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Subsidies for Succulents: Evaluating the Las Vegas Cash-for-Grass Rebate Program

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Abstract

I estimate the water savings and property value effects of a Las Vegas area water conservation program that subsidizes conversions of lawn to desert landscape. Using event studies and panel fixed-effects models, I find that the average conversion reduces baseline water consumption by 21 percent and increases property values by about 1 percent. In addition, my results show that water savings remain relatively stable over time; that water savings are inversely proportional to annual program take-up; that participants with high pre-conversion water demand save more water than participants with lower pre-conversion water demand; and that a 6 percent price increase would have achieved equivalent savings. I find little evidence of property value spillovers to neighboring properties. The program saves water at an annual rate of \$4.84/kgal and if I include an estimate of the scarcity value of water, generates net benefits of \$2.00 per square foot of desert landscape converted.[1]

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1 Introduction

Water is not an abundant resource. In the western United States, municipal water demand threatens to outstrip supplies as new water customers increase service populations and droughts decrease reservoir levels. In the past, water utilities relied on large, often federally funded, water supply augmentation projects. Today, however, these projects are costly and unpopular. And while agriculture holds large amounts of historical water rights, legal institutions, social perceptions and equity concerns appear to inhibit free trade between agricultural and urban users (Libecap, 2007; Howe *et al.*, 1990). Faced with essentially fixed supplies and growing demand, water utilities have responded with various incentive-based and command-and-control demand-side-management strategies. Common strategies include drought awareness campaigns, lawn irrigation and other water use restrictions, and subsidies for water saving capital investments (Price *et al.*, 2014).

I estimate the savings and impacts on property values of the Southern Nevada Water Authority's Water Smart Landscapes program, or Cash-for-Grass program. The program aims to reduce water demand by offering Las Vegas water customers a subsidy for replacing lawns with desert landscape. Of all the water demand management strategies, those that target lawn irrigation may achieve the greatest reductions in demand. Residential lawn irrigation accounts for over half of all residential water consumption in southwestern cities (Sovocool *et al.*, 2006), and in Las Vegas, nearly 40 percent of total water deliveries goes towards residential outdoor uses.[2] Especially in light of the growing popularity of turf replacement subsidies, comprehensive evaluations of programs targeting outdoor water use are relevant and timely.

I combine over 25 years of single-family monthly water consumption data for the Las Vegas Valley Water District (LVVWD) with single-family rebate participant information since the beginning of the program in 1996 to estimate water savings associated with Cash-for-Grass subsidized conversion to desert landscape. I use event study and panel fixed-effects models to visualize and quantify average water savings. I further test robustness of my results to three additional control samples that endeavor to account for any remaining concerns over selection bias not accounted for in my main specification. First, I estimate a model without non-participants. Second, I build a

[2]The water authority estimates that 59 percent of total water deliveries go to residential customers (SNWA, 2009). Mansur and Olmstead (2012) find that outdoor water use accounts for two thirds of total water use in arid environments. The product of these two values approximately equals 40 percent.

control sample from non-participants who applied for the rebate, but never completed the conversion process. Third, I develop a matched sample of non-participants, matching on pre-conversion water consumption and lot size.

I find an average conversion reduces monthly water consumption by 21 percent, or 5,000 gallons per month, with this result robust to my three alternative control samples. Savings remain stable over time, suggesting that program participants do not increase other water intensive activities after conversion, but simply reduce outdoor irrigation. I also find that water savings fall over time as rising rebate levels increase the number of annual rebate recipients. The inverse relationship between savings and incentive to participate highlights a possible trade-off between expanding the reach of the subsidy and maintaining the effectiveness of conversions. In addition to exploring heterogeneity in savings across time, I also explore heterogeneity in savings across participant type. I find that participants with high pre-conversion water demand save more water (relative to lot size) than participants with low pre-conversion water demand.[3] Finally, using a partial equilibrium framework, I estimate that a 6 percent price increase experienced by the entire service population in place of the rebate program would have achieved the same aggregate savings as did conversions subsidized by the program.[4]

Most evaluations of demand-side-management strategies focus on electricity programs.[5] Furthermore, these studies tend to estimate short-term impacts and there exist few analyses exploring the effect of participant heterogeneity on program outcomes (Allcott and Greenstone, 2012).[6] My results expand our knowledge of water conservation rebates and, more generally, contribute to our understanding of the long-term dynamics of conservation rebate program savings as well as how heterogeneous participant characteristics affect conservation rebate program performance.

I also estimate the effect of conversion to desert landscape on property values in the LVVWD.

[3]Deoreo *et al.* (2000) come to a similar conclusion.

[4]After completing my analysis, I became aware that another dissertation explores water savings from the Cash-for-Grass program (Brelsford, 2014). While Brelsford uses different methods and census tract consumption data, similar to my results Brelsford finds substantial savings due to the rebate program. But in contrast to my results, Brelsford finds that savings erode over time. Brelsford acknowledges, however, that "a difference in differences approach on household level data would provide a more rigorous estimate of the long term water savings generated through the [Cash-for-Grass] program." I use this approach in my analysis. And while I have not found a paper to reference, I am aware that ongoing work by Brelsford and Josh Abbott of Arizona State University also use this differences-in-differences approach to estimate Cash-for-Grass induced water savings.

[5]For example, see Alberini and Towe (2015), Davis *et al.* (2014), and Arimura *et al.* (2011).

[6]Reflecting the broader demand-side-management program evaluation literature, most studies of which I am aware that give attention to heterogeneity derive their conclusions from energy conservation programs (Allcott, 2011; Allcott *et al.*, 2015).

The impact of conversion on property values will incorporate all private impacts associated with conversion such as water bill savings, reduced lawn maintenance costs, negative energy spillovers from increased urban heat island effects (Klaiber *et al.*, 2015), and any non-monetary impacts such as aesthetic appeal. Because energy spillovers and aesthetic appeal may impact neighboring homes, I also estimate the spillover effect of conversion to desert landscape by modeling the impact of conversions on adjacent property values. While past studies have explored peer effects in capital investment subsidies (Bollinger and Gillingham, 2012) and researchers have recognized the importance of policy induced externalities in other contexts (Miguel and Kremer, 2004), to my knowledge my analysis is the first empirical investigation to explore the spillover effects of capital investment subsidies on neighboring properties.

I use Cash-for-Grass program enrollment information combined with Clark County Assessor data on property sales to estimate the value of desert landscape within a hedonic property framework. I employ a differences-in-differences strategy, controlling for unobserved characteristics with a rich set of spatial and temporal fixed-effects. I find that conversion increases the value of a home by about 1 percent, or \$3,700, and has little impact on neighboring homes. I additionally find that the existence of a conversion, rather than the area converted, drives the increase in property values. I estimate that the present discounted value of annual water bill and lawn maintenance savings approximately equals the increase in home values, suggesting that the hedonic estimate of the direct effect of conversion reflects little more than the monetary savings associated with conversion.

Considering water savings, administrative costs, rebate outlays, and out-of-pocket conversion costs to the rebate recipient, I find that the Cash-for-Grass program costs \$4.84/kgal-saved. I estimate the cost of water supply as the sum of the annual water bill for an average single-family customer and the opportunity cost of scarce water, which I base on Nevada agriculture to urban water sales. Comparing the two cost estimates, I find that the program saves water for less than the cost of supply. I also calculate net benefits by subtracting the sum of administrative and conversion costs from the sum of the direct effect of conversion and the value of scarce water. The program generates net benefits equal to \$2.00/ft^2 of desert landscape converted.

I organize the remainder of the paper as follows. Section 2 provides background on the Cash-for-Grass program. I present my analysis of water savings in section 3. Section 4 describes my estimates of the direct and spillover effect of conversion to desert landscape on property values.

Section 5 estimates program costs per gallon saved and program net benefits. Section 6 concludes.

2 The Cash-for-Grass rebate program

The Southern Nevada Water Authority's (SNWA) Cash-for-Grass rebate program is a voluntary, incentive based demand-side-management program that provides a cash rebate for Las Vegas area water customers that replace their lawns with desert landscape.[7] of The program began as a pilot study in 1996 and was rolled out to all customers beginning in 1998. Though the program has undergone several administrative regime changes,[8] throughout most of the program's history participants have received a one-time check from the water authority determined by the size of the conversion. Currently, program participants receive \$2.00 per square foot of lawn replaced[9] and can receive a maximum rebate of \$300,000.[10] The program also stipulates customers convert a minimum area and requires that conversions remain in place in perpetuity.[11] Very few customers renege on their agreement to maintain their conversion.[12]

To become a program participant, single-family applicants undergo a multi-step process illustrated in Figure 1 that begins with submitting an application and culminates with receiving their check. In between, program applicants review and verify requirements with water authority staff

[7]Current and historical program details derive from conversations with and information sent by SNWA staff members.

[8]At the beginning of the program, single-family participants received a \$5 water bill credit for every 1000 gallon reduction relative to baseline average water use. Halfway through the year 2000, however, the water authority began issuing rebates (still in the form of a water bill credit) based on the size of the conversion. Cash-for-Grass program participants continued to receive rebates in the form of a water bill credit until March, 2003, when the water authority began sending one-time checks.

[9]https://www.snwa.com/rebates/wsl.html. The \$2.00 per square foot rebate is valid for conversions of 5,000 square feet or less. Beyond 5,000 square feet, the rebate falls to \$1.00 per square foot.

