to spring protection in the treatment group (and the difference, though small, is significant at 95% confidence), but there is no significant difference in the proportion of moderate- or poor-quality water (panel B). A Kolmogorov–Smirnov test cannot reject equality of baseline home water quality distributions for the treatment and comparison groups (p-value = 0.24).

Household water quality is somewhat better than spring water quality at baseline: the average difference in ln *E. coli* MPN/100 ml 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 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, it is worth noting that 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 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 past six months, though by the second follow-up survey (when more time had passed since the outbreak) just 8% of households reported chlorinating their water in the past week.

Water quality tests were also conducted at the two main alternative sources near each sample spring during the third follow-up (2007). 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 type, at 92%, followed by boreholes (87%), unprotected springs (75%), shallow wells (73%), lakes/ponds (31%), and streams/rivers (14%). Yet the correlation between *E. coli* levels at water sources and household perceptions of source water quality (on a 1–5 scale with 1 = very clean and 5 = very unclean) is just 0.12 (s.e. 0.02), though this rises to 0.19 (s.e. 0.02) when conditioning on household fixed effects. This is just under half the correlation of actual *E. coli* counts across successive survey rounds (0.46). This moderate correlation of objective *E. coli* counts over time is presumably due both to measurement error and fluctuation in true contamination.

Most other household and child characteristics are similar across the treatment and comparison groups (Table I, 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 poor countries as about 85% of households report having a latrine, and the mean walking distance (one-way) to the closest local water source is 9 minutes (median 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.

III. Spring Protection Impacts on Water Quality and Health

This section discusses estimation and spring protection impacts on water quality and child health.

III.A. Estimation Strategy

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

$$W_{jt}^{SP} = \alpha_t + \phi_1 T_{jt} + X_j^{SP\prime}\phi_2 + (T_{jt} * X_j^{SP})'\phi_3 + \varepsilon_{jt}. \tag{1}$$

W_{jt}^{SP} is the water quality measure for spring j at time t ($t \in \{0, 1, 2, 3\}$ for the four survey rounds) and T_{jt} is a treatment indicator that takes on a value of 1 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 I). X_j^{SP} are baseline spring and community characteristics (e.g., water contamination) and ε_{jt} is a white noise disturbance term that is allowed to be correlated across survey rounds for a spring. Random assignment implies that ϕ_1 is an unbiased estimate of the ITT spring protection effect. In some specifications we explore differential effects as a function of baseline characteristics, captured in the vector ϕ_3. Survey round and wave fixed effects α_t are also included to control for time-varying factors affecting all groups, as are the variables used to balance the randomization into treatment groups (see Bruhn and McKenzie 2008; Supplementary Appendix I). Estimates of the local average treatment effect (LATE; Angrist, Imbens, and Rubin 1996) are very similar to the ITT estimates as assignment differs from actual treatment for few springs.

III.B. Impact of Protection on Spring Water

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 a 66% reduction (Table II, regression 1). These estimated effects are robust to including baseline contamination controls, and protection does not lead to a significantly larger proportional reduction where initial contamination was highest (regression 2). There is substantial mean reversion in water quality across survey rounds, likely reflecting both measurement error and transitory water quality variation.[6] There is no 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 not greater water yields (regression 5), consistent with spring protection improving water quality but not quantity. Communities maintain protected springs better than unprotected springs: protected springs also have better fencing and drainage and less fecal matter and brush in the vicinity (not shown).

III.C. Home Water Quality Effects

We use a regression analogous to Equation (1) to estimate the impact of spring protection on home water quality. 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, and we also allow for differential treatment effects as a function of these characteristics. Regression disturbance terms are clustered by spring.

The average reduction in ln *E. coli* contamination at the home is -0.27 (Table III, regression 1), or roughly 24%, considerably smaller than the impacts on source water quality. For "sole source" households, those who used only water from their reference spring in the pretreatment period, home water quality should be unambiguously better after treatment because they still rely mainly on the spring and its quality improves after protection. For baseline "multisource" water users in our data, who were roughly on the

6. For evidence on mean reversion, note the downward slope of the nonparametric plot in Appendix Figure I.A.

TABLE II
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			–0.07		
			(0.58)		
Baseline latrine density			0.90		
*Treatment indicator			(1.76)		
Baseline diarrhea prevention score			–0.04		
			(0.07)		
Baseline diarrhea prevention score			–0.29		
*Treatment indicator			(0.25)		
Baseline boiled water yesterday density			0.59		
			(0.68)		
Baseline boiled water yesterday density			0.92		
*Treatment indicator			(1.52)		
Baseline mother's years of education density			–0.06		
			(0.05)		

TABLE II
(CONTINUED)

	Dependent variable ln(spring water *E. coli* MPN)			Water clarity (observed)	Water yield (observed)
	(1)	(2)	(3)	(4)	(5)
Baseline mother's years of education density			0.06		
*Treatment indicator			(0.14)		
Treatment group 1 (phased in early 2005)			–0.25		
			(0.20)		
Treatment group 2 (phased in late 2005)			–0.17		
			(0.17)		
R^2	0.30	0.43	0.45	0.13	0.13
Observations	726	726	726	478	474
Mean (s.d.) of dependent variable in comparison group	3.63 (1.95)	3.63 (1.95)	3.63 (1.95)	0.76	0.80

Notes. Estimated using ordinary least squares. Huber–White robust standard errors are presented (clustered at the spring level), significantly different than 0 at * 90%, ** 95%, *** 99% confidence. There are 184 spring clusters with data for some of the four survey rounds (2004, 2005, 2006, and 2007). MPN stands for "most probable number" colony-forming units (CFU) per 100 ml. 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?" Outcomes in columns 4 and 5 are enumerator assessments of spring water clarity and the spring's water yield. All variables that are interacted with the treatment indicator are de-meaned. 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. Baseline iron roof density and its interaction with the treatment indicator (in column 3) 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 100 ml.

TABLE III
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	0.03
	(0.05)	(0.05)	(0.05)
Baseline multisource user		–0.29	–0.27
		(0.16)*	(0.17)
Baseline multisource user * Treatment indicator		0.04	0.06
		(0.25)	(0.26)
Baseline latrine density	–0.73	–0.73	–0.02
	(0.32)**	(0.31)**	(0.60)
Baseline latrine density * Treatment indicator			1.42
			(1.01)
Baseline diarrhea prevention score	–0.02	–0.03	–0.05
	(0.02)	(0.02)	(0.04)
Baseline diarrhea prevention score * Treatment indicator			–0.05
			(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	0.02
	(0.01)	(0.01)	(0.02)
Baseline mother's years of education * Treatment indicator			0.02
			(0.04)
Treatment group 1 (phased in early 2005)	0.00	–0.14	–0.01
	(0.14)	(0.18)	(0.27)
Treatment group 2 (phased in late 2005)	–0.10	–0.12	–0.16
	(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	3.00	3.00
	(2.27)	(2.27)	(2.27)

Notes. Estimated using ordinary least squares. Huber–White robust standard errors (clustered at the spring level) are presented, significantly different than 0 at * 90%, ** 95%, *** 99% confidence. MPN stands for "most probable number" colony-forming units (CFU) per 100 ml. 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. Additional control variables are: 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 of these control variables with the treatment (protected) indicator (not shown in the table). Baseline spring water quality, latrine density, diarrhea prevention score, and mother's education are de-meaned. The −0.27 effect in column 1 is equivalent to a 24% reduction in *E. coli* fecal coliform units per 100 ml.

margin between using their reference spring and other sources, spring water will be combined in the home with water of unknown quality from other sources, and endogenous source choice could thus cause home water quality to increase or decrease after protection depending on whether these alternative sources are cleaner or dirtier than the spring. The point estimates of contamination reductions are slightly smaller for multisource households (regression 2), as predicted, but we cannot reject equal impacts for sole- and multisource users.[7]

Using the comparison households, we also nonexperimentally estimated the relationship between the use of different water source types and household water quality. Note that we do not expect a simple microbiological relationship between source water quality and home water quality following storage because bacteria both grow and die off over time, where the extent of growth and death may depend on storage conditions. Conditional on collecting some spring water, 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* MPN/100 ml contamination (s.e. 0.18), or roughly 37% (not shown), substantially larger than the more reliable experimental estimates in Table III. Other nonexperimental approaches—such as including detailed controls for respondent education, boiling and at-home chlorination (and interaction terms), or employing distance to the protected source as an IV for use (point estimate 0.46)—also differ substantially from the experimental estimate (not shown).

