NBER WORKING PAPER SERIES

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

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Working Paper 15280
http://www.nber.org/papers/w15280

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
August 2009

This research is supported by the Hewlett Foundation, USDA/Foreign Agricultural Service, International Child Support, Swedish International Development Agency, Finnish Fund for Local Cooperation in Kenya, google.org, the Bill and Melinda Gates Foundation, and the Sustainability Science Initiative at the Harvard Center for International Development. We thank Alicia Bannon, Jeff Berens, Lorenzo Casaburi, Carmem Domingues, Willa Friedman, Francois Gerard, Anne Healy, Jonas Hjort, Jie Ma, Clair Null, Owen Ozier, Camille Pannu, Changcheng Song, Eric Van Dusen, Melanie Wasserman, and Heidi Williams for excellent research assistance, and thank the field staff, especially Polycarp Waswa and Leonard Bukeke. Jack Colford, Alain de Janvry, Giacomo DiGiorgi, Esther Duflo, Liran Einav, Andrew Foster, Michael Greenstone, Avner Greif, Michael Hanemann, Danson Irungu, Ethan Ligon, Steve Luby, Chuck Manski, Enrico Moretti, Kara Nelson, Aviv Nevo, Sheila Olmstead, Ariel Pakes, Judy Peterson, Pascaline Dupas, Rob Quick, Mark Rosenzweig, Elisabeth Sadoulet, Sandra Spence, Duncan Thomas, Ken Train, Chris Udry, and many seminar participants have provided helpful comments. Opinions presented here are those of the authors and not those of the Bill and Melinda Gates Foundation or its leadership. All errors are our own. The views expressed herein are those of the author(s) and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 15280
August 2009
JEL No. C93,H75,O13,Q25,Q51

ABSTRACT

In many societies, social norms create common property rights in natural resources, limiting incentives for private investment. This paper uses a randomized evaluation in Kenya to measure the health impacts of investments to improve source water quality through spring protection, estimate the value that households place on spring protection, and simulate the welfare impacts of alternative water property rights norms and institutions, including common property, freehold private property, and alternative "Lockean" property rights norms. We find that infrastructure investments reduce fecal contamination by 66% at naturally occurring springs, cutting child diarrhea by one quarter. While households increase their use of protected springs, travel-cost based revealed preference estimates of households' valuations are only one-half stated preference valuations and are much smaller than levels implied by health planners' typical valuations of child mortality, consistent with models in which the demand for health is highly income elastic. Simulations suggest that, at current income levels, private property norms would generate little additional investment while imposing large static costs due to spring owners' local market power, but that private property norms might function better than common property at higher income levels. Alternative institutions, such as "modified Lockean" property rights, government investment or vouchers for improved water, could yield higher social welfare.

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1. Introduction

Many view movement toward private property rights institutions as 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 example, the sale of water is generally not permitted (Faruqui, Biswas, and Bino 2001), and in societies from Tsarist Russia to contemporary west and southern Africa, land is periodically reallocated among families based on assessments of need (e.g., Adams et al. 1999, Bartlett 1990, Fafchamps and Gavian 1996, Peters 2007). Some argue that communities often 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. 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 paper makes four principal 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 less developed 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 health economists in assessing cost effectiveness of health policies 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 divergence between revealed and stated preference valuation for water-related interventions. Finally, we combine data from our randomized experiment with structural econometric methods 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 (Acemoglu, Johnson and Robinson 2001).

[1] Several recent papers combine data from randomized experiments with structural econometric methods in development economics, the best known probably being Todd and Wolpin (2006), who use experimental estimates from Mexico to validate a structural model of educational investment. We do not seek to validate a particular model in this paper, but rather combine experimental results with a structural model of water infrastructure investment to explore the implications of alternative property rights institutions on social welfare.

Policymakers have called for more investment in water infrastructure in less developed countries to provide cleaner water and reduce water-borne diseases such as diarrhea, which accounts for 20% of deaths of children under five each year (Bryce *et al.* 2005). Progress towards the sole quantifiable environmental Millennium Development Goal is currently measured by the percentage of 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 non-experimental evidence that there may be little point in investing in other water infrastructure because diarrhea is affected more by the quantity of water available for washing than by drinking water quality (Curtis, Carincross, and Yonli 2000); improved water supply has little impact without good sanitation and hygiene (Esrey 1996, Esrey *et al*. 1991); and water recontamination in transport and storage may dampen some of the benefits of improved source water quality (Fewtrell *et al*. 2004).

As the first (to our knowledge) randomized evaluation of a source water quality investment, the data used in this paper allow us to isolate the impact of a single intervention affecting the quality but not quantity of water, and to assess child health impacts.[2] We find that spring protection greatly improves water quality at the source, reducing fecal contamination by 66%, and is moderately effective at improving household water quality, reducing contamination 24%. Diarrhea among young children in treatment households falls by 4.7 percentage points, or nearly one quarter on a base diarrhea prevalence of approximately 19 percent. The incomplete pass through of spring-level water gains into the home is due both to households' collection of water from multiple sources and to partial recontamination of the water in transport and storage. There is no evidence that spring protection crowds out household water treatment measures such as boiling or chlorination. There is also no evidence that improved sanitation coverage or hygiene knowledge allows households to better translate source water quality gains into larger improvements in household water quality.

The second part of this paper 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. A discrete choice model, in which households trade off water quality against walking distance to the source, generates

[2] Two prospective studies of source water quality interventions find positive child health impacts (Aziz et al. 1990, Huttly et al. 1987), but the published articles do not mention if the treatment villages were randomly selected, and generalizing to other settings is hampered by their small sample sizes (five villages each), and they evaluate improved water quality and quantity simultaneously.

revealed preference estimates of household valuations of better water quality. Households' estimated mean annual valuation for spring protection is equivalent to 32.4 workdays. Based on household reports on the tradeoffs they face between money and walking time, this corresponds to approximately US$2.96 per household per year. 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 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. These results are consistent with an income elasticity of demand for health far greater than one.

We contrast the revealed preference valuation of spring protection, which exploits experimental variation in water source characteristics, with two stated preference methodologies: stated ranking of alternative water sources, and contingent valuation (Carson *et al.* 1996, Whitehead 2006). Most valuation estimates rely on such stated preference data, which is relatively cheap to collect, yet few stated preference estimates have been validated against reliable benchmarks since revealed preference data is rarely available in less developed countries, and many studies that do exist are prone to omitted variable bias critiques.[3] We find that stated preference approaches generate much higher valuations than revealed preference estimates, by a factor of two, with contingent valuation yielding especially imprecise estimates, casting doubt on their reliability in this setting.

Our final set of results simulates the impact of alternative social norms and property rights institutions in the rural water sector. We first show that a social planner maximizing welfare as captured by our revealed preference valuation estimates would only protect springs with a relatively large number of household users, but that a paternalistic social planner who valued health at the levels typically used by health planners would protect many more springs. Using the household water demand system derived from the revealed preference valuations, we then conduct counterfactual simulations and find that alternative property rights institutions have important social welfare

[3] Madajewicz et al (2007) find considerable responsiveness to information on water source 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 using a contingent valuation (CV) approach. However, neither considers water quality in the source choice decision and they explicitly rule out multiple drinking water sources, which we find to be empirically important. Choe (1996) compares willingness to pay for reduced river and lake pollution in an urban Philippines setting with piped water, using both travel cost and CV methods, and finds that both are similarly low but this may not generalize to rural areas. Two other papers have compared averting expenditure data to stated willingness to pay (Griffin *et al.* 1995 and Rosado *et al.* 2006 in India and Brazil, respectively), though neither exploits experimental variation in water quality. Diamond and Hausman (1994) discuss the limitations of stated preference approaches to measuring the value of non-market goods.

consequences. We first find that 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 in rural Africa have not generally been displaced by private property rights. However, we estimate that as demand for clean water rises – for instance, at higher income levels – private property norms do yield higher social welfare than common property norms, potentially shedding light on the role that underlying economic conditions might play in the evolution of social norms and institutions.

We also show that an alternative "modified Lockean" norm under which spring owners can charge for protected spring water only if they also allow continued free access to unprotected water generates a Pareto improvement relative to existing communal property norms. Finally, public investment or a government-financed voucher system for spring users could approximate the solution for either a social planner who respects households' spring protection valuations or a paternalistic planner who places extra value on child health.

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

2. Rural Water Project (RWP) overview and data

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

2. 1 Spring protection in western Kenya

Spring protection is widely used in non-arid regions of Africa to improve water quality at existing spring sources (Mwami 1995, Lenehan and Martin 1997, UNEP 1998). Protection seals off the source of a naturally occurring spring and encases it in concrete so that water flows out from a pipe rather than seeping from the ground, where it is vulnerable to contamination when people dip vessels into the water to scoop out water and when runoff from the surrounding area introduces human or animal waste into the area. As 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 rural western Kenya. Approximately 43% of rural western Kenyan households use springs for drinking water and over 90% have access to springs (DHS 2003). Survey respondents in our study area 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 last week are to sources the respondents used for drinking water (as opposed to strictly for 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 also 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.[4] Spring water simply runs off if not collected. However, 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 non-governmental organization (NGO), International Child Support (ICS). As implemented by ICS, spring protection included installing fencing and drainage and organizing a user maintenance committee, in addition to the actual construction. Spring protection cost an average of US$1024 (s.d. $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 undertake routine maintenance, including simple patching of concrete, cleaning the catchment area, and clearing drainage ditches. These costs are roughly $35 per year, and are typically expected to be covered by local contributions, although free rider problems in collecting these funds are common.

