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Do Behavioral Nudges Interact With Prevailing Economic Incentives?

Pairing Experimental and Quasi-Experimental Evidence From Water Consumption

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Working Paper 22-2
January 2022

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NOTE: These five sections (about the authors, about RFF, sharing our work, about the project, acknowledgements) can be rearranged as is necessary to make the layout look nice.

Do behavioral nudges interact with prevailing economic incentives? Pairing experimental and quasi-experimental evidence from water consumption

Daniel A. Brent and Casey J. Wichman*

Abstract

Behavioral nudges are popular instruments to promote prosocial behavior, particularly in settings with unpriced externalities. Nudges may interact with existing incentives, however, by crowding out intrinsic motivation to conserve or by increasing price salience. We investigate the interaction of prices and nudges for water conservation in two experiments in neighboring utilities. First, we layer randomized behavioral treatments on top of price variation driven by lot-size thresholds that exogenously assign marginal prices. Second, we explore whether behavioral treatments affect consumers' price elasticities. Our findings suggest that nudges do not induce more conservation at higher prices, nor do they increase price sensitivity.

Key Words: behavioral interventions, social norms, field experiments, water conservation, price sensitivity, water demand

JEL Codes: D12, C93, H42, L95, Q21, Q25

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1 Introduction

Behavioral interventions are widespread policy options for altering consumption choices. Governments, policymakers, and industries around the world now look to behavioral economics to manage private and social costs. Behavioral economics has inspired policies targeting a wide range of outcomes in areas such as tax evasion (Hallsworth et al., 2017), charitable donations (Croson and Shang, 2008; Shang and Croson, 2009), education (Levitt et al., 2016), healthy eating (Hanks et al., 2012; List and Samek, 2015), and exercise (Royer et al., 2015). These interventions have motivated, and in some cases are the output of, government-run "nudge units" such as the United Kingdom's Behavioural Insights Team.

Nowhere have behavioral nudges been more pervasive than for managing energy and water consumption (e.g., Allcott, 2011; Ferraro et al., 2011; Allcott and Rogers, 2014; Brent et al., 2015; Ito et al., 2018; Byrne et al., 2018). Regulated industries, such as electricity or water and sewer service, are limited as to how much they can use price as a conservation tool. In the state of California, for example, water utilities cannot charge a price greater than cost of service, effectively rendering scarcity pricing illegal.[1] As a result, utilities often rely on nonprice demand-management tools to encourage conservation. Researchers have shown that social comparisons can be effective nonprice policies for conservation, reducing household energy and water consumption between 2 and 5 percent (Allcott, 2011; Ferraro and Price, 2013; Brent et al., 2015). At scale, these small reductions can generate substantial benefits for the service provider at relatively low cost, potentially delaying or avoiding investment in costly new power plants or water supplies.

A notable feature of this literature on social comparisons is that the treatment estimates are causal, rising from the randomized nature of program designs implemented by companies such as OPower and WaterSmart Software. Reconciling these estimates with models of consumer behavior, however, is less transparent. Some have claimed that norm-

[1] See, e.g., http://www.latimes.com/local/orangecounty/la-me-rates-decision-20150421-story.html.

based information treatments apply a *moral tax* to consumption of externality-producing goods (Levitt and List, 2007; Ferraro and Price, 2013). Others have claimed that information treatments reduce the distortion in consumers' perceptions of price and quantity consumed, thereby reducing informational internalities—or optimization mistakes—borne by consumers (Allcott and Taubinsky, 2015; Wichman, 2017). Thus there are competing views of whether behavioral interventions affect an individual's intrinsic motivation to conserve, provide direct economic benefits to the consumer, or both.

In line with understanding the behavioral mechanism of underlying conservation behavior, we posit that behavioral policies may interact with prevailing market mechanisms in an ambiguous way. We show theoretically that existing interpretations of nudge treatment effects may confound behavioral and economic explanations if there is an economically significant interaction between prices and nudges by heightening the salience of consumption costs. The lack of a meaningful interaction with prices is consistent with moral motivations driving behavior, which can reduce any welfare gains (Allcott and Kessler, 2019). Within the current literature, there is virtually no evidence as to whether this interaction is meaningful. Our paper fills this gap.

We explore the impacts of two identical social messaging experiments and large differences in marginal prices on water conservation behavior. Our analysis produces causal effects by design: First, we evaluate the effects of two independent, randomized messaging experiments implemented by WaterSmart Software at different points in time for neighboring water utilities in Southern California. Second, we exploit two sources of variation that introduce price changes at the household level. One source of price variation comes from arbitrary lot-size thresholds within nonlinear water rate structures that we exploit in a regression discontinuity design. The second source of price variation arises from the utilities' rate-setting practices, included in an instrumental variables framework. Our methodology cleanly identifies the separate impact of the social comparison treatment and price on consumer behavior, as well as their joint effect.

Within our unique empirical approach, we answer two questions: First, do customers facing higher prices respond more strongly to norm-based conservation campaigns? We refer to this as the *price-level effect*. We identify the price-level effect by comparing responsiveness to behavioral treatments for otherwise identical households on either side of a price discontinuity introduced by arbitrary lot-size thresholds within a utility's rate structure. Second, do norm-based conservation campaigns increase customers' price sensitivity? We refer to this as the *price-sensitivity effect*. We identify the price-sensitivity effect by estimating demand equations and observing whether our randomized behavioral treatment significantly alters our estimate of the price elasticity. Both of the interactions between prices and nudges inform the underlying behavioral mechanisms through which nudges affect consumer behavior.

Our results show no consistent evidence that social comparisons generate more conservation among households facing an exogenously larger marginal price of water. Higher prices cause small and insignificant decreases in the magnitude of the treatment effect from peer comparisons. Additionally, we find similarly weak evidence for a price sensitivity effect. Treatment induces small increases in the magnitude of demand elasticity in some specifications, although these effects disappear in our preferred specifications.

Because norm-based policies are implemented broadly for water and electricity, the policy implications of this research are vast. Allcott and Rogers (2014) and Brent et al. (2015) both show that behavioral nudges interact with prevailing conservation policies. Additionally, Allcott (2015) shows significant heterogeneity in treatment effects, with larger treatment effects for utilities that participated earlier. There is also conflicting evidence about the efficacy of nudges for energy conservation when residents do not pay their bills (Myers and Souza, 2020; Brülisauer et al., 2020). Recent research shows that the mechanisms through which consumers respond to behavioral nudges have important welfare implications (Allcott and Kessler, 2019; Taylor et al., 2018). Nudges generate unambiguous welfare gains if consumers conserve as a result of correcting internalities.

However, if consumers respond because of a moral tax on consumption, then welfare increases only if the price of the resource is sufficiently below the marginal social cost. Strong interactions between nudges and prices would indicate that consumers are responding at least in part to increases in the private benefits from conservation, given the extensive evidence that consumers in these settings do not have full information about prices (Sexton, 2015; Wichman, 2017; Brent and Ward, 2019) or are not responding according to standard neoclassical theory (Sallee, 2014; Allcott and Wozny, 2014; Jacobsen, 2015). Therefore, although it is difficult to directly measure the welfare benefits of behavioral interventions, we find evidence that supports the fact that social comparisons operate as a moral tax on consumption.

A growing body of evidence, however, focuses on comparing the effects of moral and neoclassical incentives on energy and water consumption. Ito et al. (2018) explore the effectiveness of a standard moral suasion nudge relative to dynamic electricity pricing treatments. They find that moral suasion induces sizable effects in the short run that dissipate quickly relative to dynamic prices that exhibit longer-run effects. Our project is different in that we seek to understand how the moral suasion treatment interacts with *underlying* economic incentives. Additionally, Brandon et al. (2018) implemented a randomized OPower experiment in which personalized energy reports that targeted aggregate savings or peak-load savings were sent to electricity customers. The authors measured the response to these treatments during peak-load and non-peak-load events. They find that a combination of treatments induced a larger effect than the joint effect of each treatment in isolation—or in other words, that the treatments were complementary. This result suggests the importance of exploring other policy complementarities, particularly with respect to interactions with economic incentives, because nudges can highlight the private economic benefits of conservation. Additionally, West et al. (2019) show that strong fines for violating outdoor water restrictions do not influence the behavioral response from peer comparisons. Finally, List et al. (2017) show that economic incentives (via a rewards

program) can better target electricity consumption reductions from low-use, low-variance households, which are typically less responsive to nudges.

Importantly, electricity and water are often priced using nonlinear increasing-block rate structures where the economic benefits from conservation are positively correlated with consumption. Thus it is feasible that low-use, low-variance consumers respond to nudges differently because of different private economic returns from conservation. This effect is precisely what we seek to estimate in this paper.

Overall, we find little evidence that informational nudges interact with underlying economic incentives. This is an important result because nearly all behavioral public policy has the potential to interact with existing neoclassical incentives. Placed alongside the previously mentioned literature, our study provides a clearer view of the mechanisms underlying responses to behavioral treatments. Behavioral nudges can be criticized for providing too many types of information to isolate the relevant mechanism for consumer behavior, but we fail to find convincing evidence that making economic incentives more salient is a relevant factor in behavioral interventions. This finding sharpens our view of past and future conservation policies because nearly all behavioral nudges for electricity and water are layered on top of prevailing rate structures. Additionally, in situations where resource use goes unpriced, our results suggest that behavioral treatments can still be a useful policy instrument to govern consumer behavior in a socially desirable way.

2 Conceptual framework

To show how nudges and incentives interact conceptually, we begin with the general framework of Allcott and Kessler (2019). Consider a consumer with income y who gains utility from the consumption of water w and numeraire good x. w generates consumption utility of $f(w;\alpha)$, where α captures consumer tastes as a demand shifter. We include an internality parameter $\gamma > 0$ that affects choice but not experienced utility, such as imperfect information, mistakes in evaluating private benefits of water consumption, or

some other behavioral bias. For our purposes, it is useful to think of γ as inattention to water consumption. Consumers thus have perceived utility $\hat{f}(w;\alpha,\gamma)$, which we assume takes the form $\gamma^{-1}f(w;\alpha)$. Thus perceived utility is less than experienced utility for $\gamma > 1$ and greater than experienced utility for $0 < \gamma < 1$.

Following Levitt and List (2007) and Ferraro and Price (2013), we include a "moral utility" term, $M = m - \mu w$, which captures nonpecuniary impacts associated with consumption of w. We define $\mu \geq 0$ as a marginal "moral tax" on consumption of w.

We summarize individual-specific parameters in the vector $\theta = \{y, \alpha, \gamma, m, \mu\}$ so that the consumer maximizes

$$\max_{x,w} \hat{U}(\theta) = x + \gamma^{-1}f(w;\alpha) + m - \mu w \tag{1}$$

subject to their budget constraint

$$y = x + pw \tag{2}$$

where $p \geq 0$ is the marginal price for water consumption. The consumer allocates all nonwater expenditures to the numeraire, thus satisfying their budget constraint with equality. Because water is necessary for human survival, and the costs of self-supply are prohibitive, we are comfortable with ignoring corner solutions.

Standard first-order conditions govern the consumer's choice of water consumption, $\tilde{w}$:

$$f'(\tilde{w};\alpha) = \gamma(\mu + p). \tag{3}$$

Equation 3 states that consumers will choose consumption of $\tilde{w}$ to equalize their marginal experienced utility with the sum of perceived monetary and moral costs. Because γ introduces a wedge between experienced marginal utility and a consumer's true marginal utility, choice of $\tilde{w}$ is not required to be individually optimal. The framework so far is consistent with stylized formulations in Sexton (2015) and Wichman (2017) who model price (and quantity) misperceptions. The only difference is the inclusion of the Ferraro

and Price (2013) moral cost parameter.

We can express changes in consumption by totally differentiating equation 3:

$$f''(\tilde{w};\alpha)\mathrm{d}\tilde{w} = \mu\mathrm{d}\gamma + \gamma\mathrm{d}\mu + p\mathrm{d}\gamma + \gamma\mathrm{d}p. \tag{4}$$

Now, let the nudge be represented by changes in attention to water use ($d\gamma$) and changes in the moral cost of consumption ($d\mu$). Because the nudge does not affect the market price of water (i.e., $dp = 0$), we can express the demand effect of a nudge as

$$\mathrm{d}\tilde{w} = \frac{1}{f''(\cdot)}\left[(\mu + p)\,\mathrm{d}\gamma + \gamma\mathrm{d}\mu\right]. \tag{5}$$

Under standard assumptions of demand (i.e., diminishing marginal utility), f'' is weakly negative, which implies that the nudge will (weakly) reduce water demand in equilibrium for $\gamma < 1$.[2] Equation 5 shows that the total effect of the nudge depends on how changes in perceptions interact with moral and explicit prices as well as how changes in moral costs interact with perception. Most research to date assumes implicitly that the increased salience of private economic benefits of conservation is negligible; in other words, these studies interpret the effect of the nudge as if $p\mathrm{d}\gamma = 0$. That is, the majority of experiments focused on exploring the effects of salience or moral suasion ignore their underlying interaction with prices. Theoretically, this omission is a potentially important oversight because behavioral interventions for water and energy use are implemented on top of prevailing prices, which are often nonlinear. Furthermore, many nudges aimed at water and energy conservation, including the one analyzed in this paper, explicitly communicate the private financial benefits of conservation.

This simplified representation of demand translates directly to our first empirical hypothesis: the existence of an economically important interaction between behavioral treat-

[2]For clarity, we assume the nudge is corrective in that it reduces information distortions, or $d\gamma \implies \gamma \to 1$. For $\gamma > 1$, the nudge could increase consumption if, for example, consumers had been initially overperceiving the costs of consumption. This stylized result is captured empirically in Wichman (2017).

ments and conventional pricing mechanisms. We define this effect as the **price-level effect (PLE)**, which measures the magnitude of $\mathrm{d}\tilde{w}$ in response to the nudge that is driven by differences in price levels. Our null price-level hypothesis posits that $p\mathrm{d}\gamma = 0$. Evidence of a nonzero price-level effect would lend support to the notion that consumers change consumption in part because of increased salience of private economic benefits from conservation. We test this by comparing the effect of randomized nudges for households that face exogenously different marginal prices. We describe empirical identification in the subsequent section.

