Environment for Development

Discussion Paper Series April 2021 ■ EfD DP 21-05

Input Efficiency as a Solution to Externalities

A Randomized Controlled Trial

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Abstract

Resource-conserving technologies are widely reported to benefit both the environment and the people who adopt them. 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."

Keywords: common pool resource, efficiency gap, field experiment, product adoption puzzle

JEL Codes: D0, Q0, O0

AEA Register #: RCT ID AEARCTR-0007158

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APPENDIX
Alpizar, Bernedo Del Carpio & Ferraro
Input Efficiency as a Solution to Externalities: a randomized controlled trial

Data and Code Availability: The data and code for reproducing all analyses in this study are available at the project's Open Science Framework page: https://osf.io/[to be made public upon publication]

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. This contrast was chosen under the assumption that, in the downstream 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 m3 per month (SD=13.9) and the two treated groups would each consume, on average, 21.5 m3 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 the one 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 (see code posted on our OSF project page; filename Power_analysis_simulation.do). 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 1310 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 Fig. A1). 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%.

A2. Engineering Calculation: Basic Engineering Estimate (BEE)

To calculate the BEE, we use 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 of flow rates with the new technologies, the field team followed the standard engineering approach of using the laboratory-rated flows as they appeared on the product labels. For the second input of flow rates with the status quo technologies, the team collected primary data rather than use secondary data. The team collected field data from a random sample of households (N=67) from the experimental 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). By collecting field data on the status quo technologies, the team avoided concerns raised by Davis, Fuchs and Gertler (Davis et al. 2014) that the "realization gap" between projected and realized input savings associated with more efficient technologies can partly be explained by underestimates of the efficiency of the technologies being replaced. For the third input about fixture contributions to total water use, the team again relied on primary data rather than secondary data that may not reflect the use patterns in the target communities. The team randomly selected 23 of the 67 households that were used to gather data on flows with the status quo technologies (2-3 per community, depending on population size). The team 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. With these data, one can estimate the fraction of total water use that flows through each fixture, on average.

Using these three measures, one can calculate the BEE:

$$\sum_{i=1}^{3} AW_i * DF_i \qquad (1)$$

where *i* is the fixture category (shower, bathroom or kitchen fixture), AW is the average percentage of water that runs through fixture category *i*, and DF is the difference in average maximum flow rates with and without technology installed in fixture category *i*. See OSF project page for data and calculations (filename: Engineer Estimate_osf).

A3. Calculations of Returns to Technology Adoption based on Basic Engineering Estimate (BEE)

See OSF project page for all cost and benefit data and calculations (in *Welfare Calculations* folder).

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: USD $2.28 (Poder Ejecutivo 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 two stores in the capital that sold the products in 2015: $23.71 (all dollar values are in 2015 US dollars). Thus the total installation cost is assumed to be $25.99, which is an optimistic assumption because it assumes trouble-free installation, sufficient plumbing skills to avoid having to hire a professional plumber, and zero costs for transporting the technology from the capital to communities (i.e., no mark-up for offering the product locally). In the main text (5.2), we relax the trouble-free installation assumption and, using detailed data on installation costs, we adjust the installation cost to $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 matches the average water use in the experimental control group 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 comes from 2015-17 data on price changes in over one hundred CBWMOs in the province. 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 of time was an unusually active period of tariff increases encouraged by the government).

A4. 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 each treated home, the month of technology installation (May or June 2015, M_{0i})[1] and each month before and after installation from May 2014 until September 2016 (M_{pi}):

$$c_{it} = \beta_0 + \sum_{p=-13}^{16} \gamma_{pi} * M_{pi} + community_k + install\_team_j + month_t + \epsilon_i + \mu_{it}$$

where c_{it} is the monthly water consumption in the *i*th household in month *t*. As in the main specification (Equation 1), we also added dummy variables for the blocking variables (community and installation team) and for the month, and we assume that households are

[1] Installations in 166 homes were done in first week of July. For simplicity, we label them as June installations.

untreated in the month of installation (i.e., post-treatment period starts at M_1). Table A.4 reports the results from this specification.

A5. Estimating Field Performance of Technologies

To adjust the BEE, two additional field measures on flows were taken. First, in the 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.

