Input Efficiency as a Solution to Externalities and Resource Scarcity: A Randomized Controlled Trial

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Resource-conserving technologies are widely reported to benefit both the people who adopt them and the environment. Evidence for these "win-win" claims comes largely from modeling or non-experimental designs, and mostly from the energy sector. In a randomized trial of water-efficient technologies, the ex-ante engineering estimate of water use reductions was three times higher than the experimental estimate, a divergence arising from engineering and behavioral reasons other than the rebound effect. Using detailed cost information and experimentally elicited time and risk preferences, we infer that the private welfare gains from adoption are, on average, negative, implying no "efficiency paradox".

JEL: D0,Q0,O0

Keywords: adaptation to climate change, efficiency gap, field experiment, product adoption puzzle

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To address natural resource scarcity and externalities, economists emphasize property rights and prices. In contrast, scientists, engineers and policymakers are more likely to emphasize standards and technologies. In particular, they encourage the adoption of input-efficient technologies: energy-efficient technologies to mitigate climate change and reduce pollution (e.g., Field et al., 2014), water-efficient technologies to mitigate water scarcity and facilitate climate change adaptation (e.g., California Natural Resources Agency, 2009; FAO, 2014), fuel-efficient cookstoves to mitigate fuelwood scarcity and the ecosystem-damaging effects of wood extraction (e.g., Global Alliance for Clean Cookstoves), and precision technologies to mitigate the ecosystem-damaging effects of agriculture and forestry (e.g., Balmford, Green and Scharlemann, 2005).

Proponents often refer to input-efficient technologies as "win-win" because, in addition to reducing negative externalities, the technologies also reduce expenditures in resource-intensive goods and services, a reduction that is claimed to improve the welfare of humans who adopt the technologies (e.g., McKinsey and Co, 2009). In the face of low adoption rates, these claims raise a variety of "product adoption puzzles", which posit that consumers fail to adopt products with benefits that exceed their costs. These puzzles, or "efficiency paradoxes", have been identified in a variety of input-efficient technology contexts, including energy-efficiency ("the energy efficiency gap"; Allcott and Greenstone, 2012; Jaffe and Stavins, 1994), water-efficiency ("the water efficiency gap"; Golin et al., 2015), and improved cook stoves (Hanna, Duflo and Greenstone, 2016). To explain these puzzles, and justify interventions to subsidize technology adoption, proponents often point to cognitive and market barriers (Allcott and Greenstone, 2012; Allcott, Knittel and Taubinsky, 2015; Borgeson, Zimring and Goldman, 2012; Houde and Myers, 2019; Sallee, 2014) .

To expand the experimental evidence base on the environmental and economic impacts of input-efficient technologies, we report on a randomized controlled trial of water-efficient technology adoption (RCT, Alpizar, Bernedo Del Carpio and

Ferraro, 2021). To our knowledge, it is the first RCT of an input-efficient technology outside of the energy context. We (1) assessed whether the predicted effect of input-efficient technology adoption using a prospective engineering approach matches the estimated effect in the RCT; (2) explored the reasons for any divergence between the engineering estimate and the experimental estimate; and (3) assessed whether there is a "efficiency paradox", whereby the marginal benefits from adoption exceed the marginal costs by a large margin, on average, but potential users nevertheless fail to adopt the technologies.

Economists have largely been skeptical that input-efficient technologies are as impactful, environmentally or economically, as proponents claim (Gillingham, Rapson and Wagner, 2016; Greening, Greene and Di, 2000; Metcalf and Hassett, 1999). Prospective "engineering" approaches (e.g., Cooley, Christian-Smith and Gleick, 2009; Fidar, Memon and Butler, 2010; U.S. Government Accountability Office, 2000) have long been criticized for overly optimistic assumptions about technology field performance and about human preferences and behavioral responses (Hirst, 1986; Hirst and Goeltz, 1984, 1985; Metcalf and Hassett, 1999; Sebold and Fox, 1985). Retrospective approaches that use field data often find a large gap between realized savings and the savings predicted by engineering approaches (e.g., Burlig et al., 2020; Davis, Martinez and Taboada, 2020*a*; Houde and Myers, 2019). Yet much of the field data are from non-experimental designs, such as before-after designs (e.g., Davis, 2008; Lee, Tansel and Balbin, 2011) or with-without designs (e.g., Brooks et al., 2016; Kenney et al., 2008; Mayer et al., 1999). These designs are challenged by biases from unobservable differences across pre- and post-adoption periods and among adopters and non-adopters (Gillingham and Palmer, 2014). Non-experimental difference-in-differences designs (e.g., Allcott and Greenstone, 2017; Bennear, Taylor and Lee, 2012; Davis, Fuchs and Gertler, 2014; Pfeiffer and Lin, 2014) and machine learning imputation designs (Christensen et al., 2021) reduce such biases. Yet they are much rarer and still may not be able to adequately control for time-varying confounders that drive technology adoption and resource use.

Field experiments improve on engineering approaches by using field data in naturally occurring contexts, and they complement observational designs by requiring fewer assumptions for causal inference. Despite those advantages, experimental designs that create random variation in input-efficient technology adoption are rare[1]. Most designs randomize biomass cookstoves and, overall, yield ambiguous answers about the effect of improved efficiency on fuel use (Bensch and Peters, 2015; Berkouwer and Dean, 2022; Burwen and Levine, 2012; Hanna, Duflo and Greenstone, 2016; Pattanayak et al., 2019; Rosenbaum, Derby and Dutta, 2015, ; of the six cited studies, only two report input reductions with confidence intervals that exclude zero[2]). Two field experiments that have been implemented outside the cookstove context fail to detect any effects or find only a modest effect size (Carranza and Meeks, 2016; Fowlie, Greenstone and Wolfram, 2018). Furthermore, some field experiments have been criticized by technology proponents for low compliance rates; see, for example, the critique of Fowlie, Greenstone and Wolfram (2018) by NASCSP (n.d.) or the critique of Hanna, Duflo and Greenstone (2016) by Grimm and Peters (2012).

Moreover, the experimental data come from the energy sector and, except for fuel-efficient cookstove experiments and an efficient lightbulb experiment, from high-income nations. Thus, published experimental results may not generalize to other resources or countries. This concentration of experimental evidence is a concern given the wide range of sectors in which input-efficient technologies are promoted and the substantial funds invested in promoting these technologies in low and middle countries, where energy and water use per dollar of Gross

[1]We focus on input-efficiency RCTs that observe changes in input use. We therefore exclude: (i) experiments that use self-reported or imputed, rather than observed, changes in input use; (ii) experiments that do not isolate the effects of adopting more efficient technologies on input use (e.g., changes to prices, in-home displays, audits and other forms of information transfers, or peer comparisons, which can affect input use through multiple channels); and (iii) experiments that test technologies that use different inputs (e.g., switch people from biomass stoves to solar stoves).

[2]Rosenbaum et al. has a small sample size and does not report standard errors of their estimated effects. Pattanayak et al. report on an intervention that included stoves with improved efficiency and stoves that used alternative fuels, making the contribution of improved efficiency on fuel use uncertain.

Domestic Product is high[3][4] (e.g., the International Finance Corporation reports more than US$307 billion of "investment potential" in improved industrial energy efficiency alone in low and middle-income nations and reports that 110 countries committed to energy efficient investments as part of their strategy to address climate change; International Finance Coorporation, 2016).

Improving our understanding of the role that technological improvements could play in achieving more efficient water use is of particular relevance given that water scarcity is increasing as a result of a changing climate (Famiglietti, 2014; Schewe et al., 2014; Gosling and Arnell, 2016). Increasing the efficiency of water use is not just relevant for the welfare of individual households: it is a key component of adapting to climate change. More efficient water use means more water available for other purposes, and less pressure put on aquifers and reservoirs (Hallegatte, 2009; Alpízar et al., 2019; Toole, Klocker and Head, 2016). The concentration of evidence on the performance of energy efficient technologies has contributed to a better understanding of how technology can help mitigate climate change. Similar evidence is needed on the role that technological improvements (like improved cookstoves and water efficient fixtures) can play in an adaptation to climate change strategy.

Our experimental trial took place in the middle-income nation of Costa Rica. Like other Latin American governments[5], the Costa Rican government has promoted the use of water conserving technologies (Lara S., 2015; Arias, 2016). In contrast to prior experiments on input-efficient technologies, nearly 100% of the treatment group took up the technology, thereby mitigating concerns about high rates of non-compliance. Moreover, we developed detailed data on adoption costs, collected survey data on beliefs and behaviors, and elicited and jointly estimated

[3] `https://data.worldbank.org/indicator/ER.GDP.FWTL.M3.KD?most_recent_value_desc=true`

[4] `https://data.worldbank.org/indicator/EG.GDP.PUSE.KO.PP?most_recent_value_desc=true`

[5] Some Latin American governments have invested in water conserving technologies and/or advised their citizens to use these products. See `https://www.sedapal.com.pe/paginas/productos-ahorradores`, `https://www.acueducto.com.co/wps/portal/EAB2/Home/ambiente/agua/ahorro/` and `http://www.quitoinforma.gob.ec/2019/10/24/plan-de-reduccion-de-consumos-de-agua-con-ahorradores/`.

time and risk preferences. With these economic data, we can evaluate the plausibility of a product adoption puzzle in more depth than prior studies.

We find that the conventional engineering estimate of the technologies' impact on water use (28% reduction) is more than three times larger than the experimental estimate (9% reduction). Nearly half of that divergence can be closed by using more realistic assumptions about installation and actual, rather than laboratory-rated, technology performance. Similar divergences between predicted and actual performance have been reported in other low and middle-income contexts for energy technologies (e.g., Bensch and Peters, 2015; Davis, Martinez and Taboada, 2020*b*; Hanna, Duflo and Greenstone, 2016; Rom and Günther, 2019).

Yet even after those adjustments, the engineering estimate is still more than double the experimental estimate. Using survey data and supplemental analyses, we present evidence that the remaining divergence may result from post-installation disadoption of the technologies and from post-adoption behavioral changes that are often ignored in engineering and economic models: specifically, some households run the water longer as a result of non-efficiency-related changes in performance that are concomitant with the improvement in the technology's efficiency. Behavioral responses from changes in technology performance that are concomitant with improvements in efficiency have been flagged by scholars for greater scrutiny (Gillingham, Rapson and Wagner, 2016). For example, improvements in air conditioning efficiency often mean the system runs less frequently to achieve a given temperature, which may affect humidity levels in the home, which in turn induces users concerned about humidity to run the system more frequently than engineers predict. This type of behavioral response is unrelated to the price elasticity of the services provided and thus is different from the rebound effect that occupies so much of economists' attention in the context of input efficiency.