[10]There has always been a maximum allowable rebate, but this limit does not appear to affect participant decision making, at least since the Fall of 2001 when the limit was set at \$25,000. Since this date, no participant has even approached 75 percent of the limit. Early on in the program, however, limits may have been binding. Initially, single-family participants could receive no more than \$400, and during this stage of the program, 38 percent of participants received a rebate of \$400. Program requirements soon changed such that single-family participants received \$0.40 per square foot of lawn converted to desert landscape for the first 2,500 square feet of lawn replaced. In other words, participants could earn up to \$1,000, even if they converted more than 2,500 square feet. Under this regime, 11 percent of participants converted more than 2,500 square feet. For both the \$400 and \$1,000 limits, however, the distributions of area converted appear to be reasonably continuous, suggesting that these early limits on the allowable rebate had little effect on participants' decision of how large a conversion to undertake.

[11]The water authority relaxed the minimum conversion requirement in 2004. I explore the impact of this change in Appendix B. Prior to June, 2009, customers agreed to keep the conversion in place for 10 years. I explore the impact of this change in Appendix C.

[12]I have "back conversion" data since 2004. These data show that the average number of back conversions occur at an annual rate of 4.5 back conversions per year, or less than one tenth of one percent of all conversions (pers. comm. K. Sovocool, February 2015).

during pre- and post-conversion site visits as well as undertake the conversion itself. Most applicants hire a professional landscaper. In 2014, for example, only about a quarter of applicants performed the conversion themselves.[13] For those that employ a landscaper, conversions cost around $3 per square foot, though the cost can be higher for those installing artificial turf (which does qualify as "desert landscape" under the terms of the Cash-for-Grass program).[14] It takes the average applicant a little over 5 months from the date of application to the receipt of the check.[15] In the empirical models that follow, I attempt to account for the transient behavior likely present between when applicants first signal interest in the program (application) to when applicants complete the process (enrollment date).

3 Water savings

3.1 Data and summary statistics

I derive estimates of water savings using single-family Cash-for-Grass program participant information and monthly single-family residential water consumption data for the Las Vegas Valley Water District (LVVWD), the largest water utility in the Las Vegas region. The Southern Nevada Water Authority provided both data sets. Program participation data include all participants from the inception of the pilot study in 1996 through June 12, 2014. These data include the parcel identifier, size, and rebate value of the conversion, the participant type (e.g. single-family), for most conversions the date the program participant applied for the rebate, and for all conversions the date the program participant became enrolled in the program.

I focus on single-family participants that undertake one conversion. Single-family participants comprise nearly 90 percent of all conversions, and nearly 90 percent of single-family participants perform one conversion.[16] 80 percent of the observations of single-family participants undertaking one conversion include both the application and enrollment dates. Among these observations, the average period between application and enrollment date spans about 5 months (150.6 days). Since

[13] pers. comm. K. Sovocool, February 2015.

[14] Cost estimates derive from conversations the author had with several Las Vegas area landscape professionals in March, 2016.

[15] pers. comm. M. Morgan, October 27, 2015.

[16] About 10 percent of single-family participants undertake two or more conversions. Golf courses, however, are the most likely participant category to undergo multiple conversions. Of the 33 golf courses that have participated in the Cash-for-Grass program, 27 have undertaken multiple conversions.

I use time relative to application date in my event studies, I proxy for missing application dates by subtracting the average 5-month time period from the enrollment date for observations missing an application date.

Monthly single-family consumption runs from January, 1988 through April, 2014. I restrict consumption data to those service meters that supply a single parcel. Meters that serve multiple parcels or parcels served by multiple meters are primarily associated with larger properties.[17] I reassign negative water consumption values (less than 0.01 percent of observations) to 'zero' upon recommendation from water authority staff. Negative water consumption values can occur due to billing adjustments or corrections for over-estimated meter readings, which sometimes arise if a utility staff member cannot read a meter and must instead estimate that month's consumption.[18] Because I observe water consumption at the service meter level and match service meter identifiers with parcel identifiers, my unit of analysis is a parcel, not an individual. Service meter identifiers do not change when customers move and I do not observe changes in the name of the individual connected to a service meter.

Merging the water consumption with program enrollment information yields a panel of over 64 million monthly water use observations from 309,608 parcels. Of the 309,608 parcels, 26,488 parcels participate in the Cash-for-Grass program (about 9 percent). In addition, prior to program enrollment, participating parcels demand more water on average than non-participating parcels (23.8 kgal/month vs. 15.1 kgal/month). Table 1 summarizes these results. Figure 2 illustrates the cumulative number of conversions over time. Few conversions took place in the late 1990s and early 2000s, however the number of annual participants increased sharply after 2003. A second jump in participation occurred between 2006 and 2008, but after 2008, program participation has steadily declined. Figure 3 illustrates the annual number of conversions and associated major changes in the subsidy rate and average water bill. In February of 2003, the rebate was increased from $0.40 to $1.00 per square-foot converted, and in September of the same year, the average water bill increased over 25 percent. These changes were followed by an increase in the number of conversions from 225 in 2002 to 945 in 2003 to 4,456 in 2004. The subsidy rate increased again in December, 2006 to $2.00 per square-foot. This was followed by an approximate 8 percent increase in the

[17] pers. comm. M. Morgan, June 30, 2014.
[18] pers. comm. M. Morgan, October 28, 2015.

average water bill in February, 2007. The number of conversions subsequently increased from 1,735 in 2006 to 2,958 in 2007. In January, 2008, the subsidy decreased to $1.50 per square-foot and effectively remained at this level through June, 2014. Water rates, however, continued to increase. In May 2008, January 2010, and January 2011, the average water bill increased by approximately 17.5 percent, 6 percent and 5.5 percent respectively. Despite these increases in price, participation steadily declined since 2008.

An average participant converts 1,348 square feet of lawn to desert landscape (approximately 0.03 acres), but average conversion area has fluctuated since program inception. As illustrated in Figure 4, in 2003 the average converted area peaked at 1,703 square feet, but declined to just over 1,000 square feet by 2014. Falling converted area could be because recent participants have less grass area to convert. The average property size in the LVVWD has shrunk in the past 30 years, and in 2004, communities began restricting new homes from planting grass in front yards.[19] Both factors would contribute to newer homes having smaller yards.[20]

Average water use among participating parcels prior to program enrollment runs strikingly parallel to average water use among non-participating parcels, especially before the program begins in 1998. Figure 5 summarizes annual water consumption since 1988 for participating parcels prior to Cash-for-Grass program enrollment, and non-participating parcels. Sample sizes for each group change over time due to new home construction, homes being removed from the LVVWD service area,[21] or enrollment of participating parcels into the rebate program. In the empirical models described below, I rely on a differences-in-differences design. In my context, the validity of a differences-in-differences design relies critically on the assumption that average water use among participating parcels would have paralleled average water use among non-participating parcels in the absence of the rebate program. The parallel trends in water use between both groups prior to the inception of the program illustrated in Figure 5 provides at least a necessary (if not sufficient) condition for credibly assuming parallel trends.

[19] pers. comm. SNWA staff, March 14, 2016 and May 8, 2017.

[20] Additionally, since 2004, the water authority has allowed participants to convert less than 400 square feet provided the conversion covers an entire front or back yard. Relaxing the minimum conversion size requirement may also contribute to falling average conversion size.

[21] In 1998, a large number of meters were transferred from the LVVWD to the Henderson water utility (pers. comm. M. Morgan, May 17, 2016).

3.2 Event study

I first illustrate water savings using an event study. Specifically, I estimate the following model:

$$Q_{it} = \sum_{j=-60}^{60} \kappa_j \mathbb{1}\left[\tau_{it} = j\right]_{it} + \mu_i + \delta_t + \epsilon_{it} \tag{1}$$

where Q_{it} describes water use in 1000 gallons for parcel i in month of sample t, μ_i are parcel fixed-effects, δ_t are month of sample fixed-effects and ϵ_{it} is the error. I define event time τ_{it} in relation to application date (thus τ is undefined for non-participants). For example, $\tau = -12$ for a parcel observed 12-months before the month of application.[22] κ_j describes average water use across all participants j months relative to the application date (net of parcel and seasonal fixed-effects). To avoid collinearity, I omit $\kappa_{j=0}$. κ_j therefore represents average water use relative to the application month. Month of sample fixed-effects soak up average seasonal fluctuations in water use and parcel fixed-effects control for average water consumption differences across parcels. I select a five-year window around the application month, dropping all participant observations outside the five-year window. Finally, I cluster standard errors at the parcel level.[23]

Figure 6 plots resulting point estimates and 95 percent confidence intervals of κ_j from Eq. (1), and clearly illustrates a reduction in water use resulting from conversion to desert landscape. Apart from seasonality not fully captured by the time fixed-effects, there does not appear to be any noticeable trends in water consumption prior to program application. An absence of pre-trends lends credibility to fully attributing the drop in water use illustrated in Figure 6 to conversion. And while the water savings achieved by conversion looks to be largely maintained, the central tendency of the coefficient estimates in months following application exhibits a mild increase, suggesting a small erosion in savings in the months following application and subsequent conversion. Overall, Figure 6 implies conversion to desert landscape saves about 5,000 gallons per month.

The event study exhibits transient behavior around the month of application. Conversion takes place sometime between the application date (solid vertical line in Figure 6) and the enrollment date, which occurs on average five months later (indicated by the dashed vertical line in Figure

[22] I could also define event time in relation to enrollment date. However, since the date of application indicates the first time I observe participants signaling interest in the program, defining the event relative to application seems most sensible. I present event study results defining τ in relation to enrollment date in Appendix B.

