University of Nevada, Reno

Promoting Conservation by Managing Residential Outdoor Watering: Evidence from the Truckee Meadows Area in Northern Nevada

A dissertation submitted in partial fulfillment of the
requirements for the degree of Doctor of Philosophy in
Resource Economics

By

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Abstract

Many water utilities in the US and around the world have implemented days-of-week outdoor watering restrictions (OWRs) to induce conservation and delay costly capacity expansions. To date, economists have primarily focused on two aspects of OWR policies: (i) the overall effectiveness of OWRs compared to an unrestricted baseline, and (ii) the welfare effects of OWRs on consumers. The literature suggests that OWRs can reduce water consumption, and that the effect on welfare may be small. Surprisingly, the existing literature offers no guidance on the optimal implementation of OWRs. This research sheds light on this issue by (i) exploring the impact of outreach campaigns on compliance and therefore consumption, and (ii) exploring how different patterns of weekly watering events effect total and peak consumption. Our findings have important implications for optimal OWR designs and implementation.

This dissertation is composed of three separate essays. The first essay presents a theoretical model of water demand in the presence of OWRs, and how conservation campaigns could alter household utility, thus alter demand. Then we provide an empirical analysis of a unique field experiment designed to test the impact of four different conservation letter campaigns on water consumption. We find that conservation campaigns can reduce consumption on non-assigned days, and that the magnitude of the reduction is dependent on the campaign message. We discuss how the results can be incorporated into outreach programs to reduce residential water demand.

The second essay investigates the short- and long- run impacts of four conservation letter campaigns on compliance with OWRs. First, we perform cluster

analysis on observations of daily household water consumption to identify noncompliance incidents during the summers of 2007 and 2008. We then examine the immediate effects of the campaigns on noncompliance after the letters were mailed in 2007, and compare those to any lingering effects in 2008. All four campaigns significantly reduced noncompliance in 2007. And while the magnitude of the reduction diminished in the 2008, the effects of three campaigns remained significant.

There exists considerable uncertainty regarding the optimal number of weekly watering days allowed under OWRs and how the days should be assigned, or the impact of allowing customers to choose their watering days within the weekly quota. The third essay takes a closer look at the relationship between weekly watering days, total weekly consumption, and weekly use peaks using panel data on daily water consumption at the household level. We tackle the multiple econometric challenges of a discrete-continuous, triple equation system with endogenous regressors and unobserved household effects via a full-information hierarchical Bayesian framework. We find that while consumption and peaks generally increase with the number of watering days, households that exhibit weekly flexibility in their choice of watering days have significantly lower use.

Dedication

To my parents, Eloise and Willard Castledine,

who always encouraged me to follow my dreams.

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1.1. Introduction

Throughout the western United States, an increase in residential water demand due to population growth along with the rising cost of developing new water supplies has resulted in water systems that are becoming more and more stressed. The Intergovernmental Panel on Climate Change concludes that warming temperatures, and changes in the form, timing and amount of precipitation, will very likely lead to earlier melting and significant reductions in snowpack in the western mountains, an important source of and storage for urban, agricultural, and environmental water (Field, et al., 2007). All of these factors highlight the need for more efficient operation and management of existing water supplies.

Utilities and regulatory commissions have come to realize that historic reliability and supply targets based on unrestricted demand may have been inflated from an economic efficiency perspective (Howe and Smith, 1994, Griffin and Mjelde, 2000, Hensher et al., 2006). Municipal water suppliers usually base water charges on cost recovery. This delivery-cost pricing does not include current or future scarcity values and can result in the inefficient use of residential water. This is depicted in Figure 1.1. When the price per unit of water is set to the marginal cost of delivery, p_0, the water use is w_0, the intersection of p_0 and the demand curve D_0. However, if all tangible and user cost are internalized, price would be set at p^* and leading to water use at w^*, the economic efficient allocation. Brookshire, 2002, estimates that the current average cost of municipal water in the US maybe as low as 20% below the real price. However, water is generally regarded not as an economic good, but as a necessity, even a right (Berk et al., 1980). Using the price of water as an allocation mechanism is thus often subject to political opposition, equity concerns, and legal limitations. Therefore, water providers are increasingly implementing non-price conservation measures, such as educational programs and rebate programs on water efficient technologies, that shift the residential demand curve to the left (from D_0 to D_1), or to measures that place restrictions on the use of water such as outdoor watering restrictions (OWR).

There is growing literature that examines the effectiveness of non-price conservation programs. Researchers have found evidence that OWRs can indeed reduce consumption. For example, Shaw and Maidment (1987; 1988) found that in Austin, Texas mandatory restrictions in 1984 and 1985 reduced water use by an average of 3% to 8%, and in Corpus Christi restrictions reduced water use by 31% during the 1984 drought. They also found 30% to 40% savings in the San Francisco Bay area during the 1976-1977 drought (CDWR, 1991). Shaw et al. (1992) reported that San Diego's voluntary program yielded summer savings of 27% compared to 6% from Los Angeles' mandatory program. Using household-level cross-section monthly time series data for a six-year period, Renwick and Archibald (1998) found that mandatory watering restrictions in Santa Barbara reduced average household water demand by 16%. Renwick and Green (2000) analyzed agency-level cross-sectional monthly time-series data for eight water agencies in California over an eight year period. Their results suggest water restrictions reduced average household water demand by 29%. Kenney, Klein, and Clark (2004) found that mandatory summer lawn watering restrictions in the Denver metro area were effective in reducing demand between 18% and 56%, and voluntary restrictions were effective in reducing demand between 4% and 12%. Schuck, Proft, and Waskom (2006) also found that lawn watering restrictions reduced demand; however, the magnitude of reduction depends on when restrictions are instituted. Moderate restrictions adopted early in the outdoor watering season were more useful than stringent restrictions adopted later in the season.

However, there is little research on what influences households to adhere to watering restrictions. This chapter makes an important contribution to the literature because it focuses on factors that influence compliance with outdoor water restrictions, and examines how these factors can be used in outreach programs to reduce residential water demand. This study further departs from the previous research in that we conduct a controlled natural field experiment rather than a retrospective analysis of water use. Harrison and List (2004) define a natural field experiment as an experiment with a field context in the commodity, task, or information set that the subjects can use, and conducted in the environment where the subjects naturally undertake these tasks and where the subjects do not know that they are in an experiment. The experiment was conducted on households in Reno, Nevada in 2007 to examine the impact of non-price driven conservation campaigns on residential water consumption. We find that conservation campaigns can induce a decrease in consumption, and that the magnitude of the decrease depends on the conservation message. Information derived from this research is central to improving the effectiveness of compliance programs and designing future strategies for demand management.

The remainder of this study proceeds as follows. The next section provides a brief overview of the water restrictions in Reno. Section 3 describes the experimental design.

Section 4 presents a theoretical model of compliance, and Section 5 describes the econometric analysis employed. Section 6 discusses the data and presents descriptive statistics. The results are reported in Section 7. Section 8 discusses policy implications and conclusions.

1.2. Lawn Watering Restrictions in Reno, Nevada

In Reno, lawn watering is restricted to particular days and times. Assigned lawn watering days are based on addresses. If the last number of the home address is odd, assigned watering days are Sundays and Thursdays. It the last number is even, assigned water days are Wednesdays and Saturdays. Watering is prohibited between 1:00 p.m. and 5:00 p.m. There are no quantity restrictions and no restrictions on hand watering or bush, tree, or flower watering.

The purpose of these restrictions is twofold. The mandatory twice-a-week watering schedule began in 1992 as part of an effort to conserve water during the 1988 to 1994 drought. The restrictions became permanent in 1996 as part of the Truckee River Operating Agreement (TROA) between the United States Departments of the Interior and Justice, the States of California and Nevada, the Pyramid Lake Paiute Tribe, Truckee Meadows Water Authority (TMWA), and Sierra Pacific Power Company. According to TROA, water savings from conservation programs go to storage for drought, water quality, and wildlife. The restrictions may be lifted after 90% of TMWA's customers have been converted to metered accounts, another stipulation of the agreement.[1]

[1] By the end of 2008, meters were installed at all eligible single family residences with active water services. Of these 73,726 customers, 7,657 were still being charged a

However, there is a secondary benefit. The restrictions control the timing of water going through the processing plants and distribution system, which helps provide an uninterrupted supply of water while delaying costly system expansion.

Municipal ordinance allows TMWA to impose fines for noncompliance. Successive violations within a calendar year can result in fines, $25 for the second violation and $75 for the third or more violations. However, the restrictions are rarely enforced with fines. Yet, we find that for the average household in our study area, water use is substantially higher on assigned watering days than on non-assigned days. There must be additional factors other than monetary fines that influence compliance. This is the focus of our research.

1.3. Experimental Design

During the summer of 2007, TMWA monitored all residential water consumers in specific Reno neighborhoods on a daily basis. This provided a great opportunity to conduct a natural field experiment testing factors that may affect compliance to local watering restrictions.

To conduct the experiment, each household in the monitored area was randomly assigned to one of five treatment groups. Each group, except for the control group, received a different version of a letter regarding assigned watering days. All letters included the watering schedule. The first group received a letter asking them to water only on their assigned days. The second group was sent a letter stating that the local

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residential flat rate. The change to all metered rates will not occur until June 2010 or later.

mountains only received 50% of the normal snow pack, thus this is a drought year. The third group received a letter explaining that water in the Reno area is a limited resource, and that watering restrictions expand the use of the resource allowing the delay of major capital improvements in water purification plants. The forth group received a letter explaining that TMWA has noticed unusually high water demand in their neighborhood on non-assigned days. No letter was sent to the control group.

Monitoring began on June 20 and ended on August 27, 2007 giving a total of 69 potential observations per household. Letters were printed on TMWA letterhead and were mailed Friday July 20 and Monday July 23.

1.4. Theoretical Model

In this section we present a simple model of household decision-making under watering restrictions. We theorize that the household makes a choice regarding the allocation of water between assigned and non-assigned watering days, and that the optimal amount of water applied on non-assigned days (i.e. the extent of non-compliance) is motivated by moral and social factors which can be influenced by the use of targeted information from the water authority.

Two main theories of compliance that have been advanced in the sociology literature are the instrumental and the normative. The instrumental perspective focuses on the use of deterrence mechanisms to improve compliance with the law. The economic deterrence model, based largely on Becker (1968) and utility theory, falls under the instrumental perspective. According to this view, people are driven purely by self-interest and therefore weigh only the potential gains from illegal activity against the severity and certainty of sanctions. When calculating the projected costs of illegal behavior individuals take into account the probability of state-imposed sanctions (i.e. fines and incarcerations) and the severity of these sanction should they occur. The number of offenses any person chooses to commit is a function of his probability of conviction, his punishment if convicted, and other variables, such as the income available to him from illegal and illegal activities. Yet there is plenty of evidence to suggest that there are other factors besides the conventional costs and benefits associated with illegal behavior that motivate compliance. Many people will sit at a red light even though there is no traffic or law enforcement in sight. They don't litter. They don't take the tag off of their mattress. And although there is no credible threat of monetary fines for non-compliance, they follow watering restrictions.

In contrast, the normative perspective focuses on the values that lead people to comply with the law voluntarily, and includes both intrinsic and extrinsic motivations. One motivation is an internal obligation to follow one's own sense of what is right or wrong. Doing what one perceives to be morally wrong can bring feelings of guilt. This self-imposed guilt can be seen as a potential cost of non-compliance (Grasmick and Bursik, 1990). Hence, personal morals are important factors to compliance.

Another motivation is an internal obligation to follow the dictates of a legitimate authority. After analyzing the results of a 1984 study of the experiences, attitudes, and behavior of a random sample of citizens in Chicago, Tyler (1990) concluded that people obey the law if they believe it is legitimate, not because they fear punishment.[2] Thus, an individual's perceptions of the fairness and appropriateness of the law are key determinates of compliance to that law.

Lastly, social influence, an extrinsic motivation, plays a significant role in compliance behavior (see Sutinen and Kuperan, 1999, and Ajzen and Fishbein, 1980). Concern's for one's social reputation has long been recognized as a strong motivator. Social pressure not to violate a group's social norms might involve avoidance, lack of trust, loss of respect, dissemination of negative opinions about the violator, and expulsion from the group. Embarrassment stems from fear of losing social standing. This socially-imposed embarrassment is another potential cost of non-compliance (Grasmick and Bursik, 1990). Consequently, the extent of social influence is also a key factor in compliance behavior.

Therefore, without the threat of monetary fines, the key variables determining compliance include morality, legitimacy, and social influence. We present a model of compliance decision making by individual households that integrates these normative influences.

We assume households are rational economic agents who attempt to maximize their utility, subject to a budget constraint. According to the theory of household behavior, the utility function is composed of basic wants and commodities. One of the commodities that may enter a household's utility function is a green lawn. Each household also has a set of production functions that determine how much of these

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[2] Other studies that provide empirical evidence that perceptions of legitimacy are related to compliance are McEwen & Maiman (1984); MacCoun et al. (1988); Lind et al. (1993); and Tyler (1990a); Paternoster et al. 1997; and Kuperan and Sutinen (1998).

commodities can be produced with market goods, time, and other resources (Becker, 1965). Water is an essential input in the production of the lawn, along with irrigation equipment, lawn care equipment and products, and time and effort. In the long run households can make changes to lawn size, grass variety, or irrigation equipment to maximize long-term utility subject to its budget constraint. We assume that the household is in equilibrium regarding these long-term lawn decisions, and that the goal during the summer months is to maintain lawn quality. We further assume that lawn quality is separable from all other commodities and that a partial demand function exists such that demand for lawn quality is only a function of the quantities and prices of water, time, and other resources subject to the allocated budget. For simplicity, we assume that the only input to lawn quality is water.

Under water restrictions, the household must decide how to allocate water between assigned and non-assigned days.[3] If the household violates the restrictions by watering on both assigned and non-assigned days, its subutility is defined as follows

$$
\begin{gathered}
U = u\left[L(b(w_0, w_1)), c(p_w, w_0, w_1))\right] + (h+l)u(m(w_1)) + (c+n)u(s(w_1)) \\
\text{subject to} \\
p_w(w_0 + w_1) = Y
\end{gathered} \tag{1.1}
$$

where w_0 denotes water use on non-assigned watering days, and w_1 denotes use on assigned days during the budgeted period. The price of water is denoted by p_w, and Y' is the budget allocated to lawn quality. L is level of lawn quality, which is a function of the

[3] Brennan et al. (2007) model the consumer problem under OWRs as a trade-off between the production of "green lawn" via hand-held watering devices and leisure time. Disutility from noncompliance is not considered.

benefits, *b*, and the costs, c, of w_0 and w_1. The utility received for lawn quality is given by *u(L).*

The household's utility from its moral standing is given by $(h+l)u(m(w_1))$ which depends on the household's sense of moral obligation as described by shift parameter *h*, and the level of legitimacy the household accords to the restrictions as described by shift parameter *l*. If violating the restrictions is against the household's personal moral values, then for $w_1 > 0,\ u_m < 0$.

The household's utility from its social standing is given by $(c+n)u(s(w_1))$, which depends on the perceived probability of being caught by the neighbors as described by shift parameter *c*, and the level of support for the restrictions by the immediate neighbors as described by shift parameter *n*. If violating the restrictions is against the social norm then $u_s < 0$ for $w_1 > 0$. The compliance behavior of the immediate neighbors is expected to influence the household. If the neighbors are noncompliant, the household will feel less social pressure to comply and visa versa. Thus an increase in neighborhood compliance will increase the magnitude of the disutility from non-compliance.

Maximizing (1.1) with respect to w_1 leads to the first order conditions for the optimal amount of water use on assigned watering days:

$$\frac{\partial U}{\partial w_1} = u'(L) = (h+l)u'(m) + (c+n)u'(s) = 0 \tag{1.2}$$

Clearly, a decrease in moral and social standing from non-compliance results in decreased marginal utility. The magnitude of the disutility depends on the household's sense of moral obligation, the level of legitimacy the household accords to the restrictions, the level of neighborhood support for the restrictions, and the perceived probability of being caught by either the authorities or neighbors whether fined or not. We hypothesize that if these four parameters can be increased by targeted information from the water authority, water use on non-assigned days can be significantly decreased.

Since all treatment letters include the watering schedule, all would inform households that were previously ignorant of the restrictions. For those who were aware of the restrictions, the reminder of the restrictions could reinforce personal values or social norms, which in turn influence behavior.

Both the drought and capacity letters could increase moral obligation by an appeal to help the community or the environment. Additionally, both the drought and capacity letters could change beliefs about the purpose of the restrictions. This could increase the legitimacy of the watering regulations. There is nothing in the monitoring letter, however, that would change or enhance beliefs regarding the restrictions. Likewise, there is nothing to increase moral obligation.

The monitoring letter would have the greatest impact on fear of losing social standing. With this letter, households are made aware that their neighborhood is being watched for noncompliance. A noncompliant household would conclude that the probably of being caught by the neighbors has increased, and thus the probability of losing social standing has increased.

Lastly, since all letters have the possibility of increasing compliance by the receiving household, they may also increase neighborhood support, resulting in an increase in social pressure to comply.

1.5. Econometric Analysis

The main objective for this analysis is to determine the causal impact of the letters on water use on both assigned watering days and non-assigned days. Since households were randomly assigned into treatment and control groups all relevant unobservable characteristics across groups should balance out, so that ex-post comparisons between the control group and treatment groups should reveal casual impacts of the intervention. Nonetheless, it is possible that unobserved differences across groups may account for differences in average outcomes and that the omission of these unobserved effects would lead to biased results.

To estimate the impact of the letters on consumption we employ a linear regression that allows for unobserved household attributes, shifts in mean consumption, and breaks in consumption trends. Water use in Reno tends to increase during the first part of the summer and then decline during the latter. The trend break parameters capture this. Specifically, the model is estimated by fitting the following equation:

$$
y_{it} = \left(\begin{array}{l}
\alpha + \sum_{j=1}^{4} \beta_{NWL\,j}(NW * L_j) + \beta_{NWI}(NW * I) + \sum_{j=1}^{4} \beta_{NWIL\,j}(NW * I * L_j) \\
+\beta_{TNW}(T * NW) + \sum_{j=1}^{4} \beta_{TNWL\,j}(T * NW * L_j) \\
+\beta_{TINW}(TI * NW) + \sum_{j=1}^{4} \beta_{TINWL\,j}(TI * NW * L_j) \\
+\beta_{W}(W) + \sum_{j=1}^{4} \beta_{WL\,j}(W * L_j) + \beta_{WI}(W * I) + \sum_{j=1}^{4} \beta_{WIL\,j}(W * I * L_j) \\
+\beta_{TW}(T * W) + \sum_{j=1}^{4} \beta_{TWL\,j}(T * W * L_j) \\
+\beta_{TIW}(TI * W) + \sum_{j=1}^{4} \beta_{TIWL\,j}(TI * W * L_j) \\
+\mu_i + \varepsilon_{it}
\end{array} \right)
$$

$$
\mu_i \sim N(0, \sigma_\mu^2) \qquad (1.3)
$$

$$
\varepsilon_{it} \sim N(0, \sigma^2)
$$

where y is household water consumption in 100 gallons, i indicates a household and t indicates a daily observation, $t = 0,...,68$. W is an indicator variable equal to one if the observation falls on an assigned watering day and likewise, NW indicates a non-assigned day. I is an indicator variable for the post-intervention period, equal to one for days occurring in the post-intervention period, $t = 38,...,68$. L_j denotes that the household received an intervention letter of type j. Specifically,

L_1 = 1 if household received the schedule letter,

L_2 = 1 if household received the drought letter,

L_3 = 1 if household received the capacity letter,

L_4 = 1 if household received the monitoring letter.

Let $T_t = t,\ t = 0,\ \ldots,68$ represent the time trend starting at $t = 0,$ and $TI = (t-38)$ if $t \geq 38$ and $TI = 0$ if $t < 38$ represent changes in the trend after the intervention. The component μ_i denotes the household random effect.[4] The household effect primarily captures unobserved landscape characteristics (and thus irrigation needs), and household preferences for a green lawn, available time, etc., that do not vary over time. The idiosyncratic error term ε_{it} targets behavioral interventions, such as the decision to hand-water, go on vacation, have extra guest, etc., and may vary across households and time.

To control for any unobserved group effects that may account for differences in average outcomes, the model allows for consumption at $t = 0$ to differ across control and treatment groups on both assigned and non-assigned days. Let $C = 1 - \sum_{j=1}^{4} L_j$, thus $C = 1$ indicates the control group. Expected consumption at $t = 0$ for the different groups can be expressed as:

---

[4] To test for random effects, we conduct the Lagrangian multiplier test developed by Breusch and Pagan (1980) and modified by Baltagi and Li (1990) for unbalanced panels. The null hypothesis is $H_0:\ \sigma_\mu^2 = 0$, there are no random effects, and the alternative is $H_1:\ \sigma_\mu^2 \neq 0$, there are random effects. The test statistic is

$$LM = \frac{(n\overline{T})^2}{2} \frac{A^2}{\left(\sum_i T_i^2\right) - nT}$$

$$\text{where A} = \frac{1\text{-}\sum_{i=1}^{n}\left(\sum_{y=1}^{Ti} e_{it}\right)^2}{\sum_{\mathrm{i}}\sum_{\mathrm{t}} e_{it}^2}.$$

Under the null hypothesis, LM is distributed as chi-squared with one degree of freedom. We obtain a LM test statistic of 1.0e+06, which far exceeds the 95% critical value 3.84. Hence we conclude that OLS is inappropriate for this data. The result of the test is to reject the null hypothesis in favor of the random effects model.

$$
\begin{aligned}
&E[y \mid NW=1, I=0, t=0, C=1]=\alpha \\
&E[y \mid NW=1, I=0, t=0, L_j=1]=\alpha+\beta_{NWL\,j} \\
&E[y \mid W=1, I=0, t=0, C=1]=\alpha+\beta_W \\
&E[y \mid W=1, I=0, t=0, L_j=1]=\alpha+\beta_W+\beta_{WL\,j}.
\end{aligned} \tag{1.4}
$$

Ignoring consumption due to time trends, expected consumption at $t=38$, the first day of the intervention period, for the different groups can be expresses as:

$$
\begin{aligned}
&E[y \mid NW=1, I=1, t=38, C=1]=\alpha+\beta_{NWI} \\
&E[y \mid NW=1, I=1, t=38, L_j=1]=\alpha+\beta_{NWL\,j}+\beta_{NWI}+\beta_{NWIL\,j} \\
&E[y \mid W=1, I=1, t=38, C=1]=\alpha+\beta_W+\beta_{WI} \\
&E[y \mid W=1, I=1, t=38, L_j=1]=\alpha+\beta_W+\beta_{WL\,j}+\beta_{WI}+\beta_{WIL\,j}.
\end{aligned} \tag{1.5}
$$

The parameter β_{NWI} reflects changes in the control group's non-assigned day consumption between $t=0$ and $t=38$ that are not due to the control group's non-assigned day time trend or:

$$\Delta y_C=(\alpha+\beta_{NWI})-\alpha=\beta_{NWI}. \tag{1.6}$$

Likewise, changes in treatment group L_j's non-assigned day consumption between $t=0$ and $t=38$ that are not due to its non-assigned day time trend can be expressed as:

$$\Delta y_{L\,j}=(\alpha+\beta_{NWL\,j}+\beta_{NWI}+\beta_{NWIL\,j})-(\alpha+\beta_{NWL\,j})=\beta_{NWI}+\beta_{NWIL\,j}. \tag{1.7}$$

The effect of treatment *j* on non-assigned days is:

$$TE_j=\Delta y_{L\,j}-\Delta y_C \tag{1.8}$$

which is simply the parameter $\beta_{NWIL\,j}$. Similarly, the parameter $\beta_{WIL\,j}$ represents the effect of treatment *j* on assigned days. If the letters increased compliance to the restrictions, $\beta_{NWIL\,j}$ will be negative; implying that households reduced water use on non-assigned days. In contrast, the sign of $\beta_{WIL\,j}$ is ambivalent. For households who were watering on their non-assigned days instead of their assigned days, an increase in compliance may increase use on assigned days if households switch watering to their assigned days. In this case, $\beta_{WIL\,j}$ will be positive. However, for households who were watering on both their assigned days and non-assigned days, an increase in compliance may result in no change in use on assigned days, only a decrease on non-assigned days. Therefore, $\beta_{NWIL\,j}$ is our main parameter of interest.

