Conservation and Distributional Consequences of Pricing Scarce Water During Droughts*

Derek C. Wietelman† Casey J. Wichman‡ Daniel A. Brent§

November 9, 2025

JOB MARKET PAPER

[**CLICK HERE FOR MOST RECENT VERSION**]

Abstract

Using price incentives to allocate scarce resources is a core tenet of economics but may result in unpalatable distributional outcomes. We analyze the efficacy of prices as a means of inducing water conservation during severe drought by studying the introduction of surcharges enacted within existing nonlinear rate structures. We embed machine learning counterfactual prediction methods within a demand framework to isolate exogenous price variation, finding that households exhibit a significant demand response despite the temporary nature of surcharges. However, surcharges alone cannot explain a majority of the conservation observed despite steep price increases. "Budget-based" rates undercut scarcity signals by shielding large users from binding price increases, and surcharges do little to reduce the regressivity of water rates. Simpler rate structures can achieve greater efficiency and equity, especially if enhanced by progressive lump-sum transfers.

JEL codes: D12, H42, L95, Q25
Key Words: Conservation policy; drought management; nonlinear pricing; water demand

*We thank the California Data Collaborative (CaDC) and the two anonymous member utilities for facilitating access to the billing data. We are grateful for multiple conversations with Christopher Tull at CaDC, as well as multiple other staff members at CaDC and the two utilities, who helped us to interpret the data and understand the context surrounding drought policies enacted during our study period. We thank Anna Alberini, Saif Ali, Fiona Burlig, David Newburn, Bryan Pratt, Louis Preonas, and seminar participants at the 2019 Urban Water Demand Roundtable, Arizona State University, ETH Zurich, the 2019 AERE Summer Conference, the 2020 Seminar in Water Economics Online (SWELL) Series, the 2022 WEAI Annual Conference, University of Maryland, and the Inter-American Development Bank for helpful comments. This work is/was supported by the USDA National Institute of Food and Agriculture and Hatch Appropriations under Project #PEN04951 and Accession #7006541. Any remaining errors are our own.

†Wietelman: University of Maryland. Email: dcwietel@umd.edu

‡Wichman: Georgia Institute of Technology & Resources for the Future. Email: wichman@gatech.edu

§Brent: Pennsylvania State University. Email: dab320@psu.edu

1 Introduction

Severe droughts linked to climate change threaten water supplies globally. Water scarcity is particularly acute in the western United States, a region experiencing its driest conditions since at least 800 C.E. (Williams et al., 2022). Extended drought places tremendous pressure on urban water districts, especially those without their own surface or groundwater rights that rely on purchased water deliveries. In the face of shrinking and uncertain supply, water managers in these arid regions must find effective ways to curb residential water demand or face costly welfare losses from urban water rationing (Buck et al., 2016).

Economists often advocate for raising the price of water to reflect its scarcity value. However, multiple factors inhibit water prices from adjusting to real-time supply conditions. First, most residential water in the United States is supplied by municipal utilities that require a formal ratemaking process to change prices, which hinders dynamic adjustments to supply shocks during droughts (Hanemann, 1997). Additionally, the recognition of water access as a human right coupled with political pressure means that prices often fall below the long-run marginal cost of supply (Renzetti, 1992, 1999; Timmins, 2002a,b). Systematic underpricing fails to adequately signal the scarcity value of additional water consumption (Olmstead, 2010).[1] Despite this, attempts to increase prices raise serious equity concerns given that at least 10% of US households face water affordability issues (Cardoso and Wichman, 2022).

Given the tension between sending appropriate scarcity signals and maintaining affordability, how can residential water rates jointly address conservation and equity objectives during times of severe drought? We address this question by studying the introduction of two drought surcharge pricing programs during the 2011–2017 drought period in California, which included some of its driest years in recorded history (Mount et al., 2023). We study two California water utilities that implemented temporary surcharges. These surcharges were embedded within existing nonlinear rate structures, and target excess water consumption relative to customer-specific baselines.

Using a panel of monthly bills for approximately 37,000 households, we estimate a series of demand models to characterize how households responded to surcharges. One identification challenge is that these surcharges coincided with several nonprice programs designed to promote water conservation. To address this challenge, we use machine learning algorithms to predict counterfactual consumption during the drought surcharge period. We build on the framework of Burlig et al. (2020) and Prest et al. (2023) by using random forests to generate counterfactual demand predictions during the surcharge period absent any policy changes. We use these counterfactual predictions to construct an instrument for price changes in our demand models: the difference in predicted household-specific prices with and without surcharges. This isolates the exogenous policy-induced price variation needed to causally estimate price elasticities (Ito, 2014; Sears, 2021; Ito and Zhang, 2023).

[1] Municipal utilities are also further constrained by revenue recovery requirements, which limit utilities to recovering revenues adequate to cover their costs of supplying water. This is especially true in California due to Proposition 218, which limits the types of fees that local governments (including water utilities) can assess.

We estimate short-run price elasticities between -0.2 and -1.1, indicating that residential water demand, while largely inelastic, does respond meaningfully to drought surcharges (Dalhuisen et al., 2003; Sebri, 2014). Our findings are notable for two reasons. First, we identify relatively large short-run elasticities despite the expectation that long-run responses are typically more elastic as households have more margins for conservation (Espey et al., 1997). Second, our setting isolates exogenous variation in *temporary* drought surcharges rather than the permanent rate changes commonly studied. The fact that such temporary price increases generate elasticities comparable to or exceeding those in the broader literature highlights the effectiveness of surcharges in inducing immediate conservation.

Our elasticity estimates are identified conditional on other nonprice drought policies, such as information nudges and outdoor watering restrictions (e.g., Jessoe et al., 2021; West et al., 2021). Leveraging these estimates, we take additional steps that allow us to separately quantify the contribution of price and nonprice measures to overall conservation, an exercise rarely possible in this literature. Specifically, we combine our causal elasticity estimates with observed price changes to estimate water conservation that directly driven by surcharges. Comparing these back-of-the-envelope calculations to total conservation outcomes allows us to infer the share of drought response that is directly attributable to the price incentives. Under reasonable assumptions, we find that surcharges account for roughly one-fifth of aggregate conservation, implying that most short-run reductions stem from complementary nonprice interventions. These results have clear implications for policymakers–while surcharges are effective at driving short-run conservation, using prices alone will likely not be able to achieve the aggregate level of conservation needed in emergency drought situations.

We additionally extend our demand analysis to account for heterogeneity across household consumption classes. We document significant heterogeneity in elasticities, finding that larger users have the most inelastic demand. This result is in part due to the unique nature of the nonlinear rate structures employed here, which assigns individualized water budgets to households and define prices by comparing a household's monthly consumption to where it lands relative to that budget. The use of these "budget-based" rates (hereafter BBRs) potentially shields such larger users from facing higher prices by assigning them larger budgets, delaying the point at which higher marginal prices bind. These results have important implications for urban water managers. While higher prices are able to induce a significant demand response, the scarcity signal of surcharges is muted if price changes do not bind for enough households.

Heterogeneity in the price response raises important questions about how price increases affect households across the income distribution. These concerns are heightened in our setting, where each household's baseline allocation determines their individualized thresholds for nonlinear price increases, creating a unique link between consumption behavior and the ultimate marginal price they face. To examine these distributional implications, we extend our demand framework to incorporate income heterogeneity and find that the lowest-income households are the most price-responsive. We then assess whether drought surcharges altered the redistributive properties of BBRs. Using Lorenz curves of water expenditures and income in the spirit of Levinson and Silva (2022), we find that surcharges under BBRs induce little change in overall regressivity. While surcharges are not specifically designed with equity as the primary objective, significant distributional impacts could occur if wealthier, higher-use households face significant price increases relative to other household classes. Our results imply that layering the additional complexity of surcharges on top of existing BBRs does little to improve regressivity, given that prices may not bind for some high-use, high-income households.

We conclude by using our elasticity estimates to simulate how alternative rate structures distribute the burden of revenue generation across the income distribution, highlighting the trade-off between equity and efficiency. We find that uniform (flat) rates perform similarly or slightly worse than existing BBRs in terms of equity, but coupling a uniform rate with a variable fixed charge tied to income largely addresses equity concerns. Additionally, BBRs are more regressive than comparable increasing block rates (IBRs), suggesting that adding complexity to nonlinear rate structures tends to introduce equity concerns without clearly improving economic efficiency. Given that consumers often misunderstand complex nonlinear prices (Ito, 2014; Wichman, 2014; Brent and Ward, 2019; Shaffer, 2020), utilities may prefer simpler rate structures unless the added complexity yields clear welfare improvements. Nevertheless, water budgets combined with surcharges retain the ability to signal what the utility views as "wasteful" consumption, a potentially valuable nonprice conservation tool whose effectiveness warrants further study.

Our findings contribute to several distinct literatures. First, we introduce new evidence to a rich literature on the demand for residential water dating back several decades (e.g., Gottlieb, 1963; Howe and Linaweaver Jr, 1967; Young, 1973). As climate change has fueled worsening drought conditions, focus has shifted from characterizing baseline price elasticity estimates to studying the ability of both price and nonprice policies to serve as demand-management tools (e.g., Renwick and Archibald, 1998; Pint, 1999; Renwick and Green, 2000; Mansur and Olmstead, 2012). Our study contributes to the emerging literature studying the efficacy of various price and nonprice conservation policies implemented during the California drought of 2011–2017, including home water reports (Ferraro and Price, 2013; Brent et al., 2015, 2020; Jessoe et al., 2021; Brent and Wichman, 2022), public shaming and moral suasion (Sears, 2021; El-Khattabi, 2023), fees and other excess water use fines (Sears, 2021; Pratt, 2023), and automated irrigation enforcement (West et al., 2021; Browne et al., 2023). Our analysis is unique in that it focuses on identification of short-run price responses driven by *temporary* surcharges. We find more elastic demand than many other short-run studies. Additionally, since price and nonprice policies are nearly always employed simultaneously, there is relatively little well-identified evidence about which approach is more effective at inducing conservation (Browne et al., 2021). By decomposing the total conservation response for overlapping price and nonprice policies, our analysis highlights how price policies alone are unlikely to drive meaningful levels of conservation.

Second, we contribute to the literature analyzing the distributional impacts of environmental policy, specifically the ability of utility rates to serve as a redistributive policy instrument (e.g., Borenstein, 2012; Borenstein and Davis, 2012; Deryugina et al., 2019; Burger et al., 2020; Levinson and Silva, 2022). Many studies focus on energy prices, with relatively fewer papers studying the redistributive aspects of water prices despite their acute importance to low-income households (Randriamaro and Cook, 2024). Recent studies have begun addressing equity more directly, finding that seasonal rates induce higher conservation in wealthier, higher-use homes and that individualized rates may be either progressive or regressive under certain conditions (El-Khattabi et al., 2021; Smith, 2022). Our findings document that simpler uniform rates may outperform complex nonlinear rates in terms of income redistribution (Whittington and Nauges, 2020; Fuente et al., 2021). Our analysis also complements studies like Burger et al. (2020) and Borenstein et al. (2021) by illustrating the desirable equity benefits of income-based fixed fees. Understanding the equity implications of water prices will only become more important under extended drought (Cardoso and Wichman, 2022; Wichman, 2023).

Third, our analysis offers insight into pricing strategies as discussed in the broader literature on optimal rate-setting for natural monopolies (e.g., Hotelling, 1938; Coase, 1946; Brown and Sibley, 1986; Kahn, 1988). Since prices are tied to budgets, households are effectively charged individualized prices for the same quantity of water, violating standard allocative efficiency principles that recommend a uniform volumetric price set at long-run marginal cost within a two-part tariff (Coase, 1946; Levinson and Silva, 2022; Wichman, 2024). Given the multiple objectives that utilities juggle, (Bonbright, 1961), accepting some level of allocative inefficiency may be desirable if individualized budgets are particularly successful at achieving other priorities such as cost recovery or political feasibility. Economists have understood since Ramsey (1927) that charging different prices to different consumer segments based on their distinct elasticities can prioritize cost recovery while simultaneously minimizing welfare losses associated with price distortions. Our finding that over-budget households are the most price-inelastic implies that the rate structures used here may effectively act as "quasi-Ramsey" prices, as more of the cost-recovery burden will be placed on relatively inelastic households. While this may be desirable from an efficiency perspective, the degree to which nonlinear rates actually minimize welfare losses may be undercut if water budgets prevent surcharges from effectively signaling scarcity to many large users.

Finally, BBRs are rapidly being adopted in arid regions with the hope that they can effectively balance competing conservation, equity, and cost-recovery goals (Mayer et al., 2008; Allaire and Dinar, 2022). Yet, relatively few studies have evaluated their performance directly, especially with respect to distributional concerns. Baerenklau et al. (2014) estimate large demand reductions due to the introduction of BBRs in a Southern California utility. Two other studies also find that BBRs can induce conservation through information signals sent by individualized budgets (Baerenklau and Pérez-Urdiales, 2019; Pérez-Urdiales and Baerenklau, 2019).[2] Our results highlighting potential conservation and distributional concerns with BBRs stand in contrast to these existing studies and suggest a more nuanced view for policymakers considering their adoption.

[2]Other papers study individualized water rates that are similar to BBRs but with tiers defined using other metrics, such as average winter consumption or annual cumulative consumption (Smith, 2022; Li and Jeuland, 2023).

2 Background

2.1 Drought of 2011–2017

Our study focuses on the 2011–2017 drought period in California, which was one of the most severe droughts in the state's recorded history. California entered into an extended period of drier-than-average conditions in the latter half of 2011, and by mid–2012, large swaths of the state were experiencing at least ”moderate drought” (NOAA, 2023). In response to the extended drought conditions, Governor Brown declared a state of emergency on January 17, 2014. The order directed state resources toward water conservation campaigns and called on Californians to reduce water consumption by 20%.

With drought conditions growing increasingly more severe, on April 1, 2015, Governor Brown issued a second executive order that took the unprecedented step of mandating statewide water cuts from urban water suppliers. Specifically, the order directed the State Water Resources Control Board to impose restrictions that would achieve a 25% reduction in statewide urban water consumption relative to 2013 levels. The order also established a permanent water consumption and conservation reporting requirement for urban water suppliers. These mandatory cuts spurred utilities to adopt a variety of price and nonprice policy interventions and were in effect for a year, until their withdrawal in May 2016.

California entered into an especially wet period in water year 2017 (starting in October 2016). Specifically, January and February 2017 were the wettest months on record for some parts of the state, including the northern Sierra Nevada mountain range and the San Joaquin River basin (NOAA, 2017). These unusually high precipitation levels (including snowfall) helped replenish surface water levels at critical reservoirs, while at the same time inflicting significant economic damage due to flooding. The increased precipitation levels, combined with evidence that urban water suppliers were succeeding to some degree at inducing conservation, caused the lifting of the drought state of emergency on April 7, 2017.[3]

2.2 Utility Characteristics and Existing Rate Structures

We focus on efficacy of pricing policies of two water utilities in southern California as a drought management tool. Both utilities provide drinking water and sewer services to residential customers. Additionally, both utilities are heavily dependent on imported water sourced from the Sacramento-San Joaquin Bay-Delta (through the State Water Project) and the Colorado River (by way of the Colorado River Aqueduct), though one utility does hold limited groundwater rights. The first utility serves an area closer to the Pacific Coast with a relatively denser population, smaller lot sizes on average, and a relatively cooler climate. The second utility serves an area that

[3]Figure A.1 lays out a visual timeline of the events discussed in this section. More information on both the 2014 and 2015 executive orders can be found at `https://www.ca.gov/archive/gov39/2014/01/17/news18368/index.html` and `https://www.ca.gov/archive/gov39/2015/04/01/news18913/index.html`, respectively. Information on the 2017 lifting of the state of emergency can be found at: `https://www.ca.gov/archive/gov39/2017/04/07/news19748/index.html` (accessed July 26, 2024).

is further inland with a relatively less-dense population, larger lot sizes, and a relatively warmer climate. Hence, we refer hereafter to the first utility as the "Coastal Utility" and the second utility as the "Inland Utility". As part of a data-sharing agreement, we refrain from publicly identifying the two utilities here.

Both utilities price water through a BBR. BBRs are similar to traditional IBRs in that the volumetric price for consuming an additional unit of water rises as households consume higher quantities. The key difference between BBRs and their IBR counterparts is the assignment of individualized water budgets to each household that also vary month to month. These individualized budgets determine the price tiers a household faces, as opposed to IBRs, where the consumption tiers that define prices are common to all households. For example, in a simple IBR with a low and high price, all households will pay the high price for units above the common threshold k. In a BBR, each household i has their own threshold, k_i, that depends on household and weather characteristics and is determined by a water budget formula. Both utilities use roughly similar water budget formulas that define a two-part budget consisting of indoor and outdoor components. Together, the indoor and outdoor budgets define what the utility views as acceptable or non-"wasteful" water consumption for a household in a given month.

The calculation for household i's indoor water budget in billing period t is:

$$Indoor_{it} = Persons_i \times GPCD_t \times Days_{it} \times (1/748), \tag{1}$$

where *Persons* is the household size, *GPCD* is an allotment made by the utility for water usage in gallons per capita per day, *Days* is the number of days in the billing cycle, and (1/748) is a scaling factor to convert from gallons of water to hundred cubic feet (1 CCF = 748 gallons). Coastal Utility assumes a household size of four for single-family residential homes and three for condominiums, and Inland Utility assumes a household size of three for all residential customers.[4] Coastal allotted 65 GPCD through April 2015, and then 60 GPCD through the end of our study period. Inland used a value of 60 GPCD throughout the study period.

The calculation of household i's outdoor water budget in billing period t is:

$$Outdoor_{it} = Area_i \times ET_{it} \times PF_{it} \times (0.62/748), \tag{2}$$

where *Area* is the amount of irrigable area on the customer's property in square feet, *ET* is a measure of monthly evapotranspiration in inches, *PF* (Plant Factor) is a constant assumed by the utility about the types of vegetation present on a given property and the subsequent amount of water required, and (0.62/748) is a scaling factor to convert from inches to gallons/square foot, and then to CCF.[5] A household's total water budget is determined by adding the indoor

[4] It is the responsibility of the household to contact the utility to update household size away from the default, and verification is handled on a case-by-case basis. Widespread reliance on the default potentially limits one source of cross-sectional variation in indoor budgets. In Appendix D we demonstrate how these household size assumptions end up over-allocating water to many households by overestimating the number of persons in the home.

[5] Households can also request to update the amount of irrigable square footage used to calculate their outdoor

and outdoor water budgets (i.e., $Budget_{it} = Indoor_{it} + Outdoor_{it}$). Household size and irrigable area drive between-household variation in water budgets, while evapotranspiration drives both between-household variation in budgets (across the spatial landscape) and within-household variation (over the course of the year).

Figure 1 displays the nominal volumetric prices and consumption tiers over time within each utility's BBR structure. Water use within the customer's indoor budget is charged at the lowest volumetric prices. Consumption above the indoor water budget but still below the total budget is charged at the Tier 2 price, representing outdoor consumption. Any consumption above this is considered "over budget" and is charged at the relatively higher-tier prices. 125% and 150% of total budget are the relevant thresholds between Tiers 3 and 4 consumption and Tiers 4 and 5 consumption, respectively. Coastal uses five price tiers and kept rates constant through April 2015, when it lowered the marginal price in its highest tiers. Inland implemented several small increases over time. Inland also only used four price tiers for its first year of BBR implementation, adding a fifth tier in October 2012. Coastal has a higher peak-to-minimum marginal price ratio than Inland, with the highest marginal price being more than four to five times greater than the lowest marginal prices over the course of the study period.

2.3 Surcharge Pricing in Response to Drought

Following the April 2015 executive order mandating 25% water cuts statewide, both Coastal and Inland entered into elevated stages of their water shortage contingency plans (WSCPs). A crucial aspect of each utility's drought response strategy under its WSCP was the imposition of steep price increases on over-budget water users through the use of drought surcharges. Throughout the paper, we refer to these price changes implemented by the utilities as drought surcharges, as opposed to the more traditional idea of conservation prices. Under conservation pricing, rates are designed to permanently recover the lost revenue that results from selling less water over the long run. Drought surcharges are designed to be temporary in nature and are not intended to raise revenues through the highest tiers, only to cover the costs associated with purchasing higher-cost water supplies under drought.

Figure 1 illustrates the structure of these surcharges and how they were layered within the existing BBRs employed by the two utilities. Both utilities implemented such drought surcharges from the summer of 2015 until February 2017. Under their WSCP, Coastal suspended the Tier 3 and 4 prices, and assessed a \$7.43/CCF charge on all consumption over a household's water budget, the difference between the Tier 2 and Tier 5 price. For the first year under its WSCP, Inland similarly suspended Tier 3 and Tier 4 rates. Inland also reduced outdoor water budgets by 30% during this time. From June 2016 until the surcharges were lifted in February 2017, Inland restored outdoor water budgets and the Tier 3 price, and charged all consumption over 125% of budget at the Tier 5 level. Drought surcharges are represented in Figure 1 by removing

budget. Coastal assigns a constant value for plant factor across households and months (0.8 before April 2015 and 0.7 after), and Inland assigns varying plant factors depending on the month and service start date.

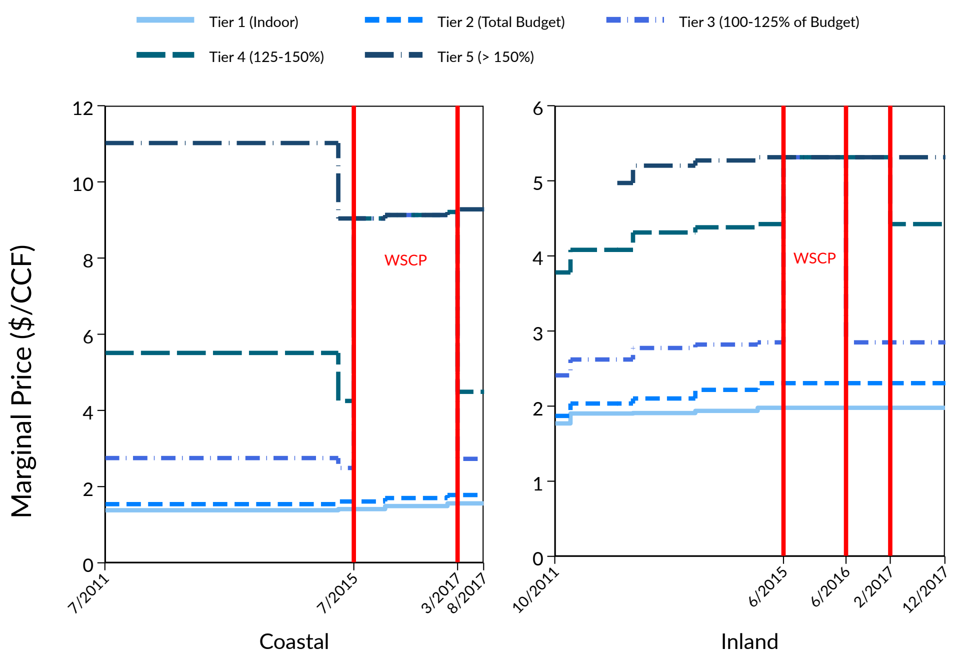


Figure 1: Rate Structures Over Time

Notes: The figure presents the nonlinear budget-based rate structures in place for each utility over the course of the study period. Marginal prices for each consmption tier are given in nominal U.S. dollars/centum cubic feet ($/CCF). The solid red vertical bars represent the period during which each utility's drought surcharges were in effect, invoked under water shortage contingency plans.

the intermediate tier prices when the WSCP was in effect, denoted by the red vertical bars. In practice, moving from 100% to 101% of a household's budget during the WSCP results in dramatic increase in marginal prices.