[23] McCrary (2007) suggests clustering at the parcel level to account for bias caused by a changing sample size throughout the event window.

6). Between application and enrollment, a steady decline in water use can be explained by two factors; conversion, and the possibility that applicants reduce or stop irrigating their lawns following application. But much of the decline I attribute to conversion takes place before the date of application, suggesting some anticipatory behavior on the part of applicants. It could be that applicants stop watering their lawns even prior to application.[24] It could also be that the conversions for which I imputed an application date took much longer than the average five months. In my models that follow, I attempt to eliminate this transient behavior from my savings estimates, both in my model specification and in robustness checks to my main results.

3.3 Empirical approach

To quantify average savings, I estimate a panel fixed-effects model, a generalization of the canonical two-period differences-in-differences design.

$$Q_{it} = \alpha[\text{pre-period}]_{it} + \beta[\text{post-enroll}]_{it} + \mu_{im} + \delta_{tc} + \epsilon_{it} \tag{2}$$

In Eq. (2) Q_{it} is again monthly water consumption (in kgal) for parcel i in month-of-sample, t. Post-enroll is an indicator for months following program enrollment. Estimates of β therefore describe the change in water use due to conversion to desert landscape. Since the event study illustrated some transient behavior prior to enrollment, I further include a pre-period indicator that describes months between the application date and enrollment date. μ_{im} and δ_{tc} are parcel by month-of-calendar year and month-of-sample by cohort fixed-effects, and ϵ_{it} is the error. Parcel by month-of-calendar year fixed-effects control for average seasonal differences across parcels. Month-of-sample by cohort fixed-effects attempt to control for possible compositional differences in parcel characteristics that would invalidate the differences-in-differences design. Newer homes in Las Vegas tend to be built on smaller lots and may have more water efficient appliances. Furthermore, since 2003, new home construction regulations preclude lawns in the front yard. Such newer home characteristics may induce differential trends in water use compared to older homes. Including

[24] But un-watered grass dies quickly in Las Vegas, and to be eligible for the rebate, residents must show that they have been maintaining a lawn. However, water authority staff conducting pre-conversion site visits have to make judgment calls regarding this requirement, and some variation regarding what constitutes a maintained lawn may have allowed for approval of some applicants that ceased watering lawns well before the date of application. Though this is purely speculative, a water authority staff member did explain to me that in earlier years, the standards for what constituted a maintained lawn were not held to as strictly as they are now.

month-of-sample by cohort fixed-effects attempts to control for any such differential trends. Since I match service meters to parcels, new home construction corresponds with the first year in which I observe a parcel in my data. I define five cohorts based on the year a parcel first appears in my panel: 1988-1989 define cohort 1 and comprise 45 percent of observations, 1990-1994 define cohort 2 and comprise 15 percent of observations, 1995-1999 define cohort 3 and comprise 16 percent of observations, 2000-2004 define cohort 4 and comprise 15 percent of observations, and 2004-2014 define cohort 5 and comprise the remaining 9 percent of observations.[25]

The event study results suggest that initial water savings may erode over time. To quantify any erosion in water savings I additionally interact my post-enrollment indicator with a monthly linear time trend describing the number of months past enrollment. I further include a quadratic term to explore the rate at which any erosion in savings takes place.

$$
\begin{aligned}
Q_{it} = &\alpha[\text{pre-period}]_{it} + \beta_1[\text{post-enroll}]_{it} \\
&+ \beta_2[\text{post-enroll}]_{it}T_{it} + \beta_3[\text{post-enroll}]_{it}T_{it}^2 + \mu_{im} + \delta_{tc} + \epsilon_{it}
\end{aligned}
\tag{3}
$$

3.4 Building alternative control samples

Participants voluntarily join the rebate program and may therefore possess systematically distinct characteristics from non-participants that would bias my water savings estimates. While I believe Figure 5 illustrates strong evidence of parallel trends and my month-of-sample by cohort fixed-effects further address concerns regarding compositional differences over time, one may still be concerned that there remains underlying differences between participating and non-participating parcels that would result in biased estimates. To address any remaining concerns, I construct three additional control groups intended to better reflect underlying characteristics of the sample of participating parcels.[26]

[25]The year when a parcel enters the panel will approximate the time of construction for most of the parcels in my sample in cohorts 2 and above. Exceptions include homes previously on groundwater that switch to city water. Many homes in cohort 1, however, will have been built prior to 1988. Cohort 1 homes should therefore be interpreted as homes built in or before 1989.

[26]In other words, in constructing additional control samples, I attempt to avoid any remaining selection bias not accounted for by my main differences-in-differences specification. To address selection bias, researchers often pursue a matching strategy or seek an appropriate instrument. Matching essentially balances the treatment and control group along observable dimensions, and therefore assumes that unobservable characteristics of the treatment group equal unobservable characteristics of the control group (on average), or that any unobservable characteristics of the treatment group do not affect the selection process and outcome variable. Instrumental variable strategies require instruments uncorrelated with any unobservables (i.e. exogenous instruments). In essence, both approaches select a control group that would have been affected by the policy in the same way as the treated group. Addressing selection,

First, I drop non-participants. By dropping non-participants, I avoid selection problems since my sample now includes only those who participate in the program. The control sample becomes those participants yet to convert.[27] Second, I build a control sample from a group of non-participating parcels that applied for the rebate, but did not become enrolled. I refer to these non-participating parcels as do-not-finishers, or DNF's. Among all non-participants, these DNF parcels are arguably the most similar to participants since they were not only aware of the rebate, but applied for the rebate as well. DNF's make an imperfect control, however. Among DNFs, over 70 percent do not finish because their application expired or they dropped out of the program and the reasons for dropping out or not following through may be correlated with water use.[28]

Third, I match participating parcels with non-participating parcels on lot size and July water consumption 2, 3, 4, and 5 years prior to enrollment into the Cash-for-Grass rebate program.[29] I use the Mahalanobis nearest-neighbor distance metric and match with replacement. I begin with a balanced panel to ensure that parcels have consumption data 5 years prior to conversion, and extract matches by running the Stata `teffects` command on a dummy outcome variable. I do not match on the year immediately prior to conversion since the event study suggests that water consumption may start to fall as much as a year before application and subsequent enrollment. Matching on 4 years of pre-enrollment consumption attempts to capture the downward trend in water use exhibited by Figure 5, and I match on July water consumption since parcels with similar peak consumption tend to have corresponding water use patterns throughout the other months of the year. Also, choosing only one month each year keeps the number of matching variables to a minimum. I build my matched control sample by pooling all non-participating parcel matches, keeping track of parcels matched more than once and weighting such parcels by the appropriate frequency in my regressions.

therefore, becomes an exercise in building a valid control.

[27]Dropping non-participants is not without its problems. As shown and discussed by the ongoing work of Borusyak and Jaravel (2016), differences-in-differences estimates derived from samples without a control can underweight long-term impacts, which in my context could lead to over-estimating savings.

[28]A further 16 percent of DNF's ineligible. Among those that are ineligible, the most common reason is a lack of turf. To be approved, applicants must demonstrate that they have been maintaining a lawn. If the grass is dead, or non-existent, the water authority staff may reject the applicant during the pre-conversion site visit. I do not consider ineligible DNF's as part of my control sample.

[29]For example, I match parcels that convert in 2010 with non-participating parcels on lot size and water consumption in July of 2005, 2006, 2007 and 2008. For parcels that convert in 1998, I match on July consumption in 1994, 1995 and 1996. Additionally including July 1993 consumption encountered collinearity issues. Davis *et al.* (2014) also matches on pre-conversion (electricity) consumption in their study of an appliance rebate program in Mexico City.

Figure 7 and Figure 8 compares annual water use of participating and non-participating parcels for the DNF and matched control samples, respectively. Both the DNF and matched control sample exhibit more similar average water consumption patterns than the full sample of non-participating parcels, and generally exhibit parallel trends prior to the beginning of the program in 1998.

3.5 Results

Water savings: main results Table 2 shows results from estimating my main model, Eq. (2).[30] In all models, I report parcel clustered standard errors in parentheses and suppress the coefficient estimate on the pre-period indicator. The pre-period indicator does little more than control for transient water behavior and is unimportant for understanding savings.

Focusing on the first five columns, the negative point estimates of the post-enroll indicator imply that conversion to desert landscape saves water. Column 1 shows results from estimating Eq. (2) with the full panel described in section 3.1. In column 2 and 3, I drop observations of program participants within 12 and 24 months of the month of enrollment. These two "donut" specifications attempt to capture the difference between steady state behavior before and after conversion to desert landscape by avoiding any transient behavior around the time of conversion not already accounted for by the pre-period indicator. The consistency between the estimates in columns 1, 2, and 3 indicate that there remains little biasing transient behavior around the time of conversion after controlling for the pre-period between application and enrollment dates. In column 4, I estimate Eq. (2) using a balanced sample. Column 4 aims to test for the robustness of water savings to differences across users not captured by the month-of-sample by cohort fixed-effects. The similarity between the estimates in column 1 and 4 indicate that any differences across cohorts do not affect my results, or that my time-cohort fixed-effects adequately control for any such differences. In Column 5, I limit observations to parcels that only have positive values of consumption throughout the panel. Excluding parcels experiencing zero water consumption attempts to control for properties under foreclosure that may have been vacant for some time. Though less precise, the estimate of savings in column 5 compare favorably to the savings estimate in column 1.

[30] I implement these and all following panel fixed-effect models, as well as my hedonic models discussed in section 4, using `reghdfe` (Correia, 2016).