7. Random assignment of springs to protection implies that we might potentially 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 (IV) framework in the sole-source users subsample, with assignment to protection as the IV for spring water quality. Sole-source users could be useful for estimating the passthrough of source water quality gains to the home if these households almost exclusively used the sample spring for drinking water in all periods. Unfortunately, water use patterns are not static across our 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-ups. 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 multisource user status becomes less meaningful over time, making it infeasible to reliably estimate passthrough in this way.

We find no evidence of differential treatment effects as a function of household sanitation, diarrhea prevention knowledge, or mother's education (Table III, 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 water sources or households 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 km) on spring water quality.[8] Because we also control for the total number of nearby springs (protected or not), we thus exploit variation induced by the spring protection experiment to identify spillover effects, in a manner related to Miguel and Kremer (2004). For springs we find little evidence of externalities in water quality: the coefficient estimate on treated springs within 3 km is small at −0.004 (s.e. 0.086), and similar results hold for springs at other distances (not shown).

On the other hand, there are positive spillovers on household water, and although we cannot completely rule out the possibility of some hydrological or epidemiological externalities, the effects appear to be mainly driven by some comparison households switching to use nearby protected sources. Households living near treatment springs are those that appear to benefit most. At baseline, 15.4% of comparison households get at least some of their drinking water from protected springs, whereas in follow-up rounds, this percentage rises to 24.5%. Some of this increase is due to the secular increase over time in spring protection funded by donors or government, but much is due to comparison household trips to our sample treatment springs. We quantify the extent of this use and simultaneously estimate the effect of the fraction of trips to protected springs on home water quality in an IV specification, using both the treatment assignment of a household's own reference spring as well as the number of springs assigned to

8. 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. There are no significant baseline differences in the total number of nearby springs within 1, 3, or 6 km for the treatment versus comparison groups (not shown).

treatment located within 1 km of the home as instruments. The protection of springs located farther than 1 km from the home did not significantly affect the fraction of trips to protected springs, and thus we focus on the 1 km densities.

The first-stage relationship is strong, as presented in supplementary Appendix Table A.II. The treatment assignment of a household's reference spring increases the fraction of trips to protected springs by 69.0 percentage points (s.e. 1.6 percentage points), as expected, and the number of protected springs within 1 km of the household is also associated with significantly more trips to protected springs (6.6 percentage points, s.e. 1.2) but only among households in spring protection "comparison" communities. This is reasonable because households in treatment communities already greatly increased their use of their now-protected reference spring, whereas households in comparison communities shifted increasingly to other springs. A Wald estimation procedure allows us to recover the LATE of increased protected spring water use on home water quality (column 2). Focusing on our preferred specification from Table III, the estimated effect of the fraction of trips to a protected spring source on the log of home water contamination is -0.430 (s.e. 0.165, column 2 of Appendix Table II).

This suggests that if there is a roughly linear impact of greater protected spring water use, then shifting from zero trips to 100% of trips to protected springs would reduce home water contamination by roughly 40%. Thus, even complete switching to protected sources would only reduce home water contamination by slightly less than half (0.43 in ln *E. coli*) of the total reduction in contamination at the source, which is 1.04 (from Table II). Recontamination of drinking water in storage and transport is likely to account for the difference between these two figures.

III.D. Child Health and Nutrition Impacts

We estimate the impact of spring protection on child health and anthropometrics in Equation (2),

$$Y_{ijt} = \alpha_i + \alpha_t + \phi_1 T_{jt} + X_{ij}'\phi_2 + (T_{jt} * X_{ij})'\phi_3 + u_{ij} + \varepsilon_{ijt}. \tag{2}$$

The main dependent variable is an indicator for diarrhea in the past week. The coefficient estimate, ϕ_1, on the treatment indicator T_{jt} captures the spring protection effect. We include child fixed effects (α_i) and survey round and month fixed effects (α_t) in some specifications. We also explore heterogeneous treatment effects as a function of child and household characteristics, X_{ij}.

Spring protection significantly reduces diarrhea for children under age three 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 IV, regression 1). The estimated impact is similar in a probit specification (−4.4 percentage points, s.e. 2.0, regression 2), and a linear specification with child and survey month fixed effects (−4.5 percentage points, s.e. 2.3, p-value = 0.05, regression 3). In our preferred specification with month and child fixed effects and gender-specific age polynomial controls, the point estimate is −4.7 percentage points (s.e. 2.3, regression 4). On a comparison group average of 19% of children with diarrhea in the past week, this is a drop of one quarter. We conclude that the moderate reductions in household water contamination caused by spring protection were sufficient to meaningfully reduce diarrhea incidence.[9]

Although the estimated reduction in diarrhea remains negative for boys, the effects are driven mainly by girls (Table IV, regression 5). For girls the estimated reduction is 9.0 percentage points. This finding is surprising because 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.[10]

Interactions with baseline latrine coverage, diarrhea 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 and multisource households

9. Using children in comparison households in a nonexperimental analysis using the same controls as in Table IV (columns 3 and 4), we once again find that nonexperimental and experimental estimates differ sharply. Households that choose to obtain water from protected springs do not have significantly lower diarrhea than other households: the coefficient on the fraction of water collection trips to protected springs is 0.007 (s.e. 0.041). Using the sample of comparison children, we also find no evidence that water quality, as measured by *E. coli* at either the household or source, significantly impacts diarrhea. This may occur because water quality measures are noisy, leading to attenuation bias, and because current quality is measured in the survey while diarrhea is for the prior week.

10. A speculative possibility is that if male infants are generally weaker than females, as suggested by some studies, then any given health improvement requires a larger increase in health inputs for males; we thank a referee for this point. Unlike Jayachandran and Kuziemko (2009), we do not find differential gender breastfeeding.

TABLE IV
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: BMI (kg/m²)	
	(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)	(0.021)	(0.023)	(0.023)	(0.029)	(0.039)	(0.075)	(0.100)	(0.13)*	(0.16)
Treatment (protected) indicator * Male					0.083**			–0.054		–0.12
					(0.040)			(0.121)		(0.18)
Treatment (protected) indicator * Baseline latrine density						0.105				
						(0.123)				
Treatment (protected) indicator * Baseline diarrhea prevention score						–0.008				
						(0.007)				
Treatment (protected) indicator * Baseline mother's years of education						0.002				
						(0.004)				

TABLE IV
(CONTINUED)

	Dependent variable: diarrhea in past week						Dependent variable: weight (kg)		Dependent variable: BMI (kg/m^2)	
	(1)	(2) Probit	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
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	0.19	0.19	0.19	0.19	0.19	0.19	11.36	11.36	17.0	17.0
variable in the comparison group	(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 ordinary least squares. Huber-White robust standard errors (clustered at the spring level) are presented, significantly different than 0 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 (3)–(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.