A6. Estimating Proportion of Water Use that Requires a Fixed Volume of Water

We estimated the proportion of water use that requires a fixed volume with data from 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. Each woman was paid ~$8 to complete the task and record the data on a pre-formatted form.

A7. 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. Using only the data from homes that are in both audits, we observe that 53% kept all technologies until endline, 35% 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 the 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 zero 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 there 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 following 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 there were without the technology in 2016 at the time the audit took place. With this assumption, we may mistakenly classify a 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 (not reported here, but in uploaded analysis code on OSF). Note that the analysis in Section 6.6 of the main text does not rely on these imputations; only the values in Figure 4 rely on them, which are simply used to give the reader a sense of the disadoption patterns in the experiment.

A8. Alternative Calculation 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 A5. 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 (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).

Here, we present another calculation that uses more months of data, but 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: -2.75 m^3/month (estimated from the lead-lag specification in section A6). 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 27 homes during the fourth month (3.6%). In other words, 10.8% of households disadopted at least one fixture during the first four months after installation. Based on a random audit of control households (Section 4), we assume no control households adopted the technologies.

Next we make two assumptions:

- "*No partial disadoption*", which 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 no re-adopt it later (an assumption that seems plausible given our survey data indicated no examples 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 treatment effects 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) and
- *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, the small size of the compliance bonus for the bonus treatment arm (~$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):

$$CACE^{4months} = -[2.75*4]/[0.024*0 + 0.024*1 + 0.024*2 + 0.036*3 + 0.892*4] = -2.94\ m^3$$

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 A7). Even if we used the imputation rules from the previous section, 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: 15.51% disadopted during the first month ([18+116]/864), 2.08% during the second month, 2.08% during the third month, and 3.13% during the fourth month (i.e., 77.2% are perfect compliers rather than 89.2%). With these values, we can compute an upper bound on the CACE at midline:

$$\text{Upper Bound CACE}^{4\text{months}} =$$

$$[-(2.75*4)/(0.1551*0 + 0.0208*1 + 0.0208*2 + 0.0313*3 + 0.772*4)] = -3.39 \text{ m}^3$$

In other words, if the missing midline audit households all disadopted immediately after installation, disadoption could explain up to 42% of the gap between the experimental estimate and the EEE in Figure 3.

References

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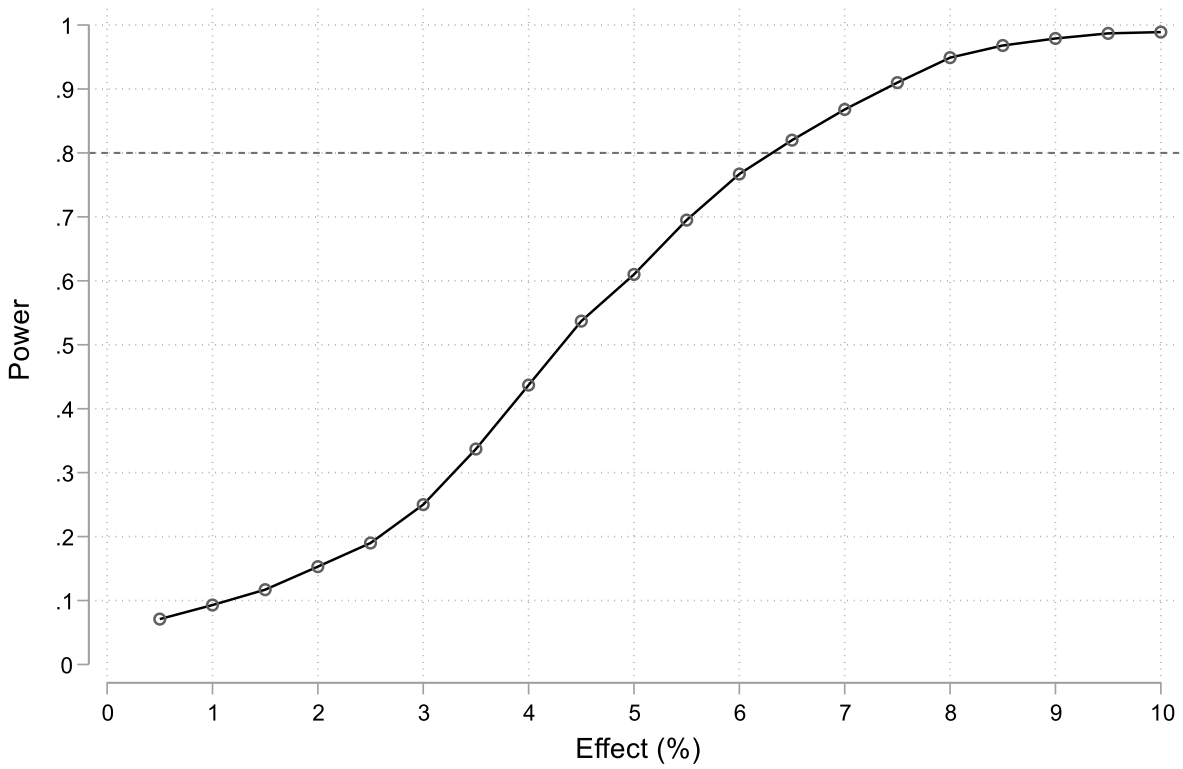
Poder Ejecutivo. 2015. *Decreto Nº 39055 Del Ministerio de Trabajo y Seguridad Social*. La Gaceta No.127, San José, Costa Rica.