Whether a resource-conserving technology improves the welfare of adopters will depend not just on the technology's impacts on resource use, but also on assumptions about adoption costs and the time and risk preferences of the potential adopters. For example, to evaluate the economic impacts of adopting input-efficient technologies, engineers typically use a net present value analysis that assumes risk neutrality and discount rates below 10% (e.g., McKinsey and Co., 2007). These assumptions contrast with a large body of economics literature that suggests many decision-makers are risk averse and have personal discount rates well above 10% (Matousek, Havranek and Irsova, 2020), assumptions that would typically make the adoption of input-efficient technologies look less favorable.

The combination of the engineering estimate and the economic assumptions typically applied by engineering analyses (e.g., no risk aversion, low discount rates) implies large welfare gains from adopting the technologies, i.e., a product adoption puzzle. In contrast, we find no welfare gains in an analysis that incorporates the experimental estimate of impact, subjective discount rates, and risk aversion and adopter uncertainty about technology lifespan and performance. Our study thus also contributes to the small set of experimental studies that estimate the private welfare effects of input-efficient technology adoption (Carranza and Meeks, 2016; Fowlie, Greenstone and Wolfram, 2018).

In the next section, we describe the study context. In Section II, we present the prospective engineering estimate and welfare calculation. In Section III and Section IV, we present the experimental design, the experimental estimate, and revised welfare calculations. In Section V, we explore potential reasons for the divergence between the experimental and engineering estimates. Section VI concludes.

I. Study Context

A. Recruitment of Study Communities

The RCT took place in rural communities in western Costa Rica (Figure 1) where overexploitation of aquifers is a concern (Imbach et al., 2015; Lyra et al., 2017). When communities pump from the aquifers, they generate two externalities: a stock externality, where groundwater users do not fully internalize the continuation value of the resource and extract too quickly, and a pumping (extraction) cost externality, where groundwater users do not fully internalize how their own pumping lowers groundwater levels, which affects the costs of pumping for other users.

The RCT was part of a larger Canadian government-funded research project on climate change adaptation and water scarcity in Central America. In about 85% of communities in the region, households obtain their water from community water distribution systems rather than private wells. About half of the community systems are run by a government agency and the other half by community-based water management organizations (CBWMOs; Madrigal-Ballestero and Naranjo, 2015; Madrigal, Alpízar and Schlüter, 2011). In 2013, a research team from the Tropical Agricultural Research and Higher Education Center (CATIE) conducted a survey among the 81 CBWMOs in a sub-region where aquifer pumping was of greatest policy concern. CATIE is a widely known, non-governmental organization that implements and studies development and environmental programs throughout Central America. In 2014, CATIE staff used the survey data to select CBWMOs that measured household water use with meters and applied variable rate pricing (i.e., households save money if they reduce water use). Staff called 66 CBWMOs that met these criteria, and asked their management committees if: (1) they had monthly water records of households dating back to 2012 and would share these data by sending them to CATIE, and (2) they were interested in having the project team install water-efficient technologies in a randomly chosen subset of their residential customers and in sharing the post-installation water data. To meet the target sample size (see power analysis, Appendix A1), the team selected nine communities from the ten that met the criteria. The use of water meters and variable pricing in these communities, in conjunction with supportive CBWMO management, provided the conditions to measure the impact of the technologies.

B. Treatment Intervention

Households were offered water-efficient technologies installed by professional plumbers: (1) 1.5 gpm (gallon per minute) shower heads; and (2) 1 gpm faucet aerators for bathroom and kitchen faucets (Figure 1). These technologies are simple devices that mix water and air, thereby reducing the flow of water, saving water, and lowering water bills. Aerators are also supposed to reduce water splashing in the basin. No surveyed home in the study area had technologies with these levels of efficiency prior to the experiment. Although the technologies were sold in some retail stores in Costa Rica, the study communities did not have such stores nearby. After installation, the team took away the fixtures that were replaced (all households consented to this installation and removal). Dishwashers were not used in the study region. Although almost all homes had toilets and manual washing devices, CATIE engineers believed that switching out these technologies was not cost-effective (for either private or social benefits).

II. Prospective Engineering Calculations

A. Effect of Technology Adoption on Water Use

The CATIE team first calculated a basic engineering estimate of impact (BEE). The approach follows the procedures of other prospective approaches (Bennear, Taylor and Lee, 2012; Fidar, Memon and Butler, 2010; Maddaus, Maddaus and Maddaus, 2017), but improves on them by using field data from the study population rather than secondary data from a broader population. The team used field-derived data to avoid under-estimating the status quo technology performance, a problem noted in other contexts (Davis, Fuchs and Gertler, 2014). If CATIE had used secondary data instead, the BEE would have been 25% larger. However, a disadvantage of CATIE's approach using primary data is that several inputs for the BEE calculation come from small samples (see below). Thus, the sample averages used to calculate the BEE might deviate from the corresponding

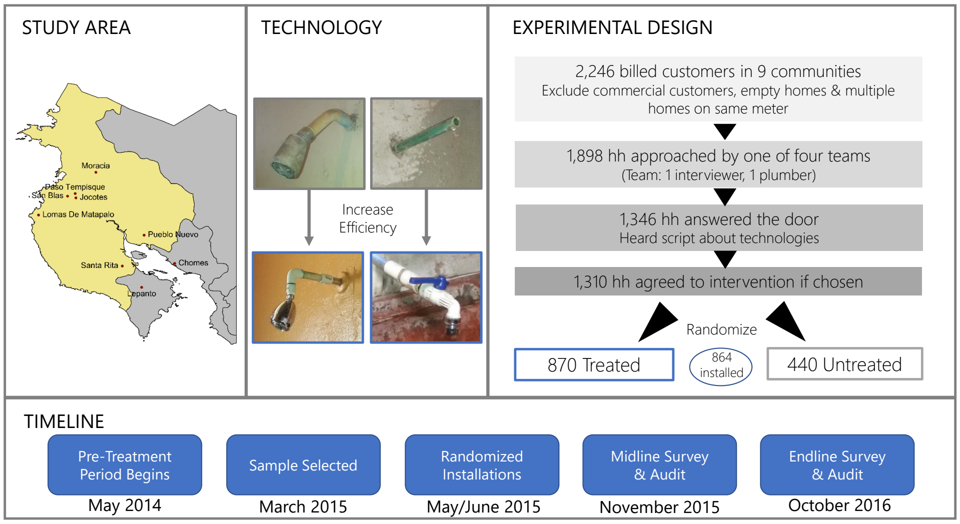

Figure 1. Study Context and Design

Note: Left panel: Study site in the provinces of Guanacaste (yellow) and Puntarenas (gray) with study communities indicated by red dots. Middle panel: Less-efficient, status quo technologies (top) are replaced with more-efficient, new technologies (bottom), sometimes requiring additional plumbing parts to complete the installation. Right panel: Experimental design. Bottom panel: Study timeline.

averages in the broader experimental sample.

The BEE calculation uses three inputs: (i) an estimate of the flow rates with the new technologies, (ii) an estimate of the flow rates with status quo technologies, and (iii) an estimate of the percentage of water consumed through each fixture with respect to the total consumption of the household (e.g., 10% of water consumed by households flows through shower fixtures). For the first input, the field team followed the standard engineering approach of using the laboratory-rated flows as they appeared on the product labels (1.5 gpm shower heads and 1 gpm faucet aerators for bathroom and kitchen faucets). The data for the other two inputs were gathered from a random sample of households (n=67) of the control group in the nine communities. To measure flow, the team measured the time it took to fill a 3-liter container from each of the fixtures. The fixture valves were opened to their maximum flow, as is required in laboratory performance rating trials (and thus comparable to the laboratory-rated flow of the new technologies). For the third input about fixture contributions to total water use, the team randomly selected 23 of the 67 households and recorded a baseline reading on the house meter and installed micro-meters on the shower, kitchen, and bathroom fixtures. At the end of one month, the team collected the micro-meter data and another reading from the house meter. The sample size and method used to collect each input appear in Table 4, panel A. Using these three measures, one can calculate the BEE:

$$\sum_{i=1}^{3} W_i \cdot F_i \tag{1}$$

where i is the fixture category (shower, bathroom or kitchen fixture), W is the average percentage of water that runs through fixture category i, and F is the percentage change in the average flow rates with and without technology installed in fixture category i. The BEE is 27.7%: in other words, for a randomly selected household, the expected reduction in water use from adopting water-efficient technologies is 27.7%, which would be 6.77 m^3 per month in the experiment's post-treatment period. This expected reduction is in line with estimates from the US Environmental Protection Agency WaterSense program, which estimates that the average American household can save 32% on water costs by retrofitting with water-efficient fixtures (EPA WaterSense, 2017).

B. *Effect of Technology Adoption on Household Welfare*

Engineers typically evaluate the economics of technology adoption by using a net present value approach (Newnan, Eschenbach and Lavelle, 2017); see, for example, the Alliance for Water Efficiency Tracking Tool v3.0. Using this approach, we make standard assumptions: the new technologies affect water use by an amount equivalent to the BEE and last for the manufacturer's advertised product lifespan (10 years), and consumers are risk neutral and have a discount rate of 7%. The same discount rate was used in a widely cited study by McKinsey and Co. (2007) to estimate the private returns to input-efficiency investments for abating greenhouse gas emissions. To estimate the private costs of installation, we use retail prices of the technologies and assumed an installation time of one hour multiplied by a prevailing wage rate. We calculate a private installation cost of US$25.99 per household (see Appendix A2 for details). Note that water-efficient technologies are not known to provide any co-benefits, unlike, for example, fuel-efficient cookstoves whose adoption may also affect health in addition to reducing fuel use. Based on the engineering assumptions, the net present value of technology adoption is US$220. For comparison, a household that monthly consumes 25 m^3 spends on average US$14[6] and the daily minimum wage of an unskilled worker in Costa Rica in 2015 was roughly US$18.21[7]. Given that no households had the technologies prior to the experiment, the estimated net present value implies a product adoption puzzle.

III. Experimental Design

Figure 1 (right panel) illustrates the experimental design (approval was obtained from a U.S. university's Institutional Review Board). The communities reported 2,246 billed customers in March 2015. Based on the pre-treatment billing data and a pre-treatment field visit, 348 customers were eliminated because they had zero consumption between December 2014 and March 2015 (assumed vacant), shared a water meter with another house, or were commercial establishments. The exclusion exercise left 1,898 households.

To contact these households, CATIE had four teams, each with an interviewer and a plumber. Interviewers had bachelors' degrees and survey experience and were trained to implement the randomization protocol. The four teams, overseen

[6]The CBWMOs charge a fixed water price per month and a marginal water price per cubic meter. The national public utility regulator in Costa Rica sets the national price schedule but CBWMOs usually charge prices lower than the schedule.