The drought surcharges implemented were intended to communicate scarcity and discourage excessive consumption by having households face the highest tier prices immediately after going over budget while still preserving low marginal prices for under-budget consumption. Both utilities implemented extensive campaigns to warn households about the impending drought surcharges and to encourage within-budget consumption. The surcharges were implemented in accordance with Proposition 218 restrictions that impact how utilities can price water under nonlinear rate structures.[6] In both utilities, any excess revenues generated from drought surcharges were directly recycled back into water efficiency programs.

[6]At least one municipal utility has been forced to alter its nonlinear rate structure in response to Proposition 218 concerns. See *Capistrano Taxpayers Association, Inc. v. City of San Juan Capistrano*, 236 Cal.App.4th 1123 (Cal. Ct. App. 2015). The case held that nonlinear water rates did not inherently violate Proposition 218, but that tier definitions must be informed by the actual cost of supplying water service in each tier.

3 Data

We use four primary data sources in our analysis: household-level monthly water billing data, demographic information from the US Census Bureau, property value and tax information from county assessor offices, and local weather data. We obtain our monthly water billing records from the California Data Collaborative (CaDC), a data nonprofit focused on water issues in California. For both utilities, we restrict the data to include only single-family residential customers for which we have a full or nearly-full panel (≥ 70 months) of billing records since the time when BBRs went into effect (July 2011 for Coastal, October 2011 for Inland). After further cleaning and imposition of filters, such as dropping extreme outliers, we are left with 1,989,521 customer-month observations for Coastal (27,006 unique households) and 789,741 observations for Inland (10,181 unique households). A complete description of the data cleaning steps is provided in Appendix B. We merge prices to these data using publicly available rate information.

Additionally, we obtained demographic information on the distribution of household size, race and income distributions at the census block group level from the US Census Bureau's American Community Survey (ACS) 2015 5-year estimates (US Census Bureau, 2015). We match households to census block groups using the block group information provided in the billing records. Next, we obtain tax assessor data for the two California counties in which the utilities are located. These records include the property values (land and improvement value) and other property characteristics, such as the total area of the lot, the number of bedrooms and bathrooms, and detailed property use code descriptions.

Finally, we obtain data on local weather beyond evapotranspiration (which was included in the billing data for the purpose of calculating water budgets). We use high-resolution weather data from the Parameter-elevation Relationships on Independent Slopes model (PRISM) and the panel of daily weather observations across our entire study period used in Schlenker and Roberts (2009) to construct average minimum and maximum temperature and average and total precipitation over the course of the billing period for each customer-month. We match our households to the 2.5-by-2.5-mile grids using household latitude and longitude provided in the billing records. We also use data from the California Irrigation Management Information System (CIMIS) for some records in Inland where evapotranspiration and/or outdoor water budget amounts were missing (CIMIS, 2018).

We present in Table 1 billing-record level summary statistics of key variables related to consumption, budgets, and property characteristics for the households in our data. Households in Inland tend to be larger homes, in terms of both of dwelling size (e.g., number of bedrooms) and lot size (e.g., irrigable square footage). Households in Inland have roughly triple the amount of irrigable square footage of homes in Coastal (on average). Recall that irrigable square footage is a direct component of the water budget calculation formula, and thus leads to higher average outdoor and total budgets in Inland. Average monthly water consumption for households in Inland is nearly double that of Coastal, which is also likely driven by the need for more outdoor water consumption due to larger lawns. Inland also experiences significantly greater evapotranspira-

Table 1: Summary Statistics

Coastal	Mean	Std. Dev.
Water consumption (CCF)	12.77	9.85
Indoor budget (CCF)	10.11	2.07
Outdoor budget (CCF)	7.29	8.09
Total budget (CCF)	17.40	8.88
Household size	3.96	0.73
Gallons/capita/day	63.05	2.44
Days in billing period	30.46	2.80
Irrigable square footage	2,717.04	2,745.27
Evapotranspiration (inches)	4.16	1.33
Max. Temp. (Deg. C)*	24.72	3.61
Total Precipitation (mm)*	17.36	29.19
No. of bedrooms*	2.53	1.59
Property value (1,000 USD)*	485.37	329.02
Median income (1,000 USD)*	114.18	33.59
Unique Accounts	27,006	
Total Billing Observations	1,989,521	

Inland	Mean	Std. Dev.
Water consumption (CCF)	24.57	20.83
Indoor budget (CCF)	9.40	3.61
Outdoor budget (CCF)	30.51	37.49
Total budget (CCF)	39.91	37.88
Household size	4.07	1.41
Gallons/capita/day	60.00	0.00
Days in billing period	30.39	3.58
Irrigable square footage	8,810.87	8,834.65
Evapotranspiration (inches)	5.25	1.92
Max. Temp. (Deg. C)*	27.14	5.75
Total Precipitation (mm)*	17.45	25.07
No. of bedrooms*	3.91	0.79
Property value (1,000 USD)*	388.19	144.85
Median income (1,000 USD)*	99.66	23.70
Unique Accounts	10,841	
Total Billing Observations	789,741	

Notes: The table presents billing record level summary statistics for each utility separately. Summary statistics are presented for the full period of billing records available: July 2011–August 2017 for Coastal and October 2011–December 2017 for Inland. All variables are sourced from the billing microdata except for those variables denoted with an asterisk (*), which are sourced from one of the supplemental data sources described in Section 3.

tion than Coastal, further driving larger water consumption needs. Finally, the Coastal service area is wealthier, as indicated by higher average property values.

4 Empirical Framework

We first study the efficacy of surcharges as a conservation tool by characterizing the demand response induced by the surcharges. Our primary parameter of interest is the short-run price elasticity of demand in response to the surcharges. Two primary challenges threaten the identification of causal elasticity estimates. First, utilities often implement many nonprice water conversation programs during droughts at the same time as price changes. During the drought of 2011-2017, utilities experimented with a suite of policies to curb demand, such as public information campaigns, rebates for installing turfgrass, water audits, etc. Simply observing changes in water consumption before and after implementation of drought surcharges cannot identify how much of any observed water conservation can be attributed to prices alone. The second econometric challenge is the well-known issue of simultaneity between prices and quantity that arises under nonlinear rates (Olmstead et al., 2007; Olmstead, 2009; Wichman et al., 2016). With BBRs (as well as traditional IBRs), marginal and average prices increase mechanically as consumption increases. Failing to account for this source of endogeneity will result in ordinary least squares (OLS) demand estimates that are biased and potentially upward sloping.

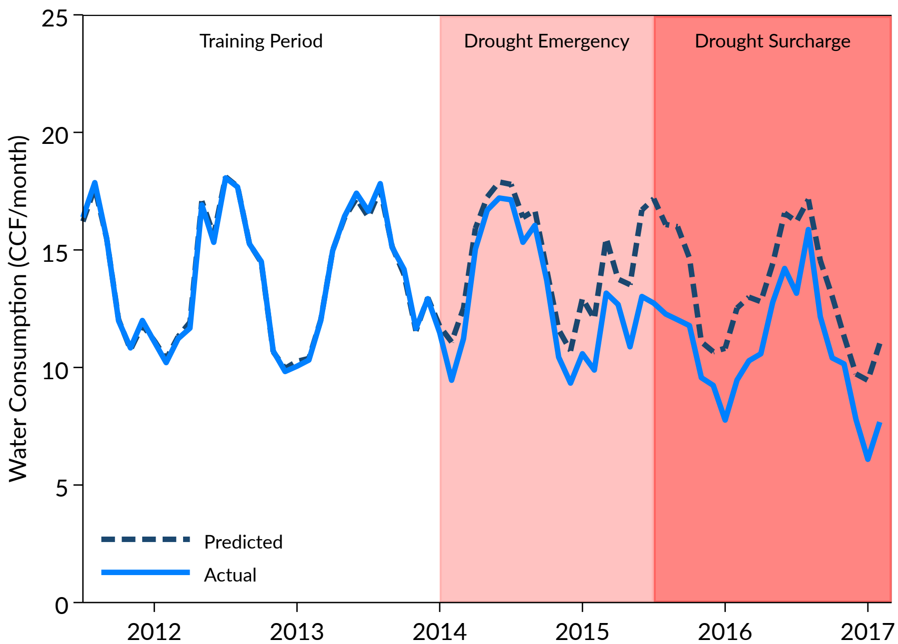
We design a novel identification strategy that solves both of these econometric issues by exploiting exogenous, policy-induced price changes to identify the causal effect of surcharge pricing on demand. First, we train machine learning models using data before the declared drought emergency to generate counterfactual out-of-sample consumption predictions for each household during the drought surcharge period. These counterfactuals represent what household consumption would have been absent the many changes in price and nonprice policies implemented during the drought emergency. We then use the counterfactuals to define a predicted price change that captures exogenous changes in relative exposure to drought surcharges. We use the predicted price change as an instrumental variable in a two-stage least squares (2SLS) demand framework to instrument for the actual marginal or average price change experienced by customers.

4.1 Counterfactual Demand Predictions

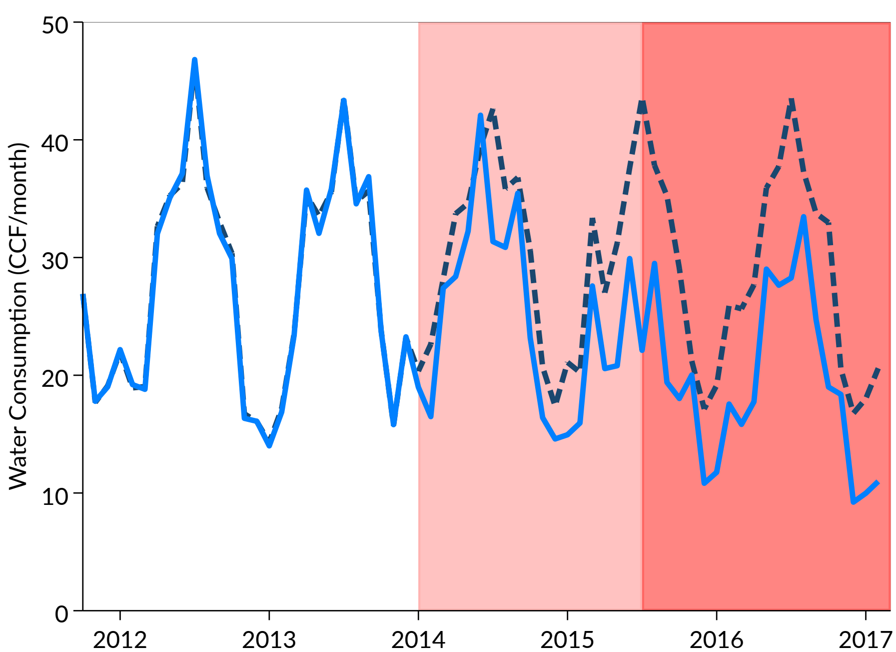
We begin our analysis by using machine learning algorithms to generate counterfactual predictions for monthly household-level water consumption. Specifically, we use random forests to train models of household water consumption using data collected before the drought emergency was declared. We use random forests in part because of their ability to capture highly complex interactions and nonlinear relationships between candidate predictor variables (Hastie et al., 2009). In Table A.1, we provide evidence that using random forests improves predictive accuracy in the predrought surcharge pricing period over simpler OLS predictions.[7] Prior literature also shows that random forests perform well in predictive settings where the goal is ultimately to recover causal estimates of policy impacts on demand for utility services, such as electricity (Prest et al., 2023).

The time period used to train the random forests is all months prior to January 2014, when the first drought emergency was declared. Although drought conditions had begun to worsen and some drought-related policies, such as conservation messaging, were already under way during this time, we are limited by our lack of data before 2011 for either utility. By limiting our training period to observations from 2011–2013, our approach defines the relevant counterfactual as the level of baseline water consumption during early stages of drought but before anticipatory consumption effects related to intensive drought emergency policies (either price or nonprice) began. Our approach mirrors that of Burlig et al. (2020) and Prest et al. (2023) in generating out-of-sample predictions for years outside of the training sample to represent a counterfactual without any policy change. We train the algorithm using a number of candidate predictor variables, including weather data, household and property characteristics, water budgets, demographic data at the census block group level, month-of-sample dummies, and zip-code dummies. We implement the algorithm by generating 500 trees separately for each utility. We then use the resulting outputted ensemble of trees to generate out-of-sample predictions from 2014 onward. We are particularly interested in the out-of-sample predictions from July 2015 to February 2017, when drought surcharges were in place for both the Coastal and Inland utilities.

[7] In Appendix C, we provide results from a series of diagnostic exercises to ensure the reliability of our generated counterfactual predictions. These include comparing model errors (Figure A.2 and Table A.1), tuning several key random forest model parameters (Figure A.3), examining variable importance plots for our final predictions (Figure



(a) **Coastal**



(b) **Inland**

Figure 2: Predicted and Actual Consumption Over Time

Notes: The figure presents the time series of average actual and predicted consumption in each month-of-sample for each utility separately. The training period data used to train the random forest algorithm up to December 2013 is unshaded. The period in which the drought emergency had been declared but drought surcharges were not yet in effect is shaded in pink (January 2014 to June 2015). The period in which drought surcharges were in effect is shaded in red (July 2015–February 2017). Actual consumption (blue solid line) falling below predicted consumption (navy dashed line) indicates water conservation in the aggregate.

We demonstrate our approach visually in Figure 2. The figure shows the time series for both average predicted ($\hat{q}_{it}$) and actual consumption (q_{it}) separately for each utility. We partition the figures into three discrete time periods: the pre-drought months used to train the random forest algorithms ("Training Period," mid-2011 to December 2013); the period in which the drought emergency had been declared but drought surcharges were not yet in effect ("Drought Emergency," January 2014 to June 2015); and the period in which drought surcharges were in effect ("Drought Surcharge," July 2015 to February 2017). As expected, during the training period, the predictions perform quite well on average. After the training period, q_{it} falls below $\hat{q}_{it}$ in the aggregate, which remains at a similar level to that of the training period.

The gap in Figure 2 between $\hat{q}_{it}$ and q_{it} that emerges in 2014 represents aggregate water conservation during the drought. This difference arises because the predictions, despite adjusting for contemporaneous weather conditions, do not incorporate any drought-related policies enacted in 2014 and beyond. The difference can be attributed to the full suite of drought policies, including the drought surcharges. That conservation occurs in the drought emergency period before price changes were implemented demonstrates that nonprice conservation is present in our setting, and highlights the importance of isolating the role of prices from other drought policies.

4.2 Construction of Price Instrument

We next turn to addressing the endogeneity of prices and quantity, which arises in this setting due to the simultaneous determination of quantities and prices under nonlinear rates. With BBRs, the marginal price faced by a household rises with quantity consumed relative to the budget. We address the simultaneity of price and quantity by exploiting predetermined differential exposure to the exogenous change in price due to drought surcharges. First, we use $\hat{q}_{it}$ and water budget formulas to generate predicted prices during the drought surcharge period. Specifically, we calculate the price the household would have faced under surcharge pricing based on its historical consumption patterns and prevailing weather conditions. This predicted price ($\hat{p}_{it}$) is defined by predicted consumption relative to the budget and serves as the first input into our instrument. Next, we use $\hat{q}_{it}$ to calculate the price the household would have paid for that level of predicted consumption before the drought emergency was declared ($\hat{p}_{it}^{pre}$). This second predicted price represents a baseline price that households regularly faced before the drought emergency.

Our final instrument is the difference (in natural logarithms) of these two predicted prices: $\Delta log(\hat{p}_{it}) = log(\hat{p}_{it}) - log(\hat{p}_{it}^{pre})$. Taking the difference of these two predicted prices isolates the exogenous variation in prices that is induced by the policy-induced rate structure change. For example, households that regularly consume well under their budget faced little to no price change as a result of the imposition of drought surcharges, but households who regularly consumed in the higher consumption tiers before the drought faced large changes in inframarginal and marginal prices when surcharge pricing was implemented. The instrument is similar to other "simulated" instruments regularly used in the water and electricity demand literature (e.g., Ito,

A.4), and generating predictions under alternative approaches (Figure A.5).

2014; Sears, 2021), and also has the spirit of a Bartik-type shift-share instrument in that it captures differential exposure to a common price shock (Bartik, 1991; Goldsmith-Pinkham et al., 2020).

Figure 3 demonstrates how the instrument works using average prices. The figure presents a binscatter that plots the means of predicted contemporaneous prices (represented by blue triangles) and predicted prices using pre-drought surcharge pricing (represented by light blue squares) across the distribution of predicted consumption relative to the budget in two percentage point bins. The figure shows that the gap between these two measures (which can be thought of as the predicted price difference instrument) is on average quite small whenever we predict that a customer will consume under the budget. This makes intuitive sense, as prices do not rise much for these households. This gap grows larger with predicted consumption relative to budget, indicating households with higher baseline consumption levels relative to their assigned budget faced relatively higher exposure to drought surcharges due to their pre-existing consumption patterns. Actual average prices faced by the household-month observations in each bin (represented by navy circles) are also plotted and are positively correlated with the instrument, indicating a potentially strong first-stage.

The validity of $\Delta log(\hat{p}_{it})$ as our instrument ultimately rests on whether it satisfies the exclusion restriction; that is, the price difference instrument must isolate exogenous variation in price changes while simultaneously being uncorrelated with unobserved drivers of conservation. Two features are particularly attractive in establishing exogeneity. First, the underlying consumption predictions are generated solely from prediction models trained on data that predate the introduction of drought surcharges, and therefore reflect predetermined consumption patterns that are uncorrelated with the exogenous introduction of the surcharges themselves. Second, the resulting predicted price differences capture price variation that stems solely from changes to the rate structure, and those difference are unaffected by a given household's response to nonprice conservation efforts or idiosyncratic demand shocks.

At this point, the primary remaining threat to identification is if a household's (latent) level of nonprice conservation strongly correlates with $\Delta log(\hat{p}_{it})$. For example, if households with larger predicted price differentials are also more likely to engage in nonprice conservation, then the instrument may be correlated with the error term. Subsequently, nonprice conservation effects will be mistakenly attributed to price changes, which will bias elasticity estimates upwards in absolute value. While this is theoretically possible, the fact that surcharges were implemented within BBRs specifically helps to alleviate concerns about this potential threat to identification. Given the inherent structure of BBRs, both small and large users in absolute terms can regularly exceed their water budget and face large predicted price differentials as a result of surcharges. This introduces substantial heterogeneity in the types of households that face large predicted price differentials and reduces the likelihood of a structural correlation between households with high price differentials and those engaging in substantial non-price conservation. We document further efforts to quantitatively account for nonprice conservation when discussing results in Section 5.

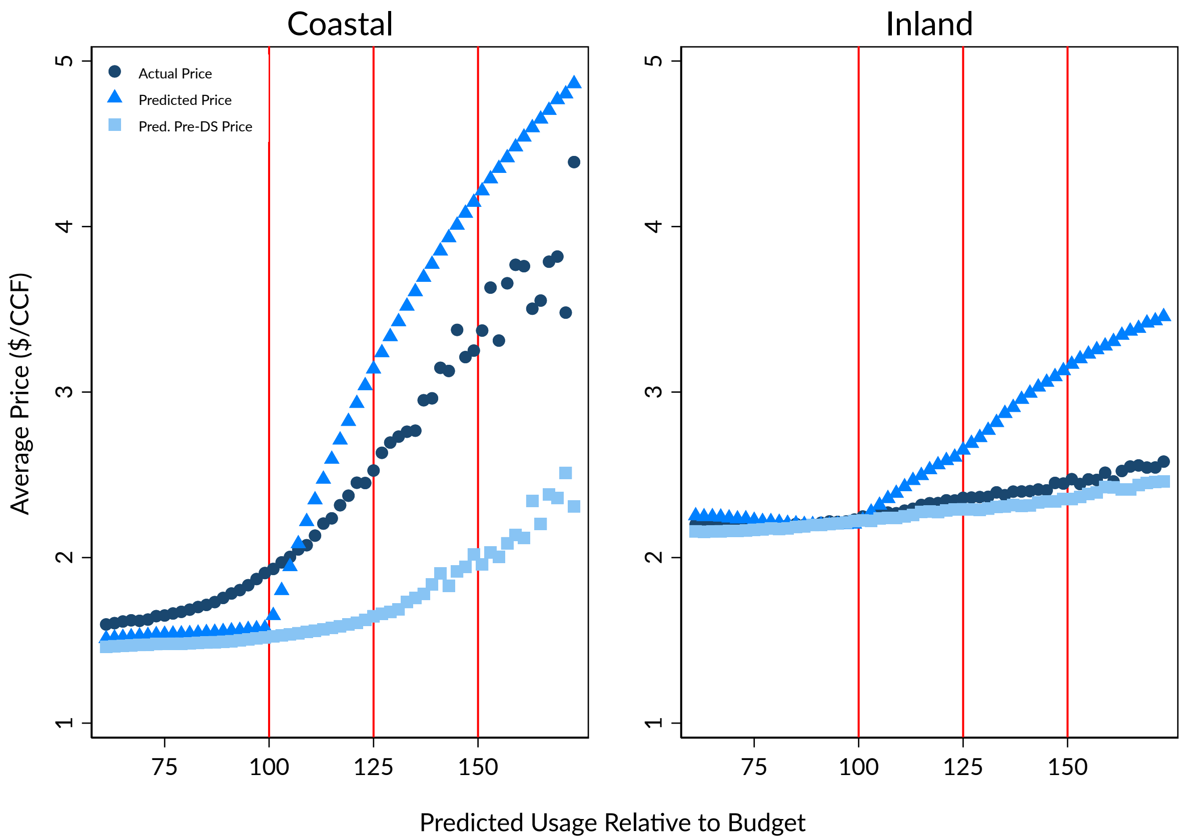


Figure 3: Relationship Between Instrument and Actual Prices

Notes: The figure presents a binscatter that illustrates the relationship between the two components of our instrument and actual prices faced. Actual average prices faced by household-months are plotted over the distribution of predicted consumption relative to budget in a series of two percentage point bins with navy circles. Predicted prices under drought surcharge pricing are represented by blue triangles, and predicted prices using predrought surcharge period prices are represented by light blue squares. The red vertical lines represent the BBR tier thresholds.

4.3 Demand Estimation

We use our predicted price changes, $\Delta log(\hat{p}_{it})$, as an instrument in a reduced-form demand framework to estimate causal price elasticities in response to drought surcharges. We estimate a standard log-log demand equation as follows:

$$\Delta log(q_{it}) = \beta \Delta log(p_{it}) + \delta W_{it} + \alpha_i + \tau_t + \epsilon_{it} \tag{3}$$

where i indexes households and t indexes billing periods (or months). $\Delta log(q_{it})$ is our dependent variable and represents the difference between logged actual consumption and the household's month-specific baseline level of logged consumption from 2011-2013, before the drought emergency: $\Delta log(q_{it}) = log(q_{it}) - log(\tilde{q}_{it})$.[8] This difference captures the total effect of drought surcharge pricing and other nonprice drought policies on consumption within a household. The primary explanatory variable is $\Delta log(p_{it})$, which represents the difference in logged prices faced for a household's observed consumption induced by surcharge pricing: $\Delta log(p_{it}) = log(p_{it}) - log(p_{it}^{pre})$. We instrument for this endogenous price difference with our predicted price difference $\Delta log(\hat{p}_{it})$ in the first stage of the 2SLS framework. We estimate models using both average and marginal prices, given the debate around what price households respond to under nonlinear rates (Nataraj and Hanemann, 2011; Ito, 2014; Wichman, 2014; Shaffer, 2020; Cook and Brent, 2021). We assume, loosely, that average price responsiveness is driven by a form of rational inattention in which households would optimize according to marginal price levels but the information costs of doing so are prohibitive (Sallee, 2014; Wichman, 2017).