Appendix Figure

Figure A.1. Power Analysis Curve



Appendix Tables

Table A.1 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%
At least one technology installed in each home with at least one fixture		95%	89%	86%

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Installation success rate*		93%	89%	83%
At least one technology installed in each home with at least one fixture		95%	89%	86%

* It is the proportion of fixtures where we were able to install a technology.

Table A.2: Estimated Treatment Effect of Technology Adoption on Water Consumption (m3/month) using a Balanced Panel

	(1)	(2)
	m3	m3
Treatment effect	0.15	-2.17
	[-1.43,1.73]	[-2.99,-1.35]
Observations	14,988	36,221
Effect in % of control group water consumption		
Treatment effect	0.60	-8.78

Column 1 presents the estimated effect of treatment assignment on pretreatment water use (May 2014-April 2015). Column 2 presents the experimental treatment effect estimate. In brackets are 95% confidence intervals, constructed from robust standard error estimates clustered at household level.

Table A.3: Estimated Treatment Effect of Technology Adoption on Water Consumption (m3/month) using Cross-Sectional Data

	m^3
Treatment Effect	-2.21
	[-3.03,-1.38]
Observations	1302
Effect in % of control group water consumption	
Treatment Effect	-9.02

Estimation includes average consumption in the pre-treatment period (May 2014-April 2015), community and interviewer fixed effects. In brackets are 95% confidence intervals, constructed from robust standard error estimates.

Table A.4: Estimated Treatment Effects of Technology Adoption on Water Consumption (m3/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.

1. Introduction

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)).

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; Houde and Myers 2019)). Yet much of the field data are analyzed using 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 Palmery 2014). Non-experimental difference-in-differences designs reduce such biases, but they are much rarer and still may not be able to adequately control for time-varying confounders that drive technology adoption and resource use (e.g., Allcott and Greenstone 2017; Bennear, Taylor, and Lee 2012; Davis, Fuchs, and Gertler 2014; Pfeiffer and Lin 2014).

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 2020; 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 2016a; Fowlie, Greenstone, and Wolfram 2018). Furthermore, some field experiments have been criticized by technology proponents for low adoption (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 Domestic Product is high[3] (e.g., the International Finance Corporation reports more than $307

[1] We focus on RCTs that test how changes in input efficiency affect input use. We therefore exclude: (i) experiments that impute, rather than observe, changes in resource use; (ii) experiments that do not isolate the effects of adopting more efficient technologies on resource use (e.g., changes to prices, in-home displays, audits and other forms of information transfers, or peer comparisons, which can affect resource 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.
[3] https://data.worldbank.org/indicator/EG.GDP.PUSE.KO.PP?most_recent_value_desc=true
https://data.worldbank.org/indicator/ER.GDP.FWTL.M3.KD?most_recent_value_desc=true

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; IFC 2016).

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. Few experimental studies estimate the private welfare effects of input-efficient technology adoption (exceptions are Carranza and Meeks 2016b; Fowlie, Greenstone, and Wolfram 2018).

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

The trial took place in the middle-income nation of Costa Rica. 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 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 compliance 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; 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 occur after the technology's efficiency is enhanced. 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.