[7]The daily salary of an unskilled worker in Costa Rica in 2015 was ₡9,599 (Ministerio de Trabajo y Seguridad Social, 2015) or US$18.21 using the exchange rate at the time (1 USD = 527 CRC).

by a field manager, went to the nine communities sequentially. Communities in rural Costa Rica do not have maps with the location of houses and houses are not numbered. Thus, to facilitate the randomization procedure and ensure measurement fidelity over time, CATIE created community maps with the location of all houses and placed identification number labels on every water meter in the community. Using the community maps, CATIE divided the community into four equally populated sectors and assigned each team to one of them. Interviews were conducted using a tablet.

The team was able to contact 1,346 heads of households. In contrast to the contacted households, the uncontacted households used, on average, 13.8% less water per month in the pre-treatment period. The interviewer read a short script that comprised: 1) an introduction of team members; 2) information from a CATIE climate study about recent and future weather changes in the region and the implications of these changes for water conservation, 3) a description of the technologies and a video of them in use, and 4) an offer to install the water-efficient technologies for free if the home was selected at random. Households were only randomized to a treatment arm if a head of household indicated he or she was interested in accepting the installed technologies for free that same day.

A key feature of the design is that all households received the same marketing script, which allows us to isolate the technologies' effect on water use separate from any effect from marketing information that refers to drought and water conservation; rarely do marketing materials for input-efficient technologies fail to mention the motivations for conserving inputs. By giving the script to both treated and control groups, the effect of the script is the same, in expectation, in both groups under the assumption that the effect is additive (i.e., the technology is not a substitute for other responses that may arise from the script).

Of the 1,346 households, 1,310 agreed to have the technologies installed should they be selected to receive them. Among these households, 395 were visited in May 2015 and the other 915 in June 2015. They were randomized into one of three treatment arms:

1. Control Group: Residents who agreed to install the technologies but did not receive the technologies.
2. No-Bonus Group: Residents who agreed to install the technologies and received the technologies.
3. Bonus Group: Residents who agreed to install the technologies and received the technologies. After they had agreed to install the technologies, they were then offered a performance bonus of US$38 if they still had all technologies installed when the team returned unannounced sometime in the following six months.

In our main analysis, we combine the two installation arms which is the most generous framing for the technology's impact. We also provide results using only the no-bonus group. The bonus group treatment is the focus of another study. Randomization was implemented by having the resident put her hand in an opaque bag with three colored chips inside, one for each treatment arm.

Summary statistics by treatment condition are in Table 1. Treatment assignment does not predict water use in the year before the treatment (Table 2, column 1). The number of each type of technology installed appears in Table A1.

A. *Compliance with treatment assignment*

The plumber was able to install at least one efficient technology fixture in all but six households (99+% success). We retain these six treated households in the analysis. To determine if any control household adopted the technologies in the post-treatment period, we conducted an audit of a random sample of 63.4% of the control group three to four months after treatment assignment. None of the households had the technologies and thus we assume zero non-compliance in the control group. To assess whether dis-adoption at $t > 0$ played a role in any divergence between the engineering and experimental estimates, CATIE conducted

Table 1—Summary Statistics by Treatment Condition

Variable	Treated		Control		All	
	Mean	SD	Mean	SD	Mean	SD
Number of HH members	3.67	1.78	3.57	1.73	3.64	1.77
Number of showers at home	1.03	0.30	1.03	0.36	1.03	0.32
Number of kitchen faucets at home	0.77	0.46	0.79	0.43	0.78	0.45
Number of bathroom faucets at home	0.58	0.58	0.65	0.62	0.61	0.59
Owns home	0.87	0.33	0.88	0.33	0.87	0.33
Years in the same home	18.24	15.31	18.61	16.29	18.37	15.64
Earns less than ₡250,000	0.65	0.48	0.65	0.48	0.65	0.48
Completed secondary school	0.27	0.44	0.27	0.45	0.27	0.44
Participated in prior two CBWMO assemblies	0.39	0.49	0.40	0.49	0.39	0.49
Pre-treatment water consumption (m^3)	24.85	14.17	24.41	13.57	24.7	13.97
Observations	870		440		1310	

Note: * In May 2015, this value was roughly the official monthly minimum wage for unskilled workers: ₡286,467 or US$544 (Ministerio de Trabajo y Seguridad Social, 2015). Pre-treatment water consumption corresponds to the period May 2014-April 2015.

two audits of treated households (November 2015, October 2016). Those data are presented in Section V.

B. Estimand and Estimator

In the analysis, we use meter data on monthly water consumption from May 2014 through September 2016. Thus, depending on a household's date of randomization, the panel comprises twelve to thirteen months of pre-treatment water consumption and fifteen to sixteen months of post-treatment consumption.

With less than 1% non-compliance at installation, we believe our design allows us to estimate the average treatment effect (ATE) of water-efficient technology adoption on monthly water use over a 16-month period among households that met the inclusion criteria. Given potential disadoption at later dates, this estimand is not the same as the ATE of adopting and keeping the technology installed for the entire post-treatment period. Although prior experimental studies of input efficiency do not explicitly consider disadoption, technology disadoption is often found when researchers look for it. For example, more than half of households who adopted compact fluorescent lightbulbs in Kenya (Figueroa, 2016), fuel-efficient cookstoves in India (Hanna, Duflo and Greenstone, 2016), anti-malarial bed nets in Uganda (Clark et al., 2016), and conservation agriculture practices in Ghana, South Africa, and Zambia (Giller et al., 2009) subsequently stopped using the technologies. No technology was displaced by a superior technology — households just reverted to the status quo technology. If there is technology disadoption in our study context (a form of non-compliance), our target estimand – the ATE of adoption – can also be interpreted as the Intent to Treat Effect (ITT) of adopting the technologies and keeping them installed for the entire post-treatment period.

To estimate the treatment effect, we use a random-effects panel data estimator with monthly water consumption data in cubic meters:

$$c_{it} = \beta_0 + \beta_1 \cdot treated_{it} + community_k + install\_team_j + month_t + \epsilon_i + \mu_{it} \tag{2}$$

where c_{it} is the monthly water consumption in the ith household in month t and $treated_{it}$ is a treatment dummy variable that switches from 0 to 1 in the month after a treated household installs the technology package and stays equal to 1 for these households in the post-treatment period. Given the block randomization, the estimator includes dummy variables for the blocking variables (community, installation team). To increase the precision of the estimate, it also includes dummy variables for the month.

IV. Experimental Results

A. *Effect of Technology Adoption on Water Use*

Column 2 in Table 2 reports the estimated treatment effect: -2.21 m^3. In the post-treatment period, the control group consumed on average 24.42 m^3 of water per month (SD = 16.34), which implies that the treatment reduced average monthly water use by 9.1%, or about 0.14 SD. As a robustness check we include other specifications in columns 3 to 7. The panel is unbalanced, with 4.7% of the sample having some missing monthly water consumption. Using the balanced panel only we obtain similar results: -2.17 m^3, or a reduction of 8.8% (column 3). Results are almost the same if we only use the control group and the treatment arm that did not receive the bonus (-2.24 m^3, or a reduction of 9.2%; see column 4), or if we were instead to use a cross-sectional OLS regression estimator using the average monthly post-treatment consumption as the dependent variable and, as covariates, the blocking variables and average monthly pretreatment consumption (-2.21 m^3, or a reduction of 9.0%; see column 7). If instead we use a fixed effects model (column 6), we obtain the same results: -2.23 m^3, or a reduction of 9.1%. However, this model is less efficient.

Our estimated reduction in water use compares favorably with the estimated reductions from behavioral interventions that aimed to reduce residential water consumption. Jessoe et al. (2021) find that the provision of three different versions of home water reports reduced average consumption by 4% to 5% during the treatment year, but no effect could be detected five months after the treatment period. In two reviews of field experiments that use social norm-based interventions to reduce residential water use (Jessoe et al., 2021; Nauges and Whittington, 2019), the estimated reductions range from 2% to 5%. In order to compare our intervention with a price instrument, we would need the price elasticity of demand for household water consumption, which unfortunately is not available for Costa Rica. Nauges and Whittington (2010) find that despite substantial heterogeneity, most studies of own-price elasticity of water demand for private consumption in developing countries are in the range of -0.3 and -0.6. That range implies that a price increase of between 15% and 30% would be necessary to reduce water consumption by 9.1%.

Recall that the engineering estimate (BEE) is 27.7% (-6.77 m^3), which is more than three times larger than the experimental estimate. The BEE also assumes that the treatment effect materializes instantly and stays constant over time. To investigate these assumptions, we estimate the average treatment effects by month before and after installation (Figure 2; see Appendix A3 for details). The estimated effect is around zero in the months before treatment assignment and then becomes negative (-2.95 m^3) in the first month after treatment, consistent with the engineering assumption of immediate effects. Whether the effect is constant over time is not easily discerned from the figure. We test the hypothesis that the monthly effects after installation are equal and reject it ($p < 0.001$). To differentiate a trend in the treatment effect from time-varying effects moderated by environmental or economic conditions (e.g., seasonal changes in water use), we tested the null hypothesis of zero difference between the average estimated effect in the three-month period immediately after treatment assignment and in the same three months in the following year. The difference between the two estimates is imprecisely estimated, but the point estimate is negative, implying that, if the treatment effect is changing over time, it is waning: -0.90 m^3, 95% CI [-2.10, 0.30]. We did the same analyses using only the control group and the treatment

Table 2—Estimated Treatment Effect of Technology Adoption on Water Consumption (m^3/month)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment Effect	0.41	-2.21	-2.17	-2.24	-2.12	-2.23	-2.21
	[-1.13,1.96]	[-3.02,-1.41]	[-2.99,-1.35]	[-3.19,-1.30]	[-2.98,-1.26]	[-3.04,-1.41]	[-3.03,-1.38]
. . . in %	1.68	-9.06	-8.78	-9.19	-8.63	-9.11	-9.02
Community dummy	Yes	Yes	Yes	Yes	No	No	Yes
Month dummy	Yes	Yes	Yes	Yes	No	Yes	No
Install team dummy	Yes	Yes	Yes	Yes	No	No	Yes
Post-treatment period	No	No	No	No	-0.15	No	No
					[-0.84,0.55]		
Pre-treatment water use	No	No	No	No	No	No	Yes
Constant	26.11	29.36	29.81	29.35	24.77	25.19	10.42
	[20.95,31.26]	[23.70,35.02]	[24.02,35.59]	[21.33,37.38]	[24.55,24.99]	[24.62,25.75]	[6.18,14.67]
Observations	15,535	37,509	36,221	25,011	37,509	37,509	1,302

Note: Column 1 presents the estimated effect of treatment assignment on pre-treatment water use (May 2014-April 2015). Column 2 to 7 present estimates of the experimental treatment effect. Results in columns 1, 2, 3 and 4 were calculated using a random effects model. Column 2 shows our preferred specification. Model in column 3 uses a balanced panel. Model in column 4 considers data from the control group and the treatment arm that was not offered a bonus. Results in columns 5 and 6 were estimated using a fixed effects model. Estimation in column 7 uses cross-sectional data. In brackets are 95% confidence intervals, constructed from robust standard error estimates clustered at household level when possible.

arm that did not receive the bonus and obtain similar results (see Figure A2).