Additionally, we explore a second, complementary approach to investigate whether nudges affect consumer demand through neoclassical price mechanisms. Consider a change in price, $\mathrm{d}p$. Using equation 3, we can define the resulting price elasticity,

$$\hat{\varepsilon}^p = \frac{\gamma}{f''(\cdot)} \frac{p}{\tilde{w}} = \gamma \varepsilon^p \tag{6}$$

where ε^p is the neoclassical price elasticity of demand and the hat indicates "perceived" price elasticities. This formulation leads directly into our second hypothesis. We define the **price-sensitivity effect (PSE)** as the degree to which nudges affect price sensitivity. Because social comparisons operate through both channels of μ and γ, our null price-sensitivity hypothesis is $\partial \hat{\varepsilon}_p / \partial \gamma = 0$. Evidence of a nonzero price-sensitivity effect would suggest that consumers' sensitivity to price is affected by the nudge, thus providing support for the idea that consumers respond to nudges at least in part because of private economic benefits due to internality correction.

3 Empirical setting and strategy

3.1 Data

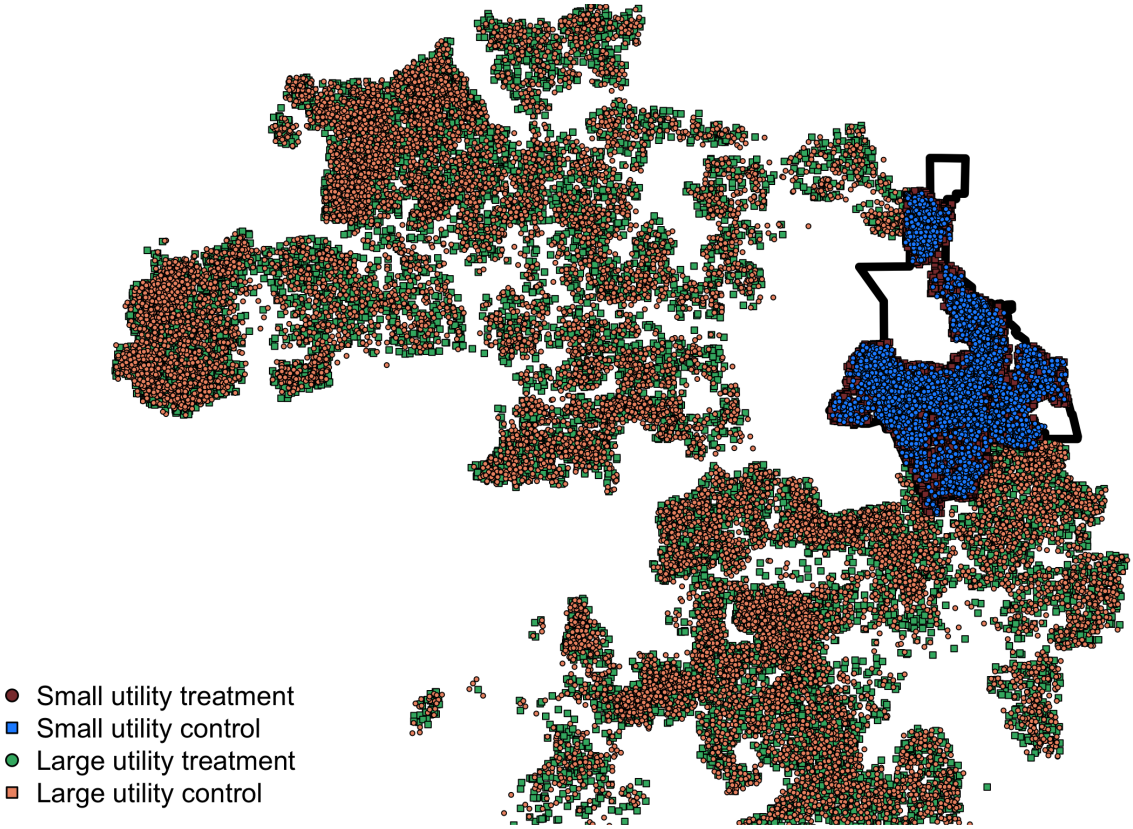
The data we use in the analysis are household-level water consumption records for two utilities in Southern California. We obtained these data through a partnership with WaterSmart Software (henceforth WaterSmart). We refer to the larger utility in our sample as the Large Utility and, correspondingly, the smaller utility as the Small Utility.[3] These two utilities share a geographic border, and their residents form a common labor market along with other nearby municipalities. Both utilities combine water and sewer services and also provide electricity. Figure 1 shows the geographic distribution of households in the treatment and control groups in each utility.

The Large and Small Utilities have different pricing structures, and the water rates have changed over time. The Large Utility has "budget-based" increasing-block rates in which consumption thresholds for the marginal price blocks vary with geographic region and lot size. This means different households will be assigned to higher marginal prices at different levels of consumption. There are three geographic zones: low, medium, and high. The geographic zones refer to the water requirements based on temperature conditions: the low zone has the most moderate weather and the high zone has hotter weather. There are 5 lot-size thresholds leading to 15 unique sets of consumption tiers that determine marginal prices. The Small Utility has a standard increasing-block rate structure where the consumption threshold for marginal prices are the same for all households. Figure 2 displays the full water rate structure over time for both utilities.

For each household in our sample, we have consumption for the given billing period and the relevant prices for consumption. Households receive water bills every two months. To protect anonymity, geographic coordinates for each household were scrambled, which permits us to identify the neighborhood of the household, but not its exact

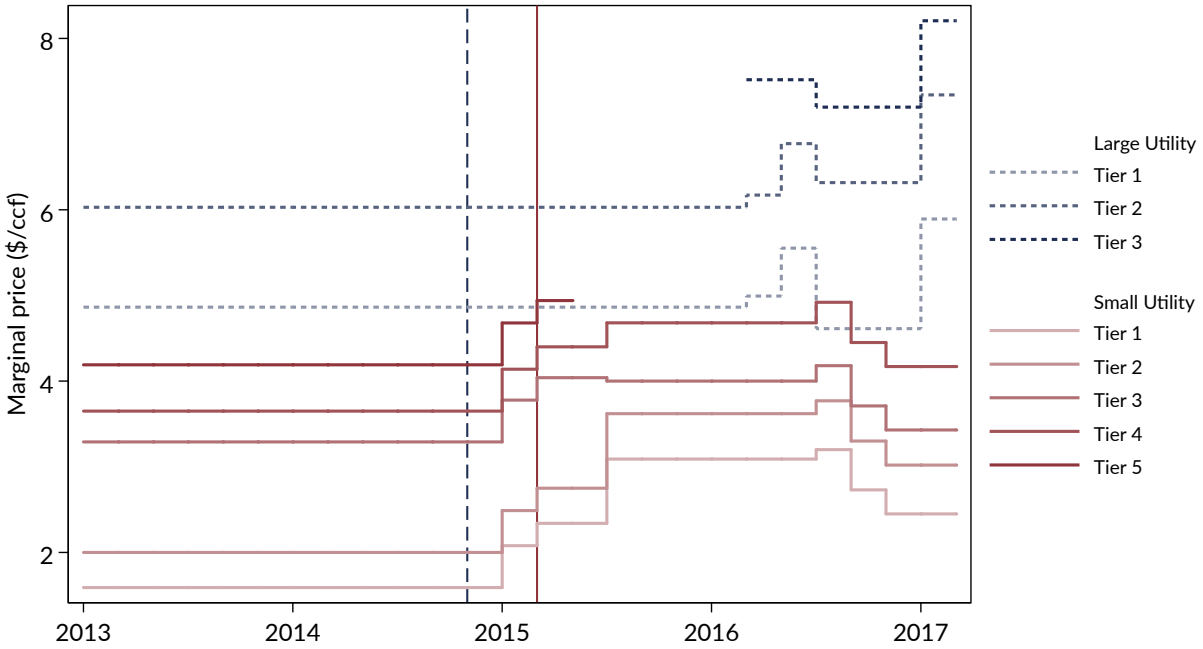
[3] As part of the confidentiality agreement we cannot disclose the names of these utilities.

Figure 1: Water Utility Service Area (Partial), Sample Boundaries, and Households by Treatment Status



Notes: The Small Utility is contained within the black border and the Large Utility is outside the border. Household locations are scrambled within 0.001 decimal degrees (a maximum of approximately 365 feet) to preserve anonymity.

Figure 2: Marginal Prices over Time



Notes: The colors depict the marginal price for different consumption tiers. The dashed lines show prices for the Large Utility and the solid lines show marginal prices for the Small Utility. The vertical dashed and solid lines depict the treatment start date for the Large and Small Utilities respectively.

address. Each account in our sample was randomized into a treatment or control group by WaterSmart Software. All households in each utility begin receiving Home Water Reports (HWRs) at the same time (details on randomization and treatment are described below). Households in both utilities are billed bimonthly, with six billing periods each year. Treatment began during the sixth billing period of 2014 in the Small Utility and during the second billing period of 2015 in the Large Utility.

3.2 Experimental design

WaterSmart is a smart-technology company that contracts with water utilities to help them manage demand.[4] In addition to providing analytical support to utilities, WaterSmart focuses primarily on helping utilities reduce water consumption by providing consumers with additional information through customized Home Water Reports (HWRs) (Figure 3)

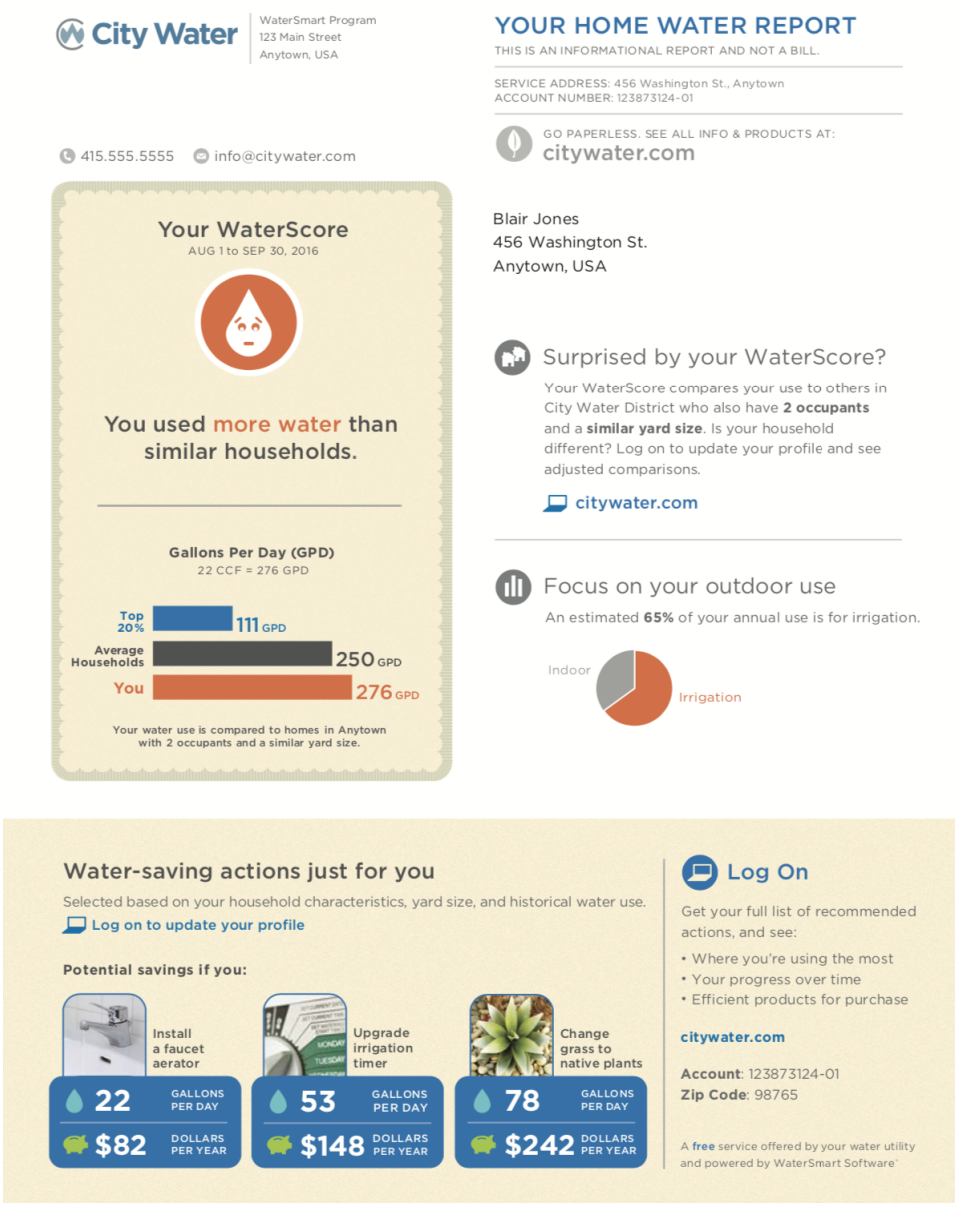
[4]More information is available at http://www.watersmartsoftware.com/.

and an online customer account portal. WaterSmart bears a resemblance to the model of OPower for electricity customers analyzed in Allcott (2011). For many utilities, WaterSmart randomizes the assignment of households that receive HWRs in order to evaluate the causal impact on water consumption (see, e.g., Brent et al. (2015)). Because customers opt in to viewing their online account, we focus here on the treatment effect for households receiving an HWR (i.e., intent-to-treat effects).

The one-page HWR as tested has three components. The main component (in the upper left of the figure) is a social comparison. WaterSmart estimates the household's total water consumption over the previous two months from utility billing records and compares that with the consumption of "average households" and "top 20" most efficient households. The comparison is with households that have the same number of occupants and similar irrigable area across the utility, such that the general water requirements within a peer group are comparable. Households with consumption above the average of their peer group receive a "Red" normative message (shown in Figure 3), those in the top 20th percentile in water efficiency receive a "Green" message, and those with consumption between the average and 20th percentile receive a "Yellow" message (see Appendix Figures A.1 and A.2).

The second component (across the bottom of Figure 3) is a list of three personalized recommendations for strategies to save water. Recommendations include installing faucet aerators and switching to native plants. Based on data available from the utility or on results from a baseline household survey with limited responses, WaterSmart personalizes these recommendations to the extent possible. For example, if a household has no outdoor area, it is not given a recommendation regarding irrigation. The personalized recommendations provide estimates of the water savings in both gallons and dollars, and the dollar estimates rely on the highest marginal price the household paid the previous month. The third component (in the upper right of Figure 3) cycles among a variety of messages about water conservation and utility programs.

Figure 3: Example of "Red" Home Water Report (HWR)



Notes: This is an example of a generic "Red" HWR. These households used more than the median of their peer group.

To show that the randomization was conducted properly we graphed average water use over time across treatment groups and performed a variety of balance tests. Figure 4 shows the average water consumption for the treatment and control groups in both utilities. The treatment and control groups had similar consumption before the intervention; after treatment, the treatment groups used less water. Table 1 shows that the treatment

Figure 4: Water Consumption by Treatment Status and Utility

Notes: The graph displays the average consumption of the treatment and control groups in each utility for every billing period in the sample. The solid lines represent the control groups and the dotted lines represent the treatment groups. The vertical dashed lines designate the start of the treatment period for each utility. The line colors designate utilities.

and control groups are well balanced on a range of observables based on a variety of parametric and nonparametric tests. Out of the 42 tests performed only three (7 percent) have a p-value less than 0.05, and only four (9.5 percent) have a p-value less than 0.1.