We also control for W_{it}, a vector of contemporaneous weather variables, including evapotranspiration, precipitation, and temperature (as well as their squares). As is standard in reduced-form models of demand in panel data settings, we include both household (α_i) and billing period (τ_t) fixed effects. We calculate standard errors in two primary ways: first by clustering at the household level and second by using a bootstrapping procedure developed to account for errors associated with our counterfactual predictions.[9] The time period included is from July 2015 to December 2016, when the drought surcharges were in effect. We drop January and February 2017 from our primary results due to the heavy levels of precipitation in these months, which can cause issues with our out-of-sample predictions. The parameter of interest in Equation 3 is β, which can be directly interpreted as the short-run price elasticity of demand for residential water in response to the temporary drought surcharges. This is the policy-relevant parameter because it characterizes the ability of prices to deliver immediate conservation when needed under drought, as opposed to estimating longer-run changes in conservation due to behavioral adjustments or upgrades to a household's water-using capital stock.

[8] The baseline measure $\tilde{q}_{it}$ is defined as a household's month-of-year specific average over the years 2011-2013. For example, to construct $\Delta log(q_{it})$ for a household in August 2015, its consumption for the months August 2011, 2012, and 2013 is averaged together ($log(\tilde{q}_{it})$) and subtracted from actual consumption in August 2015 ($log(q_{it})$).

[9] Our standard errors are complicated by the fact that our counterfactual predictions are measured with error. We provide a full exposition of our bootstrapping procedure in Appendix C.

5 Results

5.1 Price Elasticity Estimates

We implement the framework outlined in Section 4 to identify the causal effect of drought surcharges on water demand under BBRs. Before estimating our demand regressions, we first check for visual evidence of "bunching" in the consumption distribution to understand if households are responding to nonlinear prices by strategically consuming at kink points in the marginal price schedule (Saez, 2010; Ito, 2014). We check for bunching along the distribution of consumption relative to budget as opposed to consumption alone (which is standard) due to the nature of BBRs.[10] Figure A.6 presents histograms of these distributions during the drought surcharge period for the two utilities. No clear visual evidence of bunching appears at any of the kink points in the BBR schedule, which implies that average price or some other expected price measure may be the more salient price that households respond to (Ito, 2014).

Table 2 presents the base results from estimating Equation 3.[11] Given the log-log functional form, the price coefficients can be directly interpreted as an elasticity, particularly the short-run price elasticity of demand in response to the introduction of temporary surcharges. Columns (1)–(2) present results using average volumetric price (AP) and marginal price (MP) for Coastal, and columns (3)–(4) present corresponding results for Inland.[12] We report the Kleibergen-Paap rk Wald first-stage F-statistic for each specification, which are relatively large and consistent with the intuition from Figure 3 that we have a strong first stage (Kleibergen and Paap, 2006). Bootstrapped standard errors are presented below coefficient estimates in brackets.[13]

The elasticity estimates reported in Table 2 range from -0.22 (inelastic) to -1.07 (roughly unit elastic). These elasticities are within the range of standard estimates from prior meta-analyses of residential water demand studies, which generally report a central price elasticity estimate of -0.4 (Espey et al., 1997; Dalhuisen et al., 2003; Sebri, 2014). Despite their similarity to the extant literature, these results are striking for two reasons. First, our analysis is concerned with estimating the short-run price elasticity induced by drought surcharges. The meta-analyses cited previously show that long-run price elasticity estimates are generally more elastic than short-run estimates, as households have more time to adjust to higher prices by making changes such as upgrading their water-using capital stock (e.g., dishwashers, washing machines, etc.). The elasticity estimates we report here are on the higher end of many short-run estimates, and particularly in the

[10]For example, 10 CCF may be above budget for some households and below budget for others. Theoretically, there is no reason why bunching should occur at any single point in the consumption distribution. However, households could bunch at the kink points of the nonlinear BBR schedule (100%, 125% and 150% of budget).

[11]We estimate all specifications separately for each utility using the "ivreghdfe" package in Stata (Correia, 2018).

[12]Table A.2 demonstrates the need to instrument for endogenous price changes, as elasticities estimated from models that do not instrument for price are positive and suggest upward-sloping demand.

[13]Table A.3 presents the same results with the original standard errors clustered by household. Our bootstrapped standard errors in Table 2 are roughly 25-35% largely than the standard errors clustered at the household level. However, the coefficients are estimated with enough precision that statistical significance under standard levels is unaffected. This result, combined with the computational time constraints associated bootstrapping, means that we move forward with presenting clustered standard errors in other results reported in this paper.

Table 2: IV Demand Regressions

	Coastal		Inland	
	(1)	(2)	(3)	(4)
	AP	MP	AP	MP
Δ log(AP)	-0.44***		-1.07***	
	[0.05]		[0.07]	
Δ log(MP)		-0.22***		-0.48***
		[0.02]		[0.03]
Observations	477,110	477,110	203,187	203,187
Households	26,988	26,988	10,840	10,840
Household FE	Y	Y	Y	Y
Month-of-Sample FE	Y	Y	Y	Y
First-stage F-stat	2,737	2,199	4,214	5,688

Notes: The table presents estimates of $\hat{\beta}$ from estimating Equation 3. The dependent variable is the difference between contemporaneous and baseline consumption, $\Delta log(q_{it})$. Endogenous price differences are instrumented for in the first stage using $\Delta log(\hat{p}_{it})$. The time period included is from July 2015 to December 2016. Columns 1 and 3 instrument for average price differences, and Columns 2 and 4 instrument for marginal price differences. All specifications include a vector of weather covariates, including evapotranspiration, precipitation, temperature, and their squares. Bootstrapped standard errors calculated according to the procedure outlined in Appendix C are presented below coefficient estimates in brackets. ***: $p < 0.01$; **: $p < 0.05$; *: $p < 0.1$.

case of Inland are more comparable to many long-run price elasticity estimates.[14] This suggests that surcharges were effective to some degree in eliciting conservation under during drought.

Second, our results are notable because most prior studies estimate price elasticities in response to *permanent* changes in prices. However, in our setting, the price variation is largely driven by the introduction of *temporary* drought surcharges. Households were well-informed that the surcharges were imposed in response to severe drought conditions and not intended to remain in place permanently. Therefore, it is unlikely that households would have made changes to their water-using capital stock due to such temporary price increases.[15] Given this fact, we would expect *ex ante* that our price elasticity estimates would be relatively more inelastic, all else held equal. The fact that we find elasticity estimates comparable to (and in some cases, more elastic than) the extant literature runs counter to these expectations and lends further evidence to the notion that that price increases can induce conservation in the short run.

Two other notable features emerge from the base elasticity estimates. First, and consistent with other recent analyses, we find more elastic demand with respect to the average price compared to the marginal price, with elasticity magnitudes roughly double that of marginal price across specifications (Ito, 2014; Wichman, 2014; Browne et al., 2021). Second, as referenced briefly

[14] A recent quasi-experimental study in a similar setting (urban demand in California during drought) reports short-run price elasticity estimates of -0.44 to -0.32, and long-run price elasticity estimates of -1.5 to -0.6 (Sears, 2023).

[15] It is possible that customers might make changes to their water-using capital stock due to nonprice policies, such as rebates or social pressures. However, those effects are not driven by exogenous changes in prices, which we exploit through our IV approach.

above we note the heterogeneity in the elasticity estimates between Coastal and Inland. Estimated elasticities are significantly larger for Inland than for Coastal. This is plausibly driven by the presence of larger average lawn sizes for Inland, which implies that a greater proportion of overall consumption by Inland households is comprised of water used for relatively price-elastic outdoor purposes as opposed to necessary indoor activities like drinking, cooking, and bathing.

As discussed previously in Section 4, the primary threat to identification is if nonprice conservation is systematically correlated with the predicted price instrument. Although nonprice conservation is unobservable in our setting, we attempt to control for it by exploiting the fact that we have data during a time period (2014 to mid-2015) when drought awareness was high and nonprice conservation efforts were underway, but before drought surcharges were implemented. We construct a proxy variable for nonprice conservation by taking each household's month-specific logged consumption average during this intermediate period, and subtracting from it the same logged baseline consumption during 2011–2013 constructed previously: $\Delta log(q_{it}^{NP}) = log(\bar{q}_{it}^{2014-2015}) - log(\bar{q}_{it}^{2011-2013})$. This difference captures the amount of conservation in each month that households undertook before the formal introduction of price increases through surcharges. Figure A.7 displays a binscatter where average values of $\Delta log(\hat{p}_{it})$, our predicted price instrument, are plotted in each of 50 discrete bins of $\Delta log(q_{it}^{NP})$ during the surcharge pricing period for both utilities. Figure A.7, along with the low overall correlation coefficient between the two variables ($r = -0.02$) suggests that there is only weak evidence of a systematic correlation between the instrument and nonprice conservation. We report as a robustness check a series of estimating results in Table A.4 where we directly include $\Delta log(q_{it}^{NP})$ as an additional control variable to proxy for nonprice conservation. Resulting elasticity estimates are of a similar magnitude but become slightly less elastic across all four models, which is consistent with the notion that the instrument may pick up some limited nonprice conservation effects. This robustness exercise helps to assuage concerns that our short-run elasticity estimates are significantly overestimated.[16]

5.2 Contribution of Prices to Aggregate Conservation

Our empirical framework outlined in Section 4 defines the relevant counterfactual as the consumption had no drought policies (price or nonprice) been enacted. As a result, the causal elasticity estimates presented here represent households' short-run responsiveness to surcharge pricing *conditional* on the presence of the full suite of the other nonprice policies (e.g., turfgrass rebates, conservation messaging) employed by each utility. This conditional elasticity is the policy-relevant parameter of interest, as it is rare for utilities to only use price changes without any nonprice policies to promote conservation during droughts. In our setting Figure 2 shows evidence of nonprice conservation before surcharges were enacted. Given the clear presence

[16] We subject our primary elasticity estimates to additional robustness checks, which largely yield similar results. In Table A.5 we alter the form of τ_t to control for fixed effects at the month-of-sample by zip-code level. In Table A.6, we use the prediction errors generated from our counterfactual predictions directly as the dependent variable.

of both price and nonprice conservation in our setting, we next characterize how much of the aggregate water conservation observed in each utility is directly attributable to the drought surcharges themselves. To do so, we implement an approach that combines information on our price elasticity estimates, observed changes in prices under surcharges, and the total observed water conservation to back out estimates of the proportion of water conservation directly attributable to price changes.

The first input to this exercise is the price elasticity of demand, which we source from Table 2. The second input is a characterization of the price changes that households perceived under surcharges. The endogeneity of nonlinear prices again poses a problem here. Simply using actual observed price changes likely underestimates the true price-based conservation, as it does not account for the reverse causality inherent in nonlinear pricing (i.e., some households appear to have lower price changes precisely because they are responding to higher prices by lowering their consumption). At the same time, assuming that predicted price changes represent the true price change likely overestimates price-based conservation, as it attributes non-price conservation to changes in the price itself. To more accurately characterize the true price change that households faced, we calculate the average prices *under surcharge pricing* that would have been paid by households during the period in which nonprice conservation had begun but surcharges had not yet been enacted (2014–mid 2015). We then compare this to actual prices faced from the predrought period of 2011–2013 to calculate price changes. Characterizing the actual price change faced by households in this way isolates the change in price that surcharges introduced after accounting for nonprice conservation, and thus avoids the issue of systematically under- or over-estimating price changes inherent in the other two approaches.

The third and final input is an estimate of the aggregate conservation achieved by the two utilities. Rather than compare consumption under surcharge pricing to earlier years, we take predicted consumption ($\hat{q}_{it}$) during the drought surcharge period as the appropriate counterfactual, as these predictions adjust for contemporaneous weather conditions and best represent what consumption would have been in the absence of drought-related policies. We predict that, on average, households would have consumed 13.73 CCF per month in Coastal and 30.49 CCF per month in Inland during the drought surcharge period had no drought policies been implemented. Average prediction errors, which are the difference between actual consumption and our predictions, are -2.5 CCF for Coastal and -9.3 for Inland. Dividing these prediction errors by the predicted consumption yields our estimates of total conservation for each utility: an 18.5% reduction in consumption for Coastal and a 30.5% reduction in consumption for Inland.[17]

We bring our elasticity estimates, price change estimates, and estimates of total conservation together in Table 3 to demonstrate demand responses directly attributable to the drought sur-

[17]While the statewide goal was to achieve a 25% reduction in urban water consumption relative to 2013, the State Water Resources Board assigned utilities differing conservation targets according to their prior baseline consumption levels in terms of GPCD from summer 2014. These targets ranged from 4% to 36% for the highest-consuming utilities. Under this regulation, Coastal was assigned a 20% reduction target, and Inland was assigned a 32% reduction target. Our results imply that both utilities were close to achieving these targets, though our methodology differs from California's method of determining compliance.

Table 3: Decomposing Price-Driven Water Conservation Effects

Coastal		
	AP	MP
Price Conservation (%)	-3.4	-8.3
	[-4.1,-2.7]	[-10.0,-6.6]
% Aggregate Conservation	18.3	45.1
	[14.5,22.1]	[36.0,54.3]
(1) Price Elasticity	-0.44	-0.22
(2) Baseline Price	1.64	2.27
(3) Price Increase	0.13	0.87
(4) Total Conservation (%)	-18.45	-18.45
Inland		
	AP	MP
Price Conservation (%)	-6.4	-5.1
	[-7.2,-5.6]	[-5.8,-4.5]
% Aggregate Conservation	21.0	16.8
	[18.3,23.8]	[14.6,19.0]
(1) Price Elasticity	-1.07	-0.48
(2) Baseline Price	2.26	2.81
(3) Price Increase	0.14	0.30
(4) Total Conservation (%)	-30.49	-30.49

Notes: The table presents estimates of water conservation directly attributable to drought surcharges. Price Conservation (%) represents the estimated water conservation directly attributable to prices. % Aggregated Conservation represents the percentage of aggregate observed conservation in each utility that is attributable to the price-based conservation. These estimates are constructed as nonlinear combinations of elasticity coefficients from our demand models in Table 2 along with price change and conservation scalars. 95% confidence intervals using bootstrapped standard errors are presented below point estimates in brackets.

charges themselves. In the *Price Conservation (%)* row, we present changes in demand implied by our regressions by using the point-elasticity of demand formula and multiplying the relevant elasticity estimates by the corresponding percentage change in price. In the % *Aggregate Conservation* row, we divide the price conservation estimate by the total observed conservation to get an estimate of the percentage of the total observed conservation that is directly attributable to prices. Our results show that in Coastal, the price changes under surcharges induced a 3.4–8.3% demand reduction, which accounts for 18.3–45.1% of total observed conservation. The analogous demand reductions for Inland are 5.1–6.4%, which represents 16.8–21.0% of total observed conservation. Most model specifications imply that drought surcharges alone likely accounted for only around one-fifth of the total conservation observed across the two utilities.[18] Even the most generous

[18]These estimates are smaller but in the general ballpark of those reported in Browne et al. (2021), who attempt a similar exercise and attribute 40–44% of the total demand reduction observed in Fresno under drought to price changes, albeit under uniform rates and more limited price variation.

model estimates can only attribute roughly half of observed consumption to prices alone. This is surprising given the fact that our elasticity estimates are relatively large in magnitude. One explanation is that the consumers do not face significant exposure to high prices despite the large change in marginal prices as evidenced by the observed price change in Table 3. Average prices only increased by 8% in Coastal and 6% in Inland. This is partly due to the fact that nonprice conservation reduces the exposure to high marginal prices for over-budget consumption. Our results suggest that, although surcharges can be effective at inducing conservation during drought, nonprice conservation policies play a relatively larger role in managing demand, at least in the context of the BBRs considered here.

5.3 Characterizing Heterogeneity in Demand Response

So far, the analysis has not addressed the possibility that households may exhibit a heterogeneous response to the introduction of surcharge pricing based on pre-existing characteristics. We estimate a variant of Equation 3 that allows for heterogeneous responses to surcharge pricing based on two household characteristics defined using pre-drought data: average budget class and average total consumption. The budget class is the typical budget tier that a consumer faced in 2011–2013 before the introduction of drought policies. We focus on three budget classes–under budget ($< 100\%$) between $100 - 150\%$, and $> 150\%$ of budget. These consumers will experience different price changes relative to pre-surcharge pricing. A household that typically uses less than their budget will see very large marginal price increases for any demand shocks that push them over budget (though average price changes may be relatively small). Households regularly using between 100-150% will see both marginal and average price increases as a result of surcharge pricing. Households that typically consume more than 150% of budget will not experience any marginal price increase, but their total bill will rise due to inframarginal price increases on their over-budget consumption. As such, we interact dummy variables for each budget class with our price change variables. We also repeat the exercise using the simpler definition of predrought consumption terciles to facilitate comparisons.

Results of the heterogeneous demand estimation exercise assuming marginal price responsiveness are reported in Table 4.[19] Columns (1) and (3) present results for budget classes, and Columns (2) and (4) present results for consumption terciles. The omitted interaction terms are for the subgroups consuming below their budget and the first consumption quartile, respectively. Therefore, the base coefficient $\Delta log(MP)$ represents the price elasticity for the omitted group, and the elasticity for the higher consumption classes can be calculated as the linear combination of the base coefficient with the coefficient for the corresponding interaction. For example, column 3 indicates that regular under-budget households in Inland exhibit a price elasticity of -0.93, those regularly using $100 - 150\%$ of budget exhibit a price elasticity of -0.38, and those regularly using $> 150\%$ of budget exhibit a price elasticity of -0.28. Across all model specifications in Tables 4 and A.7, a consistent pattern appears: all interaction terms are positive and statistically significant,

[19] Table A.7 presents the equivalent results under average price responsiveness.

indicating that households who regularly consumed under-budget are the most responsive to drought surcharges. Additionally, elasticities monotonically decrease in absolute value (become more inelastic) as one moves up to higher budget or consumption classes across all specifications.

Table 4: Heterogenous Price Elasticities, Marginal Price

	Coastal		Inland	
	(1) Budget	(2) Consumption	(3) Budget	(4) Consumption
Δ log(MP)	-0.32***	-0.89***	-0.93***	-0.73***
	(0.02)	(0.12)	(0.06)	(0.05)
Δ log(MP) × Budget (100–150%)	0.18***		0.55***	
	(0.02)		(0.05)	
Δ log(MP) × Budget (>150%)	0.23***		0.65***	
	(0.06)		(0.05)	
Δ log(MP) × Q2		0.50***		0.30***
		(0.11)		(0.05)
Δ log(MP) × Q3		0.72***		0.33***
		(0.11)		(0.05)
Observations	477,110	477,110	203,187	203,187
Households	26,988	26,988	10,840	10,840
Household FE	Y	Y	Y	Y
Month-of-Sample FE	Y	Y	Y	Y
First-stage F-stat	501	78	698	1,107

Notes: The table presents estimates of $\hat{\beta}$ from estimating a variant of Equation 3. The dependent variable is the difference between logged contemporaneous and logged baseline consumption, $\Delta log(q_{it}) = log(q_{it}) - log(\tilde{q}_{it})$. Logged endogenous prices are instrumented for in the first stage using $\Delta log(\hat{p}_{it})$, which is interacted with dummy variables for terciles of predrought budget and consumption classes. The time period included is from July 2015 to December 2016. All columns instrument for marginal price differences. All specifications include a vector of weather covariates, including evapotranspiration, precipitation, temperature, and their squares. Standard errors are clustered at the household level and are presented below coefficient estimates in parentheses. ***: $p < 0.01$; **: $p < 0.05$; *: $p < 0.1$.

These results provide important context to our previously discussed findings. Elasticities from Table 2 suggest a clear demand response in response to surcharges, yet Table 3 documents that the surcharges are likely not driving most of the observed conservation. In addition to the budgets shielding households from aggregate price increases, the heterogeneous elasticities in Table 4 help reconcile these two results. Results demonstrate that the households who are most responsive to price changes are those least likely to actually ever face the drought surcharges: under budget households. These households can be large or small users in aggregate terms, but their defining feature is that consumption regularly falls below their assigned water budgets, effectively shielding them from facing the surcharges. This finding helps to explain why surcharges were not the primary driver of water conservation, as the households that regularly faced the surcharges exhibited the most inelastic demand response. Tying surcharges to budget assignments in this way is potentially regressive if it shields wealthier, larger users with large lawns and generous water budgets from paying high marginal prices on their outdoor water consumption, which subsequently provides an implicit subsidy from low- to high-income users.

6 Distributional Analysis

We now turn our focus to the distributional implications of the drought surcharges. Results from the demand estimation document that, while households did undertake conservation efforts in response to surcharge-driven price changes, the intensity of this demand response varied significantly across heterogenous consumption groups. Inelastic demand for an essential good (residential water) with no perfect substitutes implies that households are likely to bear a substantial portion of the burden of price increases. This naturally raises the question: to what extent were these price increases shared equitably across households? Several policy-relevant results emerge from our analysis, which we discuss in turn.

6.1 Result #1: Low-income households are the most responsive to price changes

A first-order concern in unpacking distributional effects of price changes is understanding how households across the income distribution differ in their demand response. Unfortunately, a limitation of our data is that we do not have a direct measure of income for each household. This concern is common in studies relying on utility billing microdata, as utilities historically have not had the need to collect this information. To generate estimates of household income, we implement an approach that combines property value data with Census data on average household incomes in each block group. Our procedure ranks households within each block group by their observed property value and then assigns each household an estimated income based on the observed income distribution reported in the Census data for that block group.[20] We describe this procedure fully in Appendix D.[21]

We use our household income estimates to calculate observed average monthly water expenditures as a percentage of a household's monthly income across income groups. The results, presented in Figure A.8, show that the rates we observe, like most utility expenditures, are regressive: lower-income households devote a larger share of their income to water expenditures under the existing rates, both before and after the introduction of surcharge pricing. The overall regressive nature of the rates observed here is not specific to the surcharge pricing scheme or

[20] Median income classes resulting from this procedures are $\$100 - 125$ thousand dollars in Coastal and $\$75 - 100$ thousand dollars in Inland. These results lend credence to our income estimation procedure, as they are consistent with two stylized facts known about the study areaa. First, Coastal is relatively wealthier than Inland. Second, Southern California in general is wealthier than the state of California as a whole (with a median income of $\$64,500$ in 2015 dollars). Source: `https://www.census.gov/content/dam/Census/library/publications/2016/acs/acsbr15-02.pdf`. (accessed January 23, 2025).

[21] This approach still may not serve as a reasonable proxy for income if assessed property values diverge widely from home sales prices, which are often considered a better proxy for income. We possess sales price data for Coastal only. We re-implement our income estimation procedure using sales prices for Coastal as a robustness check, and find a strong, positive correlation between our income proxy estimated using property values and the alternative proxy generated from sales data ($r = 0.82$). As a result, we feel comfortable moving forward with the property-value based income proxy, given that we do not have readily available home price data for Inland.

BBRs as a whole. It is well understood that water rates in general are a regressive means of raising revenues, as opposed to other mechanisms such as income or property taxes that more directly target wealth (Wichman, 2024).

Given that low-income households devote a higher proportion of their income to water expenditures, it is reasonable to suspect that they might be more responsive to price changes. We investigate this by returning to our demand estimation framework and estimating heterogenous price elasticities across the income distribution, reporting a distinct elasticity for each income quartile. Results assuming average price responsiveness are presented in Table 5.[22] Across both utilities, the lowest income quartile exhibits the most elastic demand response, with higher-income households exhibiting less elastic demand responses.[23]

The implications of these results for assessing the distributional impacts of price changes are nuanced. While lower-income households may exhibit the most elastic demand response, they may not actually pay the highest prices for water if they decrease demand to regularly stay under their assigned budget. Put differently, these low-income households make the tradeoff between decreased consumer surplus from water conservation in exchange for avoiding increased water expenditures. Whether this is ultimately desirable from an equity perspective depends on whether these households are cutting back on water for high-value, essential purposes (e.g., drinking, bathing) or less essential uses (outdoor irrigation). High-income households exhibit the least elastic demand response. This is potentially attractive from an equity perspective, as a higher proportion of total revenues will be raised from these higher-income households. However, the structure of BBRs potentially shields many high-income households from facing the surcharges, as larger budgets are assigned to households with larger lawns, effectively delaying the point at which surcharges kick in.[24] Ultimately, surcharges must bind if they are to induce disproportionate consumption reductions by the largest users.