(not shown). Protection effects do not differ significantly by month of year (rainy versus dry season) nor by child age up through five years (not shown).

There are no statistically significant impacts on child weight but impacts are positive and marginally significant for body mass index (BMI) in the three follow-up surveys (Table IV, 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: in a specification equivalent to regression 1 in Table IV, the point estimate is -1.7 percentage points (standard error 0.5 percentage points, not shown), on a base rate of 4.1%, though the effect is not 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 for adults (not shown).[11]

Because some comparison households also choose to obtain water from protected springs, as documented, these estimates are likely to understate the true impact of using protected spring water on diarrhea incidence, and thus we apply the IV approach used to estimate home water quality impacts. Focusing on our preferred sample and controls from column 4 of Table IV, the IV estimate of the effect of the fraction of household water trips to protected springs is a 6.0-percentage-point reduction in diarrhea (Supplementary Appendix Table A.II, p-value = 0.101), a reduction of nearly one third of baseline levels if households were to switch from zero to 100% use of protected spring water.

III.E. Estimating Water Transport, Storage, and Treatment Behavior 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 on water transport, storage, or home treatment behaviors.

11. We collected information on infant mortality in our household sample, and also from a somewhat larger sample of households with the assistance of local village elders who kept a diary of local infant births and deaths. 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 (not shown).

Empirically, however, there were no significant changes in water handling or treatment behaviors (Table V, panel A) aside from the increased use of protected springs, discussed shortly. There are also no 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 already discussed some of the implications of endogenous source choice for estimating household water quality impacts. Recall that each household in our data set is linked to a particular spring (their "reference" spring) in the baseline user list. The potential for differential impacts among users of this reference spring arises because protected spring use should increase more among multisource users than sole-source users (who are already at 100% usage). As predicted, 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 were multisource users at baseline (Table V, panel C). Underlying this increased use of protected springs are increasingly positive perceptions about their quality: 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 is no significant effect on the total number of trips made to water sources in the past week, further indication that the intervention did not change water quantity used.

IV. Valuing Clean Water

This section uses a travel cost model of water source choice to estimate revealed preference spring protection valuations, and compares them to two stated preference approaches. We argue that households' valuation of health is smaller than typically assumed by public health planners, but consistent with Hall and Jones's (2007) estimates of high income elasticity of demand for health.

IV.A. A Travel Cost Model of Household Water Source Choice

Let the valuation of water from source j be Z_j, which could reflect both health and nonhealth attributes, such as the ease of

TABLE V

TREATMENT EFFECTS ON HOUSEHOLD WATER SOURCE CHOICE AND HEALTH BEHAVIORS (2004–2007)

Dependent variable	Coefficient (s.e.) on treatment indicator full sample (1)	Coefficient (s.e.) on treatment indicator sole-source users (2)	Coefficient (s.e.) on treatment indicator multisource users (3)	Mean (s.d.) comparison group in 2006, 2007 surveys (4)
A: Water transportation and storage				
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)
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)

TABLE V
(CONTINUED)

Dependent variable	Coefficient (s.e.) on treatment indicator full sample (1)	Coefficient (s.e.) on treatment indicator sole-source users (2)	Coefficient (s.e.) on treatment indicator multisource users (3)	Mean (s.d.) comparison group in 2006, 2007 surveys (4)
C: Water collection and source choice				
Fraction of trips to reference spring	0.09 (0.03)***	0.03 (0.02)*	0.21 (0.05)***	0.76 (0.40)
Perceive water at reference 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 reference 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)

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 0 at * 90%, ** 95%, and *** 99% confidence. Reported means of the dependent variables are in the comparison group 2006 and 2007 (rounds 2 and 3 post-treatment) surveys. Reference 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 (values range from 0 to 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.

water collection. Spring protection at source j in time t (T_{jt}) contributes an additional benefit β_i to household i's indirect utility. 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 $C_i D_{ij}$, where D_{ij} is the household's round-trip distance to the 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 needs to undertake, or their mood that day. Household i's indirect utility from one water collection trip to source j at time t is:

$$u_{ijt} = \beta_i T_{jt} + Z_j - C_i D_{ij} + e_{ijt}, \tag{3}$$

where e_{ijt} is an i.i.d. type I extreme value error term. Household i chooses source j over an alternative k if its benefits outweigh any travel costs, namely, when $\beta_i(T_{jt} - T_{kt}) + (Z_j - Z_k) - C_i(D_{ij} - D_{ik}) + (e_{ijt} - e_{ikt}) \geqslant 0$. Focusing on those households on the margin between choosing two sources conceptually allows one to estimate households' valuations. The additional travel cost households choose to incur is a revealed preference measure of their willingness to pay for spring protection.[12]

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

$$P(y_{ijt}|X) = \frac{\exp(X'_{ijt}B)}{\sum_h \exp(X'_{iht}B)} \equiv \rho_{ijt}. \tag{4}$$

The ratio of the coefficient estimate on the treatment (spring protection) indicator to the coefficient estimate on walking time to

12. We follow most of the discrete choice literature in assuming a constant utility benefit from each additional trip to a water source. Although declining marginal benefits from each additional trip to a clean source is plausible if water quality is more important for some uses (like drinking) than others, we find no evidence for it in our data. To illustrate, there is no significant difference in valuation of spring protection (from Table VI, later) for households with different baseline usage of their reference spring: with spring protection valuation as the dependent variable, the coefficient estimate on an indicator for baseline sole source use is 0.84 (s.e. 1.06), and, in a separate regression, the coefficient on the proportion of collection trips to the reference spring is 1.71 (s.e. 1.14, results not shown).

a source delivers the value of spring protection in terms of minutes spent walking. We also allow the households' time costs and valuation of spring protection to vary as a function of the number of children in the household, and household sanitation, hygiene knowledge, and education, by including interactions between them and the treatment indicator and the walking time term. Given our data, note that the predicted probabilities do not vary by trip t.

After estimating the conditional logit (which does not allow for parameter heterogeneity across households), we follow Berry, Levinsohn, and Pakes (1995), Train (2003) and others in explicitly estimating heterogeneity using a mixed logit model with random coefficients on spring protection and walking distance in the household indirect utility function. This requires the imposition of more structure on the distribution of preferences. Choice probabilities are:

$$P(y_{ijt}|X) = \int_B \rho_{ijt} f(B)\, dB, \tag{5}$$

where y, X, B, and ρ are defined as before, 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 nonnegative) for the distance coefficient, although results are almost unchanged if the triangular distribution is used for the spring protection coefficient instead (not shown). Bayesian numerical methods maximize the log-likelihood to estimate the mean and standard deviation of these distributions and allow both for household specific taste parameter estimates, as well as arbitrary correlations of spring protection valuation and walking time disutility across households.[13]

We use data from the third follow-up survey, which asked respondents for the universe of water sources they could potentially choose and the number of trips made to each in the last week. The subscript t denotes a single water collection trip. The median respondent used two water sources in the last week and 65% of respondents named available alternatives that they chose not to use.

13. We use simulated maximum likelihood methods in the mixed logit estimation to obtain posterior distributions for the two variables with random coefficients (the spring protection indicator and the walking time to the source), as well as household specific preference parameters. The household specific estimates are obtained conditioning on actual household choices to generate a posterior distribution for each household (see Train 2003, chap. 11).