Table 1: Summary statistics and balance on observables

Sample	Variable	Treat	Control	Difference	KS	MW	T
Small Utility	Pre-Treatment Water	443.8	440.1	3.7	0.71	0.63	0.32
Small Utility	Pre-Treatment Water (Summer)	506.1	502.3	3.8	0.42	0.96	0.38
Small Utility	Pre-Treatment Water (Winter)	404.4	401.3	3.1	0.19	0.51	0.36
Small Utility	Lot Size	9955.6	9641.5	314.1	0.18	0.31	0.04
Small Utility	Sq. Ft.	1953.5	1954.3	-0.9	0.37	0.45	0.95
Small Utility	Beds	3.0	3.0	-0.0	0.34	0.99	0.78
Small Utility	Baths	2.2	2.2	0.0	0.94	0.44	0.58
Large Utility	Pre-Treatment Water	609.2	609.0	0.2	0.37	0.97	0.93
Large Utility	Pre-Treatment Water (Summer)	722.6	722.4	0.2	0.48	0.72	0.95
Large Utility	Pre-Treatment Water (Winter)	552.9	552.8	0.1	0.31	0.76	0.96
Large Utility	Lot Size	10345.6	10313.8	31.8	0.70	0.82	0.70
Large Utility	Sq. Ft.	2146.9	2173.5	-26.6	0.00	0.07	0.01
Large Utility	Beds	3.5	3.5	-0.0	0.42	0.97	0.68
Large Utility	Baths	2.5	2.5	-0.0	0.38	0.32	0.30

Notes: The table shows the average values for a variety of households characteristics for the treatment and control groups in each utility. All the "pre-treatment water" variables are measured in gallons per day. "Lot size" and "sq. ft." (indoor living space) are measured in square feet. "Beds" and "baths" are the numbers of bedrooms and bathrooms. The last three columns present the p-values from test statistics. KS is the nonparametric Kolmogorov-Smirnov equality of distributions test, MW is the nonparametric rank-order test, and T is the two-sided t-test for difference in means.

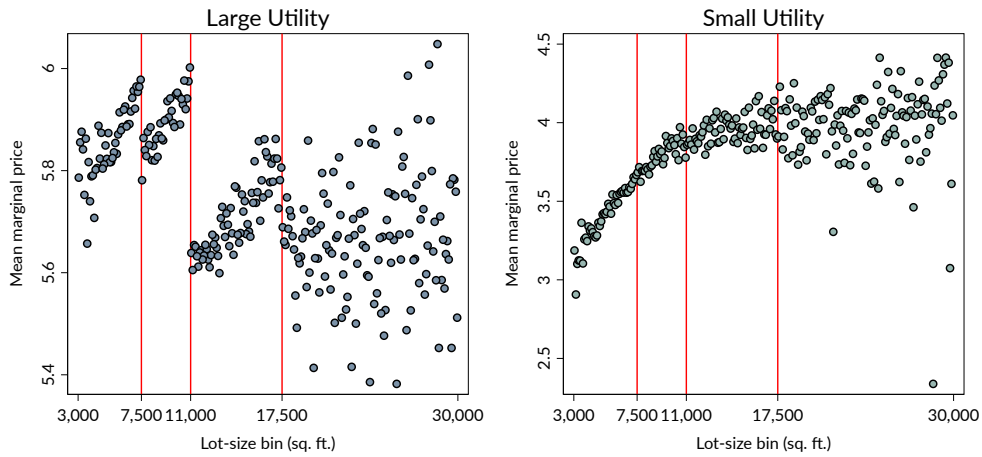
3.3 Quasi-experimental design

In order to identify the differential impact of HWRs for households facing different prices we estimate a difference-in-discontinuity model that exploits a discontinuity in the rate structure of the Large Utility. The Large Utility uses a budget-based increasing-block rate structure, where the tier thresholds depend on the climate zone and lot-size (in square feet). There are five lot size tiers (0–7,499, 7,500–10,999, 11,000–17,499, 17,500–43,559, $\geq$ 43,560), and households with smaller lot sizes are allocated less water before moving to a higher pricing tier. Therefore, households that are just below the lot size tier threshold (e.g., 7,499 sq. ft.) on average face higher prices than households just above a tier threshold (e.g., 7,500 sq. ft.).[5] We exploit this threshold by restricting the analysis to various lot-size bandwidths such that the households are relatively close to the lot size thresholds.

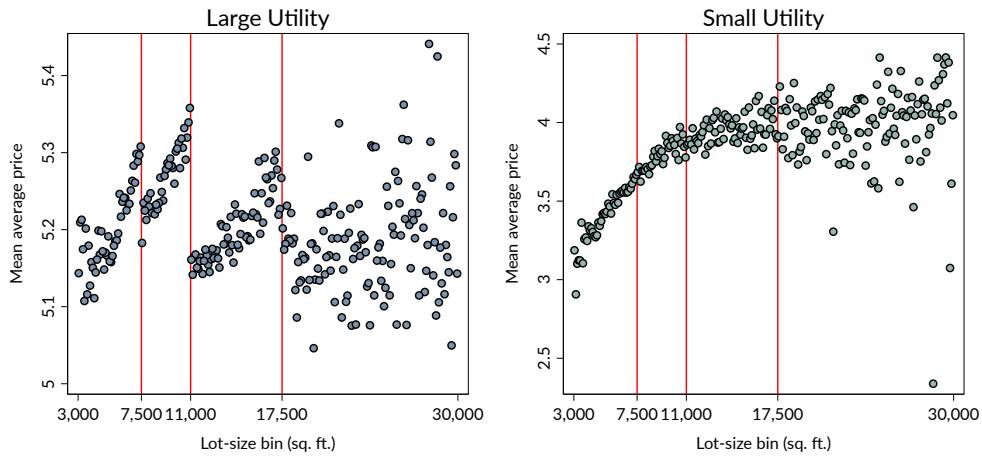
In Figure 5, we show how the lot-size threshold introduces a discontinuous effect on

[5]Because this budget-based billing occurs only in the Large Utility, we also use a third difference as a robustness check (in a difference-in-difference-in-discontinuity design) to compare similar households above and below the lot-size threshold across utility boundaries. These households below the lot-size discontinuity will face higher prices only in the Large Utility.

Figure 5: Discontinuities in Average and Marginal Price Driven by Lot Size



(a) RD treatment in marginal prices at lot-size thresholds for both utilities



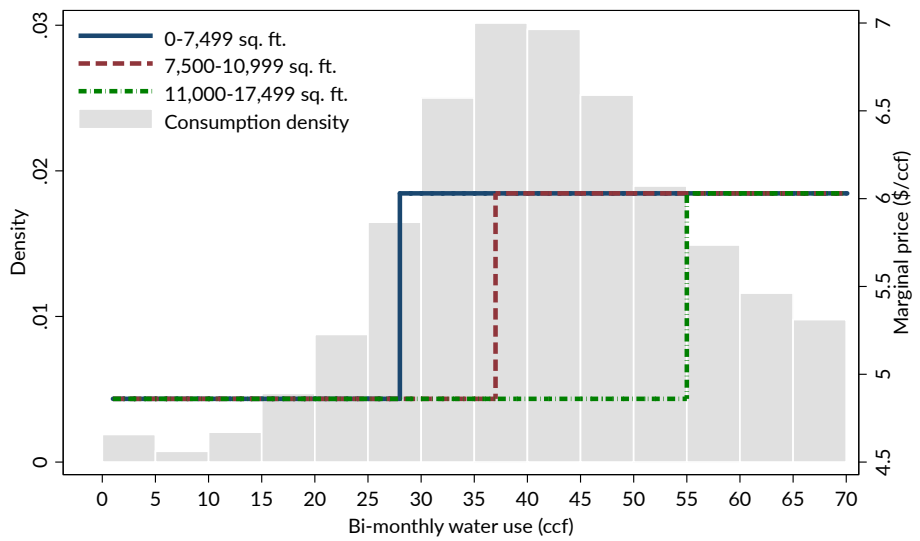
(b) RD treatment in average prices at lot-size thresholds for both utilities

the likelihood of facing a higher marginal price for each utility. These figures present mean marginal (average) prices relative to lot size in 100 sq. ft. bins. There is a distinct jump in the expected marginal price for households just below the lot-size threshold in the Large Utility (panel (a)), but not in the Small Utility (panel (b)). To highlight the difference in typical marginal prices induced by the lot-size threshold, we define "low" lot-size households as those less than 1,000 feet below a lot-size threshold (e.g., 6,499–7,499 sq. ft.) and "high" lot-size households as those less than 1,000 feet above a lot-size threshold (e.g., 7,500–8,500 sq. ft.). The raw data shows that in the Small Utility the average marginal price for low and high lot-size households is $3.65 and $3.77, respectively—so high lot-size households on average pay more for water because they are typically larger water consumers. In the Large Utility, the average marginal price for low and high lot-size households is $5.98 and $5.81, respectively—low lot-size households pay more for water even though they are lower users on average.

Note that although the average price difference in the Large Utility from the lot-size threshold is only $0.20, the marginal price increase from moving to the higher tier is more than $1.00. The average marginal prices reflect both the change in marginal prices and the probability that a household moves into the higher consumption tier. Therefore, some households will face significant marginal price increases because of the lot-size threshold discontinuity. As the lot-size threshold introduces a discontinuity in the probability that a household faces a higher marginal price, we evaluate this price change in a fuzzy regression discontinuity design.

In Figure 6, we present the price variation that we are exploiting in a different way. Here we show the different rate structures for households in three different lot-size groups. Each lot-size group faces the same set of marginal prices, but larger lots are allocated a larger proportion of bimonthly consumption at the lower marginal price. As shown, a household with a 7,400 sq. ft. lot moves into the second price tier at 28 hundred cubic feet (ccf), whereas a household with a 7,500 sq. ft. lot moves into the second price tier at

Figure 6: Changes in Marginal Prices Driven by Lot-Size Groups

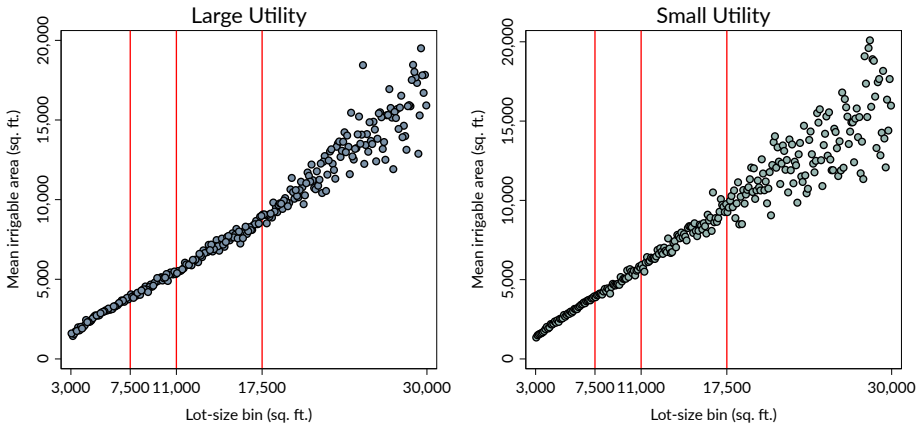


Notes: Bimonthly water distribution is truncated at 70 ccf. Rate structure presented is for the Large Utility before the introduction of a third price tier later in the sample.

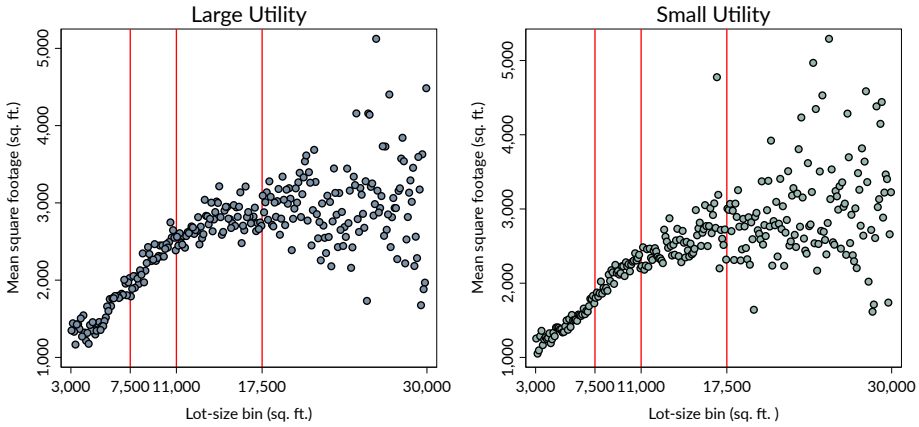
37 ccf. Further, a household with an 11,000 sq. ft. lot does not enter the second price tier until 55 ccf. Moreover, these inframarginal price differences are not trivial: households with lots smaller than 7,500 sq. ft. face a marginal price increase of 19.4% nine units of consumption sooner than do households with slightly larger lot sizes. At the 11,000 sq. ft. threshold, this inframarginal price difference is sustained for 18 ccf every two months.

The standard identifying assumptions in regression discontinuity (RD) frameworks are that: (a) other covariates move smoothly through the discontinuity induced by the running variable, and (b) the running variable cannot be manipulated. The latter assumption is satisfied by noting that lot sizes are fixed over time. The only way to change lot size would be to request that the county resurvey the resident's parcel. Further, in our setting, if other variables associated with water consumption changed discontinuously, then we would worry that the first assumption is not satisfied. As a visual test of this assumption, we present in Figure 7 three relevant variables for water consumption across our RD threshold: irrigable area of lot, indoor square footage of the home, and number of bathrooms.