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 evidence of a product adoption puzzle in an analysis that incorporates the experimental estimate of impact, risk aversion and adopter uncertainty about technology lifespan and performance, and subjective discount rates. In that analysis, the expected welfare gain is small and negative.

In the next section, we describe the study context. In Section 3, we present the prospective engineering estimate and welfare calculation. In Section 4 and Section 5, we present the experimental design, the experimental estimate, and the revised welfare calculation. In Section 6, we explore potential reasons for the divergence between the experimental and engineering estimates. Section 7 concludes.

2. Study Context

2.1 Recruitment of Study Communities

The RCT was part of a larger Canadian government-funded research project on climate change adaptation and water scarcity in Central America. 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). 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 teams 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 technology 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 in 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, makes these communities a "favorable case" for water-efficient technology impacts.

2.2 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). 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). During the study period, the offered technologies were not available in hardware stores in rural Central America. They were not common even in urban areas of Costa Rica, including the regional capital (Liberia). We found no wholesale sellers and only two retail stores selling limited numbers of shower heads in San Jose, the capital of Costa Rica, which is at least five hours away from the study communities. No surveyed home in the study area had such technologies prior to the experiment.

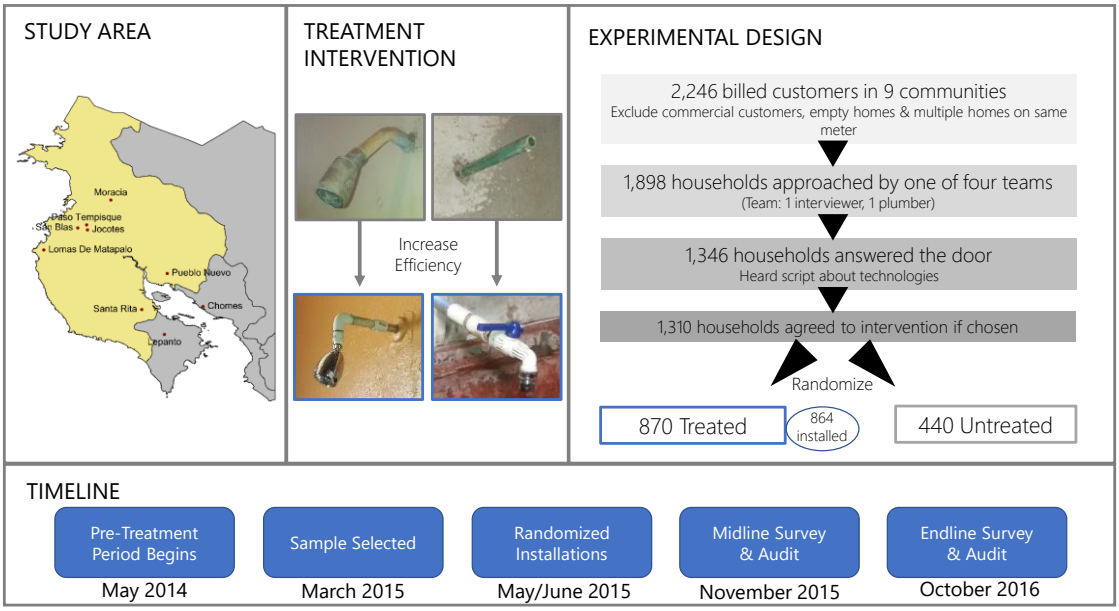

Figure 1. Study Context and Design. Left panel: Study site in the provinces of Guanacaste (yellow) and Puntarenas (gray) with study communities indicated by red dots. Middle panel: Less-efficient, status quo technologies (top) are replaced with more-efficient, new technologies (bottom), sometimes requiring additional plumbing parts to complete the installation. Right panel: Experimental design. Bottom panel: Study timeline.

3. Prospective Engineering Calculations

3.1 Effect of Technology Adoption on Water Use

The CATIE team first calculated a basic engineering estimate of impact (BEE), the calculations for which are described in the Appendix (A2). 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 (i.e., micrometer data from a random sample of households), 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. As is standard in engineering estimates, the BEE calculation implicitly assumes that the plumbers can install the entire technology package in every household and that all in-home water use from a fixture is affected by changes in efficiency. We relax these assumptions in Section 6.

