

# **Behavioral responses to two-part tariffs:**

## *Evidence from the introduction of volumetric water pricing \**

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#### **Abstract**

Economists often advocate for pricing mechanisms based on marginal prices to promote efficient water usage. However, consumers may find it difficult to interpret complex water pricing structures, such as nonlinear two-part tariffs that serve multiple policy objectives. In this study, we examine consumer behavior following the introduction of volumetric pricing in Sacramento, California. Our analysis reveals that volumetric pricing reduces average water consumption by 5%, although there is considerable variation among consumer groups. Low-consumption households, whose bills decreased under the new pricing structure, increased their water usage by 4% despite facing increases in marginal price. These results suggest that consumers are more responsive to changes in their total bills than to marginal prices. We interpret this finding as evidence that consumers respond to changes in total bill amounts when making consumption decisions, which has important implications for designing rate structures in systems using two-part tariffs.

**Key Words:** water demand, pricing, water conservation, behavioral response

**JEL Codes:** Q25, Q52, Q53, L95

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## 1 Introduction

Efficient water management remains a critical challenge today, with over half of the global population affected by water shortages for at least one month each year (Intergovernmental Panel on Climate Change 2022). In the Western U.S., the growing gap between water supply and demand is highlighted by the recent agreement for Arizona, California, and Nevada to conserve an additional 3 million acre-feet of water by 2026. Over the past two decades, the region has experienced its driest period in over 1,000 years (Williams, B. I. Cook, and Smerdon 2022). Addressing water scarcity involves both supply- and demand-side solutions. Although most cost-effective supply-side innovations have already been exhausted, there is significant potential for water conservation through demand-side management. This approach hinges on understanding how individual and institutional incentives can drive behavioral change. Economists advocate for using prices as an effective means to encourage water conservation and reflect the scarcity of water (Olmstead 2010). However, political feasibility, social acceptance, and the lack of metering infrastructure to implement volumetric pricing pose significant challenges. Moreover, a critical question persists: how do consumers truly respond to water prices?

In this paper, we evaluate behavioral responses to water pricing by exploiting a wide-scale policy that installs water meters and switches consumers from fixed monthly bills to a *two-part tariff* containing a fixed monthly fee and the introduction of a volumetric price. Transitioning to volumetric pricing generates a large and salient increase in the volumetric price of water. Standard economic models presume that consumers respond to marginal price and therefore all consumers should weakly decrease consumption when marginal prices increase from zero to a positive level. Our results show that, on average, consumers do decrease consumption after the introduction of volumetric prices. However, the response is heterogeneous depending on how a household's total bill changes. The policy was designed to be revenue neutral, so a household consuming the average amount of water would see little change in their bill. Low-use consumers see their water bills decrease after switching to volumetric rates and these consumers increase their water use even though their marginal price increases. This finding provides evidence consistent with prior research that consumers are not aware of marginal prices (Ito 2014; Daniel A. Brent and Ward 2019) and may respond to changes in total bills as opposed to marginal prices

(Ito and Zhang 2025). This suggests consumers may respond to the fixed component of utility bills, which creates challenges for water managers that want to use marginal incentives to manage demand. It also questions recent recommendations for using progressive fixed costs to achieve equity goals in utility pricing (Levinson and Silva 2022) that assume fixed costs do not affect demand.

Our empirical setting is the City of Sacramento. In 2004, California passed a law that required all urban water suppliers to meter and charge customers based on volumetric water consumption by 2025. The motivation for the policy rested on the assumption that metering and volumetric pricing were effective water conservation tools. Due to the cost and logistic requirements of installing water meters, the meter installation roll-out was staggered. Importantly for our research design, household consumption was metered for a minimum of 12 months after the meter was installed, in what the utility referred to as the “tracking period,” before the household was switched to volumetric pricing. This design allows us to identify the effect of volumetric pricing by using households with installed meters, but still on the fixed bill, as the control group.

Our results find that the introduction of volumetric pricing reduces water consumption by about 4-8% on average. The range of estimates depends on how we deal with staggered adoption, which in our setting is extreme since households transition almost continuously over the sample period, with some large cohorts that transition contemporaneously. Additionally, the treatment effects are highly heterogeneous. When conditioning across the difference between “structural winners” (who are expected to see a decrease in their bills) vs. “structural losers” (who are expected to see an increase in their bills).<sup>1</sup> This finding suggests that consumers who see their total bill decrease, increase consumption even though the marginal price of water increases. The most logical explanation is that consumers adjust their behavior based on the change in their total bill, including the fixed component.

Responsiveness to fixed costs was theorized by Liebman and Zeckhauser 2004 as “schmeduling” when consumers respond to a simplified version of a complicated price schedule. Liebman and Zeckhauser 2004 describe one such simplification as “ironing” where consumers respond to average costs at a specific portion of the price schedule as opposed to marginal cost. Ito and Zhang 2025 find evidence of this behavior in a similar setting in heating reform in China where

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<sup>1</sup>We define a structural winner (loser) as a household whose bill goes down (up) absent any demand response.

consumers switched from flat to volumetric energy prices. Rees-Jones and Taubinsky 2020 find similar behavior in the context of progressive income taxes.

Our findings speak to the recent debate about whether consumers respond to average or marginal prices when facing nonlinear price schedules. While there is evidence that marginal prices matter for residential demand under nonlinear rate structure (i.e., increasing-block rates) (Nataraj and Hanemann 2011; Elinder et al. 2024), other evidence suggests consumers respond to average price (Ito 2014; Casey J. Wichman 2014). However, as noted in J. Cook and D. Brent 2021, the papers arguing that consumers respond to average price use average *volumetric* price, which excludes the fixed fee in a two-part tariff. Daniel A. Brent and Ward 2019 show that consumers do not have accurate information about the marginal price of water and are more informed about their total bill. This literature relates to work on rational inattention since it is costly to understand and define marginal price in two-part increasing block tariffs (Sallee 2014). However, assuming that consumers respond to average volumetric price requires more information and calculations than the marginal price in many settings, and the most salient incentive is likely the total bill. Responding to the fixed fee is a larger deviation from the standard consumer optimization model than simply misperceiving the correct marginal price (Casey J. Wichman 2017), and is consistent with a model of mental accounting where water bills are largely ignored unless they deviate enough from an expected range Thaler 1985.

Our research also contributes to the literature on estimating water demand and how consumer behavior affects water use. Meta-analysis of the demand elasticities literature by Dalhuisen et al. 2003 indicates that the variation in demand elasticities depends on the underlying tariff system, and the estimates also depend on the income in the study area. Recent papers estimate the impact of prices on water demand using household microdata and variation in prices and rate structures (Nataraj and Hanemann 2011; Casey J. Wichman 2014; Casey J. Wichman, Taylor, and Haefen 2016; Casey J. Wichman 2017; Daniel A. Brent and Ward 2019; D. Brent and Casey J. Wichman 2022; Elinder et al. 2024; Li and Jeuland 2023). However, there is still no clear consensus on which price is relevant for modeling consumer demand when facing nonlinear prices. Nataraj and Hanemann 2011 exploited the introduction of an additional block rate for some households served by the City of Santa Cruz Water Department, which almost doubled their marginal price. The affected consumers reduced water consumption and the demand elasticity to marginal price

to be -0.12, which suggests that consumers do respond to marginal prices if the deviations are large enough. In contrast, analysis of increasing block rates policy by Casey J. Wichman 2014 indicates that the consumers respond to average volumetric price with average price demand elasticities to be in a range of -0.43 to -1.14. A recent study by Elinder et al. 2024 uses daily hot-water usage data from a sample of Swedish apartments and the introduction of individualized billing to estimate that consumers reduce water consumption by 18% by the introduction of a marginal price. Understanding how consumers respond to prices is important for forecasting demand and setting rates to encourage conservation. We contribute to this literature with an empirical set up with city-wide exogenous shock leading to large — ranging from more than 65% decrease to more than 65% increase in monthly total bill amount, and salient change — with monthly billing information in water rates.

The paper is divided in six sections. Section 2 discusses the conceptual background and describes Sacramento’s water metering and volumetric pricing policy. We provide details on the data in Section 3. We describe our empirical approach and methodology in Section 4. The main findings are discussed in Section 5. We provide our conceptual explanation to the demand response in 6 before offering concluding remarks in Section 7.

## 2 Conceptual Background & Policy Setting

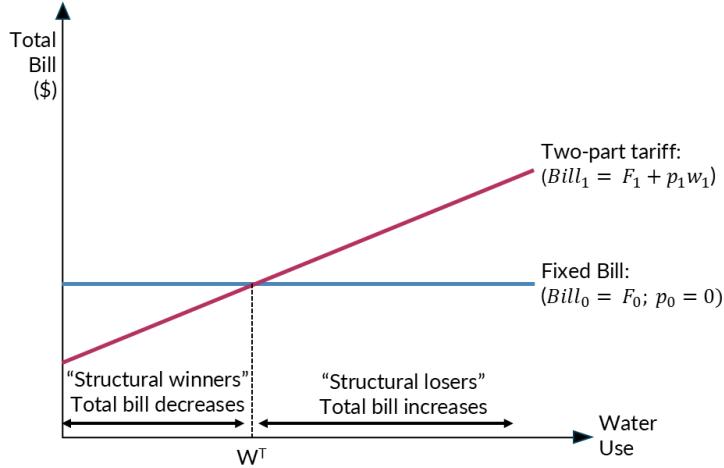
Municipal water suppliers are often bound by regulations to maintain budget neutrality. Therefore, large, revenue neutral changes in water rates may generate heterogeneous impacts to consumers with different consumption profiles. Our policy setting shifts consumers from a fixed monthly fee that does not depend on consumption (henceforth “fixed bill”) to a two-part tariff with a smaller fixed fee and a uniform marginal price (“volumetric pricing”). Figure 1 plots the total bill as a function of consumption under both rate structures. It is clear that low users pay less under the two-part tariff whereas high users pay more.

The heterogeneous effect permits an analysis of how consumers respond to marginal prices versus total bills. The bottom two panels of Figure 2 show the effects of the rate change for two classes of consumers (Low and High).<sup>2</sup> The neoclassical model that assumes consumers respond

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<sup>2</sup>Low and High can broadly be defined as consumers previously consuming below or above  $W^T$  in panel (a) of

**Figure 1:** Change in total bill due to two-part tariff



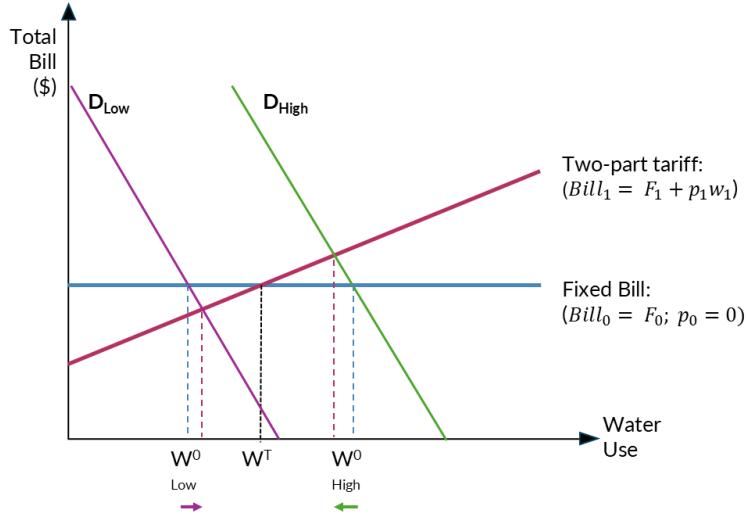
Note: This figure illustrates the rate structure change for Sacramento's household water metering under volumetric pricing. Prior to metering, consumers paid fixed bill based on number of rooms. With the new policy, a two-part (fixed fee + uniform marginal price) tariff was introduced. The fixed fee was reduced to about 30% of the fixed bill, and a uniform marginal prices were applied to water usage. As a result, consumers using less than  $W^T$  saw a drop in their total bill, while those consuming more than  $W^T$  faced an increase.

to marginal price, shown in panel (b), predicts that both classes of consumers decrease consumption as the marginal price increases. An alternative model of consumer behavior, shown in panel (c), assumes that consumers respond to average price. Consumer responses to either changes in total bills (panel (a)) or changes in average price (panel (c)) are different from responding to changes in marginal price. Here, consumer responses are consistent with "schmeduling," where low users are "structural winners" whose total bill decreases will increase consumption (Lieberman and Zeckhauser 2004). "Structural losers" are high users whose total bills increase along with their marginal price and they reduce their consumption.

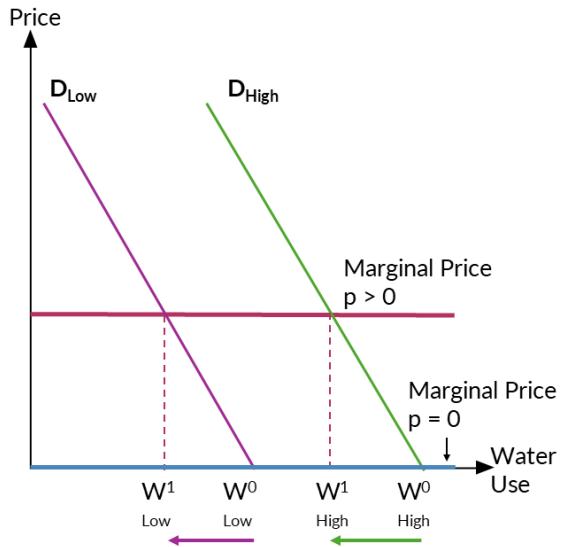
The presence of heterogeneous responses based on pre-rate change consumption suggests that consumers are responding more to average prices or their perceived total cost rather than strictly to marginal prices. This behavior aligns with a more nuanced understanding of consumer decision-making, where changes in consumption patterns reflect a responsiveness to changes total billed amounts rather than a the more standard response to changes in marginal price.