IV.B. 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 (s.e. 0.001, Table VI, regression 1) and a positive statistically significant effect on the treatment (protected) indicator term (0.51, s.e. 0.051).[14] Other terms in the regression indicate that streams, rivers, and wells are less preferred than nonprogram springs (the omitted source category), and there are only minor differences in tastes for program (sample) springs, nonprogram springs, and boreholes. The distance to the closest water source is 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 time affects choice.[15]

One issue with the interpretation of this result is possible measurement error and attenuation bias in the reported distance walking variable. The correlation across survey rounds in the reported walking distance to the reference spring is moderate, at 0.38. In addition to simple recall error, the variation in reported walking time may be due to 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 that day. To approximately correct for classical measurement error in this term, we inflate its coefficient to $-0.055/0.38 = -0.145$ and use this correction in what follows, although the correction estimated in a Monte Carlo simulation is similar at 0.3 (not shown).

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 0 (Table VI, regression 2), consistent with the possibility that

14. In Table VI, we exploit variation in spring protection status in reference springs, but estimated spring protection valuations are nearly identical using the variation in the protection of nonreference springs induced by the program: the estimated coefficient on protection of nonreference springs is 0.53, nearly identical to the 0.51 estimate in column 1 of Table VI. We focus on protection of reference springs to maintain consistency throughout.

15. We do not find significant differences at traditional confidence levels between the households in the Table VI estimation samples and the full sample along a wide range of observable characteristics (not shown).

TABLE VI
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	0.57***	0.68***		0.96***	
	(0.051)	(0.08)	(0.08)	(0.09)		(0.24)	
Mixed logit—mean (normal):					2.95***		1.46**
					(0.25)		(0.60)
Mixed logit—s.d. (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***	–0.047***	–0.053***		–0.033***	
	(0.002)	(0.002)	(0.003)	(0.002)		(0.010)	
Mixed logit—mean (restricted triangular):					–0.21***		–0.03***
					(0.01)		(0.01)
Mixed logit—s.d. (restricted triangular)					0.09		0.01

TABLE VI
(CONTINUED)

	Revealed preference					Stated ranking	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Distance * children aged 0–3			–0.008***				
			(0.002)				
Treatment indicator * children aged 0–3			0.04				
			(0.04)				
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				0.058***			
				(0.011)			
Source type: borehole/piped	–0.08		–0.10	–0.13*	–1.02***	0.07	0.04
	(0.07)		(0.07)	(0.08)	(0.14)	(0.25)	(0.27)
Source type: well	–0.28***		–0.31***	–0.31***	–1.87***	–0.43*	–0.47*
	(0.06)		(0.07)	(0.07)	(0.13)	(0.24)	(0.25)
Source type: stream/river	–0.77***		–0.70***	–0.63***	–1.46***	–2.19***	–2.25***
	(0.09)		(0.09)	(0.09)	(0.15)	(0.52)	(0.53)

TABLE VI
(CONTINUED)

	Revealed preference					Stated ranking	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Source type: lake/pond	–0.20		–0.30	–0.18	–0.32	–2.82	–2.85
	(0.20)		(0.20)	(0.19)	(0.35)	(1.86)	(1.87)
Number of observations (water collection choice situations)	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 weighting households equally). Significant at * 90%, ** 95%, and *** 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 included for children aged 3–12 at baseline interacted with the treatment indicator and distance to the water source (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.

households' greater valuation of protected springs is almost 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 objective *E. coli* contamination as an explanatory variable but excludes perceived water quality (for which respondents might give self-justifying answers that are endogenous to their actual choices) reveals that although 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 the magnitude of the amenity value attached to spring protection beyond 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 one additional year of experience with spring protection due to the phase-in of treatment (results not shown), although we cannot rule out that one additional year is insufficient for substantial learning about true impacts.

Households with young children could potentially have both greater time costs of walking to collect water (due to the demands of child care or carrying a 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 three at baseline find additional walking distance to be more costly, as predicted, and the estimate is large and significant at 99% confidence (Table VI, regression 3). The coefficient on the interaction between the treatment indicator and households with child under three is positive, suggesting somewhat greater preference for spring protection, but the effect is not significant. Given the walking time effect, however, willingness to walk for protection actually falls slightly for each additional child under age three in a household.

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 VI, regression 4), with the latter result suggesting that schooling investments for mothers might translate into greater child health investment. However, the choice of protected springs is not significantly affected by baseline respondent diarrhea prevention knowledge (in the household survey), or by stated knowledge of a direct link between contaminated water and diarrheal disease (not shown). Asset ownership does not affect the taste for protection, nor does child gender (but recall that health gains appear concentrated among girls), and including higher order walking distance polynomial controls does not substantively change the results (not shown). Although replacement models of child valuation might suggest that older mothers place greater value on child health and life, because the costs of giving birth to another child are likely to rise with age, spring protection valuations do not differ by mother age (not shown).

The ratio of the two main coefficient estimates can be computed for each household in the mixed logit analysis to yield the valuation of spring protection in terms of minutes of walking time (Table VI, regression 5 and Table VII, panel A). Using the average number of trips per week to sample springs, over the course of a year the mean value of spring protection for a household is 32.4 work days, with considerable dispersion in valuations.[16]

IV.C. Comparing Revealed versus Stated Preference Water Valuations

This subsection compares our revealed preference spring protection valuations to two different stated preference approaches, stated ranking and contingent valuation. The stated ranking approach asks respondents to rank order their potential water source options rather than relying on information on actual household water trips. This ranking is performed sequentially in the survey, with the highest ranked source eliminated from the choice set at each subsequent question. These data are then analyzed in the discrete choice travel cost framework already described. In comparing the results from this exercise to revealed preference valuation estimates, we note that stated preference valuation estimates are expected to capture both use and nonuse values, and thus should be higher than the strictly use value estimates captured by revealed preference. Although some people may derive positive value from knowing that an additional water source

16. The median value of spring protection in the mixed logit analysis is somewhat lower, at 18.5 work days (not shown), and this is similar to the median valuation in the conditional logit specification. This strengthens our main finding of quite low valuation for spring protection among most households in this setting.

TABLE VII
VALUATION OF ONE YEAR OF SPRING PROTECTION (2007 SURVEY)

	One year of spring protection	
	Mean	s.d.
A: Revealed preference valuation (from mixed logit; Table VI, column 5)		
Work days (8-hour days)	32.4 days	102.8 days
Time value from survey questions (time and monetary value)	$2.96	$11.14
Assume value of time is 25% Kenyan worker average wage	$11.57	$36.69
B: Stated preference ranking valuation (from mixed logit; Table VI, column 7)		
Work days (8-hour days)	56.2 days	12.3 days
Time value from survey questions (time and monetary value)	$4.96	$1.97
Assume value of time is 25% Kenyan worker average wage	$20.06	$4.38
C: Contingent valuation	Full round	Final wave, emphasizing trade-offs
Proportion willing to pay this for spring protection:		
US$3.57 (250 Kenyan shillings)	0.94 [308]	0.80 [98]
US$7.14 (500 Kenyan shillings)	0.90 [316]	0.79 [204]
US$14.29 (1000 Kenyan shillings)	—	0.60 [204]
	One year of spring protection	
	Mean	s.d.
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 number of observations is in brackets in C. The contingent valuation questions were only asked of households in the treatment group, since they have firsthand knowledge of protection. In the final wave of the survey, respondents were first asked if they would be willing to pay either 500 or 1000 Kenyan 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 amount, also with emphasis on the expenditure trade-off.

alternative exists, even if they are not using it, we expect nonuse values to be low in our setting.