2.2 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. NGO 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

[4] However, in another rural Kenyan region, local officials do sometimes permit landowners to charge for access to spring water once the landowners have invested in spring protection (personal communication with Scott Lee).

randomly selected (using a computer random number generator) to receive protection. Permission for protection was received from the spring landowner in all but two cases.

The NGO planned for the water quality improvement intervention to be phased in over four years due to their financial and administrative constraints. Although all springs were eventually protected, for our analysis the springs protected in round 1 (January-April 2005) and round 2 (August-November 2005) are called the treatment springs and those that were protected later are the comparison group.[5] To address concerns about seasonal variation in water quality and disease burden, all springs were stratified geographically and by treatment group and then randomly assigned to an activity "wave," and all project activities and data collection were conducted by wave.

Several springs were unexpectedly found to be unsuitable for protection after the baseline data collection and randomization had already occurred. These springs, which were found in both the treatment and comparison groups, were dropped from the sample, leaving 184 viable springs. Identification of the final sample of viable springs is not related to treatment assignment: when the NGO was first informed that some springs were seasonally dry, all 200 sample springs were re-visited to confirm their suitability for protection. In only 10 springs among the final sample of 184 viable springs did treatment assignment differ from actual treatment (for example because landowners refused to allow protection, or the government independently protected comparison springs); these springs are retained in the sample and we conduct an intention-to-treat analysis throughout. Table 1 presents baseline summary statistics for the treatment and comparison groups.[6]

A representative sample of households that regularly used each sample spring was selected at baseline. Survey enumerators interviewed users at each spring, asking their names as well as the names of other household users. Enumerators elicited additional information on spring users from the three to four households located nearest to the spring. Households that were named at least twice among all interviewed subjects were designated as "spring users". The number of household spring users varied from eight to 59 with a mean of 31. Seven to eight households per spring were 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 non-users were nonetheless retained in the analysis. The spring user list is reasonably representative of all households living near sample springs. In a census of all households living near nine sample springs that was conducted near the end of data collection, 71% of households living less than a 20 minute

[5] Appendix Figure 1 summarizes the project timeline.
[6] Additional details about the randomization into treatment groups are in Supplementary Appendix A.

walk from the source were included in the original spring users lists, with even higher rates of inclusion (77%) for those households less than a 10 minute walk from the spring.

2.3 Data collection

Water quality was measured at all sample springs and households using protocols based on those used at the U.S. Environmental Protection Agency. The water quality measure we use is contamination with *E. coli*, an indicator bacteria that is correlated with the presence of fecal matter. The household survey gathered baseline information about child diarrhea and anthropometrics, mothers' hygiene knowledge and behaviors (hand washing), household water collection and treatment behavior, and socioeconomic status. The target survey respondent was the mother of the youngest child living in the home compound (where extended families often co-reside), or another woman with childcare responsibilities if the mother of the youngest child was unavailable.[7]

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 round 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: 94% of baseline households were surveyed in at least two of the three follow-ups and 80% were surveyed in all three follow-up rounds. Attrition is not significantly related to spring protection assignment: the estimated coefficient on 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 and impacts on household water quality and health, the third follow-up survey for this subset of households is excluded from the analysis. This POU intervention is studied in other research (Kremer, Miguel, Null and Zwane 2008).

2.4 Baseline descriptive statistics

Table 1 presents baseline summary statistics for springs (Panel A), households (Panel B) and children under age three (Panel C). For completeness, we report statistics for all springs and households with baseline data (collected prior to randomization into treatment groups) even if they are dropped from

[7] Details on the data collection protocols are in Supplementary Appendix B.

the analysis because the spring was later found unsuitable for protection, although results are almost unchanged with the slightly smaller main sample (not shown). As expected with the randomization, 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, only about 5-6% of samples are of high quality, and the rest are poor quality. Household water is somewhat more likely to be high quality prior to spring protection in the treatment group (and the difference in means, though small, is significant at 95% confidence), but there is no significant difference in the proportion of moderate or poor quality water (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 on average at baseline: the average difference in ln *E. coli* is 0.51 (s.d. 2.63; results not shown). This likely occurs for at least two reasons. First, many households collect water from sources other than the sample spring: only half of the households get all their drinking water from their 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 last 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 last 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. [8]

[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. Of these, 0.4 and 2.8 are protected within 1 and 3 km, respectively, although some were protected long ago and are currently in a state of disrepair. 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).

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%) and unprotected springs (75%), shallow wells (73%), lakes/ponds (31%) and streams/rivers (14%). Yet the correlation between ln *E. coli* MPN/100ml levels at water sources and household perceptions of source water quality (on a 1 to 5 scale with 1=very clean and 5=very unclean) is just 0.12 (s.e. 0.02), though 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 actual spring contamination.

Most other household and child characteristics are similar across the treatment and comparison groups (Table 1, Panels B and C). Average mother's education is six years, less than primary school completion. Approximately four children under age 12 reside in the average compound. Water and sanitation access is fairly high compared to many other poor countries as about 86% of households report having a latrine, and the mean walking distance (one-way) to the closest local water source is 8 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.

3. Spring protection impacts on water quality and health

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

3.1 Estimation strategy

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

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

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 one after spring protection assignment, (i.e. for treatment group 1 in all follow-up survey rounds and for treatment group 2 in the second and third follow-ups, see Appendix Figure 1). 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 reduced-form 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 any time-varying factors affecting all groups, as are the variables used to balance the randomization into treatment groups (see Bruhn and McKenzie 2008 and discussion of randomization in Supplementary Appendix A). Estimates of the average treatment effect on the treated (TOT, Angrist, Imbens, and Rubin 1996) are very similar to the ITT estimates since assignment differs from actual treatment for few springs.

3.2 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 2, 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 water 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.[9] There is no statistically significant evidence of differential treatment effects by baseline hygiene knowledge (the average among local spring users), average local sanitation (latrine) coverage, or education (regression 3). Protected springs are rated by enumerators as having significantly clearer water (regression 4) but 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).

3.3 Home water quality effects

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

The average reduction in ln *E. coli* contamination at the home is -0.27, or roughly 24%, considerably smaller than the impacts on source water quality (Table 3, regression 1). For "sole source" households, those who used only water from their reference spring in the pre-treatment period, home water quality should be unambiguously better after treatment since they still rely

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[9] For evidence on mean reversion, note the downward slope of the non-parametric plot in Appendix Figure 2.

mainly on the spring and its quality improves after protection. For baseline "multi-source" water users in our data, who were roughly on the 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 multi-source households (regression 2), as predicted, but we cannot reject equal impacts for sole- and multi-source users.[10, 11]

Using the comparison households, we also non-experimentally estimated the relationship between the use of different water source types and household water quality. Conditional upon collecting 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* contamination (s.e. 0.18), or roughly 37% (not shown), substantially larger than the more reliable experimental estimates in Table 3. Other non-experimental approaches – 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 (results not shown).

We find no evidence of differential treatment effects as a function of household sanitation, diarrhea prevention knowledge, or mother's education (Table 3, regression 3). This runs counter to claims that source water quality improvements are much more valuable when sanitation access or hygiene knowledge are also in place, although the relatively large standard errors on these interaction

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[10] 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 IV framework in the sole-source users subsample, with assignment to protection as the IV for spring water quality. Conceptually, sole-source users could be useful for estimating the pass through 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 multi-source user status becomes less meaningful over time, making it infeasible to reliably estimate pass-through in this way.

[11] At baseline, 15.4% of comparison households get at least some of their drinking water from protected springs. In follow-up rounds, this percentage rises to 24.5%, but most of this increase is due to the secular increase over time in spring protection funded by donors or government: only 18.7% of comparison household trips to protected springs in the last follow-up survey round are identified as trips to our sample treatment springs. This non-compliance with treatment assignment is likely to somewhat reduce estimated protection impacts on diarrhea, and thus boost estimated valuation per case of diarrhea averted, although given the small fraction of trips that are to our treatment springs (24.5% x 18.7% $\approx$ 5% of all trips), the magnitude of any resulting bias appears unlikely to be large.

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 either 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 kilometers) on both spring and household water quality controlling for the total number of local springs (protected or not). For springs we find little evidence of externalities in water quality: the coefficient estimate on treated springs within 3 kilometers is small at -0.004 (s.e. 0.086), and similar results hold for springs at other distances (not shown). On the other hand, we cannot rule out some positive water quality spillovers for households: the coefficient on treatment springs located within 3 kilometers is -0.090 (s.e. 0.050, regression not shown). This is consistent either with some households switching to use nearby protected sources or with moderate spillover benefits to spring protection within the local area.

3.4 Child health and nutrition impacts

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

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

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. An advantage of this experimental design over existing studies, beyond the usual benefits of addressing omitted variable bias, is our ability to avoid measurement error and the associated attenuation bias in the key water quality explanatory variable through use of the treatment indicator. We include child fixed effects (α_i) and survey round and month fixed effects (α_t). We also explore heterogeneous treatment effects as a function of child and household characteristics, X_{ij}.