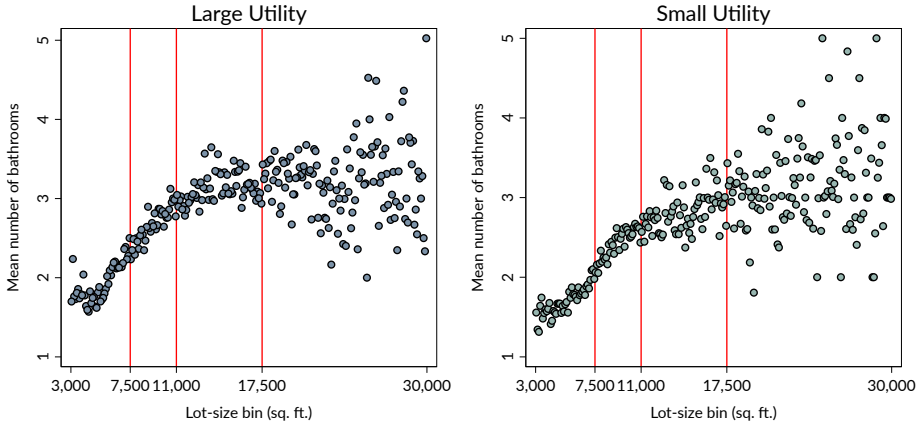
Figure 7: Covariate Distributions across Lot-Size Thresholds for Both Utilities



(a) Mean irrigable area



(b) Mean square footage



(c) Mean number of bathrooms

Notably, irrigable area, which we anticipate to be highly correlated with lot size, moves nearly linearly through the lot-size discontinuities, which provides convincing support for the RD assumptions. We observe no obvious discontinuity in square footage and number of bathrooms at the discontinuities either. We present the same graphical analysis for additional covariates in Appendix Figure A.3.

3.4 Estimating baseline treatment effects of home water reports

Our primary regression framework is a panel difference-in-difference design to estimate the effect of the randomly assigned HWR treatment on average household water use. We calculate normalized water use by dividing each household's water use in gallons per day (gpd) by the average consumption of the control group in the post-treatment period within the same utility. This specification maintains the interpretation of coefficients as percentage changes in water consumption, but unlike the logarithmic transformation, it does not dampen the effect of high users. This is important in the context of social comparisons because prior research shows that most of the savings are concentrated among high users (Allcott, 2011; Brent et al., 2015). We include household fixed effects to control for all static household heterogeneity. Although we are not concerned about traditional forms of endogeneity due to random assignment of treatment we prefer the specification with household fixed effects to focus on how any price effects from the lot-size discontinuity change once a household starts receiving HWRs.

To estimate our primary treatment effects, we specify the following equation:

$$\tilde{w}_{it} = \alpha_i + \gamma_1 \text{Treat}_{it} + \gamma_2 (\text{Treat}_{it} \times \text{Large}_i) + \beta X_{it} + \tau_{it} + \varepsilon_{it}, \tag{7}$$

where $\tilde{w}_{it}$ is normalized average daily water consumption for household i during billing period t. Treat_{it} is an indicator if household i was in the randomized treatment group in a treated time period. We interact the treatment indicator with Large_i, an indicator for

whether the household is in the Large Utility, to account for potential heterogeneity in treatment effects across the utilities. X_{it} is a vector of weather controls, α_i is a household fixed effect, τ_{it} is a period-by-utility fixed effect, and ε_{it} is the residual error term. We cluster all standard errors at the household level. $\hat{\gamma}_1$ and $\hat{\gamma}_1 + \hat{\gamma}_2$ are average treatment effect estimates of the HWRs for the Small and Large Utilities, respectively.

Additionally, to ensure that we are comparing similar populations facing similar temporal shocks in both utilities, we examine the treatment effect model restricted to households within 10 kilometers (km) from the shared utility border.

3.5 Estimating the price-level effect

The price-level effect (PLE) is the differential responsiveness to HWR treatment driven by different economic incentives that households face. We exploit two sources of variation to identify the PLE in a difference-in-discontinuity design. The first source of variation is the random assignment of treatment status, and the second source is identified by the lot-size discontinuity in the rate structure.

Formally, our approach interacts the variables in equation 7 with an indicator for whether the household is below the lot-size threshold:

$$\tilde{w}_{it} = \alpha_i + \gamma_1 \text{Treat}_{it} + \gamma_2(\text{Treat}_{it} \times \text{Low}_i) + \gamma_3(\text{Treat}_{it} \times \text{Lot}_i) + \beta X_{it} + \tau_{it} + \varepsilon_{it} \tag{8}$$

where all variables are the same as in equation 7, except that we add an interaction of treatment with a new indicator (Low_i), which signifies that a household is below any of the lot-size thresholds in the Large Utility (and, thus, more likely to face an exogenously higher marginal price). Because the lot-size discontinuity exists for only the Large Utility, we identify the PLE using Large Utility customers only. We vary the bandwidth of lot-size from $+/-$1,000 sq. ft., $+/-$750 sq. ft., $+/-$500 sq. ft., and $+/-$250 sq. ft. of lot-size thresholds, as well as an optimally chosen bandwidth following Calonico et al. (2014).

We include an interaction with treatment and the continuous lot size (Lot_i) to control for differential treatment effects based on lot size.[6] The base effect of the lot-size discontinuity is absorbed by the household fixed effects.

We include several variants of this specification. We estimate a model with differential treatment–lot size interactions on either side of the lot-size threshold. We also estimate this model for the three primary lot-size discontinuities (at 7,000 sq. ft., 11,000 sq. ft., and 17,500 sq. ft.) individually.

The specification in equation 8 is the reduced form of a difference-in-discontinuity design. The discontinuity at the lot-size thresholds (captured by Low_i) changes the probability that a given household will face the higher marginal price in that billing period, although Low_i does not assign higher marginal prices to customers perfectly. Hence we are operating with a fuzzy discontinuity. To estimate a local average treatment effect of exogenous marginal price assignment, we instrument for a whether a customer faces the higher marginal price (High Price_i) with being below the lot-size threshold (Low_i). Thus we estimate

$$\tilde{w}_{it} = \alpha_i + \gamma_1 \text{Treat}_{it} + \gamma_2 (\text{Treat}_{it} \times \widehat{\text{High Price}}_i) + \gamma_3 (\text{Treat}_{it} \times \text{Lot}_i) + \beta X_{it} + \tau_{it} + \varepsilon_{it} \quad (9)$$

where $\widehat{\text{High Price}}_i$ is predicted by Low_i in the first stage.

The *price-level effect*, the amount of the HWR treatment effect that is driven by exogenous differences in marginal price levels, is given by γ_2 in equations 8 and 9. These specifications allow us to test the hypothesis that $\gamma_2 = 0$. The regressions include both household and weather controls (X_{it}), household fixed effects (α_i), and billing period by utility (τ_{it}) fixed effects.

We also estimate the analogs of equations 8 and 9 for baseline effects on water consumption. That is, we reestimate equations 8 and 9 without our randomized treatment

[6] Lot size is correlated with water use, which is an important driver in treatment heterogeneity in peer comparisons for water conservation (Ferraro and Price, 2013; Brent et al., 2015).

indicator. This framework allows us to assess how price variation from the lot-size discontinuities affects demand directly.

As a robustness check, we include a third source of variation across utilities. The primary motivation for this robustness check is that our discontinuity depends on lot size, which in turn is correlated with water consumption. Because many studies find that high-use households are more responsive to social comparisons, we estimate a double-difference-in-discontinuities model that nets out any primary effect of the lot-size threshold. We add the third difference across utilities in the following framework:

$$\begin{aligned}\tilde{w}_{it} = \alpha_i &+ \gamma_1 \text{Treat}_{it} \\ &+ \gamma_2(\text{Treat}_{it} \times \text{Large}_i) + \gamma_3(\text{Treat}_{it} \times \text{Low}_i) + \gamma_4(\text{Treat}_{it} \times \text{Large}_i \times \text{Low}_i) \\ &+ \gamma_5(\text{Treat}_{it} \times \text{Lot}_i) + \gamma_6(\text{Treat}_{it} \times \text{Lot}_i \times \text{Large}_i) \\ &+ \beta X_{it} + \tau_{it} + \varepsilon_{it},\end{aligned} \tag{10}$$

In this setup, γ_4 is our estimate of the PLE. This specification exploits the fact that while the Large Utility has a lot-size discontinuity in the rate structure the Small Utility does not.[7] This specification addresses the potential confounding of treatment heterogeneity associated with lot size around the threshold, which is not accounted for by the linear lot size interaction with treatment.[8]

3.6 Estimating the price-sensitivity effect

Next, we estimate the *price-sensitivity effect* (PSE), which we define as the way treatment induces differential responses to price changes. We exploit price changes over time and across the utilities in order to estimate a demand equation and then interact the price

[7]Because the Small Utility does not have a corresponding High Price at the discontinuity, we do not estimate equation 10 in a fuzzy RD framework.

[8]8One might wonder why we did not consider the utility boundary as a spatial regression discontinuity similarly to Ito (2014). In our setting, water utility boundaries also serve as political boundaries that induce numerous other changes in tax rates, city regulations, and so forth; thus we did not believe the abrupt change in prices at utility borders would provide a viable identification strategy.

variable with our randomized HWR treatment variables. Our demand regressions take the following form:

$$\ln(w_{it}) = \alpha_i + \beta_1 \ln(\hat{p}_{it}) + \beta_2(\ln(\hat{p}_{it}) \times \text{Treat}_{it}) + \gamma_1 \text{Treat}_{it} + \tau_t + \varepsilon_{it} \tag{11}$$

where $\hat{p}_{it}$ is our endogenous price variable, τ_t are billing-period fixed effects, and all other variables are defined the same as in equations 8 and 9. We estimate equation 11 on our full sample including both utilities. We run an additional specification limiting our sample to households within 10 km of a common district boundary. As in the price-level effect specifications, we also estimate a baseline demand model without any treatment interactions for comparison.

The presence of increasing block rates makes price endogenous because the marginal price a consumer faces depends on the quantity consumed. Thus we estimate equation 11 using two-stage least squares where price and the associated interactions are endogenous variables. Following the framework in Olmstead (2009) and Wichman et al. (2016), we instrument for the actual price the consumer faces (either marginal or average) with the full set of marginal prices from the rate structure. All price instruments are transformed by natural logarithms. Therefore, our identification comes from variation in water rates set by the utility as opposed to changes in the households' consumption. There is an ongoing debate as to whether marginal or average price is the relevant price signal for decisionmaking when consumers face increasing block rates (Nataraj and Hanemann, 2011; Ito, 2014; Wichman, 2014), so we model price as both average and marginal price.[9]

The estimated coefficient $\hat{\beta}_2$ is a direct estimate of our price-sensitivity effect—that is, the degree to which randomized HWR treatments affect consumers' price sensitivity. Our framework allows for a direct test of the price-sensitivity hypothesis—that is, that $\hat{\beta}_2 = 0$. Evidence of a nonzero PSE would suggest that behavioral treatments interact

[9]We define average price as the volumetric proportion of the bill divided by quantity consumed that month.

with structural parameters of demand. On the other hand, evidence of a PSE equal to zero would provide support for the notion that our randomized nudges act solely through behavioral channels.

4 Results and discussion

In this section, we first summarize the baseline results for the field experiment and the natural experiment. These results set the stage for understanding the interactions between prices and social pressure when presenting the results for the price-level effect and the price-sensitivity effect.

4.1 Baseline treatment effects

In Table 2, we present treatment effects for the randomized home water reports (HWRs).[10] In the first two columns, our treatment effects for both the Large and Small Utilities are -3.7 and -3.8 percent reductions in water consumption due to randomized HWRs. Both treatment effects are significant at the $p < 0.01$ level. In the third column, we pool both utilities, but allow for different treatment responses by including an interaction between our treatment variable and and an indicator for Large Utility. In the final column, we restrict the sample of the Large Utility to households within 10km of the Small Utility's border to ensure common support across both utilities. Overall, we find consistent evidence in line with previous research that HWRs reduce water consumption by $3-5$ percent (Ferraro and Price, 2013; Brent et al., 2015) and we observe nearly identical treatment effects across utilities.

[10] Because we were not able to observe whether households actually read the HWRs, these treatment effects should be interpreted as intent-to-treat effects.

Table 2: Baseline Treatment Effects

	(1) Large	(2) Small	(3) Both	(4) 10km
Treat	-0.037***	-0.038***	-0.038***	-0.038***
	(0.004)	(0.008)	(0.008)	(0.008)
Treat*Large			0.001	0.001
			(0.009)	(0.010)
Observations	602,415	453,624	1,056,039	606,876
Households	26,729	19,395	46,124	26,174
Household FEs	Yes	Yes	Yes	Yes
Sample	Full	Full	Full	10km
Period-by-utility FEs	Yes	Yes	Yes	Yes

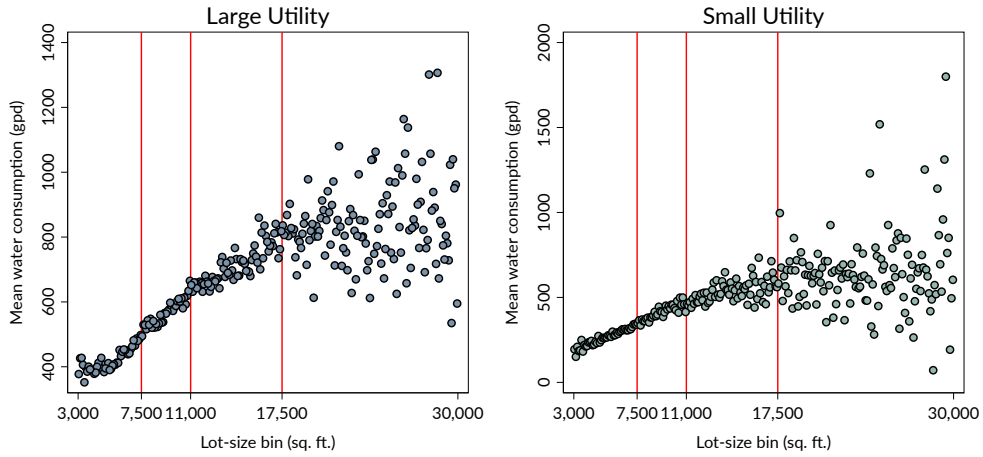
Notes: Dependent variable is average daily water consumption normalized by utility-specific control group consumption. All specifications control for evapotranspiration and precipitation. Robust standard errors are clustered at the household level. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

4.2 Baseline quasi-experimental estimates

We next present the baseline effects of how inframarginal price changes driven by lot-size discontinuities affect consumer demand. In Figure 8, there is no obvious graphical evidence of a discontinuous change in consumption that matches the discontinuity in prices shown in Figure 6. Additionally, we present results from the reduced form of the local linear discontinuity model in panel (a) of Table 3. The coefficients on Low—the indicator that a household is below the lot-size threshold and thus faces a higher expected marginal price—suggest a small negative effect at the largest bandwidth that shrinks and becomes statistically insignificant at smaller bandwidths. Because the discontinuity only increases the probability that consumers face a higher marginal price we also estimate a local linear fuzzy regression discontinuity model and present the results in panel (b) of Table 3. The results are similar to the reduced form, where there is a significant negative effect of facing higher marginal prices because of the lot-size discontinuity at larger bandwidths but no effect at smaller bandwidths. The coefficients from fuzzy discontinuity models are larger, however, because the estimated first-stage coefficient is approximately 0.2.