In columns 6 and 7 of Table 2 I test the stability of savings for a given conversion by estimating Eq. (3) with the full sample. Results from these two models demonstrate that the average conversion experiences about a 5 gallon per month erosion in water savings, but that this erosion rate decreases over time (row 3, col. 7). The erosion rate is statistically significant, and possibly makes sense as larger, mature plants will require more water than younger plants. However, at one tenth of a percent of total savings, the erosion rate hardly seems practically relevant. I conclude that conversions to desert landscape maintain their savings over the long term.

Overall, water savings estimates in Table 2 display remarkable consistency across specifications, and demonstrate an average conversion to desert landscape saves about 5,000 gal/month, confirming the results from the event study.[31] 5,000 gal/month represents an approximate 21 percent decrease in baseline water use, and for a customer in 2013, corresponds to about \$150 in annual water bill savings, or a 30 percent reduction. Appendix A provides details of these calculations. Appendix B includes further water savings results. In particular, I explore the effects of various fixed-effects specifications, robustness to parcels that exit before the end of the sample, and the effect of two program policy changes.

Water savings: additional control samples Table 3 shows results from estimating Eq. (2) with each of the three alternative control samples discussed above. All estimates are significant at the 1 percent level, and I cluster standard errors at the parcel level. For purposes of comparison, column 1 replicates the main specification result with the full sample. Column 2 includes only participating parcels. Column 3 shows estimates based on a control sample constructed from DNFs. This sample of DNF's includes only DNF's that do not later become rebate program participants. Finally, column 4 presents results derived from my matched sample. The estimates derived from the DNF and matched control samples fall about 15 percent lower than the estimate from the main specification. However, each alternative control sample still demonstrates a clear reduction in water use due to conversion to desert landscape. I therefore conclude that my estimates of savings remain generally consistent across different control samples.

[31] I also run a set of falsification tests to examine the validity of the parallel trends identifying assumption behind Eq. (2). See footnote 75.

Heterogeneous effects across time Since the program has been in place for over fifteen years, one might expect savings to have fluctuated over time. Landscape professionals may have become more skilled at installing water saving landscapes (a learning-by-doing argument), and variation in the subsidy level may induce participants with heterogeneous characteristics that differentially affect program outcomes. To explore the impact of time, I estimate the following model:

$$Q_{it} = \alpha[\text{pre-period}]_{it} + \sum_k \beta_k[\text{post-enroll}]_{it} \times \mathbb{1}[k] + \mu_{im} + \delta_{tc} + \epsilon_{it} \tag{4}$$

where the index k represents program enrollment years. The coefficient estimate on the interaction of the post-enrollment indicator and the enrollment year describes savings achieved by conversions taking place for that enrollment year. Absolute savings, however, depend upon the conversion size, and as Figure 4 demonstrates, the average conversion area has changed over time. In order to make appropriate comparisons across years, therefore, I normalize the estimates of savings by the average converted area in each year. Figure 9 shows the results of this exercise, illustrating savings achieved in each year of the program normalized by the corresponding annual average conversion area, scaled up to a per annum basis in order to compare my estimates with water authority estimates.[32] The figure demonstrates a distinct 'U'-shaped pattern, achieving an initial peak of 60 gal/ft^2/year in 2001, early in the program. Savings then fall quickly to a low of 39 gal/ft^2/year in 2005. Since 2008, however, normalized savings have a exhibited steady upward trend.[33]

The 'U'-shaped pattern observed in Figure 9 does not appear consistent with a learning-by-doing story among landscape professionals because savings achieved a peak value early in the program,

[32]Normalized savings equals $f(\hat{\beta}_k, A_k) = c\frac{\hat{\beta}_k}{A_k}$, where $\hat{\beta}_k$ represents the estimate of savings in 1000 gal/month per average conversion in year k derived from Eq. (4), A_k represents average converted area in year k, and $c = -12,000$, which converts a negative change in water use in kgal/month to a positive savings in gal/year. Because I have the universe of conversion records within the LVVWD, I consider A_k a fixed parameter and calculate standard errors as: $\text{Var}\left[\frac{c}{A_k}\hat{\beta}_k\right] = \frac{c^2}{A_k^2}\text{Var}\left[\hat{\beta}_k\right] = \frac{c^2}{A_k^2}\hat{\sigma}^2_{\beta_k} \implies \hat{se}_k = \sqrt{\frac{c^2}{A_k^2}\hat{\sigma}^2_{\beta_k}} = \frac{c}{A_k}\hat{\sigma}_{\beta_k}$. If instead, I consider A_k a random variable I would apply the Delta method to estimate standard errors for $f(\hat{\beta}_k, \hat{A}_k) = c\frac{\hat{\beta}_k}{\hat{A}_k}$. In particular, if $\sqrt{n}(\hat{\theta} - \theta_0) \xrightarrow{d} \mathcal{N}(0, V)$, then $\sqrt{n}(f(\hat{\theta}) - f(\theta_0)) \xrightarrow{d} \mathcal{N}(0, AVA')$. Dropping hats, k subscripts, and c for clarity, in my context, $f(\cdot) = \frac{\beta}{A}$, $\theta = (\beta \quad A)'$, $V = \begin{pmatrix} \sigma^2_\beta & \sigma_{\beta A} \\ \sigma_{\beta A} & \sigma^2_A \end{pmatrix}$, and $A = \begin{pmatrix} \frac{\delta f}{\delta \beta} & \frac{\delta f}{\delta A} \end{pmatrix}$. $AVA' = \begin{pmatrix} \frac{1}{A} & \frac{-\beta}{A^2} \end{pmatrix} \begin{pmatrix} \sigma^2_\beta & \sigma_{\beta A} \\ \sigma_{\beta A} & \sigma^2_A \end{pmatrix} \begin{pmatrix} \frac{1}{A} \\ \frac{-\beta}{A^2} \end{pmatrix}$, implying that $AVA' = \frac{1}{A}\left(\frac{\sigma^2_\beta}{A} - \frac{\beta\sigma_{\beta A}}{A^2}\right) - \frac{\beta}{A^2}\left(\frac{\sigma_{\beta A}}{A} - \frac{\beta\sigma^2_A}{A^2}\right) = \frac{1}{A^2}\sigma^2_\beta + \frac{\beta^2}{A^4}\sigma^2_A - 2\frac{\beta}{A^3}\sigma_{\beta A}$ and $\hat{se}^{alt} = \sqrt{\frac{1}{A^2}\sigma^2_\beta + \frac{\beta^2}{A^4}\sigma^2_A - 2\frac{\beta}{A^3}\sigma_{\beta A}}$. Note that $\hat{se}^{alt}$ reduces to the expression for $\hat{se}_k$ for fixed A_k, since then $\sigma_A = \sigma_{\beta A} = 0$. How $\hat{se}_k$ compares to $\hat{se}^{alt}$ depends upon the relative size of $\sigma_{\beta A}$ and σ^2_A. For example, $\sigma_{\beta A} \gg \sigma^2_A$ implies that my simple estimate of the standard error, considering A_k fixed, may be larger than $\hat{se}^{alt}$. A similar argument holds for Figure 10.

[33]Though not shown, I observe similar behavior for the participant only, DNF, and matched control samples. Appendix B provides further discussion.

implying that landscape professionals already knew how to design and implement efficient conversions.[34] Rather, the 'U'-shaped pattern seems most consistent with heterogeneous participants effecting program outcomes over time. Middle period participants received the largest subsidies. In order to participate, therefore, early and late period participants may have derived some (or more) non-monetary benefits not experienced by middle period participants. Such non-monetary benefits could be correlated with greater awareness regarding water scarcity or a stronger desire to conserve, both of which could explain more efficient conversions. This discussion also highlights a trade-off between increasing participation, and increasing program efficiency. Participation was high in the mid to late 2000's (Figure 3), but this period also corresponds to the lowest per unit savings, implying the lowest "bang for the buck".

Figure 9 also presents a comparison between my estimates and two estimates derived by the water authority (Sovocool *et al.*, 2006). Beginning in 1995, the water authority conducted a pilot study that recruited participants and measured irrigation specific application rates. Sovocool *et al.* conclude that conversion to desert landscape saves 55.8 gal/ft^2/year. In a follow-up analysis, Sovocool *et al.* re-estimate savings, finding that desert landscape saves 54.7 gal/ft^2/year. The re-estimate draws from participants converting in 2003 and not recruited for the pilot program. I illustrate these two estimates in Figure 9 with horizontal dashed lines. The horizontal solid line represents my estimate of 5,000 gal/month normalized by the overall average conversion area (1,348 ft^2) and scaled to gal/ft^2/year. While the Sovocool *et al.* estimates lie well above my overall average estimate, their estimates fall within the confidence intervals for my annual estimates in 2001 and 2002. Therefore, water authority estimates may not be overstated, at least for early program participants.[35] These results stand in contrast to a general finding in the demand-side-management program evaluation literature that utility based estimates often overstate savings (Joskow and Marron, 1992; Loughran and Kulick, 2004; Allcott and Greenstone, 2012). But my results also demonstrate the importance of continued program assessment; as Figure 9 illustrates, savings may fluctuate throughout the life of the program.

[34] Or highly skilled do-it-yourselfers performed the early conversions, and landscape professionals did not expand their services to include conversions until the early to middle 2000's, after which they improved their ability to install efficient desert landscape.

[35] Arguing for the importance of including confidence bounds on estimates derived from statistical methods, Auffhammer *et al.* (2008) make a similar finding in the context of energy demand-side-management programs. That is, utility based estimates may not be misstated once researchers calculate bounds on their analyses (Auffhammer *et al.*, 2008).