Spring protection leads to statistically significant reductions in diarrhea for children under age 3 at baseline or born since the baseline survey. In the simplest specification taking advantage of the experimental design, diarrhea incidence falls by -4.5 percentage points (standard error 1.2, Table 4, regression 1). In a probit specification the impact is similar, at -4.4 percentage points (s.e. 2.0, regression 2), and similarly in a linear specification with child, treatment group and survey month fixed effects (-4.5 percentage points, s.e. 2.3, p-value=0.06, regression 3). In our preferred specification with month and child fixed effects and child gender and age polynomial controls, the point estimate is -4.7 percentage points (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 significantly reduce diarrhea incidence. [12]

While the estimated reduction in diarrhea remains negative for boys, the effects are driven mainly by reduced diarrhea among girls (Table 4, regression 5). For girls the estimated reduction is 9.0 percentage points, an effect significant at 99% confidence. This finding is surprising since baseline diarrhea rates are similar for boys and girls in our sample, and differential gender impacts are rarely found in the related epidemiology literature; a decisive explanation remains elusive and further investigation is warranted.[13]

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

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 4, regressions 7-10). We do not find evidence of differential effects at points along the child weight and BMI distributions using quantile regression (not shown).

There is some suggestive evidence that spring protection produces a small reduction in diarrhea among children ages 5-12 as well. In the basic specification equivalent to regression 1 in Table 4, the point estimate is -0.017 (standard error 0.005, not shown), on a base diarrhea rate of 4.1 percent, though the effect is no longer significant when the full set of controls is included. There is

[12] Using children in comparison households in a non-experimental analysis using the same controls as in Table 4 (col. 3 and 4), we once again find that non-experimental and experimental estimates differ sharply. Households that choose to obtain water from protected springs do not have significantly lower diarrhea rates than other households: the coefficient on the fraction of water collection trips taken to protected springs is 0.007 (s.e. 0.041). Comparison households can also choose to obtain water from project treatment springs, partially complicating comparisons with our experimental estimates, although this effect should be small (as described above, under 5% of all trips by comparison households are to project treatment springs). Also, again using the sample of comparison household children, we find no evidence that water quality, as measured by ln *E. coli* MNP at either the household or the source, significantly impacts diarrhea. This may occur because water quality measures are noisy, leading to attenuation bias, and because quality is measured in the survey while diarrhea is for the week prior to the survey.

[13] Unlike Jayachandran and Kuziemko (2009), we do not find differential gender breastfeeding in our sample.

no evidence that spring protection improved school attendance in this age group, nor is there evidence of diarrhea impacts among adults (regressions not shown).[14]

3.5 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. Empirically, however, there were no significant changes in water handling or treatment behaviors (Table 5, Panel A) aside from the increased use of protected springs discussed below. 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 discussed earlier some of the implications of endogenous source choice for estimating household water quality impacts. Recall that each household in our dataset is linked to a particular spring (their "reference" spring) based on the baseline user list. The potential for differential impacts among sole-source users of this reference spring arises because protected spring use should increase more among multi-source users than sole-source users (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 multi-source users at baseline (Table 5, 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.

4. Valuing clean water

This section uses a travel cost model of water source choice to develop a revealed preference estimate of households' valuation of spring protection. We then argue that the more common stated preference approaches substantially overstate households' valuation of spring protection. Finally, we

[14] 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, although point estimates have the expected negative sign (estimated reduction -6.7 percent, s.e. 24.9 percent, not shown).

argue that households' valuation of a statistical life is smaller than typically assumed in public health expenditure cost-effectiveness analysis, but consistent with models such as Hall and Jones (2007) in which the income elasticity of demand for health is far greater than one.

4. 1 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 non-health attributes, such as the ease of water collection. Spring protection at source j in time t (T_{jt}) contributes an additional benefit β_i to household i's 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_iD_{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:

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

where e_{ijt} is an i.i.d. type I extreme value error term. Household i chooses source j over an alternative k if its benefits outweigh any travel costs, namely when $\beta_i(T_{jt} - T_{kt}) + (Z_j - Z_k) - C_i(D_{ij} - D_{ik}) + (e_{ijt} - e_{ikt}) \geq 0$. 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.[15]

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

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

[15] We follow most of the discrete choice literature in assuming a constant utility benefit from each additional trip to a water source. While declining marginal benefits from each additional trip to a particularly 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 annual household valuation of spring protection (from the mixed logit specification in Table 6 below) 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). We thank Pascaline Dupas for useful discussions on this issue.

The ratio of the coefficient estimate on the treatment (spring protection) indicator to the coefficient estimate on walking time to a source delivers the value of spring protection in terms of minutes spent walking. We 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 their health status, and household sanitation, hygiene knowledge, and education, by including interactions between these characteristics and the treatment indicator and the walking distance term.

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

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

where y, X, B and ρ are defined as above, and $f(\cdot)$ is the mixing distribution, which we take to be the normal distribution for the spring protection coefficient and the triangular distribution (constrained to be non-negative) for the distance coefficient. Bayesian numerical methods 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. 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 median respondent used two water sources in the last week, and 65% of respondents named available alternatives that they chose not to use.

4.2 Estimating willingness to pay for spring protection

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

One issue with the interpretation of this result is possible measurement error 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 below, although the correction estimated in a Monte Carlo simulation is similar at 0.3 (not shown).

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

The inclusion of terms for measured *E. Coli* contamination (available at a subset of alternative water sources) as well as the household's perception of water quality at each source reduces the coefficient estimate on the spring protection indicator to near zero (Table 6, regression 2), consistent with the possibility that households' greater valuation of protected springs is 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, while the coefficient estimate on the spring protection indicator falls by half, it remains positive and statistically significant, at 0.27 (s.e. 0.07, regression not shown). Taking these results together, it is difficult to definitively pin down 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).

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 five at baseline find additional walking distance to be more costly, as predicted, and the effect is especially large for households whose children had diarrhea at baseline: the estimate is large and significant at 99% confidence (Table 6, regression 3). The coefficient on the interaction between the treatment indicator and households with child under five who had diarrhea in the past week is also positive, although this result is difficult to interpret since households with sick children may also have different child health preferences.

Household valuations of spring protection rise with latrine ownership (perhaps reflecting underlying household taste for investing in health) and with mother's years of schooling (Table 6, regression 4). However, the choice of protected springs is not significantly affected by baseline respondent diarrhea prevention knowledge (in the household survey), or by expressing 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 (even though health gains appear concentrated among girls), and including higher order walking distance polynomial controls does not substantively change the results (not shown).

As further evidence on heterogeneous valuations, the mixed logit approach suggests considerable dispersion of spring protection valuations: the mean value of spring protection is 32.4 work days with a standard deviation of 102.8 work days (Table 6, regression 5 and Table 7, Panel A).

4.3 Comparing revealed vs. 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 described above.

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 while that on spring protection rises to 0.96 (Table 6, regression 6). Using the same attenuation bias correction for walking distance as above, 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 6, regression 7 and Table 7, Panel B). Comparing the analogous columns in Table 6 (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, while the spring protection estimate is more positive.

The second stated preference method is contingent valuation (CV). Households in protected spring communities were asked how much they would be willing to pay per year to keep their spring protected. The CV questions were only asked of households in the treatment group since they have first-hand experience with spring protection. In the final wave of the survey, respondents were first asked if they would be willing to pay either 250 or 500 Kenyan Shillings (US$7.14 or $14.29), followed by a question that emphasized the expenditure trade-off (in other words, the goods they would be giving up by spending that much on spring protection), and then were asked if they would be willing to pay the next higher amount, also with emphasis on the expenditure trade-off.[16]

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 7, 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 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 (standard deviation $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 contingent valuation subjects (surveyed after the round 3 follow-up survey) about their willingness to walk additional minutes to access a protected spring (versus an unprotected spring). As above, 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.[17] As we only had the detailed matched monetary and walking time

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

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

CV data for a subset of 104 respondents, we next regressed the estimated monetary value of water collectors' time on a detailed set of household characteristics (e.g., education, number of children, 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 day, or about 10% of the wage those carrying water would have earned for local agricultural labor.[18]

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 7, 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 1) indicate not only that stated preference methods exaggerate household willingness to pay for environmental amenities in a rural Kenyan setting, but that the revealed preference approach yields less variable valuations. One plausible explanation for the dispersion in stated preference methods is that many respondents fail to introspect carefully in hypothetical valuation exercises, and thus their resulting answers are far "noisier" than in the revealed preference case, where they face real time 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 to 50% of the average wage as a starting point (Train 1999). We thus also present revealed preference valuations using 25% of the average Kenyan wage or $0.35 per work day (in Table 7), but while 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 by construction, since both are scaled up by the same value of time.

4.4 Implications for health valuation

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

[18] While we use US$1 as the approximate local daily agricultural wage, this value likely overstates the value of time for multiple reasons, including the fact that workers often need to travel long distances to work, and that agricultural labor is not always available, being concentrated in certain peak seasons. For these reasons, the value of water collector time is plausibly more than about 10% of the local daily wage.

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.[19]

Using the household time values derived from our surveys, the bound on the value of averting one case of child diarrhea is a mere US$0.89 (=$0.088*10.1 working days), and on avoiding a child diarrhea death is $769 (=$0.088*8,742 working days). Using a standard conversion from diarrhea to disability-adjusted life years (DALYs), this 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 Kenyan wage) translates into $3,006 per averted child diarrhea death and $92.56 per DALY. For comparison, we estimate that the cost per DALY averted for this intervention is $16.75.