Estimated stated ranking valuation for spring protection is much higher than the revealed preference estimate. The magnitude of the coefficient estimate on distance walking falls to −0.033, whereas that on spring protection rises to 0.96 (Table VI, regression 6). Using the same attenuation bias correction for walking distance as before, the mixed logit estimate is almost twice as large as the revealed preference value, with a willingness to pay for one year of spring protection at 56.2 work days (Table VI, regression 7, and Table VII, panel B). Comparing the analogous columns in Table VI (regressions 1 and 6) suggests social desirability bias may also be affecting the state ranking results. 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 ranking case than in revealed preference, and 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 per year to keep their spring protected. The CV questions were only asked of households in the treatment group because they have firsthand experience with spring protection. In the final wave of the survey, respondents were first asked if they would be willing to pay either 500 or 1,000 Kenyan shillings (US$7.14 or $14.29, respectively), 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.[17]

Nearly all households said they were willing to pay $7.14 for one year of spring protection, and the majority of households say they are willing to pay twice that ($14.29) even after being walked through the expenditure trade-offs (Table VII, panel C). The use of the expenditure trade-off prompt reduces willing to pay substantially (by 11–14 percentage points), indicating that these CV results are sensitive to question framing. Valuations are also sensitive to the starting value: those respondents randomly chosen

17. See Supplementary Appendix A.II for the exact survey question wording. As is sometimes the case with contingent valuation, the unusual nature of the hypothetical posed may have made accurate valuation difficult for respondents.

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 overall is $17.64 (s.d. $13.09).

To move from walking time to monetary values for the revealed preference and stated 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 subjects (surveyed after the round 3 follow-up survey) about their willingness to walk additional minutes to access a protected spring. As before, we implemented this using a closed-end format, offering respondents discrete value choices for additional minutes walked. We then did the same thing in terms of willingness to pay money, the standard CV questions. We derive water collectors' time value by dividing their stated monetary valuation for spring protection by their walking time valuation.[18] As we only had the matched monetary and walking time CV data for a subset of 104 respondents, we next regressed the estimated monetary value of water collectors' time on a set of household characteristics (e.g., education, number of children, and asset ownership) in this subsample and then use these estimated coefficients to predict time values for the entire sample. The resulting mean value of time is about $0.088 per eight-hour work day, or about 7% of the wage those carrying water would have earned for local agricultural labor, given that the average casual labor wage in western Kenya is US$0.89 over a 5.63-hour day (Suri 2009), or $1.26 per 8-hour day.[19]

18. We know only the bounds on household time 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 willing to walk 10 but not 15 additional minutes to a protected spring, the median value is 12.61 minutes.

19. This local wage rate likely overstates the true value of time for multiple reasons, including workers' need to travel long distances to work, and the fact that agricultural work labor is not always available, being concentrated in certain peak seasons. Jeuland et al. (2009) estimate household demand for cholera vaccines in Mozambique using a travel cost method and find that the opportunity cost of time was approximately 28% of the mean hourly wage.

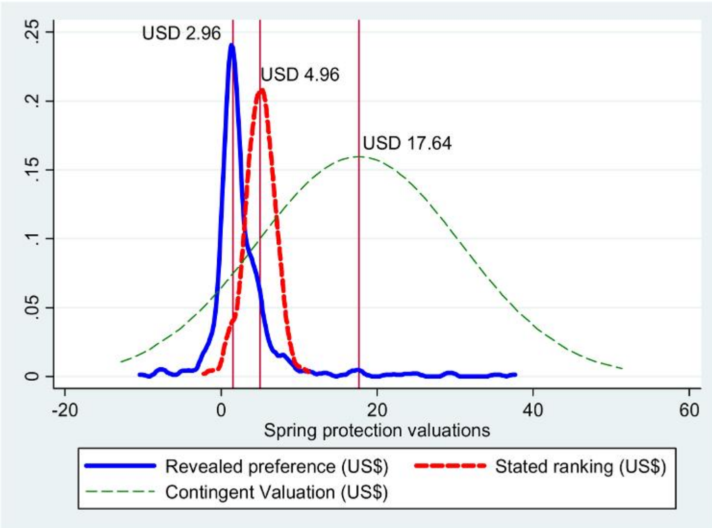

FIGURE II
Household Revealed Preference and Stated Preference Valuations of One Year of Spring Protection (2007)

The revealed preference estimates are from the mixed logit results in Table VI, regression 5, and the stated preference ranking results are from the mixed logit results in Table VI, regression 7. The contingent valuation data are presented in Table VII, panel C.

Combining these household-level estimated time values with our revealed preference mixed logit estimates, the mean valuation for a year of access to protected spring water is only $2.96 (Table VII, panel A). The analogous stated ranking estimate is nearly double at $4.96 (panel B). The estimated distributions for the three valuation approaches (in Figure II) 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 unusual hypothetical exercises and thus their resulting answers are far "noisier" than in the revealed preference case, where they face real time costs.

Because limited time-income substitution possibilities are frequently encountered empirically (Larson 1993; McKean, Johnson, and Walsh 1995), other authors also focus on a range of time values below the individual wage, often 25–50% of the average wage as a starting point (Train 1999). We thus also present revealed preference valuations using 25% of the average local casual labor wage or $0.32 per eight-hour work day (in Table VII), but although valuation levels shift upward, they remain far below the contingent valuation figures. Note that the ratio of stated ranking to revealed preference valuations is unchanged because both are scaled up by the same value of time.

IV.D. Implications for Health Valuation

Under the assumption that households are aware of the relationship between spring protection and diarrhea, combining the results from Tables IV and VI 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 (water clarity, ease of collection, or health gains other than child diarrhea); if these other factors are important, actual willingness to pay to avert child diarrhea will be lower than our estimates. Note that to the extent that people in comparison springs switch to treatment springs in response to the program, we will underestimate both the impact on health and the valuation of spring protection, but to a first-order approximation, both underestimates would be of the same magnitude, so we would not necessarily underestimate health valuations.

If households have difficulty identifying the links between spring protection and diarrhea, or diarrhea and mortality, we may not correctly estimate the valuation of child health using this approach. In a context in which there are multiple environmental channels for the transmission of fecal–oral diseases (e.g., low rates of handwashing and open defecation, particularly by children), it is plausible that the benefits of an improvement along only one dimension are difficult for households to assess. A 20% reduction in diarrhea prevalence, while biomedically important, might imply a change from five diarrhea cases per year to four cases for a typical child, which would be difficult for a parent to detect. Similarly, the cause of death is difficult to link to diarrhea alone; indeed, for children there is often endogenous feedback between diarrhea and malnutrition.

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, this yields an upper bound on the valuation of a statistical life of 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.[20]

Using the household time values derived from our surveys, and returning to the ITT health impacts estimated in Table IV, the upper bound on the value of averting one case of child diarrhea is a mere US\$0.89 (= \$0.088 * 10.1 working days), and on avoiding a child diarrhea death is \$769 (= \$0.088 * 8,742 working days). Using Monte Carlo methods, we estimate a 95% confidence interval ranging from \$555 to \$1,281. Using the same parameter values to convert diarrhea cases to DALYs as used in the calculations of the value of a statistical life and disability weights proposed by Lopez et al. (2006), the \$769 figure corresponds to an upper bound on the value of averting one DALY of about \$23.68. Using the higher time value (25% of the average western Kenyan wage) translates into \$2,715 per averted child diarrhea death and \$83.61 per DALY. These latter figures are likely to be upper bounds on true valuations because water is collected by women and young children who are likely to have much lower than average wages.