Heterogeneous effects across pre-treatment consumption I also estimate a form of Eq. (4) for savings achieved by participants within pre-enrollment consumption deciles. I derive pre-enrollment consumption deciles from a 12-month average of water use beginning 24 months prior to the month of enrollment. I normalize water use, Q_{it}, by lot size, and define pre-enrollment consumption based on this normalized water use. Since I define lot size in 1000 ft^2, my outcome variable becomes monthly water use in gal/ft^2 of lot size. Compared to high demand-small lot consumers, similarly high demand-large lot consumers use water more efficiently, and therefore may not achieve the savings realized by their high demand-small lot counterparts from an equally sized conversion to desert landscape. Normalizing water use by lot size distinguishes the high demand-small lot consumers from the high-demand large lot consumers and avoids potentially downward biasing estimates of savings achieved by higher pre-enrollment consumption deciles.

Figure 10 illustrates that high-demand consumers, relative to lot size, achieve the greatest savings.[36] I find this result robust to a series of pre-enrollment consumption decile definitions and to the different control samples discussed above.[37] To the extent that consumers in the high pre-enrollment consumption deciles over-water relative to their low decile counterparts, high decile users will save more water than low decile users with the same conversion to desert landscape. The positive relationship between savings and pre-enrollment consumption decile therefore suggests that consumers in the higher deciles use water inefficiently relative to their low decile counterparts. This further implies that the highest savings from a program like Cash-for-Grass arise from the least efficient water using customers.[38]

[36]Investigating the Cash-for-Grass pilot study conducted in the late 1990's, Deoreo *et al.* (2000) make a similar finding, and in the context of the OPOWER experiment, Allcott (2011) also uncovers a positive relationship between savings and pre-treatment consumption.

[37]I additionally derive pre-enrollment consumption deciles based on a 24, 36, and 48-month average and find a similar relationship between normalized water use and pre-enrollment deciles. I also run models that consider non-normalized water use, deriving pre-enrollment consumption deciles from a 12, 24, 36, and 48-month average. Similar to my procedure for deriving Figure 9, I then normalize my resulting savings estimates by the average conversion area within each pre-enrollment consumption decile. I again find a positive relationship between savings and pre-enrollment consumption decile. Finally, I perform the above analyses with my participant only, DNF, and matched samples. For each sample and normalization method, savings continue to be positively related to pre-enrollment consumption decile. I provide further discussion in Appendix B.

[38]I am indebted to Peter Mayer for a clarifying discussion regarding the explanation for the relationship between savings and pre-enrollment consumption decile.

3.6 Rebates versus prices

If instead of implementing the Cash-for-Grass program the water authority had raised prices, what price increase would have induced the same aggregate savings? To answer this question, I begin with a partial equilibrium (i.e. supply-demand) framework, shown in Eq. (5).

$$\%\Delta P = \frac{\%\Delta Q}{\epsilon} \approx \frac{\Delta Q}{\epsilon Q} \tag{5}$$

Since the first conversions in my panel begin in 1998 I take the baseline quantity of water, Q, to be the average water use among all single-family residential customers in 1997, or 19,047 gallons/month. In 1997, 12 percent of the service population were future Cash-for-Grass program participants. But since all customers experience a price increase, to reduce consumption by the same aggregate amount as eventually achieved by the share of the service population that would participate, $\Delta Q = 5,000 \times 0.12 = 600$, the product of the share of the service population that became program participants and the savings these program participants achieved.[39] In their respective meta-analyses of residential water demand price elasticities, Espey *et al.* (1997) find a mean elasticity of -0.51 and Dalhuisen *et al.* (2003) find a mean elasticity of -0.41. Long-term elasticities are generally higher, and since this analysis asks how consumers in 1997 would have responded over a nearly 20-year period, I take the elasticity to be -0.5.[40] Based these values, Eq. (5) predicts that a 6 percent price increase would have achieved equivalent aggregate savings.

This hypothetical percentage price increase is relatively modest. Since 1958, customers of the LVVWD have experienced actual average price increases between 5.6 percent in 2011 and 26.8 percent in 2003.[41] Since all customers experience and respond to a price increase, large aggregate savings require comparatively small individual cutbacks. This fact drives the modest hypothetical price increase estimated by Eq. (5). As the share of the service population that participates in the rebate program increases (and continues to achieve the same average savings), so does the price increase required to induce the same aggregate savings. If the entire service population participates

[39] Consider a service population of N and define aggregate monthly savings as $N\sigma$, where σ is the per-parcel monthly savings. The aggregate savings of 12 percent of this service population saving 5,000 gallons per month is $0.12 \times N \times 5,000$. Equating the two implies: $N\sigma = 0.12 \times N \times 5,000 \implies \sigma = 0.12 \times 5000 = 600$.

[40] It would be preferable to derive an estimate of elasticity using Las Vegas specific data. While my data include historical water rates and consumption, I do not have individual income data. For this reason, I select elasticity estimates from the literature.

[41] pers. comm. SNWA staff, July 2014.

in the program, Eq. (5) predicts that a 53 percent price increase is needed to realize the savings achieved by the rebate.

The price increase analysis assumes homogeneous customers. But lower income households with little to no landscape may not be able to achieve much if any savings regardless of the price increase. Eliminating these households from the analysis above would increase ΔQ since it would decrease the effective service population saving water due to the price hike. Demand elasticity would also decrease, since wealthier customers tend to have lower water demand elasticities than poorer customers (Mansur and Olmstead, 2012). Both factors would drive up the estimate of the hypothetical price increase.

Price increases may also cause regressive outcomes. If both wealthy customers and poorer customers experience the same price increase, the ratio of wealthy customer demand elasticity to poorer customer demand elasticity roughly approximates the ratio of the change in Marshallian surplus of poorer to wealthier customers.[42] Taking at face value the point estimates of wealthy and poor residential water demand elasticities derived by Mansur and Olmstead implies that the reduction in surplus for poorer customers could be as much as 67 percent greater than the reduction in surplus for wealthier customers. So despite the fact that modest price increases could achieve large aggregate savings, heterogeneity in customer characteristics may increase the estimated price hike and create large differential, and arguably inequitable, welfare effects across customer types.

4 Value of desert landscape

4.1 Motivation

Desert landscape can affect participants' utility outside water bill savings. Those choosing to convert may find desert landscapes aesthetically pleasing (Walls *et al.*, 2015) or value signaling a commitment to environmental stewardship (Mustafa *et al.*, 2010). Desert landscapes also tend to

[42] In a standard partial equilibrium framework that assumes linear demand, the reduction in Marshallian surplus from reducing water consumption given a change in price is approximated by $1/2 \times (Q/P)\Delta P^2 e^{-1}$, where ΔP is the change in the price of water, P and Q are initial prices and quantities of water consumed, respectively, and e is the demand elasticity. If wealthy and poorer customers experience similar unit prices of water, i.e. $P_w/Q_w \approx P_p/Q_p$, where w refers to wealthy and p refers to poor, then the ratio of the two changes in Marshallian surplus is given by e_w/e_p. Under block pricing, it may be that $P_w/Q_w > P_p/Q_p$ since wealthier customers tend to use more water than poorer customers, and this increased usage is priced at a higher marginal rate. If $P_w/Q_w > P_p/Q_p$, the ratio of Marshallian surplus is $(e_w/e_p) \times \frac{Q_p/P_p}{Q_w/P_w} > e_w/e_p$. The estimate of e_w/e_p could therefore be thought of as a lower bound on the differential welfare effect.

require less maintenance than lawns.[43] However, replacing grass with drought tolerant flora may lead to increased energy costs. Because of the higher evapotranspiration rates[44] of lawns versus desert landscapes, conversion to desert landscape could increase local air temperatures (Bonan, 2000), leading converting properties to demand more air-conditioning. Importantly, neighbors of desert landscaped properties may also experience these negative energy spillovers and derive aesthetic utility from neighboring desert landscapes. Therefore, simply calculating the value of saved water will not necessarily capture the full value of desert landscape to participants and will fail to reflect any externalities that conversions impose upon neighbors.

Formalized by Rosen (1974), the hedonic property method provides a theoretically consistent method for estimating the private benefits of converting to desert landscape. Modeling property values as a function of the property's individual characteristics, Rosen showed that the effect of an individual characteristic on property values represents the benefit a consumer receives from the characteristic. Since Rosen, hedonics has become a widely used strategy for valuing non-market goods, especially environmental characteristics of properties (Davis, 2004; Greenstone and Gallagher, 2008; Muehlenbachs *et al.*, 2015). Hedonic estimates, however, will fail to capture benefits not communicated through housing prices, such as benefits from reduced water-transport costs[45] and the ability to reallocate scarce water to alternative or future uses. My hedonic estimates, therefore, may understate the true effect of conversion.

In the following analysis, I estimate the private, direct and spillover effects generated by conversions to desert landscape subsidized by the Cash-for-Grass program.[46] To estimate the direct effect of conversion, I make use of the variation in conversion status across properties. To estimate the spillover effect, I make use of the variation in a property's adjacency to a conversion. In other words, I characterize properties by their conversion status and whether they lie adjacent to prop-

[43]Cash-for-Grass program staff noted that reduced lawn maintenance appears to be a primary driver for individuals who apply for the rebate (pers. comm. K. Sovocool, February 2015).

[44]A process of simultaneous evaporation and plant transpiration.