These value of life estimates are far below the estimated value of a statistical life in the U.S. and other rich countries (using hedonic labor market approaches), where the median value is approximately $7 million (Viscusi and Aldy 2003). Studies from two poorer countries (India and Taiwan) yield estimates on the order of $0.5-1 million per statistical life, although they are difficult to compare to our sample since they rely on data for urban factory workers, who are much richer than our rural respondents. Deaton *et al.* (2008) 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.

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 termed 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' GDP per capita, which for Kenya would be over $400, nearly twenty times higher than our preferred estimate. While an important source of

[19] There are 5.69 deaths per 1000 children under age five each year in sub-Saharan Africa (Lopez *et al.* 2006, Table 3B.7). With roughly 4.9 annual diarrhea episodes per African child under five (see Kirkwood, 1999), 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 tripled, the implied valuation of health and life would still fall far the below those typically used by health planners.

In contrast, 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 CRRA utility function curvature parameter (which might take on a value of 2), 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 paper, and thus we present a variety of approaches in section 5 below. We first present results following the conventional neoclassical economic approach of valuing lives according to households' own revealed preference measures. We then consider the case of a social planner with a higher valuation (whom we term "paternalistic", for convenience). This may be appropriate, for example, if the planner values averting child diarrhea deaths 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 protection's health benefits or if they are subject to time inconsistency problems.

5. Simulating alternative property rights norms and institutions

Under Kenyan law, local authorities can determine whether or not land owners have to allow neighbors access to water on their land, and authorities typically follow local social norms. In our study area, these norms 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 pre-colonial times, when there were no centralized kingdoms or chiefdoms in the area and the key local socio-political unit was the kinship clan (Were 1967, 1986). In the colonial and post-independence eras, administrative boundaries were typically set to at least roughly correspond to clan boundaries, with the region settled by a clan typically being a Kenyan administrative unit called a sublocation, and below (section 5.2) we thus consider springs within a sublocation as a natural unit for the analysis of property rights institutions. The typical rural Kenyan sublocation contains approximately four thousand inhabitants.[20]

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 throughout and instead consider the narrower question of what outcomes social norms would produce if they could be 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, since there may be significant enforcement and transactions costs not considered here, as well as other costs in the transition to a new institutional regime. Determining the most effective way of changing property rights norms remains a major topic in development economics but is beyond the scope of this paper.

To build intuition, we first consider the problem of a social planner deciding whether to protect an isolated spring in section 5.1, before moving on to the more realistic case of endogenous choice among multiple water sources in 5.2. In general, a planner's decision depends on whether other nearby springs are also protected, given households' ability to choose among sources within walking distance. Throughout we treat the marginal cost of providing spring water as zero since water flows out of the ground without a pump, user congestion is minimal, and unused water simply flows away.

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

Spring protection costs an average of $1,024 per spring in this area, with maintenance costs of $35 per year. Assuming that protected springs last for 15 years, this implies the discounted net present cost of spring protection is $1,405 (with a 5% annual discount rate). The total benefit of protecting a spring is the sum of willingness to pay for spring protection among current users. Based on the analysis above, we assume that mean household valuation is $2.96. Whether it is socially optimal to protect a particular spring depends critically on the number of users. The typical spring in our data is used by 31 households, so the total discounted net present valuation is $909. Under our assumptions, the revealed preference valuation of protection exceeds the cost of protection only for springs with 46 or more baseline household users, implying that a social planner taking household preferences as

[20] This estimate is derived from Kenyan census data: http://sedac.ciesin.columbia.edu/gpw/country.jsp?iso=KEN.

given would only protect springs in relatively densely populated areas, or in areas with few springs (where each has many users), and that protection will become increasingly attractive as local population grows. However, a paternalistic planner who valued averting a DALY at \$125, a valuation often used by health planners in poor countries, would have a much lower threshold number of users, at just 10 households. Overall, if all springs in our sample were isolated, the paternalistic planner would find it socially optimal to protect 97% of sample springs, whereas using the revealed preference valuations only the 15% of springs with the largest number of household users are protected. As we show in the next section, a paternalistic social planner optimally protects far fewer springs when taking into account households' ability to walk to other nearby sources.

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

We now consider the impact of alternative water property rights social norms and institutions 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. 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 zero. Charging positive water prices can thus lead households to choose springs that would be less preferred based on walking time and water contamination, the factors that are socially efficient for them to consider.

Consider a partition of nearby springs into contiguous subgroups, such as the springs in the territory of a clan, which is typically a subset of a Kenyan administrative sublocation. We consider each subgroup separately and then aggregate across subgroups for overall welfare outcomes. The term $j \in \{1, \ldots, J\}$ denotes the springs within each subgroup; and $i \in \{1, \ldots, I\}$ refers to households that choose among these sources. Household demand parameters are derived from the revealed preference mixed logit results (in Table 6, column 5). The utility value of spring protection for household i (with reference spring j) is β_{ij}, γ_{ij} is the disutility from an additional minute of walking time, and δ_{ij} is the monetary value of a minute of water collector time, from the survey method described above; in the notation of equation 3, the disutility of time in monetary terms $C_{ij} = \gamma_{ij} * \delta_{ij}$. We mainly focus on these monetary values below but also discuss robustness to other assumptions.

A household's preferences can be represented by $\theta_{ij} = \{\beta_{ij}, \gamma_{ij}, \delta_{ij}\}$, where $F(\theta_{ij})$ is their joint distribution among all households in the population. These parameter estimates are available for each household from the mixed logit estimation, and allow us to compute expected household utility per water collection trip (from equation 3) to a particular source w, denoted u_{wij}, as well as the total trips households choose to make to each of their alternative water sources in a full year, N_{wij}. Annual household utility (in monetary terms) for household i from water consumption is $V_{ij} = \sum_{w}(N_{wij} * u_{wij})$ minus any monetary costs incurred purchasing the water. Empirically, recall that the total quantity of household water collected varies neither with spring protection status nor with distance to the reference spring, allowing us to simplify the problem by assuming that the total number of household water trips is fixed ($\sum_{w} N_{wij} = \overline{N}_{ij}$ for household i regardless of their choices among sources $w \in \{1, .., W\}$). Recall also that since households can obtain water from other sources, such as wells or streams, springs (subscripted j) are a subset of all sources (subscripted w).

We simulate the following game in the private property cases. At t=0, the property rights regime is chosen. At t=1 profit-maximizing spring owners within a subgroup simultaneously decide whether to protect their spring, where protection is denoted by the indicator variable $protect_j$. At t=2 spring owners simultaneously set prices p_j per unit of water. The vector of protection and price decisions for all springs are denoted *protect* and p, respectively. We assume that spring owners set prices with full knowledge of each household's water source choice situation and preferences, including the distance to each of the household's options and θ_{ij}. At t=3 households choose water sources to maximize utility given protection decisions and prices.[21] We solve the model backwards.

Households can choose to obtain water from their reference spring, any other sources (such as public boreholes, streams and rivers) they listed in the household survey when asked about potential alternative drinking water sources, plus other local springs within their spring subgroup. We generally considered springs located within 1 km 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. We consider subgroups of up to four contiguous springs within the same administrative sublocation. Analyzing the full interdependence of spring protection decisions in the full sample of 167 sample springs with 2^{167} possible protection combinations would be computationally impossible, and in any

[21] We ignore any water consumption utility gains for spring owners since they would not necessarily live locally or consume spring water. We also incorporate demand from new household users post-protection using information from a household user census, as described in the Simulation Appendix. 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. 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 instance, we find in the census that many new users live a greater distance from the spring than baseline users. For a useful approximation of the welfare gains for this group, we assume in the simulation that their consumer surplus is uniformly distributed between zero and the valuation of the baseline user household that lives farthest from each spring (in our data), as might be the case if households live at a continuum of distances from the spring but their underlying taste for clean water is otherwise the same.

case subgroups of approximately the size we use seem to correspond more closely to the relevant social unit in both the pre-colonial period and under current Kenyan law, which delegates power over water issues to local authorities. In sublocations where the number of springs is not divisible by four, we created the largest possible groups (e.g., a location with seven springs was generally divided into one group of four springs and one of three springs) unless a spring was located far from any other spring (generally more than 1 km), in which case it was treated as isolated. The solution with groups of up to four nearby springs is only an approximation to the full solution but we believe it is a fairly close approximation: we have also run similar analyses with groups of one spring and two springs, in addition to the four spring case we focus on below, and, while prices charged by private spring owners do fall as the degree of competition increases, they fall at a declining rate and the social welfare rankings of alternative property rights institutions are unchanged (not shown).

Spring owners' profits are equivalent to the net present value of revenues, minus any spring protection construction and maintenance costs (since the marginal cost of providing spring water is zero) over a 15 year time horizon, and these costs are estimated at \$1,405 and denoted *K*. The fact that we have data on the actual local cost of the spring protection technology greatly simplifies the solution of the spring owner's problem. We find optimal spring owner prices conditional on protection status either through a grid search (when computationally feasible) or the numerical Nelder-Mead simplex method based on the profit functions' first order conditions described below.[22] 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/non-protection combinations (at most $2^4 = 16$ in subgroups of four springs) and search for a fixed point.

We consider the impact of changing property rights norms on spring investments and pricing outcomes, holding constant policies for other water sources. This approach is realistic in the rural Kenyan setting, where there is typically open access through public paths to naturally-occurring rivers and lakes and most boreholes are sunk on public property such as schools or market centers so people can collect water for free at these places. We assume this would continue to be possible.