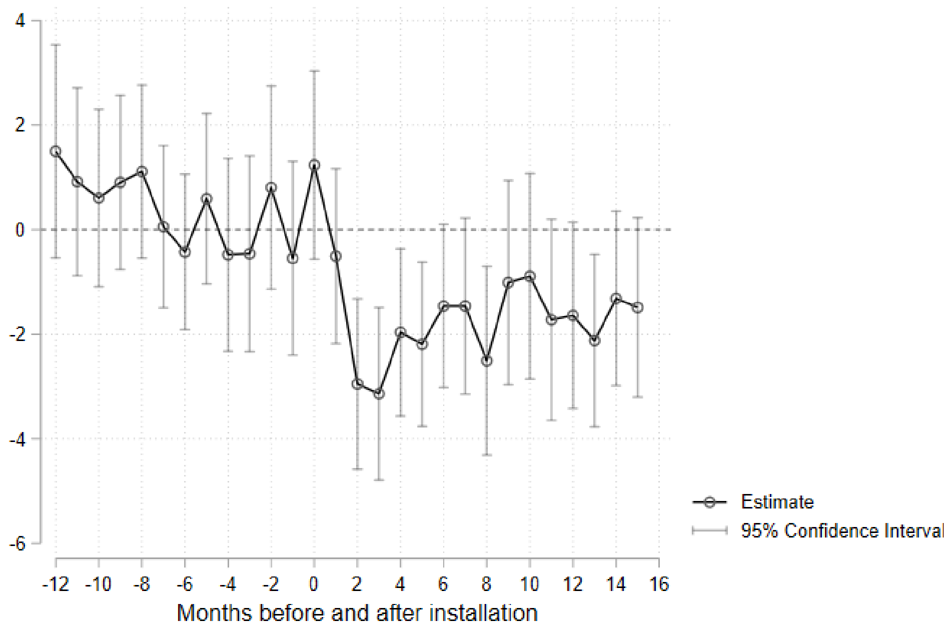

FIGURE 2. ESTIMATED TREATMENT EFFECTS PER MONTH (m^3).

Note: Installation happens in month 0. The dots indicate the estimated treatment effects in each month, while the grey lines represent the 95% confidence intervals.

B. Effect of Technology Adoption on Household Welfare

Recall that a conventional engineering net present value calculation based on the BEE implied a product adoption puzzle (Table 3, row 1). We revise the underlying assumptions of the conventional engineering net present value based on the BEE in the following ways:

1. Impact Assumption: Instead of using the BEE, we use the experimental estimate of the ATE;
2. Lifespan Assumption: Instead of assuming the product lasts for the period of the manufacturer's limited warranty, we use the average expected lifespan

reported by households in the 2016 survey (∼16 months after installation). To elicit treated household beliefs about expected lifespan, households were asked to allocate ten chips to eight different lifespans (≤2 years, 3 years, 4 years, 5 years, 6 years, 7 years, 8 years, ≥9 years); and

3. Installation Cost Assumption: Instead of assuming trouble-free installation according to manufacturer guidelines, we use detailed field data on the materials required for the installations and calculate a more realistic installation cost estimate of US$36.23, that is 39% higher than the trouble-free installation (for example, installation frequently required additional materials to retro-fit the new technologies onto the old plumbing systems).

These changes in assumptions reduce the net present value of technology adoption by 98% (Table 3, rows 2, 3 and 4): from US$220.12 to US$5.02, which is roughly 30% of the daily minimum wage of an unskilled worker in Costa Rica in 2015. Most of that reduction (77%) arises from the change in the impact assumption, and the rest from the change in the lifespan and cost assumptions.

Next, we incorporate more realistic assumptions about uncertainty and household risk and time preferences into the calculation of household welfare. We use the risk and time preference parameter estimates from Bernedo Del Carpio, Alpizar and Ferraro (2022), which were experimentally elicited using a double multiple price list design (Andersen et al., 2008) and a 2014 sample of nearly 500 individuals from thirty communities in the experimental study region (including 4 communities from this RCT). The participants made decisions both as individuals and as couples with their household partners. Preferences were best captured by a rank dependent utility (RDU) model with exponential discounting. Like the expected utility model (EUT), the RDU model permits risk aversion to the variability of payouts. In contrast to EUT, RDU also permits probability weighting. For the study sample, both individuals and couples overweight the probability of the best outcome, on average. Because decisions to purchase water-efficient technologies may be made by either individuals or couples, we use the parameter estimates of both individuals' and couples' preferences.

To develop realistic assumptions about sources of uncertainty, we selected three contextual parameters that we believe are most relevant to the adoption decision and are uncertain at the time of technology purchase: future water prices, technology lifespan, and technology performance. Each parameter is assumed to have a probability distribution. We use eight possible future scenarios, which are calculated using combinations of the 5th and 95th percentiles of the distribution of each source of uncertainty. We assume that each of the eight scenarios has the same objective probability of occurrence (12.5%). Assuming that decision-makers are optimistic probability weighters and applying the probability weighting parameter estimates from Bernedo Del Carpio, Alpizar and Ferraro (2022), we calculate the perceived probability weights in the RDU model for the eight scenarios as: 49.93%, 9.60%, 7.21%, 6.24%, 5.83%, 5.79%, 6.22% and 9.17% (52.44%, 8.09%, 6.16%, 5.44%, 5.22%, 5.39%, 6.15% and 11.10% for couples).

To construct a welfare measure that incorporates uncertainty and people's preferences, we define the discounted rank-dependent utility (DRDU) of the technology savings as:

$$DRDU = \sum_{t=0}^{T} \left(\frac{1}{1+\delta}\right)^{\frac{t}{12}} \sum_{a=1}^{8} w^a \cdot U(s_t^a) \tag{3}$$

where s_t^a is the expenditure savings at time t that varies depending on the future scenario a. Savings each year is calculated as the difference between the expenditures in water consumption with and without the technology; w^a are the probability weights of each scenario a. $U(x) = \frac{x^{1-r}}{1-r}$ is the constant relative risk aversion utility function with coefficient r, and δ is the subjective discount rate. The weighted discounted utility is summed over the product's lifespan (T). Using this framework, we calculate the expected welfare gain (EWG) as the discounted certainty equivalents (CE) of monthly savings:

$$EWG = \sum_{t=0}^{T} \left(\frac{1}{1+\delta}\right)^{\frac{t}{12}} CE_t \tag{4}$$

where $CE_t = [(1-r) \cdot \sum_{a=1}^{8} w^a \cdot U(s_t^a)]^{\frac{1}{1-r}}$.

We apply the expected welfare framework of equations 3 and 4 using the parameter estimates from Bernedo Del Carpio, Alpizar and Ferraro (2022), the three sources of uncertainty, and the new impact, lifespan and installation cost assumptions described above. Our revised welfare impact estimates indicate that technology adoption would result in a net loss for the average household, implying there is no product adoption puzzle. In other words, the calculated expected welfare gain of adopting the technologies is negative (Table 3, rows 5, 6 and 7), regardless of whether we assume risk neutrality or whether we use the preference parameters of individuals or couples. In fact, any discount rate above 13% will make the net present value under assumptions 1., 2., and 3. negative[8]. Results are the same if we redo the calculations with the ATE estimated using only the treated households that did not get a bonus.

V. The Divergence between Engineering and Experimental Estimates

In this section, we consider possible explanations for the difference between the experimental and engineering estimates (see Table 4, panel B and Figure 3).

A. *Interference among households*

In the estimation of the ATE, we assumed a household's potential water use is independent of the treatment status of other households (i.e., no interference among units; stable unit treatment values). If that assumption were violated,

[8]An anonymous reviewer pointed out that our expected welfare measure may be in fact overestimated because we do not take into account the fact that the new fixtures make cooking and cleaning more time-consuming and showers less enjoyable for some households, especially for the ones that eventually disadopted at least one of the technologies (see Table 5.)

Table 3—Expected Welfare Gain from Technology Adoption

Assumptions							Present Value (USD)
Impact	Lifespan	Installation Cost	Uncertainty	δ	r	Probability Weighting	
1. BEE	Warranty	Trouble-free	No	0.07	0	No	220.12
2. ATE	Warranty	Trouble-free	No	0.07	0	No	55.45
3. ATE	Reported	Trouble-free	No	0.07	0	No	15.26
4. ATE	Reported	Field Data	No	0.07	0	No	5.02
5. ATE	Reported	Field Data	Yes	0.30 0.44	0 0	No	(10.47) (16.17)
6. ATE	Reported	Field Data	Yes	0.30 0.44	0.81 0.77	No	(21.67) (22.84)
7. ATE	Reported	Field Data	Yes	0.30 0.44	0.81 0.77	Optimistic	(15.45) (17.95)

Note: BEE is the basic engineering estimate (Section II). ATE is the average treatment effect estimate (Table 2). The trouble-free installation cost assumes that the only cost of installation is the technology package and one hour of time. The field data include additional costs (see Appendix A2). Uncertainty arises from variance in the lifespan of the technology, the water price growth rate, and the impact of the technology. The value of the discount rate (δ) and Constant Relative Risk Aversion coefficient (r) are either based on convention (δ = 0.07, r = 0) or elicited in a field experiment for both individuals (δ = 0.30, r = 0.81) and couples (δ = 0.44, r = 0.77). See main text for details.

the interpretation of the estimated treatment effect in Figure 3 would change. One potential violation of the assumption would be when the control households, having observed the technologies in treated households, subsequently adopt the technologies (or similar ones). As reported in Section III, our random audit of a random sample of control households found no evidence that control households adopted water-efficient technologies in the post-treatment period. Another potential form of interference in our context is an effect of water conservation on flow and water availability in the gravity-fed water systems. If the treatment reduced average water use among the treated group, the flow and water availability in the community system may increase, thereby potentially increasing the amount of water consumed among households in the control group. However, in these systems the pressure on the pipes is mostly determined by the position of the household in relation to the storage tank. Lower consumption by treated households will affect the frequency by which tanks need to be filled, not water availability. An exception could occur in scenarios of extreme water scarcity, but even in this case the effect would not be different for treated and control group.