To control for possible differences across time between the control and treatment groups, we include time trend variables for each group for both assigned and non-assigned days. Since it is typical for water use in Reno to increase during the first part of the summer and then decline during the latter, we allow the trend for each group to change after the intervention. The average trends on non-assigned and assigned days during the pre-intervention period for the control group are measured by β_{TNW} and β_{TW} respectively. The parameters $(\beta_{TNW} + \beta_{TNWL\,j})$ and $(\beta_{TW} + \beta_{TWL\,j})$ estimate the pre-intervention trends experienced by the treatment group L_j. By adding the corresponding parameters that measure changes in trend after the intervention, we obtain the estimated average post-intervention trend. The trends for each group can be calculated as:

$$\begin{aligned}
&E[slope \mid NW=1, I=0, C=1] = \beta_{TNW} \\
&E[slope \mid NW=1, I=0, L_j=1] = \beta_{TNW} + \beta_{TNWL\,j} \\
&E[slope \mid NW=1, I=1, C=1] = \beta_{TNW} + \beta_{TINW} \\
&E[slope \mid NW=1, I=1, L_j=1] = \beta_{TNW} + \beta_{TNWL\,j} + \beta_{TINW} + \beta_{TINWL\,j} \\
&E[slope \mid W=1, I=0, C=1] = \beta_{TW} \\
&E[slope \mid W=1, I=0, L_j=1] = \beta_{TW} + \beta_{TWL\,j} \\
&E[slope \mid W=1, I=1, C=1] = \beta_{TW} + \beta_{TIW} \\
&E[slope \mid W=1, I=1, L_j=1] = \beta_{TW} + \beta_{TWL\,j} + \beta_{TIW} + \beta_{TIWL\,j}.
\end{aligned} \tag{1.9}$$

By allowing the post-intervention trends to vary across control and treatment groups and then comparing the differences, we can identify the relative change in each treatment effect over time. If the post-intervention trends are equal, the average treatment effect during the post-intervention period is equal to the immediate mean-shifting treatment effect at the beginning of the intervention period. However, if the trends are different, the average treatment effect is either smaller or larger than the immediate effect. Under the assumption that there was no other shock to the system that occurred only to the treatment group other than the treatment, this suggests that the treatment effect is either decreasing or increasing. Figure 1.2 compares these two situations, when post-intervention trends are equal and when they differ.

The change in treatment group L_j's non-assigned day trend after the intervention relative to the change in the control group's trend is measured by $\beta_{TINWL\,j}$. Likewise, $\beta_{TIWL\,j}$ measures relative changes in treatment group L_j's assigned day post-intervention trend. As stated earlier, water use peaks during the first part of the summer then declines in the second. Therefore, we would expect that both the treatment and control groups experience negative post-intervention trends. If the trends are equal, $\beta_{TNWL_j} + \beta_{TINWL_j} = 0$, the treatment effect is constant over time. If the treatment group has a flatter trend, $\beta_{TNWL_j} + \beta_{TINWL_j} < 0$, and the immediate mean-shifting treatment effect at the beginning of the intervention period was negative, the effect is diminishing. If the trend is steeper, $\beta_{TNWL_j} + \beta_{TINWL_j} > 0$, the effect is increasing. The opposite is true when the immediate effect is positive. A flatter trend indicates increasing effects, while a steeper effect indicates diminishing effects.

1.6. Data and Summary Statistics

The dataset contains daily meter readings taken over a three-month period during June, July, and August of 2007. Properties used in the study are single family detached residences. All properties with a change in household ownership during June, July, or August were dropped from the sample to insure an unbroken link between decision making, water consumption, and policy. An attempt was made to limit properties to ones that watered lawns. Specifically if a household used less than an average of 200 gallons per day during the summer months, it was unlikely that it was engaged in lawn watering and it was dropped.

Other reasons a property may have been excluded are missing meter readings or days with no consumption. Zero use could occur when the home is not being occupied. Any household that had five or more consecutive days of zero use was dropped as well as any household that had 14 or more total non-use days. Another concern was missing meter readings.[5] If a household had 14 or more additional days with missing readings or five or more consecutive missing readings, the household was dropped. The following dates were dropped from the sample since no metering took place on those days: August 4-6, and August 18-19. For the remaining households, any daily observations with missing meter readings were dropped for that household only resulting in an unbalanced panel.

[5] Meters are located underground and readings are sent wirelessly to a passing vehicle. At times, the vehicle might be driving too fast to pick up all meter readings or an object between the meter and vehicle might obscure the reading.

The treatment letters were mailed Friday July 20 and Monday July 23, but it is uncertain when they were actually delivered. Therefore, the days between and including Saturday July 21 and Friday July 27 were dropped from the sample.

Lastly, the dataset was divided by customers who are billed according to a metered rate based on actual use and those who are billed a flat rate, because consumption patterns vastly differed between the two groups. In this study, we only examine the consumption of metered rate households. The resulting unbalanced panel consists of 5970 households with a maximum of 57 observations each and a minimum of 44. Table 1.1 shows the number of odd and even address and total households in each treatment group. Average weekly consumption by the control group on both assigned and non-assigned watering days is shown in Figure 1.3 along with the weekly average maximum daily temperature. It is clear that assigned and non-assigned watering days have different consumption trends. Consumption on assigned days peaks later in the summer then levels off. On the other hand, consumption on non-assigned days peaks earlier and declines faster. Notice that the two weeks with the highest consumption on non-assigned days correspond to the two weeks with the highest temperature. This could mean that as temperatures peak, households are more apt to water their lawns on non-assigned days either with sprinklers or by hand-watering and/or they are increasing the watering of trees, bushes, and flowers, use of evaporated coolers, or the refilling of swimming pools, spas, and water features as evaporation increases. Note that during the study period there was no precipitation.

Basic descriptive statistics are presented in Table 1.2. During the study period, the average household used 663 gallons of water on non-assigned watering days and

1873 gallons of water on assigned days. There is high variance in consumption within groups, and although letters were randomly distributed, there are pronounced differences between groups.

1.7. Results

Estimation results are shown in Table 1.3. Robust standard errors clustered on households are reported.[6] Table 1.4 shows expected water consumption on the first day of the pre-intervention period, $t = 0$, based on the estimated model parameters.[7] Notice that the expected consumption on an assigned day is much greater than that on a non-assigned day for all groups. On average households used over 1100 more gallons of water on assigned watering days than on non-assigned days suggesting that some customers were complying with the restrictions, at least in part, before the intervention.

The coefficients for NWI and WI are not significant. Thus, after controlling for changes due to the time trend, there was no change in consumption by the control group at the intervention point. On the other hand, there were significant changes in some of the treatment groups. The treatment effect coefficients for non-assigned days, NWI_L2, NWI_L3, NWI_L4 are negative; the drought letter (L2), capacity letter (L3), and

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[6] Standard errors are calculated using the White's heteroskedastic consistent variance estimator extended to account for group-level clustering and thus contemporaneous correlation among observations within clusters (Rogers, 1993). The general form of the variance estimator for clustered data is given by

$$\hat{V}_c = (\mathbf{X'X})^{-1} \sum_{j=1}^{Nc} \left\{ \left(\sum_{i=1}^{Nj} e_i \mathbf{x_i} \right)' \left(\sum_{i=1}^{Nj} e_i \mathbf{x}_i \right) \right\} (\mathbf{X'X})^{-1}$$

where *Nc* corresponds to the number of clusters (households) and *Nj* corresponds to the number of *i*-observations within unit *j*.

[7] Calculation of expected water consumption at $t = 0$ can be found in Appendix C.

monitoring letter (L4) reduced consumption on non-assigned days. The monitoring letter (L4) had the greatest impact on non-assigned days, a decrease of 78 gallons relative to the control group, followed by the drought letter (L2), 65 gallons, and the capacity letter (L3), 49 gallons. The L3 treatment effect is significant at the 0.05 level, and the L4 and L2 treatment effects are both significant at the 0.01 level. The effect of schedule letter (L1) on non-assigned days is not statistically significant. Water consumption on assigned days increased for all treatment groups, suggesting that outdoor irrigation was switched from non-assigned to assigned days. However, there is more uncertainty in these estimates. The only significant treatment effect coefficient for assigned days is WI_L4; the monitoring letter increased consumption on assigned days relative to the control group by 82 gallons, and is significant at the 0.05 level. Figure 1.4 shows the estimated treatment effects with 95% confidence intervals.

Another way to illustrate treatment effects is to calculate the estimated change in weekly use after the intervention date. Since each household has five non-assigned days and two assigned days per week, the estimated change can be calculated using the equation $(5*\beta_{NWIL_j}*100 \text{ gallons} + 2*\beta_{WIL_j}*100 \text{ gallons})$. Perusal of Figure 1.5 highlights the results. We see that all treatment groups except L1 used less water per week than the control group. So although consumption increased on assigned days for these groups, overall consumption decreased. Note that the confidence intervals include zero for all treatment groups, thus we cannot say that there was a statistically significant reduction in weekly consumption.

Figure 1.6, a graphically portrayal of the estimated model, shows average trends in water use on non-assigned and assigned watering days during the pre- and post-intervention periods. All groups experienced increasing consumption during the pre-intervention period except for the SL group, which experienced a very flat trend. After the intervention, all groups experienced decreasing consumption. Table 1.5 displays the differences in post intervention slopes between the control group and each treatment group.[8] We perform four Wald Tests to test the hypothesis that the control group's post-intervention trend is equal to each of the treatment group's post-intervention trend. None of the differences were statistically significant. This implies that the average treatment effects during the post intervention period are equal to the immediate mean-shifting treatment effects at the beginning of the intervention period. P-values from the Wald Tests are included in Table 1.5.

1.8. Conclusions and Policy Implications

Water utilities across the country are turning to non-price mechanisms, such as OWRs, to control residential water demand, yet there is little or no recognition of sociological factors that may affect the extent of compliance. In Reno the water authority does have the legal right to impose fines for non-compliance with watering restrictions. However, enforcement services are not without costs. TMWA prefers to encourage compliance through the use of information and communication.

We hypothesize that if the household's sense of moral obligation and/or fear of losing social standing could be influenced by targeted information from the water authority, significant levels of increased compliance could be achieved. To test this hypothesis we conduct a controlled natural field experiment in which each household in

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[8] Calculation of the pre- and post- intervention trends can be found in Appendix D.

the monitored area was randomly assigned to one of five treatment groups. Four of the groups each received a different letter regarding the watering restrictions. The fifth group was the control group and thus received no letter. Using regression analysis, we were able to determine the causal impact of the letters on water use on both assigned watering days and non-assigned days. If the letters were effective in increasing compliance, water use on non-assigned days would decrease. The effect on assigned watering days, however, is ambivalent. For households who were watering on their non-assigned days instead of their assigned days, an increase in compliance may increase use on assigned days if households switch watering to their assigned days. For households who were watering on both their assigned days and non-assigned days, an increase in compliance may result in no change in use on assigned days, only a decrease on non-assigned days. Therefore, our main interest is how the letters effect consumption on non-assigned days. The results suggest that the water authority can increase compliance to watering restrictions by using targeted information that appeals to one's sense of moral obligation, legitimacy, and/or social standing.

The results also show that receiving the schedule by mail was not enough to significantly change water consumption on non-assigned days or assigned days. Adding information regarding the drought or the capacity issues did have a significant impact, reducing water use on non-assigned days as households increased compliance to the restrictions. Overall, both groups saw a decrease in weekly consumption compared to the control group. The drought (L2) and capacity (L3) letters may have caused households to increase compliance either to avoid self-imposed guilt or to boost self-esteem. If this is the case, the water authority may benefit from communication campaigns that highlight civic duty and thus enhance the sense of moral obligation. If the drought and capacity letters added to the legitimacy of the restrictions, a water authority may benefit from campaigns that would reinforce the validity of restrictions. This could include obtaining community input on and consent with demand side management mechanisms before their implementation.

The monitoring letter (L4), which threatens social standing, had the strongest effect on both assigned and non-assigned days, and reduced overall consumption. This suggest that water authorities may benefit from developing advertising campaigns or neighborhood programs with neighborhood leaders that would add social pressure to comply with water restrictions.

There were no statistically differences in post intervention slopes between the control group and each treatment group suggesting that the average treatment effects during the post-intervention period are equal to the immediate mean-shifting treatment effects at the beginning of the intervention period. Further research on the longevity of treatment effects should be done to assess the long-term effectiveness of targeted information.

While this article addresses factors that affect compliance and offers suggestions on how the factors may influence a household's utility function, we recommend further research be done. A survey in conjunction with a similar field experiment may help to decipher exactly how the treatments influence consumption. In turn, this would greatly benefit policy compliance campaigns and the design of future strategies for demand management.

Table 1.1. Distribution of Treatment Groups

	Treatment Group					
	Control	Schedule	Drought	Capacity	Monitoring	Total
Even Addresses	649	615	562	587	600	3,013
Odd Address	646	584	589	568	570	2,957
Total Households	1,295	1,199	1,151	1,155	1,170	5,970

Table 1.2. Average Household Water Consumption Per Day in 100 Gallons

	Pre-Intervention Period, Non-Assigned Watering Days				Post-Intervention Period, Non-Assigned Watering Days			
	Mean	Std Dev	Min.	Max.	Mean	Std Dev	Min.	Max.
Control	649	572	27	5,680	652	596	26	5,515
Schedule	653	582	31	4,742	611	571	19	4,588
Drought	698	717	18	10,010	651	656	30	6,380
Capacity	706	633	25	5,814	659	636	12	5,751
Monitoring	671	659	25	8,194	591	626	10	9,239

	Pre-Intervention Period, Assigned Watering Days				Post-Intervention Period, Assigned Watering Days			
	Mean	Std Dev	Min.	Max.	Mean	Std Dev	Min.	Max.
Control	1,798	1581	7	13,381	1888	1,744	44	14,873
Schedule	1,795	1559	39	12,864	1918	1,639	31	14,134
Drought	1,791	1525	36	14,304	1852	1,595	46	14,693
Capacity	1,885	1770	18	20,629	1973	1,839	80	15,597
Monitoring	1,852	1651	34	14,630	1994	1,767	39	13,253

Table 1.3. Estimation Results

Variable	Coeff.	Robust SE	Variable	Coeff.	Robust SE
constant	6.2328***	(0.1636)	TNW	0.0173***	(0.0045)
NW_L1	0.3118	(0.2512)	TNW_L1	-0.0188***	(0.0067)
NW_L2	0.4571	(0.2822)	TNW_L2	0.0012	(0.0079)
NW_L3	0.4884*	(0.2539)	TNW_L3	0.0048	(0.0072)
NW_L4	0.2095	(0.2700)	TNW_L4	-0.0004	(0.0075)
NWI	-0.0295	(0.1435)	TINW	-0.0395***	(0.0070)
NWI_L1	-0.0457	(0.1948)	TINW_L1	0.0213**	(0.0101)
NWI_L2	-0.6490***	(0.2391)	TINW_L2	0.0085	(0.0110)
NWI_L3	-0.4856**	(0.2264)	TINW_L3	-0.0110	(0.0113)
NWI_L4	-0.7829***	(0.2248)	TINW_L4	-0.0002	(0.0111)
W	11.1294***	(0.4381)	TW	0.0436***	(0.0091)
W_L1	0.0459	(0.6276)	TW_L1	-0.0059	(0.0129)
W_L2	0.0679	(0.6331)	TW_L2	-0.0092	(0.0138)
W_L3	0.8145	(0.6729)	TW_L3	0.0024	(0.0137)
W_L4	0.7466	(0.6584)	TW_L4	-0.0137	(0.0131)
WI	-0.1356	(0.2496)	TIW	-0.0465***	(0.0128)
WI_L1	0.5749	(0.3572)	TIW_L1	0.0027	(0.0179)
WI_L2	0.2570	(0.3988)	TIW_L2	-0.0115	(0.0197)
WI_L3	0.2522	(0.3984)	TIW_L3	-0.0202	(0.0188)
WI_L4	0.8192**	(0.3999)	TIW_L4	0.0154	(0.0193)
R^2:					
within	0.2335				
between	0.0012				
overall	0.1654				
Rho	0.3391				

Notes: Significance levels of 0.01, 0.05, and 0.10 are denoted by three, two, and one asterisks (***, **, *) respectively.

Table 1.4. Expected Water Consumption at $t = 0$ on Non-Assigned and Assigned Watering Days for Control and Treatment Groups (in Gallons)

	Control Group	L1 (schedule)	L2 (drought)	L3 (capacity)	L4 (monitoring)
Non-Assigned	623.28	654.45	668.99	672.12	644.22
Assigned	1736.22	1740.81	1743.01	1817.67	1810.89

Table 1.5. Expected Post-intervention Trends for Non-assigned and Assigned Watering Days for Control and Treatment Groups (in Gallons)

Non-Assigned	Control Group	L1 (schedule)	L2 (drought)	L3 (capacity)	L4 (monitoring)
Slope	-2.2207	-1.9686	-1.2516	-2.8409	-2.2472
Difference		-0.2521	-0.9691	0.6202	0.0265
P-value		0.7188	0.1788	0.4426	0.9726

Assigned	Control Group	L1 (schedule)	L2 (drought)	L3 (capacity)	L4 (monitoring)
Slope	-0.2948	-0.6187	-2.3632	-2.0825	-0.1238
Difference		0.3240	2.0684	1.7877	-0.1709
P-value		0.7824	0.1087	0.1471	0.8959

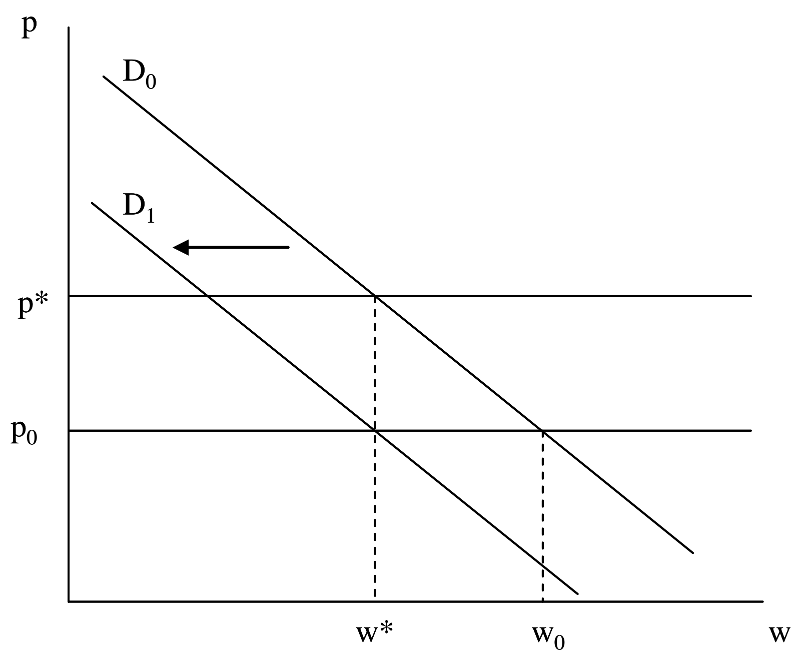

Figure 1.1. Optimal level of water consumption.