**Figure 2:** Theoretical basis of anticipated behavioral responses

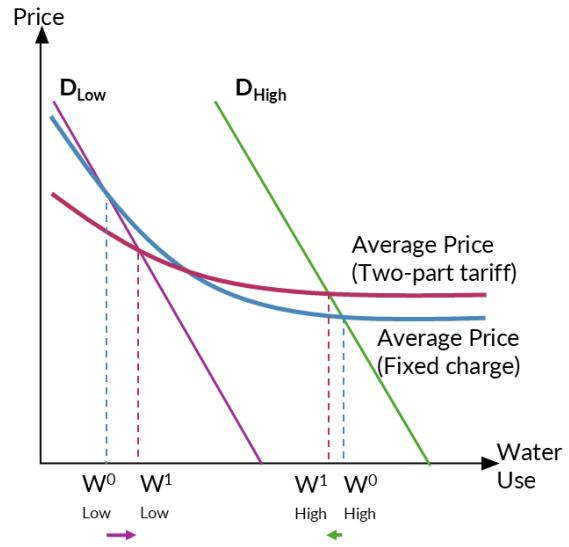
(a) Consumers responding to total bill



(b) Consumers responding to marginal price



(c) Consumers responding to average price

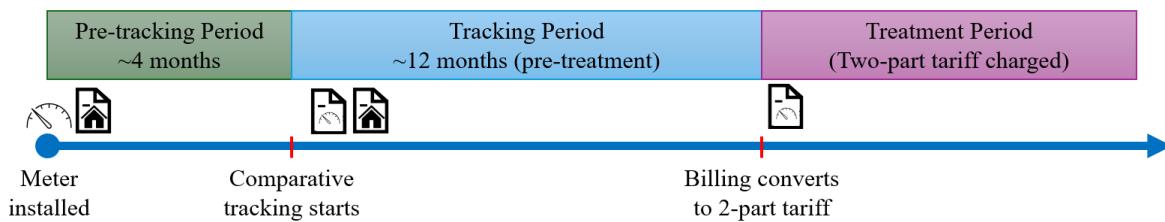


Note: Panel (a) illustrates the expected behavioral response to total bill change once the two-part tariff was introduced. The two-part tariff has two components: a fixed fee that consumers pay irrespective of usage, and a variable component based on water use and charged at a uniform marginal price. Consumers with higher water use experience increased bills and are expected to reduce consumption, whereas low water users face reduced bills and may increase consumption. Panel (b) shows the response to marginal prices, where demand drops across all consumption levels due to higher marginal prices. In Panel (c), consumers respond to average prices, leading to heterogeneous effects.

## 2.1 Water metering and adoption of volumetric pricing

We exploit a large-scale policy in Sacramento, CA, where the City installed water meters and transitioned customers from a fixed bill billing system to a two-part tariff. In this new rate structure, the fixed component (referred as a fixed fee in this paper) of the total bill is lower than the previous fixed bill, and consumers were charged a positive, uniform volumetric rate. In 2004, California passed Assembly Bill 2572 to require all urban water suppliers to meter and charge customers for the actual amount of water they use by 2025.

**Figure 3:** Metering rollout



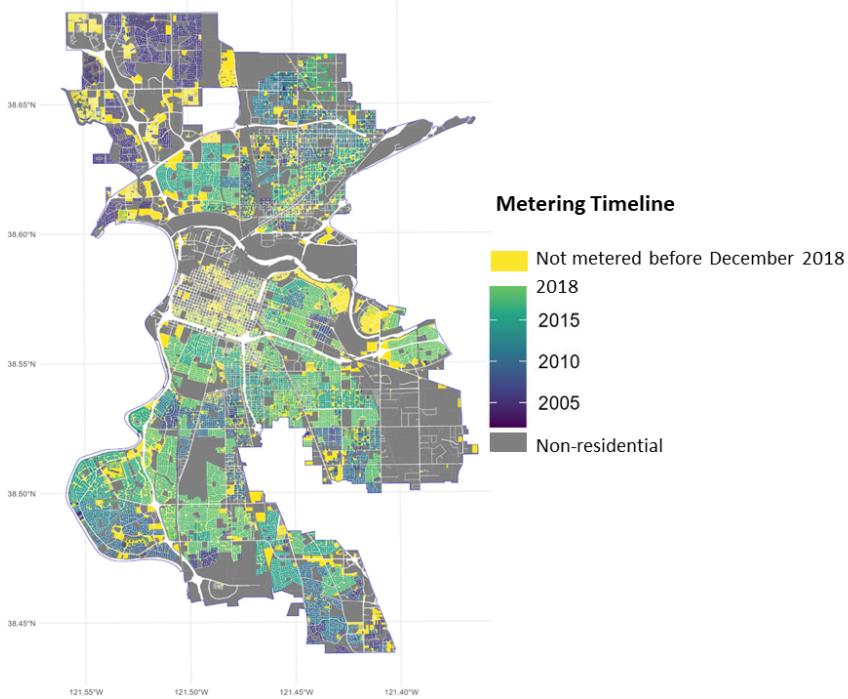
*Note: The metering program had three stages. After meters were installed, they were tested by the utilities for up to four months. Then they started providing comparative bills to the consumers for about a year. This is called the tracking period. After the tracking period ends, consumers switch to mandatory volumetric pricing.*

Sacramento's metering program was structured so that households initially received a comparative bill, detailing their volumetric consumption and what their charges would be under the volumetric pricing system, although they continued to pay the fixed bill for 12 months. This 12-month period of receiving comparative bills is referred to as the "tracking period." After this period, households were automatically switched to a two-part tariff. The two-part tariff contains a fixed monthly fee and a uniform volumetric rate charged on the water consumed. The fixed fee is based on the size of the meter installed. Starting in January 2010, Sacramento households that already had a meter for more than 12 months were transitioned to volumetric pricing (Figure 3). At the time, less than 20% of the City of Sacramento's water connections were metered.<sup>3</sup> As shown in Figure 5, the meters were installed gradually increased.

Meter installation stagnated during the drought period of 2014 to 2016, after which the city began a large-scale meter installation project in May 2017 called the Accelerated Water Meter

<sup>3</sup>The City's information about metering is available here: <https://www.cityofsacramento.org/Utilities/Water/Conservation/water-wise-tools/water-meters>.

**Figure 4:** Sacramento city land use and water metering rollout



*Note: Indicates the timeline for the meter installation in different single-family residential properties as per the SWA staggered water metering policy. We have data from January 2008 to December 2018. Households not yet treated in our data are assumed to be treated in 2025.*

Program (AWMP) with work scheduled to be completed in 2021. The water metering program had two goals. First, meters provide valuable information on each customer's water consumption as opposed to aggregated data, which could be used to detect inefficient usage or leaks. Second, meters allow the utility to charge customers based on how much water they use, which creates private incentives for conservation.

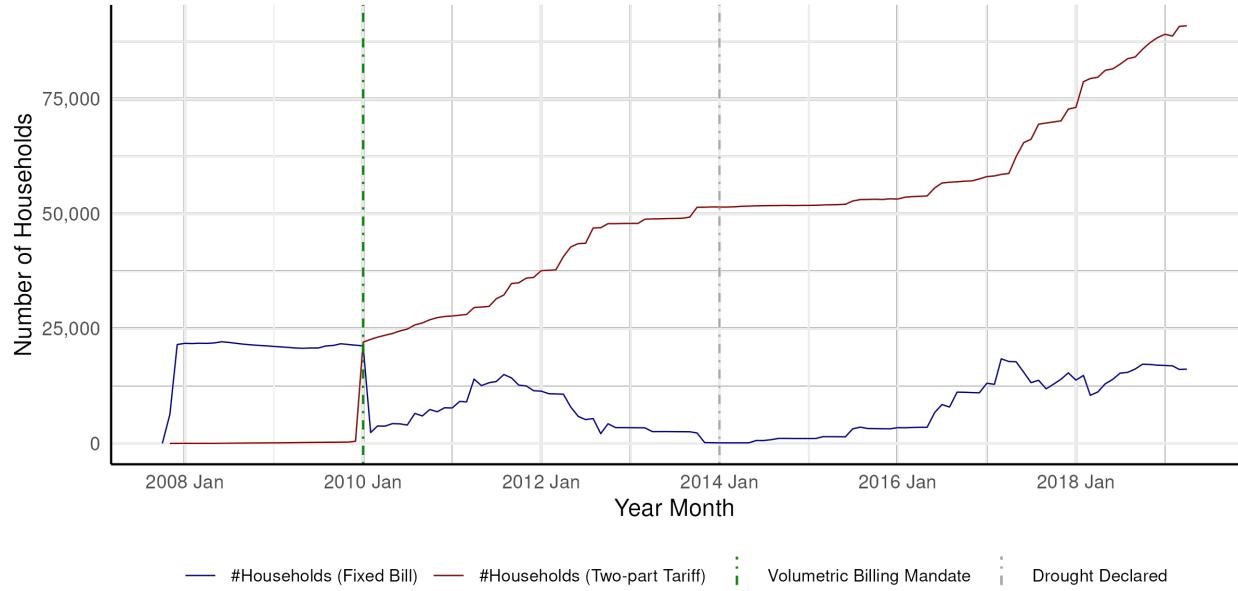
Before water metering was implemented, consumers were charged a fixed bill. The fixed bill was based on the number of rooms within the household. A room is defined as an area with a minimum of fifty square feet that is structurally or functionally distinct from other rooms or areas in a residential dwelling unit receiving domestic service. Room count was determined by the utilities department and/or the revenue division in accordance with the city's billing criteria. There were no surcharges levied during the summer months. This meant that each household paid roughly the same amount every month, regardless of actual water usage. With the introduction of water metering, households were informed for the first time about how much

water they consumed.

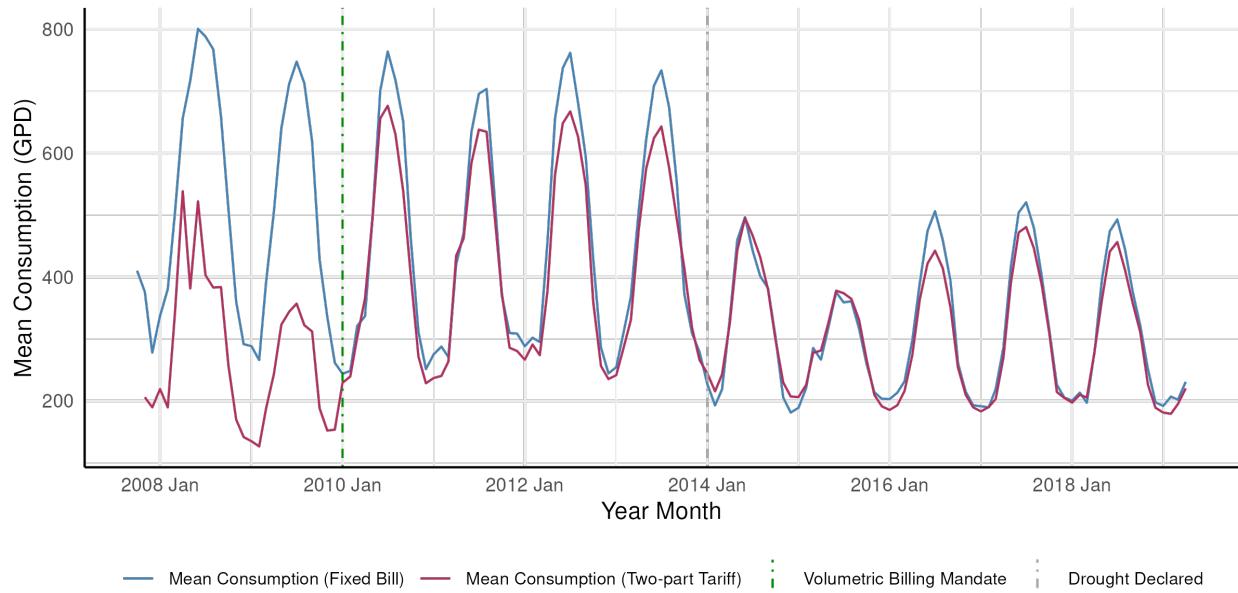
The volumetric rate was derived by the revenue department to recover the variable costs City of Sacramento incurs for maintaining the infrastructure that delivers water services to customers. The billing data indicates that the fixed fee dropped to an average of around 70% of the total bill once consumers switched to volumetric rates.

**Figure 5:** Rollout of water metering, volumetric pricing and mean consumption

(a) Number of households under tracking and volumetric billing



(b) Mean daily water consumption



Note: The figure shows the number of households and their mean daily water consumption (gallons per day (GPD)) during the “tracking period” when consumers received information on volumetric consumption and a bill comparison between the flat-fee and the two-part tariff with volumetric pricing. Our data panel starts from Jan 2008 and ends in Dec 2018. Before volumetric pricing started in Jan 2010 (indicated by green line), around 20 percent of households were already metered but very few (<300) were charged a volumetric price.

## 2.2 Staggered roll-out of metering

The City of Sacramento's metering policy had three stages for an individual household. First, a meter is installed on a customer's property. Second, four months after installing the meter, during which the utility can monitor consumption and ensure the meter is functioning properly, the customer starts receiving the "comparative water bills" for approximately 12 months. During this period the customer still pays a flat (i.e., non-volumetric) monthly fee for water, but they observe how much they would pay once they switch to the uniform volumetric rate. This information can help a customer plan for the upcoming switch to volumetric rates. We call this period the "tracking period." We observe most that customers metered between January 2010 and January 2018 received 10 to 13 tracking period bills (see Figure 15). Third, after the customer received comparative water bills for 12 months, they were permanently switched to the two-part tariff, which we call the "volumetric period." Customers can voluntarily switch to the volumetric rate at any time during the 12 months of comparative water bills. Figure 4 shows the spatial pattern of meter installations in Sacramento. Meters were installed based primarily on the engineering feasibility. Discussions with the utility manager suggested that meter installations were planned to coincide with scheduled maintenance for water distribution lines. For this reason, the timing of meter installations is not likely to be structurally correlated with water consumption. Additionally, our primary empirical strategy focuses on using households with meters installed within 4-8 months of treated households as a control group. Thus, eliminating potential endogeneity in metering roll-out.

The water metering and volumetric billing followed a staggered roll-out and it is unique in three ways: 1) there are many treatment cohorts, 2) there is limited pre-treatment data and thus limited overlapping time periods for counterfactual groups, 3) there is seasonality in water use.

New meters were installed each month and households were switched into the tracking period and volumetric pricing period continuously over time. Between January 2010 (when volumetric billing starts) and December 2018 (when our data ends), there are 108 treatment cohorts. Some of them have larger cohort sizes than others. Figure 5 shows the total number of households in tracking and volumetric billing periods at each point in time. Additionally, the staggered adoption does not exhibit uniform roll out, which means there are more than 2000 households

treated in October 2013 but no treatment cohorts with more than 100 households treated between November 2013 and June 2015.

Household consumption is not measured until meters are installed. So we can only focus on the data available from the tracking and volumetric periods. This practical limitation has two primary implications: First, it limits the number of potential counterfactual consumption observations for comparison with the treated groups. For example, a group of households treated in time  $t$ , called “Cohort  $t$ ,” will have  $t - 12$  to  $t - 1$  observations in the “tracking period.” These observations are the pre-treatment period in the conventional difference-in-difference (DiD) design. Our data set ends in December 2018, so households in cohort  $t$  have  $t + 1$  through December 2018 as their post-treatment period. To construct a counterfactual for cohort  $t$ , we require tracking-period observations and pre-treatment observations that overlap with the treatment period to test for parallel trends. This limits the set of potential counterfactual groups for cohorts treated up to 12 months after time  $t$  (i.e., cohorts  $t + 1$  through  $t + 12$ ). For example, using cohort  $t + 4$  as a counterfactual for cohort  $t$  results in four periods where the counterfactual cohort has pre-treatment data that overlaps with cohort  $t$ ’s post-treatment data ( $t + 1$  through  $t + 4$ ) and eight periods with overlapping pre-treatment data to evaluate the parallel trends assumption. Ideally, we would want counterfactual data with perfect overlap with treatment timing. However, the policy design was conducted with an irregular roll-out, so there are varying degrees of comparison cohort observations before and after treatment.