6.2 Result #2: Surcharges do little to improve the overall regressivity of existing rates

As is clear from the prior discussion, adding drought surcharges on top of BBRs add an additional layer of complexity to understanding the distributional impacts of price changes in our set-

[22]We present analogous results assuming marginal price responsiveness in Table A.8. For robustness, in Table A.9, we additionally estimate the same demand regressions for Coastal only using our alternate income proxy defined from sales prices. Results are nearly identical to those presented for Coastal in Tables 5 and A.8, lending further credibility to our income proxy measures defined using property values.

[23]These results are consistent with some others found in the extant literature. Yoo et al. (2014) find that lower-income and lower-consumption households are more responsive to prices. Wichman et al. (2016) also find that low-income households are more sensitive to price increases, but they find that large users are more responsive to nonprice conservation policies. El-Khattabi et al. (2021), however, find that large users are more responsive to price, and price elasticities do not vary across the income distribution. So, the evidence is ultimately mixed.

[24]We investigate this point descriptively by assessing how water expenditures are distributed across the largest users that surcharges are intended to target. In Table A.10, we calculate shares of total expenditures and consumption that are borne by those who go above their water budgets on average, and those in the top quartile of the budget and irrigable area distributions, all defined using the predrought surcharge pricing period. We find that the share of total revenues and consumption by these large user groups remains largely unchanged between the predrought surcharge pricing period and under surcharge pricing.

Table 5: Heterogenous Elasticities by Income Quartiles, Average Price

	Coastal	Inland
	(1) AP	(2) AP
Δ log(AP)	-0.71***	-1.22***
	(0.08)	(0.09)
Δ log(AP) × I2	0.04	0.07
	(0.08)	(0.10)
Δ log(AP) × I3	0.24***	0.17*
	(0.08)	(0.10)
Δ log(AP) × I4	0.35***	0.24**
	(0.07)	(0.10)
Observations	477,110	203,187
Households	26,988	10,840
Household FE	Y	Y
Month-of-Sample FE	Y	Y
First-stage F-stat	367	827

Notes: The table presents estimates of $\hat{\beta}$ from estimating a variant of Equation 3. The dependent variable is the difference between logged contemporaneous and logged baseline consumption, $\Delta log(q_{it}) = log(q_{it}) - log(\tilde{q}_{it})$. Logged endogenous prices are instrumented for in the first stage using $\Delta log(\hat{p}_{it})$, which is interacted with dummy variables for quartiles of predrought income classes. The time period included is from July 2015 to December 2016. All specifications include a vector of weather covariates, including evapotranspiration, precipitation, temperature, and their squares. Standard errors are clustered at the household level and are presented below coefficient estimates in parentheses. ***: $p < 0.01$; **: $p < 0.05$; *: $p < 0.1$.

ting. Although surcharges are designed with cost-recovery and conservation as the primary goals (as opposed to equity), they could reduce the potential regressivity of BBRs if high-income, high-use households face binding price increases. However, it is difficult to draw any definitive conclusions about changes in relative regressivity resulting from the imposition of surcharges from the prior analyses because of the nonprice conservation that occurs between the pre-surcharge pricing period and the surcharge pricing period.

To assess whether surcharges can improve the redistributive properties of BBRs, we again leverage our counterfactual predictions during the surcharge pricing period from the demand analysis, $\hat{q}_{it}$. We calculate bills using $\hat{q}_{it}$ under the BBRs with surcharges and also using prices from before the introduction of surcharge pricing. Using $\hat{q}_{it}$ to calculate counterfactual bills is useful because the predictions represent a baseline level of consumption for each household that is unaffected by the full suite of drought policies in place during the surcharge pricing period.

This allows us to isolate changes in expenditures that result from the introduction of surcharges.

We illustrate the distribution of predicted expenditures before and during surcharge pricing by constructing Lorenz curves and Gini coefficients similar to Levinson and Silva (2022). Under the standard approach to constructing Lorenz curves, one plots the share of income held by each percentile of households, ordered by income. Lorenz curves further away from the 45-degree diagonal indicate higher levels of income equality (i.e. the poorest 50% of households may only hold 20% of aggregate income). Gini coefficients on a scale of 0 to 1 can then be calculated to indicate the relative level of income inequality. We first plot the share of predicted water bills paid by each percentile of households ordered by income, both before and during surcharge pricing. Plotting expenditures instead of income implies that a lower-hanging Lorenz curve signals more inequality in water expenditures across the income distribution. We additionally construct standard income-based Lorenz curves and Gini coefficients for comparison. By comparing the two sets of curves, we can assess the relative progressivity of the rate structures we observe. If the share of water expenditures is more equal than the share of income across the income distribution (i.e., the water-expenditure Lorenz curve is closer to the 45-degree line than the income Lorenz curve), then water bills are regressive, as lower-income households pay a higher share of water expenditure relative to their share of income.

Figure 4 illustrates our water expenditure Lorenz curves under drought surcharges for each utility. The Lorenz curves under surcharge pricing fall slightly below the diagonal in each utility, signaling that lower-income households do bear a proportionally lower share of total water expenditures. The Gini coefficients associated with these Lorenz curves are 0.11 and 0.07 for Coastal and Inland, respectively. Figure 4 also displays the expenditure Lorenz curves that are calculated under predrought surcharge pricing, or standard BBRs. The Gini coefficients associated with these Lorenz curves are 0.08 and 0.07 for Coastal and Inland, respectively. In both utilities, the expenditure Lorenz curves lie nearly on top of each other, with surcharges inducing some limited increases in progressivity in Coastal.[25] We compare these results to the standard income Lorenz curves, which are also plotted on Figure 4 with the blue solid line. The income Lorenz curves fall further below the diagonal than the expenditure Lorenz curves, with associated Gini coefficients of 0.32 for each utility.[26]

Two key takeaways emerge. First, the overall regressivity documented in Figure A.8 is further confirmed by the Lorenz curves in Figure 4. This regressivity is implied by the fact that the share of total water expenditures faced by lower-income households is higher than the share of total wealth held at each point along the income distribution. For example, in Coastal, the bottom 50% of households in terms of income hold only 25% of aggregate income, but cover 42% of total water expenditures. In Inland, the poorest 50% of households also hold only 26% of aggregate

[25]Note that these Gini coefficients are not directly comparable to the "electric" Ginis reported in Levinson and Silva (2022), as we calculate Ginis based on the income distribution rather than the consumption distribution. This allows us to focus on how surcharges potentially redistribute income between relatively wealthier and poorer households.

[26]Estimated Gini coefficients for the entire United States are on the order of 0.4-0.42. This indicates that estimated incomes in our two utilities are slightly more equal than in the country as a whole. Source: `https://fred.stlouisfed.org/series/SIPOVGINIUSA` (accessed January 23, 2025).

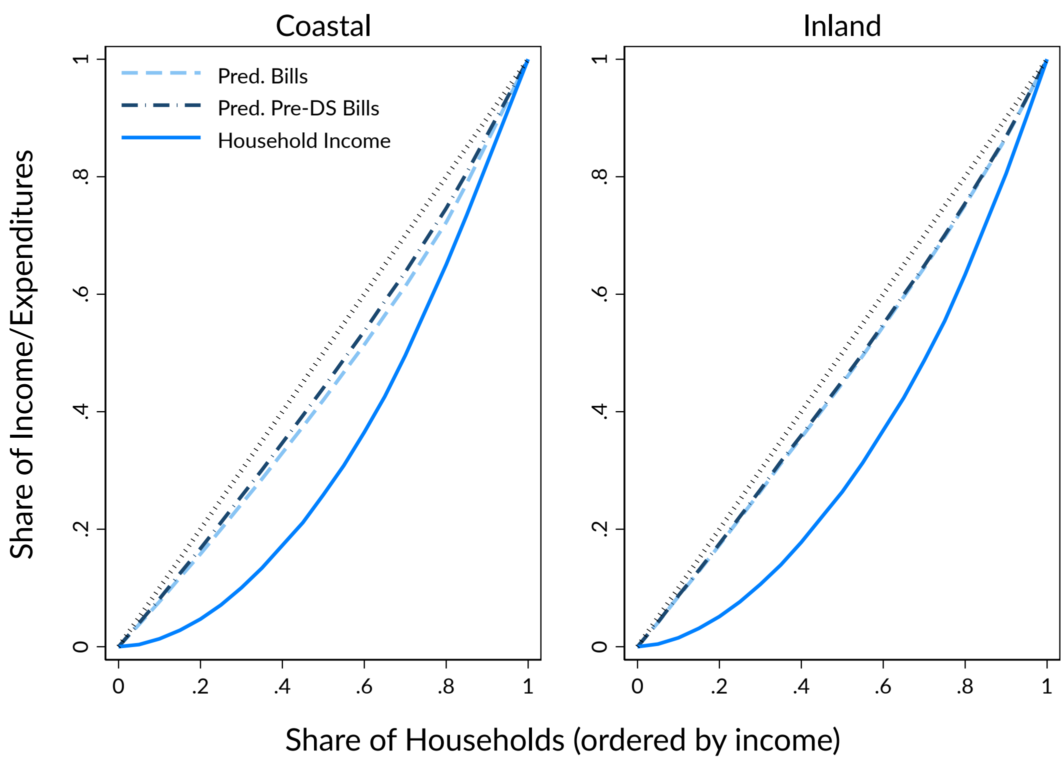


Figure 4: Distribution of Water Expenditures and Household Income

Notes: The figure presents Lorenz curves indicating the share of predicted water expenditures under surcharge pricing (dashed light blue line), the share of predicted water expenditures under pre-surcharge pricing (dashed navy line), and share of household income (solid blue line) that accrue to each percentile of the household distribution ordered by income. The time period included is the drought surcharge period (July 2015–December 2016). The 45° diagonal is plotted in the dotted black line and represents perfect equality (i.e., the bottom x% of households pay x% of water expenditures).

income but pay 45% of total water expenditures. For water rates to be progressive, expenditure shares must be lower than income shares. Overall, the rates observed here do not lead households to pay for water in proportion to their income. Although water utilities generally want to have progressive rate structures, their ultimate equity objectives are often not made explicit. Without knowing a utility's underlying objective function, it is difficult to say whether BBRs are achieving their equity objectives.

Second, the drought surcharges themselves do not appear to increase progressivity relative to the prices charged before the introduction of surcharges, as evidenced by the similarity of the Gini coefficients and expenditure Lorenz curves under both sets of prices. Although surcharges are not designed with equity as the primary goal, significant income shifts could occur if wealthier, higher-use households face binding price increases. We observe little to no change in progressivity due to surcharges, despite using predicted consumption that captures pre-drought baseline consumption and does not allow for households to engage in price or nonprice conservation in response to changing conditions. This result implies that, even under optimistic assumptions, surcharges should not be expected to be much more progressive than prevailing water rates, as they do not appear to bind for enough, or the right type of, households.[27]

6.3 Result #3: Simpler rate structures possess desirable equity properties

The analysis so far is shows that BBRs in our setting are regressive and that drought surcharges do not materially affect equity. Next, we seek to understand how the distributional properties of the observed rates perform relative to feasible alternative rate structures. To facilitate this analysis, we construct counterfactual rate structures for comparison with the bills households face under the existing BBRs, focusing on the surcharge pricing period. We again leverage $\hat{q}_{it}$, our measure of predicted consumption, to calculate bills under the various alternatives. Using the predictions to calculate counterfactual bills allows us to focus on changes in bills due to variation in the alternative rate structures themselves, and not due to other nonprice conservation.

We construct counterfactual bills under three alternative rate structures: a uniform rate, a uniform rate coupled with a variable fixed fee tied to household income (Burger et al., 2020; Borenstein et al., 2021), and an IBR designed to mimic the tiers of the existing BBRs. To facilitate comparisons, we calculate prices charged under each counterfactual rate structure under an assumption of revenue neutrality, where the aggregate variable commodity charge revenues raised remains constant given observed consumption. Appendix D contains a full description of these counterfactual prices and how they are calculated. We then directly incorporate price responsiveness by allowing households to adjust their consumption in response to the new prices that they face under each counterfactual rate, with ultimate consumption adjustments determined by the average price elasticities reported in Table 2. For example, a Coastal household who faces

[27] Another explanation is that the correlation between income and water consumption is too weak for any rate structure to be meaningfully progressive. The correlation coefficient between consumption and household income is 0.31 in Coastal and 0.25 in Inland.

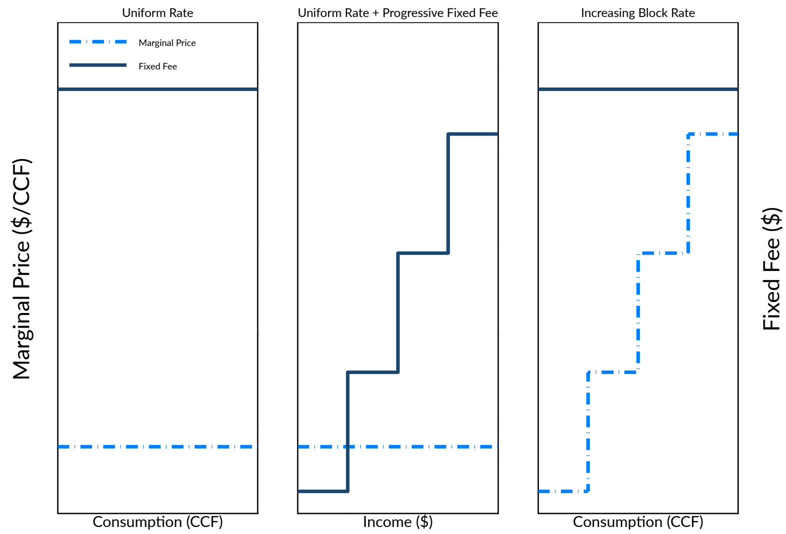


Figure 5: Counterfactual Rate Structures

Notes: The figure conceptually illustrates the three counterfactual rate structures considered: a uniform rate, the same uniform rate combined with a "progressive" fixed fee tied to income, and an increasing block rate. In each panel, marginal prices are graphed on the left vertical axis, and fixed fees are graphed on the right vertical axis. The horizontal axis is consumption in CCF for the uniform rate and increasing block rate, and household income for the uniform rate + progressive fixed fee.

higher average prices under an IBR than a BBR will lower their consumption in response based on the estimated elasticity.

Figure 5 illustrates how each of the three alternative rate structures works in theory. Under uniform rates, households pay both a flat fixed fee and marginal price that are constant across all units of consumption. In the second panel, we graph rates against income to illustrate how pairing the uniform rate with a progressive fixed charge operates. As before, the marginal price is constant and therefore does not vary with income. The progressive fixed fee, however, does rise with income. We illustrate this rise in Figure 5 as a series of discrete tiers, but in theory, utilities could design such a fee in a number of ways, including as a continuous measure. We return to depicting consumption on the horizontal axis in the third panel showing a hypothetical IBR. As before, the fixed fee does not vary with consumption, but the marginal price increases in discrete tiers as users move into higher consumption tiers. Recall that these tiers are the same for all households and are not defined individually as under BBRs.

We proceed to compute our counterfactual bills as the sum of variable commodity charges for water and fixed service changes, while abstracting away from other fees, such as sewer charges. Table A.11 presents average bills for each rate structure, broken out by quartiles of the estimated income distribution. Average bills are higher in Inland due to higher consumption overall. Bills monotonically increase along with income under all rate structures for both utilities, indicating that consumption is correlated to a degree with income. Average bills tend to be higher for IBRs

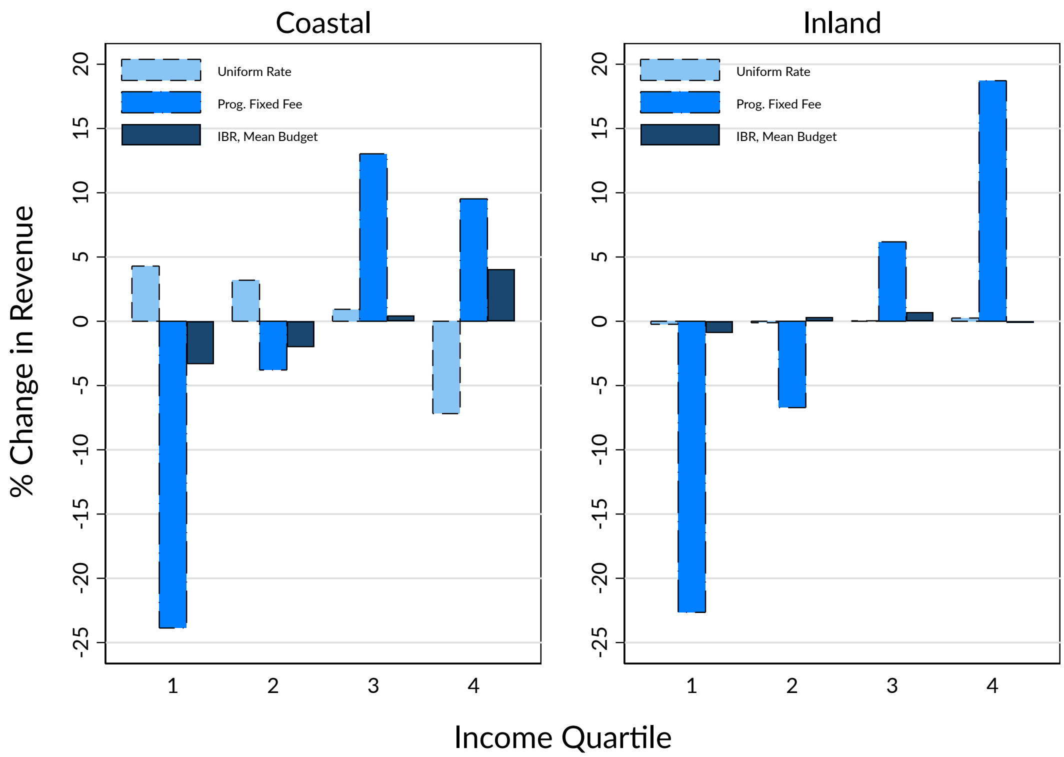


Figure 6: Revenue Changes by Income Quartile

Notes: The figure presents percentage changes in the proportion of revenues raised from each quartile of the income distribution when moving from BBRs to each of three counterfactual rate structures, respectively. Negative values indicate that proportionally less revenue is raised from that quartile of homes under the alternative structure compared to a BBR, and positive values indicate that proportionally more revenue is raised from that quartile relative to BBRs. Results incorporate household price response by allowing for a one-time consumption adjustment in response to new prices using average price elasticities sourced from Table 2.

in the highest income quartiles. When considering the range of average bills, the progressive fixed fee and the IBR tend to provide the largest spread between the lowest and highest income quartiles in both utilities. For the progressive fixed fee, this result indicates that such a charge is successful in its goal of increasing the progressivity of water expenditures. For IBRs, this result indicates that wealthier, high-consuming users face higher marginal prices on more units of consumption than they did under BBRs.

Focusing on average bills alone can mask important heterogeneity in how each rate structure redistributes revenue. In Figure 6, we calculate the percentage change in the proportion of revenue raised from each quartile of the income distribution as a result of switching from BBRs to each of the three alternatives. Results show that in Coastal, a uniform rate would shift the burden toward less wealthy households, but in Inland, the uniform rate performs quite similarly to the existing BBRs. Redistributing income through the fixed fee appears to be quite effective, as evidenced by the fact that substantially less revenue is raised from the bottom two income quartiles. In Coastal, the IBR structure shifts some of the burden onto households in the highest income quartile. However, this same result does not hold in Inland. This divergence could partially be driven by the fact that larger, wealthier homes with larger outdoor water budgets face higher prices sooner under the IBR than they did under BBRs, and adjust consumption downwards accordingly. Additionally, recall from Table 2 that demand is relatively more elastic for Inland households relative to Coastal, which allows for greater flexibility in response to increased prices.[28]

6.4 Discussion

Our distributional analysis raises several points about the equity properties of combining surcharges with BBRs. First, BBRs appear more progressive than uniform rates only in certain settings. If a utility does not wish to prioritize equity through the rate structure directly, then it might be preferable to employ uniform rates, which avoid complexities with nonlinear rates that are difficult to communicate to households (Kahn and Wolak, 2013; Brent and Ward, 2019; Shaffer, 2020). Such rates also require much less information to be collected by the utility about household size, lot size, and other factors that go into calculating water budgets. However, BBRs do have one distinct equity advantage over uniform rates in that lower-consumption tiers can be subsidized using local tax revenues and other fees. Both utilities we study here subsidize consumption in the first two (under-budget) tiers by using revenues from property taxes to lower marginal prices below the cost of supplying water in those tiers. Using property tax revenues driven by wealthier homes to lower water costs for the entire service area improves the progressivity of BBRs relative to uniform rates. That said, subsidizing volumetric rates for water use is known to generate allocative inefficiencies by setting incorrect incentives for consumption, particularly when lump-sum transfers can be used to redistribute costs (Levinson and Silva, 2022; Wichman, 2024).

If a utility does seek to incorporate equity concerns into the rate structure, redistributing income through the fixed service charge can result in a relatively more progressive distribution of bills with far lower information costs for the utility. By combining a single marginal price that reflects the cost of supply with an income-varying service charge, such rates embed attractive efficiency and equity properties (Burger et al., 2020; Levinson and Silva, 2022; Wichman, 2024). However, political constraints may make individualized service charges directly tied to income difficult to implement in practice, as evidenced by California's current efforts to implement such charges for electricity.[29] If such rates are politically infeasible, IBRs present another option.

[28]We present results from two additional scenarios in Appendix A. In Figure A.9 we report revenue changes under a no price response scenario, where households are not allowed to adjust consumption in response to the new counterfactual prices. Such a scenario enforces revenue neutrality across all counterfactual scenarios at the expense of abstracting away insights gained from consumer price responsiveness. One change is that IBRs hit the highest income quartiles harder, as these high-use households are unable to adjust consumption downwards in response to facing higher prices sooner than they do under BBRs. Figure A.10 incorporates a heterogenous price response by using the income-varying average price elasticities from Table 5. The results here are largely similar to those in Figure 6.

[29]The California Public Utilities Commission is moving forward with efforts to implement an

Our results suggest IBRs can achieve progressivity gains relative to BBRs, and utilities could potentially also subsidize consumption in the lower tiers with property taxes in the same way that they currently do with BBRs.[30]

Equity considerations are also different during drought than during times of abundance. When entering into an extended drought period, utilities must assess not only how to induce permanent conservation, but also how to temporarily curb excessive or wasteful uses of water while maintaining base levels of indoor water use. By assigning each household an individualized water budget, BBRs have the ability (unlike the other rate structures considered here) to transmit individualized information to each household about what consumption the utility would ultimately consider "wasteful." Such an approach may be a more effective option to curb excess water demand than other approaches such as mandatory water rationing.[31]

Considering the sum of the evidence, combining drought surcharges with nonlinear BBRs possesses both positive and negative equity properties. Our analysis has noted several issues with BBRs, most notably that by tying prices to a budget, higher prices are less likely to bind for large users with higher budgets, counteracting the conservation signal that utilities intend to communicate. Because household income is positively correlated with the inputs to the budget formula, BBRs embed an implicit transfer from low-income to high-income households. At the same time, the use of local property taxes to subsidize lower consumption tiers works to reverse this effect by embedding transfers from high-income to low-income households, with the net effect of these competing transfers ambiguous. Ultimately, whether the water budgets themselves effectively transmit information about scarcity and serve as a nonprice conservation tool is a key question that we are unable to address directly, as we lack sufficient pre-BBR data.[32] Knowledge of the efficacy of the budgets themselves (along with knowledge of the utility's ultimate objective function when setting rates) is needed to definitively claim that BBRs are useful tools to achieve conservation and equity goals simultaneously.

income-based fixed fee for electricity consumption, first proposed by Borenstein et al. (2021) and subsequently mandated by law in 2022. These efforts have generated significant political backlash and face potential repeal efforts. Sources: `https://www.utilitydive.com/news/california-lawmakers-backpedal-on-income-based-utility-charges-as-ious-oth/707859/`. & `https://energyathaas.wordpress.com/2024/05/13/reality-checking-californias-income-graduated-fixed-charge/` (accessed January 23, 2025).