We normalize social welfare to zero in the benchmark "status quo" case with common property rights to water and no spring protection (Table 8, row 1 in panels A and B). We express household utility and social welfare in U.S. dollar values on a *per spring* basis. We generally do not present results on a per capita basis since the number of users can change with protection, but for a

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[22] Further details on the solution methods are in the supplementary Simulation Appendix available upon request.

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.

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. We also evaluate various property rights institutions from the perspective of each type of social planner. The decision problem of a social planner maximizing revealed preference can be represented as follows, where W^S denotes social welfare:

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

and $V_{ij}(protect \mid \theta_{ij})$ denotes household utility given that the planner is fully informed about true household preference parameters, θ_{ij}. In particular, given that households' average revealed preference valuation of health is equivalent to \$23.68 per DALY averted, we assume the paternalistic planner increases the utility value (β_{ij}) placed on spring protection approximately five-fold when computing social welfare and making spring protection choices. We assume both the neoclassical and paternalistic social planners are constrained to allow households to make their own water collection choices based on revealed preference. (In section 5.4 below we also consider a third type, an *unconstrained paternalistic* social planner who also determines household water collection choices based on a welfare function that values each DALY averted from spring protection at \$125.)

We next consider four forms of property rights – common property (the status quo described above), "freehold" private property, Lockean private property, and modified Lockean (all defined below) – and then consider two non-budget balancing mechanisms, public provision and vouchers. We analyze each from the standpoint of both neoclassical and paternalistic social planners.

In freehold private property social norms, there are no restrictions on spring owners' behavior. Private property rights are somewhat analogous to patents in this analysis. Creating private property rights may spur potentially productive investments but induce static distortions since spring owners have some market power in setting prices, given the travel costs households face in water collection. To model this tradeoff, we assume spring owners set prices and make protection decisions to maximize profits taking the decisions of neighboring spring owners as given, and we solve for the Nash equilibrium. The owner of spring j maximizes

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

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

The simulations were run ten times, each with an independent draw of household preference parameters (from mixed logit), and the results presented below are the average of the ten runs.[23]

5.3 Social Planner and Property Rights Simulation Results

The neoclassical social planner respecting households' revealed preference valuations protects 29% of springs, typically those with many baseline household users (Table 8, panel A, row 2). The net social gain across all springs (protected and unprotected) is \$351 per spring, or roughly \$1.75 per capita. A paternalistic social planner valuing a DALY averted by spring protection at \$125 would protect 63% of springs (Panel B, row 2). This is fewer than the 97% figure cited in section 5.1 for isolated springs, since households can choose among multiple local springs in this case.

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

Freehold private property rights substantially reduce social welfare as measured by household revealed preference relative to the status quo of communal property rights, with a social welfare loss of \$91 per spring. This finding may be a partial explanation why communal water rights have been so durable in African settings like ours: they may simply yield higher social welfare than

[23] A simulation run takes two days, so for practical reasons we use 10 runs (results from each available on request).

private property rights. The logic is straightforward: the dynamic gains from spring protection are small since nearly all springs remain unprotected, but there are large static losses from households walking further or choosing dirtier sources to avoid paying for water from springs located on private land. The proportion of household trips to rivers and streams, the dirtiest local sources, increases by 45% relative to common property rights, 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 by over a minute, from 11.6 to 12.9 minutes, and this amounts to over 100 extra hours per household each year.[24]

Freehold property rights lead to both under- and over- protection. On the one hand, only 13% of the springs that would be protected by a neoclassical social planner are protected under private property, but on the other hand some springs this social planner would not protect are protected under private property. The inability of spring owners to capture the 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, since land owners do not consider the negative impact of spring protection on owners of competing nearby springs. The tendency of private owners with market power to focus on the marginal consumer's valuation of spring protection, rather than that of the average consumer, could lead them to under- or over-protect springs depending on local demand patterns (Tirole 1988).

We do not present paternalistic social planner welfare outcomes for the freehold (or Lockean) private property cases in Table 8, Panel B since they are not directly comparable to the communal property outcomes. This is a product of the way we model the paternalistic planner's social welfare function in panel B: the planner places additional value on *spring protection* (essentially scaling up the β_{ij} coefficient five-fold), but not on health gains from using other clean sources; a richer simulation that incorporates the health impacts from the full range of sources awaits future research. Yet a brief examination of panel B indicates that communal property will almost certainly continue to yield higher social welfare in this case: while freehold private property leads to some spring protection, it continues to generate increased average *E. coli* water contamination (as families turn from relatively safe springs to more contaminated sources like rivers and streams due to high user fees), and water collection walking times also rise considerably relative to common property.

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

It is also worth considering other private property rights institutions beyond the stylized extremes of common property and freehold private property, since actual social norms are often more complex. Locke (1689 [2002]) argued that people acquire property rights in land when they mix their labor with it, for example by clearing land or planting a crop. This element of property rights is common in rural Africa and elsewhere. For example, in Ghana actively farming a plot is critical to securing property rights (Goldstein and Udry 2005) and clearing land is traditionally necessary for establishing rights to plots at the margin of Amazon tropical forests. A "Lockean" private property norm in our context would only permit spring owners to charge positive prices if they had invested in spring protection. In the model, the spring owner's profit maximizing condition is similar but now the price is constrained to be $p_j=0$ if $protect_j=0$.

Historically, some legal systems seem to have evolved from common property for water towards 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 1901: 316, Wanasharisi 1909: 285). Lockean norms may also be emerging in another Kenyan region: local officials in Nyanza province do sometimes permit spring owners to charge for access to spring water once they have invested in spring protection (personal communication with Scott Lee).

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. In practice, there is substantial under-protection (Table 8, panel A, row 4). The simulations suggest Lockean private property rights yield somewhat higher investment in water infrastructure than freehold private property: 12% of owners now find it profit maximizing to invest in spring protection. Although social welfare (using revealed preference valuation) remains lower than the status quo, Lockean rights are marginally better for welfare than freehold private property: the average annual social welfare loss per community is only \$43, average fecal contamination increases by just 4 log points and average walking time rises slightly, from 11.6 to 12.3 minutes. Yet the increase in both fecal contamination and walking time suggests that a paternalistic planner would also likely prefer common property to Lockean property norms.[25]

[25] It is possible that disaggregating trips by water use (something we are unfortunately unable to do with our data) could partially affect the conclusion, since households would have the option of purchasing some drinking water at springs and collecting water for other uses elsewhere. We leave this for future research.

A “modified Lockean” private property rights regime would permit spring owners to charge for water from spring protection infrastructure as long as they also allowed free unimpeded access to unprotected water from the spring. In our setting, a system of modified Lockean property rights could be achieved simply by requiring owners who protect springs to allow some water to flow out of the pipe and away from the protected spring, where it becomes a pool of unprotected spring water exposed to the environment that anyone can access for free. In this case, the spring owner’s profit maximizing condition is equivalent to that under the Lockean case, with the critical change that under protection the spring owner must now take into account that households have the choice of continuing to consume unprotected spring water at price $p_j=0$ at the source. This regime generates a Pareto improvement over the common property status quo, since the availability of free unprotected spring water shields households from the utility losses experienced under the other private property cases, and spring owners cannot be worse off than under common property.

This regime performs best within the set of set of budget-neutral social norms. Under modified Lockean property rights, 2% of springs are protected (Table 8, panel A, row 5). Six percent of the springs a neoclassical social planner would protect are protected in this case, while only 1% of springs the neoclassical planner would not protect are protected (not shown). While this form of property rights is far from attaining the social optimum, it incentivizes spring owners to perform some socially beneficial spring 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 8, they are in fact lower by 0.002 under the modified Lockean system. Yet these social welfare gains from modified Lockean rights are sufficiently small that it is perhaps unsurprising that there has been limited institutional innovation along these lines in Kenya, given the transition costs of changing property norms, enforcing charges for protected sources, preventing owners from impeding access to unimproved water, and any engineering costs of allowing spring water to freely run off.

Public funding, either through direct government provision or vouchers, performs much better than the budget-neutral property rights norms considered above. Consider the impact of public funding by a hypothetical benevolent government that had access only to distortionary taxation and knows only the distribution of preferences in the population, $F(\theta_{ij})$, not individual household preferences in each community. Consider first a policymaker that maximizes household revealed preference welfare. This policymaker’s optimization problem is the following, where DW denotes the deadweight loss of taxation per dollar of revenue raised:

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

Assuming that DW=0.3 (Ballard *et al.* 1985)[26], the government protects 22% of springs (Table 8, panel A, row 6) but there is some misallocation of protection: 49% of those springs the neoclassical social planner would optimally protect get protected, and 12% of those the planner would not protect (not shown). The welfare gain per spring is \$131, less than the \$351 gain attained by a social planner, due to the tax distortion and the misallocation of protection across springs due to policymakers' limited information on households' preferences. A paternalistic government that values health gains from protection at \$125/DALY would protect 67% of springs (panel B, row 6), coming much closer to the corresponding social planner than any of the budget-neutral approaches.