Seasonality in water use also complicates our empirical design. As shown in Figure 5, monthly water consumption is lowest during winter months. From March onwards, water use gradually increases to a summer peak in June and July, and it again starts gradually decreasing from August and repeats the cycle from December. Because we do not have a full 12-month overlap between treated and comparison groups, we need to rely on a counterfactual group with sufficient overlap. To do this we adopt a rule that ensures a minimum of four months of overlap between treatment and counterfactual consumption. Thus, we can use data from treatment groups  $t + 4$  to  $t + 8$  to ensure reasonable overlap in tracking period for cohort  $t$ ’s pre-treatment and post-treatment periods (see the illustration in Figure 17).

These practical implementation challenges limit our ability to evaluate long-term treatment effects. We do not have enough counterfactual observations beyond six months to make a fair

comparison between treatment and control observations. As a result, we focus on short-term (within 6 months) treatment effects only.

## 3 Data

### 3.1 Household billing records

Our primary data are billing records obtained through a partnership with the California Data Collaborative (CaDC). The CaDC is a non-profit that works as an intermediary between researchers and water utilities and produces original analyses of California water data. Through this partnership, we obtained the universe of billing data for the City of Sacramento, including unique records of billed amounts and consumption from each account in the City's database. We use data for single-family residential properties in our analysis. The data distinguishes water accounts from the property. If property ownership is changed, the new resident will get a new unique utility billing id. This allows us to track consumption and billed amounts for each account. In our data, we observe 107,929 single-family properties and 150,823 unique accounts. The data include the total billed amount (whether on flat-rate or two-part tariff), water consumption during the tracking period and volumetric periods, and the volumetric price and fixed fee during the volumetric period. We also observe any changes to the bill due to utility assistance programs and vacancy adjustments during the flat period. Although the utility did monitor consumption for 2–4 months after installing the meters, but prior to sending comparative bills, we do not observe these records as they did not enter the billing system.

Our data include all billing records from January 2008 through December 2018. Figure 5 shows the transition over time to the different rates. The transition to volumetric rates began primarily in January 2010. After that time, the number of households on volumetric bill consistently increases. If accounts changed within a property, the new account-holder did not receive the tracking period information and they would start by paying volumetric charges only. Thus, in our sample, there are 93,606 such households with the tracking period consumption records available and we observe tracking as well as volumetric consumption data for 73,941 households.<sup>4</sup>

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<sup>4</sup>We process the data for anomalous or potentially erroneous records. We remove all bills that are less than zero or over \$1000. We also drop water consumption records greater than the 99.9th percentile of consumption. Lastly, we drop very short (<15 days) and very long billing (>90 days) periods; the typical billing period is one month,

Although the metering policy intended for each account to have 12 months of tracking data prior to transitioning billing, in practice the number varies. Consumers who were already metered before volumetric pricing started in January 2010 had longer than 12 months in the tracking period (around 25% of the sample). Around 5% of consumers opted into volumetric pricing at least two months earlier than 12 months leaving fewer tracking period observations.<sup>5</sup> The rest of the households have received 10 to 14 bills during the tracking period.

Customers are billed monthly but the exact number of days within each billing period may vary, thus, we use gallons per day (GPD) to calculate average daily consumption during the billing period. Figure 5 shows the cyclical pattern in water usage. Household water consumption peaks during the summer months typically between May to August and it is lowest during the winter period between November to February. The sharp rise during the summer months is due to outdoor use (such as for watering lawns or gardens). Summer consumption averages roughly three times winter consumption. Figure 5 shows the change of water use over time for households on the flat fee (during the tracking period) and on volumetric rates. Water use is typically higher for households in the tracking period, but it is important to note that the composition of households in the tracking and volumetric groups is also changing over time.

One contemporaneous shock to water use in our period was the severe drought in California that officially started in January 2014 when the governor declared the drought a state of emergency. Water consumption for both groups drops severely in 2014 in response to the drought. After the drought, the consumption gap between households billed on the flat fee vs. volumetric rate also decreased. This decrease is likely due to non-price demand-side management policies such as outdoor water restrictions and public messages about the need to conserve water (Buck et al. 2016; Newkirk 2014; Brekke 2014; Maddaus et al. 2020; California Department of Water Resources 2021).

To highlight how the transition to volumetric billing changes households' total bills, we calculate the predicted bill that a household would pay had they been on volumetric rates during or approximately 25-35 days (see Figure 16). We also drop households with < 1 gpd average usage during the pre-treatment period, assuming these might be vacant households. The data cleaning drops fewer than 1% of the observations in our data.

<sup>5</sup>Our data does not reflect the early opt-in decisions but based on the length of tracking period we observe, 5% of total households who transitioned have tracking period less than 300 days and can be considered to have volunteered for the early switch.

the tracking period. The utility provides this information as part of their bill during the tracking period when they receive comparative bills. We only have access to the volumetric price so we use the household's average fixed fee during the volumetric period as an estimate of the "pre-treatment" fixed fee. However, we note that the fixed fees are rising over time in our data. The same households pay higher fixed fee as the metering charges increase over time. Thus the imputed fixed cost during the tracking period might be an overestimate, which would bias our estimate of the predicted bill upwards. In the early periods of the sample, almost all households would pay more under volumetric rates. However, as time progresses and the flat rate increases, more consumers would be better off under volumetric pricing absent any demand response. Typically, low users see smaller bills when switching to volumetric rates even if they do not change their behavior, while high users pay substantially more than they previously paid.

We use this variation between predicted and observed bills to test the heterogeneous response to an increase in marginal prices. We plan to cluster the households within three categories based on the bill differences, (1) Winner: experiencing monthly bills going down, (2) Neutral: experiencing minimal changes, (3) Loser: increased bill on transitioning from flat-fee to volumetric metering. We further discuss the quantitative definitions behind different ways to assign the three groups in Section 5.

### 3.2 Household characteristics

We also use CoreLogic's property characteristics data from 2020 (CoreLogic Inc 2020). Given our focus on understanding how household characteristics explain household-level water consumption, we use three primary variables that explain distinct components of household water consumption. Total number of bathrooms (indoor use), potential lawn area defined as lot size minus ground floor area (potential irrigation water use), and the presence of a swimming pool (outdoor swimming pool use). We also use year built to check for any vintage effects that could affect water efficiency. The data is matched with the household billing data using the APN (Assessors' Parcel Number).

## 4 Methodology

The empirical specification builds on a two-way fixed effects model (TWFE) with both account and year-month fixed effects to estimate the effect of transitioning to volumetric billing as shown in Equation 1.

$$\ln(GPD_{it}) = \alpha_i + \tau_t + \delta \mathbf{1}[\text{Volumetric}_{it}] + \epsilon_{it} \quad (1)$$

Our dependent variable is the natural log of water consumption in gallons per day (GPD) for household  $i$  in billing period  $t$ .  $\mathbf{1}[\text{Volumetric}_{it}]$  is an indicator equal to one if a household faced volumetric prices at time  $t$  and zero otherwise. We include household fixed effects ( $\alpha_i$ ) to account for any unobservables unique to the household and year-by-month fixed effects ( $\tau_t$ ) to control for any common time-varying factors at the utility level such as changes in weather, marginal prices, and non-price conservation policies over time. Our primary identifying assumption is that the tracking period observations serve as a valid counterfactual for consumption during the volumetric period, conditional on household and time fixed effects. Within this generalized difference-in-difference design,  $\delta$  can be interpreted as the Average Treatment Effect on the Treated (ATT).

We also analyze heterogeneity by key factors that could affect how households respond to volumetric billing. First, we examine heterogeneity across the distribution of consumption and expected bill differentials. Many low users may see their bill decrease after transitioning to volumetric pricing, while higher users will experience larger bill increases. We estimate our primary specifications within structural winners and losers based on the predicted bill differential. We also test whether the timing of the transition matters. Water use is much higher in the summer, and therefore volumetric bills will be relatively higher than flat rate bills during the summer. We run separate regressions for whether the transition occurred in the summer or winter. Lastly, the 2014 drought dramatically reduced water use due to public messaging campaigns and outdoor watering restrictions. We test whether volumetric pricing was more or less effective at reducing demand before and after the drought.

Our estimation framework is a difference-in-differences (DID) design with staggered adop-

tion. Many recent papers show that the TWFE model is only appropriate when assuming homogeneous treatment effects (De Chaisemartin and d'Haultfoeuille 2020; Goodman-Bacon 2021; Callaway and Sant'Anna 2021; Sun and Abraham 2021; Dube et al. 2023), which may be unrealistic here. The staggered adoption in our setting is unusual, which prevents us from using the most common estimators proposed in the literature. In our setting, there are many treatment cohorts: every month new households are metered and transitioned to volumetric pricing and some cohorts have large number of treated households whereas most others have very few treated. This challenge makes applying off-the-shelf staggered DiD estimators computationally burdensome.

#### 4.1 Custom matching-based treatment effect estimates

To address the staggered adoption we augment the stacked regression approach—motivated by Cengiz et al. 2019—with matching to better defend the parallel trend assumption. We describe this for a representative cohort to make the estimation strategy transparent. First, we define a treated cohort as all households who transitioned to volumetric prices within the same month, have at least 4 consecutive bills in the tracking period, and at most 14 bills during the tracking period. We refer to a cohort that transitions in month  $t$  as cohort  $t$ . The corresponding eligible group is defined as households with at least 4 consecutive months of tracking period observations before and after cohort  $t$  transitions to treatment as explained in Section 2.2. For example, “cohort January 2010,” a household is conserved an eligible control if and only if it has tracking period bills in the month of September 2009 ( $t - 4$ ) through April 2010 ( $t + 4$ ). The logic of this approach is identifying cohorts that transitioned at similar times. Overlap ( $t - 4$  through  $t - 1$ ) in the tracking period allows the most direct test for parallel trends that captures both the cyclical and acyclical time trends in water use (such as drought-induced demand response). Tracking-period observations in the control group during the treated cohort’s treated period ( $t + 1$  through  $t + 4$ ) are required to estimate treatment effects. The cohort treated in time  $t$  together with the corresponding control group forms a “Group  $t$ .” We estimate an average treatment effect for each such group using standard two-way fixed effects regression and refer to the estimand as the Group Average Treatment Effect (GATE), which is commonly used term in the staggered adoption literature.

We test the estimator for treatment groups between January 2008 and September 2018. We visualize the data availability for treated households and their respective counterfactual groups in Figure 6, panel (a). The figure shows the time series of number of eligible households available for each treated cohort, where we define eligibility based on having 4 month overlap during the pre-/post-treatment. We take a cross-section for the two sample cohorts, June 2011 and December 2017 to analyze availability of treated and control group observations. As shown in Figure 6, panels (b) and (c), there are not enough control group observations beyond six months of treatment during pre- or post-treatment periods. Thus, we restrict our estimation to within 6 months of treatment. Once we estimate the GATE for each group, we take a weighted-average of all groups to estimate the final treatment effects, as well as test for parallel trends using event-study plots. The assigned weights are defined based on the total number of treated households in each group divided by the sum of treated households across the groups. For instance, Group  $G_x$  has  $N_x$  households treated out of a total  $N_{Matched}$  that remain after filtering households that have tracking period between 4 to 14 months. Hence, that cohort will receive a weight:  $N_x/N_{Matched}$ .

In each group-wise subset of the data, we estimate the GATE using three methods. First, we develop the group-wise stacked regression approach, estimating GATE by using two-way fixed effect models within each group. We then manually take a weighted average of GATE to calculate the ATT.

Second, we use matching on the pre-treatment outcome for control group within each treated cohort to strengthen the parallel trend assumption. While there is some skepticism about using matching on pre-treatment outcomes in DiD frameworks, Chabé-Ferret 2017 suggests either estimating DiD without matching (as done in the first case) or if matching on outcome variable is used, match on multiple (ideally 3 or more) pre-treatment outcomes. In our design, we are able to match on four pre-treatment observations of monthly water use. We perform nearest-neighbor matching based on these four months of overlapping data during the tracking period. Matching on more than four months reduces the availability of control group observations, especially post-treatment. To exclude households without close matches, we use a caliper of 0.2 standard deviations.

Third, we match on static household characteristics. We match households based on characteristics likely to influence water use: year of construction, number of bathrooms (a proxy for

indoor water use), presence of a pool (swimming pool use), and potential lawn area (calculated as the difference between lot size and ground floor area, representing potential irrigation use). For this matching, we use Mahalanobis distance, a preferred method for multivariate matching, and apply a caliper of 0.2 standard deviations to exclude households without close matches.

For these estimators, inference is drawn using standard errors derived through bootstrap method. Bootstrapping includes four steps in our case. First, we use the group-level treated and control group data and draw a random sample with replacement. Second, we estimate the GATEs for these randomly sampled group-level data and aggregate all GATEs into ATT for the iteration. Third, we iterate same process for 100 times.<sup>6</sup> Fourth, we take standard deviation of 100 ATTs from these iterations and use it as a standard error estimates.

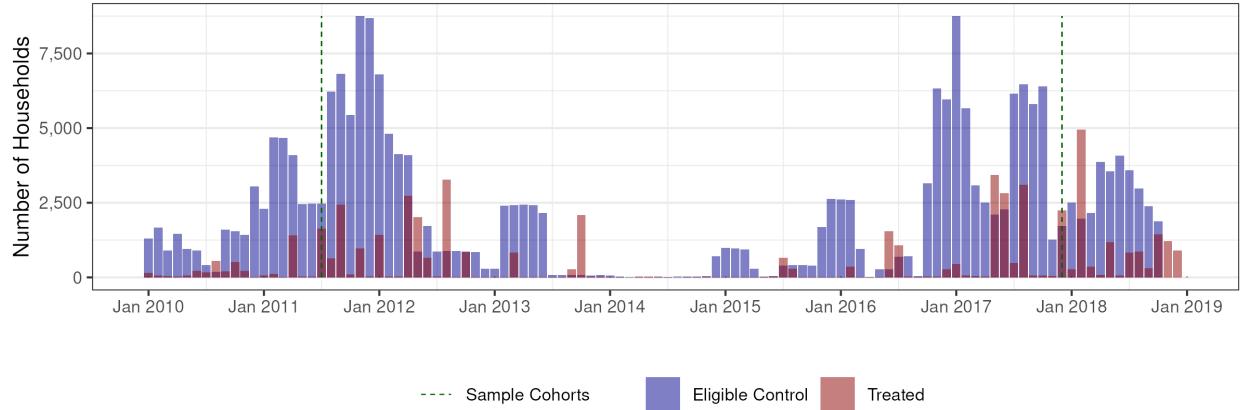
For each of the three estimators, we restrict our sample to within 6 months of the treatment period. As shown in Figure 6b & 6c, we do not have enough valid counterfactual observations beyond 6 months of the treatment and we have no observations beyond 12 months, due to the 12-month tracking period.