This revealed preference bound on the willingness to pay per DALY averted 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 identified health interventions that cost less than \$150 per DALY as "extremely cost effective" (World Bank 1993), and others have used a threshold of \$100 per DALY (Shillcutt et al. 2007). Sachs (2002) has argued for setting health cost effectiveness thresholds per DALY at levels corresponding to countries' gross domestic product per capita, which for Kenya would be over \$400, nearly 20 times higher than our preferred estimate. Although an important source of

20. There are 5.69 deaths per 1,000 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 1991), 1.16 deaths from diarrhea would be averted for each 1,000 diarrhea cases eliminated if mortality is proportional to morbidity.

uncertainty in our valuations is the conversion from the value of time to monetary value, it is worth noting that even if our preferred time values were quadrupled, the implied valuation of health and life would still fall below those typically used by public health planners.

These value of life estimates are also far below the estimated value of a statistical life in the United States and other rich countries (using hedonic labor market approaches), where values typically range from $2 to $7 million (Viscusi and Aldy 2003). Studies from two poorer countries (India and Taiwan) yield estimates on the order of $0.5–1 million, although they are difficult to compare to our sample because they rely on data for urban factory workers. Deaton, Fortson, and Tortora (2009) also find low values of life in African samples using a subjective life evaluation approach. We are unaware of hedonic value of statistical life estimates from the poorest less developed countries.

Our revealed preference estimate of the value of health is consistent with models in which there is a high income elasticity of demand for health, and thus where households' valuation on life in less developed 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. In a calibration of their model (using data from UNDP 2007), in which the value of a year of life is roughly proportional to per capita annual consumption raised to the constant relative risk aversion utility function curvature parameter (which Hall and Jones suggest plausibly takes on a value of two), the value of a statistical life in Kenya ranges from $953 to $2,711. If per capita consumption in our rural study site is only four fifths of the Kenyan national average, this range becomes $477 to $1,603, accommodating our revealed preference estimate of $769.

Establishing the ideal way to conduct welfare analysis here is important but beyond the scope of this article, and thus we present a variety of approaches in Section V. We first present results following the conventional "neoclassical" approach of valuing lives according to households' own revealed preference measures. We then consider the case of a social planner with a $125/DALY valuation (whom we call "paternalistic," for convenience). This may be appropriate, for example, if the planner values averting child diarrhea more than other forms of household consumption, if children receive less weight in the household welfare function than in the planner's welfare function, or if households consider only private benefits of reducing diarrhea and ignore disease externalities.

Using higher spring protection valuations might also be appropriate if households systematically underestimate the health benefits or if they are subject to time inconsistency problems.

V. Simulating Alternative Property Rights Norms and Institutions

Under Kenyan law, local authorities can require landowners to allow neighbors access to water on their land. In our study area, local social norms also prevent spring owners from charging for water. Perhaps partially as a result of these common property rights, virtually no springs are privately protected in our study area. Social norms regarding water rights in the study region date to precolonial times, when there were no centralized kingdoms in the area and the key local sociopolitical unit was the kinship clan (Were 1967, 1986). In the colonial and postindependence eras, administrative boundaries were typically set to at least roughly correspond to clan boundaries, with the region settled by a clan typically being an administrative unit called a sublocation.

In this section, we determine the socially optimal level of spring protection under the alternative assumptions that the social planner takes household revealed preference valuations as given, or that the social planner values child health at levels similar to those assumed by health planners working in poor country contexts, and then estimate social welfare under various property rights norms and institutions. We abstract from the costs of enforcing property rights, focusing on the narrower question of what outcomes social norms would produce if they were costlessly implemented. This discussion should thus be taken as an analysis of the welfare impacts of alternative social norms and institutions and not necessarily an exploration of short-run Kenyan government policy options, because there may be significant enforcement and transactions costs not considered here, as well as other costs in the transition to new institutions. Here we present the main results; the full results, including technical details, are in Supplementary Appendix C.

V.A. *Impacts of Alternative Property Rights Norms*

We consider the impact of alternative water property rights social norms and institutions when households endogenously choose among multiple local water sources, trading off source quality, walking distance, and water price, and spring owners choose whether to invest in spring protection based on profitability. We start by treating the marginal cost of providing spring water as 0 because water flows out of the ground without a pump, user congestion is minimal, and unused water simply flows away. Private property rights allow spring owners to charge for access to spring water, providing an incentive to invest in protection, but also introduce a static distortion in water source choice, since the marginal cost of providing spring water is 0. 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 are socially efficient for them to consider.

We simulate the following game. At $s = 0$, the property rights regime is chosen. At $s = 1$ profit-maximizing spring owners within a subgroup simultaneously decide whether to protect their spring. At $s = 2$ spring owners simultaneously set water prices with full knowledge of each household's water source choice set and preferences, including their distance to each source. At $s = 3$ households choose water sources to maximize utility given protection decisions and prices.[21] We solve the model backward. The Nash equilibrium solution is a vector of protection and price decisions for each spring and consumption decisions for all households, such that 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.

Household demand parameters derived from the revealed preference mixed logit results (in Table VI, column 5) allow us to compute the number of water collection trips made to each spring,

21. We ignore any water consumption utility gains for spring owners because they would not necessarily live locally or consume spring water. We also incorporate demand from new household users postprotection using information from a household user census, as described in Supplementary Appendix C. A household census conducted at a subset of nine springs suggests that protection increases the number of user households by 22% when the water is free. We find in the census that many new users live a greater distance from the spring than baseline users. The welfare gains to protection for these new households are presumably smaller, since they preferred an alternative source to the reference spring at baseline. For a useful approximation of the welfare gains for this group, we assume that their consumer surplus is uniformly distributed between 0 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.

as well as other sources respondents listed in the household survey when asked about potential alternative drinking water sources, as a function of the price and protection status of each source.[22]

Rather than solve the model for the entire area, we consider subgroups of up to four contiguous springs within the same sublocation. Spring owners' profits are equivalent to the net present value of revenues, minus an indicator variable for the protection status of the spring times $1,305, the estimated discounted cost of spring protection construction and maintenance over the estimated 15-year lifespan of a protected spring in this region. We assume that neither spring owners nor planners can prevent resale of water, so pricing is linear in the quantity collected and there is no price discrimination. To determine the Nash equilibrium choice of protection with multiple springs, we estimate best responses to all possible protection/nonprotection combinations and search for a fixed point. When considering the impact of changing property rights norms for springs on private land, we hold constant policies for other water sources. In the rural Kenyan setting, there is typically open access through public paths to rivers and lakes and most boreholes are sunk on public property, such as schools or market centers, so water collection is free at these places.

Household utility and social welfare are expressed in U.S. dollar values on a per spring basis, normalizing social welfare to 0 in the benchmark "status quo" case with common property rights to water and no spring protection (Table VIII, row 1 in panels A and B).[23] We first consider the problem of a neoclassical social planner maximizing the utility of households as indicated by revealed preference, and then a paternalistic social planner valuing each DALY averted from spring protection at $125, roughly five times our preferred estimate of households' average revealed

22. Results presented are averages from 10 runs of the simulations, each with an independent draw of household preference parameters (from the revealed preference mixed logit results). We generally considered springs located within one kilometer of each other to be part of the same subgroup, although in some cases springs at a slightly greater distance from each other were grouped together.