Finally, consider a regime where the government does not protect springs directly but rather provides households with vouchers for protected spring water which they can pay to spring owners, and which spring owners can then exchange for a fixed payment from the government. The government optimally sets the voucher payment taking into account the later non-cooperative protection decisions of private spring owners. We assume that the policymaker only knows the distribution of water preferences in the entire population, but that spring owners still have perfect knowledge of local preferences. Spring owners are assumed to be restricted from charging top-up fees to water users, so households' ex post decisions on which water sources to use are efficient. The policymaker maximizes social welfare W^V by setting the uniform voucher price p_v:

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

subject to spring owner protection decisions being profit maximizing:

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

where spring owner profits are given by $\pi_j = p_v \sum_{j'=1}^{J'}\sum_{i=1}^{I}\left\{N_{ijj'}(protect_j, protect_{-j}^{*}, 0) \mid \theta_{ij}\right\} - K * protect_j$.

For a government that respects household revealed preferences, and again faces 30% deadweight loss, the optimal voucher price is \$0.0010 per trip to a protected spring (much less than the price charged in the freehold private property rights case), and social welfare gains per spring under water vouchers are \$138 (or roughly \$0.69 per capita). 12% of spring owners protect their springs (Table 8, panel A, row 7), still short of the social optimum, but there is less misallocation of protection in this case, since spring owners with good information on local demand are the ones

[26] The main results are unchanged under the assumption that DW=0.5 (results not shown).

making protection decisions: 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 all the budget-neutral cases, and social welfare is comparable to government investment.

A paternalistic policymaker with health valuation at $125/DALY sets a uniform voucher price of $0.0041 (Table 8, panel B, row 7), four times higher than the previous price level, and as a result 65% of spring owners choose protection. For the paternalistic planner, we estimate slightly higher spring protection and social welfare under government provision than under vouchers. We conjecture that this is, in part, because raising the voucher price to increase protection above the 65% level associated with vouchers does not effectively target those springs a social planner would protect but instead leads to increased protection of other springs due to rent-stealing effects (recall that spring owners make protection decisions in the voucher case). However, both government cases perform much better than any budget-neutral property rights system under both the neoclassical social planner and the paternalistic planner.

5.4 The Determinants of Optimal Property Rights Institutions

If households' spring protection valuation increased by 20%, freehold private property rights and Lockean private property rights norms would both become preferable to common property norms (not shown, available on request). If these valuations increase with income, and the income elasticity of demand for health is greater than one (as in Hall and Jones 2007), this implies that per capita income growth of less than 20% in this region could make private property rights more attractive than the current norm of communal access. If this finding that private property rights are optimal at higher income levels holds more broadly, it could help explain the strong cross-sectional relationship documented between institutions and income levels, and suggests it would be risky to assume that causality always runs from stronger property rights protection to higher income levels (North 1990).

The social welfare ordering of the public cases (government investment and vouchers) versus private property rights cases is remarkably robust to alternative assumptions about households' monetary valuation of spring protection (and equivalently, the value of water collectors' time). In results not presented in Table 8, public investment and vouchers both yield far higher social welfare than any of the private property rights cases when household valuation for spring protection is 2, 3, 4, or 5 times larger than the revealed preference estimates. Note that the evaluation of social welfare by a planner in this exercise is equivalent to the case of the *unconstrained paternalistic* social planner mentioned above, in which the planner not only makes investment and pricing decisions but also determines household water collection decisions.

Yet for household spring protection valuations sufficiently close to zero (such that zero protection investment is socially optimal), it is straightforward to show that communal property rights yield social welfare at least as high as all other property rights norms, including the government cases. It thus seems likely that communal property norms for water were in fact socially optimal when they emerged in pre-colonial Kenya, a historical period when income was 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.

Finally, the analysis above abstracts from transaction costs in collecting water user fees, but these are likely to be large under private property norms. In particular, it may be expensive for spring owners to monitor the amount of water that households collect, and these transaction costs would reduce social welfare benefits. More generally, there are likely to be non-trivial transition costs in moving from any one system to another, and these may be particularly large for water privatization that runs against traditional communal social norms, as in rural Kenya. Certain cases may also have real-world advantages that are not modeled here, especially with regards to ongoing incentives for spring maintenance over time. For instance, an inept or corrupt government's investment program may collapse after a short time, in which case a voucher system (where maintenance remains with private spring owners) or the modified Lockean case could be preferable.

6. 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 disability adjusted life year (DALY), roughly one fifth of the valuations typically used by public health planners. The estimated valuations are consistent with models such as Hall and Jones (2007) in which the elasticity of demand for health is much greater than one.

Using structural econometric methods in tandem with the spring protection experiment, we carry out counterfactual simulations based on estimated household revealed preference valuations for spring protection, 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 persisted in this rural African region. Simulations suggest that private property becomes more attractive than communal norms when health valuations reach a sufficient level, which seems like to occur as incomes rise. We also conjecture (as argued in Platteau 2000) that higher population densities, or other factors that affect water scarcity would also boost local willingness to pay for water and thus the attractiveness of freehold private property rights relative to communal property norms. Water scarcity is an increasingly pressing issue in India, for instance, where expanding irrigation and urban water use is leading to rapid drops in the water table.

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 may also be delayed in practice if political institutions make it impossible 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 paper suggests their approach is reasonable given current technology and household preferences.

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Table 1: Baseline descriptive statistics (2004 survey)

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

	Treatment (protected) Mean (sd)	Obs	Comparison Mean (sd)	Obs	Treatment – Comparison (se)
Respondent years of education	5.71	731	5.66	717	-0.05
	(3.61)		(3.60)		(0.23)
Children under age 12 in the compound	4.04	736	3.93	719	0.11
	(2.48)		(2.46)		(0.14)
Iron roof indicator	0.70	735	0.68	717	0.03
	(0.46)		(0.47)		(0.03)
Walking distance to closest water source (minutes)	8.74	725	8.03	714	0.71
	(8.40)		(6.82)		(0.49)
Water collection trips per week by household	48.03	733	47.99	716	0.04
	(36.51)		(38.48)		(2.51)
Ever collects drinking water at “reference” spring indicator	0.82	661	0.80	668	0.02
	(0.38)		(0.40)		(0.03)
Multi source 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)
Water storage container in home was covered	0.90	673	0.93	656	-0.03
	(0.30)		(0.26)		(0.02)**
Yesterday's drinking water was boiled indicator	0.25	731	0.29	711	-0.03
	(0.43)		(0.45)		(0.02)
Respondent diarrhea prevention knowledge score	3.06	736	3.19	719	-0.13
	(2.14)		(2.26)		(0.15)
Respondent said “dirty water” causes diarrhea	0.68	736	0.67	719	0.01
	(0.47)		(0.47)		(0.03)
Household has soap in the home	0.91	733	0.91	717	0.00
	(0.28)		(0.29)		(0.02)
Panel C: Child demographics and health					
Child age (years)	1.70	1047	1.72	995	-0.02
	(0.95)		(0.97)		(0.04)
Child male (=1)	0.52	1047	0.53	995	-0.01
	(0.50)		(0.50)		(0.02)
Child had diarrhea in past week indicator	0.23	996	0.20	961	0.03
	(0.42)		(0.40)		(0.02)
Child height (cm)	76.10	870	76.13	835	-0.03
	(11.67)		(12.16)		(0.57)
Child weight (kg)	9.98	864	10.02	810	-0.05
	(3.04)		(3.09)		(0.16)

Notes: The treatment springs were later protected (in 2005). Huber-White robust standard errors are clustered at spring level when using household level data, significant at * 90% ** 95% *** 99% confidence. Reference spring is based on spring user lists. Children in Panel C were under age 3 at baseline or were born since then.

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

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

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

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

	Dependent variable: ln(Home water *E. coli* MPN)		
	(1)	(2)	(3)
Treatment (protected) indicator	-0.27	-0.29	-0.67
	(0.15)*	(0.19)	(0.27)**
Baseline ln(Spring water *E. coli* MPN)	0.01	0.03	0.035
	(0.05)	(0.05)	(0.05)
Baseline multi-source user		-0.29	-0.27
		(0.16)*	(0.17)
Baseline multi-source 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 (2.27)	3.00 (2.27)	3.00 (2.27)

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

Additional control variables are: season fixed effects, number of children under 12 living in the home, home has iron roof indicator, iron roof density within spring community. When differential treatment effects are reported in column 3, we also include interactions 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.

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

The -0.27 effect in column 1 is equivalent to a 24% reduction in *E. Coli* fecal coliform units per 100ml.

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

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

Notes: Column 2 estimated using probit (marginal effects presented), columns 1 and 3-10 estimated using OLS. Huber-White robust standard errors (clustered at the spring level) are presented, significantly different than zero at * 90% ** 95% *** 99% confidence. Data from all four survey rounds (2004, 2005, 2006, 2007), sample restricted to children under age three at baseline (in 2004) and children born since 2004. Diarrhea defined as three or more "looser than normal" stools within 24 hours at any time in the past week. The gender-age controls include linear and quadratic current age (by month), and these terms interacted with a gender indicator. Columns 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.

Table 5: 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 Multi-source users (3)		Mean (s.d.) comparison group in 2006, 2007 surveys (4)	
Panel 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)
Panel B: Sanitation and hygiene behaviors								
Diarrhea prevention knowledge score	0.14	(0.14)	0.21	(0.18)	0.04	(0.19)	3.92	(2.07)
Respondent says drinking clean water is a way to prevent diarrhea	-0.03	(0.03)	-0.03	(0.04)	-0.04	(0.04)	0.73	(0.44)
Household has soap in the home indicator	-0.01	(0.02)	-0.02	(0.02)	0.01	(0.03)	0.89	(0.31)
Fingers with bacterial contamination (fecal *Streptococci* colonies) [(d)]	0.10	(0.12)	0.41	$(0.23)^{*}$	0.11	(0.21)	0.71	(1.26)
Panel C: Water collection and source choice								
Fraction of trips to 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 zero at * 90% ** 95% *** 99% confidence. Reported means of the dependent variables are in the comparison group 2006 and 2007 (rounds 2 & 3 post-treatment) surveys. 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 0-5).