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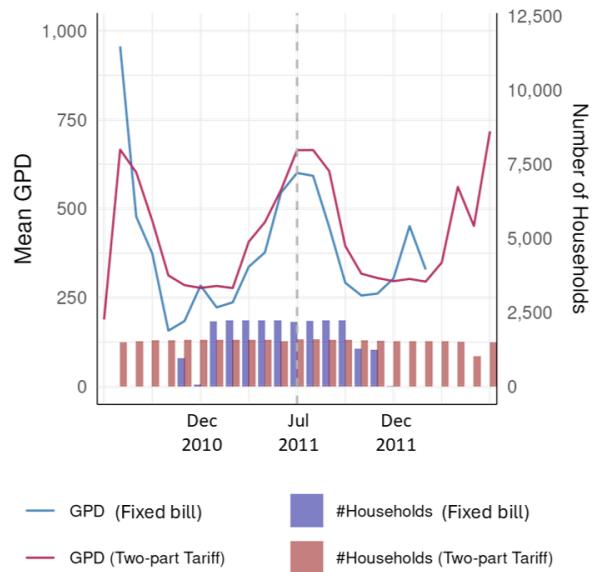
<sup>6</sup>For sample iterations, we tried 200/500/1000 iterations as well but the standard deviations did not significantly change for additional iterations and the distributions for ATT estimates with 100 iterations were convincingly normal.

**Figure 6:** Treated and eligible control groups

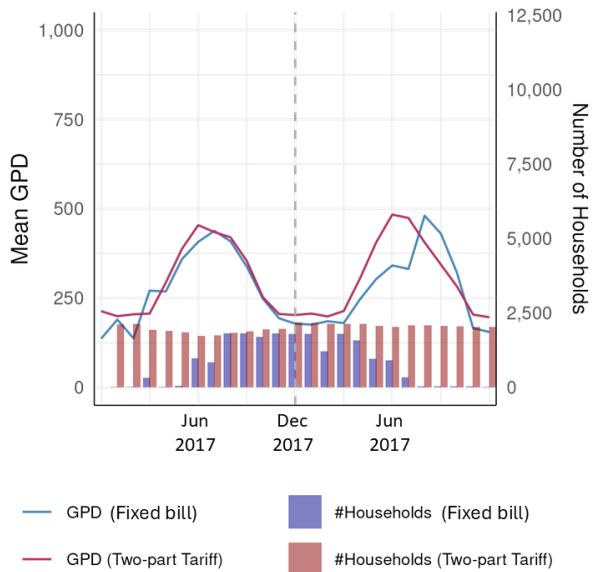
(a) Cohort-wise treated and eligible control group



(b) July 2011 group



(c) December 2017 group



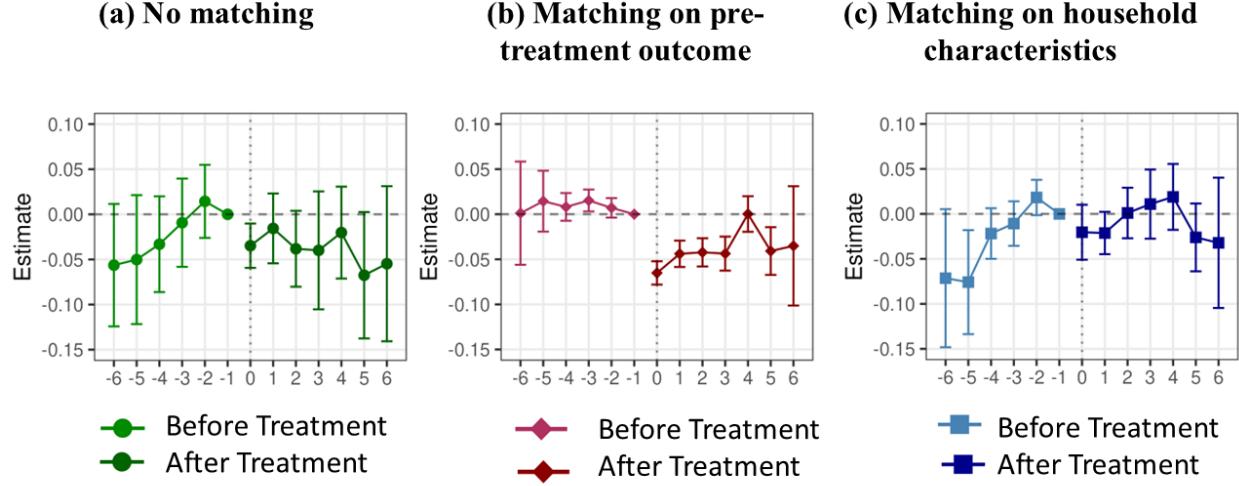
Note: This figure illustrates the staggered adoption of volumetric billing and our approach to constructing cohort comparison groups. Panel (a) shows the number of households treated in a specific cohort. Cohort “Jan 2010” refers to all households who switched from the tracking period to the volumetric period in Jan 2010. The corresponding “Eligible Control” group is defined such that the control group households have tracking period observations for at least 4 consecutive months before and at least 4 months after the switch for the treatment cohort. For example, for Cohort Jan 2010, a household is considered eligible control if and only if it has tracking period bills for Dec 2009 ( $t-1$ ), Nov 2009 ( $t-2$ ), Oct 2009 ( $t-3$ ), Sep 2009 ( $t-4$ ), and has 4 observations after Jan 2010 ( $t$ ). Panel (b) and (c) show the number of treated and eligible control groups and their mean daily water consumption (gallons per day (GPD)) for early-treated cohorts “Jun 2011” and “Dec 2017”, respectively.

**Table 1:** Average treatment effects on the treated

	log(Gallons per day)					
	TWFE			Aggregate-GATE		
	All Records	No Always Treated	No Always Treated (Within 6 months)	No Matching	Matching on Pre-treatment Outcome	Matching on Household Characteristics
	(1)	(2)	(3)	(4)	(5)	(6)
<b>Volumetric Pricing</b>	-0.100*** (0.002)	-0.076*** (0.002)	-0.028*** (0.004)	-0.033 (0.026)	<b>-0.052***</b> <b>(0.006)</b>	-0.001 (0.01)
<b>Accounts</b>	150,823	93,606	93,550	93,606	<b>93,606</b>	93,606
<b>Treated</b>	73,941	73,941	73,885	-	-	-
<b>Matched</b>	-	-	-	48,254	<b>41,024</b>	48,254
<b>Std. Errors</b>	Clustered at account level	Clustered at account level	Clustered at account level	Bootstrapped	<b>Bootstrapped</b>	Bootstrapped
<b>Dropped Accounts</b>	-	No Always Treated	No Always Treated	No more than 14 months or less than 4 months tracking	<b>No more than 14 months or less than 4 months tracking</b>	No more than 14 months or less than 4 months tracking

Note: This table compiles Average Treatment on Treated (ATT) estimates using different methods. In column (1), we use all available observations across all households with a standard two-way fixed effect (TWFE) estimator. For columns (2) and (3), we apply TWFE but drop always-treated households, as recommended by the staggered adoption DiD literature, and further limit observations to within 6 months of the switch to volumetric rates in column (3). For columns (4)–(6), we devise group-level estimators. We filter cohort-level treated households so that they were treated in the respective cohort and not tracked less than 4 months or longer than 14 months. The eligible control group consists of households with observations overlapping for at least 4 consecutive pre-treatment and 4 post-treatment months. The estimator in column (4) uses no matching and provides a cohort-weighted average of cohort-level TWFE estimates for treated and eligible control groups. Column (5) adds a 4-month dynamic matching of pre-treatment outcomes (gallons per day) for cohort-level estimates. In column (6), matching is based on observable household characteristics. We prefer ATT estimates from column (5) as they effectively address staggered adoption concerns and offer a defensible event study analysis. \* $p<0.1$ ; \*\* $p<0.05$ ; \*\*\* $p<0.01$

**Figure 7:** Event study plots for the overall estimates



Note: In this figure, we use event study plots for within 6-months ATT based on three different estimation methods to identify the suitable overall treatment effect strategy. The method is suitable if there is no trend before the start of the treatment effect, hence defend the no parallel trend assumption for the difference-in-difference estimation. Parallel trend assumption is hard to defend for panel (a) and (c). Thus, matching on pre-treatment outcome-based estimator which suggests no pre-trend shall be a good fit for ATT estimates.

The three estimators have significantly different point estimates. We check validity of these three new estimators using event study plots to defend the parallel trend assumption and identify the estimator of choice. Figure 7 has event study plots for the selected data for each of the three estimators. The event study plots for “No matching” and “Matching on household characteristics” (see Figure 7 (a) and (c)) suggest that the pre-treatment water use across treatment and comparison households are not convincing to assume parallel trends. For “Matching on pre-treatment outcomes,” we find a more convincing evidence for parallel trend assumption. It should also be noted that the parallel trends holds beyond the periods where we explicitly match ( $t = 5$  and  $t = 6$ ). Thus, we prefer the matching on pre-treatment outcome estimator for our further analysis and identification of overall GATEs, as well as the heterogeneous treatment effects. This estimator intuition is based on limiting the counterfactual households to households that had a meter installed during a similar time and had similar water use immediately prior to switching to volumetric rates. We use the “Matching on pre-treatment outcomes” as an estimator

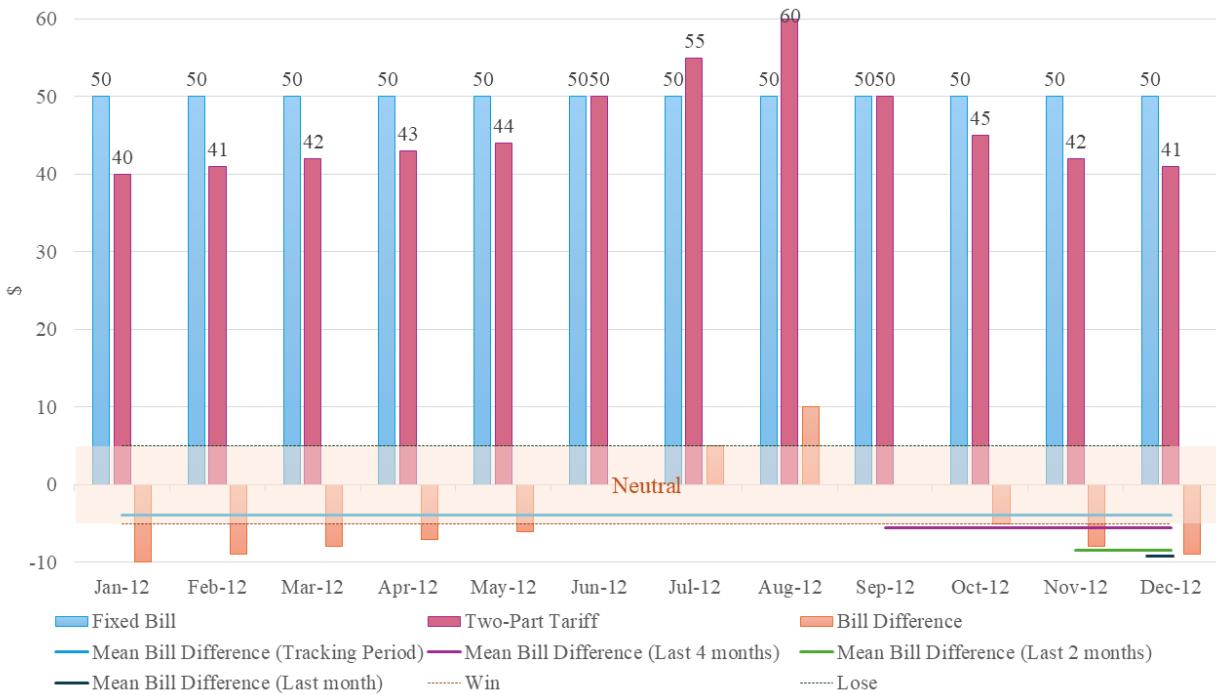
of choice for rest of our analysis. Our core analysis will focus on the heterogeneity in treatment effect based on the observed bill difference during the tracking period for the transitioning the billing flat fee to volumetric pricing.

## 4.2 Defining winners and losers

The core analysis examines consumers response heterogeneity to tariff changes by categorizing them into three groups, based on the difference between their actual bills (under the fixed tariff) and predicted bills (under a two-part tariff) during the tracking period. This heterogeneity is defined using the average bill difference across four *observance periods*: (1) the entire tracking period, (2) the last four months, (3) the last two months, and (4) the final month. These observance periods reflect different lengths of time during which consumers may have been exposed to price signals and had the opportunity to adjust their behavior accordingly. If the household experiences the average bill during chosen period of observance drops by more than \$5, the household is defined "Winner". If the average bill increases by \$5, the household would be expected to pay higher bill during when transitioned to two-part tariff pricing and shall be defined as "Loser". We define the "Neutral" group with household experiencing the average bill increase or decrease during the chosen observation period to be within \$5. For example, consider a hypothetical household that was paying a flat bill of \$50 every month and expected to transition into volumetric pricing in January 2013. As shown in Figure 8, let's assume their predicted bill during the 12 months of tracking period between January 2012 to December 2012 is \$40, \$41,\$42, \$43,\$44, \$50, \$55,\$60,\$50, \$45,\$42, and \$41. Thus, their mean bill difference during the entire tracking period is \$3.91, which is within \$5 assigning the household neutral based on the entire tracking period. When we consider last 4 months only, September 2012 to December 2012, the average bill difference is \$5.5. So the heterogeneity defined based on last 4 months of observed bill difference would categorize the household as "Winner". We use the 4 month definition as our preferred estimator matches the outcome of last 4 months and we are interested in testing if the household would be more vigilant during last few months before transitioning. We further define heterogeneity based on last 1 months and last 2 months of tracking to check if the households are more vigilant immediately prior to transitioning to two-part tariff.