These results provide evidence that there is no statistically significant first-order response of the lot-size threshold on consumption. Coefficients in smaller bandwidths,

Figure 8: Effect of Discontinuity on Consumption at Lot-Size Thresholds



although insignificant, are positive. This result is surprising considering that both Low and High Price indicate that the customer faces a higher price. There are several explanations for why consumers do not appear to respond to this price differential as anticipated. First, consumers may respond to average as opposed to marginal prices, and while the effect of the discontinuity on average prices is still present, it is not as large as the impact on marginal prices. Alternatively, the difference in prices may not be sufficiently large to warrant a demand response. However, as noted in section 3.3, the change in marginal prices at the lot-size threshold is roughly 20 percent. Another argument is that customers may not respond to *infra*-marginal prices induced by the rate structure and may only respond to information included on their periodic utility bills.

Overall, our initial analysis produces estimates of average treatment effects for HWRs that are squarely within the results of previous studies. This consistency provides us with confidence that the experiments were conducted accurately. We must consider the lack of a primary demand effect in the natural experiment using lot-size discontinuities to identify variation in marginal prices when interpreting the results. Despite the lack of a primary effect, we believe it is important to examine the interaction between prices and nudges for two reasons: First, the lack of a response may constitute an internality that the nudge may correct. Second, the information in the HWR on financial savings is dictated

Table 3: Baseline effects of lot-size discontinuities on household water consumption

(a) Reduced form

	(1) 1,000 sq. ft.	(2) 750 sq. ft.	(3) 500 sq. ft.	(4) 250 sq. ft.	(5) Optimal
Low	-0.023**	-0.010	-0.010	0.012	0.011
	(0.012)	(0.013)	(0.015)	(0.021)	(0.024)
Sq.ft.	0.833***	1.303***	1.425***	3.071***	2.804
	(0.135)	(0.201)	(0.361)	(1.037)	(1.745)
Low*Sq.ft.	-0.027	-0.071	-0.113**	-0.065	-0.068
	(0.045)	(0.050)	(0.051)	(0.061)	(0.072)
Observations	160,209	125,142	90,021	54,849	43,524
Households	12,615	9,849	7,082	4,309	3,417
Household FEs	No	No	No	No	No
Period-by-utility FEs	Yes	Yes	Yes	Yes	Yes
Bandwidth (sq. ft.)	1000	750	500	250	169

(b) Fuzzy discontinuity

	(1) 1,000 sq. ft.	(2) 750 sq. ft.	(3) 500 sq. ft.	(4) 250 sq. ft.	(5) Optimal
High Price	-0.134**	-0.057	-0.055	0.062	0.057
	(0.064)	(0.065)	(0.074)	(0.078)	(0.078)
Sq.ft.	0.880***	1.344***	1.479***	2.915***	2.582***
	(0.124)	(0.167)	(0.308)	(0.730)	(0.756)
Low*Sq.ft.	-0.018	-0.069***	-0.112***	-0.067*	-0.052
	(0.024)	(0.026)	(0.031)	(0.037)	(0.037)
Observations	160,209	125,142	90,021	54,849	54,509
Households	12,615	9,849	7,082	4,309	4,282
Household FEs	No	No	No	No	No
Period-by-utility FEs	Yes	Yes	Yes	Yes	Yes
Bandwidth (sq. ft.)	1000	750	500	250	250
First-stage coef.	0.18	0.19	0.19	0.20	0.20
First-stage SE	0.007	0.008	0.009	0.01	0.01

Notes: Sample includes households from the Large Utility only. Robust standard errors are clustered at the household level. *p<0.1; **p<0.05; ***p<0.01

by the marginal price the consumer faces (see bottom of Figure 3), so the financial benefits of conservation will appear larger to households below the lot-size threshold relative to similar households above lot-size thresholds.

4.3 Estimates of the price-level effect

In Table 4 we present our primary results of the price-level effect. Recall that the PLE in our setting is the interaction between the treatment effect of the HWR and the exogenous

assignment of a higher marginal price via the lot-size discontinuity. We present results only for the Large Utility, because the Small Utility does not have discontinuous changes in price due to lot-size thresholds (see Figure 5). We focus first on the results from the reduced-form equation in panel (a). The coefficient on the interaction Treat*Low is our estimate of the PLE. We vary the bandwidth (distance from the lot-size discontinuity) in each of the columns. For all bandwidths, we estimate a small positive effect, which means that the higher prices in the Low group decrease the conservation generated from the HWRs. However, the effect is not always statistically significant. In our narrowest bandwidth, the interacted coefficient is 0.013 with a standard error (robust to within-household correlation) of 0.013.

Next, in panel (b) of Table 4, we estimate the fuzzy RD version of the PLE. In this specification we interact an indicator for whether a household faces the higher price with our randomized treatment indicator, where the interaction variable is instrumented by the Lot*Treatment interaction. In this model there is a positive and significant effect of being treated while facing higher marginal prices in all specifications except the tightest two bandwidths. These results may be due to financial incentives crowding out intrinsic incentives (Pellerano et al., 2017). The first-stage coefficient in these specifications ranges from 0.10 to 0.14.

In both panels of Table 4, our primary treatment effect increases from its baseline level. This result suggests that focusing only on households within narrow bandwidths around the lot-size threshold may change the composition of households from our primary sample. We suspect that the larger treatment effects are driven by larger water users who might also have larger lot sizes. Thus, because we pool all lot-size discontinuities in the rate structure together, our PLE estimates in Table 4 may mask important heterogeneity.

In Table 5, we estimate PLEs for each discontinuity separately, again for the Large Utility only. For the 7500 and 11,000 sq. ft. discontinuities in panels (a) and (b), we again find precisely estimated null effects, with standard errors increasing slightly with smaller

Table 4: Price-Level Effect

(a) Reduced form

	(1) 1,000 sq. ft.	(2) 750 sq. ft.	(3) 500 sq. ft.	(4) 250 sq. ft.	(5) Optimal
Treat	-0.061***	-0.064***	-0.059***	-0.062***	-0.062***
	(0.006)	(0.007)	(0.008)	(0.010)	(0.010)
Treat*Low	0.012*	0.013	0.017*	0.011	0.011
	(0.007)	(0.008)	(0.009)	(0.012)	(0.012)
Observations	284,298	222,168	160,100	97,496	96,881
Households	12,615	9,849	7,082	4,309	4,282
Sample	Large only	Large only	Large only	Large only	Large only
Household FE	Yes	Yes	Yes	Yes	Yes
Period-by-utility FEs	Yes	Yes	Yes	Yes	Yes
Lot-size int.	Yes	Yes	Yes	Yes	Yes
Bandwidth (sq. ft.)	1000	750	500	250	250

(b) Fuzzy discontinuity

	(1) 1,000 sq. ft.	(2) 750 sq. ft.	(3) 500 sq. ft.	(4) 250 sq. ft.	(5) Optimal
Treat	-0.110***	-0.112***	-0.113***	-0.096***	-0.109***
	(0.019)	(0.020)	(0.021)	(0.025)	(0.025)
Treat*High Price	0.120***	0.116***	0.135***	0.085	0.102*
	(0.041)	(0.044)	(0.044)	(0.054)	(0.054)
Observations	284,298	222,168	160,100	97,496	84,784
Households	12,615	9,849	7,082	4,309	3,745
Household FEs	Yes	Yes	Yes	Yes	Yes
Period-by-utility FEs	Yes	Yes	Yes	Yes	Yes
Bandwidth	1000	750	500	250	205
First-stage coef.	0.10	0.11	0.13	0.14	0.14
First-stage SE	0.006	0.006	0.007	0.010	0.01

Notes: Dependent variable is average daily water consumption normalized by utility-specific control group consumption. All specifications control for evapotranspiration and precipitation. Columns designate the bandwidths around the lot size thresholds in sq. ft. Robust standard errors are clustered at the household level. *p<0.1; oi**p<0.05; ***p<0.01

bandwidths. For these models, the baseline treatment effect is also much more similar to the baseline effects in Table 2. These results are based on larger, more representative subsamples of our data. We only present coefficients from reduced form models. Because our reduced-form coefficients are 0.007 and -0.001 in the optimal bandwidth models, the first-stage coefficients would need to be extraordinarily small for the local average treatment effects to be economically meaningful in the fuzzy RD framework.

For the discontinuity at 17,500 sq. ft., however, we observe both a substantially larger

Table 5: Price-Level Effect at Individual Discontinuities

(a) 7500 sq. ft. discontinuity

	(1)	(2)	(3)	(4)	(5)
	1,000 sq. ft.	750 sq. ft.	500 sq. ft.	250 sq. ft.	Optimal
Treat	-0.040***	-0.039***	-0.043***	-0.034***	-0.035***
	(0.007)	(0.007)	(0.009)	(0.011)	(0.011)
Treat*Low	0.007	0.007	0.010	0.002	0.003
	(0.007)	(0.008)	(0.010)	(0.013)	(0.013)
Observations	182,130	141,984	98,483	58,625	58,556
Households	8,152	6,350	4,393	2,611	2,608
Sample	Full	Full	Full	Full	Full
Household FE	Yes	Yes	Yes	Yes	Yes
Period-by-utility FEs	Yes	Yes	Yes	Yes	Yes
Lot-size int.	No	No	No	No	No
Bandwidth	1000	750	500	250	250

(b) 11,000 sq. ft. discontinuity

	(1)	(2)	(3)	(4)	(5)
	1,000 sq. ft.	750 sq. ft.	500 sq. ft.	250 sq. ft.	Optimal
Treat	-0.038***	-0.041***	-0.030**	-0.047***	-0.047***
	(0.011)	(0.012)	(0.014)	(0.017)	(0.017)
Treat*Low	0.009	0.005	0.010	0.011	0.006
	(0.014)	(0.017)	(0.019)	(0.024)	(0.024)
Observations	74,331	57,842	45,132	28,856	28,807
Households	3,249	2,526	1,969	1,259	1,257
Sample	Full	Full	Full	Full	Full
Household FE	Yes	Yes	Yes	Yes	Yes
Period-by-utility FEs	Yes	Yes	Yes	Yes	Yes
Lot-size int.	No	No	No	No	No
Bandwidth	1000	750	500	250	250

(c) 17,500 sq. ft. discontinuity

	(1)	(2)	(3)	(4)	(5)
	1,000 sq. ft.	750 sq. ft.	500 sq. ft.	250 sq. ft.	Optimal
Treat	-0.119***	-0.143***	-0.112***	-0.121***	-0.121***
	(0.025)	(0.027)	(0.032)	(0.041)	(0.041)
Treat*Low	0.051*	0.065*	0.066*	0.074	0.088*
	(0.030)	(0.034)	(0.037)	(0.047)	(0.049)
Observations	26,948	21,599	15,903	9,623	9,126
Households	1,176	941	695	422	400
Sample	Full	Full	Full	Full	Full
Household FE	Yes	Yes	Yes	Yes	Yes
Period-by-utility FEs	Yes	Yes	Yes	Yes	Yes
Lot-size int.	No	No	No	No	No
Bandwidth	1000	750	500	250	250

Notes: Dependent variable is average daily water consumption normalized by utility-specific control group consumption within 1,000 sq. ft. of the lot-size discontinuity. All specifications control for evapotranspiration and precipitation. Robust standard errors are clustered at the household level. *p<0.1; **p<0.05; ***p<0.01

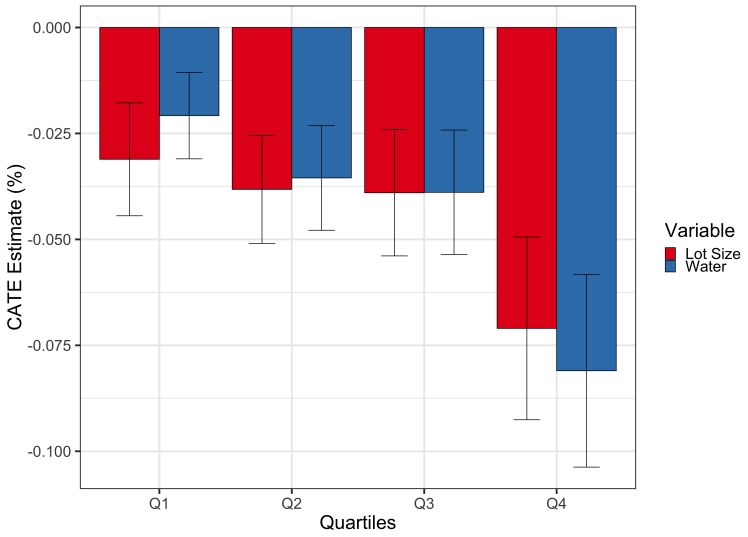
base treatment effect (11–14 percent reductions in average daily consumption) and a large, positive PLE. This PLE estimate, however, is estimated with large confidence intervals, due in part to the smaller number of households near this discontinuity (only 374 households are included in the optimal bandwidth subsample). Households with larger lots tend to use more water for irrigation, which we suspect is why we see larger base treatment effects near the 17,500 sq. ft. discontinuity. Because this estimate is positive, we interpret this as suggestive evidence that higher prices crowd out conservation among high use households on larger lots. Overall, though, we place more confidence in our precisely estimated null effects based on larger and more representative samples near the 7,000 and 11,000 sq. ft. thresholds.[11]

We suspect that large effects at the 17,500 sq. ft. threshold are driven by relatively few unrepresentative households and may be biasing the our primary PLE estimates. We replicate our primary models presented in Table 4 excluding households near the 17,500 sq. ft. threshold. These results, presented in Appendix Table A.1 for the reduced form and Appendix Table A.2 for the fuzzy RD, support our intuition. Removing the 17,500 threshold results in PLE estimates closer to zero with smaller standard errors. In our optimally chosen bandwidth models, our reduced-form PLE estimate is 0.003 (0.011) and our fuzzy RD PLE estimate is 0.025 (0.047).

Because high water-use households are more responsive to social comparisons, it is important to evaluate how the interactions may differ across the distribution of pre-treatment consumption. Figure 9 shows that treatment heterogeneity is similar across quartiles of lot size and baseline water use. Households with large lots and high-use households possess the largest treatment effects. The average consumption levels using the 1000 sq.