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 post-treatment period. This estimate 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).

3.2 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 costs of installation, we use retail prices of the technologies and expected installation time (based on field data) multiplied by a prevailing wage rate (see Appendix A3 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 $220. For comparison, the daily minimum wage of an unskilled worker in Costa Rica in 2015 was roughly $18. Given that no households had the technologies prior to the experiment, this value implies a product adoption puzzle.

4. 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: customers that 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 for contact.

To contact the 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 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 their 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.

Of the 1346 households, 1310 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 also offered a performance bonus of $38 if they still had all technologies installed when the team returned unannounced sometime in the following six months. A proportional bonus was paid if some, but not all, of technologies were still installed at the time of the audit.

In our analysis, we combine the two bonus arms. The bonus 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 A.1.

4.1 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 four to five 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 two audits of treated households (November 2015, October 2016). Those data are presented in Section 6.

Table 1. Summary Statistics by Treatment Condition

	(1) Treated		(2) Control		(3) All	
	Mean	SD	Mean	SD	Mean	SD
Number of household members	3.67	1.78	3.57	1.73	3.64	1.77
Number of showers at home	1.03	0.3	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 colones*	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	24.85	14.17	24.41	13.57	24.7	13.97
Pre-treatment water consumption (m^3)	0.39	0.49	0.4	0.49	0.39	0.49
Observations	870		440		1310	

*In May 2015, this value was roughly the official monthly minimum wage for unskilled workers: 286,467 colones (USD $544).

4.2 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, the estimand 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:

$$c_{it} = \beta_0 + \beta_1 * post\_treated_{it} + community_k + install\_team_j + month_t + \epsilon_i + \mu_{it} \quad (1)$$

where c_{it} is the monthly water consumption in the *i*th household in month *t* and $post\_treated_{it}$ is a treatment dummy variable, which equals 1 in the months after installation of the technology package. The estimator includes dummy variables for the blocking variables (community, installation team) and, to increase the precision of the estimate, dummy variables for the month.

5. Experimental Results

5.1 Effect of Technology Adoption on Water Use

Column 2 in Table 2 reports the estimated treatment effect. 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 monthly water use by 9.1%, or about 0.14 SD. The panel is unbalanced, with 4.7% of the sample having some missing monthly water consumption. We obtain similar results using only the balanced panel (Table A.2). The estimate is almost the same if we were instead to use a cross-sectional, OLS regression estimator using the average monthly post-treatment consumption as the dependent variable and the blocking variables and average

monthly pretreatment consumption as covariates (2.21 m3, or a reduction of 9.0%; see Table A.3).

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

	(1) m^3	(2) m^3
Treatment Effect	0.41	-2.21***
	[-1.13,1.96]	[-3.02,-1.41]
Observations	15,535	37,509
Effect in % of control group water consumption		
Treatment effect	1.68	-9.06

Note: Column 1 presents the estimated effect of treatment assignment on pretreatment water use (May 2014-April 2015). Column 2 presents the experimental treatment effect estimate. In brackets are 95% confidence intervals, constructed from robust standard error estimates clustered at household level.

Recall that the engineering estimate (BEE) is 27.7% (-6.84 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 A4 for details). The estimated effect is near zero in the months before treatment assignment and then becomes negative (~-12%) 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 exact 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].