(a): Because of changes in survey design, responses to this question are not available for the third (2006) round of data collection.

(b): Because of changes in survey design, responses to this question are not available for the first (2004) round of data collection.

(c): Because of changes in survey design, responses to this question are not available for the fourth (2007) round of data collection.

(d): Because information on fingertip contamination was collected only in the third (2006) round of data collection, this cell reports the difference between the treatment and comparison groups rather than the differences-in-differences treatment effect.

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

	Revealed Preference					Stated Ranking	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment (protected) indicator	0.51***	-0.02	0.34***	0.68***		0.96***	
	(0.04)	(0.08)	(0.08)	(0.09)		(0.24)	
Mixed logit – Mean (normal):					2.95***		1.46**
					(0.25)		(0.60)
Mixed logit – Std. dev. (normal):					5.73***		1.22
					(0.33)		(0.75)
ln (source water E. coli MPN)		-0.14***					
		(0.01)					
Water quality at source perceived to be above average		1.14***					
		(0.07)					
Distance to water source (minutes walking)	-0.055***	-0.059***	-0.031***	-0.053***		-0.033***	
	(0.001)	(0.002)	(0.003)	(0.002)		(0.010)	
Mixed logit – Mean (restricted triangular):					-0.21***		-0.03***
					(0.01)		(0.01)
Mixed logit – Std. dev (restricted triangular)					0.09		0.001
Distance * Children aged 0-5 with diarrhea last week			-0.006***				
			(0.001)				
Treatment indicator * Children aged 0-5 with diarrhea last week			0.09***				
			(0.03)				
Treatment indicator * Baseline latrine ownership				1.80***			
				(0.25)			
Treatment indicator * Baseline diarrhea prevention score				0.023			
				(0.020)			
Treatment indicator * Baseline mother's years of education				0.057***			
				(0.011)			
Source type: Borehole/piped	-0.08		-0.05	-0.13*	-1.02***	0.07	0.04
	(0.05)		(0.07)	(0.08)	(0.14)	(0.25)	(0.27)
Source type: Well	-0.28***		-0.35***	-0.31***	-1.87***	-0.43*	-0.47*
	(0.05)		(0.07)	(0.07)	(0.13)	(0.24)	(0.25)
Source type: Stream/river	-0.77***		-0.71***	-0.63***	-1.46***	-2.19***	-2.25***
	(0.06)		(0.09)	(0.09)	(0.15)	(0.52)	(0.53)
Source type: Lake/pond	-0.20		-0.20	-0.18	-0.32	-2.82	-2.85
	(0.14)		(0.20)	(0.19)	(0.35)	(1.86)	(1.87)
Number of observations (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 *** 99% confidence. In columns 1-5 each observation is a unique household-water source pair in one water collection trip. In columns 6-7, each observation is a household-water source pair from questions where the respondent chooses their preferred source. The dependent variable is an indicator equaling 1 if the household chose the water source represented in the household-source pair. The omitted water source category is “non-program spring”. The coefficient estimate on the indicator for the household’s reference sample spring is included in the analysis but not shown in the table. In column 3, additional controls are children aged 0-5 and 5-12 at baseline, directly and 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.

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

Panel A: Revealed preference valuation (from mixed logit – Table 6, column 5)	One year of spring protection	
	Mean	Std. dev.
Work days (8 hour days)	32.4 days	102.8 days
Assume value of time is 25% Kenyan worker average wage	\$11.57	\$36.69
Time value from survey questions (time and monetary value)	\$2.96	\$11.13
Panel B: Stated preference ranking valuation (from mixed logit – Table 6, column 7)		
Work days (8 hour days)	56.2 days	12.3 days
Assume value of time is 25% Kenyan worker average wage	\$20.06	\$4.38
Time value from survey questions (time and monetary value)	\$4.96	\$1.97
Panel C: Contingent Valuation	Full Round	Final Wave, emphasizing trade-offs
Proportion willing to pay this for spring protection:		
US\$3.57 (250 Kenya Shillings)	0.94 [308]	0.80 [98]
US\$7.14 (500 Kenya Shillings)	0.90 [316]	0.79 [204]
US\$14.29 (1000 Kenya Shillings)	-	0.60 [204]
	One year of spring protection	
	Mean	Std. dev.
Sample: Final Wave, emphasizing trade-offs	\$17.64	\$13.09
Subsample with 250 KSH starting value	\$12.62	\$11.06
Subsample with 500 KSH starting value	\$23.91	\$14.28

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

Table 8: 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
Panel A: Revealed preference valuation, Neoclassical planner							
(1) Communal property (status quo)	0	0	0	0	11.6	4.66	0
(2) Social planner	29	0	0.	726	12.5	4.45	351
(3) Freehold private property	5	0.0027	420	-511	12.9	4.92	-91
(4) Lockean private property	12	0.0069	84	-127	12.3	4.70	-43
(5) Modified Lockean property	2	0.0058	8	26	11.7	4.66	35
(6) Public investment	22	0	0	510	12.4	4.48	131
(7) Public vouchers	12	0.0010	39	348	12.1	4.56	138
Panel B: Paternalistic social planner							
(1) Communal property (status quo)	0	0	0	0	11.6	4.66	0
(2) Social planner	67	0	0	3,586	12.9	4.32	5,138
(3) Freehold private property	5	0.0027	420	-511	12.9	4.92	--[a]
(4) Lockean private property	12	0.0069	84	-127	12.3	4.70	--[a]
(5) Modified Lockean property	2	0.0058	8	26	11.7	4.66	366
(6) Public investment	67	0	0	944	12.8	4.33	4,724
(7) Public vouchers	65	0.0041	1,160	891	12.4	4.40	3,915

Notes: [a] Social welfare in the freehold and Lockean cases in panel B cannot be reliably compared to the communal property case, as described in section 5.3. Profits, utility and welfare are net present values (5% annual discount over 15 years). Household spring protection valuations are from Table 6, 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:

(1) Communal property rights: The price of spring water is zero. No springs are protected. Social welfare is normalized to zero.

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

(3) Freehold private property rights: Spring owners simultaneously choose whether to protect springs and then simultaneously choose price per unit of water non-cooperatively (in groups of up to four). Spring owners know preferences θ_{ij} for each household.

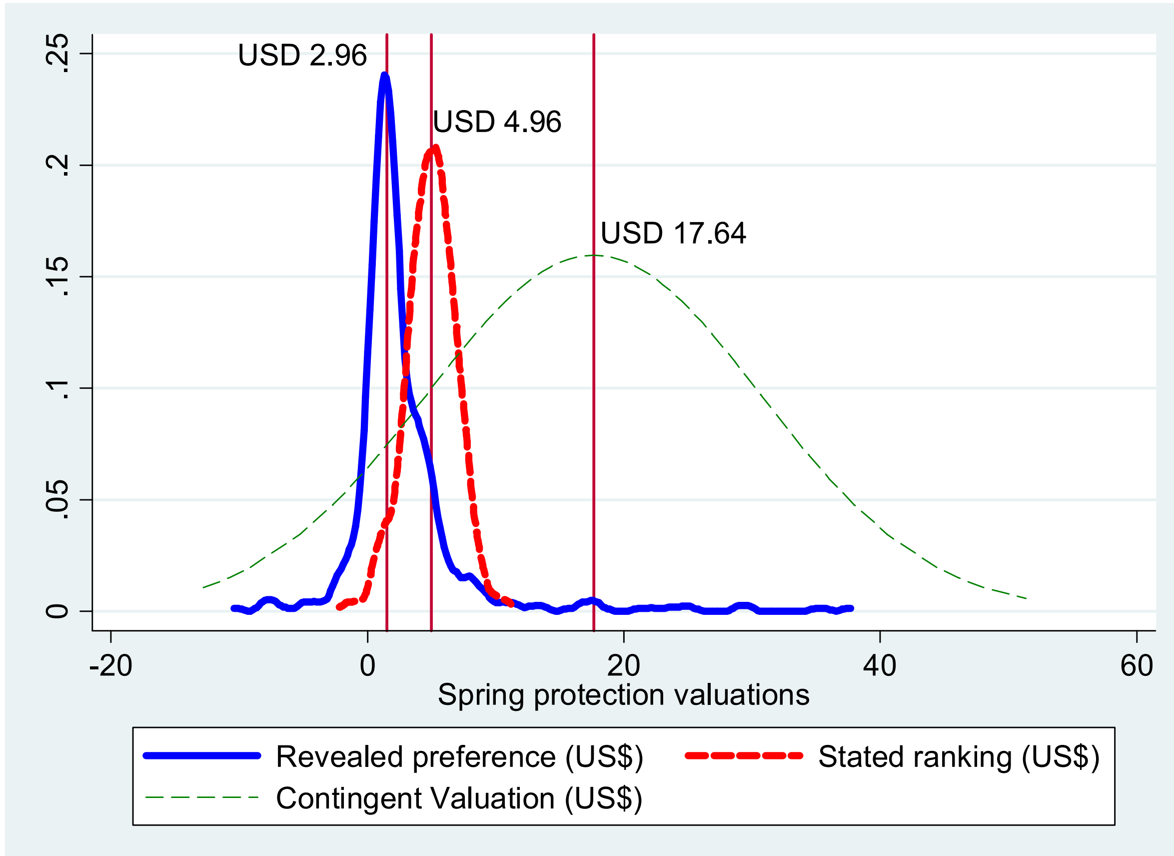
(4) Lockean private property rights: Same as the freehold private case except the price of unprotected spring water is constrained to be zero.