B. *Actual rather than rated performance ratings*

As is typical in engineering estimates, the BEE is based on the flow rate reported by the manufacturer, which is assessed under a strict laboratory protocol. The actual flow rate under naturally occurring field conditions, however, may differ because of field attributes like the home's water pressure, its water quality, and the way in which the residents open the valves. Divergences between rated efficiency and field efficiency of technologies have been recognized across many technology sectors (e.g., fuel efficiency in vehicles and energy efficiency in HVAC systems and lightbulbs; Nelsen, 2015; The Economist, 2016). To adjust the BEE, two additional field measures on flows were taken. First, in the control homes in which the field team evaluated the status quo technology flows with the valves completely open, the team also measured flow after asking residents to open the valves as they normally do in their daily activities. Second, in 32 randomly selected treated households from the nine communities, the team did the same with the new technologies during the first audit in 2015. Using field data of actual flow rates of the new and old technologies as they are typically used by residents, we revise the BEE and obtain an expected 24.4% reduction in water consumption.

C. Actual rather than assumed installation success

Engineers typically assume the technologies are installed per the manufacturer's recommendations. In contrast, installations in technology adoption programs often deviate from those recommendations (Domanski, Henderson and Payne, 2014; Jessoe and Rapson, 2014). In CATIE's program, the team did not install some technologies because one or more of the fixtures was missing, the plumbing could not be adapted to fit the technology, or the head of household did not allow the field team to replace one of the fixtures. To incorporate the installation success rates, the engineering estimate is calculated using equation 5.

$$\sum_{i=1}^{3} W_i \cdot F_i \cdot R_i \tag{5}$$

where i is the fixture category, W is the average percentage of water that runs through fixture category i, F is the percentage change in the average field-measured flow rates with and without technology installed in fixture category i, and R is the installation success rate per fixture category i. The estimate implies a 22.1% reduction in water consumption.

D. Actual rather than assumed water uses affected by efficiency

Engineers typically assume 100% of in-home water use is affected by changes in efficiency. When preparing food and beverages, however, people often use water in fixed quantities; e.g., to prepare a cup of rice, people use one cup of water. When water is used in fixed quantities, the improvements in fixture efficiency will merely increase the time required to fill the pot or glass. It will not reduce the amount of water used. We could not find an engineering model that adjusts impact estimates for such uses. Based on interviews, we assume that water used in fixed quantities in our sample comes from kitchen fixtures during meal preparation. Because we could not find published estimates of the percentage of water used as a cooking input in Central America, we estimate it from a sample of ten households from one of the study communities. The field team asked female heads of these households, who traditionally do the cleaning and food preparation in the study communities, to record their water consumption during breakfast, lunch, and dinner on a weekend day. The women were trained to measure water consumption for cooking and beverages using a one-liter container, and to measure the time spent on cleaning dishes and food and for any other activity using a chronometer. The team also measured the flow rate in the kitchen faucet. The estimated average total amount of kitchen water use from the ten households is similar to the estimated total amount of kitchen water use that we estimated from the larger sample of households in the micrometer sample: 107 liters/day (vs 124 liters/day in the micrometer sample). We obtain that the percentage of water consumed in the kitchen that is affected by efficiency is 85%.

Using the new input, and the estimates of actual field performance and installation success (Sections V.B and V.C), we obtain what we label the Enhanced Engineering Estimate (EEE). It is calculated as

$$\sum_{i=1}^{3} W_i \cdot F_i \cdot R_i \cdot S_i \tag{6}$$

where S is the percentage of water consumed in the kitchen that is affected by efficiency and equals one in the case of the shower head and the bathroom faucet. The EEE implies that the adoption of the technology is expected to reduce monthly water use by 20.5%, a value that is still more than double the experimental estimate and one that yields a expected welfare gain of US$56.29.

Table 4—Enhancements of the Basic Engineering Estimate

Estimate	Inputs	Source	Procedure	Result
Panel A.				
Basic Engineering Estimate (BEE)	1. average flow rates with new technologies (liters per minute)	-	Flows provided in product labels of shower head and aerators	27.7%
	2. average flow rates with status quo technologies (liters per minute)	67 control hhs from all communities	Team measures time it takes to fill a 3-liter container on each fixture, opening valves to their maximum flow.	
	3. average percentage of water consumed through each fixture with respect to total household consumption	23 control hhs (out of the 67 above)	Team records water consumption readings on house meter and on micro-meters installed on each fixture at baseline and a month later.	
Panel B.				
BEE adjusted for field performance of technology	1. average flow rates with new technologies (liters per minute)	32 treated hhs from all communities	Team measures time it takes to fill a 3-liter container on each fixture, opening valves as residents normally do.	24.4%
	2. average flow rates with status quo technologies (liters per minute)	67 control hhs from all communities	Team measures time it takes to fill a 3-liter container on each fixture, opening valves as residents normally do.	
	3. average percentage of water consumed through each fixture with respect to total household consumption	23 control hhs (out of the 67 above)	Team records water consumption readings on house meter and on micro-meters installed on each fixture at baseline and a month later.	
BEE adjusted for field performance of technology and installation success rate	1. average flow rates with new technologies (liters per minute)	32 treated hhs from all communities	Team measures time it takes to fill a 3-liter container on each fixture, opening valves as residents normally do.	22.1%
	2. average flow rates with status quo technologies (liters per minute)	67 control hhs from all communities	Team measures time it takes to fill a 3-liter container on each fixture, opening valves as residents normally do.	
	3. average percentage of water consumed through each fixture with respect to total household consumption	23 control hhs (out of the 67 above)	Team records water consumption readings on house meter and on micro-meters installed on each fixture at baseline and a month later.	
	4. technology installation success rate	All treated hhs	Percentage of technologies installed with respect to all units available.	
Enhanced Enginnering Estimate (EEE)	1. average flow rates with new technologies (liters per minute)	32 treated hhs from all communities	Team measures time it takes to fill a 3-liter container on each fixture, opening valves as residents normally do.	20.5%
	2. average flow rates with status quo technologies (liters per minute)	67 control hhs from all communities	Team measures time it takes to fill a 3-liter container on each fixture, opening valves as residents normally do.	
	3. average fraction of water consumed through each fixture with respect to total household consumption	23 control hhs (out of the 67 above)	Team records water consumption readings on house meter and on micro-meters installed on each fixture at baseline and a month later.	
	4. technology installation success rate	All treated hhs	Percentage of technologies installed with respect to all units available.	
	5. average percentage of water used to clean with respect to total water used in kitchen	10 hhs from one community	Women recorded water consumption for cooking and beverages using a one-liter container, and time spent on cleaning dishes and food using a chronometer. The team measures kitchen faucet flow rate.	

E. Sampling error of the engineering estimate

We could not find engineering reports on the impacts of input-efficient technologies that report standard errors or other measures of uncertainty (see, for example, the McKinsey report on greenhouse gas emissions abatement cost curves, McKinsey and Co., 2007). The lack of such measures may stem from the common use of secondary, rather than primary, data. In our study, however, the BEE and EEE are based on primary data. To incorporate standard errors into the EEE, we randomly draw from our data the percentages of water flowing through each fixture and the flow rates with and without the technology and then calculate an EEE. We do this draw-calculate procedure one million times to generate a 95% confidence interval for the EEE. As can be seen in Figure 3, some of the remaining difference between the EEE and the experimental estimate may arise from sampling variability, but there is still an economically relevant gap between the two estimates. We next consider behavioral reasons for the gap.

F. Disadoption and behavioral responses to changes in product attributes

Engineering estimates like the EEE are based on the assumption that, after a household adopts a technology, it keeps it. Adoption, however, may be followed by disadoption. For example, disadoption rates of efficient cookstoves and lightbulbs have been reported to be at least 32% and 63%, respectively (Figueroa, 2016; Hanna, Duflo and Greenstone, 2016). In our RCT, treatment assignment led to adoption in all but six households. Yet, by the endline audit, 53.2% had disadopted one or more of the installed fixtures (Figure 4).[9]

The estimand that most closely matches the EEE is the ATE of keeping the fixtures installed until the 2016 endline. This estimand is a weighted combination of the average treatment effect for the households that use the technologies for

[9]The team was unable to audit all treated homes. For the values reported in Fig. 4, we impute the missing audit status (see Appendix A4). Considering only the values from homes observed in both audits, 50.6% kept all technologies until endline, 36.6% disadopted at least one fixture between midline and endline, and 12.8% disadopted at least one fixture before midline.

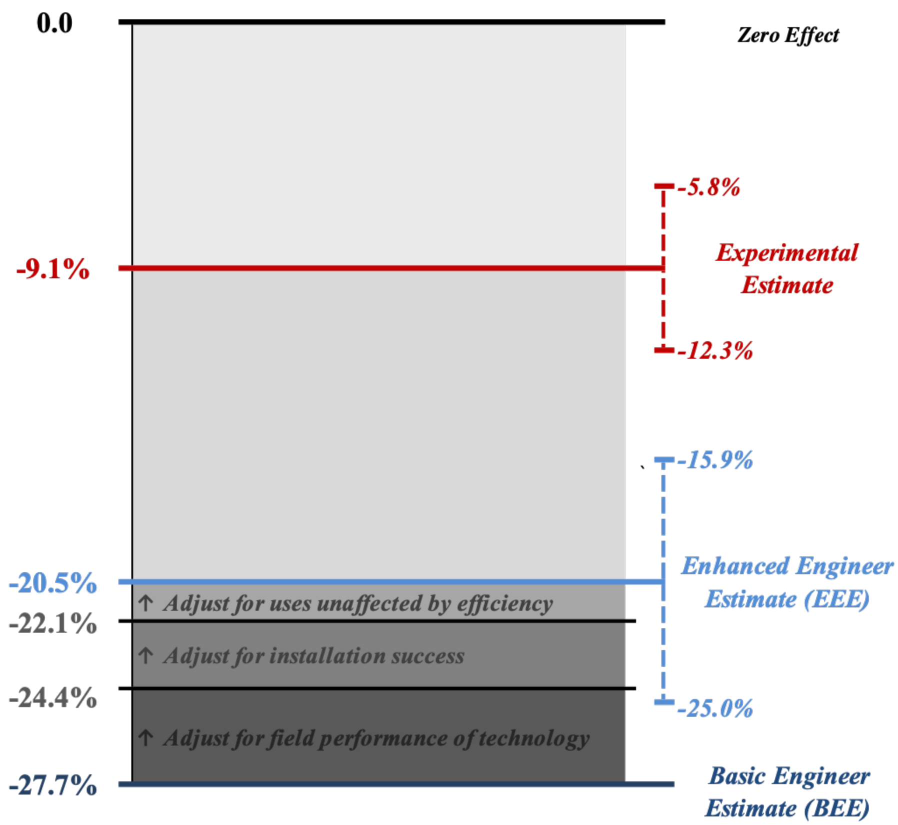

Figure 3. Reasons for Divergence between Engineering and Experimental Estimates of Effect of Technology Adoption on Water Use.