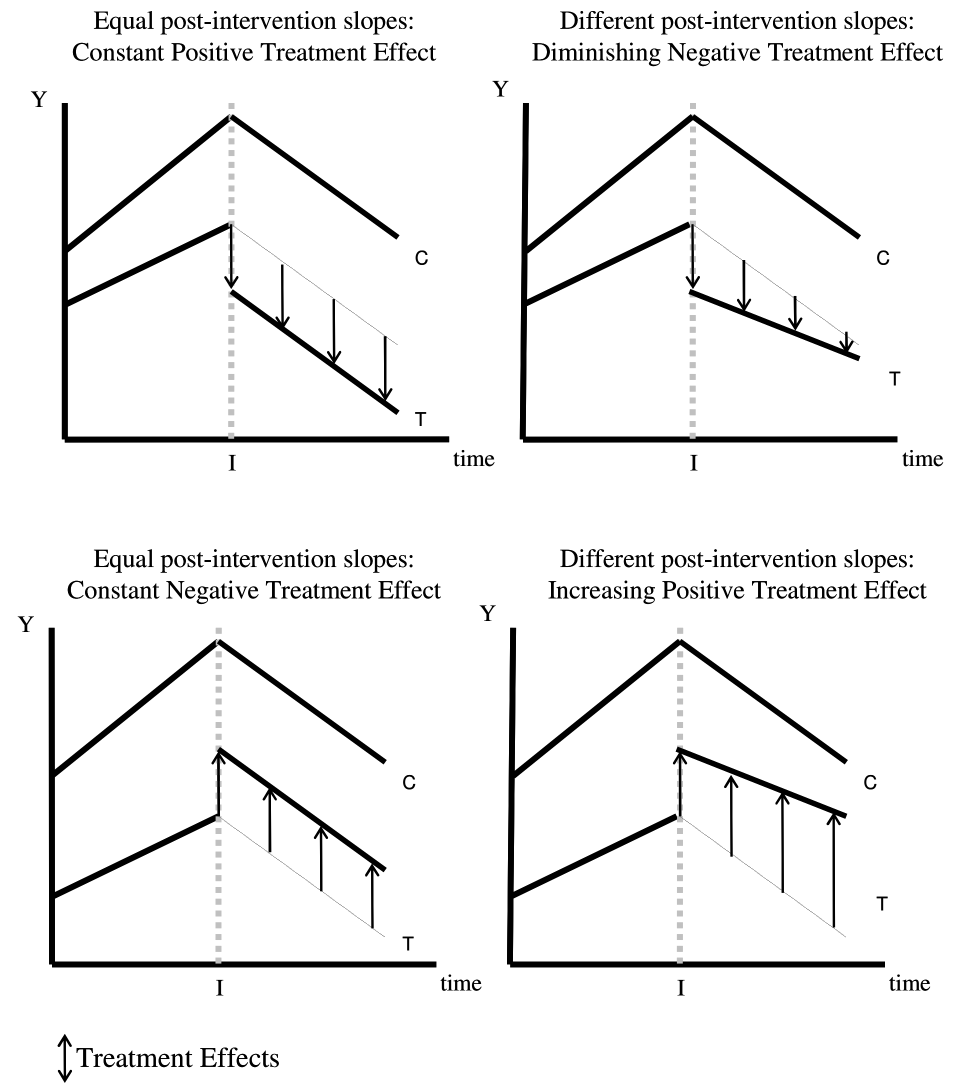

Figure 1.2. Average treatment effect over time.

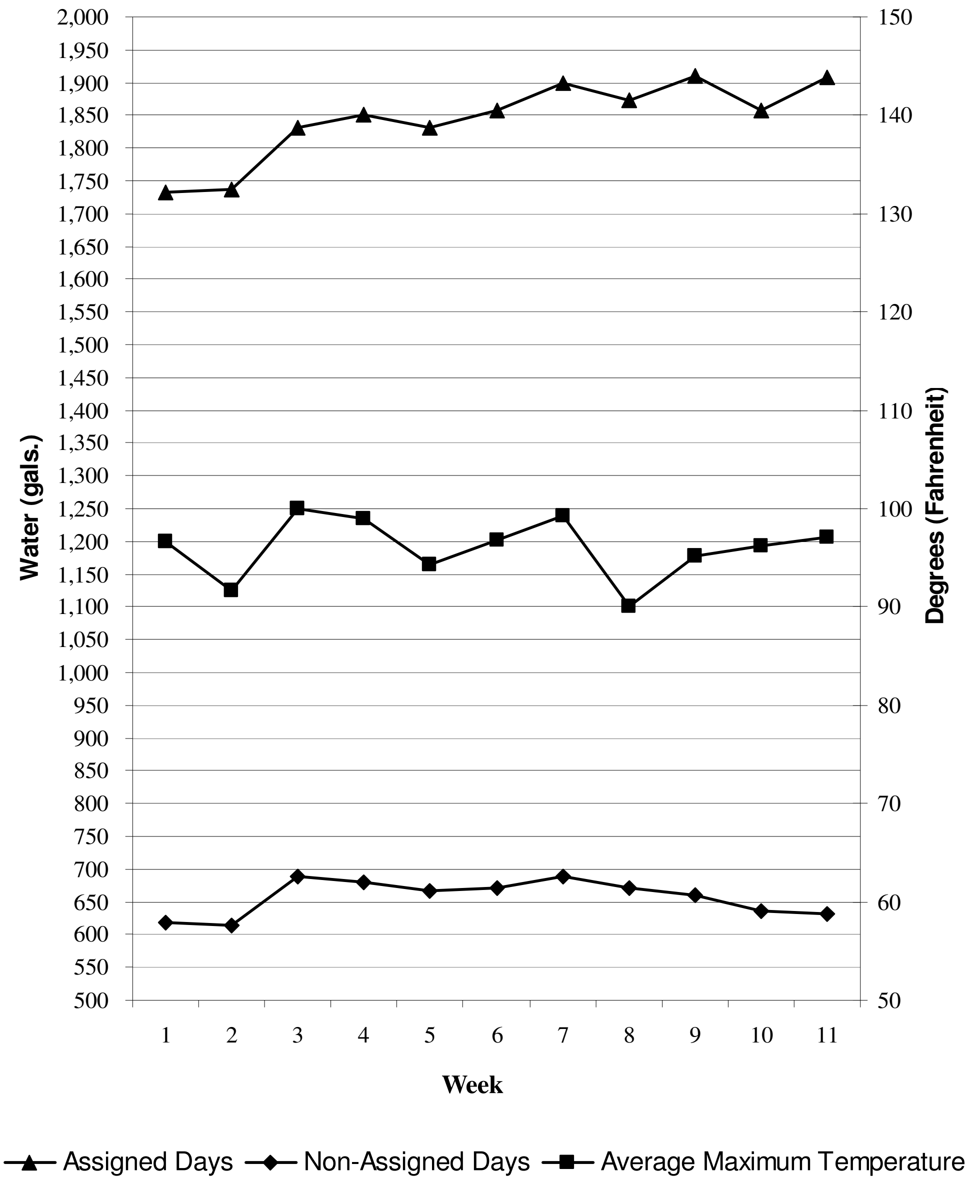

Figure 1.3. Average Weekly Water Use by the Control Group on Assigned and Non-assigned Watering Days during Study Period with Corresponding Average Weekly Maximum Daily Temperature

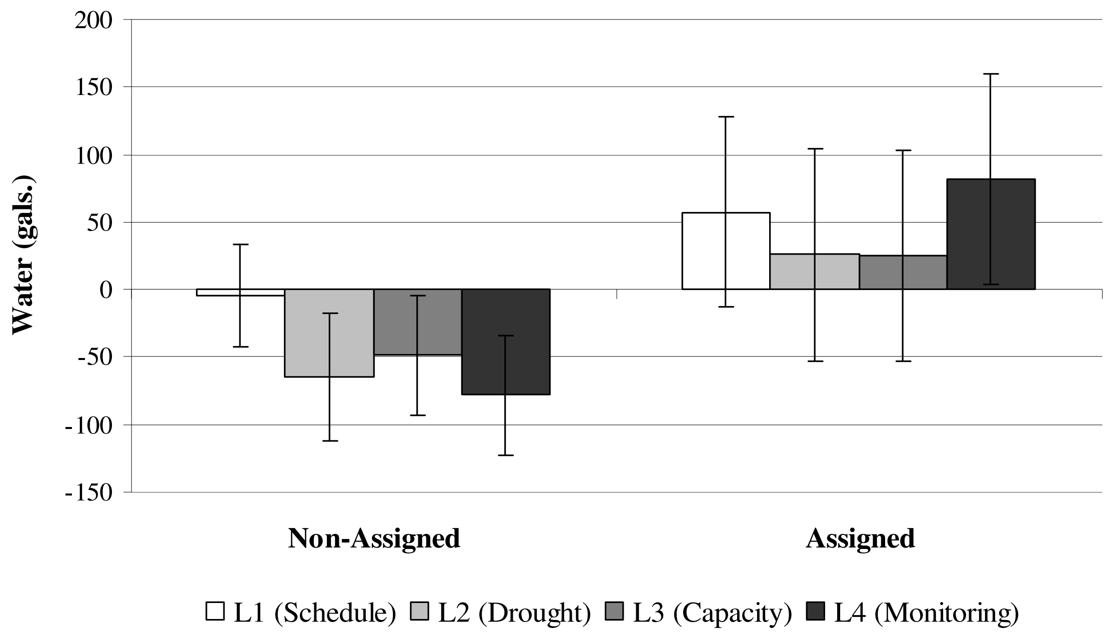

Figure 1.4. Estimated Treatment Effects with 95% Confidence Intervals

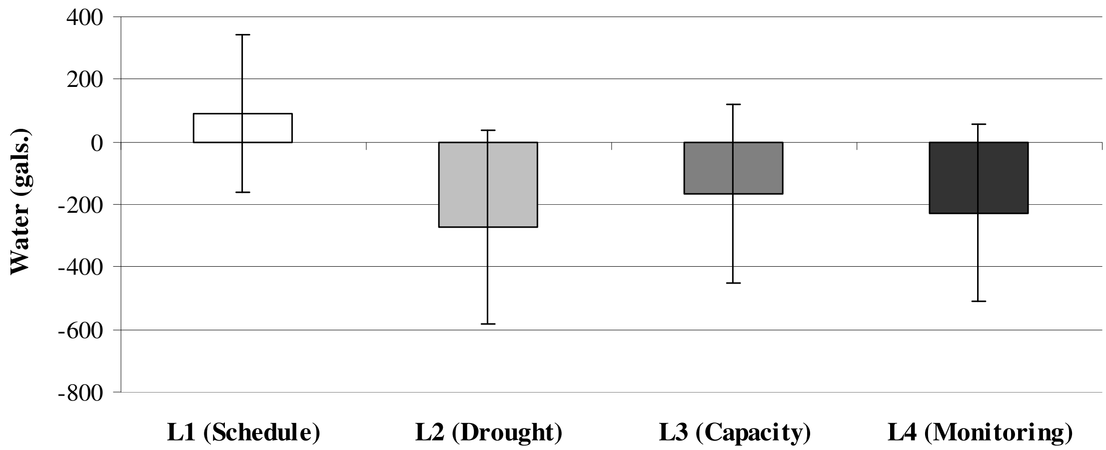

Figure 1.5. Estimated Treatment Effects on Weekly Consumption with 95% Confidence Intervals

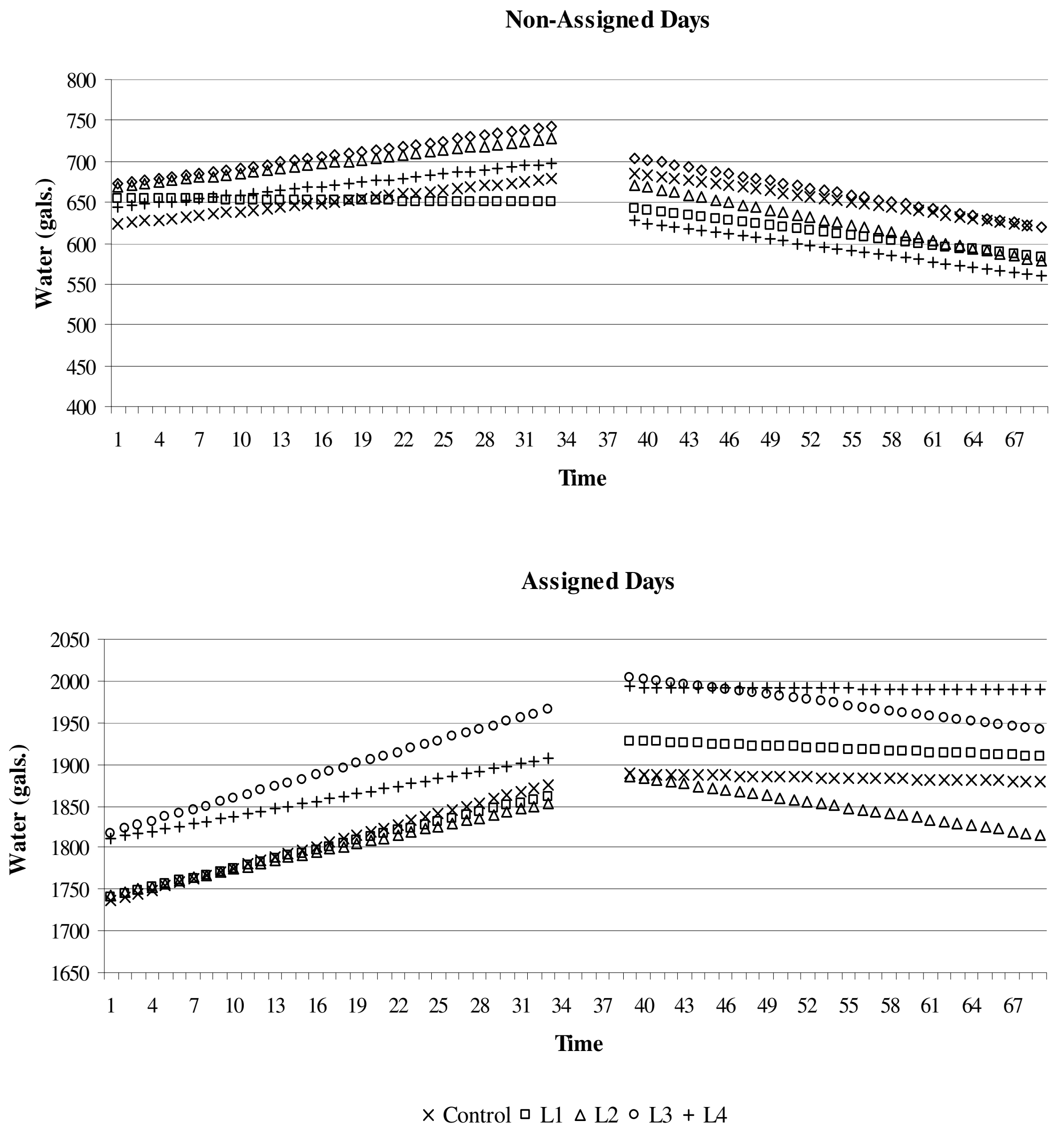

Figure 1.6. Estimated Pre- and Post-intervention Slopes

2. Short- and Long-Run Effects of Conservation Campaigns on Compliance to Outdoor Watering Restrictions

2.1. Introduction

Many water utilities in arid regions are increasingly implementing demand side management (DSM) measures, such as outdoor watering restrictions, retrofitting indoor appliances, and advertising campaigns, in order to reduce the quantity of residential water demanded. These measures may be geared toward temporary conservation in periods of drought or permanent conservation as a way to postpone the costly expansion of distribution systems or existing supplies, such as constructing new reservoirs, sourcing new ground water supplies, or building reuse or desalination facilities (supply side management).

Researchers have tried to determine the success of DSM measures, typically through econometric analysis of aggregated data collected in drought and non-drought years. For example, Renwick and Green (2000) develop a residential water demand model that incorporates non-price policy instruments (such as water allocations, use restrictions, public education) and increasing block pricing schedules. The analysis relies on cross-sectional monthly time-series data for eight water agencies in California during the years 1989 through 1996, a period that included a statewide drought. Results suggest that both price and alternative non-price policies were effective in reducing demand. However, the magnitude of the reduction in demand varied among policy instruments. More stringent mandatory policies, such as use restrictions and water allocations, reduced aggregate demand more than voluntary measures, such as public information campaigns and retrofit subsidies. Others studies have used similar approaches (e.g. Campbell, Johnson and Larson, 2004, Kenney, Klein, and Clark, 2004, Schuck, Profit, and Waskum, 2004, Renwick and Green, 2000, Michelsen, McGuckin, and Stumpf, 1999, Wang, et al., 1999, Renwick and Archibald, 1998). A few of these studies have included educational campaigns. Halich and Stephenson (2009), for example, studied the influence of informational efforts on non-price water-use restrictions, and found that water-use reductions increased with progressively higher levels of information.

In this analysis, we focus on the impact of conservation campaigns on compliance with twice a week lawn watering restrictions. Our research is unique in that it is an empirical analysis of a natural field experiment designed to assess the impact of conservation campaigns on residential water consumption. The field experiment was conducted in Reno, Nevada during the summer of 2007. Follow-up monitoring was done during the summer of 2008. We pool the 2007 and 2008 data to first identify noncompliance incidents, and then to examine short-run and long-run effects of conservation campaigns on compliance.

The remainder of this study proceeds as follows. The next section provides a brief overview of the water restrictions in Reno. Section 3 describes the experimental design and data preparation. Section 4 describes the method used to identify noncompliance, and Section 5 describes the econometric analysis employed. The results are reported in Section 6. Section 8 discusses policy implications and conclusions.

2.2. Lawn Watering Restrictions in Reno, Nevada

The long-standing outdoor water restrictions for Reno, which were also effective throughout our entire research period, allow the use of sprinklers in the morning and evening on two assigned days per week based on the last digit of a resident's address. There are no quantity restrictions, and no restrictions on hand watering or bush, tree, or flower watering. These regulations were originally implemented in 1992 in reaction to a prolonged drought. They became permanent in 1996 primarily to guard against future droughts through sufficient water storage, and to assure adequate flows of the Truckee River to Pyramid Lake, an important spawning habitat for trout and other fish species. These restrictions are only mildly enforced, with infrequent water patrols and nominal fines for repeated violations in the same calendar year.

2.3. Experimental Design and Data

In the summer of 2007 we conducted a field experiment in collaboration with TMWA to examine the impact of non-price driven conservation campaigns on residential water consumption. During this time, TMWA was monitoring all residential water consumers in specific Reno neighborhoods on a daily basis. To conduct the experiment, each household in the monitored area was randomly assigned to one of five treatment groups. Each group, except for the control group, received a different version of a letter regarding assigned watering days. All letters included the watering schedule. The first group (L1) received a letter asking them to water only on their assigned days. The second group (L2) was sent a letter explaining the current drought situation. The third group (L3) received a letter explaining that watering restrictions expand the use of the resource delaying costly capacity expansion. The forth group (L4) received a letter explaining that TMWA has noticed unusually high water demand in their neighborhood on non-assigned days.

TMWA originally selected 10,000 residences for the daily metering program. These addresses were further randomly assigned to a given letter group and the control group. Only single-family detached residences, under metered rate billing, and with an average use of over 200 gallons per day where used in this study. All properties with a change in household ownership during June, July, or August were eliminated from the sample to insure an unbroken link between decision-making, water consumption, and policy. The following dates were dropped from the sample since no metering took place on those days: August 4-6, and August 18-19. Additional observations were dropped from the original sample according to the following criteria:

- Eliminate all <u>households</u> (i.e. the entire panel) with 5 or more consecutive missing readings
- Eliminate all <u>households</u> (i.e. the entire panel) with a total of 14 or more missing readings
- Eliminate all <u>households</u> (i.e. the entire panel) with 5 or more consecutive days of zero consumption
- Eliminate all <u>households</u> (i.e. the entire panel) with 14 or more days of zero consumption
- Eliminate all other <u>observations</u> within specific households with missing readings

These cleaning steps resulted in a final sample of 5970 customers and 362,137 valid observations on daily use.

The daily metering campaign was repeated in 2008 for 6057 of the original households included in the 2007 experiment, distributed across original (i.e. pre-cleaning) letter and control groups. The 2008 metering took place between June 23, 2008 and August 18, 2008 for 57 uninterrupted days. We submitted the 2008 sample to the same cleaning steps as described above for the 2007 cases. Most importantly, we dropped all premises from the 2008 sample that showed an ownership change since 2007 or within the 2008 period. After cleaning, we have 3694 re-sampled households. Each re-sampled residence generated a full set of 57 observations. Thus, there are 210,558 valid observations from 2008.

Our data set also includes information on basic lot and building characteristics, publically available from the Washoe County Assessor's Office, and climate data, publically available from the Western Regional Climate Center. Sixteen households were dropped due to missing information on building characteristics. Pooling data from the two years, we have 5954 households and 571,366 valid observations of daily use. The split-up of residences over letter groups is shown in Table 2.1.

2.4. Identification of watering days

The main objective for this analysis is to determine the causal impact of the treatment letters on compliance. In order to do this, we must first determine what constitutes a violation of the restrictions, what we call a cheating incident. This section outlines the procedures used to determine cheating incidents from daily patterns of water consumption.

Data loggers and flow trace analysis software can distinguish between the different types of residential water use. Data loggers are instruments that measure the flow through a residential water meter. The flow trace analysis software uses the flow record to identify the patterns of specific fixtures within the household and outdoor irrigation systems. Using data loggers, a water utility could identify cheating incidents. However, data loggers and flow trace analysis software are expensive. We suggest a method that can detect cheating incidents from daily patterns of water consumption.

Lawn watering can greatly increase water consumption. The American Water Works Association (AWWA) reported in its 1999 Residential End Use Study that 42% of annual water use was for indoor purposes and 58% for outdoor purposes (Mayer and Deoreo, 1999).[9] Since the majority of outdoor water use occurs during the growing season, we would expect the proportion of water used for outdoor purposes to greatly increase during the summer months. Moreover, lawns require a large amount of water and usually take up a large proportion of the landscaped area, so we would expect a large increase in consumption on days when lawn watering occurred. Thus if the household is conforming to the lawn watering schedule, we should see higher consumption on assigned watering days and lower consumption on all other days. If the pattern doesn't conform to the watering schedule, its consumption behavior is considered noncompliant. Of course, this noncompliant label may or may not reflect reality. In our case, we cannot distinguish water used for lawn watering from water used for other outdoor or indoor

[9] The study examines water consumption of 1,000 single-family detached residential accounts in 12 US cities using data loggers and flow trace analysis along with information on the type of fixtures and irrigation system for each household obtained by a mail survey.

water use. An increase in water consumption may be due to a broken pipe, a leaky faucet, an increase in household occupancy, filling a spa, watering other outdoor plants, or hand watering, etc. Thus determining violations presents a challenge. Figure 2.1 gives an example of a household with an obvious watering pattern and one in which the watering pattern is not clear.

Detecting cheating incidents can be cast as a classification problem: we wish to classify an observation as a day in which the household engaged in lawn watering, or a day in which no lawn watering occurred. Then if an observation is classified as a watering day, yet it falls on a non-assigned watering day, it is labeled as a cheating incident. We accomplish this through *K*-means cluster analysis.