The rationale behind assigning the winners and losers is to test the hypothesis whether household respond to marginal prices or the demand response to prices depend on what they see on their total bills. If the consumers respond to marginal prices as illustrated in Figure 2a, we shall expect the Winners increase their water consumption on transitioning to two-part tariff and Losers shall reduce the consumption. The empirical set up would not be able to distinguish the demand response to "Total Bill" vs "Average Price" since they are correlated. However, it might be difficult to assume that the households understand quantity of water they use in order to be responsive to average price. It might be cognitive ease for the consumers to make their water consumption decisions just based on the total bill difference they experience.

**Figure 8:** Illustrative example for categorizing winners and losers



*Note: The figure explains a hypothetical case of a household during the tracking period where they receive information about predicted volumetric bills while still being charged a fixed bill. It also illustrates how the mean bill difference is estimated using four different definitions based on the observance period.*

## 5 Results & Discussion

### 5.1 Consumer water use adjustments following tariff reform

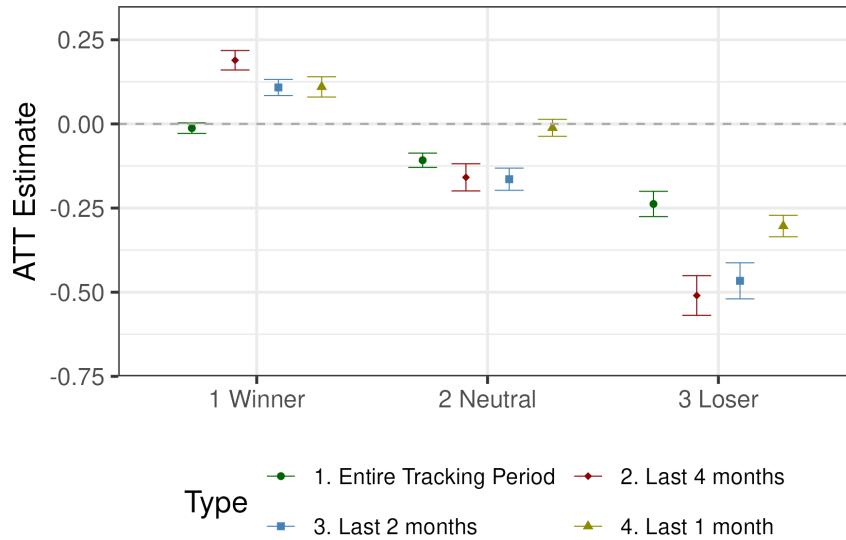
We estimate treatment effect of transitioning to volumetric pricing across the three categories of households and four observance period based bill difference. The point estimates and standard errors estimated through bootstrapping are compiled in Figure 9. The estimates use the same estimation model but separately estimates ATTs for the three categories defined based on the bill difference. We find strong evidence of heterogeneous responses. The Winners based on observance period shorter than four months significantly increased their water consumption. For full length tracking period, the Winners did not increase the consumption but they also have similar trend of heterogeneity across three categories. This results can help us interpret two things. First, the consumers do not respond to marginal prices, if they were responding to the marginal prices, we should not expect the strong heterogeneous responses across the three groups. Second, the consumers are responding to the total bill difference is more evident for the shorter time period. Focus on the estimates for 4-months and 2-months observance period based heterogeneity, the estimates reflect similar trend. Albeit the trend is slightly stronger for 4-months than 2-months, they indicate consumers respond to the total bill they observed in last few months and may adjust their usage based on the experienced bill difference, if their bill are expecting their monthly water bills to be dropped more than \$5, they would increase their water consumption. The trend for 1-month based categorization is interesting too. They winners, neutral and loser align perfectly with the hypothesized total bill response with neutral group having zero change. Overall, the results backs the hypothesis that the consumers are responding to changes in their total bill.

It is important to note that defining consumer heterogeneity based on the entire tracking period is a particularly stringent criterion. Due to seasonal variation in water use, much of the observed difference between fixed and predicted volumetric bills is averaged out over time (as illustrated in Figure 8). As a result, only those consumers whose predicted volumetric bills remain consistently lower than their fixed charges throughout the tracking period are classified as winners under this definition. However, because our estimator relies on a six-month pre-/post-transition comparison, consumers who had already adjusted their consumption in response to

consistent savings may exhibit little to no change in the estimation window—making their behavioral response difficult to capture.

Given this limitation, a more behaviorally relevant approach is to assess heterogeneity based on the billing information available to households shortly before the transition. Bill differences over the final four months, for instance, may better reflect the information consumers used to form expectations and make decisions. At the same time, relying on only the final month could introduce noise due to temporary anomalies and it is hard to expect that the consumer would make decision based on just last one month of observed bill difference. Therefore, we consider the four-month average bill difference to be the most appropriate observance period for capturing consumers' experienced incentives and likely behavioral response and use it for the further analysis.

**Figure 9:** Treatment effect estimates



*Note: The bars represent 95% confidence intervals generated from bootstrapped standard errors. Each parameter is based on a separate ATT estimation. The "Type" label indicates the length of the tracking period used to define the average bill difference, which is subsequently used to define heterogeneity.*

## 5.2 Treatment effects during drought

Our set up has an additional exogenous shock. Governor of California declared drought in January 2014 and this generated substantial public pressure and demand side management that

reduced water demand (California Department of Water Resources 2021; Maddaus et al. 2020). Figure 5 shows a dramatic decrease in consumption for both two-part tariff and fixed bill households after 2014.<sup>7</sup> This decrease in consumption prior to switching to two-part tariff rates also makes households more likely to be winners.

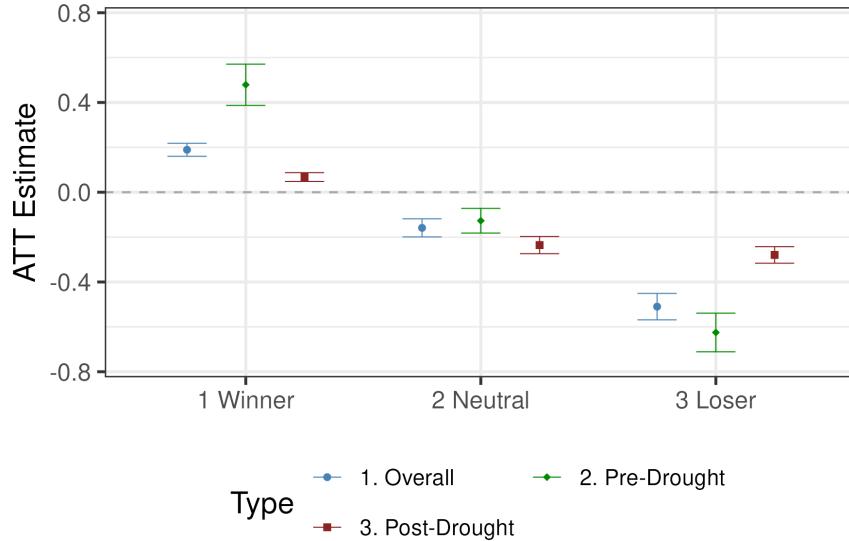
The overall estimates are compiled for the groups transitioned before and after the drought separately. The ATTs for 4-month based heterogeneity definition before and after the drought are compiled in Figure 10. There is a larger treatment effect prior to the drought, which could be due to non-price conservation crowding out the impact of prices during the drought period (Casey J. Wichman, Taylor, and Haefen 2016). As already evident from the Figure 5, the average consumption across household already dropped and there would be limited consumption to increase water use post drought. For example, the major household level consumption normally happens for watering the lawn. The non-price conservation heavily focused on cutting down the lawn use during the drought. The consumers although seeing their water bill going down would mainly adjust their water consumption indoors which is less elastic and have limited room to drastically increase the consumption.

Although we see treatment effects aggregated over different treatment groups, we examine how the GATE changes across different groups. Figure 19 plots GATE for each group with at least 0.5 percent of total records. The idea here is to understand how the treatment effects and heterogeneous groups vary over time. There are more structural winners after drought declaration since Jan 2014. Alternatively, higher consumers are mostly concentrated in early treated groups. Since 2014, there have been many drought-related non-monetary water conservation programs, which may have already pushed the optimal usage. Restricting our sample to pre-drought can help with better identification of treatment effects without confounding drought-related non-monetary water conservation efforts. However, it will also leave us with a limited sample for structural winners and low consumption households.

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<sup>7</sup>The governor of California declared a state of emergency due to the drought on January 17, 2014, which makes this month reasonable starting period for the post-drought period. See the documentation at <https://ca.water.usgs.gov/california-drought/california-drought-comparisons.html>.

**Figure 10:** Drought and change in ATT (last 4 month predicted bill difference)



*Note: In 2014, California's governor declared a drought emergency, prompting water conservation efforts that were independent of the pricing policy. We estimate treatment effects before and after the drought for winners and losers, defined using the last 4 months of bill differences.*

## 6 Demand Response to Total Bill

The empirical estimates indicate that consumers do not respond to marginal prices and their behavioral response is more consistent with responding to average prices or total bills. Ito and Zhang(2025) use a similar design for energy use in China conclude that consumers are responding to average price as suggested by Liebman and Zeckhauser (2004). They apply a non-parametric estimation method to test for schmeduling behavior. Their approach estimates treatment effects for deciles based on the change in average price. We follow a similar strategy by dividing the consumers into deciles based on two expected changes in water rates: the change in total bills (Figure 11). They show the point estimates and confidence intervals for each subgroup along with the change in total bills. The pattern, particularly for the lowest decile better fits the change in total bills. The highest decile consumers do not respond to the change in the rate structure: their consumption does not change significantly even when the total bill increases by approximately 65%.<sup>8</sup>

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<sup>8</sup>We also check the demand response to other definitions of heterogeneity for overall, pre-drought, and post-drought samples in the Figure 21,22, and 23 respectively.

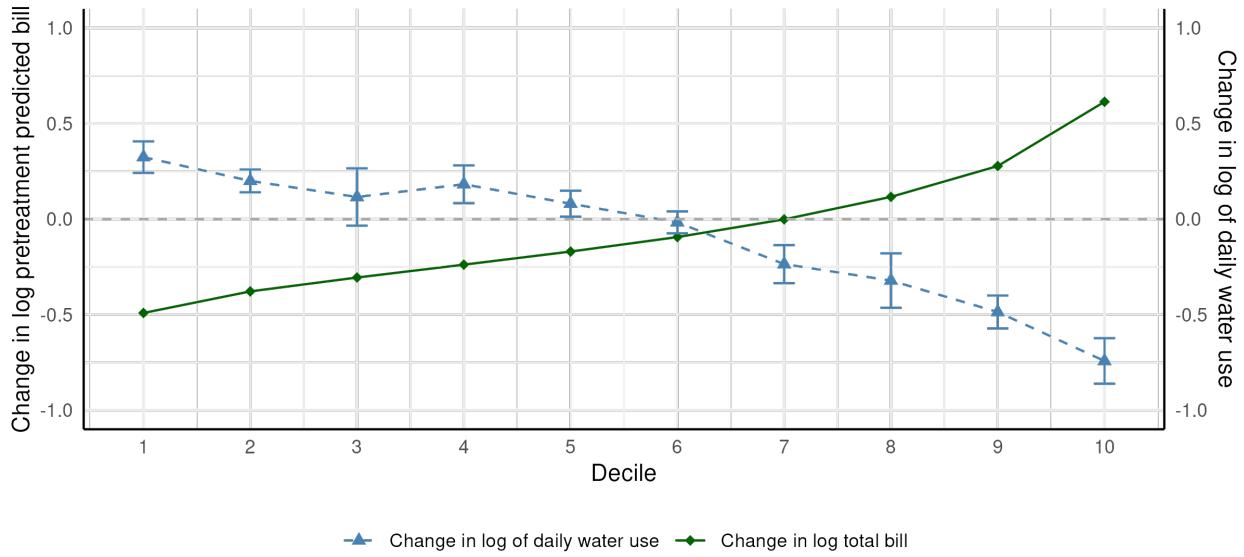
However, the relationship does not suggest a clear distinction of total bill over average price and it remains difficult to disentangle the specific price to which consumers are responding. Although the empirical evidence is not decisive, it is helpful to consider what is likely the most salient information for consumption decisions. Not responding to marginal price is consistent with models of rational inattention (Sallee 2014). In these models, the difference in the behavioral response between costly and cheap information does not warrant the investment in acquiring costly information. In our setting, the cheapest and most salient information is the total bill. This information is written directly on the bill and is what they will see on their credit card or bank account. Calculating the average price takes significantly more effort and is not an intuitive metric for most consumers. Daniel A. Brent and Ward (2019) show that consumers do not have good knowledge of marginal or average prices, but do know their total bills. This is also consistent with a model of mental accounting where consumers do not pay much attention to their water bill unless it goes beyond some defined interval (Thaler 1985).

A key element of responding to either average price<sup>9</sup> or total bill include the fixed price. Consumers would include the fixed price in their response accounting rather than neoclassical assumption of responding to variable cost only.

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<sup>9</sup>We define average price as total bill divided by quantity as opposed to some research that uses average volumetric price interchangeably with average price (J. Cook and D. Brent 2021)

**Figure 11:** Volumetric pricing induced change in water consumption  
 TB | Year-month Prediction | 4 mths



Note: This figure estimates demand based on the response to total bill changes. Consumers are grouped into deciles based on changes in their log-transformed total bill ( $\log(\text{Total Bill}_{\text{predicted}}) - \log(\text{Total Bill}_{\text{actual}})$ ). The solid line indicates the total bill change for each decile. We estimate the average treatment effect on the treated (ATT) for log daily usage using outcome-matching for each decile. Error bars represent 95% confidence intervals with bootstrapped standard errors.

## 7 Conclusion

In this analysis, we estimate the impact of transitioning from fixed water rates that do not depend on consumption to volumetric rates in the City of Sacramento. The transition to volumetric water rates was enabled by a large-scale metering campaign, before which consumers did not know how much water they used. Moving to volumetric rates decreased water use by about on an average. However, the lowest users who are most likely to see their total bill go down increased water use even though their marginal price increased. This is consistent with consumers using heuristics to respond to complicated rate schedules Liebman and Zeckhauser (2004) instead of optimizing for the correct marginal incentives. These results may be considered a lower bound because we use water in a “tracking period” as the control group. During the tracking period, consumers are shown how much they will pay once they transition to volumetric rates, so some behavioral changes may occur before consumers start paying volumetric rates.

These results have implications for water rate design. First, changes in marginal incentives may have limited impacts on conservation without large changes in total bills, which is politically

challenging. A recent trend towards budget-based rates that individualize the thresholds in an increasing block rate based on household characteristics insulates consumers from dramatic bill changes because high marginal prices typically only affect a small proportion of water use (Wietelman, Casey J Wichman, and Daniel A Brent 2024). One way to interpret that low-users increase their water use is that consumers respond to changes in their total bill, including the fixed fee. If the consumers incorporate the fixed fee in their response, there is a challenge to make utility bills more progressive without affecting the efficiency properties. Higher fixed bills will cause marginal price to deviate from marginal cost and hence less efficient recovery of bills (Levinson and Silva 2022). If consumers respond to fixed costs, this strategy becomes more complicated since changes in the fixed cost will affect demand.