Note: The estimates are presented in terms of the estimated percent reduction in monthly household water use from technology adoption. The BEE is derived from a conventional engineering modeling approach supplemented with micrometer field data on water consumption patterns. The EEE adjusts the BEE with refinements based on additional field data (see Sections V.B - V.D). The red and blue bars around the EEE and the experimental estimate are 95% CIs.

the entire post-treatment period (Perfect Compliers) and the average treatment effects for the different types of compliers (disadopters at 1 month, disadopters at 2 months, etc.) had they not disadopted the technologies.

Thus, whether disadoption can explain the divergence between the EEE and the experimental estimate in Figure 3 depends on the values of these unobservable average treatment effects. We cannot directly estimate these average treatment effects in the counterfactual world where households do not disadopt the technolo-

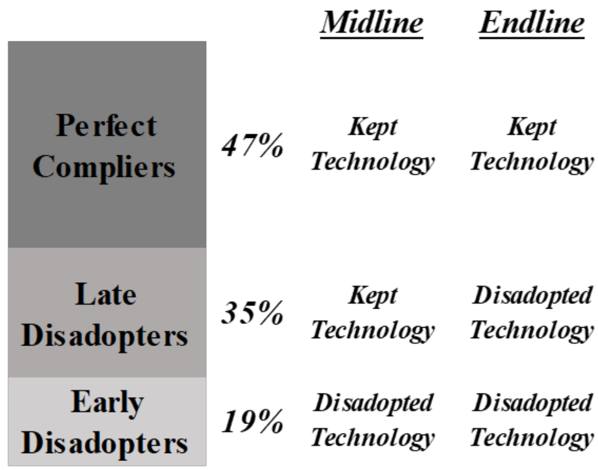

Figure 4. Patterns of disadoption at midline ("Early") and endline ("Later").

Note: "Disadoption" means the household uninstalled one or more of the installed fixtures. Only 8.1% of households uninstalled all fixtures by midline, but 23.5% of households uninstalled all fixtures by endline.

gies. However, we can calculate an upper bound on the ATE had all households kept the technologies installed until the endline. To do so, we assume no waning or growth in the monthly average treatment effects within complier type. In other words, whatever the value of the ATE is for a particular complier type in the first month after installation, we assume the value is the same for all future months.

With this "no waning or growth" assumption, the estimated treatment effect for the first month after installation captures the ATE for the post-treatment period had all households been forced to use the technologies for the entire period. The estimated effect of the technologies in the first month after installation is a reduction of 2.95 m^3, or 12.1% (see Figure 2 and Appendix Table A2). Even if we were to use this estimated impact to revise the expected welfare gain from technology adoption (Table 3), the present value of the gain in the model that incorporates field measures of time and risk preferences remains negative (-US$16.76 to - US$15.67, depending on the discount rate used).

In the midline audit, households self-reported the months that they disadopted fixtures. Only 2.4% reported disadopting one or more fixtures during the first month after installation. If we make the extreme assumption that these households had disadopted all of their fixtures immediately after installation (and thus were non-compliant during the entire first month), the complier average causal effect for the first month is -3.02 m^3 (-2.95/0.98), which implies a 12.4% reduction.

If, in contrast to our assumption, the monthly average treatment effects were to wane over time for one or more complier groups if they were forced to keep the technologies, then our estimated ATE in the previous paragraph is an over-estimate. If, however, the monthly treatment effect were to grow over time for one or more complier groups, we would under-estimate the contribution of disadoption. We have no reason to expect the monthly treatment effect to grow over time.

Thus, based on these calculations, we believe that disadoption could explain, at most, 29.0%[10] of the gap between the EEE (-5.01 m^3) and the experimental estimate (-2.21 m^3) in Figure 3. We draw similar conclusions using alternative assumptions (see Appendix A5). For instance, if we use only the control group and the no-bonus treated group, disadoption could explain, at most, 36.7% of the gap.

Why might disadoption not explain a large part of the gap between the EEE and the experimental estimate? The survey data suggest that one reason may be that households end up running the water longer to adapt to an undesirable, lower flow rate. In the midline and endline surveys, we asked households to compare the time it took them to shower, wash dishes and use the bathroom faucet with and without the new technology, and whether they liked the flow of the new technologies. More than one-third of households reported running a fixture for a longer time to complete an activity in the post-installation period compared to the pre-installation period. Moreover, there appears to be a rank ordering of households running the fixtures longer and their perception of the desirability of

[10]We calculate the portion of the gap explained by disadoption as $\dfrac{(-3.02-(-2.21))}{(-5.01-(-2.21))}=0.29$.

Table 5—Perceptions of Technologies by Household Type

Variable	Perfect Compliers	Late Disadopters	Early Disadopters
<u>Audit 2015</u>			
Activity takes longer			
bathroom faucet	8%	8%	11%
kitchen faucet	27%	31%	31%
shower head	36%	42%	56%
Person likes flowrate			
bathroom faucet	99%	99%	95%
kitchen faucet	95%	92%	56%
shower head	94%	90%	60%
<u>Audit 2016</u>			
Activity takes longer			
bathroom faucet	10%	19%	18%
kitchen faucet	27%	36%	48%
shower head	36%	48%	59%
Person likes flowrate			
bathroom faucet	98%	92%	79%
kitchen faucet	95%	81%	59%
shower head	92%	75%	56%

the flow rates. In comparison to the Perfect Complier group, the Late Disadopters reported lower rates of liking the flow of all the new technologies and higher rates of taking longer to do their household activities (Table 5, columns 2 and 3). The Early Disadopter households reported even higher rates of keeping the faucets open for longer periods of time and lower percentages of households that like the flowrate (column 4 in Table 5). Moreover, during the 2015 audit, when we asked Early Disadopters the reasons why they uninstalled the technologies, the majority responded that the flow rate was too slow.

In other words, to achieve greater water efficiency, the fixtures slow the flow of water exiting the fixture and some households respond by keeping the faucet open for a longer period (e.g., to properly clean dishes and clothing). This behavioral response is, on the surface, like the conventional rebound effect extensively studied by economists. Yet its source seems to be fundamentally different: the behavioral response arises because of an undesired change in a product attribute that accompanies the improvement in efficiency. Less than 1% of the respondents reported an increase in the frequency of using water-related services. Although a conventional rebound effect cannot be ruled out with the information we have at hand, the behavioral response to an undesired change in a product attribute is the most plausible behavioral reason we have uncovered for the remaining divergence between the EEE and the experimental estimate.[11]

VI. Conclusions

In a 2014 study on American's perceptions of water use, Attari (2014) notes that "[w]hen asked for the most effective strategy they could implement to conserve water in their lives, or what other Americans could do, most participants mentioned curtailment (e.g., taking shorter showers, turning off the water while brushing teeth) rather than efficiency improvements (e.g., replacing toilets, retrofitting washers). This contrasts with expert recommendations." Interpreting this gap between user and expert perceptions as arising from misinformed users, the author writes that "well-designed efforts to improve public understanding of household water use could pay large dividends for behavioral adaptation to temporary or long-term decreases in availability of fresh water." Our study results suggest that consumer misinformation may not be the main driver of low adoption rates.

We contribute to the literature on the economics of input efficiency by leveraging a randomized experiment on the field performance of water-efficient technologies and detailed primary data on preference parameters of potential adopters. We

[11] An alternative reason could be a version of moral licensing, whereby the adoption of water-efficient technologies allows a household to maintain a "conservationist" image while refraining from other water conservation behaviors in which it may have otherwise engaged (e.g., turning fixture off when soaping up). For this channel to be active, households must have engaged in these conservation behaviors for pro-social or moral reasons. We cannot eliminate this rival explanation with our data, but the 2013 survey conducted by CATIE, which included the nine communities of our study, asked households whether they had acted in response to warmer and longer summers in the previous five years and, if so, how: less than 5% self-reported taking efforts to reduce their water consumption and, even for this small subgroup, it is unclear if they were motivated by pro-social or moral reasons.

estimated both the effect of water-efficient technologies on water use 16 months after adoption and the welfare gains to adopters.

Consistent with prior work in the energy context, the ex post experimental estimate is much smaller than an ex ante engineering estimate: 3 times smaller. We attribute the difference between the experimental and engineering estimates to divergences between expected and actual field performances of the technologies and to post-adoption behavioral responses of the adopters.

Part of the divergence between expected and actual field performance arose because the manufacturer-rated performance did not match the field performance, a problem that has been reported in other sectors (see, for example, popular news media articles on exaggerated rated performances in the vehicle and lighting sectors (Nelsen, 2015; Singer, 2019; The Economist, 2016). Part of the divergence arose because of differences in assumed installation success rates and actual installation success rates, a phenomenon that has also been reported in other sectors (e.g., Domanski, Henderson and Payne, 2014). The remaining gap between expected and actual performance may be specific to the water context: some household water uses require fixed amounts of water and thus are not affected by improvements in efficiency.

However, even after using our own field data to correct the performance and installation assumptions, the engineering estimate is still almost double the experimental estimate. Some of the remaining divergence may be due to sampling error or disadoption of the technologies, but even after adjusting for those features of the data, a gap remains. Using survey data, we find suggestive evidence of a behavioral reason for the gap: some households respond to the lower flow rates of the efficient technologies by running the water longer (e.g. to properly wash dishes); in other words, they react to changes in technology performance that are concomitant with efficiency improvements. This reaction to a change in a basic feature of the technology (i.e. its low flow) is different from a rebound effect, which is a reaction to lower effective prices of showering or doing the dishes.

Notably, this suggests that even if the magnitude of rebound effects were exaggerated in the energy literature, as argued by some economists (Gillingham et al., 2013), there are still reasons to be concerned that engineering claims about the impacts of input-efficiency on input use may be exaggerated and policymakers may do better by relying on price to reduce input uses.

Moreover, we find no evidence of an “efficiency paradox.” Given the modest post-adoption average reduction in water use and the large average household discount rates, the average adopter would experience negative returns from adopting the technologies. Thus, to explain low product adoption rates at our study site, we do not need to seek psychological reasons, such as present bias or status quo bias, or economic reasons, such as market access or credit constraints (such reasons could still be important in some contexts; e.g., credit constraints in low-income countries, Berkouwer and Dean, 2022).