23. We generally do not present results on a per capita basis because the number of users can change with protection, but for a rough sense of per capita gains, recall that the average baseline number of household spring users is 31 and households contain an average of 6.6 members, for roughly 200 persons per community.

valuation. We assume both planners are constrained to allow households to make their own water collection choices based on revealed preference. We then consider the equilibrium under various forms of property rights and two non-budget-balancing mechanisms, public provision and vouchers.

V.B. Social Planner and Property Rights Simulation Results

The neoclassical social planner protects 29% of springs, typically those with many baseline household users (Table VIII, panel A, row 2). The net social gain across all springs (protected and unprotected) is $349 per spring, or roughly $1.75 per capita. The paternalistic social planner would protect 74% of springs (panel B, row 2).

Freehold property rights are somewhat analogous to patents in this analysis, because they spur potentially productive investments but induce static distortions as spring owners have market power in setting prices, given the travel costs households face in water collection. Under freehold private property, only 5% of spring owners find it profitable to protect their spring (Table VIII, panel A, row 3). The net present value of profits per spring owner is $417 and the average price charged per water collection trip is $0.0027. For a household with typical water consumption, this is equivalent to $4.49 per year, about a week's wages. Freehold private property rights substantially reduce social welfare relative to the communal property status quo, with a loss of $91 per spring. The dynamic gains from spring protection are small because few springs are protected, but static losses are large from households walking farther or choosing dirtier sources to avoid paying for spring water. The proportion of household trips to rivers and streams, the dirtiest sources, increases by 38% relative to common property, and as a result average fecal contamination as measured by *E. coli* in collected water increases by 26 log points (from 4.66 to 4.92). Average walking time per collection trip also rises from 11.6 to 12.9 minutes, and this amounts to over 100 extra hours per household each year.

Freehold property rights lead to both under- and over-protection. Though only 13% of the springs that would be protected by a neoclassical social planner are protected under private property, some springs the planner would not protect get protected. The inability of spring owners to capture the

TABLE VIII
PROPERTY RIGHTS NORMS AND INSTITUTIONS: COUNTERFACTUAL 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
A: Revealed preference valuation, neoclassical planner							
Communal property (status quo)	0	0	0	0	11.6	4.66	0
Social planner	29	0	0	722	12.5	4.44	349
Freehold private property	5	0.0027	417	–508	12.9	4.92	–91
Lockean private property	12	0.0069	83	–127	12.3	4.70	–43
Modified Lockean property	2	0.0058	8	26	11.7	4.66	34
Public investment	22	0	0	507	12.4	4.48	130
Public vouchers	11	0.0012	72	336	12.1	4.56	124

TABLE VIII
(CONTINUED)

	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
B: Paternalistic social planner							
Communal property (status quo)	0	0	0	0	11.6	4.66	0
Social planner	74	0	0	2,412	12.7	4.31	2,540
Freehold private property	5	0.0027	417	–508	12.9	4.92	—[a]
Lockean private property	12	0.0069	83	–127	12.3	4.70	—[a]
Modified Lockean property	2	0.0058	8	26	11.7	4.66	91
Public investment	71	0	0	929	12.7	4.32	2,113
Public vouchers	46	0.0032	765	775	12.4	4.43	1,732

Notes. [a]Social welfare in the freehold and Lockean cases in B cannot be reliably compared to the communal property case, as described in Section IV.C.
Profits, utility, and welfare are net present values (5% annual discount over 15 years). Household spring protection valuations are from Table VI, column 5, and utility is converted into USD using households' predicted time value. The Neoclassical planner values spring protection at households' revealed preference level, while the paternalistic planner values it at US$125/DALY averted. A summary of key assumptions is as follows. Communal property rights: the price of spring water is 0. No springs are protected. Social welfare is normalized to 0. Social planner: planner maximizes social welfare. The price of spring water is 0, 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. Freehold private property rights: spring owners simultaneously choose whether to protect springs and then simultaneously choose price per unit of water noncooperatively (in groups of up to four). Spring owners know preferences θ_{ij} for each household. Lockean private property rights: same as the freehold private case except the price of unprotected spring water is constrained to be 0. Modified Lockean property rights: same as the Lockean private case except the spring owner must always provide access to free unprotected water. Public investment: policy maker maximizes social welfare. The price of spring water is 0. There is 30% deadweight loss to raising funds for spring protection. The policy maker knows the distribution of preferences $F(\theta_{ij})$ in the population but not preferences for each household. Vouchers: the policy maker sets the voucher price for protected spring water to maximize social welfare, taking into account effects on spring owners' subsequent investment. Spring owners then make profit-maximizing protection decisions in simultaneous noncooperative play. There is 30% deadweight loss to funding the vouchers. The policy maker knows the distribution of preferences $F(\theta_{ij})$ in the population; spring owners know preferences θ_{ij} for each household.

full consumer surplus of potential users due to heterogeneity in valuations leads to under-protection, but a rent-stealing effect can lead to over-protection, as spring owners do not consider the negative impact of spring protection on owners of competing nearby springs.[24]

It is also worth considering other private property rights institutions beyond the stylized extremes of common property and freehold private property, because actual social norms are often more complex. Locke (2002 [1689]) 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). A "Lockean" property norm in our context would only permit spring owners to charge positive prices if they had invested in spring protection.[25]

Under 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 freehold private property case. On the other hand, the possibility that protecting a spring allows owners to capture not just the valuation on spring protection but also part of the surplus from consuming unprotected water could lead to over-protection. The simulations suggest Lockean private property rights yield somewhat higher investment than freehold private property although there is still substantial under-protection: 12% of owners now find it profit maximizing to invest in spring protection (Table VIII, panel A, row 4). Although social welfare remains lower than the status quo, Lockean rights are marginally better than freehold private property: the social welfare loss per community is only \$43, average fecal contamination increases by just 4 log points,

24. We do not present paternalistic social planner welfare for the freehold or Lockean cases in Table VIII, panel B because they are not directly comparable to the communal property outcomes; see Supplementary Appendix C for a discussion. Note that an alternative policy, which we do not consider, could make transfers to households so valuation of life would be increased. We thank a referee for this point.

25. Historically, some legal systems seem to have evolved from common property for water toward the Lockean norm. For instance, despite the fact that water sales are discouraged by several *hadith* (see Caponera 2006), certain Islamic legal traditions evolved to allow the builders of wells and irrigation canals to charge for access to the water made available by their investments (Mawardi 1900–1901, p. 316; Wansharisi 1909, p. 285).

and average walking time rises slightly. Yet with both fecal contamination and walking time increasing, a paternalistic planner would still likely prefer common property to Lockean norms.

A “modified Lockean” regime under which spring owners could charge for water from protected springs as long as they also allowed free access to unprotected water from the spring generates a Pareto improvement over the common property status quo. 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 that anyone can access. The availability of free unprotected spring water shields households from the utility losses experienced under the other private property cases, and spring owners cannot be worse off than under common property.

Under modified Lockean property rights, 2% of springs are protected (Table VIII, panel A, row 5). Seven percent of the springs a neoclassical social planner would protect are protected, whereas only 1% of springs the planner would not protect are protected (not shown). Although this is far from attaining the social optimum, it incentivizes spring owners to perform some beneficial protection while leaving no households worse off. The paternalistic planner would also prefer this modified Lockean system to common property. Although average ln(*E. coli*) levels are the same to two decimal places in Table VIII, they are in fact lower by 0.003 under the modified Lockean system.