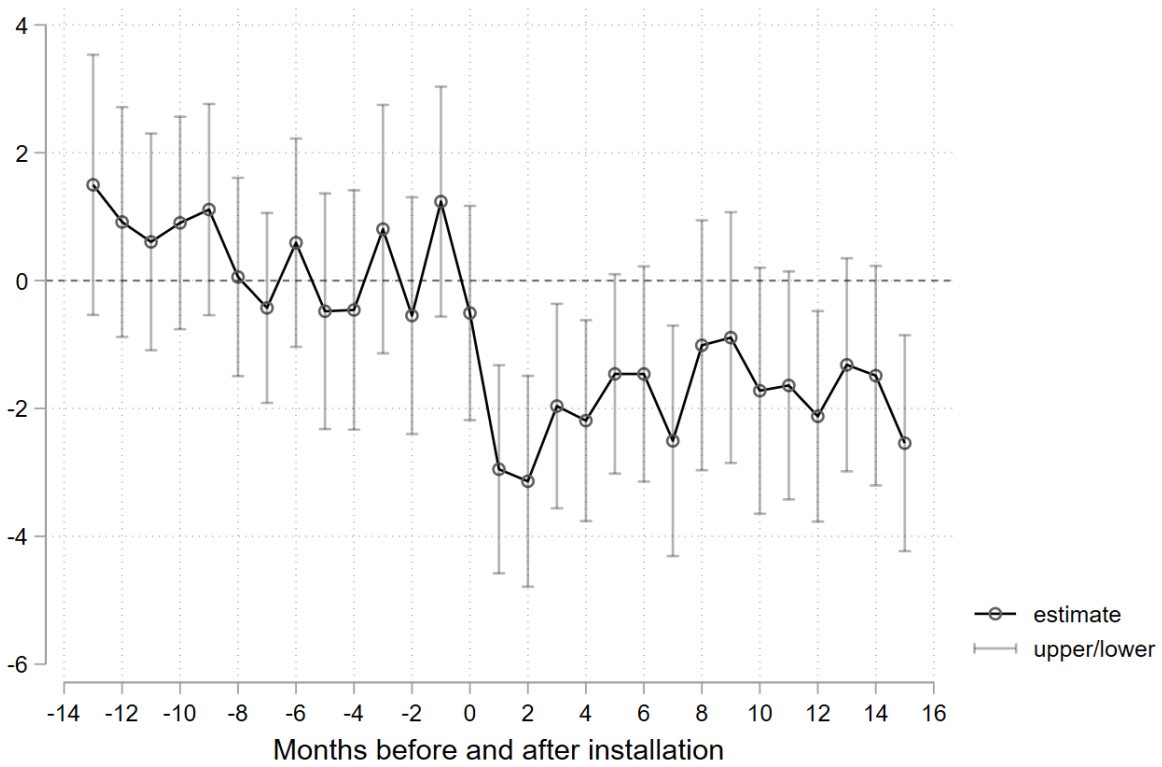

Figure 2. Estimated Treatment Effects per Month (m3). The dots indicate the estimated treatment effects, while the grey lines represent the 95% confidence intervals.

5.2 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 that calculation in the following ways:

(1) *Impact Assumption*: Instead of using the BEE, we use the experimental estimate of the ATE;

(2) *Installation Cost Assumption*: Instead of assuming trouble-free installation according to manufacturer guidelines, we use detailed field data on the time and materials required for the installations and calculate a more realistic installation cost estimate 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); and

(3) *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).

These changes in assumptions reduce the net present value of technology adoption by 98% (Table 3, row 2): from $220.12 to $5.02, which is roughly 30% of the daily minimum wage of an unskilled worker in Costa Rica in 2015.

Next, we make more realistic assumptions about uncertainty and household risk and time preferences. To develop more realistic assumptions about time and risk preferences, we draw on (Bernedo, Alpizar, and Ferraro 2020), which reported experimentally elicited and econometrically estimated time and risk preference parameters using a double multiple price list elicitation 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 the 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%). If we assume that decision-makers are optimistic probability weighters, the perceived probability weights in the RDU model for the eight scenarios are: 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).

We define the discounted rank-dependent utility (DRDU) of the technology savings as:

$$DRDU = \sum_{t=0}^{T} \left(\frac{1}{1+\delta}\right)^{t/12} \sum_{a=1}^{8} w^a \left[U(s_t^a)\right] \quad (1)$$

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 life span (T). Using this framework, we calculate the expected welfare gain (EWG) as the discounted certainty equivalents (CE) of monthly savings:

$$\text{EWG}=\sum_{t=0}^{T}\left(\frac{1}{1+\delta}\right)^{t/12} CE_t \qquad (2)$$

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

With these more realistic assumptions about uncertainty and household risk and time preferences, we calculate 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 3 and 4), regardless of whether we assume probability weighting or whether we use the preference parameters of individuals or couples.