Cluster analysis is the identification of groups of observations that are cohesive and separated from other groups. Intuitively, consumption on a non-watering (NW) day will be more similar to consumption on other NW days than to consumption on watering (W) days; hence the separate groups are W and NW days. The method of *K*-means clustering aims to partition the data into *K* mutually exclusive clusters. In our case, *K* is equal to two. *K*-means clustering is one of the most popular clustering algorithms, and has a long history. Although the name "*K*-means algorithm" was first used by MacQueen (1967), the algorithm was first proposed by Steinhaus (1956). Since then, many researchers have proposed various extensions. Bock (2007) provides a detailed history of the *K*-means algorithm and its extensions.

Basically, *K*-means clustering treats observations as objects having locations and distances from each other. The objects are partitioned into *K* clusters, such that objects within each cluster are as close to each other as possible, and as far from objects in other clusters as possible. This process is done iteratively. Starting with initial centroids (cluster centers) observations are assigned to the cluster that minimizes the sum of distances from all objects to their centroid. The centroids are recalculated and the observations reassigned. This process is continued until a convergence criterion is met (e.g., there is no reassignment of any observation from one cluster to another). Various distance measures can be used. We use the L1 distance, also called the City-Block and Manhattan distance, which is calculated as the sum of absolute differences (Vinod, 1969, Späth, 1975, and Massart et al., 1983). Each centroid is the component-wise median for that cluster. Because distances are not squared, this measure is more robust to outliers than measures that use squared distances such as the Euclidean distance (Bishop, 1995).

There is tremendous variation in household water consumption. For example, the mean household average daily use for the control group is 956 gallons with a standard deviation of 644 and median of 803. This variation complicates classification. The quantity of water used on a NW day by one household may be greater than the quantity used by another on a W day. The first household might have a high demand for indoor water, while the second might have a very small lawn. To account for this heterogeneity, *K*-means clustering is performed separately on each household, clustering on daily consumption.

Since *K*-means is sensitive to starting values, the clustering algorithm is repeated five times with different starting values for each replicate. The first three replicates begin from randomly selected initial centroids. For the last two, we select initial centroids based on two reasonable guesses of clustering groups. For replicate four, observations occurring on non-assigned days are initially assigned to the NW group, and those occurring on assigned days are initially assigned to the W group. The initial set of centroids is the mean daily use for observations in the NW group and the mean daily use for observations in the W group. Replicate five uses different starting centroids. Observations that are one standard deviation greater than the mean use of all observations are initially placed in the W group. All others are place in the NW group. Again the initial set of centroids is the means of the two groups. Recall that the clustering algorithm is performed on individual households, thus the above calculations on done for each household separately. In other words, each household has its own set of starting values. Classification results from the replicates with the lowest total sum of absolute differences are used for final classification.

For 94% of the observations, more than one replicate shared the lowest total sum of absolute differences. All five replicates shared the same sum for 68% of the observations. Again only the replicates with the lowest total sum were used to classify an observation. The classifications results from these replicates were in agreement except for 300 observations. These observations fall equally between centroids and can be placed in either group without increasing the sum of absolute differences. We classify these ambiguous observations as NW days.

Clustering resulted in 359,431 observations classified as NW day, and 211,935 classified as W days, 62.91% and 37.09% respectively. Columns 2, 3, and 4 of Table 2.2 present the number of W and NW days by treatment group as well as watering days as a percentage of total observations. On days classified as W days, mean water consumption was 1932 gallons per day with a standard deviation of 1632 and median of 1470. On days classified as NW days, the mean was 397 gallons per day with a standard deviation of 462 and median of 250. The classifications for the two households featured in Figure 2.1 are displayed in Figure 2.2.

As stated earlier, if an observation is labeled as a W day but occurs on a non-assigned watering day, it is labeled as a cheating incident. The variable y indicates the cheating incidents:

> y = 1 if observation is classified as a W day yet occurs on a non-assigned watering day, 0 otherwise.

It will be the dependent variable in the following econometric analysis. Columns 5 and 6 of Table 2.2 show the number and percentage of cheating incidents by treatment group. There are 410,427 non-assigned water days in the sample. Approximately 22% of those days were labeled cheating incidents.

2.5. Econometric Analysis

The main objective of this analysis is to examine short- and long-run effects of the treatment letters on cheating incidents. We assume that when making the decision to water on a non-assigned day, the household weighs the costs and benefits. The benefit of watering is the utility received from a greener lawn, but that is a function of current lawn and soil conditions, weather variables, such as temperature, wind speed, and precipitation, the cost of water and other lawn care products, and the value of time and effort. Additionally, self-imposed guilt and socially-imposed embarrassment can be seen as a potential cost of noncompliance (Grasmick and Bursik, 1990). The magnitude of disutility from these costs depends on the household's sense of moral and social obligation. The treatment letters may increase costs of noncompliance if they increase this sense of moral and social obligation. We model the net indirect utility of cheating as an unobserved latent variable y^* where

$$
\begin{aligned}
&y_{it}^{\ *} = \mathbf{x}_{it}'\boldsymbol{\beta} + \mathbf{z}_{\mathbf{it}}'\boldsymbol{\gamma} + u_i + \varepsilon_{it}, \quad i = 1,\ldots,N, \ \ t = 1,\ldots,T_i, \\
&\mathbf{x}_{it}'\boldsymbol{\beta} = \beta_0 + \sum_{j=1}^{4}\beta_{L_j}L_j + \beta_{I07}I07 + \sum_{j=1}^{4}\beta_{I07L_j}(I07 * L_j) \\
&\qquad\quad + \beta_{I08}I08 + \sum_{j=1}^{4}\beta_{I08L_j}(I08 * L_j), \\
&\mathbf{z}_{\mathbf{it}}'\boldsymbol{\gamma} = \gamma_1 age + \gamma_2 lnvalue + \gamma_3 lnland + \gamma_4 grwdy + \gamma_5 maxspd.
\end{aligned}
\tag{2.1}
$$

In (2.1) L_j denotes that the household received an intervention letter of type *j*. Specifically,

$L1$ = 1 if household received the schedule letter

L_2 = 1 if household received the drought letter

L_3 = 1 if household received the capacity letter

L_4 = 1 if household received the monitoring letter.

The variable *I07* equals one if the observation occurred during the 2007 post-intervention period, $t = 38,...,69,$ and zero otherwise, and *I08* equals one if the observation occurred during the 2008 post-intervention period, $t = 70,...,126,$ and zero otherwise. Variables that control for observed lot and building characteristics and weather are the age of the house (a*ge*), the log of the 2007 assessed property value (*lnvalue*) as a proxy for income, the log of lot size in square feet (*lnland*) as a proxy for irrigable area, growing degree days[10] in Fahrenheit (*grwdy*), and daily maximum sustained wind speed in knots (*maxspd).* Precipitation is not included because very little precipitation fell during the two summers. Drought conditions persisted throughout 2007 and 2008. A price variable

[10] Growing degree days = (maximum daily temperature – minimum daily temperature)/2 – 50.

is also excluded from the model due to lack of variation over time and between households. Unobserved household specific heterogeneity is denoted by u_i, and captures differences in households that are unobservable to the researcher, such as landscape characteristics and household preferences for a green lawn, but which may nonetheless affect the propensity to cheat. It is important to control for this, since failure to do so may lead to biased coefficients (Heckman, 1981). And lastly, ε_{it} is the observation specific error term.

Parameter β_0 represents the control group's unobserved net indirect utility from cheating in the pre-intervention period. The time dummies β_{I07} and β_{I08} capture aggregate factors that would cause changes in y^* even in the absence of a policy change. The group dummies β_{Lj} capture possible differences between the treatment and control groups prior to the policy change. The key idea is that the cross-sectional perspective, comparing the treated group to the control group, accounts for any intertemporal differences that may occur, such as in temperature and precipitation, while the longitudinal perspective, the difference between the pre- and post-intervention outcomes for each group, controls for any permanent differences between those groups. The coefficients of the interaction of β_{I07L_j} and β_{I08L_j}, time period and treatment group indicators, reflect the changes in the treatment group relative to the control group that are due to the treatment.

Of course, the utility of lawn watering on a non-assigned day is unobserved; we only observe whether the household cheated. Therefore, our observation is

$$\begin{aligned} y &= 1 \quad \text{if } y^* > 0, \\ y &= 0 \quad \text{if } y^* \leq 0. \end{aligned} \tag{2.2}$$

Estimation proceeds along the lines of probabilistic statements. Here, the probabilities of household i cheating is

$$\Pr\left[y_{it}=1\right] = \Pr\left[\mathbf{x}'_{it}\boldsymbol{\beta}+\mathbf{z}'_{\mathbf{it}}\boldsymbol{\gamma}+u_i+\varepsilon_{it}>0\right]=\Pr\left[\varepsilon_{it}>-\mathbf{x}'_{it}\boldsymbol{\beta}-\mathbf{z}'_{\mathbf{it}}\boldsymbol{\gamma}-u_i\right]=1-F(-\mathbf{x}_{\mathbf{it}}\boldsymbol{\beta}-\mathbf{z}'_{\mathbf{it}}\boldsymbol{\gamma}-u_i) \tag{2.3}$$

where F denotes some cumulative distribution function.

We specify a standard normal distribution, $N(0,1)$ for the observation specific error term ε_{it}, and a normal distribution, $N(0,\sigma_\mu^2)$, for the household specific random effect u_i. Furthermore, ε_{it} is assumed independent across all periods and all households, and u_i is assumed independent across individuals, time invariant, and uncorrelated with the elements of $\mathbf{x}_i$ and $\mathbf{z}_i$ in all periods. This is the specification of the random-effects Probit model.[11] The marginal probability of a positive outcome is

$$\Pr[y_{it}=1]=\Phi\left(\frac{\mathbf{x}'_{\mathbf{it}}\boldsymbol{\beta}+\mathbf{z}'_{\mathbf{it}}\boldsymbol{\gamma}}{\sqrt{1+\sigma_u^2}}\right) \tag{2.4}$$

where Φ is the standard normal cumulative density function. Thus the conditional probability that $y=1$ given the group and time period, or the conditional mean of y, can be calculated as follows:

<u>Pre-intervention period</u>

$$E\left[y=1 \mid C=1, L_j=0, I07=0, I08=0\right]=\Phi\left(\frac{\beta_0+\overline{\mathbf{Z}}'\boldsymbol{\gamma}}{\sqrt{1+\sigma_u^2}}\right) \tag{2.5}$$

---

[11] We use random-effects instead of fixed-effects because there does not exist a sufficient statistic that allows the fixed effects to be conditioned out of the Probit likelihood (Greene, 2003). Furthermore, some explanatory variables are time invariant, and thus cannot be included in a fixed-effects model due to collinearity with the fixed effects.

$$E\left[y=1 \mid C=0, L_j=1, I07=0, I08=0 \right] = \Phi\left(\frac{\beta_0 + \beta_{L_j} + \bar{\mathbf{Z}}'\boldsymbol{\gamma}}{\sqrt{1+\sigma_u^2}} \right) \tag{2.6}$$

<u>2007 post-intervention period</u>

$$E[y=1 \mid C=1, L_j=0, I07=1, I08=0] = \Phi\left(\frac{\beta_0 + \beta_{I07} + \bar{\mathbf{Z}}'\boldsymbol{\gamma}}{\sqrt{1+\sigma_u^2}} \right) \tag{2.7}$$

$$E[y=1 \mid C=0, L_j=1, I07=1, I08=0] = \Phi\left(\frac{\beta_0 + \beta_{L_j} + \beta_{I07} + \beta_{I07L_j} + \bar{\mathbf{Z}}'\boldsymbol{\gamma}}{\sqrt{1+\sigma_u^2}} \right) \tag{2.8}$$

<u>2008 post-intervention period</u>

$$E[y=1 \mid C=1, L_j=0, I07=1, I08=1] = \Phi\left(\frac{\beta_0 + \beta_{I08} + \bar{\mathbf{Z}}'\boldsymbol{\gamma}}{\sqrt{1+\sigma_u^2}} \right) \tag{2.9}$$

$$E[y=1 \mid C=0, L_j=1, I07=1, I08=0] = \Phi\left(\frac{\beta_0 + \beta_{L_j} + \beta_{I07} + \beta_{I07L_j} + \bar{\mathbf{Z}}'\boldsymbol{\gamma}}{\sqrt{1+\sigma_u^2}} \right) \tag{2.10}$$

where $C=1$ indicates the control group and $\bar{\mathbf{Z}}'\boldsymbol{\gamma}$ is $\mathbf{Z}'\boldsymbol{\gamma}$ calculated at the mean values of $\mathbf{Z}$.

The coefficients estimated by the Probit model cannot be directly interpreted as marginal effects, hence β_{I07L_j} and β_{I08L_j}, the coefficients of the interaction of the time period and treatment group indicators are not direct estimates of the treatment effect. However, the literature is divided on the proper calculation of the treatment effect. Ai and Norton (2003) argue that the treatment effect is equal to the interaction effect. For an interaction of two binary regressors this is the cross difference of the conditional expectation:

$$\frac{\Delta^2 E\left[y \mid I07, L_j, \mathbf{Z}\right]}{\Delta I07 \Delta L_j} =$$

$$\begin{aligned}
&\Big[E[y=1 \mid C=0, L_j=1, I07=1, I08=0] \\
&-E[y=1 \mid C=0, L_j=1, I07=0, I08=0]\Big] \\
&-\Big[E[y=1 \mid C=1, L_j=0, I07=1, I08=0] \\
&-E[y=1 \mid C=1, L_j=0, I07=0, I08=0]\Big] \\
&=\left[\Phi\left(\frac{\beta_0+\beta_{I07}+\beta_{L_j}+\beta_{I07L_j}+\bar{\mathbf{Z}}'\boldsymbol{\gamma}}{\sqrt{1+\sigma_u^2}}\right)-\Phi\beta_0\left(\frac{\alpha+\beta_{L_j}+\bar{\mathbf{Z}}'\boldsymbol{\gamma}}{\sqrt{1+\sigma_u^2}}\right)\right] \\
&-\left[\Phi\left(\frac{\beta_0+\beta_{I07}+\bar{\mathbf{Z}}'\boldsymbol{\gamma}}{\sqrt{1+\sigma_u^2}}\right)-\Phi\left(\frac{\beta_0+\bar{\mathbf{Z}}'\boldsymbol{\gamma}}{\sqrt{1+\sigma_u^2}}\right)\right]
\end{aligned} \tag{2.11}$$

$$\frac{\Delta^2 E\left[y \mid I08, L_j, \mathbf{Z}\right]}{\Delta I08 \Delta L_j} =$$

$$\begin{aligned}
&=\Big[E[y=1 \mid C=0, L_j=1, I07=0, I08=1] \\
&-E[y=1 \mid C=0, L_j=1, T07=0, T08=0]\Big] \\
&-\Big[E[y=1 \mid C=1, L_j=0, I07=0, I08=1] \\
&-E[y=1 \mid C=1, L_j=0, I07=0, I08=0]\Big] \\
&=\left[\Phi\left(\frac{\beta_0+\beta_{I08}+\beta_{L_j}+\beta_{I08L_j}+\bar{\mathbf{Z}}'\boldsymbol{\gamma}}{\sqrt{1+\sigma_u^2}}\right)-\Phi\left(\frac{\beta_0+\beta_{L_j}+\bar{\mathbf{Z}}'\boldsymbol{\gamma}}{\sqrt{1+\sigma_u^2}}\right)\right] \\
&-\left[\Phi\left(\frac{\beta_0+\beta_{I08}+\bar{\mathbf{Z}}'\boldsymbol{\gamma}}{\sqrt{1+\sigma_u^2}}\right)-\Phi\left(\frac{\beta_0+\bar{\mathbf{Z}}'\boldsymbol{\gamma}}{\sqrt{1+\sigma_u^2}}\right)\right]
\end{aligned} \tag{2.12}$$

Puhani (2008) takes a different approach. He argues that the treatment effect is the difference between two cross differences: it is the cross difference of the conditional expectation of the <u>observed</u> outcome with treatment minus the cross difference of the conditional expectation of the <u>potential</u> outcome without treatment. For example, the treatment effect of L_j in the 2007 post-intervention period is

$$
\begin{aligned}
TE_{j07} &= \frac{\Delta^2 E\left[y \mid I07, L_j\right]}{\Delta I07 \Delta L_j} - \frac{\Delta^2 E\left[y^0 \mid I07, L_j\right]}{\Delta I07 \Delta L_j} \\
&= \left[\Phi\left(\frac{\beta_0 + \beta_{I07} + \beta_{L_j} + \beta_{I07L_j} + \bar{\mathbf{Z}}'\boldsymbol{\gamma}}{\sqrt{1+\sigma_u^2}}\right) - \Phi\left(\frac{\beta_0 + \beta_{L_j} + \bar{\mathbf{Z}}'\boldsymbol{\gamma}}{\sqrt{1+\sigma_u^2}}\right)\right] \\
&\quad - \left[\Phi\left(\frac{\beta_0 + \beta_{I07} + \bar{\mathbf{Z}}'\boldsymbol{\gamma}}{\sqrt{1+\sigma_u^2}}\right) - \Phi\left(\frac{\beta_0 + \bar{\mathbf{Z}}'\boldsymbol{\gamma}}{\sqrt{1+\sigma_u^2}}\right)\right] \\
&\quad - \left[\Phi\left(\frac{\beta_0 + \beta_{I07} + \beta_{L_j} + \bar{\mathbf{Z}}'\boldsymbol{\gamma}}{\sqrt{1+\sigma_u^2}}\right) - \Phi\left(\frac{\beta_0 + \beta_{L_j} + \bar{\mathbf{Z}}'\boldsymbol{\gamma}}{\sqrt{1+\sigma_u^2}}\right)\right] \\
&\quad - \left[\Phi\left(\frac{\beta_0 + \beta_{I07} + \bar{\mathbf{Z}}'\boldsymbol{\gamma}}{\sqrt{1+\sigma_u^2}}\right) - \Phi\left(\frac{\beta_0 + \bar{\mathbf{Z}}'\boldsymbol{\gamma}}{\sqrt{1+\sigma_u^2}}\right)\right] \\
&= \Phi\left(\frac{\beta_0 + \beta_{I07} + \beta_{L_j} + \beta_{I07L_j} + \bar{\mathbf{Z}}'\boldsymbol{\gamma}}{\sqrt{1+\sigma_u^2}}\right) - \Phi\left(\frac{\beta_0 + \beta_{I07} + \beta_{L_j} + \bar{\mathbf{Z}}'\boldsymbol{\gamma}}{\sqrt{1+\sigma_u^2}}\right) \qquad (2.13)
\end{aligned}
$$

This is the marginal effect of the interaction term, the difference in conditional expectation when $\beta_{I07L_j} = 1$ and $\beta_{I07L_j} = 0$. Likewise the treatment effect of L_j in the 2008 post-intervention period is

$$
TE_{j08} = \Phi\left(\frac{\beta_0 + \beta_{I08} + \beta_{L_j} + \beta_{I08L_j} + \bar{\mathbf{Z}}'\boldsymbol{\gamma}}{\sqrt{1+\sigma_u^2}}\right) - \Phi\left(\frac{\beta_0 + \beta_{I08} + \beta_{L_j} + \bar{\mathbf{Z}}'\boldsymbol{\gamma}}{\sqrt{1+\sigma_u^2}}\right) \qquad (2.14)
$$

Both methods produced similar results, thus we will only report the treatment effect as defined by Puhani, (2.13) and (2.14).

Lastly, for purposes of identifying cheating incidents we use all observations in the "cleaned" sample. In the econometric analysis we only use non-assigned watering days (a cheating incident cannot occur on an assigned day). Additionally, the treatment letters were mailed during the week of July 21-27, 2007. Observations occurring during this week were omitted from the final data set to assure that all households had received their letters by the start of the intervention period. This leaves 389,955 observations; 127,261 in the pre-intervention period, 111,486 in the 2007 post-intervention period, and 151,208 in the 2008 post-intervention period. The average number of observations per household is 66.5, with a minimum of 29 and a maximum of 82.

2.6. Results

The regression results are shown in Table 2.3. First, note that during the pre-intervention period the treatment group parameters are not significantly different than zero; all groups are equally likely to cheat. The probability of cheating is negatively associated with the age of the house (*age*) and the log of the property value (*lnval*). The lot size (*lnland*) did not have a significant effect. The probability of cheating is also negatively associated with wind speed (*maxspd*). On windy days, the benefit of sprinkler use declines because the water tends to be blown away. On the other hand, growing degree days (*grwdy*) is positively associated with the probability of cheating. As heat accumulates, lawns require more water.

The treatment effects along with standard errors and 95% confidence intervals are given in Table 2.4, and are graphically displayed in Figure 2.3. All letters significantly reduced the probability of cheating in the 2007 post-interaction period, from 0.0167 to 0.0358 with the L4 (monitoring) letter having the largest point estimate. The point estimates for the L1 (schedule), L2 (drought), and L3 (capacity) treatment effects are very similar. Adding the additional drought or capacity message to the letter did not significantly reduce the probability of cheating in the 2007 post-intervention period beyond that of receiving just the schedule.

Looking at the treatment effects in the 2008 post-intervention period, we find that the schedule letter was not effective in reducing the probability of cheating, yet the other treatment effects remained significant. Moreover, the effects of the drought and capacity letters in 2008 diminished slightly to those in 2007, dropping from 0.0167 and 0.0209 respectfully in 2007 to 0.0141 and 0.0178 in 2008. Thus adding the additional drought and capacity message to the letter did not make a difference in the short term, but did in the long term. The effect of the monitoring letter, while persisting in significance, was much weaker in 2008, dropping to 0.0135. Hence it appears that the threat of getting caught cheating has a very strong effect in the short term, but starts to fade in the long term.

2.7. Conclusion and Policy Implications

In this chapter, we investigate the short- and long-run impact of conservation campaigns on compliance with outdoor water restrictions. A unique natural field experiment was conducted on households in Reno, Nevada during the summer of 2007 to examine the impact of non-price driven conservation campaigns on residential water consumption. Follow-up monitoring was done during the summer of 2008.

In order to determine the causal impact of the letters on compliance, we first identify noncompliance, or cheating incidents, from the household daily consumption obtained during monitoring. We use K-means cluster analysis to accomplish this. Since water use varies greatly between households, the clustering algorithm is performed separately on each household, clustering on daily consumption. We found that households on average cheat approximately 22% of the time.