[11]We explore this result further in Appendix Figure A.4 in the appendix, in which we plot the base treatment effect coefficients interacted with 250 sq. ft. lot-size bins near the lot-size thresholds. We do so for both utilities. Evidence of a nonzero PLE would be revealed by a discontinuous jump in treatment effect estimates at the lot-size thresholds. Specifically, in the presence of a PLE that increases conservation from HWRs, we expect the treatment effect immediately to the left of the threshold to be larger in magnitude than the treatment effect immediately to the right of the threshold. For the 7,500 sq. ft. discontinuity, we observe the treatment effect move smoothly through the discontinuity for both utilities. All estimates are statistically similar, shown by overlapping 95 percent confidence intervals. The results for the 11,000 and 17,500 sq. ft. thresholds are noisier, but confidence intervals also overlap for all estimates within a utility.

Figure 9: Treatment Heterogeneity by Baseline Consumption and Lot-Size Quartiles



ft. bandwidth for the 7,5000, 11,000, and 17,500 sq. ft. thresholds roughly correspond to the 50th, 75th, and 90th percentiles of the full consumption distribution. Therefore, the results presented in Table 5 also provide insight on the treatment heterogeneity, and we see no significant interactions between prices and nudges across the distribution of consumption.[12]

4.3.1 Power for price-level effects

So far, we have provided evidence that suggests there is no economically meaningful interaction between prices and nudges. A natural question is whether our design is sufficiently powered to identify null effects. In Figure 10, we present minimum detectable effect sizes (MDEs) for different standard deviations and sample sizes that correspond with our sample bandwidths in Table 4, panel (a). In all power calculations, we use pre-treatment water use for Large Utility customers only. We set power equal to 0.8 and use the significance level of 0.05. In the left panel, we calculate MDEs using the unconditional within-household standard deviation of pre-treatment water use (0.38). In the right panel, we regress pre-treatment water use on period and household fixed effects and then

[12]For reference, using the 1,000 sq. ft. bandwidth, average consumption is 492 gpd around the 7,500 sq. ft. threshold, 662 gpd around the 11,000 sq. ft. threshold, and 891 gpd around the 17,500 sq. ft. threshold.

calculate the within-household SD from residual consumption (0.33) for use in the MDE calculation.

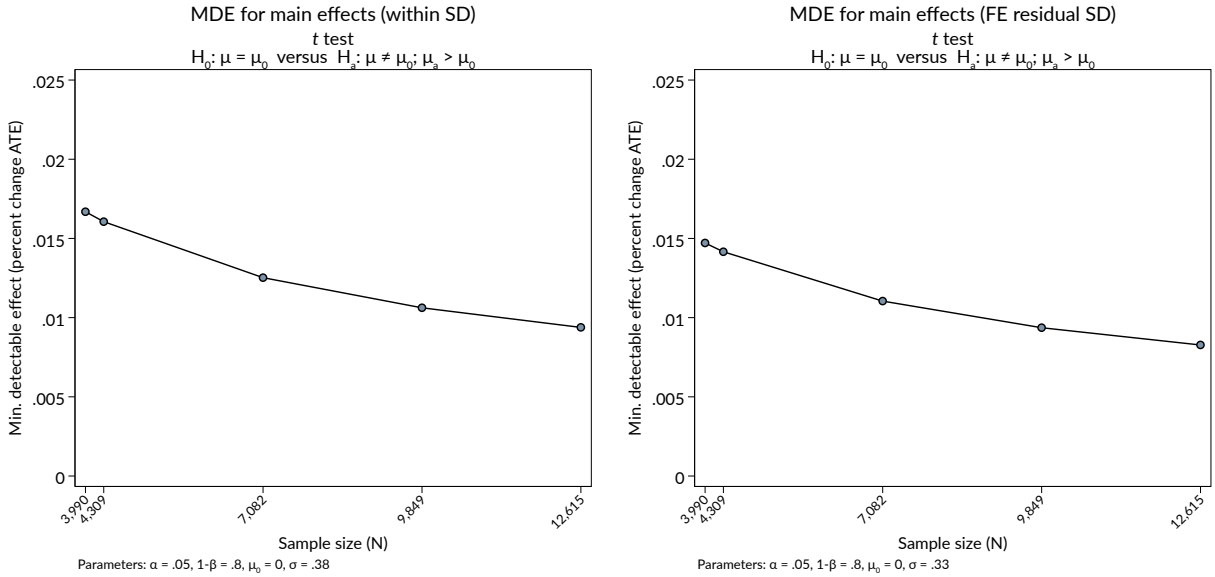
Our results from the power analysis are similar across panels: within the smallest bandwidth ($N = 3,990$), our MDE size for the price-level effect is 0.015–0.017, or approximately 25–28 percent of our estimated HWR treatment effect (a roughly 6 percent treatment effect in Table 4, panel a). For our larger bandwidth ($N = 12,615$), the MDE is 0.008–0.010, or approximately 13–17 percent of our estimated HWR treatment effect. In other words, our analysis is powered to identify PLE effects that would increase responsiveness to the HWR treatment by 25–28 percent within our larger bandwidths or 13–17 percent within our smallest bandwidths. These results hold for our reduced-form PLE effects. For our PLE effects identified from the fuzzy RD, we would need to scale the MDE by the inverse of the first-stage coefficient (ranging from 0.10–0.14 in Table 4). The fuzzy RD, then, has considerably weaker power in our setting.

Alternatively, we can use the standard errors on the estimated price-level effects in Table 4 to infer minimum detectable effects ex post. In column 1, the estimated standard error on Treat*Low is 0.007, which suggests that we could identify a treatment effect of roughly 0.014; this is consistent with the preceding analysis. Within our tightest bandwidth, a standard error of 0.013 implies an MDE of roughly 0.026, which is larger although still less than 50 percent of our estimated HWR treatment effect.

4.3.2 Robustness of price-level effect

We include several additional analyses to support our results. First, we add a third difference to our difference-in-discontinuity design because it is possible that responsiveness to HWRs is greater for households with higher consumption levels, which is correlated positively with lot size (our running variable in the regression discontinuity). To implement the third difference, we estimate equation 11 on a sample including both utilities. We present these results in Table 6. In these specifications, the reduced-form PLE is identified

Figure 10: Minimum Detectable Effect Sizes for Price-Level Effect



by the coefficient on Treat*Low*Large, or the marginal change in the treatment effect due to facing an exogenously higher marginal price by being just below the lot-size threshold relative to similar households in the Small Utility that face no price discontinuity. In these specifications, we find no statistical evidence of a PLE, which is a precisely estimated zero for bandwidths of 1000 sq. ft. and 750 sq. ft. At the smaller bandwidths, the effect becomes negative and is still small and insignificant. When we replicate these models excluding households near the 17,500 threshold, we find PLEs that are closer to zero with smaller standard errors. Results are presented in Appendix Table A.3.

We include several other robustness checks. First, we include interaction terms with lot size on both sides of the threshold, as is typical in local linear RD designs. The results, in Appendix Table A.4, are virtually unchanged: we find small positive but insignificant effects for the PLE. Additionally, we perform a falsification test in the Large Utility at false discontinuities of 9000 sq. ft. and 13,000 sq. ft. We choose these thresholds because they are near our true thresholds without overlapping at the largest bandwidths (1,000 sq. ft.). These falsification tests examine whether our lot size thresholds would partially pick up the smaller treatment effects (in absolute value), associated with smaller lots that use less

Table 6: Price-Level Effect: Difference-in-Difference-in-Discontinuity

	(1)	(2)	(3)	(4)	(5)
	1,000 sq. ft.	750 sq. ft.	500 sq. ft.	250 sq. ft.	Optimal
Treat	-0.068***	-0.067***	-0.045*	-0.072**	-0.072**
	(0.016)	(0.019)	(0.023)	(0.033)	(0.034)
Treat*Large	0.007	0.002	-0.015	0.010	0.009
	(0.017)	(0.020)	(0.025)	(0.035)	(0.035)
Treat*Low	0.013	0.009	0.007	0.026	0.028
	(0.010)	(0.012)	(0.015)	(0.019)	(0.019)
Treat*Low*Large	-0.000	0.005	0.011	-0.014	-0.017
	(0.013)	(0.015)	(0.017)	(0.023)	(0.023)
Observations	414,260	318,956	226,140	134,954	134,132
Households	18,176	13,983	9,904	5,907	5,871
Sample	Both Utilities	Both Utilities	Both Utilities	Both Utilities	Both Utilities
Household FE	Yes	Yes	Yes	Yes	Yes
Period-by-utility FEs	Yes	Yes	Yes	Yes	Yes
Lot-size int.	Yes	Yes	Yes	Yes	No
Bandwidth	1000	750	500	250	250

Notes: Dependent variable is average daily water consumption normalized by utility-specific control group consumption. All specifications control for evapotranspiration and precipitation. Columns designate the bandwidths around the lot size thresholds in sq. ft. Robust standard errors are clustered at the household level. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

water. If the true PLE is negative (households who face higher prices are more responsive to HWRs) the small lot size effect will bias our estimates of the PLE towards zero. These results are presented in Table A.5. Here again, we find statistical zeros, and the point estimates switch between positive and negative values.[13]

Lastly, we consider the possibility for dynamic adjustment in the PLE. Ito et al. (2018) shows different levels of persistence for financial incentives compared with moral suasion in energy, and Brent et al. (2017) show that financial nudges are more persistent than nudges using moral suasion for water conservation. Therefore, the PLE may not be present until the conservation effect of social pressure begins to wane. We estimate price-level effects interacted with an indicator for the year after treatment begins to show differences in the persistence of the PLE. We present the regression results in Appendix Table A.6. There is no evidence of a PLE in the year of or year after treatment. We also explore seasonal effects by interacting the PLE with a dummy variable for summer in Ap-

[13]There is a larger and noisy negative PLE at the false 13,000 sq. ft. discontinuity, but that is likely due to noisy estimates in a small sample ($\approx$ 400 households).

pendix Table A.7. These results suggest that negative PLEs are observed in the summer months (around 2–3 percent reductions), when conservation signals and prices might be more salient, and we observe positive PLEs of similar magnitudes in nonsummer months. In our tightest, optimally chosen bandwidth, both of these effects are statistically similar to zero.

To recap, we find little evidence that suggests that exogenously assigned differences in marginal prices increase the effectiveness of HWRs. In fact, we uncover some evidence that higher prices may slightly decrease conservation effects from behavioral interventions among high-use households. This result is somewhat surprising because the HWRs make the private economic benefits of water conservation more salient (e.g., bottom panel in Figure 3). HWRs provide cost-savings information that consumers might expect from changing behavior or technology. Consumers just above/below the price discontinuities we use for identification would thus face nontrivial differences in expected cost savings despite being otherwise similar types of households, but we observe no statistical difference in their response to HWRs. Our analysis thus far suggests that the primary mechanism for the HWR operates through channels of increasing (the salience of) the moral costs of water consumption.

Table 7: Baseline Demand Specifications

	(1) MP	(2) AP	(3) MP	(4) AP	(5) MP	(6) AP
ln(MP)	0.549***		-0.246***		-0.278***	
	(0.007)		(0.012)		(0.017)	
ln(AP)		0.564***		-0.169***		-0.187***
		(0.008)		(0.011)		(0.016)
Observations	939,775	929,842	928,032	921,537	572,528	570,981
Households	43,133	43,132	43,124	43,120	25,990	25,987
Household FEs	Y	Y	Y	Y	Y	Y
Period FEs	Y	Y	Y	Y	Y	Y
IV	–	–	Y	Y	Y	Y
Sample	Full	Full	Full	Full	10km	10km

Notes: MP = marginal price; AP = average price. Dependent variable is the natural log of average daily water consumption. All specifications control for evapotranspiration and precipitation. Prices are instrumented with full set of marginal prices from the utility rate schedule and associated interactions with exogenous variables. Robust standard errors are clustered at the household level. *p<0.1; **p<0.05; ***p<0.01

4.4 Price-sensitivity effects

We now turn to our results of the price-sensitivity effects. We first present our initial demand specifications in Table 7. In the first two columns, we present naïve models using endogenous marginal and average price variables. As expected with increasing block-rate structures, we observe positive price elasticities. Our instrumental variables (IV) approach, in columns (3) and (4), performs comparatively better, providing sensible demand elasticities (-0.25 for MP and -0.17 for AP) that are within the range of previous estimates for both marginal and average prices arising from reduced-form and structural models of water demand (Dalhuisen et al., 2003; Olmstead, 2009; Nataraj and Hanemann, 2011; Wichman, 2014). In the present analysis, we do not take a stand on whether average or marginal price responsiveness is the correct specification; rather, we model them side by side. In columns (5) and (6), we restrict the sample to households within 10km of the shared border, and our elasticity estimates are similar.

We present results for the PSE in Table 8. Recall that the PSE is the degree to which HWRs increase consumers' price sensitivity, such as by making the costs of consumption more salient. Identification of this effect is straightforward: we estimate price elasticities

of water demand equation as in equation 11 and interact our price variables with the randomized HWR treatment. The resulting coefficient on that interaction is the PSE.

Table 8: Price-Sensitivity Effect

	(1) MP	(2) AP	(3) MP	(4) AP
Treat	-0.035***	-0.042***	-0.041***	-0.044***
	(0.003)	(0.003)	(0.005)	(0.005)
ln(MP)	-0.221***		-0.236***	
	(0.011)		(0.015)	
ln(MP)*Treat	-0.030**		-0.012	
	(0.014)		(0.020)	
ln(AP)		-0.191***		-0.189***
		(0.010)		(0.014)
ln(AP)*Treat		-0.002		0.002
		(0.012)		(0.016)
Observations	928,032	921,537	572,528	570,981
Households	43,124	43,120	25,990	25,987
Household FEs	Y	Y	Y	Y
Period FEs	Y	Y	Y	Y
IV	Y	Y	Y	Y
Sample	Full	Full	10km	10km

Notes: MP = marginal price; AP = average price. Dependent variable is the natural log of average daily water consumption. All specifications control for evapotranspiration and precipitation. Prices are instrumented with full set of marginal prices from the utility rate schedule and associated interactions with exogenous variables. Interactions with indicators for treatment periods are included but coefficients are not reported for clarity. Robust standard errors are clustered at the household level. *p<0.1; **p<0.05; ***p<0.01

In columns (1) and (2) of Table 8, we report PSE estimates for our pooled sample. We find statistically significant evidence that HWRs increase price sensitivity under the marginal price, but not the average price, demand specification. The PSE coefficient in column (1) is -0.03, which increases price sensitivity by approximately 13 percent for the MP specification. In column (2), however this coefficient is -0.002, which suggests that there is no PSE for specifications that include average prices. We might be concerned that in addition to price variation, there is significant variation in unobservables across utilities that could affect price elasticity. We control for these cross-border differences by restricting the sample to households that are less than 10 km away from the Small Utility's

boundary. Columns (3) and (4) present the PSE estimates in the restricted sample, and we find no evidence of a statistically or economically significant PSE in either the MP or AP specification. Overall, these results are slightly less decisive than our PLE results, but we still do not find conclusive evidence that social comparisons have meaningful interactions with prevailing economic incentives.