Table 3. Expected Welfare Gain from Technology Adoption

Impact Assumption	Lifespan Assumption	Installation Cost Assumption	Uncertainty	Discount Rate (δ) and Risk Aversion Coefficient (*r*) Assumptions	Probability Weighting Assumption	Present Value (in 2015 US$)
BEE	Warranty time	Trouble-free	No	δ = 0.07, *r* = 0	No	220.12
ATE	Reported by households	Field Data	No	δ = 0.07, *r* =0	No	5.02
ATE	Reported by households	Field Data	Yes	δ = 0.30, *r* = 0.81	No	(21.67)
				δ = 0.44, *r* = 0.77	No	(22.84)
ATE	Reported by households	Field Data	Yes	δ = 0.30, *r* = 0.81	Optimistic	(15.45)
				δ = 0.44, *r* = 0.77	Optimistic	(17.95)

Note: BEE is the basic engineering estimate (Section 3). ATE (Section 5) is the average treatment effect estimate. 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 A3). 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.

6. The Divergence between Engineering and Experimental Estimates

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

6.1 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, the interpretation of the estimated treatment effect in Figure 3 would change. A common violation of the assumption is when the control households, having observed the technologies in treated households, subsequently adopt the technologies (or similar ones). As reported in Section 4, 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 in the gravity-fed water systems. If the treatment reduced average water use among the treated group, the flow in the community system may have improved, thereby potentially increasing the amount of water consumed among households in the control group. Correcting for that form of interference, however, would make the difference between the experimental and engineering estimates grow, not shrink.

6.2 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). Using field data of actual flow rates of the new and old technologies as they are typically used by residents (see Appendix A5), we revise the BEE and obtain an expected 24.4% reduction in water consumption (Figure 3).

6.3 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. Once the field data on actual installation success and on field-measured flows (Section 6.2) are incorporated, the engineering estimate implies a 22.1% reduction in water consumption (Figure 3).

6.4 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 field data collected from a sample of households (see Appendix A6).