Public funding, either through direct government provision or vouchers, performs much better than the budget-neutral property rights norms already considered. A hypothetical government that has access only to distortionary taxation that creates a 30% deadweight loss, that knows only the distribution of preferences in the population but not individual household preferences, and that seeks to maximize welfare protects 22% of springs (Table VIII, panel A, row 6), including 12% of springs a social planner would not protect. The welfare gain per spring is \$130, less than the \$349 gain attained by a social planner, due to the tax distortion and the misallocation of protection across springs due to policy makers’ limited information on households’ preferences. A government that values health gains from protection at \$125/DALY would protect 71% of springs (panel B, row 6), coming much closer to the corresponding social planner than any of the budget-neutral approaches.

Finally, suppose the government provides households with vouchers that they can pay to spring owners, and spring owners can then exchange these vouchers for a fixed payment from the government. We assume the government restricts spring owners from charging top-up fees to water users (so households' ex post decisions on which water sources to use are efficient), and sets the voucher payment taking into account the later noncooperative protection decisions of private spring owners, once again knowing only the distribution of water preferences in the entire population but not the preferences of individual households, which we assume local spring owners do know. We find that the optimal voucher price is $0.0012 per trip to a protected spring (much less than the price charged under freehold private property), and social welfare gains per spring under water vouchers are $124. Eleven percent of spring owners protect their springs (Table VIII, panel A, row 7), still short of the social optimum, but there is less misallocation of protection because spring owners with better information on local demand make decisions on spring protection: only 3% of the sample springs the social planner would not protect get protected here (not shown in the table). This policy improves social welfare substantially relative to the budget-neutral cases, and social welfare is comparable to government investment. A policy maker with health valuation at $125/DALY sets a uniform voucher price of $0.0032 (panel B, row 7), nearly three times higher than the previous price level, and as a result 46% of spring owners choose protection. Both government cases perform much better than any budget-neutral property rights system under both neoclassical and paternalistic social planners.

V.C. The Determinants of Optimal Property Rights Institutions

Although our simulations suggest that communal property rights deliver higher welfare than freehold private property, they also suggest that freehold property rights could yield higher welfare if income were even moderately higher than current rural Kenyan levels, if technological progress allowed provision of clean water at lower cost, or if water became increasingly scarce. Under the Hall and Jones (2007) claim that the income elasticity of demand for health is roughly 2, and taking into account that both the opportunity cost of travel time and the labor costs of spring protection and maintenance increase proportionally with rising income (but that the costs of the materials used to protect springs, i.e., piping, do not),[26] we estimate that if income increased by 10%, freehold private property rights would become preferable to common property norms. This implies that rural western Kenya is currently very near the income threshold above which private property norms yield higher social welfare than communal norms (and may already have crossed the threshold given the modeling uncertainty inherent in the simulation). If this finding that private property rights become optimal at higher income levels holds more broadly, it could help explain the strong cross-sectional relationship documented between institutions and income levels, and suggests it would be risky to assume that causality always runs from stronger property rights protection to higher income levels (as also noted by Demsetz 1967; North 1990; among others). An analogous argument implies that if new technologies emerged that provided clean water at lower cost, or if population density were higher, thus increasing the social benefits of spring protection, freehold property would again likely become more attractive, because the benefits of encouraging investment would rise.

It thus seems likely that communal property norms for water were in fact socially optimal when they emerged in precolonial Kenya, a historical period when incomes were much lower, the spring protection technology was costlier, and population density—and thus water contamination levels and the number of potential users among whom to share fixed investment costs—was lower.

Water extraction at springs could impose negative externalities on others, for instance, by lowering the local water table. This is in contrast to local rivers, streams, ponds, and lakes, where most water would have flowed away or evaporated. Assuming that neither household water users nor spring owners take into account the negative social costs of water collection at springs, positive water pricing at springs can lead households to collect water in a less socially costly way. However, our simulations suggest that private property would not become efficient in our

26. Using information on actual contractor costs in western Kenya, we estimate that materials constitute roughly 25% of the cost of spring protection and maintenance, and labor costs account for 75%.

context unless the extra social cost of water collection at springs (relative to streams or rivers) was very high, at twice the price that landowners charge for water in equilibrium (in Table VIII), thus constituting a substantial fraction of household income. More generally, allowing for positive pricing could also be more efficient if water collection at springs imposes other social costs, for example, by interfering with the privacy of the landowners who live near spring sites.[27] Our results are in line with Demsetz's (1967) classic discussion, which argues that private property rights become socially optimal, and thus are more likely to emerge, as natural resources become increasingly scarce.

VI. Discussion and Conclusion

Spring protection dramatically improved source water quality in a rural African setting, reducing fecal contamination by two thirds and both home water contamination and child diarrhea by one quarter. By capitalizing on changes in water source choice in response to spring protection, 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 many typical econometric concerns and can be used to validate the reliability of stated preference estimates. Revealed preference estimates of spring protection valuation are far below stated preference estimates and, assuming people understand the effect of spring protection on health, imply valuations of only US$23.68 per DALY, roughly one fifth of the valuations typically used by public health planners. The estimated valuations are consistent with models such as Hall and

27. Supplementary Appendix III presents the details. We thank Robert Barro for suggesting this exercise. When water collection imposes costs on landowners, it is easy to show that private property rights become socially optimal as this cost becomes sufficiently high. Under communal property, higher collection costs lead to increasingly large social losses, with costs borne by landowners. However, under private property, landowners have the option of setting arbitrarily high prices, and thus are able to limit collection trips to their springs. For sufficiently high collection costs, private property rights strictly dominate communal property, once usage costs are so high that they dominate the loss in utility experienced by households diverted from springs to other, less desirable source types. Charging for water could also reduce total water consumption, an important issue in areas where water is scarce. However, this is not a pressing concern in our Kenyan study area. We do not find an impact of walking distance to water sources on the quantity of household water used in our sample (not shown).

Jones (2007), in which the elasticity of demand for health is much greater than 1.

Using structural econometric methods in tandem with the spring protection experiment, we carry out counterfactual simulations to examine the consequences of alternative property rights norms for water. Existing social norms allowing communal access to naturally occurring springs yield higher social welfare than private property norms in this setting, providing a rationale for why communal water rights have been so historically durable in this rural African region. Simulations suggest that private property becomes more attractive than communal norms when water is scarce or health valuations reach a sufficient level that freehold property rights can spur investment, which appears likely to occur with moderate increases in incomes above current rural Kenyan levels.

The property rights simulations abstract away from transaction costs in collecting water user fees, but these are likely to be large under private property norms. Many historical analyses of movements from common property to private property institutions, such as British land enclosures (Allen 1982), may well find they are associated with increased social welfare. Yet this is not inconsistent with arguments that common property institutions are in fact sometimes optimal in other contexts. If health valuations rise with income, and there are substantial costs to shifting to new property rights institutions, then if income follows a stochastic process, societies may only elect to change property rights norms at income levels far above those at which such shifts appear statically efficient. Such changes could also be delayed in practice if political institutions make it difficult for "winners" from the new property rights to credibly compensate "losers," or they can only be undertaken with the consent of supra-majorities.

Our property rights simulations suggest that even at current Kenyan income levels, new institutions and policies could potentially be layered onto existing common property norms to improve social welfare, including government provision or vouchers through which the government pays spring owners based on the number of water users, or by allowing spring owners to charge for improved water while maintaining access to unimproved sites, what we call the modified Lockean approach. In our setting, the government of Kenya and foreign aid donors are in fact protecting increasing numbers of springs over time while maintaining common property access. The analysis in this article suggests their approach is reasonable given current technology and household preferences.

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SUPPLEMENTARY MATERIAL

Supplementary material is available at *The Quarterly Journal of Economics* online.

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