5 Conclusions

In this paper, we have explored the interaction of prices and behavioral nudges. We compare water consumer responses to a randomized behavioral messaging campaign for households that face differential exogenously assigned marginal prices. The results from our analysis suggest that households that have a greater economic incentive to conserve respond to the social comparison similarly to households with less economic incentive to conserve. This result suggests that the private economic benefits of conservation are inconsequential for behavioral treatments to be effective. Additionally, we estimate the degree to which the behavioral treatment affects price sensitivity, finding limited evidence that there is an economically meaningful relationship between prices and nudges. Overall, our results suggest that although there is theoretical justification for why behavioral nudges and economic incentives should interact, we find little empirical support for this interaction being meaningful.

Behavioral nudges do not exist in a vacuum. Although the randomized deployment of many behavioral nudges provides strong internal validity for the estimation of causal effects, as Allcott (2015) shows, the treatment effects from any given location may be a function of the underlying characteristics of the specific population. In order to ensure that the estimates from any one location are externally valid, it is critical to identify the sources of heterogeneity and adjust the magnitude based on the characteristics of the target population. This is challenging when the entire experimental sample faces the same set of existing policies. We focus on how variation in prevailing water prices affects

consumer responsiveness to behavioral nudges intended to encourage water conservation that include social comparisons. We do not find any evidence that the response to this prevalent behavioral nudge has any meaningful interactions with underlying water rates. This is an important result for many settings in which resource prices are low or zero, or when scarcity pricing may not be politically feasible, where we show that behavioral nudges may still be effective mechanisms to encourage conservation.

In addition to external validity, our results have implications for the behavioral mechanisms through which nudges operate. The lack of evidence of heterogeneity due to different private benefits of conservation leads us to conclude that consumers are primarily responding to social comparisons because of increased salience of the moral cost of water consumption. This finding has important implications for the welfare effect of nudges as shown by Allcott and Kessler (2019). If nudges operate as a moral tax, which is consistent with our findings in this paper, they will only be welfare enhancing only if the social cost of energy/water exceeds the current private costs. This suggests a rethinking of behavioral policies that specifically target welfare improvements as opposed to simply changing behavior. Given substantial evidence of behavioral biases in energy and water markets (Allcott and Wozny, 2014; Sexton, 2015; Wichman, 2017; Brent and Ward, 2018, 2019), it is worthwhile to find ways to promote prosocial behavior that also improves private decisions.

One drawback of our design is that there is limited demand response to the price variation around the discontinuity. Therefore, while we do not find that nudges generate more conservation when consumers face higher prices it is possible the price differential in our setting is not large enough. The finding is still important because the lack of a demand response may constitute an internality that the nudge did not correct. This is true even though the nudge included financial information based on the marginal price. Additionally, we do not find any evidence for the nudge affecting the price elasticity of demand. Future research can investigate interactions between prices and informational

nudges across larger, or more salient, price differences.

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Online Appendix: Additional Results

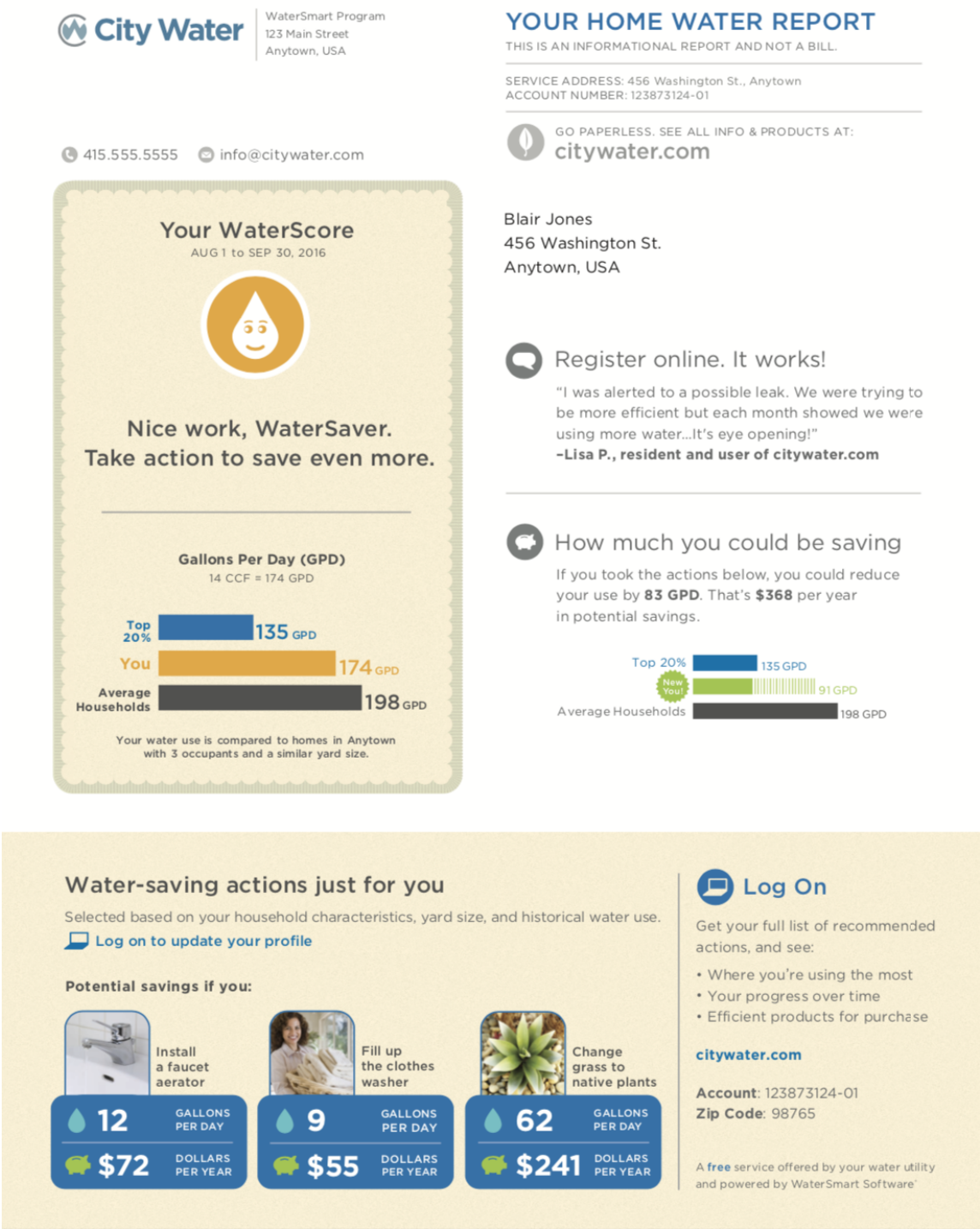

Figure A.1: Example "Yellow "Home Water Report

Notes: This is an example of a generic "Yellow" Home Water Report (HWR). Households receiving this report used less water than their peer group average.

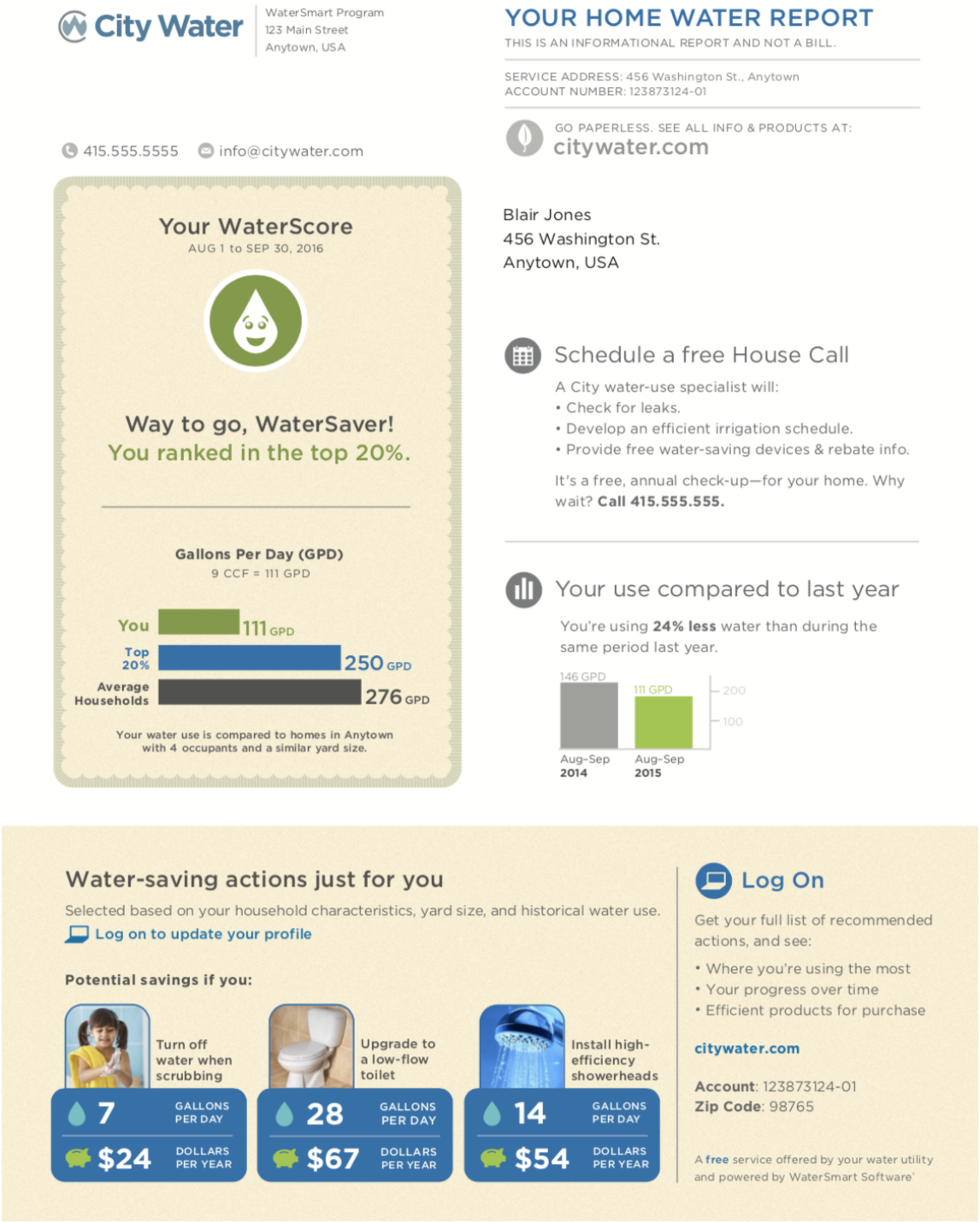

Figure A.2: Example "Green" Home Water Report

Notes: This is an example of a generic "Green" Home Water Report (HWR). Households receiving this report were in the bottom 20% of their peer group.

Figure A.3: Additional Covariate Distributions across Lot-Size Thresholds for Both Utilities