The size of the gap that we calculate is similar to the one found in several studies in the context of energy in low- and middle-income countries. This is the first estimate in the context of water and further studies are needed. We believe that the reasons for the divergence that we uncover would also appear in other resources and in regions where other water efficient technologies could be adopted (washing machine, dishwashers, toilets). However, their relative importance might differ depending on disadoption rates, performance, fixed water use or installation rates. Moreover, applying our approach to calculate the welfare gains with more realistic assumptions and time and risk preferences would generate lower welfare measures that could explain the low adoption rates that we see for these technologies.

In summary, claims of a “win-win” outcome associated with the adoption of input-efficient technologies in our study context are not supported by the data. Whether the installation costs and the modest water use reductions warrant government subsidies for technology adoption to reduce extraction on common pool aquifers in the region is a subject for future research. A social cost-benefit analysis that incorporates the costs of externalities associated with groundwater pumping may support the use of such subsidies. However, relying on private motives alone to reduce pressures on the aquifers is unlikely to be successful.

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Appendix

A1. Power Analysis

Our original power analysis was designed for a non-zero contrast of the means across the three treatment arms with equal sample sizes in each arm (control, bonus, no-bonus). This contrast was chosen under the assumption that, in a related study on whether the exposure bonus induced lower disadoption rates (a different study from the one presented here), we may be asked to perform that contrast. We sought to detect a 6% change in water use from exposure to the treatment (about one-quarter to one-fifth of the expected effect based on engineering predictions). Based on May-October 2014 water billing data, we assumed the control group would consume, on average, 22.5 m^3 per month (SD=13.9) and the two treated groups would each consume, on average, 21.5 m^3 per month (SD=13.9). We ran the power analysis using the software program PASS and the ANCOVA method. The required total sample size was 1128. This power analysis is reported in the AEA Registry. The CATIE team had acquired data from 10 communities (N=2250 water customers). To save on field expenses (particularly travel time), the team decided to drop one of the small communities at random because the remaining nine communities could provide enough households to meet the target sample size. A community with 72 customers in 2014 was dropped (El Roblar). After randomization and installation, we ran a power analysis by simulation using pre-treatment data from the nine communities and the random-effects estimator that we planned to use to analyze the post-treatment data. The simulation used monthly water use data from May 2013 to October 2014. The simulated experiment's pre-treatment period was from May 2013 to May 2014 and the post-treatment period was from June 2014 to October 2014. The simulation assumed a sample size of 1,310 households spread over the nine communities in proportions similar to their distribution in the original dataset. The households in each community were randomly assigned into treatment and control groups, with the treatment group twice the size of the control group. Using our random-effects estimator with community and month dummy variables, we performed 1000 estimation replications for each water reduction effect size ranging from 1% to 10%. Setting the Type 1 error rate to 5%, we generated a power curve that shows the estimated power to detect varying levels of effect sizes (see Figure A.1). The power simulation implies that, with 80% power, our design can detect a treatment effect of a reduction in water use of about 6%-6.5%.

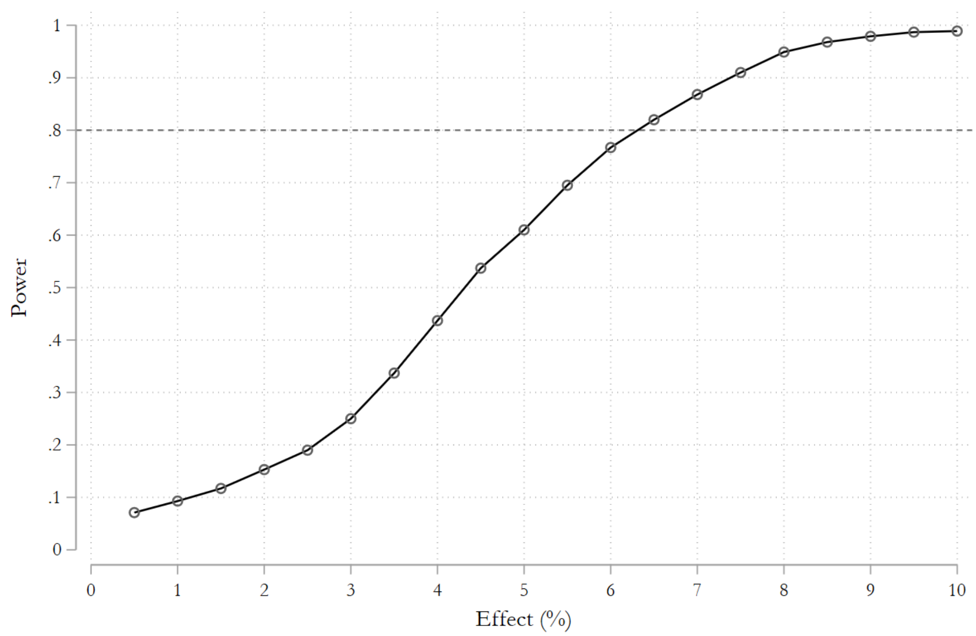

Figure A1. Power Analysis Curve.

A2. Calculations of Returns to Technology Adoption based on Basic Engineering Estimate (BEE)

We calculate costs and benefits by month (t), where $t = 0$ is the moment at which a household installs the water-efficient technologies. We assume each household installs the technologies in an hour and we value that hour by the minimum wage for unskilled workers in 2015: US$2.28 (Ministerio de Trabajo y Seguridad Social, 2015). The purchase price for the set of technologies that could be deployed in the average home is assumed to be the retail price in one of the Costa Rican stores that sold the products in 2015: US$23.71. Thus the total installation cost is assumed to be US$25.99, which is an optimistic assumption because it assumes trouble-free installation and sufficient plumbing skills to avoid having to hire a professional plumber. Later in the main text, we relax the trouble-free installation assumption and, using detailed data on installation costs, we adjust the installation cost to US$36.23. To calculate the benefits from technology adoption, we must define the path of monthly water use with and without the new fixtures for the expected lifespan of the fixtures. We assume the new fixtures all have the same expected lifespan and, to start, we assume this lifespan matches the manufacturer's warranty: 10 years. We relax this assumption in subsequent analyses. We assume that, in the absence of the new fixtures, a household would continue using the status quo technologies. To define monthly water use in the absence of the new fixtures (i.e., in the presence of the status quo technologies), we assume water use in a particular month for each household in the control group matches the average water use during the same month for the period 2013-2016 (except for October, November and December, for which the period of available data is 2013-2015). To define monthly water use in the presence of the new technologies, we assume that the technologies reduce the status quo monthly water use by the BEE and we assume the technologies are installed at the beginning of June, roughly the timing of installation in our experiment (i.e., the entire month of June is affected by the new technologies). To measure monthly expenditures based on water use with and without the technologies, we use the 2015 price schedules in each community. We assume that tariffs increase every three years by 20.85%, which is the average price increase in over one hundred CBWMOs in the province during the 2015-2017 period. We believe this growth rate is likely to be an overestimate of future price changes, making the returns to technology adoption look larger than they are. According to AyA staff, this period was an unusually active period of tariff increases encouraged by the government. We still prefer to use this estimate rather than a different one because it is based on actual tariff data of the region.

A3. Lead-lag Specification from Figure 2

To estimate the monthly effects of the technologies on water use, we estimate a random effects model with dummy variables that indicate, for treated homes, the month of technology installation (May or June 2015, M_{0i}) and each month before and after installation from May 2014 until September 2016 (M_{pi}):

$$\text{(A1)} \quad c_{it} = \beta_0 + \sum_{p=-13}^{16} \gamma_{pi} \cdot M_{pi} + community_k + install\_team_j + month_t + \epsilon_i + \mu_{it}$$

where c_{it} is the monthly water consumption in the ith household in calendar month t. In this specification, the parameter γ_{pi} can be interpreted as the effect of the treatment on monthly water consumption for each month before or after installation. As in the main specification (Equation 2), we also added dummy variables for the blocking variables (community and installation team) and for the month, and we assume that households are untreated in the month of installation (i.e., post-treatment period starts at M_1). Table A2 reports the results from this specification.

A4. Imputing Missing Audit Observations for Figure 4 in Main Text

The field teams were unable to contact and enter the homes of every treated household to do the audit: 10.9% of the households were unaudited only in 2015, 3.5% were unaudited only in 2016, and 2.5% were unaudited in both years. Failure to audit households typically occurred because no one was home or, less commonly, because a woman was home alone and did not feel comfortable letting the team into the house. Households that had at least one failed audit had pre-treatment water use of 22.08 m^3/month (SD=17.23) versus 25.43 m^3/month (SD=16.32) in fully audited households. Using only the data from homes that are in both audits, we observe that 51% kept all technologies until endline, 37% disadopted at least one technology between midline and endline, and 13% disadopted at least one technology before midline. To impute the missing audit status at midline for households unaudited in 2015 and audited in 2016 (10.9% of treated households), we make two assumptions about this subgroup:

1. If the household was observed with the technology in 2016 [33%], we assume they had the technology in 2015 at the time the audit took place. In other words, we assume that no one uninstalled the technology in 2015 and then re-installed it later. We believe this assumption is justifiable because the surveys imply that disadoption was driven by dissatisfaction with the technology and, in the sample of households that are in both audits, we observe only 15 households in which a fixture technology was uninstalled by the 2015 audit and then re-installed by the 2016 audit.

2. If the household was observed without the technology in 2016 [67%], we assume they were without technology in 2015 at the time the audit took place. With this assumption, we may mistakenly classify some Late Disadopters as Early Disadopters.

For households audited in 2015 and unaudited in 2016 (3.5% of treated households), we make the two assumptions about this subgroup:

3. If the household was observed without the technology in 2015 [3%], we assume they were without the technology in 2016 at the time the audit took place. In other words, like in Assumption (1), we assume that no one uninstalled the technology in 2015 and then re-installed it later. The same justifications that make Assumption (1) credible are applicable to assessing the credibility of Assumption (3).

4. If the household was observed with the technology in 2015 [97%], we assume

they were without the technology in 2016 at the time the audit took place. With this assumption, we may mistakenly classify a Perfect Complier as a Late Disadopter.

For households unaudited in both years (2.5% of treated households), we assume they are missing independent of potential outcomes and exclude these households from the imputation procedure. Although this assumption is strong, the proportion of the sample in this subgroup is small and, if anything, the assumption favors the returns to technology adoption because these households tend to be low water users (often because the homes are vacant). The treatment effect would be expected to be lower among low water users and that expectation is consistent with the estimates from a quantile regression estimator: the estimated treatment effect is lowest for the bottom quantile. Note that the analysis in Section V.F of the main text does not rely on these imputations. Only the values in Figure 4 rely on them, which is used simply to give the reader a sense of the disadoption patterns in the experiment.