After identifying cheating incidents, we use a random effects Probit model to examine short- and long-run effects of the treatment letters on the probability of cheating. The results suggest that conservation campaigns can significantly increase compliance in both the short and long term. All treatments significantly reduced the probably of cheating in the 2007 post-intervention period, with the monitoring letter having the strongest impact. The effect of the schedule letter did not persist in 2008. The monitoring letter had the largest effect in 2007 reducing the probability of cheating by 0.0358, yet the effect diminished in 2008 to 0.0135. The threat of being caught appears to be a powerful incentive to comply with restrictions in the short run. As time goes on, the threat becomes less credible and the rate of compliance decreases. If water utilities choose this type of campaign, they need to back the threat up in order to see lasting results. On the other hand, the impact of the drought and capacity letters from 2007 to 2008 saw a very slight decrease, from 0.0167 and 0.0209 respectfully in 2007 to 0.0141 and 0.0178 in 2008. Perhaps having a better understanding of the reasoning behind the watering restrictions caused a lasting change in behavior. Information derived from this research can be used by water utilities to improve the effectiveness of compliance programs and to design future strategies for demand management.

Table 2.1. Distribution of Treatments

	Control	L1	L2	L3	L4	Total
2007 Households	1,291	1,196	1,150	1,149	1,168	5,954
2008 Households	798	756	700	717	717	3,688

Table 2.2. Results of *K*-means Clustering

Treatment Group	Watering Days	Non-Watering Days	Percent Watering Days	Cheating Incidents	Percent Cheating Incidents
L1	42,407	73,265	36.66%	17,820	21.45%
L2	41,084	68,512	37.49%	17,817	22.63%
L3	41,097	69,550	37.14%	17,948	22.58%
L4	40,956	70,766	36.66%	16,761	20.89%
Control	46,391	77,338	37.49%	20,425	22.97%
Total	211,935	359,431	37.11%	90,771	22.12%

Table 2.3. Estimation Results from Random Effects Probit Model

Variable	Coeff.	SE
constant	1.5419***	(0.4004)
L1	-0.0164	(0.0421)
L2	0.0473	(0.0425)
L3	0.0530	(0.0425)
L4	0.0081	(0.0424)
I07	-0.0080	(0.0140)
I07*L1	-0.1013***	(0.0204)
I07*L2	-0.0765***	(0.0204)
I07*L3	-0.0961***	(0.0204)
I07*L4	-0.1729***	(0.0205)
I08	-0.1251***	(0.0142)
I08*L1	0.0281	(0.0202)
I08*L2	-0.0689***	(0.0205)
I08*L3	-0.0875***	(0.0204)
I08*L4	-0.0676***	(0.0206)
age	-0.0073***	(0.0009)
lnval	-0.2457***	(0.0414)
lnland	0.0538	(0.0330)
grwd	0.0068***	(0.0007)
wind	-0.0056***	(0.0008)
Log LH	-164,382.99	
σ_u	0.9684	
Rho	0.4839	
Likelihood ratio test of Rho = 0:		
chibar2(01) = 8100, Prob ≥ chibar2 = 0.00		

Notes: Significance levels of 0.01, 0.05, and 0.10 are denoted by three, two, and one asterisks (***, **, *) respectively.

Table 2.4. Impact of Treatment Letters

		Treatment Effects			
		Coef.	Std. Err.	(95% Conf. Interval)	
2007	L1	-0.0211***	0.0065	-0.0338	-0.0084
	L2	-0.0167***	0.0057	-0.0278	-0.0055
	L3	-0.0209***	0.0063	-0.0333	-0.0085
	L4	-0.0358***	0.0093	-0.0541	-0.0175
2008	L1	0.0057	0.0043	-0.0027	0.0141
	L2	-0.0141***	0.0054	-0.0246	-0.0035
	L3	-0.0178***	0.0060	-0.0295	-0.0061
	L4	-0.0135**	0.0053	-0.0239	-0.0031

Notes: Significance levels of 0.01, 0.05, and 0.10 are denoted by three, two, and one asterisks (***, **, *) respectively.

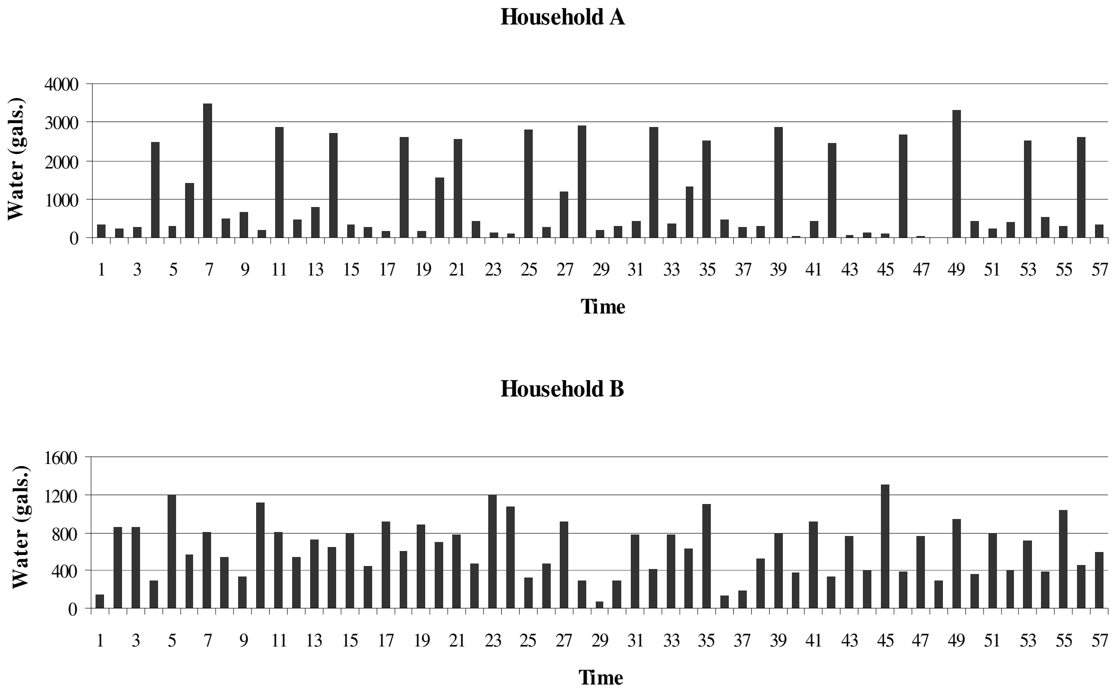

Figure 2.1. Examples of Daily Water Consumption during 2008

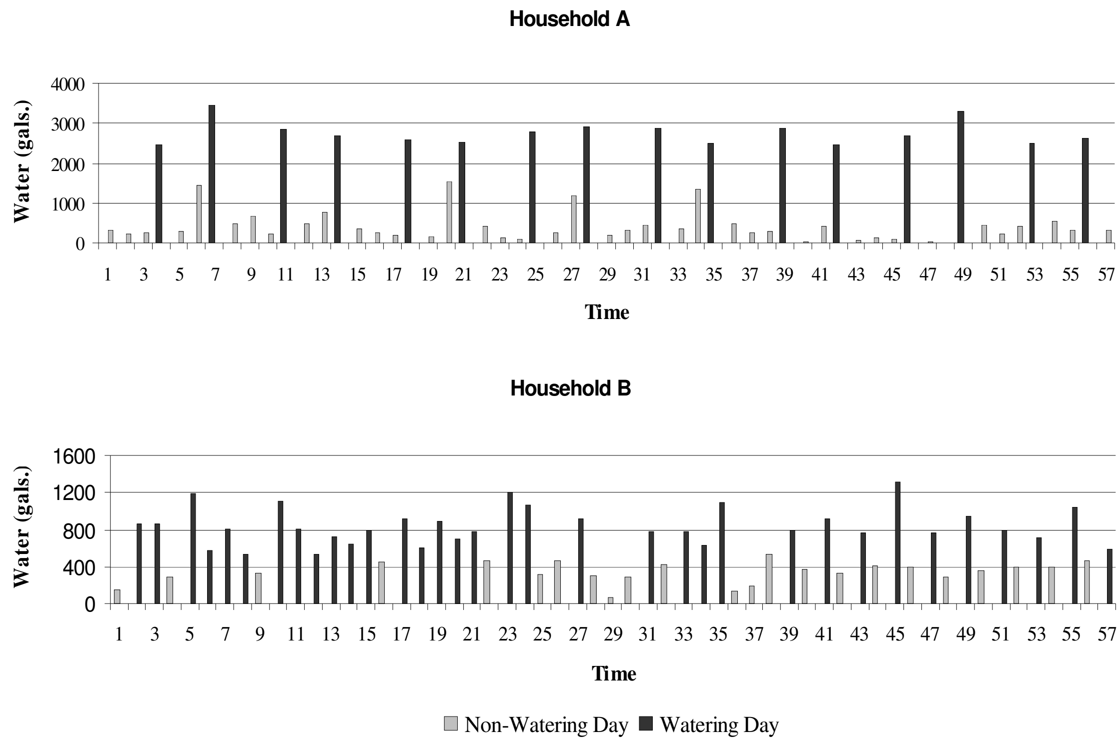

Figure 2.2. Example of Daily Water Consumption during 2008 with Non-watering and Watering Days Classified by *K*-Means Clustering

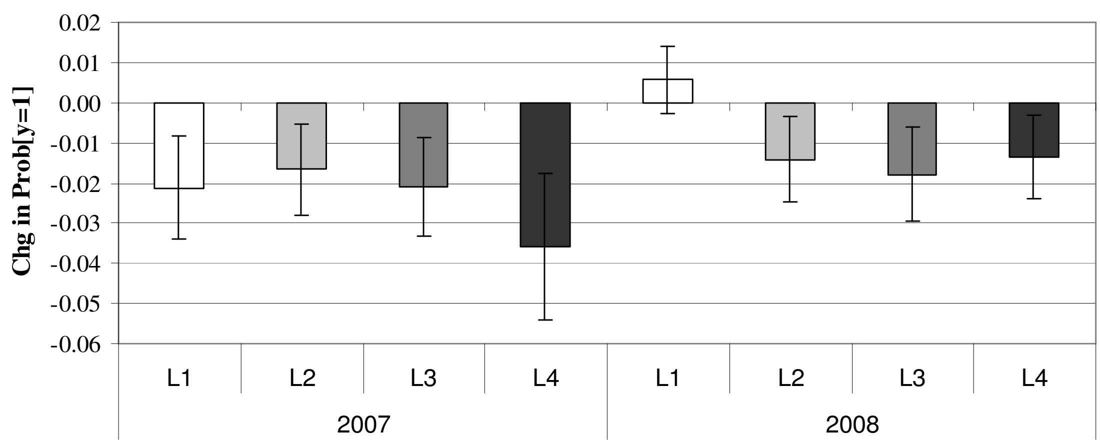

Figure 2.3. Treatment Effects in Each Post-Intervention Period with 95% Confidence Intervals

3. Free to Choose: Promoting Conservation by Relaxing Outdoor Watering Restrictions

3.1. Introduction

Outdoor watering restrictions have shown promise as an effective Demand Side Management (DSM) tool to promote conservation amongst residential customers (Renwick and Archibald, 1998, Renwick and Green, 2000, Shaw and Maidment, 1987). While originally implemented as temporary measures to cope with extreme drought events in the western United States and other arid regions around the world, residential irrigation restrictions are increasingly adopted by municipalities as a standard and recurrent 'best practice' regulatory mechanism to curb consumption (e.g. Brennan et al., 2007).

There are several reasons for the growing popularity of this use-specific rationing strategy: (i) price-based interventions have shown to be of limited use in inducing conservation due to the price-inelastic nature of water demand, and the lack of households' awareness of the billing system or even their own monthly water bill (Griffin and Mjelde, 2000, Renwick and Archibald, 1998, Worthington and Hoffman, 2008), (ii) there appear to be fewer equity concerns and thus less political resistance to targeted use restrictions compared to price-based interventions and general allocation quotas (Renwick and Archibald, 1998, Brennan et al., 2007) (iii) utilities and regulatory commissions have come to realize that historic reliability and supply targets based on unrestricted demand may have been inflated from an economic efficiency perspective (Howe and Smith, 1994, Griffin and Mjelde, 2000, Hensher et al., 2006), and (iv) growing concerns about global warming and related rising costs of water provision may lead to further reduction in the planning capacity of water supply systems, thus increasing the pressure on utilities to achieve conservation via targeted non-price DSM tools.

Despite the growing importance of outdoor watering restrictions (OWRs) as a DSM intervention, surprisingly little is known about the relative performance of different OWR implementation strategies. In this study we shed some light on this issue by examining the effect of different weekly patterns of outdoor watering events on weekly water use and use peaks at the household level. We find that households that exhibit some flexibility in their weekly watering pattern use, on average, less water and have lower daily use peaks than households that adhere to a rigid watering schedule. This has important implications for optimal OWR designs.

Our analysis poses several econometric challenges. Since a customer's decision on how often to water and how much water to use per watering event are intrinsically related, we combine a truncated integer model for the decision on the number of weekly watering days with two continuous consumption models. The latter capture, respectively, total weekly use and weekly peak (i.e. the highest daily use in a given week). To answer our primary research questions we employ the observed number of weekly watering events and related information on watering patterns as explanatory variables in the two consumption equations. Since there are likely unobserved household effects and potentially even household-week specific effects that enter the three model components, we need to explicitly capture correlation patterns for these unobservables to avoid endogeneity bias.

We address these challenges via a full-information hierarchical Bayesian framework. Thus, on the econometric side, this study contributes to the growing literature on Bayesian modeling and Markov-Chain Monte Carlo simulations to tackle inter-related multi-equation systems with limited dependent variables and selection or endogeneity components, which would be cumbersome to estimate in a classical parametric framework.

In the next section we discuss in more detail existing OWR strategies and related literature. Section III introduces the modeling framework. This is followed by an empirical section that introduces the data, describes the OWR regulations underlying our application, and discusses estimation results. Section V concludes.

3.2. Outdoor Watering Restrictions

As discussed above, OWRs have been implemented in many areas within and outside the United States. For example, Brennan et al. (2007) describe how OWRs have become the norm in many Australian cities, impacting a large majority of residential customers. Table 3.1 depicts a sample list of U.S. cities that are currently under OWR regimes. As captured in the right column of the table, these OWRs include a limitation on the number of watering days in a given week, two or three in most cases. For most municipalities the permitted watering days are assigned days-of-week, usually based on the street address of a given residence. This strategy is designed to avoid excessive consumption peaks on any given day-of-week (DoW).[12] Most OWRs operate under a ban

[12] However, as illustrated in Shaw and Maidment (1987), some trial and error may still be needed to arrive at address-assignment mappings that do not induce undesired weekly cycles in daily use.

on lawn sprinklers on non-assigned days, but allow non-sprinkler watering (e.g. via a hand-held hose) on a daily basis. In virtually all cases, watering of any kind is prohibited during the hottest hours of the day.

To date, economists have primarily focused on two aspects of OWR policies: (i) the overall effectiveness of OWRs compared to an unrestricted baseline, and (ii) the welfare effects of OWRs on consumers. For example, using a daily time series of system-wide consumption during the 1984-85 drought years in Austin, Texas, Shaw and Maidment (1987) find that a one-per-five days watering restriction reduced overall demand by 3-5%. Renwick and Archibald (1998) report a reduction in water use by 16% for residential customers in two Southern Californian communities following a strictly enforced total ban on virtually all landscape irrigation except for hand irrigation and drip systems during the 1985-92 drought in that region. Their findings are based on a survey of 119 households and their monthly water use over this six-year period. Based on a cross-sectional study of system-wide monthly consumption in eight California water utilities over the same time horizon, Renwick and Green (2000) find that OWRs of a general nature (including bans on washing cars and other non-irrigation related outdoor use) reduced consumption by close to 30% compared to an unrestricted situation.

The second set of studies with focus on welfare implications of OWRs and other drought-related water use restrictions largely employ non-market valuation techniques to elicit households' willingness-to-pay (WTP) to avoid such restrictions (Griffin and Mjelde, 2000, Hensher et al., 2006), or a reduction in future reliability (i.e. an increased risk of future restrictions) (Howe Smith, 1994), Griffin and Mjelde, 2000). In contrast, Brennan et al. (2007) model the consumer problem under OWRs as a trade-off between the production of 'green lawn' via hand-held watering devices and leisure time. Using scientific input on lawn appearance under different watering regimes and calibration techniques they simulate a household's optimal watering decision under different parameter settings for lawn production and consumer preferences.

While conceptually attractive, the Brennan et al. (2007) model remains to be subjected to empirical verification using actual field data. Griffin and Mjelde (2000) do not specify any details on OWR in their water shortage scenarios. This leaves Hensher et al. (2006) as the most relevant welfare study for our purpose. They specify very detailed restriction scenarios in their choice experiment, varying by frequency, duration, and days-of-week implementation regimes. Their main finding from the perspective of this study is that households have near-zero WTP to avoid OWRs that still allow sprinkler use on several days per week, as do most regimes currently in place in the U.S. and Australia.

While there is undoubtedly room for additional valuation studies that examine the welfare implications associated with OWR's, the available evidence suggests that the bulk of net economic gains flowing from standard OWRs may well be related to water conservation and related cost savings on the production side. It is therefore quintessential to understand how different OWR regimes affect conservation outcomes. Returning to Table 3.1, why is it that some cities allow sprinkler use on three days per week, while others have implemented a twice-a-week schedule? In the same vein, why are sprinkler days pegged to specific days-of-week in most cases, while some communities allow households to choose their own implementation days?[13]

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[13] Examples are the community of Del Mar near San Diego, CA, and the Suwannee River Water Management District in Florida.

Surprisingly, the existing literature offers no guidance on the optimal implementation of OWRs. Naturally, from a welfare perspective, any regime that reaches a given conservation objective with fewer restrictions on household activities will be Pareto-superior to a more restrictive version. However, as we will show in this study, it is a fallacy to believe that more stringent restrictions on the household end always produce better conservation outcomes. It appears that giving customers some “freedom to choose” can actually enhance conservation.

3.3. Modeling Framework

3.3.1 Conceptual model and consumer types

Consider an individual *i* who for a given week *p* has to decide on the number of outdoor watering days, y_{1ip}, and the water amount per watering day. This determines total weekly consumption, y_{2ip}, and weekly peak, y_{3ip}. In alignment with our empirical application, we further assume that OWRs with address-based DoW assignments exist, but are loosely enforced. As a result, the degree of compliance may vary across individuals. Hand watering is permitted on any day. Assume that the individual has identified an optimal level of 'greenness' or 'yard health', perhaps following the decision process suggested in Brennan et al. (2007). We interpret this level as being associated with the amount of water stored in the yard's soil. If water storage falls below $q^*_{\min}$ lawn 'greenness' will be negatively affected.

For simplicity, assume for now that hand watering is not an option due to binding time constraints or an unfavorable ratio of the marginal gain from hand watering to the marginal value of additional leisure time (Brennan et al., 2007). Further assume that the customer is risk-averse and prefers to maintain the water storage level at $q^*_{\max}$. The customer will then set an automated sprinkler system to maintain the water storage level at or above $q^*_{\max}$ while accounting for two sources of losses that occur with each watering event: direct wind and overspray losses due to missing the targeted irrigation area, and subsequent losses due to evaporation, runoff, and drainage.

This situation is visualized in Figure 3.1 for two twice per week watering scenarios. In the first case the resident fully complies with her OWRs which, to mirror our empirical application, assigns Wednesday and Saturday as designated watering days for her address. Anticipated wind and overspray losses are depicted as a_{1c} and a_{2c}, respectively for the two watering events. Evaporation, runoff, and drainage losses are labeled as b_{1c} and b_{2c}. Only the amounts c_{1c} and c_{2c} are actually absorbed by the yard's biomass. All losses and plant consumption are assumed to be correctly anticipated by the consumer such that water storage levels, indicated by the dashed line, decline exactly to $q^*_{\max}$ by the time of the next designated watering event. We have drawn $a_{2c} > a_{1c}$, $b_{2c} > b_{1c}$, and $c_{2c} > c_{1c}$ to acknowledge that the Saturday watering has to bridge a longer time period than the Wednesday event. In summary, the top graph of Figure 3.1 depicts the typical watering week of a fully compliant household.

Additional water conservation gains may be possible by relaxing the assigned DoW constraint, while maintaining a twice per week watering frequency. For example, consider an individual that cancels and postpones watering whenever direct wind losses would exceed a maximum tolerable amount. Such a case is shown in the bottom graph of Figure 3.1, where the Saturday watering event has been moved to Sunday. Such lossaverting behavior has three effects compared to the full-compliance scenario: (i) wind / overspray losses are smaller for the second watering day, (ii) evaporation / runoff / drainage losses for the second event are smaller due to changes in the spacing of events, and (iii) ground storage falls below the original level $q^*_{\max}$ for some time during the week, perhaps as low as $q^*_{\min}$, due to additional plant consumption and losses compared to the ex-ante expected amounts. Assuming that the risk-averse individual wants to maintain water storage at $q^*_{\max}$, the resulting shortfall has to be met through additional watering on Sunday. Thus, b_{2f} or c_{2f} might be larger than b_{2c} or c_{2c} despite the shortened time span between the second watering of this week and the first application of the next week. However, as long as $\left(\mathrm{a}_{2f}+b_{2f}+c_{2f}\right)<\left(\mathrm{a}_{2c}+b_{2c}+c_{2c}\right)$ the flexible scenario will save water without making the household worse off.

A similar scenario is depicted in Figure 3.2, but now the assigned watering days are on Thursday and Sunday. In the upper half, the resident fully complies with her OWRs over a two-week period. As before anticipated wind / over spray losses during the first week are depicted as a_{1c} and a_{2c}. In the lower half, the resident chooses to postpone the second watering until Monday. Once more, as long as $\left(\mathrm{a}_{2f}+b_{2f}+c_{2f}\right)<\left(\mathrm{a}_{2c}+b_{2c}+c_{2c}\right)$ the flexible scenario will save water even with three watering days in the second week.