[45]Water requires substantial energy to deliver, and reducing water use decreases greenhouse gas emissions and other pollutants through lower energy production. Reduced energy consumption also implies lower energy bills for the utility, freeing up funds for alternative uses.

[46]I am not the first to estimate the impact of desert landscape on property values. Both Baker (2004) and Rollins (2008) find that desert landscape increases Las Vegas home values. In particular, using a hedonic framework with neighborhood characteristics defined at the zip code level, Rollins finds that desert landscape increases home values by about 7 percent. Using a larger data set which includes more recent home sales as well as a highly spatial and temporally refined set of fixed-effects (quarter of sample by census block fixed-effects), I find smaller effects on home values due to desert landscape.

erties that convert. I focus on single-family participants as they account for over 90 percent of all conversions, have received the largest share of rebate monies (41 percent) and are responsible for the largest share of area converted (36 percent).[47] To maintain consistency with the water savings analysis, I limit my analysis to the LVVWD service area.

4.2 Data

I construct a panel of residential sales occurring within the LVVWD from historical Las Vegas area sales data provided by the Clark County Assessor's Office. I include sale price and date, home age, parcel, home, garage, and pool square footage, and finally the number bedrooms and bathrooms (full and half bathrooms). I convert all sale prices to $2014 using the CPI housing index, and drop observations outside of the first and ninety-ninth percentiles of the sale price distribution. I restrict my observations from January 1, 1996 to June 12, 2014, the latest program enrollment date.[48] I keep arms-length, single-family transactions, and further drop all parcels with a negative age, a parcel area of zero, or a home size of zero. Finally, I associate parcels with 2010 U.S. Census block areas using GIS software. On average, approximately 29 properties fall within each block.

Including properties that undergo unobserved structural changes may bias my analysis. Since the assessor data do not record changes in property characteristics, I develop three criteria for assessing whether a property may have undergone a structural change. The assessor data include the square footage of any additions made to a parcel, which often occurs if the property owner converts a garage to living space.[49] I drop parcels with positive addition area. The assessor data also provide the year of home construction, as well as the effective year of home construction. For most homes, these years are equivalent. If the construction year does not equal the effective construction year, I conclude the parcel likely underwent an addition, and drop such parcels. I drop remaining parcels that have a detached garage if the year built or effective year built of the detached garage does not equal the home construction year.

I merge the resulting panel of sales with the enrollment panel described in section 3 and a

[47] Multi-family participants have received 29 percent of total rebate monies and have converted 31.5 percent of the total converted area; golf courses received 20 percent of total rebate monies and converted 21.5 percent of the total converted area, and commercial and industrial participants have received 10 percent of total rebate monies and converted the remaining 11 percent of total converted area.

[48] The rebate pilot program begins in 1996, and the first enrollment occurred on May 6, 1996.

[49] pers. comm. E. Martinet, May 2016.

"neighbors" dataset that I construct using GIS software. I consider all single-family participating parcels, some of which undertook more than one conversion.[50] Illustrated in Figure 11, I define neighbors as parcels that lie directly adjacent to any single-family participating parcel. Importantly, neighbors may themselves be participating parcels. The neighbors dataset contains enrollment dates and converted areas of conversions *adjacent* to the parcel.[51]

Six variables characterize conversion to desert landscape. P_{it} takes one if parcel i has converted by the sale date, t. N_{it} takes one if parcel i lies adjacent to a conversion by the sale date, t. DP_i and DN_i describe parcel i's status as an eventual program participant (i.e. the parcel converts at some point in the panel) or eventual neighbor of a program participant, respectively.[52] Pa_{it} and Na_{it} describe the total converted area and total adjacent converted area for parcel i at sale date t.[53] If a parcel neighbors multiple participating parcels prior to the sale date, the total converted area from all adjacent participating parcels define Na_{it}.

Table 4 displays summary statistics for the resulting panel described above, comparing average home characteristics across non-participants and participants ($P = 0$ vs. $P = 1$) and non-neighbors and neighbors ($N = 0$ vs. $N = 1$). Panel (a) shows results from estimating Eq. (6). Panel (b) shows results from estimating a model with repeat sales (i.e. including parcel fixed-effects in Eq. (6)). The repeat sales model contains fewer observations because not every home observed in the assessor data sells multiple times.

Compared to non-participants or non-neighbors, panel (a) and panel (b) demonstrate that participating homes and homes neighboring participants are lower priced, older, sit on larger lots, have larger garages, and more likely have a pool. In both panels, the average magnitude of the remaining home characteristics excepting half bathrooms compare similarly across participants and non-participants, and across neighbors and non-neighbors. Despite the large difference in means

[50] A small number of enrollments that the water authority considered non-single-family match with the single-family assessor data. I drop these observations. In general, though, the water authority's classification of single-family agrees closely with that of the assessor's office.

[51] Some neighboring enrollments occurred on the same date. For these cases, I include the total rebate and converted area for that particular enrollment date.

[52] In the language of differences-in-differences, P_{it} and N_{it} describe an interaction between the treatment indicator DP_i or DN_i, and a post-treatment period indicator.

[53] For example, consider a parcel that sells three times and undergoes two conversions of 500 square feet between the first and second sale, and another conversion of 500 square feet prior to the third sale, and lies adjacent to a property that converts 500 square feet between the second and third sale. In this illustrative example, $DP_i = DN_i = 1$ for each sale observation. In addition, for the first sale observation, $P_{it} = N_{it} = Pa_{it} = Na_{it} = 0$. For the second sale observation, $P_{it} = 1$ and $Pa_{it} = 1000$, and $N_{it} = Na_{it} = 0$. Finally, for the third sale observation, $P_{it} = N_{it} = 1$, $Pa_{it} = 1500$, and $Na_{it} = 500$.

for the age and lot size variables, I show in Appendix C that the distributions of age and lot size overlap for participants and non-participants and neighbors and non-neighbors.

Table 5 displays summary statistics after restricting sales to pre-2007.[54] Similar to Table 4, Table 5 shows that participating and neighboring homes selling before 2007 are older, sit on larger lots, have larger garages, and more likely include a pool, compared to their non-participating or non-neighboring counterparts. But unlike in Table 4, participating and neighboring homes sell for higher average prices, suggesting that the housing crash may have disproportionately affected areas concentrated with rebate program participants. The average magnitude of the remaining home characteristics excepting half bathrooms compare similarly across participants and non-participants, and across neighbors and non-neighbors.

Table 4 and Table 5 suggest that participants in the Cash-for-Grass rebate program and their immediate neighbors reside in older sections of Las Vegas. Older areas of Las Vegas may be correlated with unobserved characteristics that influence the value of desert landscape. Below I propose an empirical strategy with a rich set of spatial and temporal fixed-effects to address concerns over unobserved characteristics.

4.3 Empirical strategy

I estimate the direct and spillover effect of conversion to desert landscape with the panel fixed-effects model shown in Eq. (6). I regress the natural log of sale price in \$2014 for parcel i on sale date t on the indicators characterizing conversion to desert landscape described above, home characteristics, Z_i and census block-by-quarter fixed-effects, b_{iq}. For each property, the vector Z_i includes parcel, home, pool, and garage square footage, home age, and the number of bedrooms, full bathrooms, and half bathrooms.

$$\ln p_{it} = \alpha_1 DP_i + \beta_1 P_{it} + \alpha_2 DN_i + \beta_2 N_{it} + \delta Z_i + b_{iq} + \epsilon_{it} \tag{6}$$

[54] I exclude summary statistics for a pre-2007 repeat sales model since so few observations exist. Under repeat sales, $N_{P=1} = 15$ and $N_{N=1} = 87$.

The intuition behind Eq. (6) is differences-in-differences.[55] β_1 describes the approximate[56] average percentage change in the value of a parcel that converts to desert landscape. β_2 describes the approximate average percentage change in the value of a home that lies directly adjacent to a property that converts to desert landscape. β_2 therefore describes the spillover effect, or externality associated with neighboring conversions.

Census block-by-quarter fixed-effects control for average differences across 2010 U.S. Census block boundaries in each quarter of the sample. I am able to group parcels within 6,819 blocks, with the average block in each sample containing about 29 parcels. By controlling for neighborhood effects at such a refined geographic level, I endeavor to minimize concerns that unobserved neighborhood fixed-effects will bias results.

Larger conversions may have stronger impacts than smaller conversions. To explore this possibility, I interact the variables describing area converted or area adjacent at the time of sale (Pa_{it} and Na_{it}) with P_{it} and N_{it}, as shown in Eq. (7). θ_1 and θ_2 describe the percentage change in the value of a home from an additional square foot of desert landscape, or an additional adjacent square foot of desert landscape.

$$\begin{aligned} \ln p_{it} &= \alpha_1 DP_i + \beta_1 P_{it} + \theta_1 (P_{it} \times Pa_{it}) \\ &\quad + \alpha_2 DN_i + \beta_2 N_{it} + \theta_2 (N_{it} \times Na_{it}) + \delta Z_i + b_{iq} + \epsilon_{it} \end{aligned} \tag{7}$$

4.4 Results

In the results below, I show that Cash-for-Grass subsidized conversion to desert landscape increases property values by about 1 percent with little evidence for any spillovers. In appendix C, I explore the potential effect of two policy changes on the value of desert landscape, investigate heterogeneous effects over time, and demonstrate robustness of my results to additional specifications.