[30] Proposition 218 presents another constraint on the ability of municipal utilities in California to incorporate equity concerns into the ratemaking process. The restrictions placed on water utilities by Proposition 218 may make it exceedingly difficult to experiment with alternative rate structures. Considering this constraint, in Appendix D, we discuss a final set of counterfactual results (presented in Figure A.11 and Figure A.12) in which we assume that the utilities are restricted to keeping their BBRs intact and can only implement changes to the budget formula itself.

[31] One of the utilities in this study enforced mandatory rationing days during an earlier California drought in 2008-2009. Internal data showed no significant overall conservation due to a rebound effect where water use increased on non-rationing days, and the utility faced widespread customer backlash.

[32] Coastal used an IBR structure before switching to BBRs, and Inland previously used a uniform rate. Pérez-Urdiales and Baerenklau (2019) provide early evidence that budgets can serve as an effective information signal to high users when switching from uniform rates.

7 Conclusion

In this paper, we study the introduction of drought surcharges layered within nonlinear BBRs as a tool for urban water demand management. Our demand estimation indicates that consumers do conserve water in response to surcharge–driven price increases despite their temporary nature. However, further investigation reveals that surcharges alone cannot explain the majority of the conservation we observe. Although utilities often seek to combine price and nonprice conservation approaches, for drought surcharges to sufficiently signal scarcity, they must bind for a significant portion of households. BBRs undercut the effectiveness of surcharges by shielding high users with large lawns from facing higher prices. Our comparison of hypothetical rate structures suggests that BBRs do not clearly dominate other rate structures along equity dimensions, although we cannot definitively conclude that BBRs are equity dominated.

Climate change will continue to exacerbate water scarcity moving forward, making the need to effectively conserve water during droughts increasingly important. Our results stress the need for policymakers to consider the role that nonprice policies play in inducing conservation, as surcharges alone are not enough to explain the demand response observed in the data. Whether the budgets themselves effectively serve as a nonprice conservation tool is another understudied question that future research should address. When turning to price-based policies, it is vital that they send an appropriate price signal that accurately reflects the scarcity value of water. Assigning high marginal prices, but then allocating large quantities of cheap water to households with large lawns through water budgets muddies this price signal and undercuts the effectiveness of surcharge pricing. Ultimately, utilities concerned with balancing conservation and equity concerns during drought should consider carefully how surcharges interact with existing policies, such as water budgets, before adoption.

References

Allaire, Maura and Ariel Dinar, "What drives water utility selection of pricing methods? Evidence from California," *Water Resources Management*, 2022, pp. 1–17.

Baerenklau, Kenneth A and María Pérez-Urdiales, "Can Allocation-Based Water Rates Promote Conservation and Increase Welfare? A California Case Study," *Water Economics and Policy*, 2019, *5* (02), 1850014.

Baerenklau, Kenneth A., Kurt A. Schwabe, and Ariel Dinar, "The residential water demand effect of increasing block rate water budgets," *Land Economics*, 2014, *90* (4), 683–699.

Bartik, Timothy J, "Who benefits from state and local economic development policies?," 1991, *WE Upjohn Institute for Employment Research.*

Bonbright, James C, *Principles of public utility rates*, Columbia University Press, 1961.

Borenstein, Severin, "The redistributional impact of nonlinear electricity pricing," *American Economic Journal: Economic Policy*, 2012, *4* (3), 56–90.

_ **and Lucas Davis**, "The equity and efficiency of two-part tariffs in US natural gas markets," *The Journal of Law and Economics*, 2012, *55* (1), 75–128.

_ **, Meredith Fowlie, and James Sallee**, "Designing electricity rates for an equitable energy transition," *Energy Institute at Haas Working Paper*, 2021, *314.*

Brent, Daniel A. and Casey J. Wichman, "Do Behavioral nudges interact with prevailing economic incentives? Pairing experimental and quasi-experimental evidence from water consumption," *RFF Working Paper*, 2022.

— **and Michael B. Ward**, "Price perceptions in water demand," *Journal of Environmental Economics and Management*, 2019, *98*, 102266.

— **, Corey Lott, Michael Taylor, Joseph Cook, Kimberly Rollins, and Shawn Stoddard**, "What causes heterogeneous responses to social comparison messages for water conservation?," *Environmental and Resource Economics*, 2020, *77*, 503–537.

— **, Joseph Cook, and Skylar Olsen**, "Social comparisons, household water use and participation in utility conservation programs: Evidence from three randomized trials," *Journal of the Association of Environmental and Resource Economists*, 2015, *2* (4), 597–627.

Brown, Stephen J and David Sumner Sibley, *The theory of public utility pricing*, Cambridge University Press, 1986.

Browne, Oliver R, Ludovica Gazze, and Michael Greenstone, "Do conservation policies work? Evidence from residential water use," *Environmental and Energy Policy and the Economy*, 2021, *2* (1), 190–225.

— **, — , — , and Olga Rostapshova**, "Man vs. machine: Technological promise and political limits of automated regulation enforcement," *The Review of Economics and Statistics*, 2023, pp. 1–36.

Buck, Steven, Maximilian Auffhammer, Stephen Hamilton, and David Sunding, "Measuring welfare losses from urban water supply disruptions," *Journal of the Association of Environmental and Resource Economists*, 2016, *3* (3), 743–778.

Burger, Scott P, Christopher R Knittel, Ignacio J Pérez-Arriaga, Ian Schneider, and Frederik Vom Scheidt, "The efficiency and distributional effects of alternative residential electricity rate designs," *The Energy Journal*, 2020, *41* (1).

Burlig, Fiona, Christopher Knittel, David Rapson, Mar Reguant, and Catherine Wolfram, "Machine learning from schools about energy efficiency," *Journal of the Association of Environmental and Resource Economists*, 2020, *7* (6), 1181–1217.

Cardoso, Diego S. and Casey J. Wichman, "Water affordability in the United States," *Water Resources Research*, 2022, *58* (12).

CIMIS, "CIMIS Daily Station Data," `https://cimis.water.ca.gov/Resources.aspx` 2018.

Coase, Ronald H., "The Marginal Cost Controversy," *Economica*, 1946, *13* (51), 169–182.

Cook, Joseph and Daniel Brent, "Do Households Respond to the Marginal or Average Price of Piped Water Services?," in "Oxford Research Encyclopedia of Global Public Health" 2021.

Correia, Sergio, "IVREGHDFE: Stata module for extended instrumental variable regressions with multiple levels of fixed effects," 2018.

Dalhuisen, Jasper M, Raymond JGM Florax, Henri LF De Groot, and Peter Nijkamp, "Price and income elasticities of residential water demand: a meta-analysis," *Land Economics*, 2003, *79* (2), 292–308.

Deryugina, Tatyana, Don Fullerton, and William A Pizer, "An introduction to energy policy trade-offs between economic efficiency and distributional equity," *Journal of the Association of Environmental and Resource Economists*, 2019, *6* (S1), S1–S6.

El-Khattabi, Ahmed Rachid, "Social opprobrium and compliance: Evidence from water conservation," *Water Resources and Economics*, 2023, *42*, 100218.

— **, Shadi Eskaf, Julien P Isnard, Laurence Lin, Brian McManus, and Andrew J Yates**, "Heterogeneous responses to price: Evidence from residential water consumers," *Journal of Environmental Economics and Management*, 2021, *107*, 102430.

Espey, M., J. Espey, and W. D. Shaw, "Price elasticity of residential demand for water: A meta-analysis," *Water Resources Research*, 1997, *33* (6), 1369–1374.

Ferraro, Paul J and Michael K Price, "Using nonpecuniary strategies to influence behavior: Evidence from a large-scale field experiment," *Review of Economics and Statistics*, 2013, *95* (1), 64–73.

Fuente, David, Jane Kabubo-Mariara, Peter Kimuyu, Mbutu Mwaura, and Dale Whittington, "Assessing the performance of water and sanitation tariffs: The case of Nairobi, Kenya," *Water Resources Research*, 2021, *57* (9), e2019WR025791.

Goldsmith-Pinkham, Paul, Isaac Sorkin, and Henry Swift, "Bartik instruments: What, when, why, and how," *American Economic Review*, 2020, *110* (8), 2586–2624.

Gottlieb, Manuel, "Urban domestic demand for water: A Kansas case study," *Land Economics*, 1963, *39* (2), 204–210.

Hanemann, W. Michael, "Price and rate structures," in Duane Baumann, John Boland, and W. Michael Hanemann, eds., *Urban Water Demand Management and Planning*, number Ch. 5 McGraw-Hill New York 1997.

Hastie, Trevor, Robert Tibshirani, Jerome H Friedman, and Jerome H Friedman, *The elements of statistical learning: data mining, inference, and prediction*, Vol. 2, Springer New York, 2009.

Hotelling, Harold, "The general welfare in relation to problems of taxation and of railway and utility rates," *Econometrica: Journal of the Econometric Society*, 1938, pp. 242–269.

Howe, Charles W and F Pierce Linaweaver Jr, "The impact of price on residential water demand and its relation to system design and price structure," *Water Resources Research*, 1967, *3* (1), 13–32.

Ito, Koichiro, "Do consumers respond to marginal or average price? Evidence from nonlinear electricity pricing," *American Economic Review*, 2014, *104* (2), 537–63.

_ **and Shuang Zhang**, "Do Consumers Distinguish Fixed Cost from Variable Cost? "Schmeduling" in Two-Part Tariffs in Energy," Technical Report, National Bureau of Economic Research 2023.

Jessoe, Katrina, Gabriel E Lade, Frank Loge, and Edward Spang, "Residential water conservation during drought: Experimental evidence from three behavioral interventions," *Journal of Environmental Economics and Management*, 2021, *110*, 102519.

Kahn, Alfred E, *The economics of regulation: principles and institutions*, Vol. 1, MIT press, 1988.

Kahn, Matthew E. and Frank A. Wolak, "Using Information to Improve the Effectiveness of Nonlinear Pricing," *Working Paper*, 2013.

Kleibergen, Frank and Richard Paap, "Generalized reduced rank tests using the singular value decomposition," *Journal of Econometrics*, 2006, *133* (1), 97–126.

Levinson, Arik and Emilson Silva, "The electric gini: income redistribution through energy prices," *American Economic Journal: Economic Policy*, 2022, *14* (2), 341–65.

Li, Li and Marc Jeuland, "Household water savings and response to dynamic incentives under nonlinear pricing," *Journal of Environmental Economics and Management*, 2023, *119*, 102811.

Mansur, Erin T and Sheila M Olmstead, "The value of scarce water: Measuring the inefficiency of municipal regulations," *Journal of Urban Economics*, 2012, *71* (3), 332–346.

Mayer, Peter, William Deoreo, Thomas Chesnutt, and Lyle Summers, "Water budgets and rate structures: Innovative management tools," *Journal - American Water Works Association*, 2008, *100* (5), 117–131.

Mount, Jeffrey, Ellen Hanak, and Caitlin Peterson, "Water Use in California," *Public Policy Institute of California Fact Sheet*, 2023, *San Francisco.*

Nataraj, Shanthi and W. Michael Hanemann, "Does marginal price matter? A regression discontinuity approach to estimating water demand," *Journal of Environmental Economics and Management*, 2011, *61* (2), 198 – 212.

NOAA, "Heavy Precipitation Events, California and Northern Nevada, January and February 2017," https://www.cnrfc.noaa.gov/storm_summaries/janfeb2017storms.php 2017.

— , "National Integrated Drought Information System (NIDIS): California," https://www.drought.gov/states/california#historical-conditions 2023.

Olmstead, Sheila M., "Reduced-form versus structural models of water demand under nonlinear prices," *Journal of Business & Economic Statistics*, 2009, *27* (1), 84–94.

— , "The economics of managing scarce water resources," *Review of Environmental Economics and Policy*, 2010, *4* (2), 179–198.

— **, W. Michael Hanemann, and Robert N. Stavins**, "Water demand under alternative price structures," *Journal of Environmental Economics and Management*, 2007, *54* (2), 181–198.

Pérez-Urdiales, María and Kenneth A Baerenklau, "Learning to live within your (water) budget: Evidence from allocation-based rates," *Resource and Energy Economics*, 2019, *57*, 205–221.

Picard, Robert, "GEONEAR: Stata module to find nearest neighbors using geodetic distances," 2012.

Pint, Ellen M, "Household responses to increased water rates during the California drought," *Land Economics*, 1999, pp. 246–266.

Pratt, Bryan, "A fine is more than a price: Evidence from drought restrictions," *Journal of Environmental Economics and Management*, 2023, *119*, 102809.

Prest, Brian C., Casey J. Wichman, and Karen Palmer, "RCTs Against the Machine: Can Machine Learning Prediction Methods Recover Experimental Treatment Effects?," *Journal of the Association of Environmental and Resource Economists*, 2023, *10* (5), 1231–1264.

Ramsey, Frank P, "A Contribution to the Theory of Taxation," *The Economic Journal*, 1927, *37* (145), 47–61.

Randriamaro, Mary T. and Joseph Cook, "Income Redistribution, Water Scarcity, and Water Rates in the 225 Largest US Cities," *Working Paper*, 2024.

Renwick, Mary E. and Richard D. Green, "Do residential water demand side management policies measure up? An analysis of eight California water agencies," *Journal of Environmental Economics and Management*, 2000, *40* (1), 37–55.

— **and Sandra O. Archibald**, "Demand Side Management Policies for Residential Water Use: Who Bears the Conservation Burden?," *Land Economics*, 1998, *74* (3), 343–359.

Renzetti, Steven, "Evaluating the welfare effects of reforming municipal water prices," *Journal of Environmental Economics and Management*, 1992, *22* (2), 147–163.

— , "Municipal water supply and sewage treatment: costs, prices, and distortions," *Canadian Journal of Economics*, 1999, *32* (3), 688–704.

Saez, Emmanuel, "Do taxpayers bunch at kink points?," *American Economic Journal: Economic Policy*, 2010, *2* (3), 180–212.

Sallee, James M, "Rational inattention and energy efficiency," *The Journal of Law and Economics*, 2014, *57* (3), 781–820.

Schlenker, Wolfram and Michael J. Roberts, "Nonlinear temperature effects indicate severe damages to U.S. crop yields under climate change," *Proceedings of the National Academy of Sciences*, 2009, *106* (37), 15594–15598.

Schonlau, Matthias and Rosie Yuyan Zou, "The random forest algorithm for statistical learning," *The Stata Journal*, 2020, *20* (1), 3–29.

Sears, James, "Culpable Consumption: Residential Response to Price and Non-Price Water Conservation Measures," *Working Paper*, 2021.

— , "Fluid demand responses: Long-and short-run urban water price elasticities," *Working Paper*, 2023.

Sebri, Maamar, "A meta-analysis of residential water demand studies," *Environment, Development and Sustainability*, 2014, *16*, 499–520.

Shaffer, Blake, "Misunderstanding nonlinear prices: Evidence from a natural experiment on residential electricity demand," *American Economic Journal: Economic Policy*, 2020, *12* (3), 433–461.

Smith, Steven M, "The effects of individualized water rates on use and equity," *Journal of Environmental Economics and Management*, 2022, *114*, 102673.

Timmins, Christopher, "Does the median voter consume too much water? Analyzing the redistributive role of residential water bills," *National Tax Journal*, 2002, *55* (4), 687–702.

— , "Measuring the dynamic efficiency costs of regulators' preferences: Municipal water utilities in the arid west," *Econometrica*, 2002, *70* (2), 603–629.

Tull, Christopher, *RateParser: Calculate Water Bills from an OWRS File* 2016. R package version 0.1.0.

US Census Bureau, "American Community Survey 5-year Estimates," `https://www.census.gov/data/developers/data-sets/acs-5year.2015.html#list-tab-1036221584` 2015.

West, Jeremy, Robert W Fairlie, Bryan Pratt, and Liam Rose, "Automated enforcement of irrigation regulations and social pressure for water conservation," *Journal of the Association of Environmental and Resource Economists*, 2021, *8* (6), 1179–1207.

Whittington, Dale and Céline Nauges, "An assessment of the widespread use of increasing block tariffs in the municipal water supply sector," in "Oxford research encyclopedia of global public health" 2020.

Wichman, Casey J., "Perceived price in residential water demand: Evidence from a natural experiment," *Journal of Economic Behavior & Organization*, 2014, *107*, 308–323.

— , "Information provision and consumer behavior: A natural experiment in billing frequency," *Journal of Public Economics*, 2017, *152*, 13–33.

— , "The unequal burdens of water scarcity," *Nature Water*, 2023, *1* (1), 26–27.

— , "Efficiency, Equity, and Cost-Recovery Trade-Offs in Municipal Water Pricing," Technical Report, Resources for the Future 2024.

— **, Laura O. Taylor, and Roger H. von Haefen**, "Conservation policies: Who responds to price and who responds to prescription?," *Journal of Environmental Economics and Management*, 2016, *79*, 114–134.

Williams, A. Park, Benjamin I. Cook, and Jason E. Smerdon, "Rapid intensification of the emerging southwestern North American megadrought in 2020–2021," *Nature Climate Change*, 2022, *12* (3), 232–234.

Yoo, James, Silvio Simonit, Ann P Kinzig, and Charles Perrings, "Estimating the price elasticity of residential water demand: the case of Phoenix, Arizona," *Applied Economic Perspectives and Policy*, 2014, *36* (2), 333–350.

Young, Robert A, "Price elasticity of demand for municipal water: A case study of Tucson, Arizona," *Water Resources Research*, 1973, *9* (4), 1068–1072.

Online Appendix – Not For Publication

A Additional Results

A.1 Additional Figures

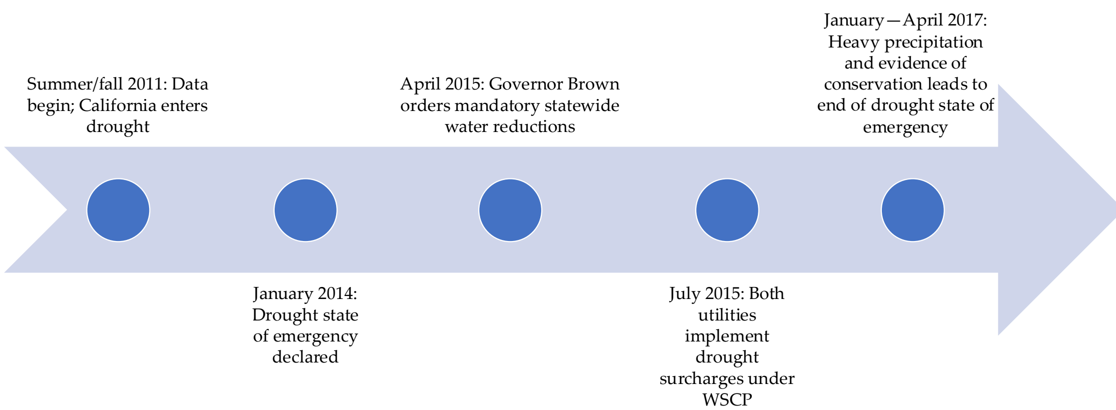
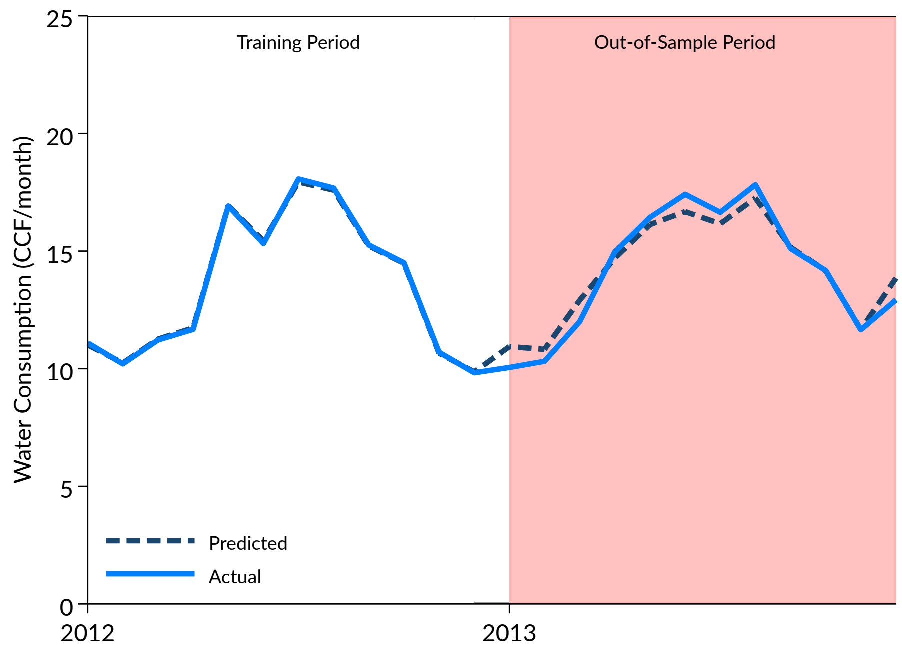
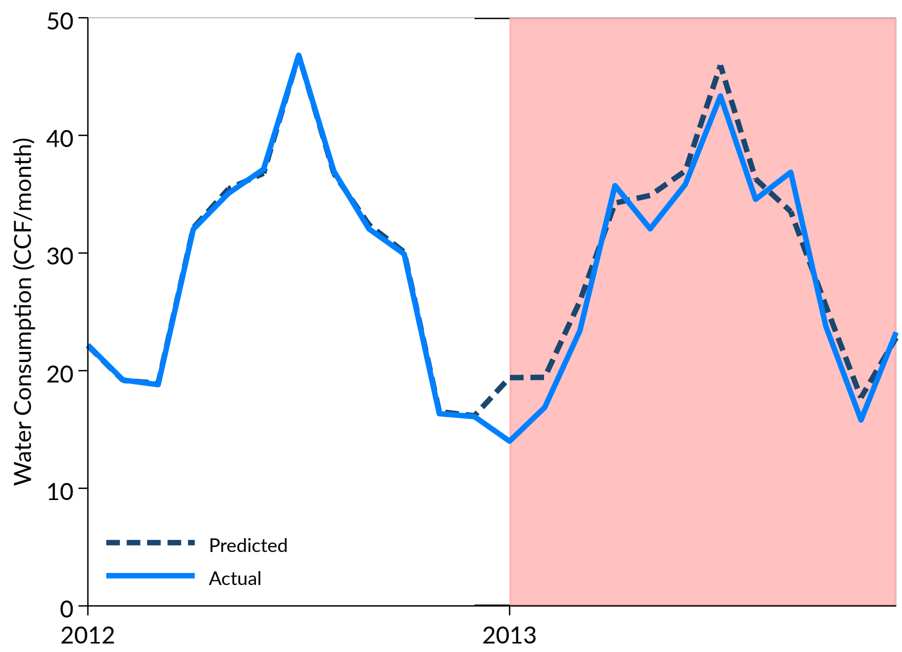


Figure A.1: Timeline of 2011–2017 California Drought

Notes: The figure presents a visual timeline of the important events surrounding the California drought of 2011–2017 and how they relate to the billing data used in the analysis.