One positive element of consumers responding to fixed costs is revenue stability. Utilities often have high fixed costs but want to generate revenue with volumetric charges to encourage conservation. Our design does not allow us to estimate long-term effects. Still, if we assume the treatment effects persist, utilities can maintain revenue stability and conservation if consumers respond to total bills by collecting more revenue from fixed fees. However, if the goal of the utilities is to reduce household water demand, they should invest more time and effort in communicating bills to customers.

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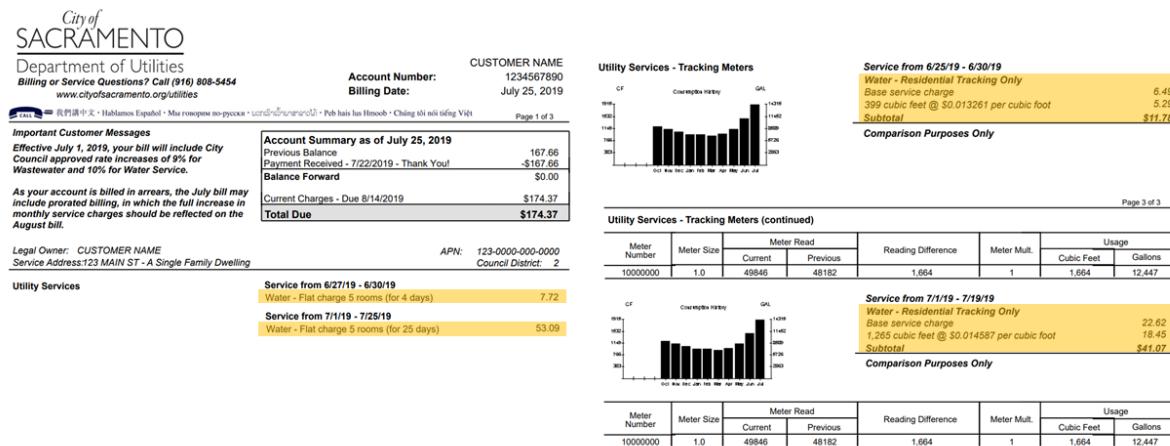
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## A Billing Information

**Figure 12:** Sample of utilities bill during tracking period for residents served by Sacramento Water Authority (SWA)

### Bill Template [Tracking Period]

**Actual Bill: \$ 7.72+53.09 = 60.81, Predicted Bill: \$ 11.78 + 41.07 = 52.85**



*Note: The bill provided information about the currently charged fixed bill and predicted two-part tariff based volumetric bill with same consumption. The bill represented here has a rate change happening from July 2019.*

**Figure 13:** Information pamphlet circulated by SWA

# YOU HAVE A NEW WATER METER - NOW WHAT?

## Front side

# HOW TO READ YOUR NEW WATER METER BILL

**City of Sacramento**  
Utility Services Bill

Account Number: [REDACTED]  
Billing Date: [REDACTED]

**Consumption History**

CF	GAL
1	1000
2	1200
3	800
4	900
5	700
6	1100

**Monthly Service Charge**

2,259 cubic feet @ \$0.0075 per cubic foot	\$ 16.94
<b>Subtotal</b>	<b>\$ 38.29</b>

Compared to the flat rate system, metered water bills are typically higher in the summer and lower in the winter. When reviewed over the course of the year, the majority of households on a metered rate find that metered bills are the same as – or less than – flat rate bills.

You will pay a monthly service fee on your new metered bill. The charge is based on the size of your water meter, as well as a charge for the amount of water you actually use.

	Current Meter Read	Previous Meter Read		Usage
[REDACTED]	[REDACTED]	[REDACTED]	[REDACTED]	[REDACTED]

**Keep in mind, your first metered bill will not be an accurate representation of your average monthly water usage.**  
When your meter is installed, it is set to "0." The Automated Meter Infrastructure (AMI) system that wirelessly sends your meter reading to the City's billing center is not brought online until at least six months after installation.  
So, your first comparative bill will show use for multiple months, but your second will show a more accurate monthly use.  
For billing/usage questions, contact **916-808-5454**.

## MOST WATER USE AND WASTE OCCURS OUTDOORS

We can make a big impact in the amount of water we use everyday by making simple changes.  
Take the following actions to see how much water you can spare each day.

Water your yard before 10 a.m. or after 6 p.m.  
Spare 20-25 gallons/day

Reduce each irrigation cycle by 1-3 minutes  
Spare 10-25 gallons/minute, up to 250 gallons/cycle

Use an auto shut-off nozzle on your hose  
Spare 8-18 gallons/minute

Repair leaks and broken sprinkler heads  
Spare 20 gallons/day per leak

Use a broom instead of water to clean driveways/sidewalks  
Spare 8-18 gallons/minute

Plant water friendly trees and plants  
Spare 30-60 gallons/day per 1000 square feet

Adjust sprinklers to prevent overspray and runoff  
Spare 15-25 gallons/day

Small water conservation efforts can add up to BIG savings!

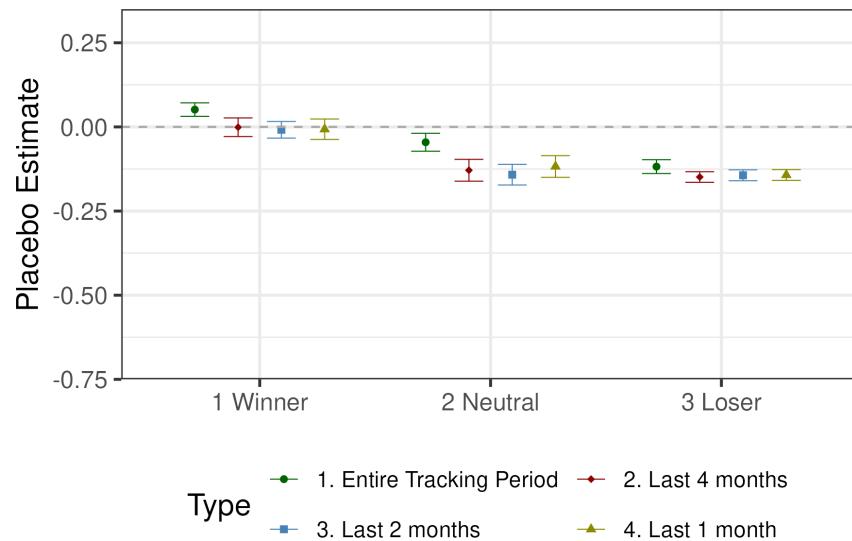
**400 gallons/day**

Back side

## B Placebo test

It is possible that the heterogeneous results are a product of mean reversion since low users in the pre-treatment period are more likely to be higher users in the post-treatment period. We believe our matching techniques alleviate this concern because we are comparing low users with other low users. We explicitly test for mean reversion using a placebo test. Here we drop all post-treatment data for two years which likely eliminates any dynamic treatment effects due to switching to two-part tariff. Then we randomly assign treatment cohorts for all households to mimic our original setup where households have 12 months of pre-treatment data. Focusing exclusively on the post-treatment period and eliminating the initial periods that likely exhibit dynamic treatment effects means that we are comparing treated households to other treated households. If our identification assumptions hold there will be a null placebo treatment effect. If our identification strategy suffers from mean revision we should see similar patterns of heterogeneity in the placebo estimates as we do in our actual estimates. The results, shown in Figure 14, show that the ATT estimates for winners are centered at zero and statistically insignificant whereas the estimates for neutral and loser groups are significant negative. The estimates for the losers are of much smaller in magnitude as compared to the actual estimates compiled in Figure 9. Since the drop in water consumption for the neutral and loser group is comparable to average drop for the neutral group, we can suspect that the groups are showing the natural tendency of decreasing consumption. This placebo test signals that the patterns of heterogeneity cannot be primarily due to mean reversion and the consumers are responding to the observed change in their total bill.

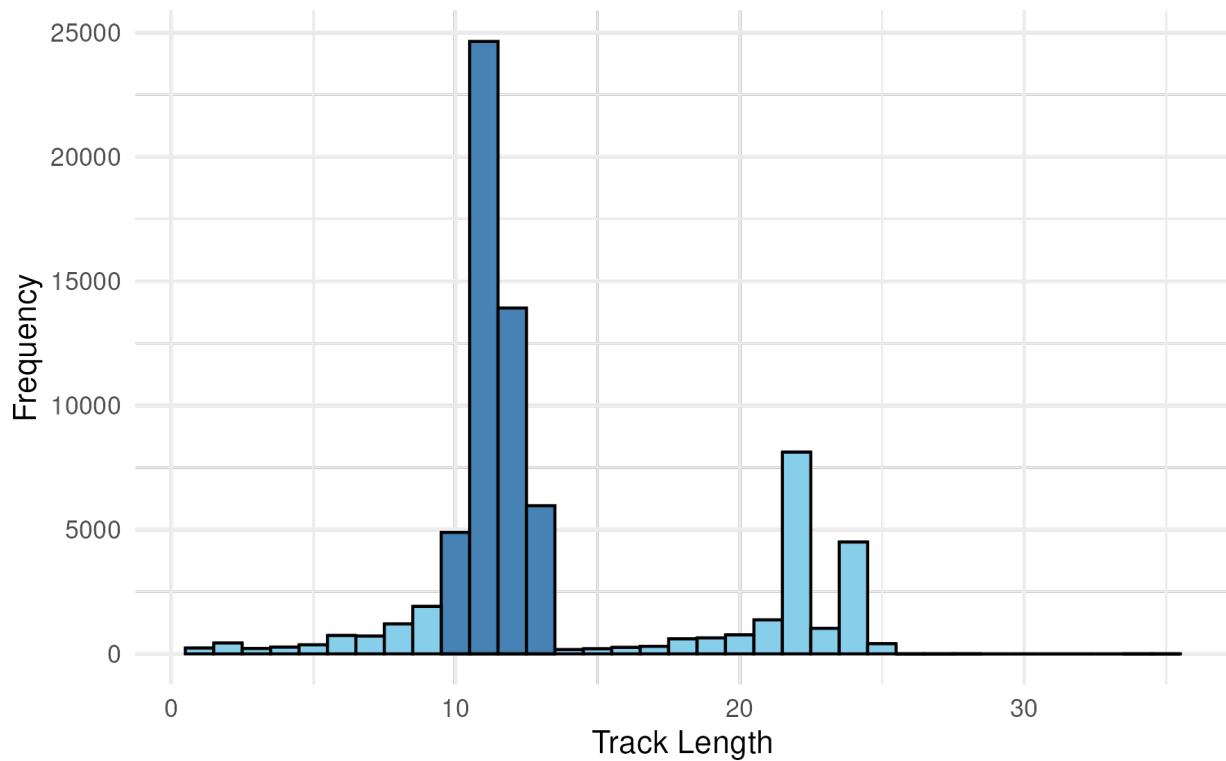
**Figure 14:** Placebo test using post-treatment data



Note: This figure shows placebo ATT estimates using matching on pre-treatment water use. The sample is limited to post-treatment data at least 2 years after transition to reduce treatment dynamics. Households are randomly assigned to placebo treatment cohorts with mimicked tracking and two-part tariff periods, replicating the estimation strategy used in Figure 7. As expected, null effects are observed.

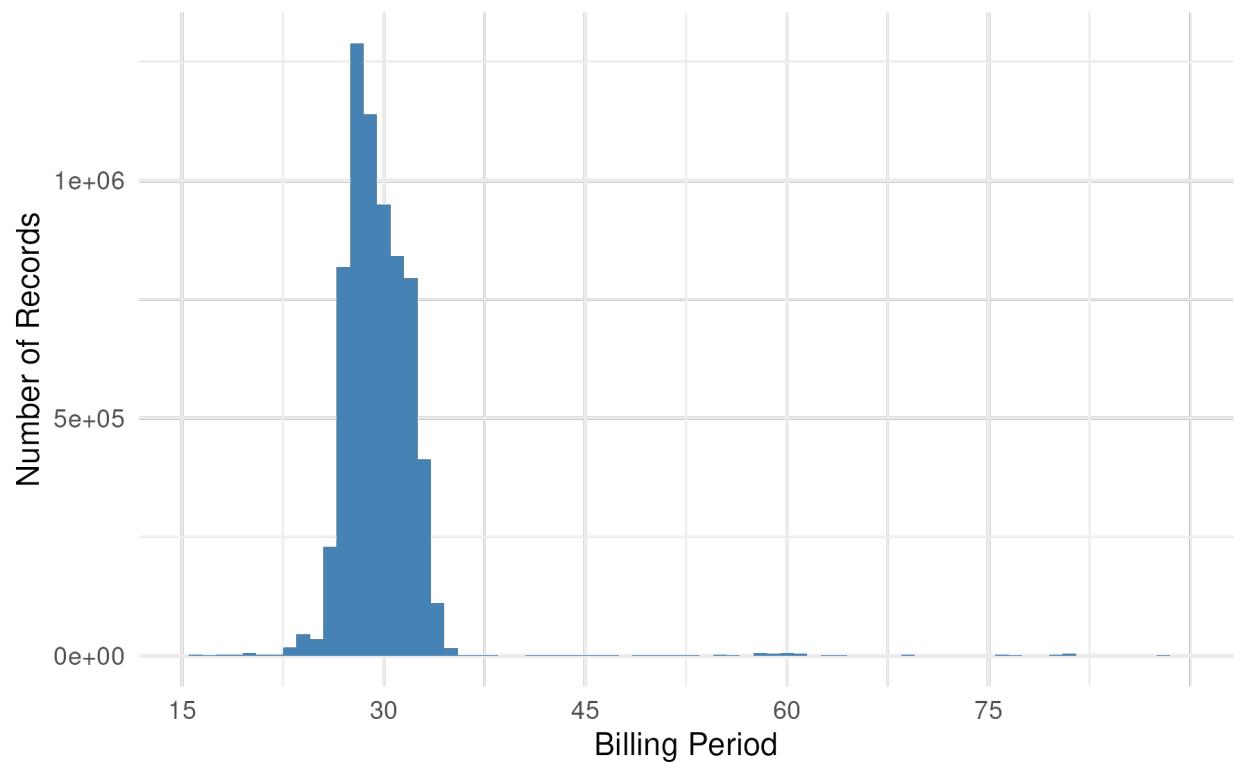
## C Additional Analysis & Figures

**Figure 15:** Number of tracking period bills



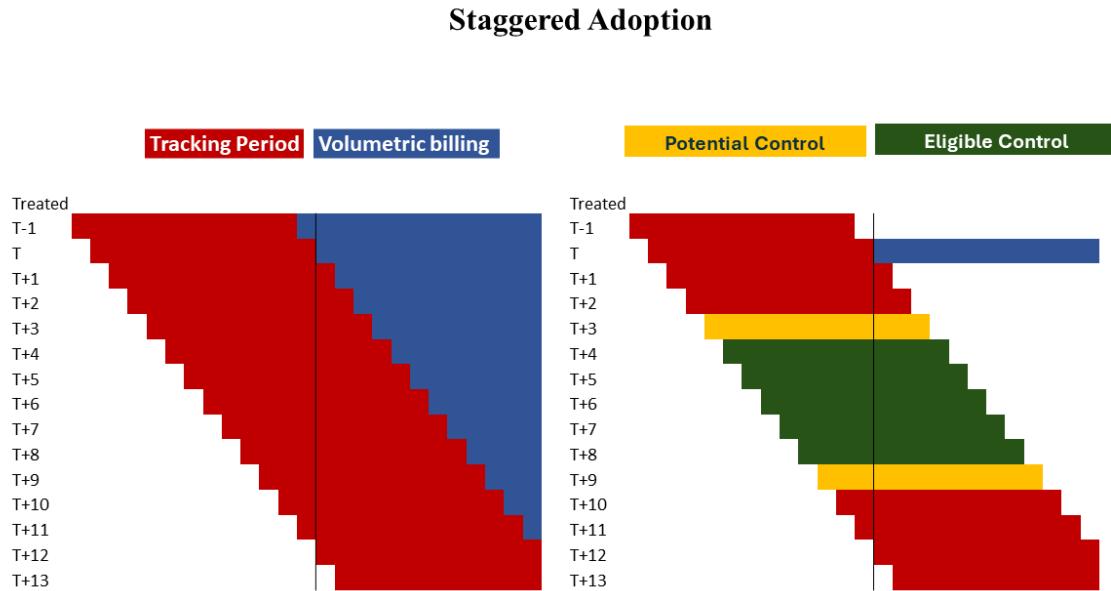
*Note: Consumers typically had 12 months of tracking period. Most received 10–13 bills during that time, highlighted with darker shade bars. Some had over 14 months of tracking, having been metered before Jan 2008 but not billed volumetrically until Jan 2010.*