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

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

(7) Vouchers: The policymaker 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 non-cooperative play. There is 30% deadweight loss to funding the vouchers. The policymaker knows the distribution of preferences $F(\theta_{ij})$ in the population; spring owners know preferences θ_{ij} for each household.

Figure 1: Household revealed preference and stated preference valuations of one year of spring protection (2007)



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

Supplemental Online Appendix (not intended for publication)

Appendix A: Randomization procedures

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

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

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

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

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

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

Water quality

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

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

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

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

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

Child diarrhea

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

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

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

Contingent valuation surveys
The exact contingent valuation question wording was:

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

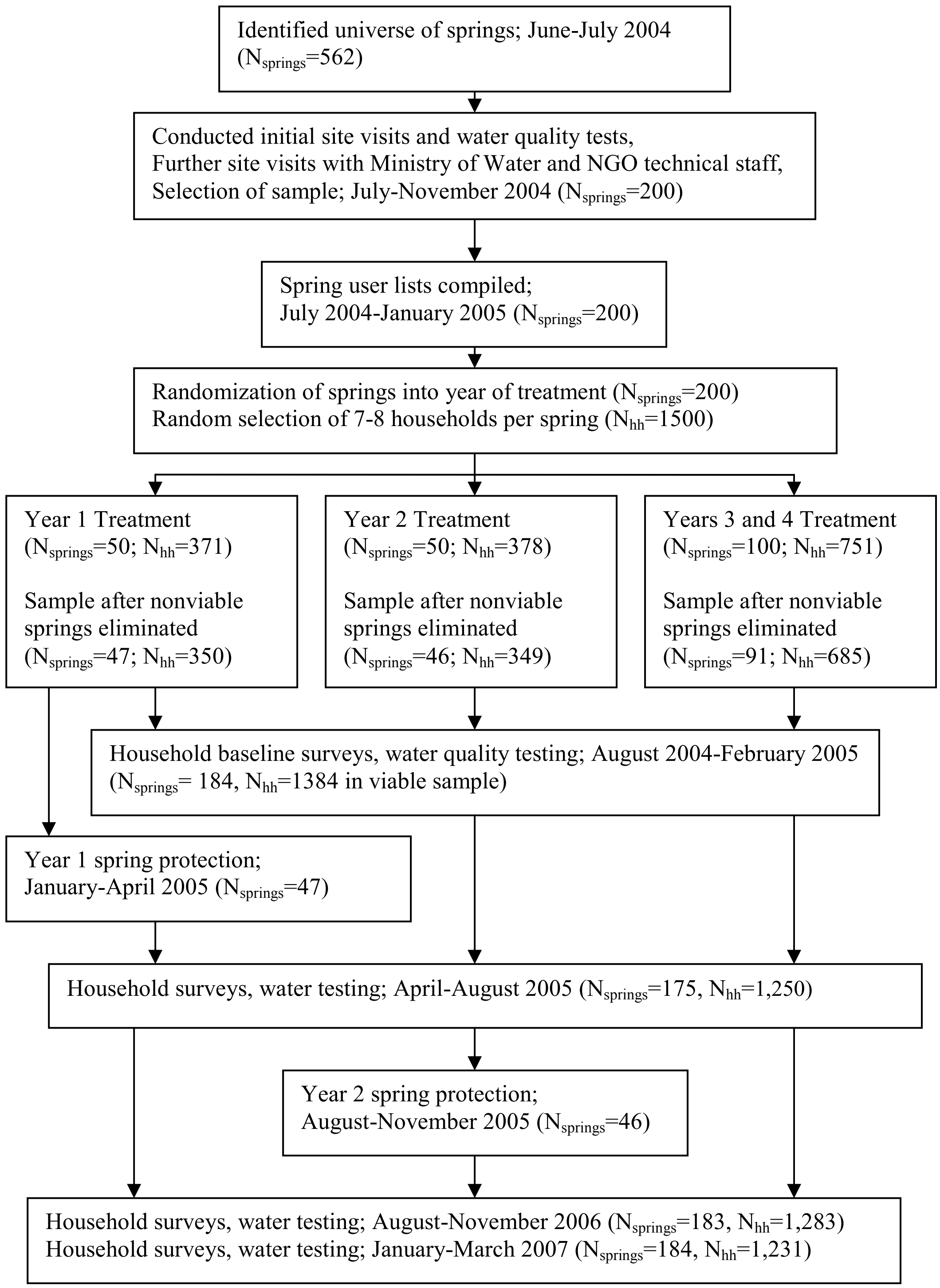
This closed-end format, offering discrete value choices, is standard in the contingent valuation literature (Bateman and Willis 1999).

The wording of the question emphasizing expenditure trade-offs was:

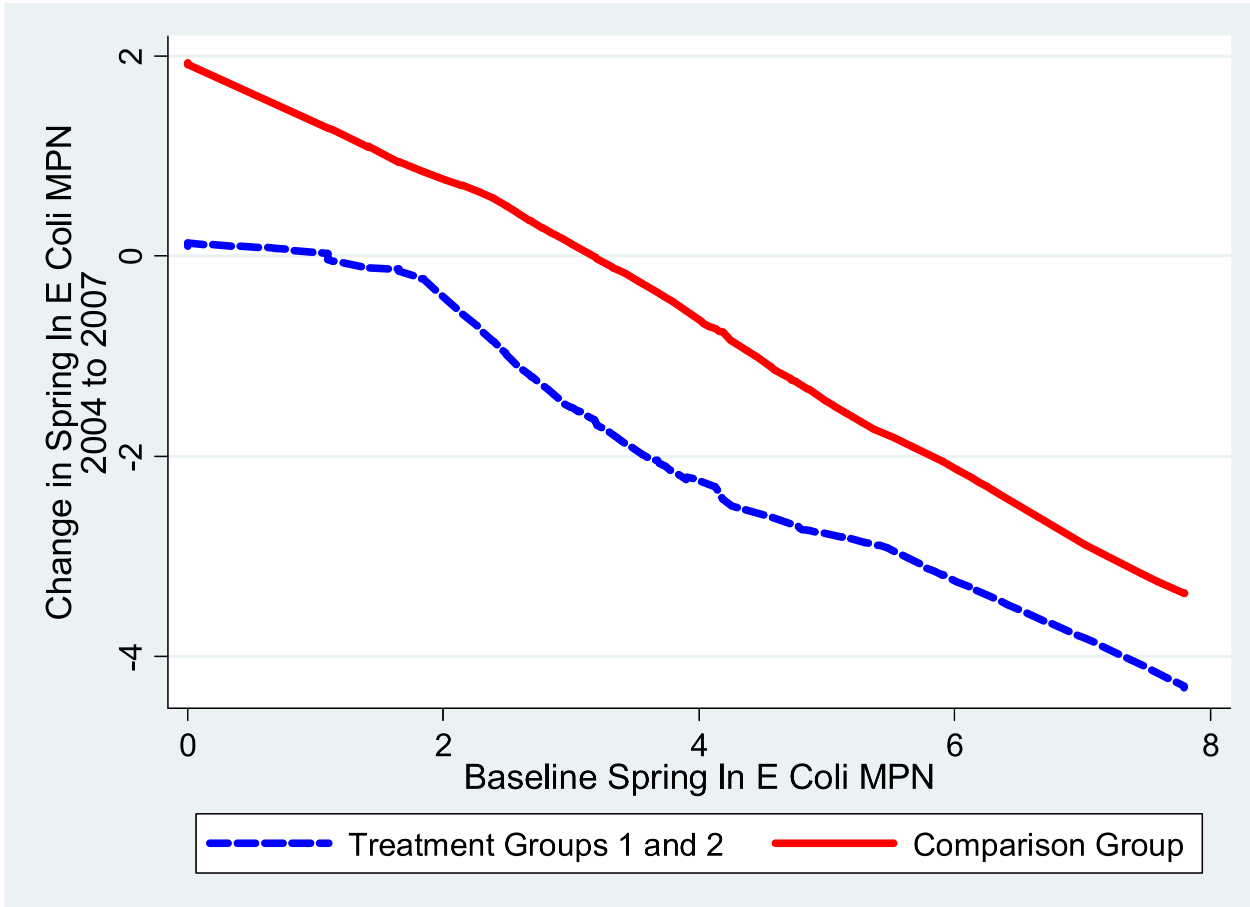
> *So, just to be sure I understand, you would be willing to give up [price] Ksh of purchases that you currently make in order to have access to the protected part of the spring. 250 Ksh per year is about 20 Ksh every month. That's a little bit less than a half-liter of kerosene or a quarter-kilo of sugar every month. For another reference, a school uniform costs about 500 Ksh. If you had to give up something you would otherwise spend money on, would you still be willing to pay [price] Ksh for access to the protected part of the spring?*

We thank Michael Hanemann for discussions on the phrasing and framing of these questions.

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



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



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