A5. Alternative Calculations of the Effect of Disadoption on the Gap between the EEE and the Experimental Estimate

In the main text, we calculated disadoption's potential contribution to the gap between the EEE and the experimental estimate using the estimated treatment effect (ITT) from the first month after installation, which comes from the lead-lag specification in Section A3. Based on the assumptions we made, we interpreted that estimated effect as reflecting an upper bound on the ATE had we been able to force all households to keep their technologies until the endline. In the main text, we also divided that value by an upper bound estimate of non-compliance in the first month to calculate a slightly larger complier average treatment effect for the first month.

Using the same calculation method but only the control group and the treated households that did not get a bonus, we obtained similar results as indicated in the main text. The estimated treatment effect (ITT) from the first month after installation is -3.14 m^3 which implies a reduction of 12.8%. In this group, 3.7% homes disadopted one or more fixtures during the first month after installation. If we assume that these households disadopted the fixtures immediately after installation, then the complier average causal effect for the first month is -3.26 m^3, which corresponds to a 13.3% reduction. Based on these calculations, disadoption could explain, at most, 36.7% of the gap between the EEE and the experimental estimate.

Another way to calculate disadoption's potential contribution to the gap uses more months of data and yields a similar result to the one reported in the main text. This calculation starts with the estimated monthly ITT for the first four months after installation: -2.56 m$^3/month$ (estimated from the lead-lag specification in Section A3). As noted in the main text, in the midline audit, households self-reported the month after installation that they disadopted a fixture for the first time. Those data imply that 18 homes disadopted during the first month (2.4%), 18 homes disadopted during the second month (2.4%), 18 homes during the third month (2.4%), and 24 homes during the fourth month (3.2%). In other words, 10.4% of households disadopted at least one fixture during the first four months after installation. Based on a random audit of control households (Section III, main text), we assume no control households adopted the technologies. Next, we make two assumptions:

- *No partial disadoption.* This implies that "technology use in month j" is a binary variable – a household either uses all the technologies for the entire month or they use none of the technologies for the entire month. Implicit in this assumption is a "no return" assumption: once a household disadopts a technology, they do not re-adopt it later (an assumption that seems plausible given our survey data indicated only a few cases of such a pattern).

- *No heterogeneity in treatment effects across user types and no waning or growth in the monthly treatment effect.* The "no waning or growth" part of the assumption is explained in the main text. The no heterogeneity in treatment effects across user types is a new assumption that makes the calculations below easier. If, instead, the households that disadopted earlier had smaller treatment effects than households who disadopted later, we would over-estimate the target ATE. If the pattern of treatment effects were the opposite, we would under-estimate the target ATE (it's unclear why households with the largest water savings would be the first to disadopt).

We also assume that randomization of treatment is a valid instrumental variable. In other words, we make two additional assumptions:

- *Monotonicity.* The duration of technology use would be as long or longer under assignment to the treatment condition as under the control condition.
- *Excludability.* Randomization has no effect on potential water use except through its effect on treatment status.

We believe these latter two assumptions are plausible in our context given the nearly 100% compliance with treatment assignment (installation), the small size of the compliance bonus for the bonus treatment arm ($\sim$ US\$40), and the fact that both treatment and control households heard the same script about water efficiency prior to randomization. With these assumptions, we can calculate a complier average causal effect for the four months after installation (a local average treatment effect):

(A2)
$$CACE_{4m} = -\frac{2.56 \cdot 4}{0.024 \cdot 0 + 0.024 \cdot 1 + 0.024 \cdot 2 + 0.032 \cdot 3 + 0.896 \cdot 4} = -2.73$$

This estimand is the average return to a month of technology use among the compliers (i.e., households who use the technology for as long as they did in the experiment when randomized to the treatment group and do not use it otherwise). Given our "no waning or growth" assumption, we can infer that this value, which is similar to the value in the main text, is the average monthly reduction in water use that one would observe if one could force all households to keep their technologies. In the calculation above, we implicitly make one additional assumption that applies to the households missing from the midline audits (Section A4). Even if we used the imputation rules from the Section A4, we cannot identify which month during the first four months a missing household may have disadopted the technologies. Given that the imputation had little effect on the estimated percentages in Figure 4, we instead assume that that had we been able to audit these homes, we would have seen disadoption in proportion to what we saw in the audited homes (e.g., 2.4% of the missing households would have disadopted their first fixture in the first month). Alternatively, we could make the most conservative assumption one could make to address the missing audits: assume all missing households disadopted all of their technologies in the first month. Under this alternative assumption, we update the disadoption patterns: 15.5% disadopted during the first month ([18+116]/864), 2.1% during the second month, 2.1% during the third month, and 2.8% during the fourth month (i.e., 77.5% are perfect compliers rather than 89.6%). With these values, we can compute an upper bound on the CACE at midline:

(A3)
$$CACE^{UB}_{4m} = -\frac{2.56 \cdot 4}{0.155 \cdot 0 + 0.021 \cdot 1 + 0.021 \cdot 2 + 0.028 \cdot 3 + 0.775 \cdot 4} = -3.16$$

In other words, if we make the strong assumption that the missing midline audit households all disadopted immediately after installation, disadoption could explain up to a maximum of 33.8% of the gap between the experimental estimate and the EEE in Figure 3.

If we apply this same method and only consider the treated households that did not receive the bonus, we reach a similar conclusion. In this case, the estimated monthly ITT for the first four months after installation is -2.44 $m^3/month$. In this group, 13 homes disadopted at least one technology during the first month (3.7%), 14 homes disadopted during the second month (3.9%), 11 homes during

Table A1—Installation

	Units	Shower	Kitchen aerator	Bathroom aerator
Number of households with 0, 1 or 2 fixture units available	0	27	214	403
	1	791	640	426
	2	52	16	41
Number of households with 0, 1, or 2 technologies installed	0	72	284	467
	1	760	575	383
	2	38	11	20
Installation success rate*		93%	89%	83%

Note: * Installation success rate is the proportion of fixtures in treated households where the field team was able to install the new technologies. The percentages in the last row are higher than the percentages in the penultimate row because, in homes with two fixtures of a particular type, the field team could successfully install the new efficient technologies on one of the fixtures but not the other. Only six households had at least one fixture but no successful installations.

the third month (3.1%), and 18 homes during the fourth month (5.1%). Using these values we calculate the complier average causal effect for the four months after installation: $CACE_{4m} = -2.69$. Assuming that all missing households disadopted the technologies in the first month, the complier average causal effect becomes $CACE_{4m}^{UB} = -3.23$. This means that disadoption could explain up to 35.8% of the gap.

Table A2—Estimated Treatment Effects of Technology Adoption on Water Consumption (m^3/month) per Month

Month	Coefficient	95% Confidence Interval	
		Lower Bound	Upper Bound
-13	1.4984	-0.5371	3.5338
-12	0.9165	-0.8812	2.7142
-11	0.6056	-1.0891	2.3004
-10	0.9025	-0.7590	2.5640
-9	1.1113	-0.5414	2.7640
-8	0.0562	-1.4945	1.6069
-7	-0.4269	-1.9132	1.0593
-6	0.5934	-1.0362	2.2230
-5	-0.4797	-2.3229	1.3635
-4	-0.4591	-2.3310	1.4127
-3	0.8055	-1.1379	2.7489
-2	-0.5472	-2.3988	1.3044
-1	1.2357	-0.5627	3.0340
0	-0.5069	-2.1824	1.1686
1	-2.9506	-4.5784	-1.3228
2	-3.1394	-4.7887	-1.4901
3	-1.9628	-3.5622	-0.3634
4	-2.1903	-3.7623	-0.6182
5	-1.4597	-3.0194	0.1000
6	-1.4609	-3.1442	0.2224
7	-2.5070	-4.3102	-0.7039
8	-1.0123	-2.9673	0.9426
9	-0.8917	-2.8535	1.0702
10	-1.7226	-3.6463	0.2011
11	-1.6398	-3.4237	0.1441
12	-2.1233	-3.7696	-0.4770
13	-1.3175	-2.9845	0.3495
14	-1.4867	-3.2039	0.2305
15	-2.5439	-4.2331	-0.8546
16	-0.2454	-2.7569	2.2661

Note: Month 0 is the month of technology installation. 95% confidence intervals are constructed from robust standard error estimates clustered at household level. The treated units in 13th month before treatment comprise only the June 2015 installations. The treated units in 16th month after comprise only the May 2015 installations.

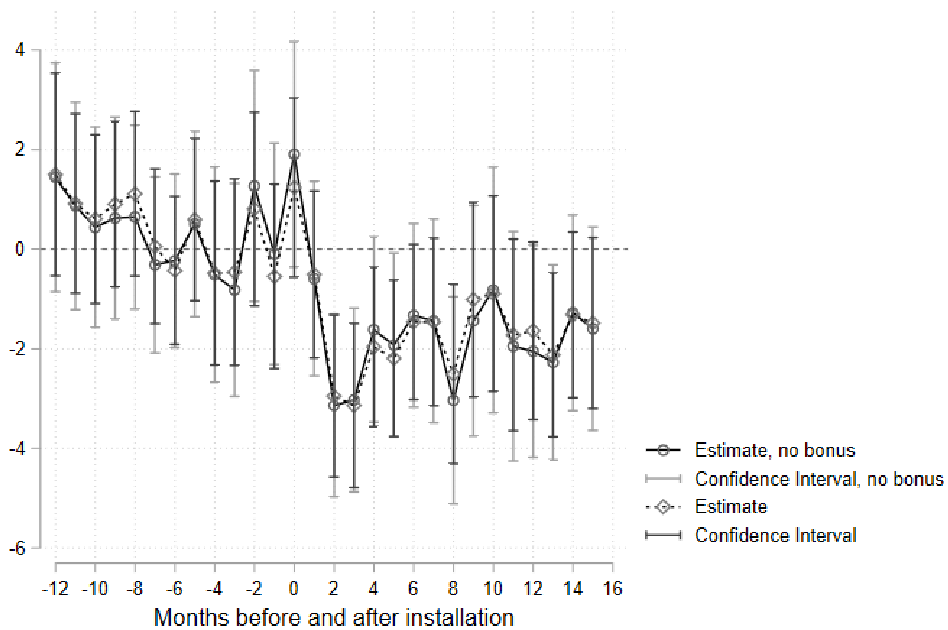

Figure A2. Estimated Treatment Effects per Month, Whole Sample and No Bonus (m^3).

Note: We compare monthly average treatment effects using the whole sample and the no-bonus treatment arm. Month 0 is the month of technology installation. The dots indicate the estimated treatment effects in each month, while the lines represent the 95% confidence intervals.