**Figure 16:** Number of days in a billing period



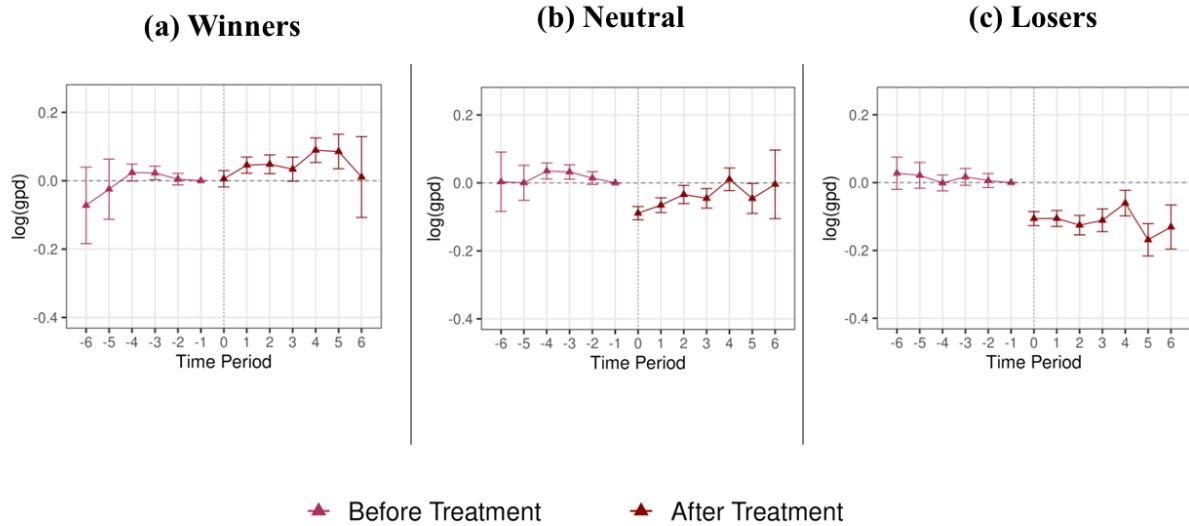
*Note: Most billing periods are 27–32 days. We drop records <16 or >89 days from the figure and analysis due to being unrepresentative. These account for just over 1% of all records.*

**Figure 17: Staggered adoption**



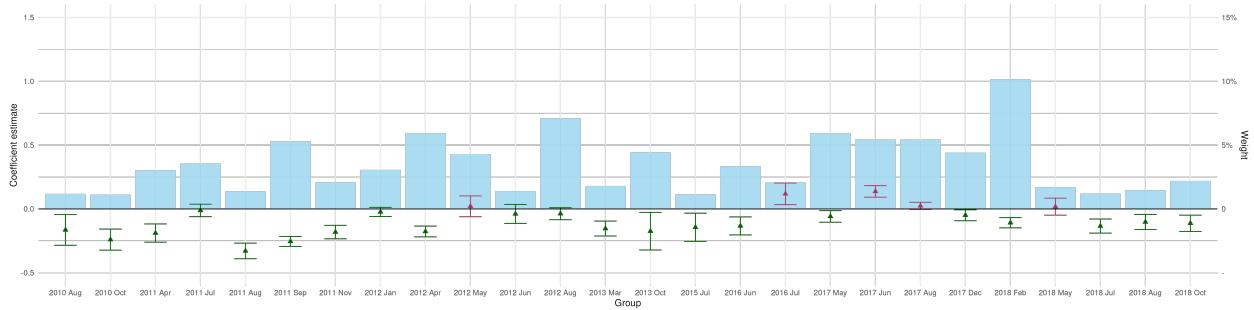
*Note: The figure illustrates the nature of our set up. It is staggered adoption with imbalanced panel. We observe only 12 months pre-treatment data. So, if a cohort is treated in time  $T$ , Only groups having any pretreatment as well as posttreatment overlapping with the cohort  $T$  are cohorts  $T+1$  to  $T+11$ . For  $T+12$ , there will be no overlap for pre-treatment observation and for  $T-1$ , there will be no post-treatment data overlap. Also, water consumption follows seasonality, water consumption changes based on the month of the year. To balance out the seasonality, we need sufficient control group spanning as many calendar months as possible. The ideal group for control shall be  $T+6$ . However, the staggered adoption does not follow uniformity across the months, there is strong possibility that there were no or very few households treated in  $T+6$  cohort. Thus, we relax the 6 month overlap condition to 4-month overall condition. This will allow use to find eligible control groups from treatment cohorts from  $T+4$  to  $T+8$  (Indicated in green). 3-month overlap (indicated in yellow) might be too short, and we will not be able to find good match for the pre-treatment outcome-based matching to be used for ATT estimation.*

**Figure 18:** Event study plots | bill difference heterogeneity



Note: The event study plots for the outcome matching-based ATT for the groups defined based on the bill difference (structural winners vs losers). The plots suggests that the parallel trend holds even beyond the matched observations (i.e.  $t-5$  and  $t-6$ ). Thus, the matching on outcome-based estimator holds critical assumptions for the difference-in-difference set up and we use it for our demand estimation.

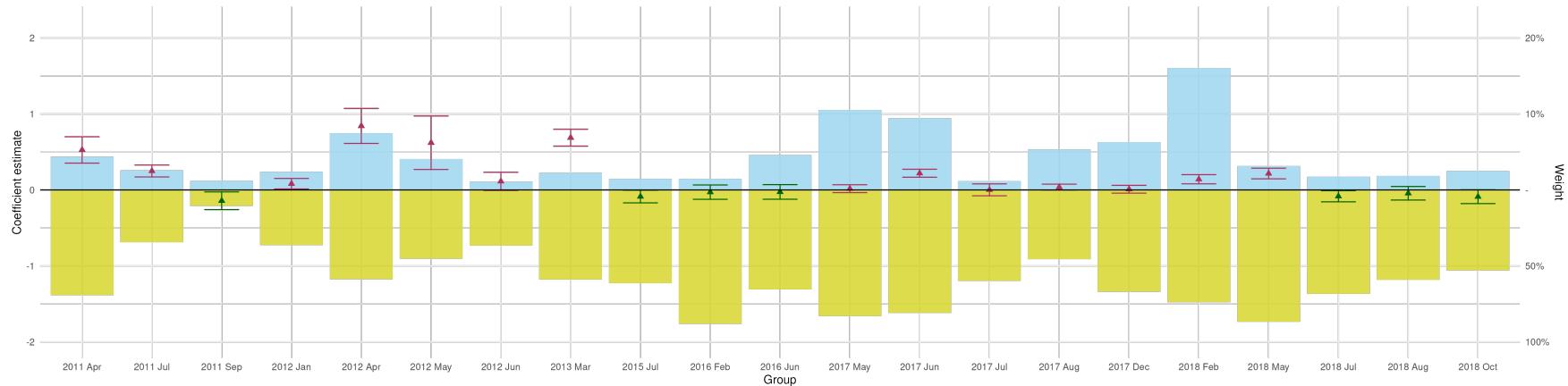
**Figure 19:** Cohort-level treatment estimates



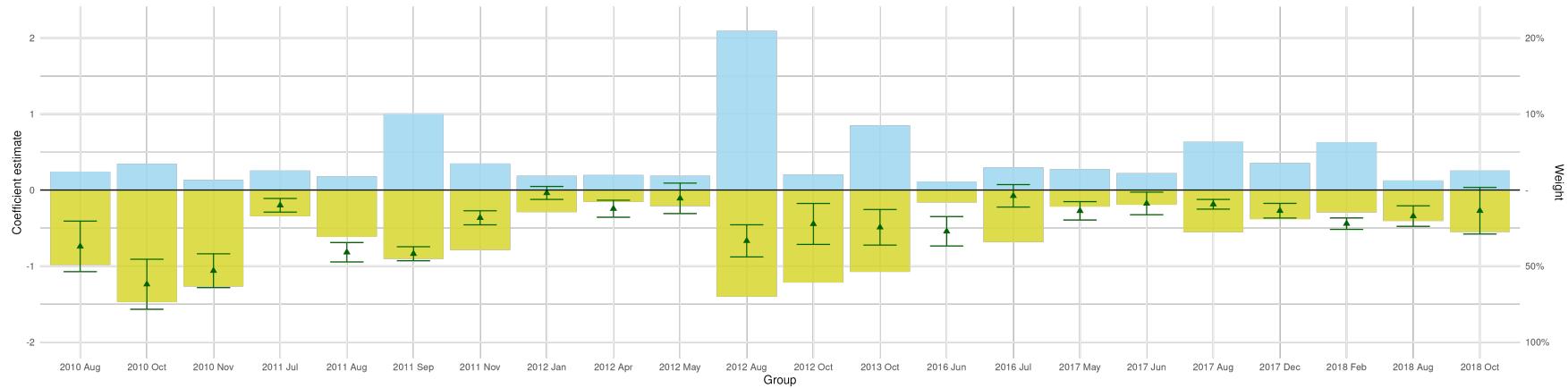
Note: This figure shows group-wise heterogeneity in ATT for groups with at least 1% weight. Green markers show negative estimates; maroon markers show positive estimates.

**Figure 20:** Cohort-level treatment estimates for winners and losers

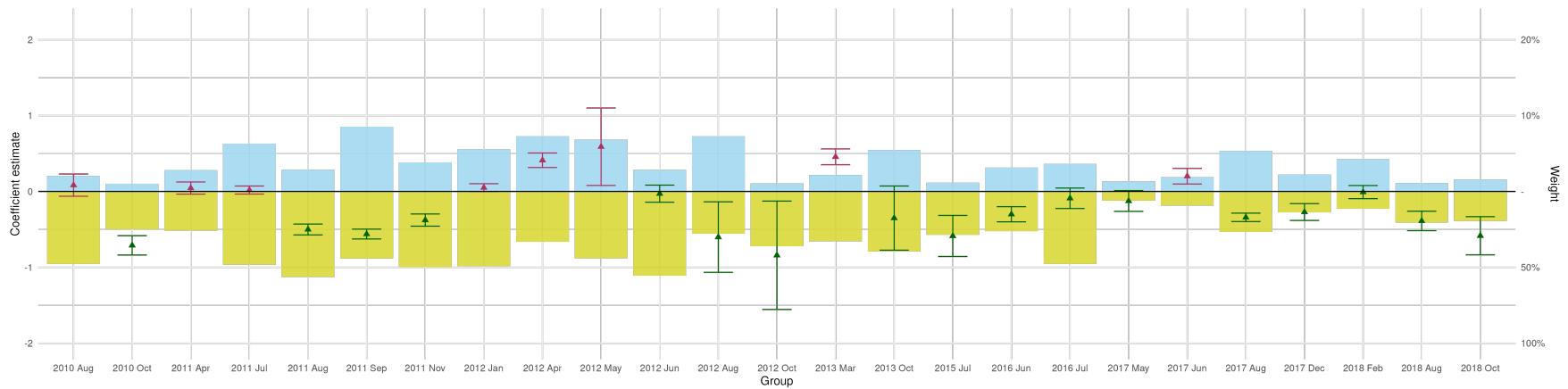
(a) Group-wise heterogeneity estimates for "Winners" based on last 4 months



(b) Group-wise heterogeneity estimates for "Losers" based on last 4 months



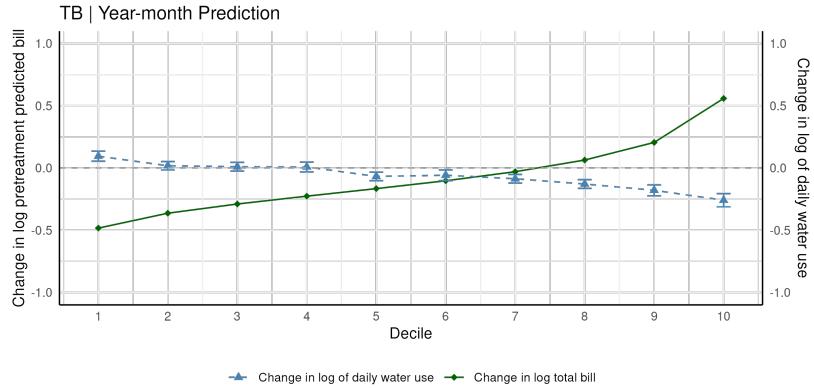
(c) Group-wise heterogeneity estimates for "Neutral" based on last 4 months



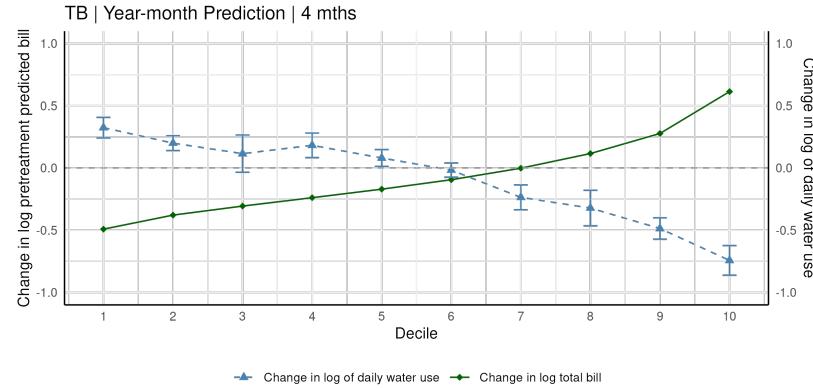
Note: This figure shows the group-wise heterogeneity in treatment effects for treatment groups having at least 1% weight in the overall ATT. Blue bars indicate relative weight of each group in the ATT (indicating the percent treated in the cohort as compared to total treated), and yellow bars indicate the category percentage within the same cohort (e.g., percent of winner households in the respective cohort). Pointers and error bars in green indicate negative estimates and in maroon indicate positive estimates.