Figure 3.3 compares a two times per week watering scenario with a three times per week scenario. The household in the upper half is concerned about complying, but not operating efficiently. She sets her automatic sprinklers without regard to wind, overspray, evaporation, runoff, and evaporation losses. In contrast, the household in the lower panel sets his system to water more efficiently. The sprinklers run at night when temperatures are cooler and there is less wind. He allows the sprinklers to run just long enough that the water starts to puddle, then allows the water to soak into the ground. This cycle is repeated a few times during the night to allow the soil to absorb as much water as possible while minimizing runoff and drainage losses. He also has a buffer between the lawn and impervious surfaces, such as the sidewalk, to avoid overspray. Finally, he recently adjusted the timing of the sprinklers according to current evapotranspiration rates and other climate indicators. Compared to the twice per week case less water can be allotted to wind, overspray, evaporation, runoff and evaporation losses, and thus conservation is achieved.

Naturally, the second scenario in Figure 3.3 is based on the assumption that the customer has a profound understanding of his yard's water needs. This may not be the case for all households. In addition, the second scenarios in Figures 3.1, 3.2, and 3.3 assume that the individual is willing to (i) break OWR rules, and (ii) make efficiency-enhancing adjustments to the watering system. Again, households will likely differ in that respect. Furthermore, some households may augment sprinkler watering with hand watering to further fine-tune water application to their yard's need. Thus, there is a strong argument for taking an empirical route to determine the relationship between watering frequency and consumption.

3.3.2 Econometric Modeling Framework

Our model builds on three equations with observed outcomes y_{1ip}, y_{2ip}, and y_{3ip}. The first outcome takes the form of an integer that is naturally truncated from above at U=7. The remaining outcomes are continuous with support over $\mathbb{R}^+$. Each equation also includes a household-specific unobserved effect that is invariant over the entire watering season $p = 1, \ldots, P$, plus an household / week-specific error term. The household effect primarily captures unobserved landscape characteristics (and thus irrigation needs), and household preferences for a lush vegetation cover, in the spirit of Brennan et al. (2007). The idiosyncratic error targets behavioral interventions, such as adjustments to the sprinkler cycle or decisions to hand-water, which may vary across households and time.

To incorporate these modeling considerations in a computationally tractable fashion we combine a truncated Poisson-lognormal (PLN) density for the watering frequency equation with two exponential-lognormal (ELN) densities for weekly consumption and peak. The PLN was originally proposed by Aitchison and Ho (1989) as a flexible extension of the Poisson distribution. An application of the ELN, in conjunction with an (untruncated) PLN is given in Munkin and Trivedi (2003). The ELN has similar distributional characteristics as the familiar log-normal regression model, but exhibits more desirable mixing properties in our Bayesian estimation framework. Adding the household effects yields our full specification, which we label the hierarchical truncated Poisson-lognormal / hierarchical exponential-lognormal (HTPLN-HELN) model.

The HTPLN component is given as

$$f\left(y_{1ip} \mid \lambda_{1ip}, 0 \le y_{1ip} \le U\right) = \frac{\exp\left(-\lambda_{1ip}\right)\lambda_{1ip}^{\ \ y_{1ip}}}{y_{1ip}!\left(\sum_{k=0}^{U} \frac{\exp\left(-\lambda_{1ip}\right)\lambda_{1ip}^{\ \ k}}{k!}\right)} = \frac{\lambda_{1ip}^{\ \ y_{1ip}}}{y_{1ip}!\left(\sum_{k=0}^{U} \frac{\lambda_{1ip}^{\ \ k}}{k!}\right)} \tag{3.1}$$

$$E\left[y_{1ip} \mid \mathbf{x_{1ip}}, u_{1i}, \varepsilon_{1ip}\right] = \lambda_{1ip} = \exp\left(\mathbf{x'_{1ip}\beta_1} + u_{1i} + \varepsilon_{1ip}\right)$$

where the log of the untruncated conditional expectation, λ_{1ip}, is a linear function of vector $\mathbf{x_{ip}}$ containing household and climate variables, household-specific effect u_{1i}, and observation-level error ε_{1ip}. The HELN part is specified as

$$\begin{aligned} &f\left(y_{jip} \mid \lambda_{jip}\right) = \lambda_{jip} * \exp\left(-\lambda_{jip} y_{jip}\right) \\ &\lambda_{jip} = \exp\left(-\mathbf{z'_{jip}\psi_j} - \mathbf{d'_{ip}\delta_j} - u_{ji} - \varepsilon_{jip}\right) \\ &E\left[y_{jip} \mid \mathbf{z_{jip}}, \mathbf{d_{ip}}, u_{ji}, \varepsilon_{jip}\right] = \lambda_{jip}^{\ \ -1} = \exp\left(\mathbf{z'_{jip}\psi_j} + \mathbf{d'_{ip}\delta_j} + u_{ji} + \varepsilon_{jip}\right), \qquad j = 2,3 \end{aligned} \tag{3.2}$$

where the **z**-vectors capture again household and climate information, the random terms are as in (3.1) and $E[\cdot]$ denotes the expectation operator. Importantly, vector $\mathbf{d_{ip}}$ comprises a set of U indicator variables, one for each possible value of y_{1ip} that exceeds zero. The element of $\mathbf{d_{ip}}$ corresponding to the observed value of y_{1ip} is set to one, all others to zero. More concisely:

$$\begin{aligned} &d_{ip,k} = 1 && \text{if} \quad y_{1ip} = k \\ &d_{ip,k} = 0 && \text{otherwise} \qquad k = 1 \ldots U \end{aligned} \tag{3.3}$$

Thus, we are allowing the intercept of the logged expectation of $y_{jip}, j = 2,3$, to shift with the observed number of watering days compared to the implicit baseline of zero outdoor watering. This implies a proportional change of $\exp\left(\mathbf{d'_{ip}\delta_j}\right), j = 2,3,$ for the expectation in absolute terms. The importance of allowing each possible outcome in the watering frequency stage to have a separate effect on equations two and three will become apparent in our empirical application.

The model is completed by stipulating a joint density for the household effects and error terms:

$$\begin{aligned} \mathbf{u_i} &= \begin{bmatrix} u_{1i} & u_{2i} & u_{3i} \end{bmatrix}' \sim mvn\left(\mathbf{m_u}, \mathbf{V_u}\right) \\ \boldsymbol{\varepsilon}_{\mathbf{ip}} &= \begin{bmatrix} \varepsilon_{1ip} & \varepsilon_{2ip} & \varepsilon_{3ip} \end{bmatrix}' \sim mvn\left(\mathbf{0}, \boldsymbol{\Sigma}\right) \end{aligned} \tag{3.4}$$

where *mvn* denotes the multivariate normal density, $\mathbf{m_u}$ is a mean vector, and the variance matrices $\mathbf{V_u}$ and $\boldsymbol{\Sigma}$ are ex-ante unrestricted. If either matrix contains non-zero covariances, a naïve model ignoring the linkage across the three equations would be plagued by endogeneity bias.[14]

Letting $\boldsymbol{\beta}_2 = \begin{bmatrix} \boldsymbol{\psi}_2' & \boldsymbol{\delta}_2' \end{bmatrix}'$, $\boldsymbol{\beta}_3 = \begin{bmatrix} \boldsymbol{\psi}_3' & \boldsymbol{\delta}_3' \end{bmatrix}'$, $\boldsymbol{\beta} = \begin{bmatrix} \boldsymbol{\beta}_1' & \boldsymbol{\beta}_2' & \boldsymbol{\beta}_3' \end{bmatrix}'$, $k_1 = \dim\left(\boldsymbol{\beta}_1\right)$, $k_2 = \dim\left(\boldsymbol{\beta}_2\right)$, $k_3 = \dim\left(\boldsymbol{\beta}_3\right)$, $k = k_1 + k_2 + k_3$, $\boldsymbol{\varepsilon}_{\mathbf{i}} = \begin{bmatrix} \boldsymbol{\varepsilon}_{\mathbf{i1}}' & \boldsymbol{\varepsilon}_{\mathbf{i2}}' & \cdots & \boldsymbol{\varepsilon}_{\mathbf{iP_i}}' \end{bmatrix}$, and collecting all outcomes and explanatory data in vector $\mathbf{y}$ and matrix $\mathbf{X}$, respectively, the likelihood function for our model over all individuals $i = 1, \ldots, N$, unconditional on all error components, takes the following form:

[14] Our primary reason for choosing individual random effects over fixed effects is that a substantial proportion of households in our empirical sample always select the same number of watering days throughout the entire time period. This would cause identification problems for the marginal effects of $\mathbf{d_{ip}}$ in the HELN part of the model.

$$p(\mathbf{y} \mid \boldsymbol{\beta}, \boldsymbol{\Sigma}, \mathbf{X}) =$$

$$\prod_{i=1}^{N} \left\{ \begin{array}{l} \displaystyle\int_{\mathbf{u_i}} \left(\prod_{p=1}^{P} \int_{\boldsymbol{\varepsilon}_{\mathbf{ip}}} \left(\frac{\lambda_{1ip}^{\;y_{1ip}}}{y_{1ip}! \left(\sum_{k=0}^{U} \frac{\lambda_{1ip}^{\;k}}{k!} \right)} * \lambda_{2ip} \lambda_{3ip} \exp\left(-\left(\lambda_{2ip} y_{2ip} + \lambda_{3ip} y_{3ip} \right) \right) \right) f\left(\boldsymbol{\varepsilon}_{\mathbf{ip}} \mid \boldsymbol{\Sigma} \right) d\boldsymbol{\varepsilon}_{\mathbf{ip}} \right) \\ f\left(\mathbf{u_i} \mid \mathbf{m_u}, \mathbf{V_u} \right) d\mathbf{u_i} \end{array} \right\} \tag{3.5}$$

Given the N multi-dimensional integrals over $\mathbf{u_i}$ and $n = N * P$ multi-dimensional integrals over $\boldsymbol{\varepsilon}_{\mathbf{ip}}$ this model would be challenging to estimate using conventional Maximum Likelihood procedures. We therefore embark on a Bayesian estimation path, starting with the specification of prior distributions for our primary model parameters $\boldsymbol{\beta}$, $\mathbf{m_u}$, $\mathbf{V_u}$, and $\boldsymbol{\Sigma}$.[15]

We choose standard multivariate normal priors for $\boldsymbol{\beta}$ and $\mathbf{m_u}$, and inverse Wishart (*IW*) priors for $\mathbf{V_u}$ and $\boldsymbol{\Sigma}$, i.e. $\boldsymbol{\beta} \sim mvn\left(\boldsymbol{\mu}_{\mathbf{0}}, \mathbf{V}_0\right)$, $\mathbf{m_u} \sim mvn\left(\boldsymbol{\mu}_{\mathbf{u0}}, \mathbf{V}_{\mathbf{u0}}\right)$, $\mathbf{V_u} \sim IW\left(\psi_0, \boldsymbol{\Psi}_{\mathbf{0}}\right)$, and $\boldsymbol{\Sigma} \sim IW\left(v_0, \mathbf{S_0}\right)$. The *IW* parameters are the degrees of freedom and scale matrix, respectively. The *IW* density is parameterized such that $E\left(\mathbf{V_u}\right) = \left(\psi_0 - k_r - 1\right)^{-1} \boldsymbol{\Psi}_{\mathbf{0}}$, and $E\left(\boldsymbol{\Sigma}\right) = \left(v_0 - k_r - 1\right)^{-1} \mathbf{S_0}$. When combined with the likelihood function these priors yield tractable conditional posterior densities. We further improve the speed and efficiency of our posterior simulator (Gibbs Sampler) by

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[15] We thus follow the recommendation in Chib et al. (1998) and specify the individual random effects in an "uncentered" fashion, with their own non-zero mean vector to improve the mixing and convergence properties of our posterior simulator. This implies that $\mathbf{x_{1ip}}$ in (3.1) and the $\mathbf{z_{jip}}$, $j = 1, 2,$ vectors in (3.2) do not contain an intercept, and the posterior means of $\mathbf{m_u}$ for equations two and three can be interpreted as the baseline intercept associated with zero watering days.

augmenting the model with draws of the error components $\{\mathbf{u_i}\}_{i=1}^{N}$ and $\{\boldsymbol{\varepsilon}_{\mathbf{ip}}\}_{ip=1}^{n}$. A general discussion of the merits of this technique of data augmentation is given in Tanner and Wong (1987). Applications with data augmentation involving hierarchical count data models include Chib et al. (1998) and Munkin and Trivedi (2003). For further efficiency gains we bundle the error draws over all *P* periods for a given individual, i.e. we draw $\{\boldsymbol{\varepsilon}_{\mathbf{i}}\}_{i=1}^{N}$, $\boldsymbol{\varepsilon}_{\mathbf{i}} = \begin{bmatrix} \boldsymbol{\varepsilon}_{\mathbf{i1}}' & \boldsymbol{\varepsilon}_{\mathbf{i2}}' & \cdots & \boldsymbol{\varepsilon}_{\mathbf{iP}} \end{bmatrix}'$.

The augmented posterior distribution will be proportional to the priors times the augmented likelihood, i.e.

$$\begin{aligned}
&p\left(\boldsymbol{\beta}, \mathbf{m_u}, \mathbf{V_u}, \boldsymbol{\Sigma}, \{\mathbf{u_i}\}_{i=1}^{N}, \{\boldsymbol{\varepsilon}_{\mathbf{i}}\}_{i=1}^{N} \mid \mathbf{y}, \mathbf{X}\right) \propto \\
&p(\boldsymbol{\beta})\, p(\mathbf{m_u})\, p(\mathbf{V_u})\, p(\boldsymbol{\Sigma})\, p\left(\{\mathbf{u_i}\}_{i=1}^{N} \mid \mathbf{m_u}, \mathbf{V_u}\right) p\left(\{\boldsymbol{\varepsilon}_{\mathbf{i}}\}_{i=1}^{N} \mid \boldsymbol{\Sigma}\right) * \\
&p\left(\mathbf{y} \mid \boldsymbol{\beta}, \{\mathbf{u_i}\}_{i=1}^{N}, \{\boldsymbol{\varepsilon}_{\mathbf{i}}\}_{i=1}^{N}, \mathbf{X}\right)
\end{aligned} \tag{3.6}$$

The last term describes the likelihood function conditioned on all error terms. Thus, the data augmentation step also circumvents the need to evaluate the integrals in (3.1).

The Gibbs Sampler draws consecutively and repeatedly from the conditional posterior distributions $p\left(\boldsymbol{\beta} \mid \{\mathbf{u_i}\}_{i=1}^{N}, \{\boldsymbol{\varepsilon}_{\mathbf{i}}\}_{i=1}^{N}, \mathbf{y}, \mathbf{X}\right)$, $p\left(\mathbf{m_u} \mid \{\mathbf{u_i}\}, \mathbf{V_u}\right)$, $p\left(\mathbf{V_u} \mid \{\mathbf{u_i}\}_{i=1}^{N}\right)$, $p\left(\boldsymbol{\Sigma} \mid \{\boldsymbol{\varepsilon}_{\mathbf{i}}\}_{i=1}^{N}\right)$, $p\left(\{\boldsymbol{\varepsilon}_{\mathbf{i}}\}_{i=1}^{N} \mid \boldsymbol{\beta}, \boldsymbol{\Sigma}, \{\mathbf{u_i}\}_{i=1}^{N} \mathbf{y}, \mathbf{X}\right)$, and $p\left(\{\mathbf{u_i}\}_{i=1}^{N}, \mid \boldsymbol{\beta}, \mathbf{m_u}, \mathbf{V_u}, \{\boldsymbol{\varepsilon}_{\mathbf{i}}\}_{i=1}^{N} \mathbf{y}, \mathbf{X}\right)$. Draws of $\boldsymbol{\beta}$, $\{\boldsymbol{\varepsilon}_{\mathbf{i}}\}_{i=1}^{N}$, and $\{\mathbf{u_i}\}_{i=1}^{N}$ require Metropolis-Hastings (*MH*) subroutines in the Gibbs Sampler. Posterior inference is based on the marginals of the joint posterior distribution $p\left(\boldsymbol{\beta}, \mathbf{m_u}, \mathbf{V_u}, \boldsymbol{\Sigma} \mid \mathbf{y}, \mathbf{X}\right)$. The detailed steps of the posterior simulator and the Matlab code to implement this model are available from the authors upon request.

3.4. Empirical Application

3.4.3 Data

The consumption data for our empirical application flow from a daily monitoring project of 781 residential water consumers in the Reno, Nevada, metropolitan area for eight consecutive weeks between June 23, 2008 and August 18, 2008. The long-standing OWRs for this area, which were also effective throughout our entire research period, allow sprinkler use in the morning and evening on two assigned days per week based on the last digit of a resident's address. Hand watering is allowed on any day. These regulations were originally implemented in 1992 in reaction to a prolonged drought. They became permanent in 1996 primarily to guard against future droughts through sufficient water storage, and to assure adequate flows of the Truckee River to Pyramid Lake, an important spawning habitat for trout and other fish species. These OWRs are only mildly enforced, with infrequent water patrols and nominal fines for repeated violations in the same calendar year.

An immediate challenge for this analysis was the identification of watering days based on the daily consumption patterns for a given household. We use a *K*-means clustering algorithm (MacQueen, 1967) at the household level to group daily use into watering and non-watering days. The algorithm proved robust to different starting values for group centroids. Details for this procedure are available from the authors upon request.

The cluster analysis generates our outcome variable for the water frequency component of our model, i.e. y_{1ip} in (3.1). Weekly consumption (y_{2ip}) is simply the aggregate use for each household and week, and weekly peak (y_{3ip}) is the highest daily observed use in a given week. Table 3.2 shows sample statistics for the three dependent variables. As is evident from the table the largest proportion of observations (over 40%) are associated with twice / week watering. Approximately 2% of household-weeks exhibit no outdoor irrigation. Sizeable sample shares are also allocated to three and four-day watering weeks. The table also shows that average weekly consumption and peak use do not monotonically increase over the number of watering days. To some extent this is likely the result of confounding household characteristics that are not captured at this purely descriptive stage of the analysis, such as household and lot size.

An important feature of our data is that it includes households that always follow their assigned schedule, others that always water with the same weekly frequency but not necessarily on their assigned DoWs, and a third group that exhibits a more flexible watering pattern with varying weekly frequencies. Throughout the remainder of this analysis we will label these three types as "compliant", "habitual", and "flexible", respectively. Table 3.3 depicts the distribution of habitual customers for our sample. Clearly, the largest proportions of habitual types water twice per week. This group comprises 129 households, or 16% of the entire sample. Of that group, the majority (112 households or slightly over 14% of the sample) follow the assigned OWR schedule. These sample shares of habitual and compliant customers are large enough to allow for the identification of interesting differences in consumption and peaks across the three user types, as will be shown below.

Our data set also includes information on basic lot and building characteristics, publically available from the Washoe County Assessor's Office, and climate data obtained from the Western Regional Climate Center. Summary statistics for these variables are given in Table 3.4.

Our first equation includes the following explanatory variables: the log of lot size (*lnland*) as a proxy for irrigable area, the log of the 2007 taxable property value (*lnvalue*) as a proxy for income, the weekly average of the daily maximum temperatures in Fahrenheit (*maxtemp*), and the weekly average of the maximum daily sustained wind speeds in knots (*maxspd*). Importantly for model identification, these two climate variables are not included in equations two and three. For these equations we use the number of weekly growing degree days in units of 10 (*grwdy0*) as a climate indicator instead. Daily growing degrees are defined as (maximum daily temperature + minimum daily temperature)/2-50. Summing this index over the entire week yields the weekly value. Additional variables in the consumption and peak equations are: the log of the building's square footage (*lnsf*), the number of bathrooms (*baths*), and an indicator taking a value of one if a given residence has a pool (*pool*). The number of watering days (the elements of $\mathbf{d}_{\mathbf{ip}}$ in (3.2)) are captured by indicators *wtwk1* through *wtwk7*. The specification is completed by an indicator for households with habitual watering patterns (*habitual*), for households that habitually water 3 times per week (*habitual3*), for households that habitually water 4, 5, or 7 times per week (*habitua457*), and for household-weeks where the observed watering pattern is in compliance with the assigned schedule (*comply*). Since no households habitually water 6 times per week and only one household habitually waters once per week, *habitual* reflects households that habitually water twice per week, but not necessarily on the assigned days. Thus, the intersection of *habitual* and *comply* marks observations for perfectly OWR compliant households, as defined above.

3.4.4 Estimation Results

We estimate all models using the following vague but proper parameter settings for our priors: $\boldsymbol{\mu}_0 = \boldsymbol{\mu}_{u0} = 0$, $\mathbf{V}_0$=100, $\mathbf{V}_{u0} = 10$, $\nu_0 = \psi_0 = 4$, and $\mathbf{S}_0 = \boldsymbol{\Psi}_0 = \mathbf{I}_3$. We first run the model using simulated data to assure the accuracy of our computational algorithm. For the actual estimation run we discard the first 3000 draws generated by the Gibbs Sampler as "burn-ins", and retain the following 1000 draws for posterior inference. We evaluate the performance of the posterior simulator using Geweke's (1992) convergence diagnostics (CD), and inefficiency (IEF) scores as described in Chib (2001). The CD scores clearly indicate convergence for all our models.

Estimation results are summarized in Table 3.5. The table shows posterior means and standard deviations for each coefficient and each element of the variance matrices $\mathbf{V}_u$ and $\boldsymbol{\Sigma}$. The latter are given in the form of standard deviations and correlations. Four key results stand out. The first important finding is that the random effects correlations are located away from zero and estimated with high precision. This confirms that the three components of the model, frequency, consumption, and peaks, are linked via unobservable household effects. Specifically, watering frequency is negatively correlated with both weekly consumption and peak (terms rho12 and rho13 towards the bottom of the second column of the table), while equations two and three exhibit a strong, positive correlation. This pattern would be consistent, for example, with a person that wants to adhere to the observed OWRs, yet still has a high demand for outdoor water, and prefers not to water by hand on non-assigned days. Since the majority of his outdoor water use occurs on the two assigned days, the peaks would also be higher than if water application was distributed more evenly throughout the week. In contrast, a person that doesn't mind bending the rules, and has a profound knowledge of his yards' water needs, might have lower consumption and peaks due to more efficient watering techniques.

The second key finding is that the standard deviations and correlations for the observation-level errors, given at the bottom of column five, are of negligible magnitude. We can therefore conclude that the three equations are primarily linked through time-invariant household effects, and that household-week specific unobservable effects play only a minor role for this application.