[55] In their investigations into the impact of crime on property values, Linden and Rockoff (2008) and Pope (2008) employ a differences-in-differences strategy that considers "treatment" properties to be within a 0.1 mile radius of a residence of a sex offender, and "control" properties to be between 0.1 and 0.3 miles of a sex offender. In principle, my exploration of the spillover effect of desert landscape mirrors this strategy, but instead of defining treatment and control based on distance, I define treatment and control based on adjacency. My modeling of the direct and spillover effects of desert landscape is in part inspired by the model of health and education externalities proposed by Miguel and Kremer (2004).

[56] In semi-log models with explanatory indicators variables, Halvorsen and Palmquist (1980) show that the coefficients on these indicators do not directly describe percentage effects. However, for small coefficient estimates, the bias is minimal.

Main results Table 6 shows estimates of the direct and spillover effects of conversion to desert landscape. In all models, the coefficient estimates on the control variables effect housing prices in expected ways[57] and I cluster standard errors at the block level. Column 1, my preferred specification, shows results from estimating Eq. (6) with the full range of sale years, 1996 to 2014. Column 2 runs a similar model, but for sales restricted from 1996 to 2006. Restricting sales to pre-housing crisis dates tests the robustness of my column 1 estimates to any additional mortgage spillovers associated with the housing market crash not absorbed by the quarter-block fixed-effects. In column 3, I further address concerns regarding potential unobserved neighborhood and household characteristics by estimating a model that additionally includes parcel fixed-effects (i.e. a repeat-sales model). Across each specification, Table 6 demonstrates similarly positive estimates of the direct effect, suggesting my results are robust to concerns over unobserved neighborhood effects and housing crisis impacts.

Cash-for-Grass program requirements preclude conversions to barren landscapes. Furthermore, property owners in Las Vegas often leave potentially landscaped areas uncultivated, or covered in rock. Preference for desert landscape over barren landscape could partially explain my positive coefficient estimates for the direct effect. In columns 4 and 5, I test whether barren landscapes drive results by re-estimating the models in columns 1 and 2 with a subset of non-participants and non-neighbors that applied for the rebate program, were approved for the rebate, but never became enrolled. Since the water authority only approves applicants that have been maintaining a lawn prior to application, estimating my hedonic model with the control group of non-enrolled applicants creates a more direct comparison between desert landscape and turf landscape. The estimate of the direct effect in column 4, however, compares quite favorably to the estimates in columns 1 through 3. And while the estimate of the direct effect in column 5 is not statistically significant due to the reduced sample size, the magnitude of the point estimate falls within the range of the estimates in columns 1 through 3. These two results provide evidence that barren landscapes do not drive the positive, direct effect of desert landscape on property values.[58] In absolute terms, my results in

[57]The two exceptions involve the negative coefficient on full bathrooms in columns 1, 2, and 4, and the negative coefficient on half bathrooms in column 1. Toilets make up the largest share of indoor water use (Bennear *et al.*, 2013), and the negative coefficient on bathrooms may reflect consumers' recognition of higher water bills associated with an increased number of water-intensive fixtures.

[58]Importantly, I assume that non-enrolled applicants continued to maintain a lawn. But non-enrolled applicants may have converted to desert landscape and not taken the rebate. This seems unlikely to have taken place on a large scale. First, if approved, applicants would have little incentive to decline the rebate unless the terms of the

column 1 imply that conversion to desert landscape increase the value of a home by \$3,700, with 95 percent confidence bounds approximately between \$1,600 and \$5,700.[59]

Turning to the spillover effect, results in Table 6 show statistically insignificant and fairly precisely estimated zero effects across specifications. Klaiber *et al.* (2015) find positive spillovers associated with lawns in Phoenix and that consumers value cooler temperatures. One might therefore expect negative spillovers from converting grass to desert landscape, which may increase ambient temperatures around a property. My finding of no spillovers of desert landscape imply that conversion to desert landscape has no effect on micro-climates in Las Vegas, or that a combination of positive aesthetic or other positive spillover effects and negative micro-climates counterbalance each other. I am currently working towards securing electricity demand data to test for an effect of conversion to desert landscape on electricity consumption.

Effect of an additional square foot of desert landscape I present results of estimating the direct and spillover effects of an additional square foot of desert landscape in Table 7. The point estimates for the additional direct and spillover impact of an extra square foot of desert landscape (rows 3 and 6 of Table 7, respectively) are statistically indistinguishable from zero at the 5 percent level, implying that the presence, rather than the size, of the conversion primarily drives the effect of conversion to desert landscape. Furthermore, the estimates for the direct and spillover effects (rows 2 and 5 respectively), generally agree with those of Table 6, reinforcing the conclusion that the direct effect of desert landscape raises the value of a home by small a percentage.[60] And while

rebate were sufficiently burdensome to them. My discussions with water authority staff suggest that only a very few individuals do not follow through on account of program requirements. Second, if non-enrolled applications were converting on a large scale, one would not expect positive point estimates on the direct effect, since there would be little difference between treated observations and control observations. To develop a more precise sense of the relationship between subsidized conversions and total conversions, the water authority provided me with a summary of aerial footage analysis done in 2006, 2008 and 2010. Between 2006 and 2008, the total converted areas are about 50 percent of the change in turf determined by aerial footage. But between 2008 and 2010, aerial footage detected only about 25 percent of the total converted area subsidized by the Cash-for-Grass program (Brand, J. ASPRS Annual Conference, May 3, 2011 - slide deck). The accuracy of aerial footage may therefore make it challenging to directly assess the assumption I make that only those that enroll in the program undertake a conversion to desert landscape.

[59] Absolute increases represent the product of the average home sale and percentage effect corrected for the fact that in log-linear models, estimates on indicator variables do not have a direct interpretation as percentage effects (Halvorsen and Palmquist, 1980). $\$309,077 \times \left(e^{\beta} - 1\right) \approx \$3,700$.

[60] I do estimate positive, though very small, spillover effects in column 2, significant at the 10 percent level, and negative area effects in columns 1 and 2, also significant at the 10 percent level. While these results provide some evidence for a positive spillover effect that decreases in the size of the conversion, the weight of the evidence I present seems to point towards the conclusion of zero spillover effects.

not shown, the coefficient estimates on each covariate effect housing prices in expected ways.[61]

Relationship between water savings and capitalization In principle, hedonic estimates will capture all private benefits associated with conversion to desert landscape. Two obvious benefits include water bill savings and reduce lawn maintenance costs. Saving 5,000 gal/month yields \$150 in annual savings, and I estimate reduced lawn maintenance to be \$79 per year.[62] The present discounted value of an infinite stream of these savings with a 5 percent discount rate equals \$4,800. This value falls within the confidence interval of the increase in value from converting to desert landscape derived by the hedonic model above. For the present discounted value of water bill and maintenance savings to equal \$3,700, the consumers would need to be applying a 7 percent, which seems reasonable. These results suggest that prices of desert landscaped homes reflect monetary savings, but little to no other benefits associated with conversions. This finding is consistent with Myers (2016), who shows that home buyers fully capitalize differences in energy costs between homes utilizing different heating sources.

5 Estimates of cost per gallons saved and net benefits

Cost per gallons saved Since the beginning of the program, the water authority has rebated single-family residents converting once a total of \$53M (\$2014 dollars). I estimate that the resources required to administer the program equal 22.5 percent of total outlays, or about \$12M.[63] The average conversion saves 5,000 gallons per month, implying that total savings for the 26,488 conversions equal 1.6M kgal/year. Following Bennear *et al.* (2013), I convert total rebate outlays plus administrative costs to an annual equivalent by annuitizing \$65M using a 5 percent discount rate and a 30-year time horizon.[65] Dividing annual equivalent expenditures by total annual sav-

[61]The two exceptions involve negative coefficient estimates on full and half bathrooms in columns 1 and 2. As explained above, this could arise from consumers' recognition that more water-intensive fixtures, like toilets, may lead to higher water bills.

[62]A quick internet search for average lawn care costs in the Las Vegas area revealed that mowing and maintenance required \$37.52 per visit per quarter acre and a fertilization visit per quarter acre cost \$61.57. I normalize these values to costs per square feet, and multiply by the average conversion size, 1,348 ft^2. I further assume 3 fertilization applications (based on a posting about lawn care) and 12 mowing visits. The total annual cost comes out to \$78.60.

[63]I calculate the percentage based on the ratio of total labor costs and overhead to the budget for rebate outlays during the 2015/2016 fiscal year.[64] Since most of the rebates were funded through one-time connection charges, I ignore financing costs. Appendix D provides further discussion.

[65]Similar to the argument that Bennear *et al.* make for toilets, it is unlikely that savings from desert landscape will last indefinitely. I calculate annualized rebate outlays using the following formula: $\frac{r \times \text{Costs}}{1-(1+r)^{-t}}$, where $r = 0.05$

ings yields an annual program cost of \$2.65/kgal-saved.[66] If I additionally include out-of-pocket conversion costs to the rebate recipient (about \$54M) and repeat the calculations above, annual program costs increase from \$2.65/kgal-saved to \$4.84/kgal-saved. The water bill for an average customer in 2013 equals \$3.54/kgal. If the bill reasonably reflects the cost of supplying water, from the water authority's perspective, the Cash-for-Grass program saves \$0.89/kgal every year. But from a societal perspective, conserving water with the Cash-for-Grass program costs more than supplying the same amount. Appendix D provides further details.