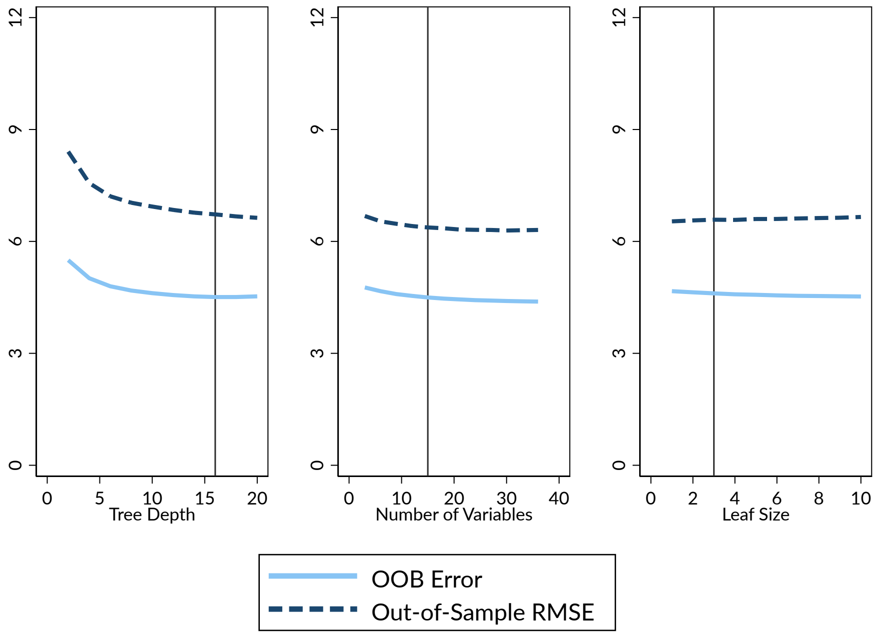
(a) **Coastal**



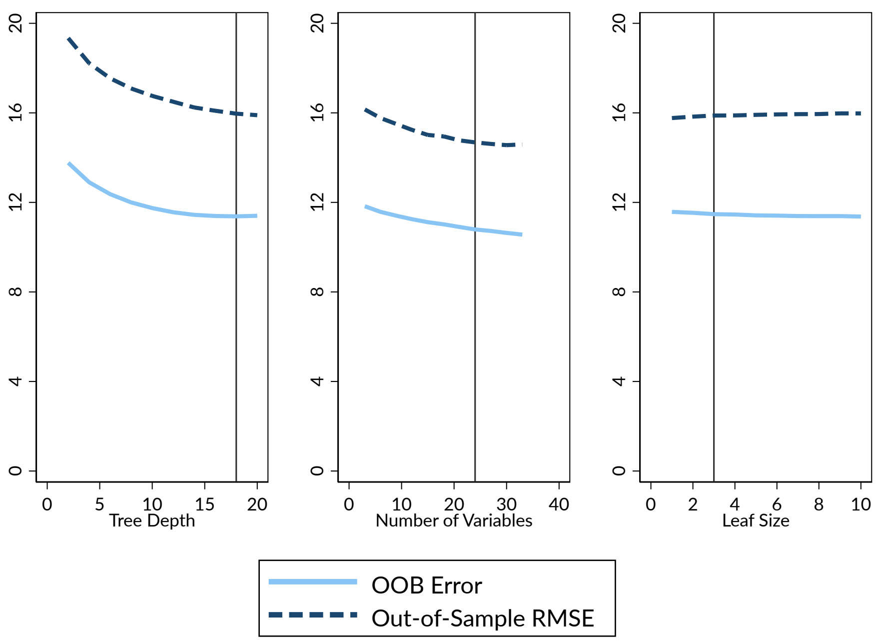
(b) **Inland**

Figure A.2: Predicted and Actual Consumption Over Time, Diagnostic Predictions

Notes: The figure presents the time series of average actual and predicted consumption in each month-of-sample for each utility separately, with actual consumption represented by the blue solid line and predicted consumption represented by the navy dashed line. Monthly averages are plotted for the diagnostic exercise in which we use 2012 data only to predict entirely out-of-sample in 2013.



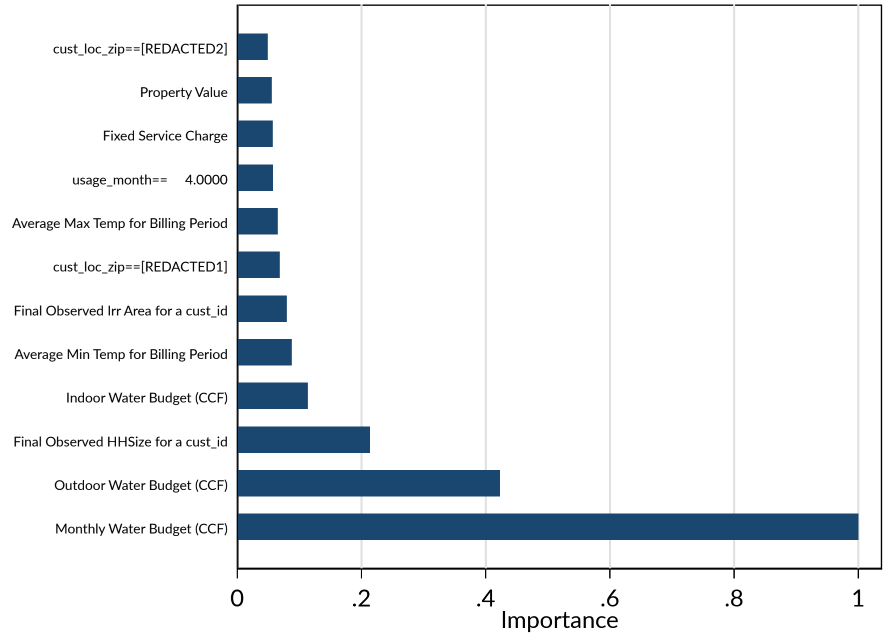
(a) **Coastal**



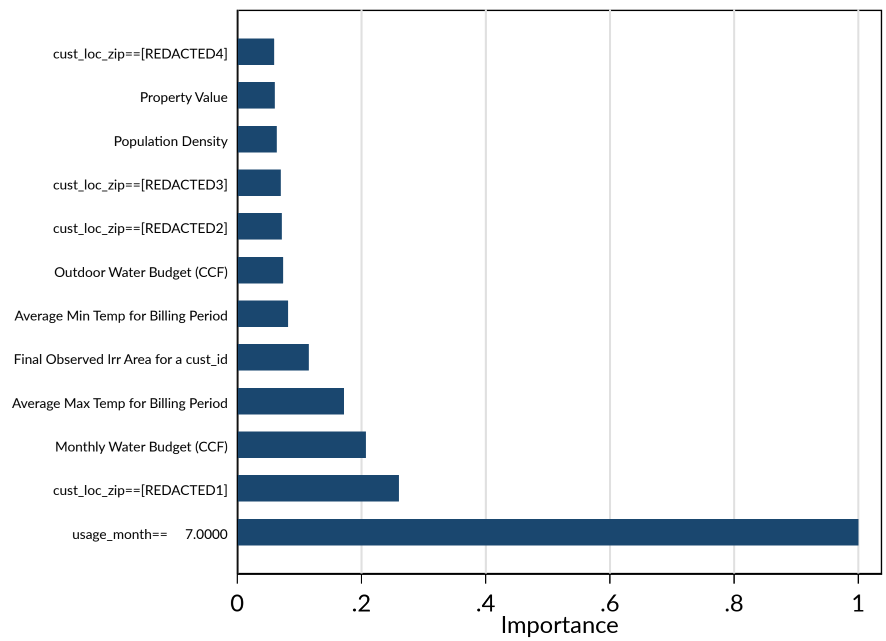
(b) **Inland**

Figure A.3: Random Forest Parameter Tuning

Notes: The figure presents results from our parameter tuning exercise in which we re-estimate predictions and errors over a range of discrete values. We repeat this exercise for three random forest tuning parameters, separately for each utility: tree depth, number of candidate predictor variables made available to the random forest algorithm, and minimum leaf size. Light blue solid lines plot OOB error rates, and dashed navy lines plot out-of-sample RMSE values over the range of parameter values considered. The values we choose for use in our generation of our full set of predictions are represented by the black vertical lines in each panel.



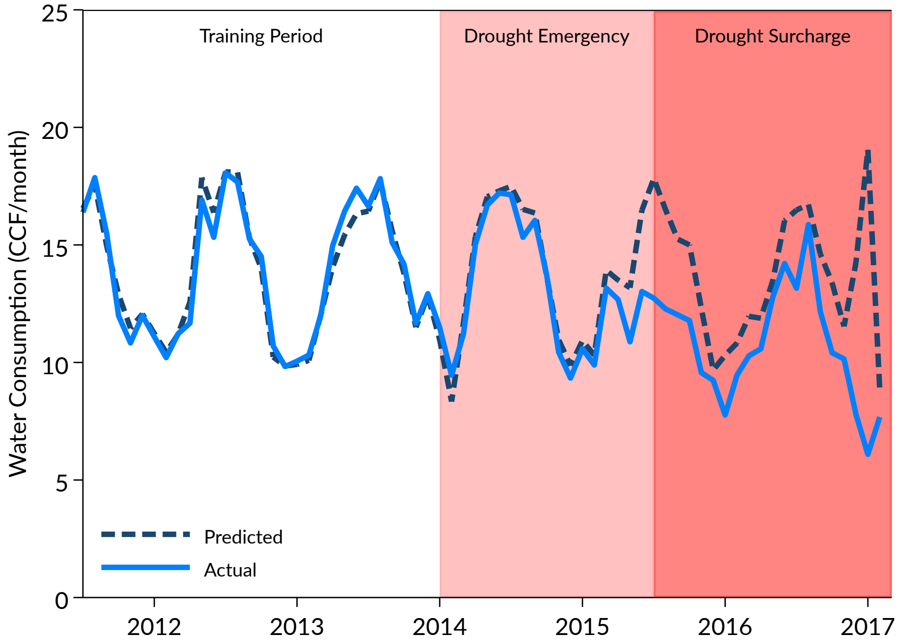
(a) **Coastal**



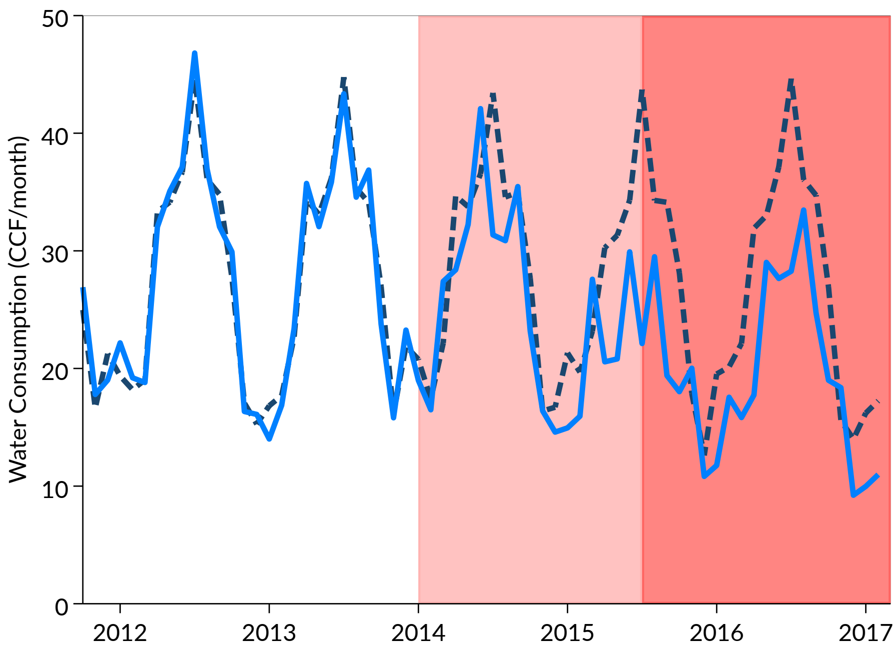
(b) **Inland**

Figure A.4: Random Forest Variable Importance Plots

Notes: The figure presents standard random forest variable importance plots for each utility separately. The top 12 most influential predictors are presented on a rescaled measure [0, 1], with 1 being the most influential and 0 being the least influential.



(a) **Coastal**



(b) **Inland**

Figure A.5: Predicted and Actual Consumption Over Time, Panel Fixed Effect Predictions

Notes: The figure presents the time series of average actual and predicted consumption in each month-of-sample for each utility separately. The predictions here are generated using a panel fixed effects specification with weather covariates and household-by-month-of-sample fixed effects. The training period data used to estimate the model up to December 2013 is unshaded. The period in which the drought emergency had been declared but drought surcharges were not yet in effect is shaded in pink (January 2014 to June 2015). The period in which drought surcharges were in effect is shaded in red (July 2015–February 2017). Actual consumption falling below predicted consumption indicates water conservation in the aggregate.

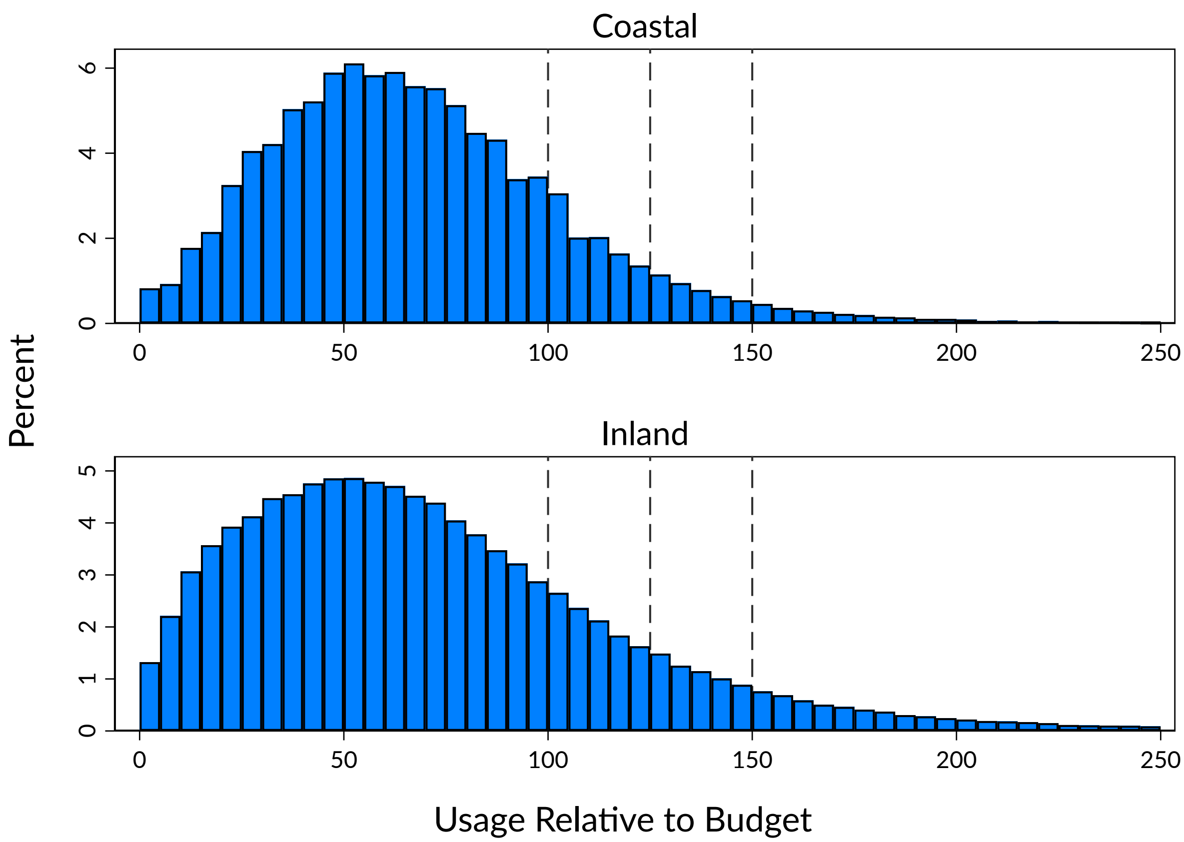


Figure A.6: Distribution of Usage Relative to Budget

Notes: The figure presents histograms that illustrate the distribution of usage relative to a household's water budget for household-months during the drought surcharge period. Dashed vertical lines show the relevant break points for the BBR tier thresholds.

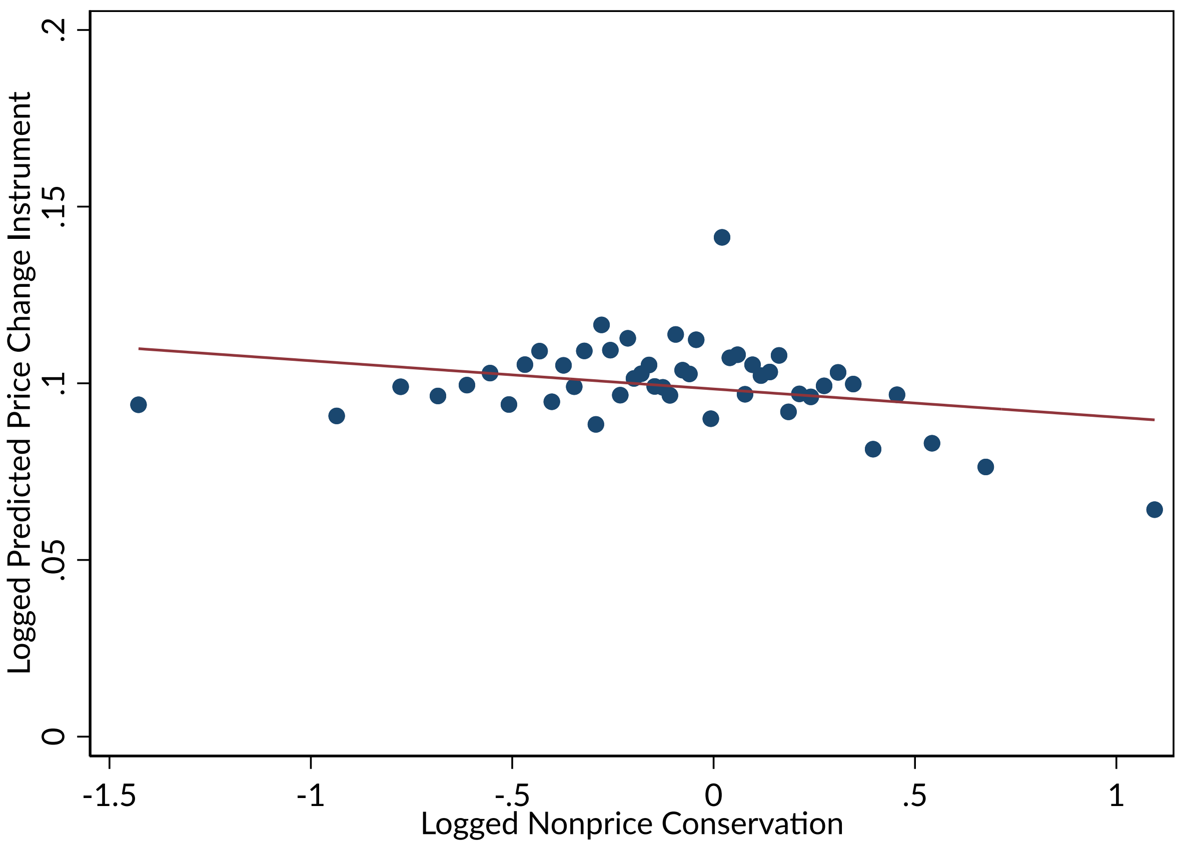


Figure A.7: Binscatter of Instrument against Nonprice Conservation Under Surcharge Pricing

Notes: The figure presents a binscatter of average values for the predicted logged price change instrument in each of 50 bins for the estimated nonprice conservation proxy variable for both utilities under surcharge pricing.

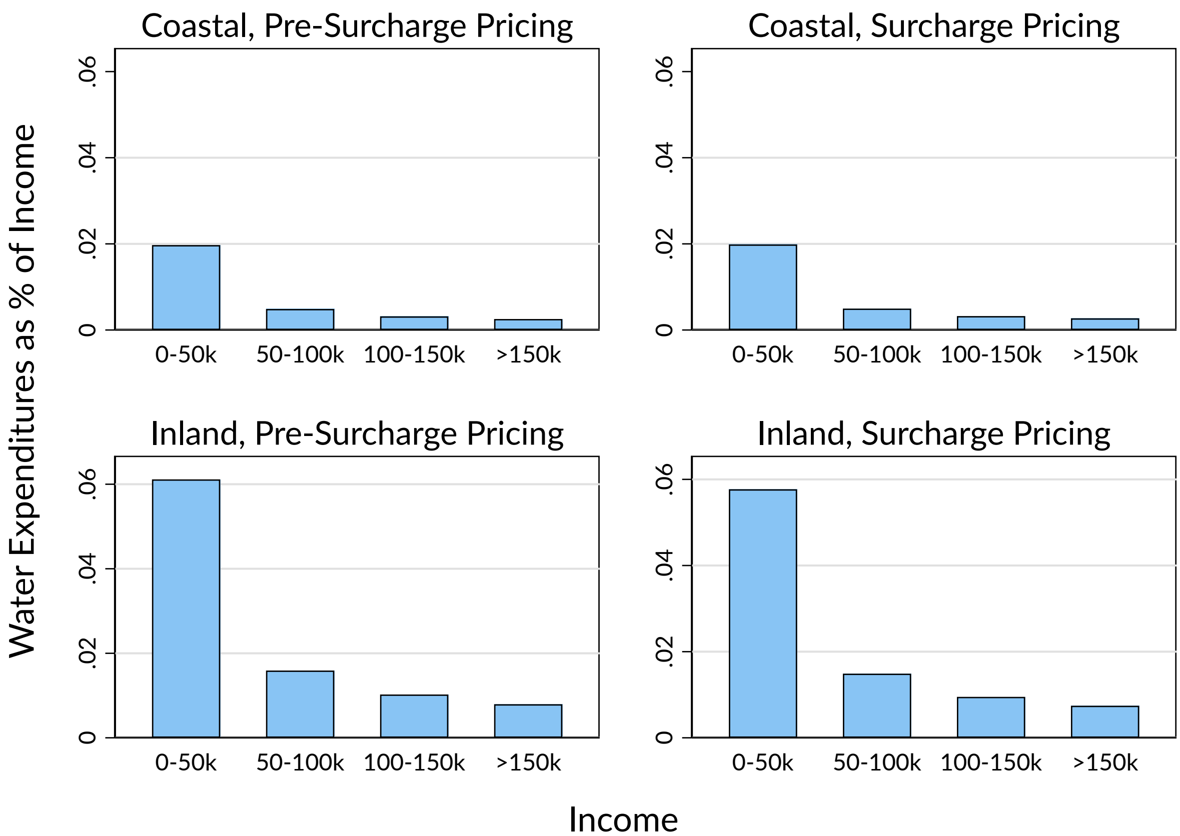


Figure A.8: Average Monthly Water Expenditures as Share of Income

Notes: The figure plots the percentage of monthly income that households allocate to water expenditures over discrete household income groups, calculated by taking average monthly bills and dividing by monthly income. This procedure is repeated separately for both Coastal and Inland, and separately during pre-surcharge pricing (2011–2013) and the drought surcharge pricing period (July 2015–December 2016).