The third important result flowing from our analysis relates to the marginal effects of watering days on weekly consumption and peaks. Controlling for basic customer characteristics and climate we can see that the posterior means for the *wtwk* indicators in equation two increase monotonically with watering frequency. These values can be approximately interpreted as proportional changes in expected consumption compared to the no-watering case. For example, watering twice a week increases a typical household's use by over 50% compared to its strictly-indoors consumption. A five day watering week more than doubles indoor use.

A similar monotonic increase can also be observed for weekly peaks, with one notable exception: the posterior mean for *wtwk3* is actually slightly lower than for *wtwk2*. In other words, a typical household that waters three times in a given week can be expected to have lower maximum daily consumption that week than a customer that waters only twice. In general, incremental changes in peak use over watering days are of smaller proportion than changes in total weekly consumption. Watering frequency decisions thus appear to affect primarily total consumption, while they have a relatively small impact on peaks

Fourth and most importantly the posterior means for *habitual* emerges as positive for both consumption and peaks. Specifically, for any given week with two watering events, a household that chooses to water twice every week has approximately 28% higher use and 34% higher peaks compared to a household that follows a more flexible schedule. The posterior means for *comply* is negative for consumption and positive for peaks, but in both cases it is very small. An additional 0.4% for peaks is added if the habitual weekly watering pattern follows the official OWR schedule, and 1% for water use is subtracted. The posterior means for the interaction terms *habitual3* and *hatitual457* are negative. Subtracting from the respective means for *habitual*, a household that chooses to water three times every week has approximately 16% higher use and 15% higher peaks compared to a household that follows a more flexible schedule. Yet a household that chooses to water four, five, or seven times every week has slightly lower consumption and peaks, 5% and 3% respectively. Thus, we find that a decrease in watering flexibility results in an increase in use and peaks for the (otherwise) typical customer with two or three watering events per week.

For a more direct comparison of weekly consumption and peak across households with different watering patterns we generate posterior predictive densities (PPDs) by combining the parameter draws generated by our posterior simulator with sample means of customer and climate characteristics, and different settings for the *wtwk* indicators, *habitual*, *habitual3, habitual45,* and *comply*.

Table 3.6 summarizes predicted use and peak for two, three, and four watering-day regimes, each under a flexible and habitual implementation. The corresponding PPDs are depicted in Figures 3.4 and 3.5. Clearly, the posterior mean for total use increases with the number of watering days, while the mean for weekly peak remains virtually unchanged. Weeks with two watering days display slightly higher peaks than those with three watering days, and slightly less than weeks with two watering days. Moreover, for weeks with either 2 or 3 watering days, the habitual scenario produces pronouncedly higher posterior expectations than the flexible approach. Astonishingly, the household that habitually waters 2 times per week uses approximately the same amount of water in a given week than those that habitually water 3 or 4 times per week, and uses substantially more than a flexible user watering 3 times during the week. Presumably, households with a flexible watering pattern exert more direct control on their watering activities. This may entail adjusting sprinkler settings based on daily wind and temperature conditions, and substituting or complementing sprinkler irrigation with targeted manual watering. Another explanation is that customers using automatic sprinklers set the timer to water on the same days each week for the same length of time resulting in habitual watering patterns, while customers using manual sprinklers are more willing to change irrigation patterns depending on current weather conditions. On average, however, a flexible schedule lowers use and peaks in a given week compared to the habitual case. As the number of watering events increase, the ‘flexibility’ effect is weakened.

For twice-per-week watering, we can also add a 'full compliance' scenario to this comparative predictive analysis. The corresponding results are captured in Table 3.7 and

Figure 3.6. As is evident from the figure the habitual and compliant types exhibit almost identical PPDs and expectations, and the entire PPDs for these two user types are shifted to the right compared to the flexible user. As shown in the table, weekly consumption under the habitual pattern exceeds flexible use by 30%. For peak use, these relative differences are even more pronounced. Clearly, a habitual watering pattern, whether or not the water events occur on the exact same days every week is not an efficient strategy from a conservation perspective.

This conclusion is also supported by a full-season predictive analysis. Table 3.8 and Figure 3.7 show the results of a simulated eight-week period consumption analysis for four weekly watering types: 2-day / compliant, 2-day / habitual, 3-day / habitual, and flexible with alternating weeks of 2 and 3-day watering. We find that the 2-day / compliant, 2-day / habitual, and 3-day / habitual types exhibit very similar seasonal PPDs and expectations. Perhaps even more stunning, the alternating flexible type uses significantly less water than the 3-day habitual customer. This is a key result from a policy perspective, as the local utility is currently contemplating switching from the DoW-pegged twice-a-week OWR to a flexible schedule with a three-day cap.

3.5. Conclusion

This study focuses on outdoor water use for residential customers. It is the first to examine the relationship between watering frequency, total use, and consumption peaks. This allows for inference on the effectiveness of existing day-of-week watering restriction, and the potential effectiveness of alternative regulatory strategies in promoting conservation.

We identify outdoor watering events using a rich data set of daily consumption at the household level, and trace a customer's watering patter over an eight-week period during the height of the irrigation season. Using a full-information hierarchical Bayesian estimation framework we estimate the marginal effect of watering frequency and weekly habitual watering behavior on use and peak. We also compare predictive distributions for a variety of weekly and seasonal watering patterns. We find that households that exhibit a habitual two or three day watering pattern always consume more water and exhibit higher peaks than households that implement a more flexible pattern. This is evidence that customers that alter their number of watering days across weeks use outdoor water more efficiently than residents with habitual weekly watering frequency.

Naturally, our analysis benefits from the fact that many households in our sample deviate from the official watering schedule for some or all of our research period. We do not consider the possible welfare loss these households might experience from knowingly violating official regulations or existing social norms. In the same vein, we implicitly assume full compliance and equal administrative and enforcement costs when we compare alternative watering restriction policies. These shortcomings may be overcome with a broader data set that compares the watering behavior and use of fully complying households across utility districts with different watering restrictions. This would be a fruitful avenue for future research.

However, our study provides unambiguous evidence that allowing households to alternate weekly watering frequency within an allotted weekly maximum promotes conservation. Thus, from a policy perspective, there may be considerable conservation gains in promoting *flexibility* in outdoor watering. For example, this could be accomplished via information campaigns on the merits of frequent adjustments to sprinkler settings, and on the proper sprinkler settings (e.g. time of day, number of cycles per day, and length of cycles) to irrigate more efficiently, along with the promotion of water-efficient landscaping, and computer- and climate-controlled irrigation systems.

Perhaps it is time for conservation-concerned water utilities and policy makers to shift focus from merely restricting the frequency of irrigation to promoting the efficient implementation of outdoor watering. Our findings suggest that many households already know which watering pattern best satisfies their irrigation needs without deviating "too much" from the official schedule. Thus, we conclude from our study that an economically efficient outdoor watering policy will meet system-wide conservation targets while preserving a maximum degree of consumer sovereignty.

Table 3.1. Sample List of cities with Outdoor Watering Restrictions

City	permitted watering days / week
Atlanta, GA	3
Aurora, CO	3
Austin, TX	3
Cheyenne, WY	2
El Paso, TX	3
Lafayette, LA	3
Long Beach, CA	3
Los Angeles, CA	2
Miami, FL	2
Orlando, FL	2
Raleigh, NC	3
Reno, NV	3
San Diego, CA	3

Table 3.2. Weekly Use and Peaks by Number of Watering Days

			Weekly Use (1000 gallons)			
watering days / week	obs.	%	mean	std	change from 0	% change from 0
0	388	6.21	3.12	(2.17)		
1	494	7.91	4.19	(2.85	1.07	34.35
2	2,857	45.73	6.60	(4.20)	3.48	111.63
3	1,109	17.75	6.22	(3.68)	3.10	99.48
4	795	12.72	7.37	(5.45)	4.25	136.19
5	332	5.31	7.91	(5.56)	4.79	153.55
6	138	2.21	8.61	(6.90)	5.49	175.78
7	135	2.16	12.44	(9.78)	9.32	298.56
Total	6,248	100.00	6.47	(4.70)		

	Weekly Peak (1000 gallons)			
watering days / week	mean	std	change from 0	% change from 0
0	0.74	(0.52)		
1	1.58	(1.18)	0.84	114.13
2	2.58	(1.77)	1.84	248.37
3	1.95	(1.39)	1.21	163.50
4	1.99	(1.72)	1.25	168.82
5	1.90	(1.45)	1.16	156.18
6	1.85	(1.51)	1.11	149.56
7	2.35	(1.88)	1.61	218.12
Total	2.14	(1.66)		

Table 3.3. Habitual Watering Patterns

	HHs	obs.	% (of entire sample)
always water … days / week			
1	1	8	0.13
2	129	1,032	16.52
3	15	120	1.92
4	11	88	1.41
5	4	32	0.51
7	2	16	0.26
Total	162	1416	20.74
always follow schedule	112	896	14.34

Table 3.4. Sample Statistics for Explanatory Variables

HH characteristics					
variable	obs.	mean	std	min	max
age	781	21.15	(17.62)	1.00	99.00
lot size (1000 sqft)	781	11.04	(8.45)	3.05	87.12
home value ($10,000)	781	22.84	(13.77)	5.33	168.78
square footage (1000)	781	2.02	(0.77)	0.78	8.32
# bathrooms	781	2.27	(0.64)	1.00	5.00
pool	781	0.01	-	0.00	1.00

Climate variables					
variable	obs.	mean	std	min	max
mean of min. daily temperatures, F	8	59.88	1.86	56.84	63.26
mean of max. daily sustained wind speeds, knots	8	16.04	(2.74)	11.30	18.69
growing degree days, units of 10	8	19.50	(1.35)	17.91	21.61

Table 3.5. Estimation Results, HTPLN/HELN

	equation 1		equation 2		equation 3	
	mean	std	mean	std	mean	std
lnland	0.039	(0.040)				
lnvalue	0.128	(0.040)				
mintemp	-0.018	(0.005)				
mxspd	-0.022	(0.005)				
wtwk1			0.281	(0.065)	0.739	(0.084)
wtwk2			0.523	(0.062)	1.000	(0.073)
wtwk3			0.682	(0.069)	0.984	(0.081)
wtwk4			0.853	(0.076)	1.043	(0.089)
wtwk5			1.006	(0.097)	1.087	(0.103)
wtwk6			1.092	(0.121)	1.113	(0.128)
wtwk7			1.350	(0.133)	1.270	(0.147)
lnland			0.367	(0.048)	0.379	(0.057)
lnvalue			-0.403	(0.081)	-0.508	(0.101)
lnsf			0.214	(0.119)	0.258	(0.140)
age			0.001	(0.001)	0.001	(0.001)
baths			0.136	(0.040)	0.147	(0.044)
pool			0.261	(0.178)	0.323	(0.192)
grwdy0			-0.010	(0.010)	-0.015	(0.010)
habitual			0.278	(0.143)	0.336	(0.165)
habitual3			-0.119	(0.203)	-0.191	(0.222)
habitual457			-0.332	(0.223)	-0.374	(0.241)
comply			-0.014	(0.141)	0.004	(0.165)
RE mean	0.398	(0.061)	-1.406	(0.173)	-1.986	(0.266)

std's and correlations for RE's			std's and correlations for errors		
sig1	0.444	(0.015)	sig1	0.111	(0.006)
rho12	-0.173	(0.067)	rho12	-0.027	(0.067)
sig2	0.445	(0.018)	sig2	0.114	(0.005)
rho13	-0.231	(0.064)	rho13	0.014	(0.093)
rho23	0.933	(0.009)	rho23	0.051	(0.080)
sig3	0.495	(0.002)	sig3	0.115	(0.007)

Table 3.6. Posterior Predictive Results for 2, 3, and 4 Watering Days, Flexible vs. Habitual Regimes

weekly use (1000 gallons)						
	flexible		habitual		fractional difference	
watering days / week	mean	std	mean	std	mean	std
2 days	2.204	(1.164)	2.942	(1.631)	0.333	(0.189)
3 days	2.582	(1.360)	3.054	(1.674)	0.186	(0.179)
4 days	3.067	(1.623)	2.941	(1.623)	-0.042	(0.142)

weekly peak (1000 gallons)						
	flexible		habitual		fractional difference	
watering days / week	mean	std	mean	std	mean	std
2 days	1.446	(0.826)	2.051	(1.123)	0.419	(0.232)
3 days	1.421	(0.807)	1.658	(0.974)	0.172	(0.190)
4 days	1.507	(0.857)	1.475	(0.891)	-0.025	(0.152)

Table 3.7. Comparison of Two-Day / Week Watering Regimes

2 days / week watering regime		weekly use (1000 gallons)		peak use (1000 gallons)	
code	description	mean	std	mean	std
1	flexible	2.204	(1.164)	1.446	(0.826)
2	habitual	2.942	(1.631)	2.051	(1.231
3	compliant	2.870	(1.514)	2.028	(1.150)
fractional difference, 3 vs. 1		0.304	(0.075)	0.409	(0.099)

Table 3.8. Comparison of Seasonal (8-Week) Watering Regimes

weekly watering regime		seasonal use (1000 gallons)	
code	description	mean	std
1	2 days, compliant	23.085	(12.176)
2	2 days, habitual	23.660	(13.109)
3	alternating 2 and 3 days	19.250	(10.134)
4	3 days, habitual	24.567	(13.458)
	fractional difference, 1 vs. 3	0.201	(0.067)

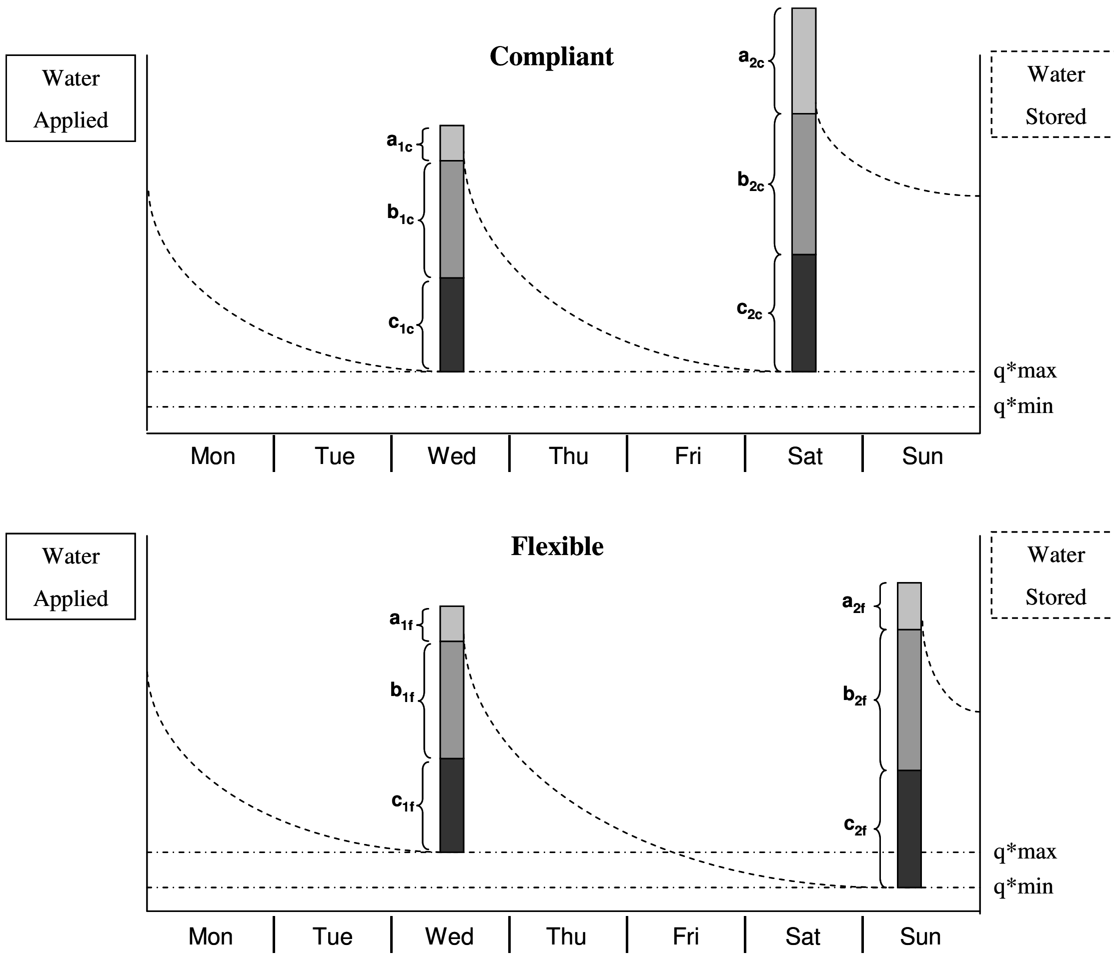

Figure 3.1. Examples for Twice / Week Watering
Note: a = immediate wind and overspray loss, b = evaporation, runoff, and drainage loss, c = uptake from biomass

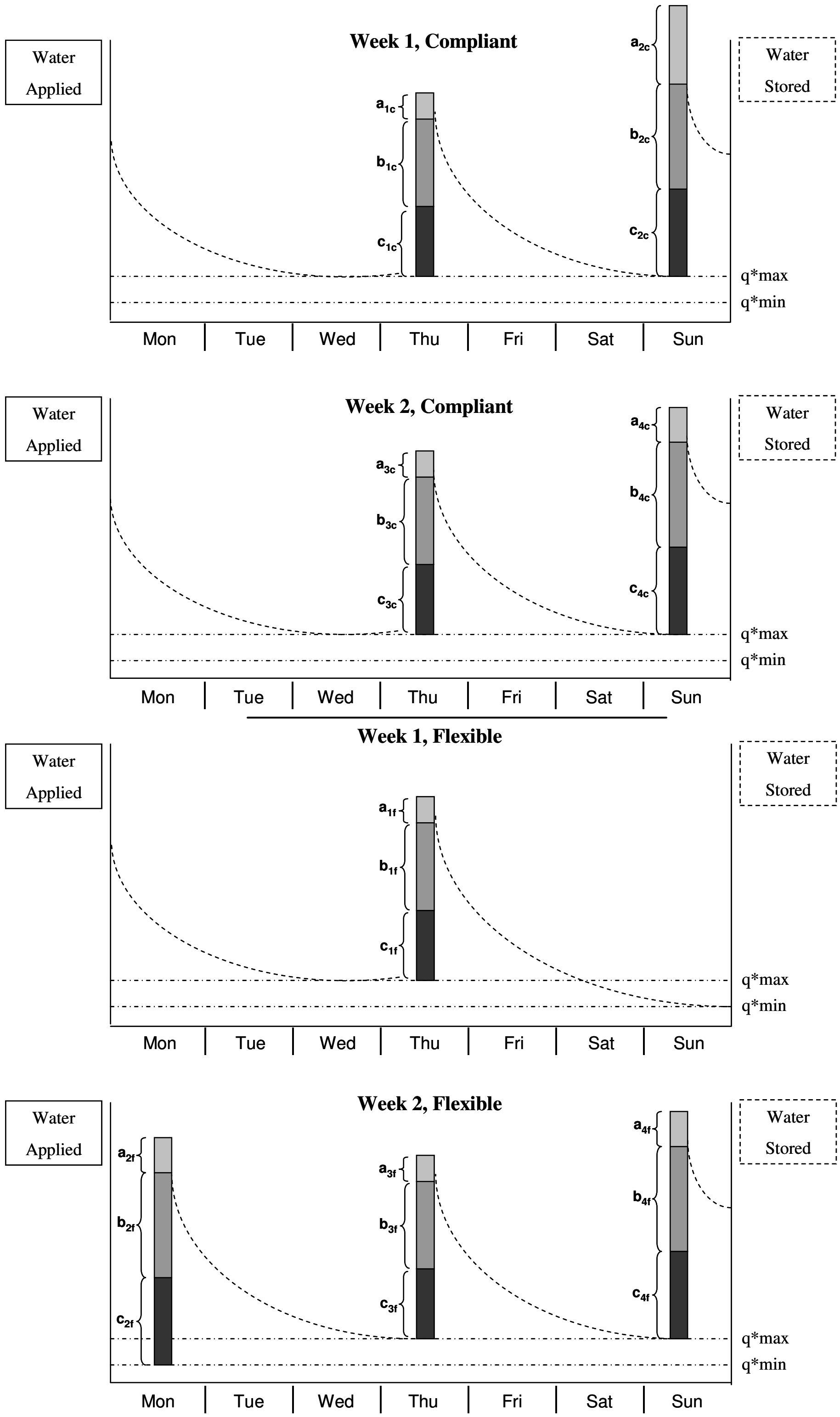

Figure 3.2. Examples for Compliant and Flexible Over Two Weeks
Note: a = immediate wind and overspray loss, b = evaporation, runoff, and drainage loss, c = uptake from biomass