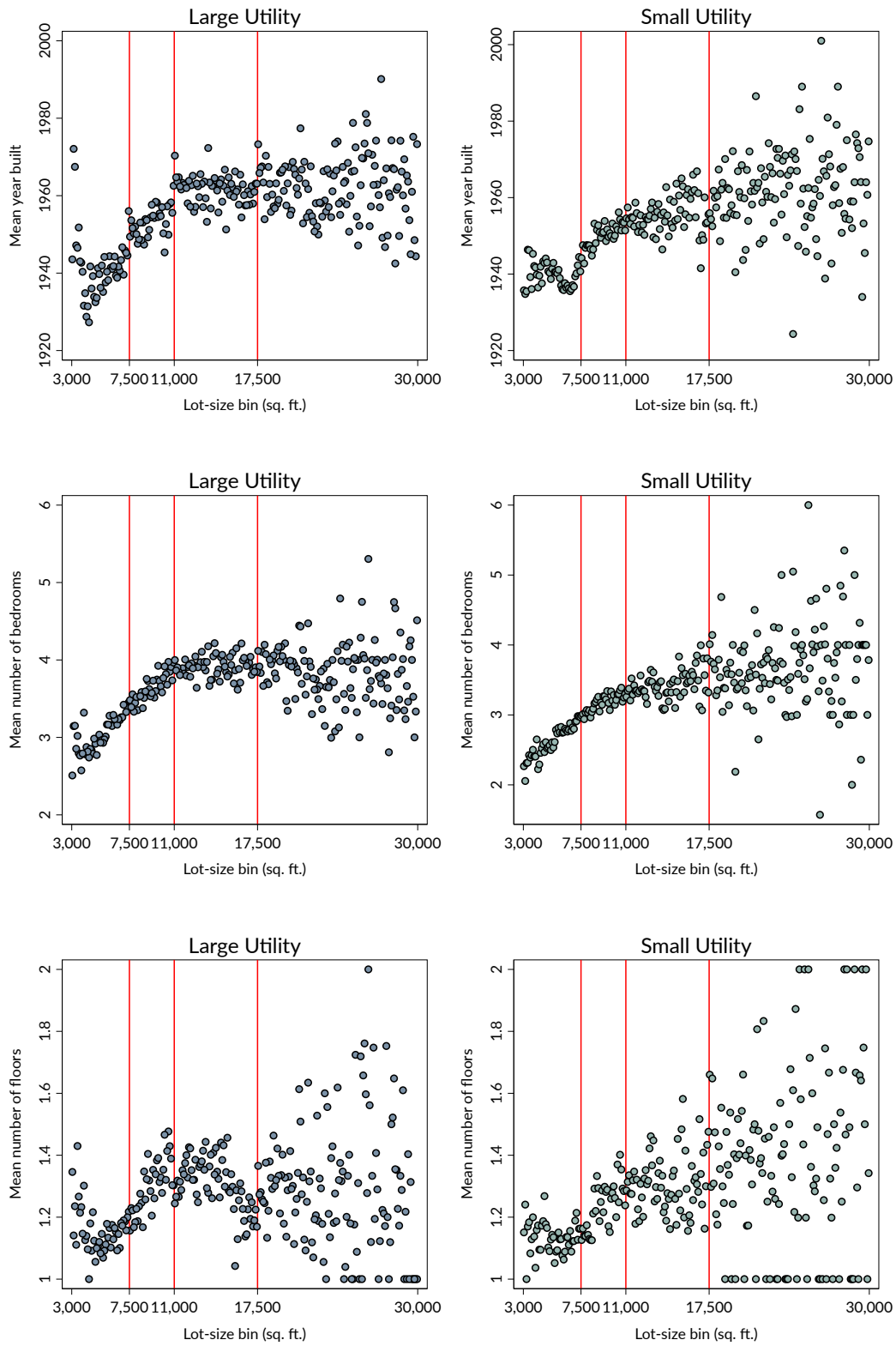

Figure A.4: Treatment Effects by Lot-Size Bin

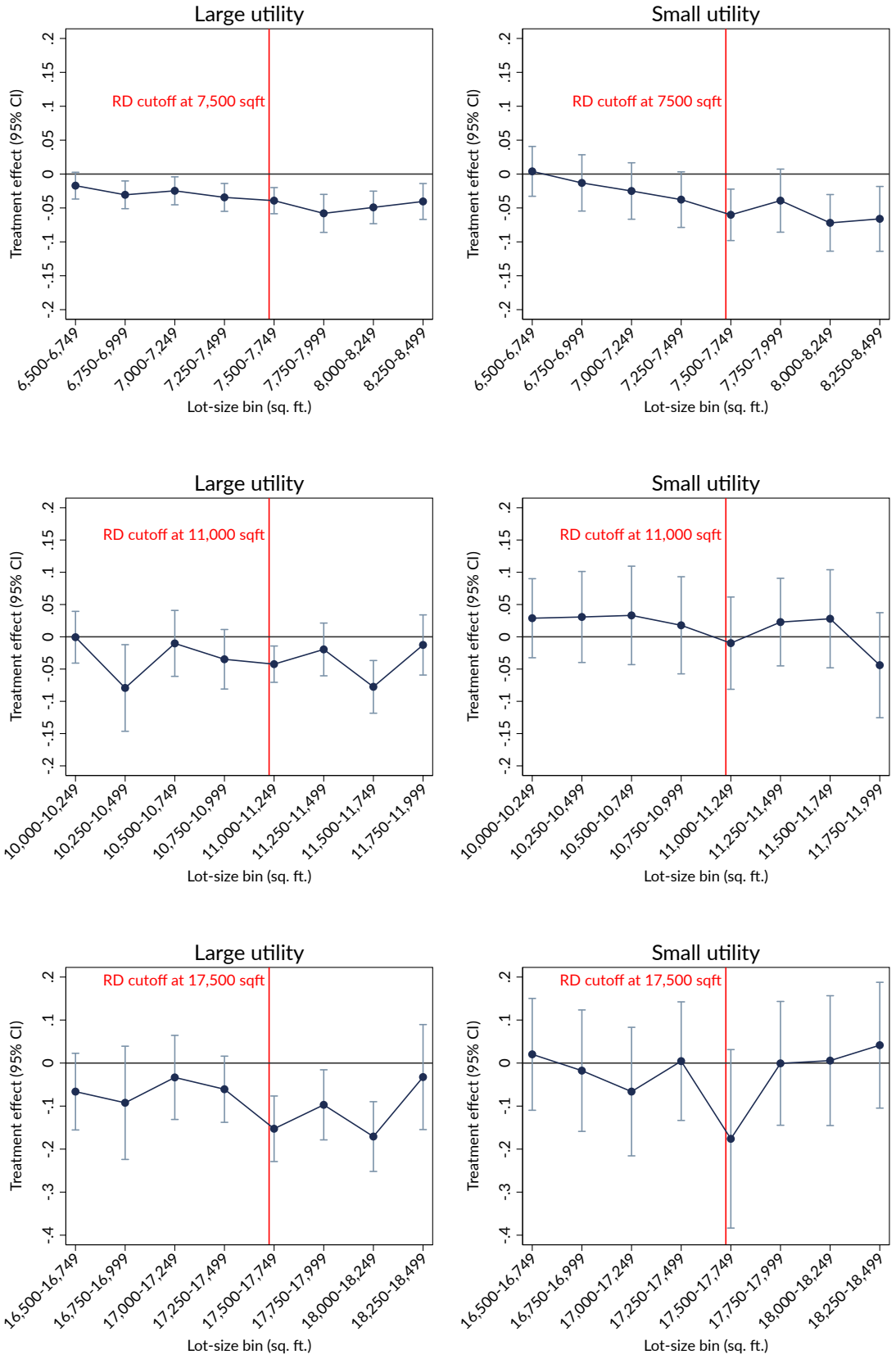

Table A.1: Reduced-Form Estimates of the Price-Level Effect without 17,500 Lot-Size Threshold

	(1)	(2)	(3)	(4)	(5)
	1,000 sq. ft.	750 sq. ft.	500 sq. ft.	250 sq. ft.	Optimal
Treat	-0.062***	-0.065***	-0.058***	-0.063***	-0.064***
	(0.007)	(0.008)	(0.009)	(0.011)	(0.011)
Treat*Low	0.006	0.005	0.009	0.003	0.001
	(0.008)	(0.008)	(0.010)	(0.012)	(0.012)
Observations	256,461	199,826	143,615	87,481	87,363
Households	11,401	8,876	6,362	3,870	3,865
Sample	Large only	Large only	Large only	Large only	Large only
Household FE	Yes	Yes	Yes	Yes	Yes
Period-by-utility FEs	Yes	Yes	Yes	Yes	Yes
Lot-size int.	Yes	Yes	Yes	Yes	Yes
Bandwidth (sq. ft.)	1000	750	500	250	250

Notes: Dependent variable is average daily water consumption normalized by utility-specific control group consumption. All specifications control for evapotranspiration and precipitation. Robust standard errors are clustered at the household level. *p<0.1; **p<0.05; ***p<0.01

Table A.2: Fuzzy RD Estimates of the Price-Level Effect without 17,500 Lot-Size Threhold

	(1)	(2)	(3)	(4)	(5)
	1,000 sq. ft.	750 sq. ft.	500 sq. ft.	250 sq. ft.	Optimal
Treat	-0.085***	-0.083***	-0.086***	-0.070***	-0.065***
	(0.018)	(0.019)	(0.019)	(0.022)	(0.023)
Treat*High Price	0.058	0.046	0.072*	0.019	-0.010
	(0.042)	(0.045)	(0.042)	(0.051)	(0.052)
Observations	256,461	199,826	143,615	87,481	83,509
Households	11,401	8,876	6,362	3,870	3,695
Household FEs	Yes	Yes	Yes	Yes	Yes
Period-by-utility FEs	Yes	Yes	Yes	Yes	Yes
Bandwidth	1000	750	500	250	228
First-stage coef.	0.10	0.11	0.13	0.14	0.14
First-stage SE	0.006	0.007	0.008	0.01	0.01

Notes: Dependent variable is average daily water consumption normalized by utility-specific control group consumption. All specifications control for evapotranspiration and precipitation. Robust standard errors are clustered at the household level. *p<0.1; **p<0.05; ***p<0.01

Table A.3: Reduced-Form Diff-in-Diff-in-Discontinuity Estimates of the Price-Level Effect without 17,500 Lot-Size Threshold

	(1)	(2)	(3)	(4)	(5)
	1,000 sq. ft.	750 sq. ft.	500 sq. ft.	250 sq. ft.	Optimal
Treat	-0.106***	-0.097***	-0.078***	-0.108***	-0.110***
	(0.016)	(0.020)	(0.025)	(0.037)	(0.037)
Treat*Large	0.043**	0.031	0.019	0.044	0.045
	(0.018)	(0.021)	(0.027)	(0.038)	(0.038)
Treat*Low	0.003	0.004	0.004	0.016	0.017
	(0.010)	(0.012)	(0.014)	(0.019)	(0.019)
Treat*Low*Large	0.003	0.001	0.006	-0.013	-0.016
	(0.013)	(0.015)	(0.017)	(0.022)	(0.022)
Observations	379,115	290,848	205,660	123,161	122,878
Households	16,648	12,762	9,011	5,392	5,380
Sample	Both Utilities	Both Utilities	Both Utilities	Both Utilities	Both Utilities
Household FE	Yes	Yes	Yes	Yes	Yes
Period-by-utility FEs	Yes	Yes	Yes	Yes	Yes
Lot-size int.	Yes	Yes	Yes	Yes	No
Bandwidth	1000	750	500	250	250

Notes: Dependent variable is average daily water consumption normalized by utility-specific control group consumption. All specifications control for evapotranspiration and precipitation. Robust standard errors are clustered at the household level. *p<0.1; **p<0.05; ***p<0.01

Table A.4: Price-Level Effect: Large Utility with Lot-Size Interactions on Both Sides of the Threshold

	(1)	(2)	(3)	(4)	(5)
	1,000 sq. ft.	750 sq. ft.	500 sq. ft.	250 sq. ft.	Optimal
Treat	-0.062***	-0.066***	-0.060***	-0.063***	-0.063***
	(0.006)	(0.007)	(0.008)	(0.011)	(0.011)
Treat*Low	0.019**	0.020**	0.024**	0.016	0.017
	(0.009)	(0.010)	(0.011)	(0.014)	(0.014)
Observations	284,298	222,168	160,100	97,496	96,881
Households	12,615	9,849	7,082	4,309	4,282
Sample	Large only	Large only	Large only	Large only	Large only
Household FE	Yes	Yes	Yes	Yes	Yes
Period-by-utility FEs	Yes	Yes	Yes	Yes	Yes
Lot-size int.	Yes	Yes	Yes	Yes	Yes
Bandwidth (sq. ft.)	1000	750	500	250	250

Notes: Dependent variable is average daily water consumption normalized by utility-specific control group consumption. All specifications control for evapotranspiration and precipitation. Columns designate the bandwidths around the lot size thresholds in sq. ft. Robust standard errors are clustered at the household level. *p<0.1; **p<0.05; ***p<0.01

Table A.5: Price-Level Effect Falsification Test: Large Utility Only, with Lot-Size Interactions, at Individual Discontinuities

(a) False 9000 sq. ft. discontinuity

	(1)	(2)	(3)	(4)	(5)
	1000sqft	750sqft	500sqft	250sqft	Optimal
Treat	-0.030**	-0.040***	-0.034**	-0.038**	-0.041**
	(0.012)	(0.013)	(0.015)	(0.019)	(0.020)
Treat*Low	-0.002	0.009	0.004	-0.002	0.006
	(0.012)	(0.014)	(0.017)	(0.021)	(0.023)
Observations	86,467	61,694	40,375	21,396	18,280
Households	3,845	2,742	1,793	947	806
Sample	Large only	Large only	Large only	Large only	Large only
Household FE	Yes	Yes	Yes	Yes	Yes
Period-by-utility FEs	Yes	Yes	Yes	Yes	Yes
Lot Size Int.	No	No	No	No	No
Bandwidth	1000	750	500	250	212

(b) False 13,000 sq. ft. discontinuity

	(1)	(2)	(3)	(4)	(5)
	1000sqft	750sqft	500sqft	250sqft	Optimal
Treat	-0.096***	-0.086***	-0.095***	-0.041	-0.041
	(0.020)	(0.023)	(0.029)	(0.039)	(0.045)
Treat*Low	0.021	0.004	-0.010	-0.063	-0.069
	(0.024)	(0.027)	(0.036)	(0.051)	(0.058)
Observations	36,675	26,487	17,100	9,288	7,473
Households	1,605	1,160	748	408	328
Sample	Large only	Large only	Large only	Large only	Large only
Household FE	Yes	Yes	Yes	Yes	Yes
Period-by-utility FEs	Yes	Yes	Yes	Yes	Yes
Lot Size Int.	No	No	No	No	No
Bandwidth	1000	750	500	250	212

Notes: Dependent variable is average daily water consumption normalized by utility-specific control group consumption within 1,000 sq. ft. of the lot-size discontinuity. All specifications control for evapotranspiration and precipitation. Robust standard errors are clustered at the household level. *p<0.1; **p<0.05; ***p<0.01

Table A.6: Persistence of the Price-Level Effect

	(1)	(2)	(3)	(4)	(5)
	1,000 sq. ft.	750 sq. ft.	500 sq. ft.	250 sq. ft.	Optimal
Treat	-0.060***	-0.065***	-0.062***	-0.063***	-0.062***
	(0.006)	(0.007)	(0.008)	(0.011)	(0.010)
Treat*Year 2	-0.001	0.001	0.008	0.002	0.005
	(0.006)	(0.007)	(0.008)	(0.010)	(0.010)
Treat*Low	0.017**	0.015*	0.021**	0.008	0.014
	(0.008)	(0.008)	(0.010)	(0.013)	(0.012)
Treat*Low*Year 2	-0.012	-0.007	-0.009	0.009	-0.001
	(0.007)	(0.008)	(0.010)	(0.013)	(0.012)
Observations	284,298	222,168	160,100	97,496	110,801
Households	12,615	9,849	7,082	4,309	4,897
Sample	Large only	Large only	Large only	Large only	Large only
Household FE	Yes	Yes	Yes	Yes	Yes
Period-by-utility FEs	Yes	Yes	Yes	Yes	Yes
Lot-size int.	Yes	Yes	Yes	Yes	Yes
Bandwidth	1000	750	500	250	309

Notes: Dependent variable is average daily water consumption normalized by utility-specific control group consumption. All specifications control for evapotranspiration and precipitation. Columns designate the bandwidths around the lot size thresholds in sq. ft. Robust standard errors are clustered at the household level. *p<0.1; **p<0.05; ***p<0.01

Table A.7: Seasonality of the Price-Level Effect

	(1)	(2)	(3)	(4)	(5)
	1,000 sq. ft.	750 sq. ft.	500 sq. ft.	250 sq. ft.	Optimal
Treat	-0.061***	-0.064***	-0.056***	-0.056***	-0.053***
	(0.006)	(0.007)	(0.008)	(0.011)	(0.010)
Treat*Summer	0.001	-0.001	-0.008	-0.015*	-0.016*
	(0.006)	(0.006)	(0.007)	(0.009)	(0.008)
Treat*Low	0.030***	0.030***	0.032***	0.022*	0.024**
	(0.008)	(0.008)	(0.010)	(0.013)	(0.012)
Treat*Low*Summer	-0.042***	-0.043***	-0.037***	-0.026**	-0.026**
	(0.007)	(0.007)	(0.009)	(0.012)	(0.011)
Observations	284,298	222,168	160,100	97,496	110,801
Households	12,615	9,849	7,082	4,309	4,897
Sample	Large only	Large only	Large only	Large only	Large only
Household FE	Yes	Yes	Yes	Yes	Yes
Period-by-utility FEs	Yes	Yes	Yes	Yes	Yes
Lot-size int.	Yes	Yes	Yes	Yes	Yes
Bandwidth	1000	750	500	250	309

Notes: Dependent variable is average daily water consumption normalized by utility-specific control group consumption. All specifications control for evapotranspiration and precipitation. Columns designate the bandwidths around the lot size thresholds in sq. ft. Robust standard errors are clustered at the household level. *p<0.1; **p<0.05; ***p<0.01