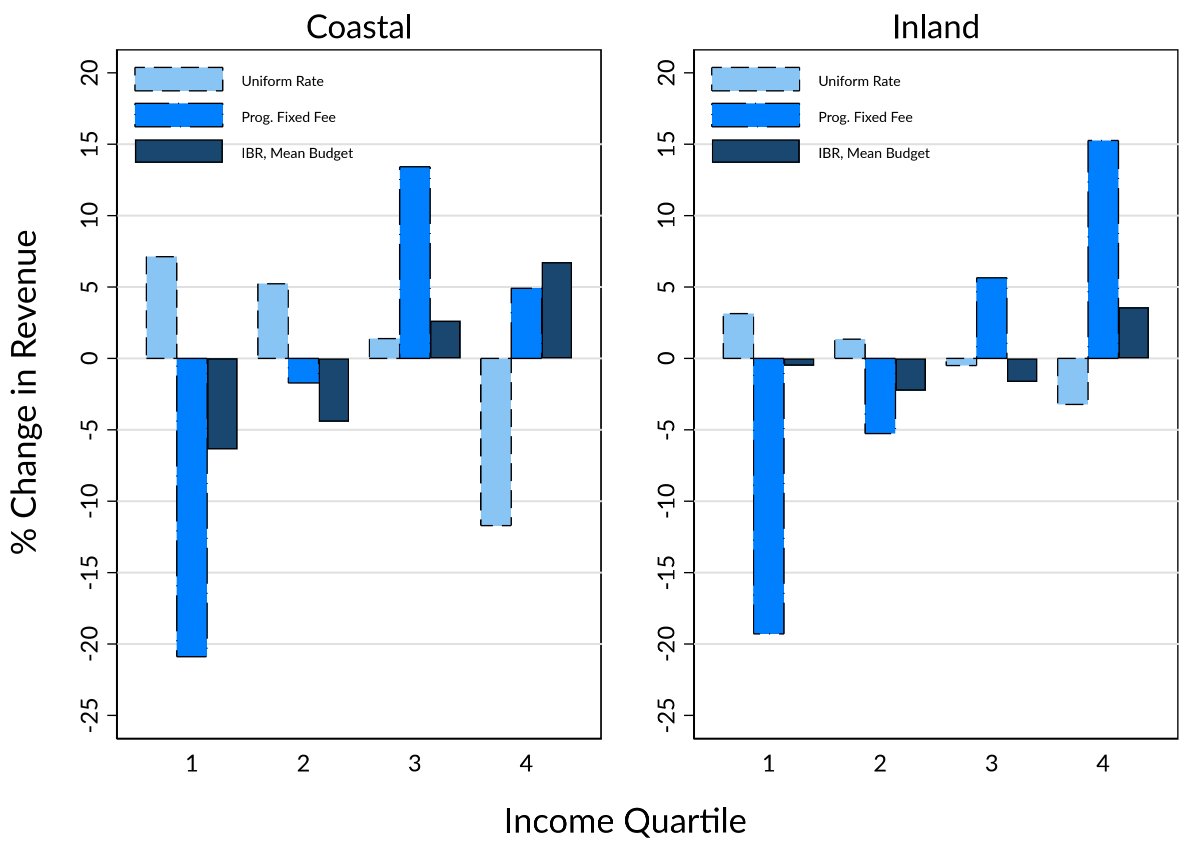


Figure A.9: Revenue Changes by Income Quartile, No Price Response

Notes: The figure presents percentage changes in the proportion of revenues raised from each quartile of the income distribution when moving from BBRs to each of three counterfactual rate structures, respectively. Negative values indicate that proportionally less revenue is raised from that quartile of homes under the alternative structure compared to a BBR, and positive values indicate that proportionally more revenue is raised from that quartile relative to BBRs. Results differ from Figure 6 in that households are not allowed to adjust consumption in response to the new counterfactual prices, ensuring revenue neutrality in the aggregate.

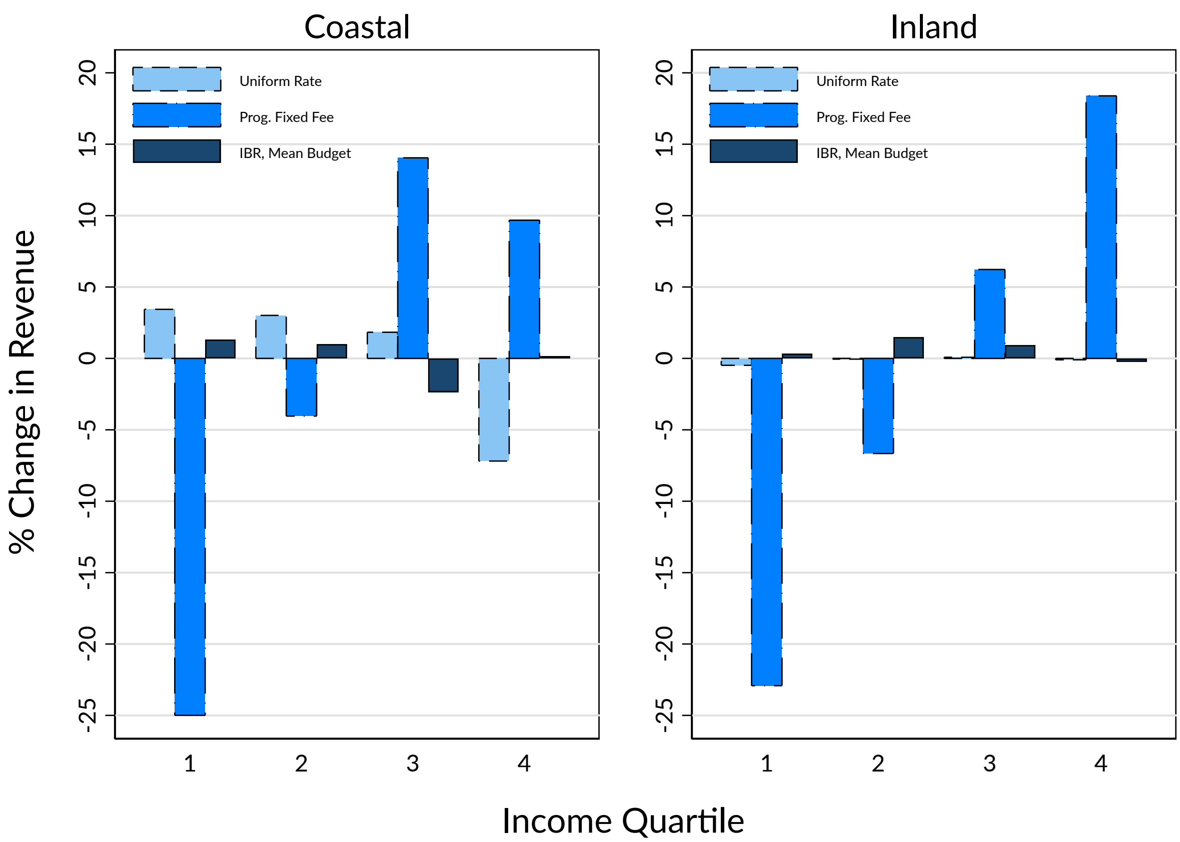


Figure A.10: Revenue Changes by Income Quartile, Heterogenous Price Response

Notes: The figure presents percentage changes in the proportion of revenues raised from each quartile of the income distribution when moving from BBRs to each of three counterfactual rate structures, respectively. Negative values indicate that proportionally less revenue is raised from that quartile of homes under the alternative structure compared to a BBR, and positive values indicate that proportionally more revenue is raised from that quartile relative to BBRs. Results differ from Figure 6 in that we allow for a heterogenous demand response along the income distribution using average price elasticities sourced from Table 5.

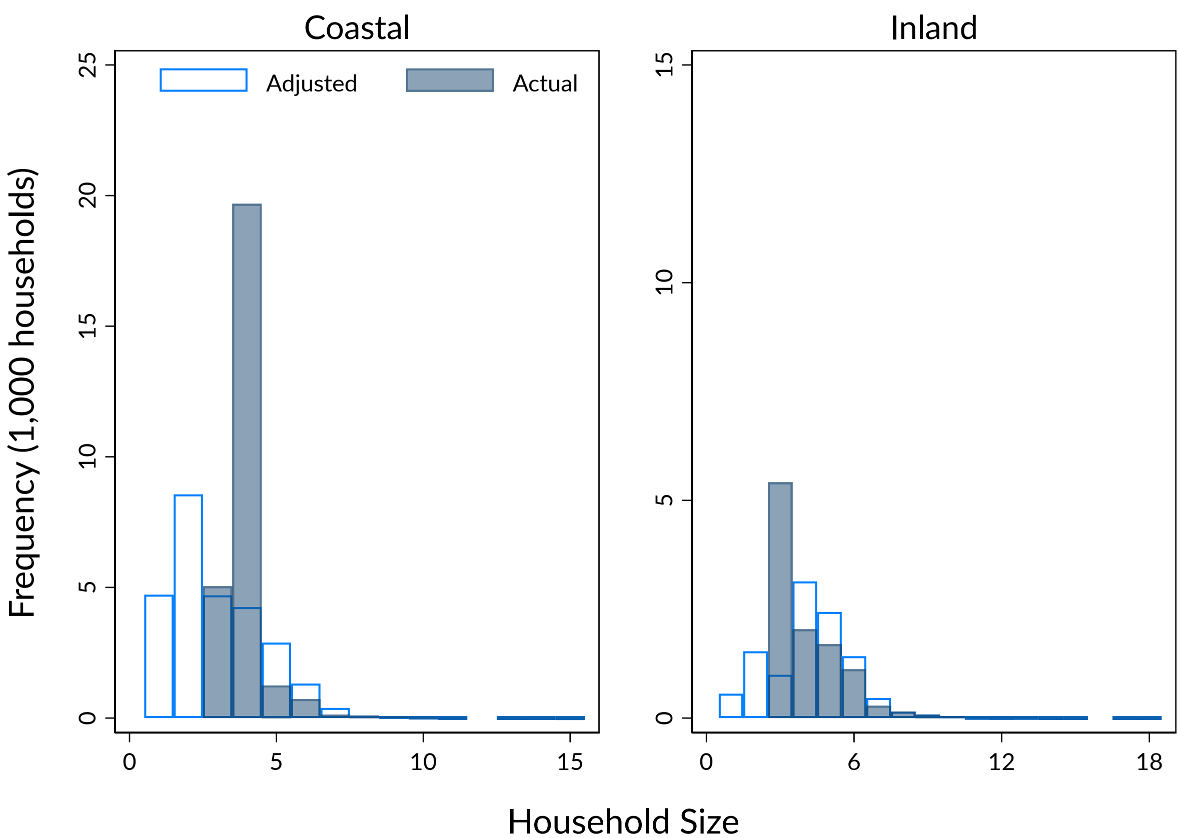


Figure A.11: Distribution of "Adjusted" Household Sizes

Notes: The figure presents histograms of the actual household sizes reported in the billing microdata and the "adjusted" distribution of household sizes after implementing our household size correction procedure with census data. The reported or actual household size distribution is represented by the solid bars, and the adjusted household size values are represented with the transparent outlined bars.

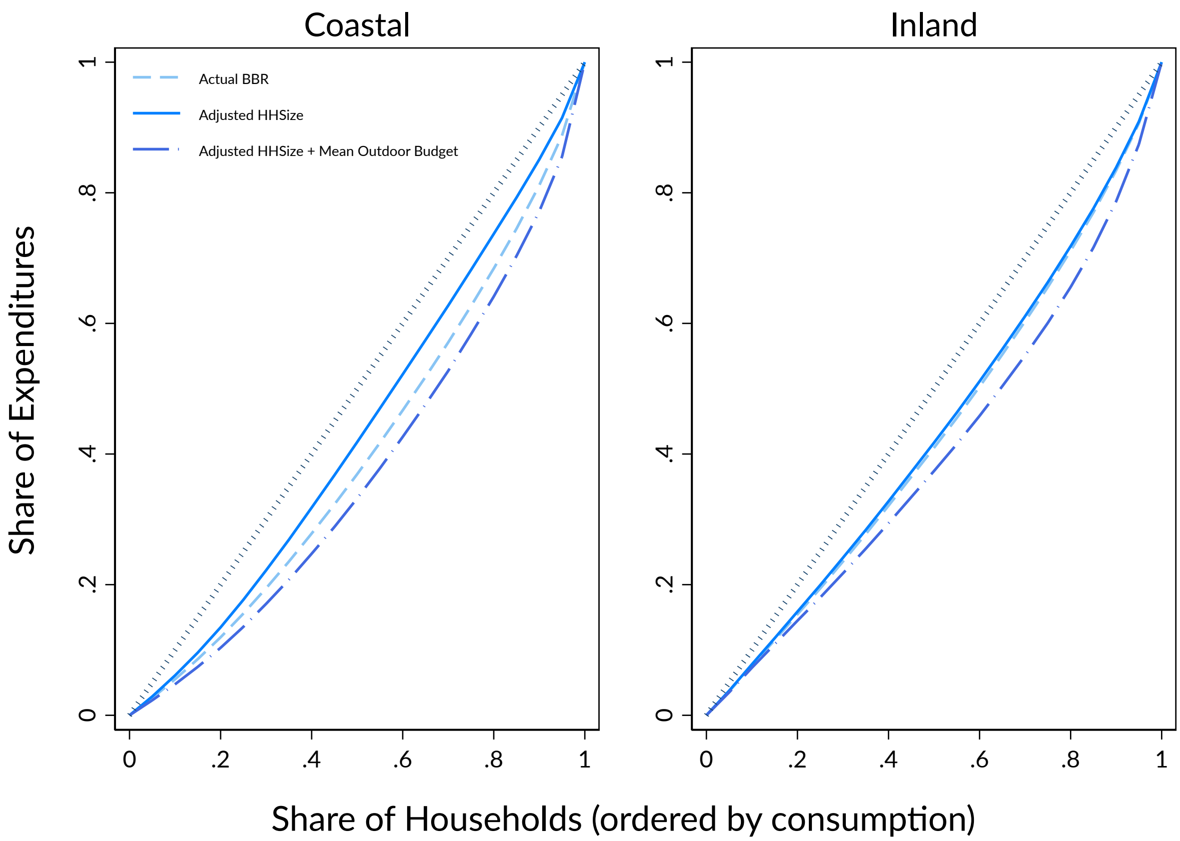


Figure A.12: Concentration Curves for Counterfactual Expenditure Shares

Notes: The figure presents Lorenz-style concentration curves indicating the share of water expenditures that accrue to each percentile of the household distribution ordered by consumption, separately for actual BBRs and the two counterfactual BBRs we develop. The time period included is the drought surcharge pricing period (July 2015–December 2016). The 45° diagonal is plotted in the dotted black line and represents perfect equality (i.e., the bottom x% of households pay x% of water expenditures).

A.2 Additional Tables

Table A.1: Validation Checks for Diagnostic Predictions

Coastal	Mean
Out-of-Bag Error	4.44
Out-of-Sample RMSE, RF	6.61
Out-of-Sample RMSE, OLS	7.31

Inland	Mean
Out-of-Bag Error	10.79
Out-of-Sample RMSE, RF	15.05
Out-of-Sample RMSE, OLS	17.61

Notes: The table presents errors for the diagnostic predictions using 2012-2013 data.

Table A.2: Base Demand Regressions, Not Instrumenting for Price

	Coastal		Inland	
	(1) AP	(2) MP	(3) AP	(4) MP
Δ log(AP)	0.71***		1.21***	
	(0.01)		(0.02)	
Δ log(MP)		0.16***		0.26***
		(0.00)		(0.01)
Observations	477,110	477,110	203,187	203,187
Households	26,988	26,988	10,840	10,840
Household FE	Y	Y	Y	Y
Month-of-Sample FE	Y	Y	Y	Y

Notes: The table presents estimates of $\hat{\beta}$ from estimating Equation 3 without instrumenting for observed price changes. The dependent variable is the difference between contemporaneous and baseline consumption, $\Delta log(q_{it})$. The time period included is from July 2015 to December 2016. All specifications include a vector of weather covariates, including evapotranspiration, precipitation, temperature, and their squares. Standard errors are clustered at the household level and are presented below coefficient estimates in parentheses. ***: $p <$ 0.01; **: $p < 0.05$; *: $p < 0.1$.

Table A.3: IV Demand Regressions, No Bootstrapping

	Coastal		Inland	
	(1) AP	(2) MP	(3) AP	(4) MP
Δ log(AP)	-0.44***		-1.07***	
	(0.03)		(0.06)	
Δ log(MP)		-0.22***		-0.48***
		(0.02)		(0.02)
Observations	477,110	477,110	203,187	203,187
Households	26,988	26,988	10,840	10,840
Household FE	Y	Y	Y	Y
Month-of-Sample FE	Y	Y	Y	Y
First-stage F-stat	2,737	2,199	4,214	5,688

Notes: The table presents estimates of $\hat{\beta}$ from estimating Equation 3. The dependent variable is the difference between contemporaneous and baseline consumption, $\Delta log(q_{it})$. Endogenous price differences are instrumented for in the first stage using $\Delta log(\hat{p}_{it})$. The time period included is from July 2015 to December 2016. Columns 1 and 3 instrument for average price differences, and Columns 2 and 4 instrument for marginal price differences. All specifications include a vector of weather covariates including evapotranspiration, precipitation, temperature, and their squares. Standard errors are clustered at the household level and are presented below coefficient estimates in parentheses. ***: $p < 0.01$; **: $p < 0.05$; *: $p < 0.1$.

Table A.4: IV Demand Regressions, Accounting for Nonprice Conservation

	Coastal		Inland	
	(1) AP	(2) MP	(3) AP	(4) MP
Δ log(AP)	-0.33***		-0.90***	
	(0.03)		(0.05)	
Δ log(MP)		-0.16***		-0.40***
		(0.02)		(0.02)
Observations	476,142	476,142	202,342	202,342
Households	26,974	26,974	10,837	10,837
Household FE	Y	Y	Y	Y
Month-of-Sample FE	Y	Y	Y	Y
First-stage F-stat	2,736	2,195	4,225	5,688

Notes: The table presents estimates of $\hat{\beta}$ from estimating Equation 3. The dependent variable is the difference between contemporaneous and baseline consumption, $\Delta log(q_{it})$. Endogenous price differences are instrumented for in the first stage using $\Delta log(\hat{p}_{it})$. The time period included is from July 2015 to December 2016. Columns 1 and 3 instrument for average price differences, and Columns 2 and 4 instrument for marginal price differences. All specifications include a vector of weather covariates including evapotranspiration, precipitation, temperature, and their squares. Standard errors are clustered at the household level and are presented below coefficient estimates in parentheses. ***: $p < 0.01$; **: $p < 0.05$; *: $p < 0.1$.

Table A.5: IV Demand Regressions, Alternative Fixed Effects

	Coastal		Inland	
	(1) AP	(2) MP	(3) AP	(4) MP
Δ log(AP)	-0.18***		-1.20***	
	(0.03)		(0.07)	
Δ log(MP)		-0.10***		-0.55***
		(0.02)		(0.03)
Observations	477,121	477,121	203,188	203,188
Households	26,999	26,999	10,841	10,841
Month-of-Sample × Zip FE	Y	Y	Y	Y
First-stage F-stat	4023.0	4020.5	6277.9	7426.5

Notes: The table presents estimates of $\hat{\beta}$ from estimating Equation 3. The dependent variable is the difference between contemporaneous and baseline consumption, $\Delta log(q_{it})$. Endogenous price differences are instrumented for in the first stage using $\Delta log(\hat{p}_{it})$. The time period included is from July 2015 to December 2016. Columns 1 and 3 instrument for average price differences, and Columns 2 and 4 instrument for marginal price differences. All specifications include a vector of weather covariates, including evapotranspiration, precipitation, temperature, and their squares. Standard errors are clustered at the household level and are presented below coefficient estimates in parentheses. ***: $p < 0.01$; **: $p < 0.05$; *: $p < 0.1$.

Table A.6: IV Demand Regressions, Prediction Errors

	Coastal		Inland	
	(1) AP	(2) MP	(3) AP	(4) MP
Δ log(AP)	-0.21***		-0.93***	
	(0.03)		(0.06)	
Δ log(MP)		-0.10***		-0.42***
		(0.02)		(0.03)
Observations	477,326	477,326	203,259	203,259
Households	26,995	26,995	10,840	10,840
Household FE	Y	Y	Y	Y
Month-of-Sample FE	Y	Y	Y	Y
First-stage F-stat	2,737	2,200	4,216	5,689

Notes: The table presents estimates of $\hat{\beta}$ from estimating a variant of Equation 3. The dependent variable is the prediction error, or the difference between logged actual and logged baseline consumption, $log(PE) = log(q_{it}) - log(\hat{q}_{it})$. Endogenous price differences are instrumented for in the first stage using $\Delta log(\hat{p}_{it})$. The time period included is from July 2015 to December 2016. Columns 1 and 3 instrument for average price differences, while Columns 2 and 4 instrument for marginal price differences. All specifications include a vector of weather covariates, including evapotranspiration, precipitation, temperature, and their squares. Standard errors are clustered at the household level and are presented below coefficient estimates in parentheses. ***: $p < 0.01$; **: $p < 0.05$; *: $p < 0.1$.

Table A.7: Heterogenous Demand Elasticities, Average Price

	Coastal		Inland	
	(1) Budget	(2) Consumption	(3) Budget	(4) Consumption
Δ log(AP)	-0.77***	-1.63***	-2.61***	-1.92***
	(0.06)	(0.20)	(0.16)	(0.14)
Δ log(AP) $\times$ Budget (100–150%)	0.51***		1.63***	
	(0.05)		(0.13)	
Δ log(AP) $\times$ Budget (>150%)	0.67***		2.12***	
	(0.09)		(0.15)	
Δ log(AP) $\times$ Q2		0.71***		0.91***
		(0.17)		(0.12)
Δ log(AP) $\times$ Q3		1.30***		1.13***
		(0.18)		(0.13)
Observations	477,110	477,110	203,187	203,187
Households	26,988	26,988	10,840	10,840
Household FE	Y	Y	Y	Y
Month-of-Sample FE	Y	Y	Y	Y
First-stage F-stat	476	143	525	646

Notes: The table presents estimates of $\hat{\beta}$ from estimating a variant of Equation 3. The dependent variable is the difference between logged contemporaneous and logged baseline consumption, $\Delta log(q_{it}) = log(q_{it}) - log(\tilde{q}_{it})$. Logged endogenous prices are instrumented for in the first stage using $\Delta log(\hat{p}_{it})$, which is interacted with dummy variables for terciles of predrought budget and consumption classes. The time period included is from July 2015 to December 2016. All columns instrument for average price differences. All specifications include a vector of weather covariates, including evapotranspiration, precipitation, temperature, and their squares. Standard errors are clustered at the household level and are presented below coefficient estimates in parentheses. ***: $p < 0.01$; **: $p < 0.05$; *: $p < 0.1$.

Table A.8: Heterogenous Elasticities by Income Quartiles, Marginal Price

	Coastal	Inland
	(1)	(2)
	MP	MP
Δ log(MP)	-0.35***	-0.53***
	(0.04)	(0.04)
Δ log(MP) × I2	0.03	0.03
	(0.04)	(0.04)
Δ log(MP) × I3	0.12***	0.06
	(0.04)	(0.04)
Δ log(MP) × I4	0.16***	0.09**
	(0.04)	(0.04)
Observations	477,110	203,187
Households	26,988	10,840
Household FE	Y	Y
Month-of-Sample FE	Y	Y
First-stage F-stat	270	1,084

Notes: The table presents estimates of $\hat{\beta}$ from estimating a variant of Equation 3. The dependent variable is the difference between logged contemporaneous and logged baseline consumption, $\Delta log(q_{it}) = log(q_{it}) - log(\tilde{q}_{it})$. Logged endogenous prices are instrumented for in the first stage using $\Delta log(\hat{p}_{it})$, which is interacted with dummy variables for quartiles of predrought income classes. The time period included is from July 2015 to December 2016. All specifications include a vector of weather covariates, including evapotranspiration, precipitation, temperature, and their squares. Standard errors are clustered at the household level and are presented below coefficient estimates in parentheses. ***: $p < 0.01$; **: $p < 0.05$; *: $p < 0.1$.