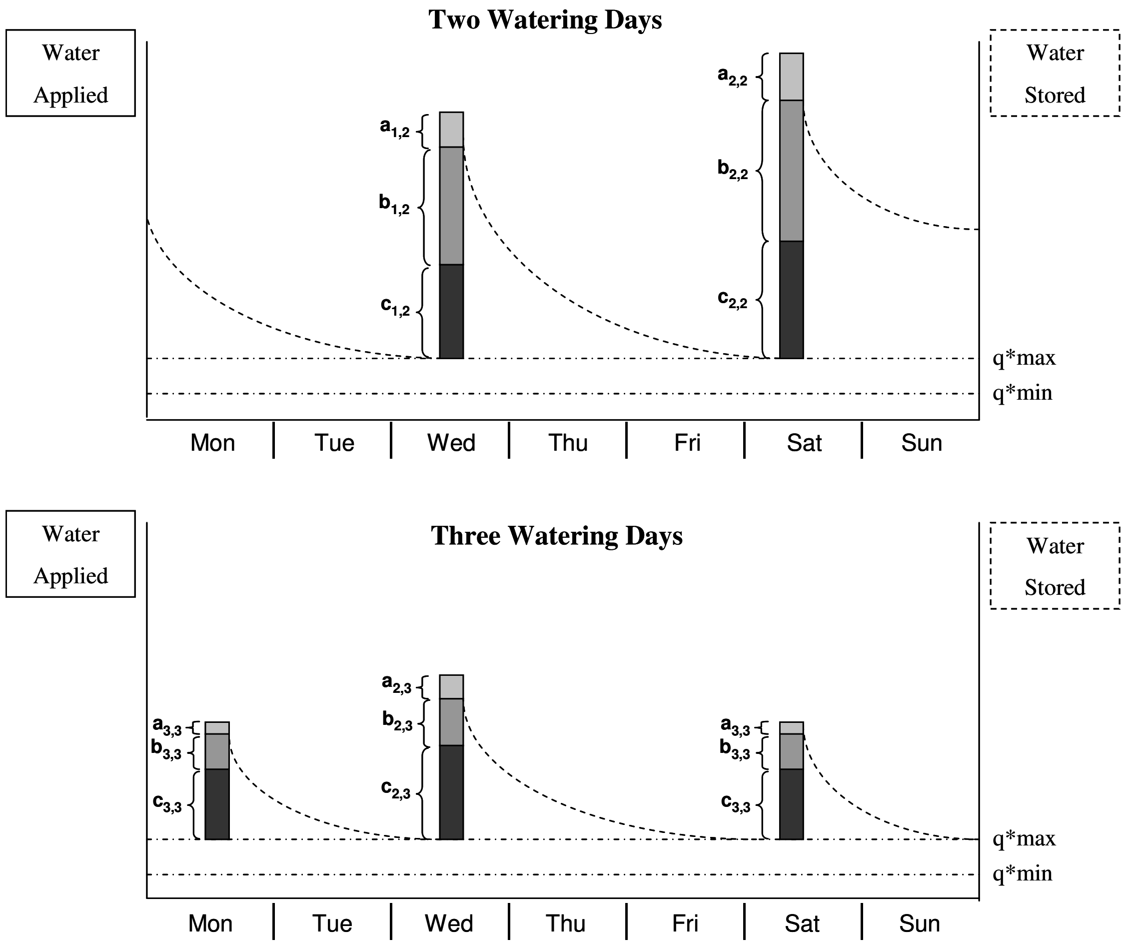

Figure 3.3. Examples for Two and Three Times per Week Watering
Note: a = immediate wind and overspray loss, b = evaporation, runoff, and drainage loss, c = uptake from biomass

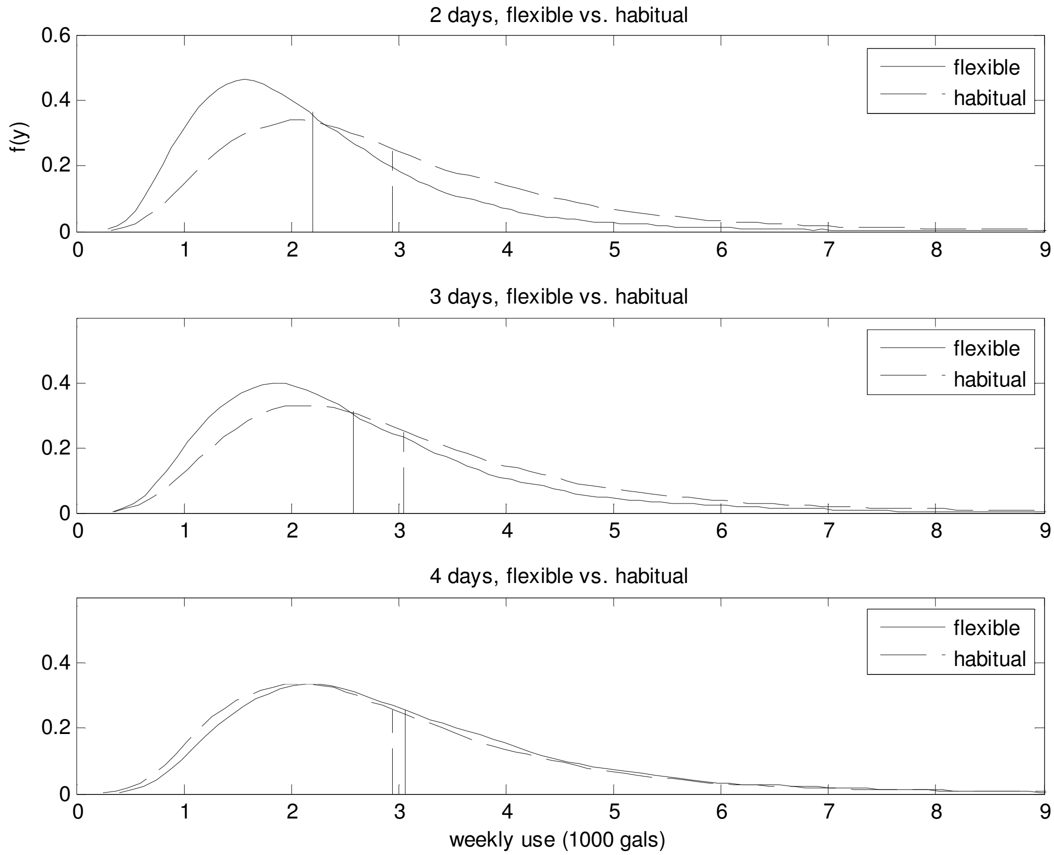

Figure 3.4. Posterior Predictive Results for Weekly Use by Watering Days and Regimes
Note: vertical lines = posterior predictive means

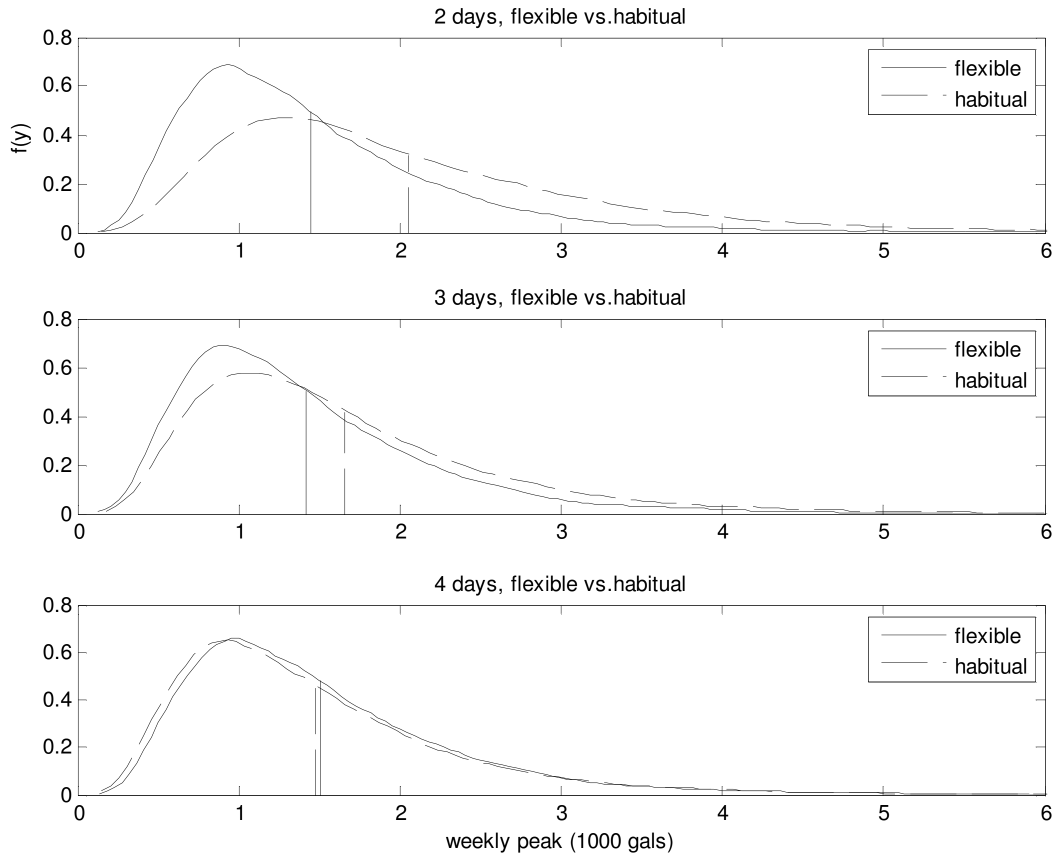

Figure 3.5. Posterior Predictive Results for Weekly Peak by Watering Days and Regimes
Note: vertical lines = posterior predictive means

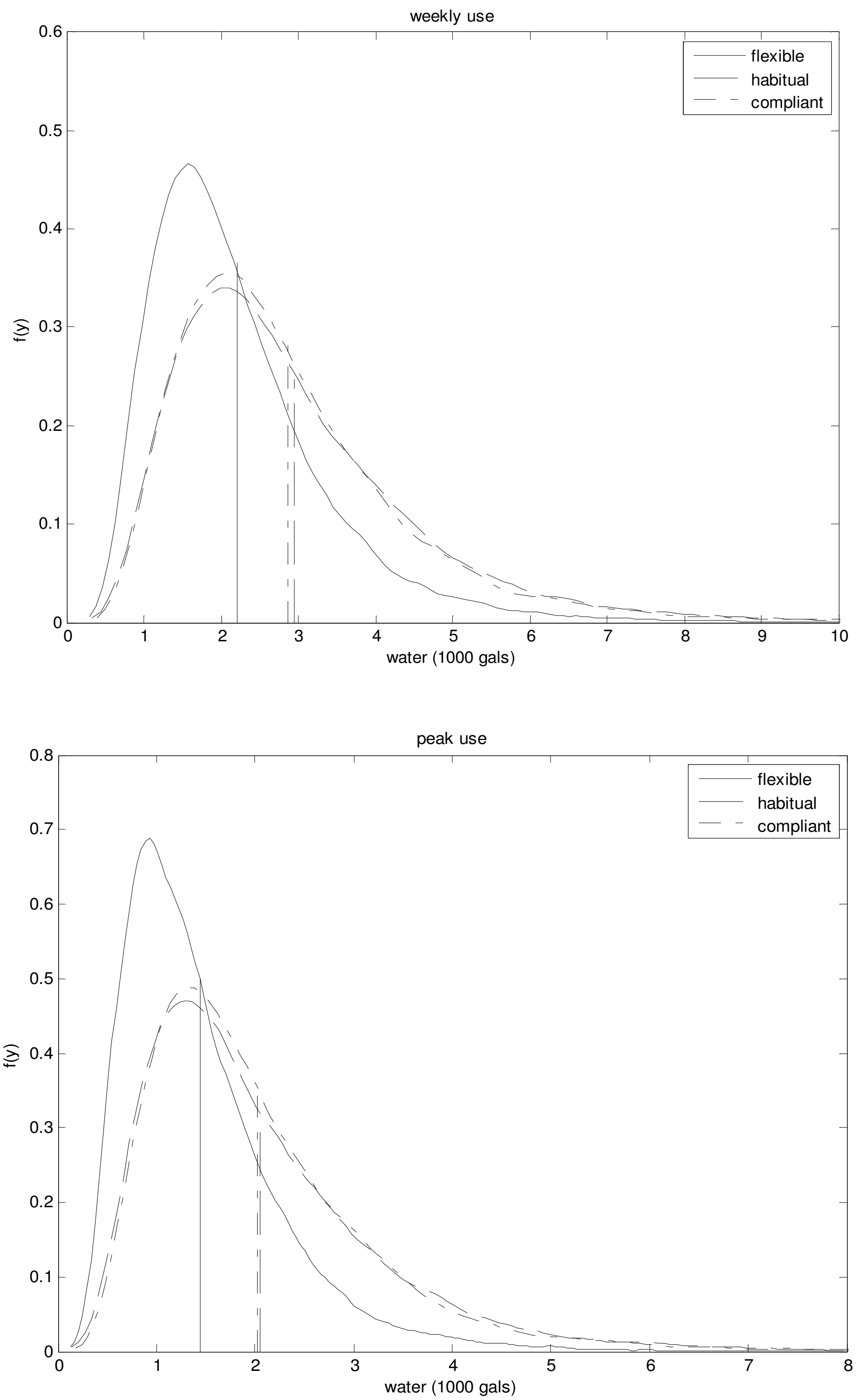

Figure 3.6. Posterior Predictive Results for Two-Day / Week Watering Regimes
Note: vertical lines = posterior predictive means

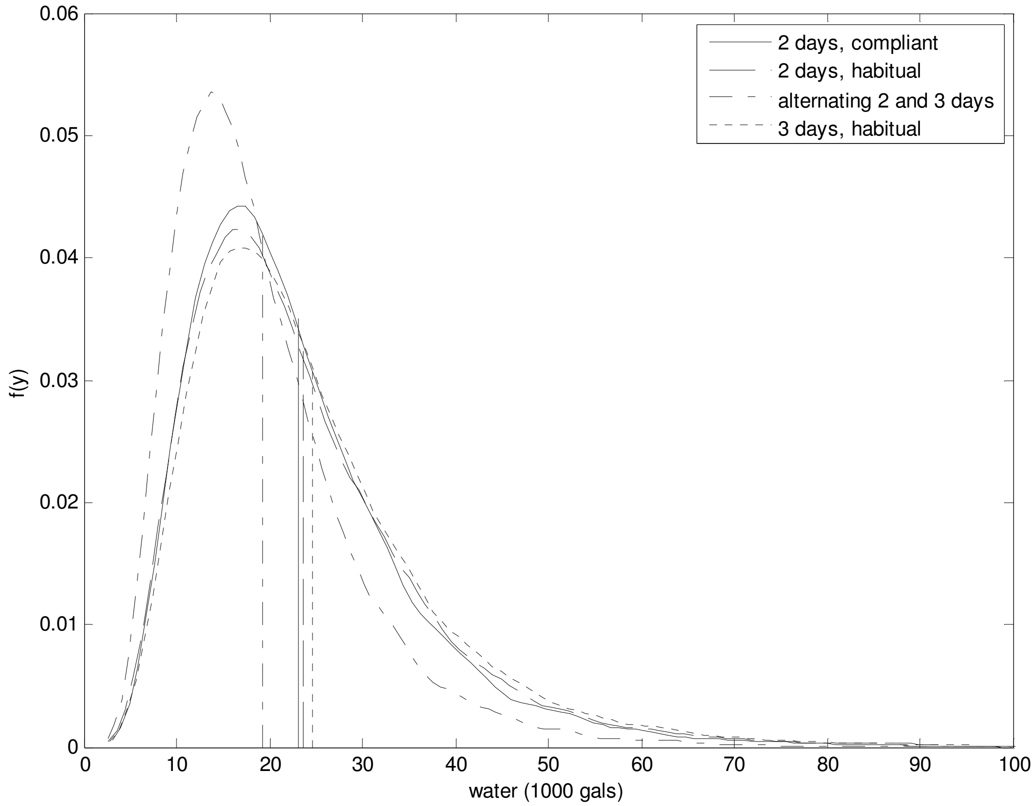

Figure 3.7. Predicted Seasonal Use under Different Weekly Watering Regimes
Note: vertical lines = posterior predictive means

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Appendix A.

Variables Used in Linear Regression Model with Trend Change

Variable	Description
constant	= 1 for all households
NW_L1	= 1 if observation occurred on a non-watering day and household received schedule letter, 0 otherwise
NW_L2	= 1 if observation occurred on a non-watering day and household received drought letter, 0 otherwise
NW_L3	= 1 if observation occurred on a non-watering day and household received capacity letter, 0 otherwise
NW_L4	= 1 if observation occurred on a non-watering day and household received monitoring letter, 0 otherwise
NWI	= 1 if observation occurred on a non-watering day after intervention, 0 otherwise
NWI_L1	= 1 if observation occurred on a non-watering day intervention and household received schedule letter, 0 otherwise
NWI_L2	= 1 if observation occurred on a non-watering day after intervention and household received drought letter, 0 otherwise
NWI_L3	= 1 if occurred on a non-watering day after intervention and household received capacity letter, 0 otherwise
NWI_L4	= 1 if observation occurred on a non-watering day after intervention and household received monitoring letter, 0 otherwise
W	= 1 if observation occurred on a watering day, 0 otherwise
W_L1	= 1 if observation occurred on a watering day and household received schedule letter, 0 otherwise
W_L2	= 1 if observation occurred on a watering day and household received drought letter, 0 otherwise
W_L3	= 1 if observation occurred on a watering day and if household received capacity letter, 0 otherwise
W_L4	= 1 if observation occurred on a watering day and household received monitoring letter, 0 otherwise
WI	= 1 if observation occurred on a watering day after intervention, 0 otherwise
WI_L1	= 1 if observation occurred on a watering day after intervention and household received schedule letter, 0 otherwise
WI_L2	= 1 if observation occurred on a watering day after intervention and household received drought letter, 0 otherwise
WI_L3	= 1 if observation occurred on a watering day after intervention and household received capacity letter, 0 otherwise
WI_L4	= 1 if observation occurred on a watering day after intervention and household received monitoring letter, 0 otherwise

Variable	Description
TNW	= t if observation occurred on a non-watering day
TNW_L1	= t if observation occurred on a non-watering day and household received schedule letter
TNW_L2	= t if observation occurred on a non-watering day and household received drought letter
TNW_L3	= t if observation occurred on a non-watering day and household received capacity letter
TNW_L4	= t if observation occurred on a non-watering day and household received monitoring letter
TINW	= $t - 38$ if observation occurred on a non-watering day after intervention, 0 otherwise
TINW_L1	= $t - 38$ if observation occurred on a non-watering day after intervention and household received schedule letter, 0 otherwise
TINW_L2	= $t - 38$ if observation occurred on a non-watering day after intervention and household received drought letter, 0 otherwise
TINW_L3	= $t - 38$ if observation occurred on a non-watering day after intervention and household received capacity letter, 0 otherwise
TINW_L4	= $t - 38$ if observation occurred on a non-watering day after intervention and household received monitoring letter, 0 otherwise
TW	= t if observation occurred on a watering day, 0 otherwise
TW_L1	= t if observation occurred on a watering day and household received schedule letter, 0 otherwise
TW_L2	= t if observation occurred on a watering day and household received drought letter, 0 otherwise
TW_L3	= t if observation occurred on a watering day after and household received capacity letter, 0 otherwise
TW_L4	= t if observation occurred on a watering day after and household received monitoring letter, 0 otherwise
TWI	= $t - 38$ if observation occurred on a watering day after intervention, 0 otherwise
TWI_L1	= $t - 38$ if observation occurred on a watering day after intervention and household received schedule letter, 0 otherwise
TWI_L2	= $t - 38$ if observation occurred on a watering day after intervention and household received drought letter, 0 otherwise
TWI_L3	= $t - 38$ if observation occurred on a watering day after intervention and household received capacity letter, 0 otherwise
TWI_L4	= $t - 38$ if observation occurred on a watering day after intervention and household received monitoring letter, 0 otherwise

Appendix B.

Calculation of expected water consumption at $t = 0$ (in 100 gallons).

$(E[y \mid NW = 1, I = 0, t = 0, C = 1] = 6.2328$

$(E[y \mid NW = 1, I = 0, t = 0, L_1 = 1] = 6.2328 + 0.3118 = 6.5445$

$(E[y \mid NW = 1, I = 0, t = 0, L_2 = 1] = 6.2328 + 0.4571 = 6.6899$

$(E[y \mid NW = 1, I = 0, t = 0, L_3 = 1] = 6.2328 + 0.4884 = 6.7212$

$(E[y \mid NW = 1, I = 0, t = 0, L_4 = 1] = 6.2328 + 0.2095 = 6.4422$

$(E[y \mid W = 1, I = 0, t = 0, C = 1] = 6.2328 + 11.1294 = 17.3622$

$(E[y \mid W = 1, I = 0, t = 0, L_1 = 1] = 6.2328 + 11.1294 + 0.0459 = 17.4081$

$(E[y \mid W = 1, I = 0, t = 0, L_2 = 1] = 6.2328 + 11.1294 + 0.0679 = 17.4301$

$(E[y \mid W = 1, I = 0, t = 0, L_3 = 1] = 6.2328 + 11.1294 + 0.8145 = 18.1767$

$(E[y \mid W = 1, I = 0, t = 0, L_4 = 1] = 6.2328 + 11.1294 + 0.7466 = 18.1088$

Appendix C.

Calculation of pre- and post-intervention slopes (in 100 gallons).

$E[slope \mid NW = 1, I = 0, C = 1] = 0.0173$
$E[slope \mid NW = 1, I = 0, L_1 = 1] = 0.0173 - 0.0188 = -0.0016$
$E[slope \mid NW = 1, I = 0, L_2 = 1] = 0.0173 + 0.0012 = 0.0184$
$E[slope \mid NW = 1, I = 0, L_3 = 1] = 0.0173 + 0.0048 = 0.0220$
$E[slope \mid NW = 1, I = 0, L_4 = 1] = 0.0173 - 0.0004 = 0.0168$

$E[slope \mid NW = 1, I = 1, C = 1] = 0.0173 - 0.0395 = -0.0222$
$E[slope \mid NW = 1, I = 1, L_1 = 1] = 0.0173 - 0.0188 - 0.0395 + 0.0213 = -0.0197$
$E[slope \mid NW = 1, I = 1, L_2 = 1] = 0.0173 + 0.0012 - 0.0395 + 0.0085 = -0.0125$
$E[slope \mid NW = 1, I = 1, L_3 = 1] = 0.0173 + 0.0048 - 0.0395 - 0.0110 = -0.0284$
$E[slope \mid NW = 1, I = 1, L_4 = 1] = 0.0173 - 0.0004 - 0.0395 + 0.0002 = -0.0225$

$E[slope \mid W = 1, I = 0, C = 1] = 0.0436$
$E[slope \mid W = 1, I = 0, L_1 = 1] = 0.0436 - 0.0059 = 0.0377$
$E[slope \mid W = 1, I = 0, L_2 = 1] = 0.0436 - 0.0092 = 0.0344$
$E[slope \mid W = 1, I = 0, L_3 = 1] = 0.0436 + 0.0024 = 0.0460$
$E[slope \mid W = 1, I = 0, L_4 = 1] = 0.0436 - 0.0137 = 0.0299$

$E[slope \mid W = 1, I = 1, C = 1] = 0.0436 - 0.0465 = -0.0029$
$E[slope \mid W = 1, I = 1, L_1 = 1] = 0.0436 - 0.0059 - 0.0465 + 0.0027 = -0.0062$
$E[slope \mid W = 1, I = 1, L_2 = 1] = 0.0436 - 0.0092 - 0.0465 - 0.0115 = -0.0236$
$E[slope \mid W = 1, I = 1, L_3 = 1] = .0436 + 0.0024 - 0.0465 - 0.0202 = -0.0208$
$E[slope \mid W = 1, I = 1, L_4 = 1] = 0.0436 - 0.0137 - 0.0465 + 0.0154 = -0.0012$