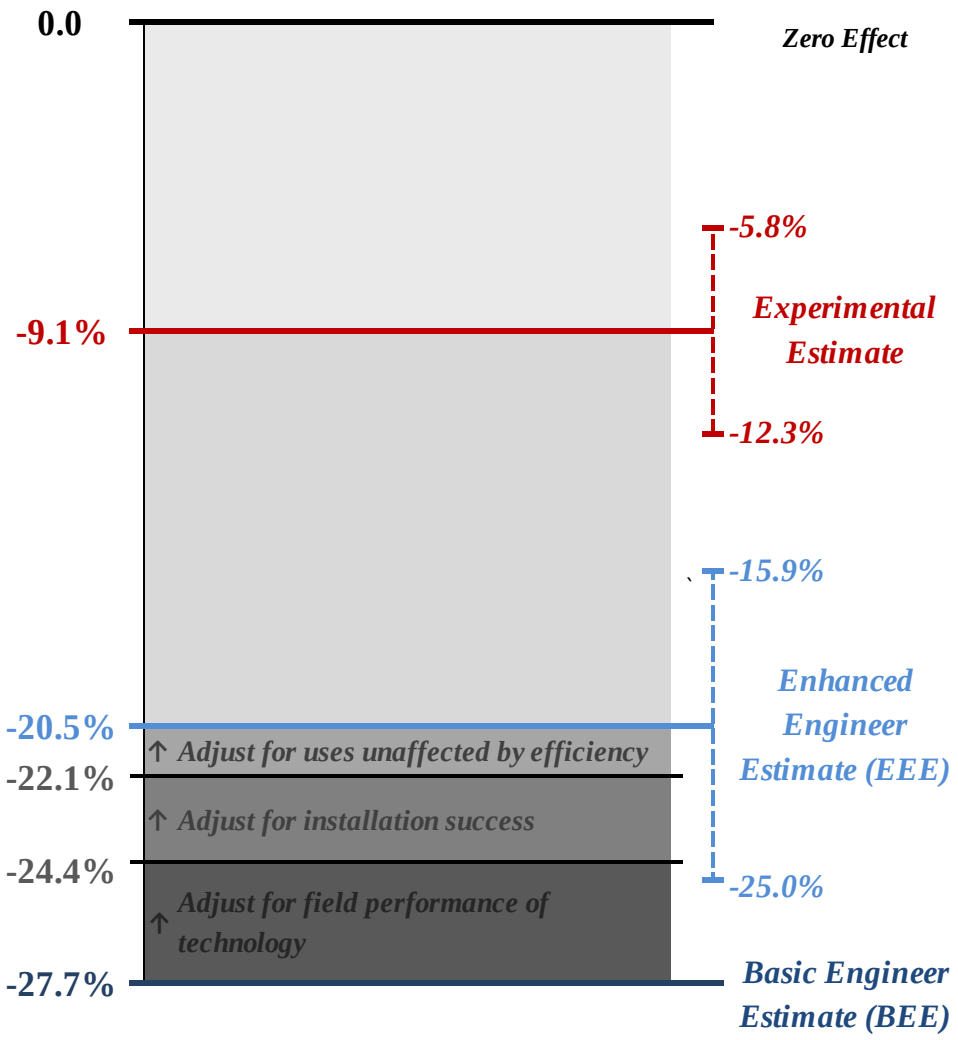

Figure 3. Reasons for Divergence between Engineering and Experimental Estimates of Effect of Technology Adoption on Water Use. The estimates are presented in terms of the estimated percent reduction in monthly household water use as a result of technology adoption. The BEE is derived from a conventional 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 6.1-6.3). The red and blue bars around the EEE and the experimental estimate are 95% CIs.

Using this estimate, and the estimates of actual field performance and installation success (sections 6.1 and 6.2), we obtain what we label the Enhanced Engineering Estimate (EEE). The EEE implies that the adoption of the technology is expected to reduce monthly water use by 20.5% (Figure 2), a value that is still more than double the experimental estimate and one that yields a product adoption puzzle (EWG=$56.29).

6.5 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.

6.6 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, 51% had disadopted one or more of the installed fixtures (Figure 4).[4]

		Midline	***Endline***
Perfect Compliers	***49%***	***Kept Technology***	***Kept Technology***
Later Disadopters	***33%***	***Kept Technology***	***Disadopted Technology***
Early Disadopters	***18%***	***Disadopted Technology***	***Disadopted Technology***

Figure 4. Patterns of disadoption at midline ("Early") and endline ("Later"). "Disadoption" means the household uninstalled one or more of the installed fixtures. Only 5% of households uninstalled all fixtures by midline, but 20% of households uninstalled all fixtures by endline.

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 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.) <u>had they not disadopted the technologies</u>.

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

[4] The team was unable to audit all treated homes. For the values reported Fig. 4, we impute the missing audit status (see Appendix A7). Considering only the values from homes observed in both audits, 53% kept all technologies until endline, 35% disadopted at least one fixture between midline and endline, and 13% disadopted at least one fixture before midline.

cannot directly estimate these average treatment effects in the counterfactual world where households do not disadopt the technologies. 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% (see Figure 2 and Appendix Table A4).

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 (-2.95/0.976), which also implies a 12% 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, about one-quarter 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 and the water use and midline audit data; in fact, the estimated contribution of disadoption is lower using these alternative calculations (see Appendix A8).

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 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 4, columns 1, 3 and 5). 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 (Columns 1, 2 and 4 in Table 4). 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 is fundamentally different: the behavioral response arises because of an undesired change in a product attribute that accompanies the improvement in efficiency, not because of a decrease in the effective price of the services provided by water (less than 1% of the respondents reported an increase in the frequency of using water-related services). Although the survey evidence is not definitive, this behavioral response is the most plausible behavioral reason we have uncovered for the remaining divergence between the EEE and the experimental estimate.[5]

Table 4. Perceptions of Technologies by Household Type

Variable			Perfect Compliers	Late Disadopters	Early Disadopters
Audit 2015	Activity takes longer	bathroom faucet	0.08	0.08	0.11
		kitchen faucet	0.27	0.31	0.31
		shower head	0.37	0.43	0.56
	Person likes flowrate	bathroom faucet	0.99	0.98	0.95
		kitchen faucet	0.95	0.92	0.56
		shower head	0.94	0.89	0.59
Audit 2016	Activity takes longer	bathroom faucet	0.1	0.19	0.18
		kitchen faucet	0.27	0.36	0.48
		shower head	0.36	0.48	0.6
	Person likes flowrate	bathroom faucet	0.98	0.92	0.79
		kitchen faucet	0.95	0.81	0.59
		shower head	0.56	0.92	0.75

[5] An alternative reason could be a version of moral licensing (*citation*), 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.

7. Conclusion

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 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 2020)).

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 to reduce pressures on the aquifers is unlikely to be successful.

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