**Figure 21:** Demand response curves (overall data)

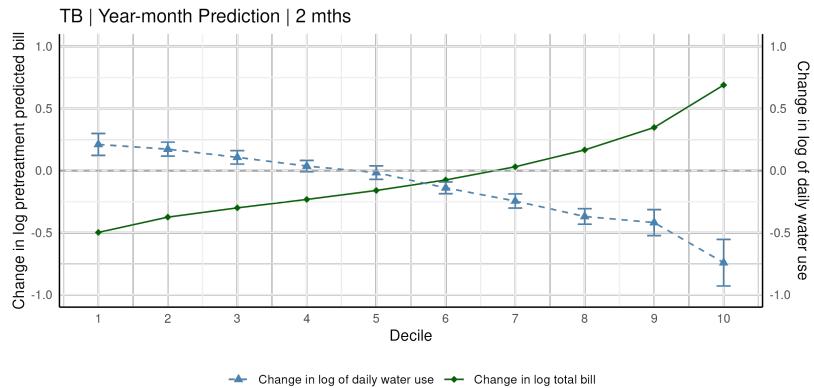
(a) Average bill difference in entire tracking period



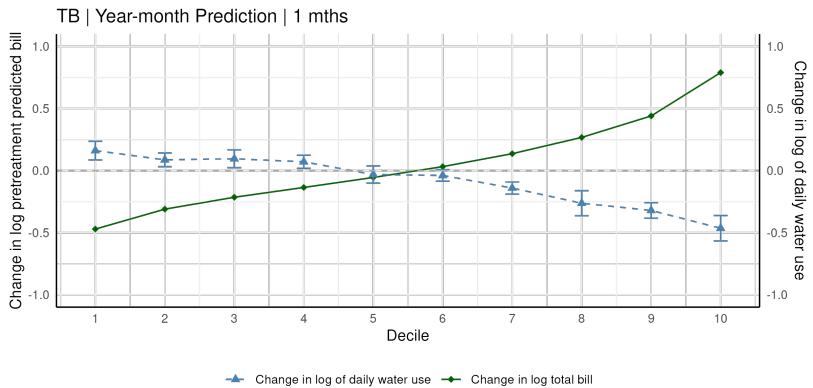
(b) Last 4 months



(c) Last 2 months

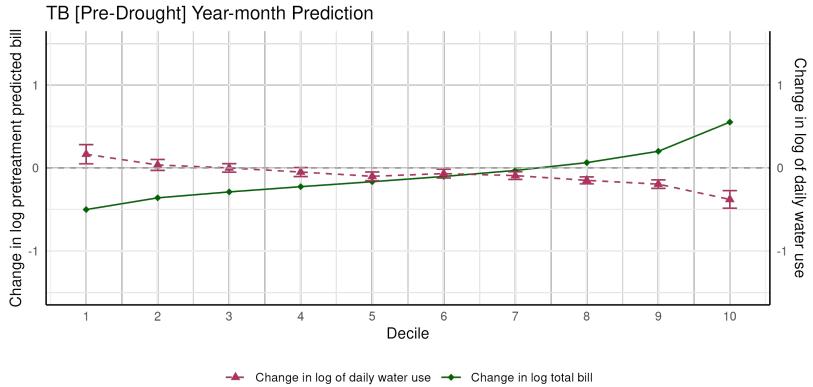


(d) Last month

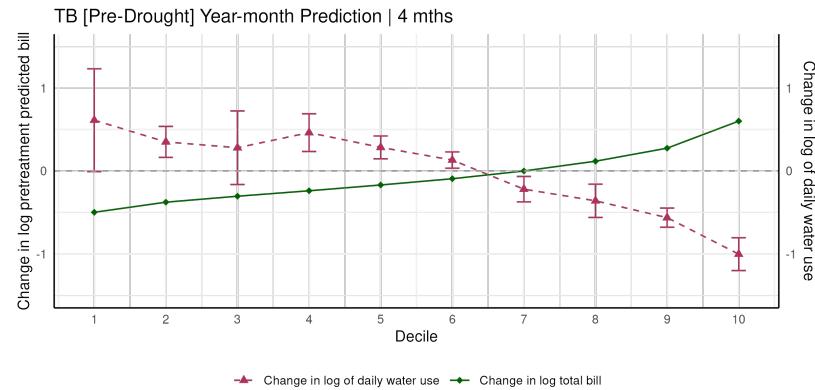


**Figure 22:** Demand response curves (pre-drought)

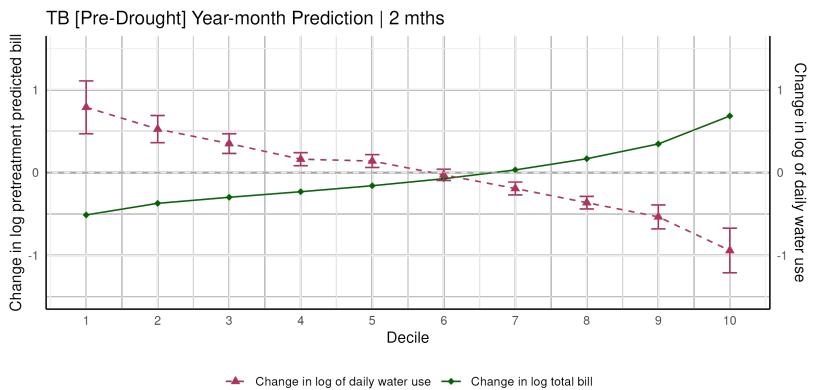
(a) Average bill difference in entire tracking period



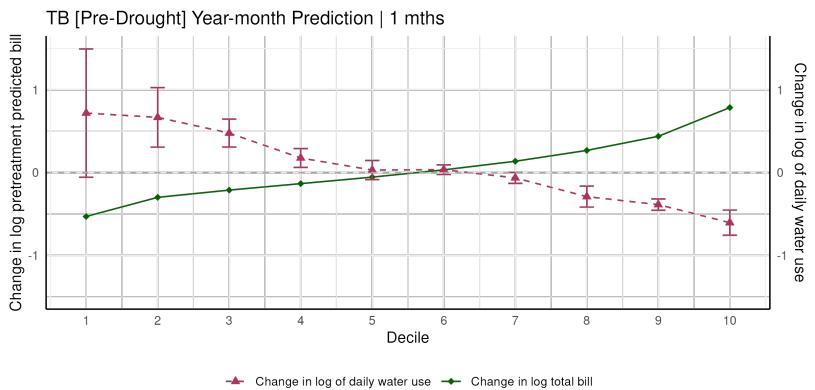
(b) Last 4 months



(c) Last 2 months

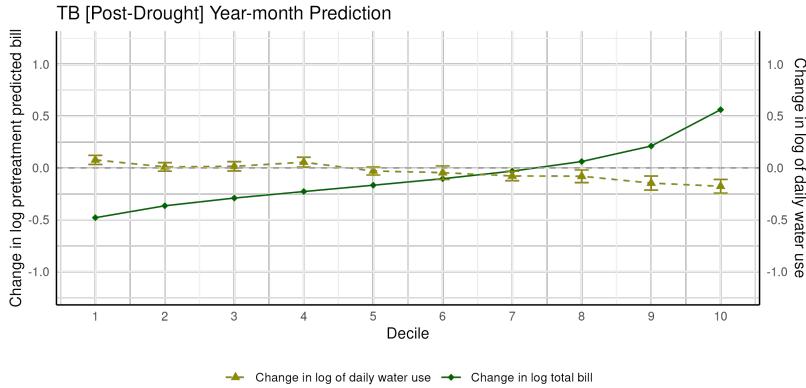


(d) Last month

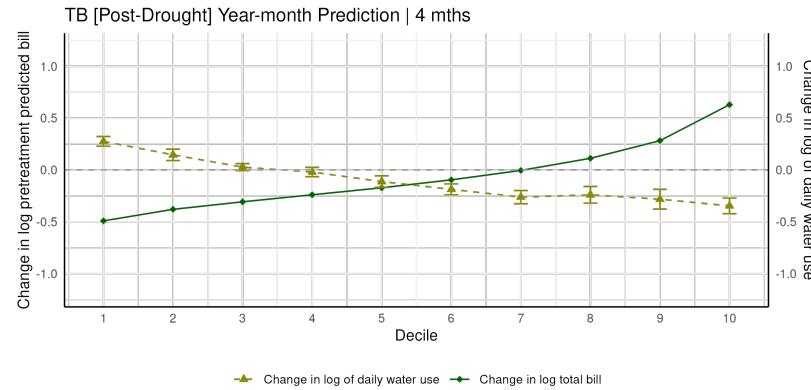


**Figure 23:** Demand response curves (post-drought)

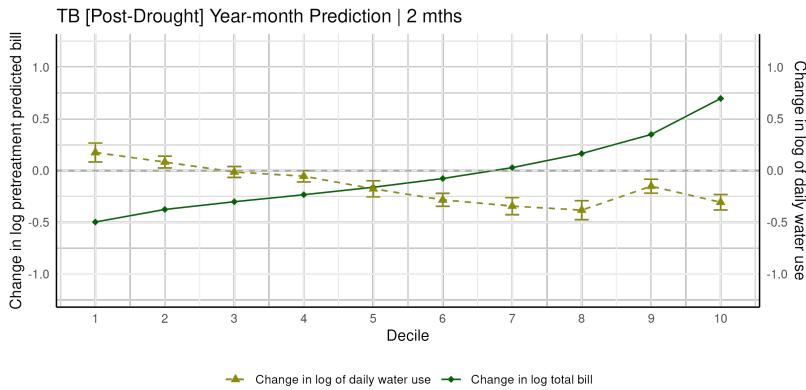
(a) Average bill difference in entire tracking period



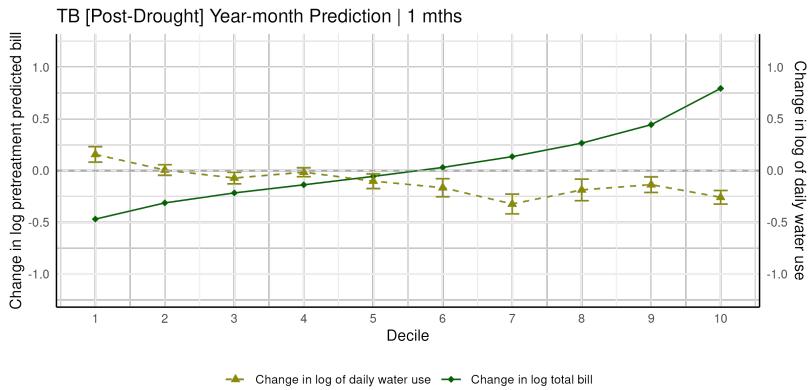
(b) Last 4 months



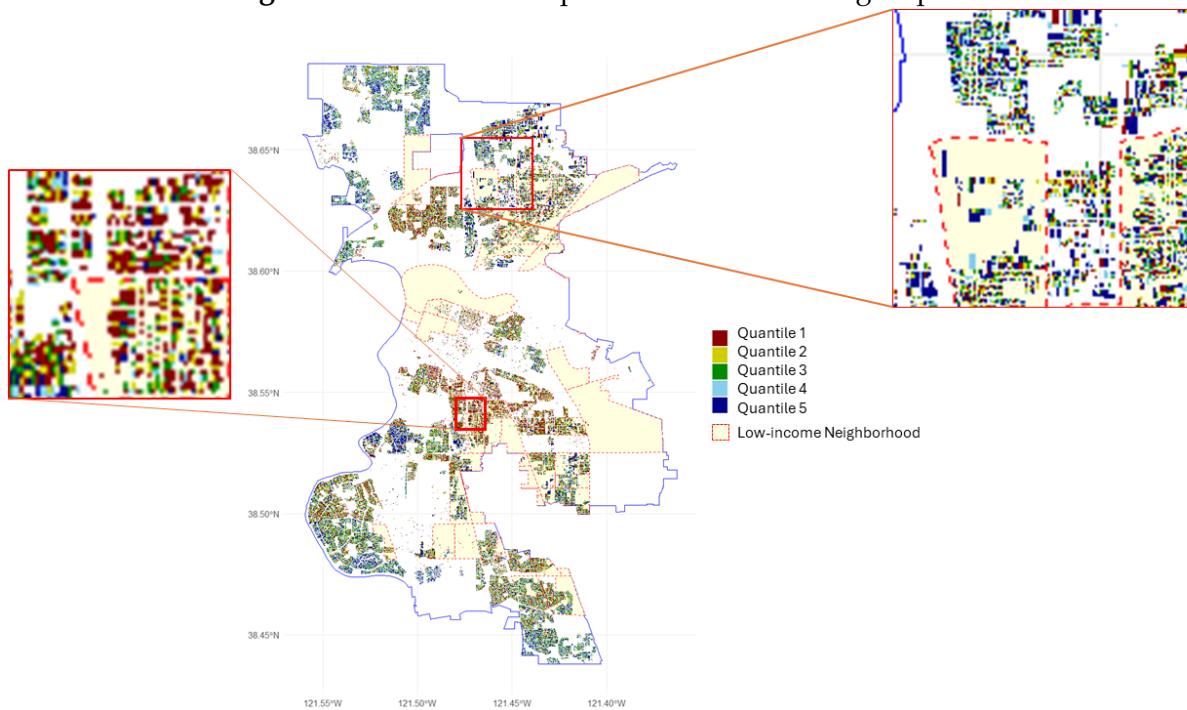
(c) Last 2 months



(d) Last month



**Figure 24:** Water consumption vis-à-vis income groups



*Note: Consumption quintile vis-à-vis layer of low-income (<\$50,000) neighborhoods.*