Table A.9: Heterogenous Elasticities by Income Quartiles, Alternate Income Classification for Coastal Only

	(1) AP	(2) MP
Δ log(AP)	-0.65***	
	(0.07)	
Δ log(AP) × I2	0.10	
	(0.07)	
Δ log(AP) × I3	0.17**	
	(0.07)	
Δ log(AP) × I4	0.29***	
	(0.07)	
Δ log(MP)		-0.32***
		(0.03)
Δ log(MP) × I2		0.06*
		(0.04)
Δ log(MP) × I3		0.09**
		(0.04)
Δ log(MP) × I4		0.14***
		(0.03)
Observations	477,110	477,110
Households	26,988	26,988
Household FE	Y	Y
Month-of-Sample FE	Y	Y
First-stage F-stat	462	353

Notes: The table presents estimates of $\hat{\beta}$ from estimating a variant of Equation 3. The dependent variable is the difference between logged contemporaneous and logged baseline consumption, $\Delta log(q_{it}) = log(q_{it}) - log(\tilde{q}_{it})$. Logged endogenous prices are instrumented for in the first stage using $\Delta log(\hat{p}_{it})$, which is interacted with dummy variables for quartiles of predrought income classes. The time period included is from July 2015 to December 2016. Results are presented for Coastal only. All specifications include a vector of weather covariates, including evapotranspiration, precipitation, temperature, and their squares. Standard errors are clustered at the household level and are presented below coefficient estimates in parentheses. ***: $p < 0.01$; **: $p < 0.05$; *: $p < 0.1$.

Table A.10: Shares of Revenue and Consumption Borne by Large Users

	Pre-Surcharge Pricing	Surcharge Pricing	Difference
		Coastal	
Over-Budget Households			
Share Total Revenue	0.38	0.38	-0.01
Share Total CCF	0.30	0.29	-0.02
Heavy-Use Households			
Share Total Revenue	0.51	0.51	-0.00
Share Total CCF	0.45	0.44	-0.01
Large-Lawn Households			
Share Total Revenue	0.39	0.40	0.01
Share Total CCF	0.39	0.39	0.00
		Inland	
Over-Budget Households			
Share Total Revenue	0.50	0.47	-0.03
Share Total CCF	0.46	0.44	-0.02
Heavy-Use Households			
Share Total Revenue	0.47	0.46	-0.01
Share Total CCF	0.46	0.44	-0.02
Large-Lawn Households			
Share Total Revenue	0.35	0.36	0.01
Share Total CCF	0.37	0.37	0.01

Notes: The table presents the shares of total revenue and total consumption that are generated by three separate user classes. Over-budget households are those that go over their budget on average across all months in the predrought surcharge pricing training period of 2011-2013. Heavy-use and large-lawn households are those in the top quartile of the distribution for these variables during the same preperiod, respectively. The third column presents differences in proportions between the two periods. We define total revenue as aggregate revenues raised from variable commodity charges specifically, and abstract away from fixed charges.

Table A.11: Summary Statistics for Counterfactual Rates

	(1)	(2)	(3)	(4)
Coastal	Mean	Mean	Mean	Mean
Budget-Based Rate	28.55	31.93	36.78	49.74
Uniform Rate	29.62	32.77	36.92	45.92
Progressive Fixed Fee	21.62	30.55	41.35	54.19
IBR, Mean Budget	27.57	31.25	36.91	51.71
Unique Accounts	6,774	8,387	6,507	5,338
Total Billing Observations	120,725	149,206	115,436	95,027

	(1)	(2)	(3)	(4)
Inland	Mean	Mean	Mean	Mean
Budget-Based Rate	94.84	98.74	108.28	131.05
Uniform Rate	94.69	98.71	108.41	131.49
Progressive Fixed Fee	73.42	92.18	115.06	155.72
IBR, Mean Budget	94.36	99.48	109.51	131.44
Unique Accounts	2,949	2,661	2,662	2,569
Total Billing Observations	55,390	50,033	50,025	48,376

Notes: The table presents summary statistics for the counterfactual bill analysis. Consumption is defined as predicted consumption in the drought surcharge period using the predictions from our random forests. Mean bills in USD ($) under each rate structure are broken out by quartiles of the income distribution. Bills are defined as the variable commodity charge plus the fixed fee, and abstract away from other charges, such as sewer fees and other delivery charges.

B Data Construction and Cleaning Appendix

B.1 Raw Data Cleaning

We first import the raw data files and keep only observations in the Single-Family Residential category and in the periods during which BBRs were in place for the two utilities (July 2011–August 2017 for Coastal and October 2011–December 2017 for Inland). The raw billing records for Coastal contain 3,836,406 observations. The Inland billing records contain 2,097,552 observations. For Coastal, condos are sometimes listed as single family and sometimes as multi-family. We chose to keep both initially but then filter out buildings that are clearly master-metered apartment buildings and not condos through the use of a more descriptive property use code identifier. We then filter further by dropping accounts that use recycled water instead of standard drinking water. After these initial screens, we are left with 2,121,852 observations for Coastal and 1,025,381 observations for Inland.

We then merge each of three supplemental datasets as described in Section 3. We first merge in the US Census 2015 ACS five-year estimates for household size, household race, and household income distributions at the census block group level (US Census Bureau, 2015). The raw billing records were geocoded to include latitude and longitude coordinates and block and block group numbers, which facilitates the merge to census data. We also merge some limited demographic information received from CaDC by using customer and billing record ID numbers.

Next, we merge the county assessor data from the two southern California counties in which our utilities are located. For Coastal, the assessor parcel number matches well with that in the assessor data (over 99% match rate). We then drop addresses that match to more than one assessor parcel number and keep the one most likely to be the actual household (as determined by similarities in address strings). We calculate the Levenshtein difference between strings for these parcels and keep those with low scores and those in which street numbers of the houses match between the raw data and assessor. For Inland, the assessor parcel number did not match well with the assessor data, in part due to some data issues with extra digits at the end of numbers. String cleaning and manipulation was never able to generate higher than a 70% match rate. Even for those records that did match, in many cases hand-inspecting addresses revealed differences between the raw data and the assessor data. Therefore, for Inland, we created a full address string variable by which to merge the assessor data to the billing data. This resulted in an 80% match rate. We then drop a limited number of households with more than one parcel number for a given address. At this stage in the data cleaning, we are left with 2,017,749 observations for Coastal and 886,529 observations for Inland.

Furthermore, we merge in our weather variables in addition to evapotranspiration as described in the paper and from Schlenker and Roberts (2009). The data consist of daily weather measures for 2.5-by-2.5 mile grids across the contiguous 48 states. We keep records for relevant grids in southern California over the study period of our analysis (2011–2017). We then match customers in the billing data to their nearest grid in the daily weather data using the `geonear` Stata package (Picard, 2012). We then take each billing record and calculate the average daily maximum and minimum temperature and average and total precipitation based on the daily weather data for the corresponding weather grid and dates for the billing record. A small number of parcels in both data sets had missing coordinates and are dropped in this stage.

We apply four final filtering criteria to our data. We first drop a small number of remaining observations that are less than 15 days or more than 45 days, as these observations are not representative of a normal billing period that approximates a calendar month's worth of time. We then drop very large outliers in consumption and budgets. These potentially indicate months where the customers had a variance to fill a swimming pool, or potentially had a leak or other water emergency on their property. For both variables, we drop observations greater than the 99.75th percentile. Third, we apply the filtering criteria that households must have a relatively full panel ($\geq$ 70 months) worth of billing records in order to guarantee that enough data is available to generate consumption predictions. Finally, we also drop a small number of households that have no variability in water consumption across all months, as these are potentially households with no water consumption. After applying these filters, we are left with the final data used in the empirical analysis: 1,989,521 observations for Coastal (representing 27,006 unique households) and 789,741 observations for Inland (representing 10,841 unique households).

B.2 Evapotranspiration and Outdoor Budget Construction in Inland

Beginning in 2016, many Inland records have missing information on indoor and outdoor water budgets. We can exactly recreate indoor budgets using the indoor budget formula, but we must rely on an estimate of the outdoor water budget. This is because we only observe aggregate evapotranspiration over the entire billing period, but Inland calculates outdoor budgets on a daily level and then aggregates them to get a total outdoor budget for the billing period. However, Inland's plant factors correspond to calendar months, and most billing periods include days from two separate months. Therefore, from the raw data alone, we cannot exactly recreate outdoor water budgets because we are unsure of how much evapotranspiration occurred in each calendar month of a billing period. To improve upon using the overall evapotranspiration measure, we use publicly available data from the California Irrigation Management Information System (CIMIS, 2018) to calculate an estimate of the percentage of evapotranspiration that occurred in each month of the billing period. We then apply those percentages to the total evapotranspiration observed for the billing record, and generate two new variables for each billing record that represent the portion of the overall evapotranspiration that occurred in each calendar month of the billing record. Then, we are able to apply the correct daily plant factors to these adjusted evapotranspiration variables, and more accurately recreate outdoor water budgets.

B.3 Price and Bill Calculations

We gather historical information about residential water rates and budget tiers from both utilities' financial records and other publicly available documents in order to merge price information with our billing records. We formally code the rate structure for each budget period using the Open Water Rate System (OWRS) developed by the California Data Collaborative. We calculate final bill amounts using the R package `RateParser` developed by Tull (2016). This package allows users to bring in data on monthly water budgets and consumption and apply the rate structures coded in OWRS format to easily calculate total monthly bills. Although the Coastal billing records did not include the final bill amount, we repeat this process for Inland despite having final bill amounts to help us ensure the accuracy of our calculations. This also indirectly helped us to confirm that our estimates of outdoor budgets discussed previously were accurate as our calculated bills were very close to the provided bill amounts. Our analysis here accounts for the fact that Coastal rounds budgets to the nearest integer, but Inland does not.

C Empirical Framework Appendix

C.1 Prediction Generation

The first step in our empirical analysis is to develop predictions that reflect what counterfactual consumption would have been in the absence of price and nonprice conservation policies (described in Section 4). We use random forests, a machine learning algorithm commonly used in predictive exercises, to generate these predictions. We use the `rforest` Stata package from Schonlau and Zou (2020) to implement our predictive exercise.

Our predictions use data from 2011–2013 to predict consumption in 2014–2017. An underlying assumption that we make is that random forests have the ability to predict reliably well in entirely out-of-sample years. Although we expect our predictions to not match the observed consumption in 2014–2017 (due to the presence of drought policies), we do want the predictions in 2014–2017 to reliably capture the baseline consumption from 2011–2013. We test this assumption by performing a diagnostic exercise to check the ability of random forests to predict entirely out-of-sample. The core of the exercise is to limit the data to just two years, 2012–2013, and use 2012 data to predict 2013 consumption entirely out-of-sample. Since the full suite of drought policies had not been enacted, 2012 data should be able to predict out-of-sample in 2013 well. The results from this exercise are presented in Figure A.2. On average, the out-of-sample predictions in 2013 are close to the levels of actual consumption in 2013, and no consistent gap emerges between the two.

We additionally use the 2012-2013 prediction diagnostic exercises to perform other standard random forest model checks. In Table A.1, we present out-of-bag (OOB) error rates and out-of-sample root mean square error (RMSE) values for our diagnostic predictions compared to an alternative in which we use simple OLS models to generate predictions. OOB error rates are calculated by constructing random forest predictions for each observation in the training set using only the trees in which that observation was not included in the bootstrap sample used to develop that tree. OOB error rates are conceptually similar to errors calculated using k-fold cross validation in other machine learning applications, such as the least absolute shrinkage and selection operator (LASSO). As expected, out-of-sample RMSE values are higher than the OOB errors for our random forest predictions. However, Table A.1 does illustrate that the random forest does buy us additional predictive accuracy over using simple OLS for predictions, as evidenced by the lower out-of-sample RMSE for random forests in each utility compared to OLS.

We conclude our diagnostic exercise by using the 2012–2013 data to tune a number of important parameters for our random forests. For each utility separately, we allow tree depth, number of predictor variables made available to the random forest, and minimum leaf size to vary over a range of reasonable values, estimate predictions, OOB errors, and out-of-sample RMSE values and select appropriate values to use for these parameters when estimating the primary predictions using the full data. We seek to minimize these errors while at the same time respecting computational constraints. For example, there is a clear tradeoff between allowing trees to grow deeper for more predictive accuracy, and the amount of computational time it would take to estimate those deeper trees. Figure A.3 graphs OOB errors and out-of-sample RMSE values over the range of values considered for each of the three tuning parameters separately (and also separately by utility). The value we choose for each is represented by the vertical black lines and represents our judgment of the value beyond which the benefits of improvements in predictive accuracy are outweighed by the cost of additional computing time. We choose final values for the tuning parameters as follows: for Coastal, 16 for tree depth, 15 for number of predictor variables, and 3 for minimum leaf size; for Inland, 18 for tree depth, 24 for number of predictor variables, and 3 for minimum leaf size.

Given these chosen values, we proceed to generate our out-of-sample predictions using the full study period, with 2011–2013 as the training period and 2014–2017 as the out-of-sample test period. We examine which variables contribute the most influence to shaping the predictions by inspecting standard variable importance plots that calculate each variable's contribution to predictive accuracy and present a variable-importance metric on the scale of 0 to 1 (with 1 being most influential). The results of this exercise are presented in Figure A.4, where the top 12 most influential predictors are presented for each utility separately. Outdoor and total water budgets are influential in both utilities, as they send a normative signals to household about how much water consumption is "appropriate." Other influential predictors include weather variables and month and zip code dummies, especially in Inland.

Finally, we ensure that our results are not the result of some idiosyncratic feature unique to random forests by generating an alternative set of out-of-sample counterfactual predictions for 2014–2017 using a simple panel fixed effects approach. We include a vector of weather covariates and interactions and household-by-month-of-year fixed effects to generate these predictions. The time series of average monthly values is presented in Figure A.5. These predictions also reliably capture household consumption on average in the training period and replicate the observed gap between predicted and actual consumption that we observe when using random forests.

C.2 Bootstrapping Procedure

Our initial approach of clustering standard errors at the household level does not account for the fact that our random forest predictions are estimated with error. Without correcting for this, it is likely that our clustered standard errors will be too small. To the best of our knowledge, there is not fully clear guidance from the econometrics literature on how to handle this issue, and in practice, it is common to bootstrap both the prediction and regression steps of the estimation procedure to fully account for the variance associated with our predictions (for example, the procedure described in Burlig et al. (2020).) To construct standard errors for $\hat{\beta}$ that properly account for errors associated with our predictions, we implement the following bootstrap procedure:

- Sample households with replacement up to the full number of households in each utility. Sampling a household means that all of its data across years is included in the sample.

- Train the random forest using the 2011-2013 data from the bootstrap sample, and predict $\hat{q}_{it}^{b}$ out-of-sample in 2014–2017 for the bootstrap sample.

- Construct the predicted price change instrument $\Delta log(\hat{p}_{it})^{b}$ in the same way using $\hat{q}_{it}^{b}$.

- Estimate Equation 3 on the bootstrap sample (weighting by number of times household was sampled) and save values of $\hat{\beta}^{b}$.

- Repeat the process B times. We set $B = 500$ to balance having enough bootstrap replications to capture the important variability while respecting computational constraints.

- Calculate the mean and variance of the B estimates of $\hat{\beta}$, and report the bootstrapped standard error as the square root of the estimated variance.

D Distributional Appendix

D.1 Construction of Household Income Variable

We do not directly observe a measure of income for the households in our study. We create an estimated measure of household income to use in constructing our water Lorenz curves presented in Section 6. To estimate income, we use the following method, incorporating both information on property values from our assessor data and annual household incomes from the ACS Census 2015 5-year estimates at the block group level (US Census Bureau, 2015).

ACS provides estimates of the number of households in discrete income ranges at the block group level. We combine these estimates with data on the total households in the block group to create proportions of homes in each income category. For reference, the annual income categories are as follows: less than \$10,000; \$10,000–\$14,999; \$15,000–\$19,999; \$20,000–\$24,999; \$25,000–\$29,999; \$30,000–\$34,999; \$35,000–\$39,999; \$40,000–\$44,999; \$45,000–\$49,999; \$50,000–\$59,999; \$60,000–\$74,999; \$75,000–\$99,999; \$100,000–\$124,999; \$125,000–\$149,999; \$150,000–\$199,999; and more than \$200,000. To illustrate the calculation we make, consider a relatively wealthy block group that is estimated to have 200 homes overall, of which 40 homes each belong to the top five income brackets. Therefore, the proportion of homes in each income range is 0 for each of the lower-income brackets, and 0.2 for each of the top five income brackets.

We proceed by ranking the households in each block group for both utilities by that household's observed property value. We then take the proportions calculated previously and apply them to our household rankings. Now, consider the same hypothetical block group from before. We apply the percentages calculated from the ACS data to the households in this block group that are in our data. In this example, this would result in no households being assigned to the lower income groups, and 20% of the households in our data being assigned to each of the top five income groups. We conclude by assigning each household the midpoint of its discrete income range. Functionally, this means that each household is assigned one of the following annual income values: \$5,000; \$12,500; \$17,500; \$22,500; \$27,500; \$32,500; \$37,500; \$42,500; \$47,500; \$55,000; \$67,500; \$87,500; \$112,500; \$137,500; \$175,000; and \$200,000.

Our procedure depends on two primary assumptions. The first assumption that the households in our data are representative of the block group as a whole, and that the income distribution illustrated by the ACS data accurately describes the income distribution of the households in our data. The second assumption is that property values, which we do observe in our assessor data, are correlated with income and can be used to compare households in our data, such that a household with a higher property value also has a higher income. This is a strong assumption but necesssary given data limitations. For Coastal, we do have home sales prices data available, which may serve as a more reliable proxy. We repeat the procedure described above, with the relevant change being that households are ranked within a block-group by their most recent sales price (adjusted to 2015 dollars) before assignment to their corresponding income group.

D.2 Construction of Alternative Rate Structures

We construct counterfactual bills under three alternative rate structures: a uniform rate where the marginal price paid for each unit of water is constant, the same uniform rate coupled with a fixed service charge that varies with household income, and an IBR designed to mimic the budget tiers observed in practice. We discuss the construction of each set of alternatives in turn.

The uniform rate is the simplest of the three alternative rate structures. We begin by aggregating total volumetric revenue and total consumption in the drought surcharge pricing period.

Recall that these aggregate measures are based on *predicted* consumption. We then divide total predicted revenue by total predicted consumption to calculate the single uniform rate that satisfies our assumption of revenue neutrality. For Coastal, this uniform marginal price is \$1.79/CCF. For Inland, it is \$2.55. These prices for both utilities fall between the Tier 2 and Tier 3 prices under the existing BBRs.

Next, we combine the uniform rate derived in the previous structure with a "progressive" fixed service charge, similar to the analysis in Burger et al. (2020). To construct a fee that varies with income, we start with our estimated household income measure defined for the Lorenz curve analysis and aggregate income utility-wide. This allows us to determine each household's share of total utility-wide income. We then aggregate total revenues from the existing fixed service charges. We conclude by multiplying each household's share of total income by the aggregate service charge revenue to determine each household's "progressive" service charge. The service charges we calculate range from \$0.49 to \$19.62 in Coastal, with a mean of \$11.22. For Inland, the service charges range from \$1.45 to \$58.06, with a mean of \$29.90.

We conclude by constructing a revenue-neutral IBR structure. We construct the IBR tiers by taking the average indoor and outdoor water budgets for each utility under drought surcharges and use those as the block cutoff points between Tier 1 and 2 consumption, and Tier 2 and 3 consumption, respectively. We further mimic the BBRs we observe in practice by setting 125% of the total budget as the cutoff between Tier 3 and 4 consumption, and 150% of the total budget as the cutoff between Tier 4 and 5 consumption. The functional difference between this alternative rate structure and the existing BBRs is that rates are defined utility-wide and no longer household specific. We then distribute predicted consumption to each of the new IBR blocks.

We proceed to determine a schedule of prices consistent with our revenue-neutrality assumption. Given the infinitely many combinations of prices that result in the same overall volumetric revenue, we solve a system of linear equations that includes the total revenue equation (price times quantity within each block) and a series of equations that maintain the ratio of prices in higher blocks to the prices in Tier 1 of the actual BBR structure. This setup results in a unique solution that preserves the nature of an IBR structure as well as the ratio of prices in the original BBR structure. The price schedule we calculate for our alternative IBR is as follows for Coastal: \$1.01 for Tier 1 consumption, \$1.15 for Tier 2 consumption, and \$6.29 for consumption in Tiers 3–5. Recall that we are mimicking drought surcharge pricing, which is why the price for all consumption above budget is constant. For Inland, the IBR price schedule is \$1.96 for Tier 1 consumption, \$2.29 for Tier 2 consumption, \$4.38 for Tier 3 consumption, and \$5.37 for consumption in Tiers 4 and 5. Note that we obtain four unique prices instead of three as with Coastal because Inland restored the Tier 3 price one year into drought surcharges. The breakpoints between our five consumption tiers are 10, 16, 20, and 24 CCF in Coastal and 10, 36, 45, and 54 CCF for Inland.

D.3 Counterfactual BBR Structures

Proposition 218 in California limits the extent to which local governments can assess new taxes and fees. Utilities considering changes to their rate structures must take care to not run afoul of Proposition 218 restrictions. Given this, it might be infeasible to assume that our two utilities could change to a different type of rate structure as we do in our counterfactual bill analysis in Section 6. A natural question arises from these restrictions: if other rate designs are infeasible and utilities are set on using BBRs, how can changes to the water budget formula itself affect its redistributive properties? We consider here two feasible changes to the water budget calculations.

First, we examine the assumptions that utilities make about household size for homes in their service territories. As referenced earlier, both utilities make an initial assumption about single family household sizes (3 or 4 in Coastal and 3 in Inland) that households can later update. Given that household size is a direct component of indoor budgets, this policy incentivizes households with more than the assumed number of persons to update their household size with the utility and receive a larger budget. We verify this trend in the data in Figure A.11, as households do not update their household size to be smaller than the default. The solid bars represent the distribution of household sizes reported in the data and show no households with one or two people. This points to a potential issue, as it is unlikely that this distribution of household sizes will closely match the actual distribution in each utility's service territory.

We use Census data on household sizes in our study areas to create an adjusted measure of household size. We sort households first by number of bedrooms then by irrigable square footage within census block groups. We assume that households that have updated their size with the utility are "correct" and only include households that are assigned the default size in this correction. Then, in a similar process used in our estimation of household income, we assign each ranked household an adjusted or "corrected" size according to the distribution of sizes in each block group. For example, if ACS says that 10% of homes in a block group are one-person homes, we will assign the bottom 10% of our households in our ranking a household size of 1 instead of the default. Figure A.11 also plots this adjusted distribution in the transparent bars, showing the gap between what the observed data and what the census data imply about household sizes.

This is our first counterfactual BBR: we keep all other factors the same, but just calculate budgets using this adjusted household size. Our second counterfactual BBR focuses on another large driver of variation in water budgets: irrigable square footage of a household's lawn. Our analysis has shown that this portion of water budgets allows households with large lawns to consume more water at lower marginal prices. To correct for this, we suspend the individualized outdoor budget and instead calculate all household outdoor budgets based on the utility-wide average irrigable square footage area. We combine these new outdoor budgets with our adjusted indoor budgets from the previous counterfactual to define a household's new budget. In both of our counterfactual BBRs, we introduce a further simplification in that we collapse our budget down to two tiers, with an under-budget and over-budget price.

Figure A.12 displays Lorenz-style concentration curves where we plot the share of bills paid under actual BBRs and our two counterfactual BBRs over the distribution of households ordered by consumption. When plotting expenditures, a lower-hanging curve is more redistributive. Our results show that only correcting the household size actually makes BBRs slightly less redistributive relative to the observed rates, indicating that the liberal household size assumptions are relatively more beneficial to lower-consumption, smaller homes. Assigning all households a single average outdoor budget significantly improves the redistributive nature of BBRs relative to both alternatives, as many large households with large budgets have their outdoor budgets reduced as a result of the policy change.