Two caveats bear discussion. On the one hand, water bills tend not to incorporate the opportunity cost of scarce water supplies (Griffin, 2001), which Griffin suggests can be estimated using water market transactions. Edwards and Libecap (2015) report that the median price of a series of agriculture to urban water sales in Nevada during the 2000s equals \$0.06/gal. Most water sales grant the buyer a perpetual right to a certain amount of water each year. Therefore, I assume \$0.06/gal reflects the present discounted value of an infinite stream of benefits arising from the right to withdraw a gallon of water each year in perpetuity. Assuming a 5 percent discount rate, \$0.06/gal implies an annual measure of about \$3/kgal.[67] Including an estimate of the opportunity cost of scarce water therefore increases the real annual cost of supply from \$3.54/kgal to \$6.54/kgal. The program now appears economical, since it saves water for less than the cost of supply.

On the other hand, the measure of program costs per gallon saved ignores the possibility that some rebate recipients may have converted to desert landscape without the subsidy. Savings and associated out-of-pocket conversion costs from any such free-riders should not be attributed to program savings or program costs. I estimate that if free-riders account for over 39 percent of program participants, the program costs more than the cost of supply. In other contexts, free-riding has been found to account for at least half of all rebate recipients (Bennear *et al.*, 2013; Houde and Aldy, 2014; Boomhower and Davis, 2014). These studies, however, explore free-riding in rebates for relatively small ticket items (Bennear *et al.* investigate toilet rebates and Houde and Aldy

refers to the discount rate and $t = 30$ equals the number of years of assumed sustained savings.

[66]Bennear *et al.* (2013) find a high-efficiency toilet rebate program costs \$7.33/kgal-saved, and Price *et al.* (2014) find that a range of rebate programs cost \$0.39/kgal-saved (for low flow shower heads) to \$8.33/kgal-saved (for an additional low flow toilet), and that a desert landscape rebate costs \$4.51/kgal-saved, assuming the desert landscape stays in place for 25 years (assuming 25 years of sustained savings, the Cash-for-Grass program costs \$2.89/kgal-saved, or \$5.28/kgal-saved if I include the opportunity cost of scare water). Price *et al.* estimate cost-effectiveness using a levelized-cost method which divides the present value of costs by the present value of savings.

[67]Present discounted value (PDV) $= \frac{B(1+r)}{r} \implies B = \frac{0.05 \times 0.06}{1+0.05} = \$2.84$/kgal/year.

and Boomhower and Davis examine energy appliance rebates). In contrast, re-landscaping poses substantial costs and occurs infrequently. It therefore seems reasonable to hypothesize a small share of individuals planning to convert to desert landscape apart from the subsidy. However, estimating free-riding would be a valuable extension to this research.[68]

Net benefits While hedonic theory promises to recover willingness to pay for a non-market good, in practice, identifying welfare effects from hedonic analyses pose empirical challenges. Because I exploit panel variation, my estimates of the effect of desert landscape on property values measure a capitalization rate, not necessarily real benefits (Kuminoff and Pope, 2014; Muehlenbachs *et al.*, 2015). Kuminoff and Pope explain this capitalization rate can only be interpreted as a measure of benefits if the hedonic price schedule remains fixed throughout time.[69] Following the suggestion of Kuminoff and Pope, I test for a stable hedonic price schedule by estimating the direct effect of conversion in each year. Figure 16 in Appendix C illustrates consistent point estimates of the direct effect across time. I therefore assume that the hedonic price schedule is stable and the estimated direct effect reflects real benefits. I estimate the benefits per square foot of desert landscape by dividing the average increase in home values by the average conversion area.[70] I find benefits equal \$2.67/ft^2, with 95 percent confidence bounds ranging from \$1.18/ft^2 to \$4.16/ft^2.

Because the hedonic estimate of the direct effect of conversion only captures monetary savings from reduced water bills and maintenance expenditures (section 4), my measure of benefits does not reflect the value of scarce water. As above, I assume the \$0.06/gal median sale price reported by Edwards and Libecap reflects the value of the right to use a gallon of water every year in perpetuity. If one assumes that savings do not erode over time, then my results in section 3 imply that conversions save 44.5 gal/ft^2 every year in perpetuity. The product of the sale price (\$0.06/gal) and the average savings per square foot (44.5 gal/ft^2) therefore yields a rough approximation of the scarcity value of water embedded in converting one square foot to desert landscape, or \$2.66/ft^2.

I estimate total costs to be \$3.33/ft^2. The largest share of program costs arises from conversion

[68]One way forward is to estimate a demand model for desert landscape, and then extrapolate to demand at zero subsidy. Boomhower and Davis (2014) essentially take this approach.

[69]A second challenge associated with hedonic estimation of welfare effects involves estimating demand parameters for the purpose of deriving aggregate welfare impacts. Epple (1987) and Bartik (1987) provide clarifying discussions.

[70]Since my hedonic estimates include parcels undertaking more than one conversion, I include data from this larger set of participants in my analysis of net benefits. Total rebate outlays equal \$62M, the number of conversions equals 31,049, and the average conversion size equals 1,379 ft^2.

costs, \$3.00/ft^2. I again estimate administrative cost to be 22.5 percent of total rebate outlays, then divide by the number of conversions and the average conversion area.[71] Administrative costs come to \$0.33/ft^2. Rebates are simply transfers from the utility to the customer, and therefore do not represent costs from a societal perspective.

Ignoring scarcity, program net benefits equal -\$0.66/ft^2, with a 95 percent confidence interval of -\$2.15/ft^2 to \$0.84/ft^2. Including scarcity, net benefits increase to about \$2.00/ft^2. My estimate of net benefits, however, may not accurately reflect true net benefits. One the one hand, I have not incorporated any positive health and climate externalities arising from reducing the energy needed to treat and distribute water. Including positive externalities would increase net benefits.[72] On the other hand, I have ignored free-riders. Including free-riders would reduce the benefits arising from conversions as well as the associated conversion costs, leading to lower estimates of net benefits. But provided most rebate recipients would not convert to desert landscape without the subsidy, my analysis suggests that the Cash-for-Grass program may enhance welfare.

6 Conclusion

I have analyzed the water savings and net benefits generated by the Southern Nevada Water Authority's Cash-for-Grass rebate program. Using event studies and panel fixed-effects models, I estimate an average conversion saves about 5,000 gallons per month. Furthermore, these savings remain stable over time, and are robust to a series of specifications and control samples. The stability of savings suggests that program participants do not substitute to other water intensive activities, but simply cut back on outdoor irrigation. Finally, since savings per square foot are inversely related to incentives to participate, encouraging greater participation over the life of the program may have come at the expense of program cost-effectiveness.

Consumers value conversions to desert landscape. I find that a conversion increases the value of a Las Vegas single-family home by \$3,700 (about 1 percent), and that the present discounted value of estimated water bill and lawn maintenance savings essentially accounts for the entire increase in home values. I also find little evidence that conversions to desert landscape have any net impact on

[71] See footnote 70.

[72] I may also be understating net benefits if reductions in utility revenue are less than benefits from reduced operating costs. Appendix D provides further discussion.

neighboring properties. To further explore desert landscape externalities, I am securing electricity demand data for the Las Vegas area and plan to test for any impact that conversion to desert landscape has on electricity consumption, both on converting and neighboring homes.

When I incorporate an estimate of the value of scarce water, the Cash-for-Grass program yields positive net benefits. But I also find that a modest 6 percent price increase would have achieved similar savings over the life of the program. Increasing prices raise distributional concerns, but subsidies also pose issues of equity. I show that program participants with higher pre-conversion water demand achieve the most savings, and thus targeting these individuals would increase program cost-effectiveness (Allcott, 2011). However, higher water users also tend to earn larger incomes. Public utility managers deciding between prices and subsidies may therefore face a trade-off between regressive price policies and subsidizing wealthy individuals.

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Figures

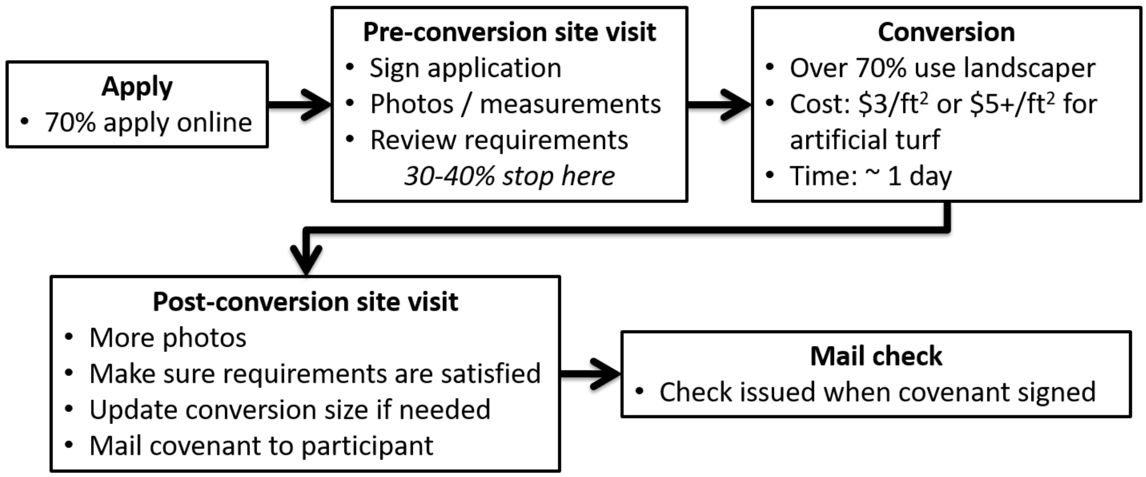


Figure 1: Illustration of the process required for a single-family customer to become enrolled in the Cash-for-Grass program.

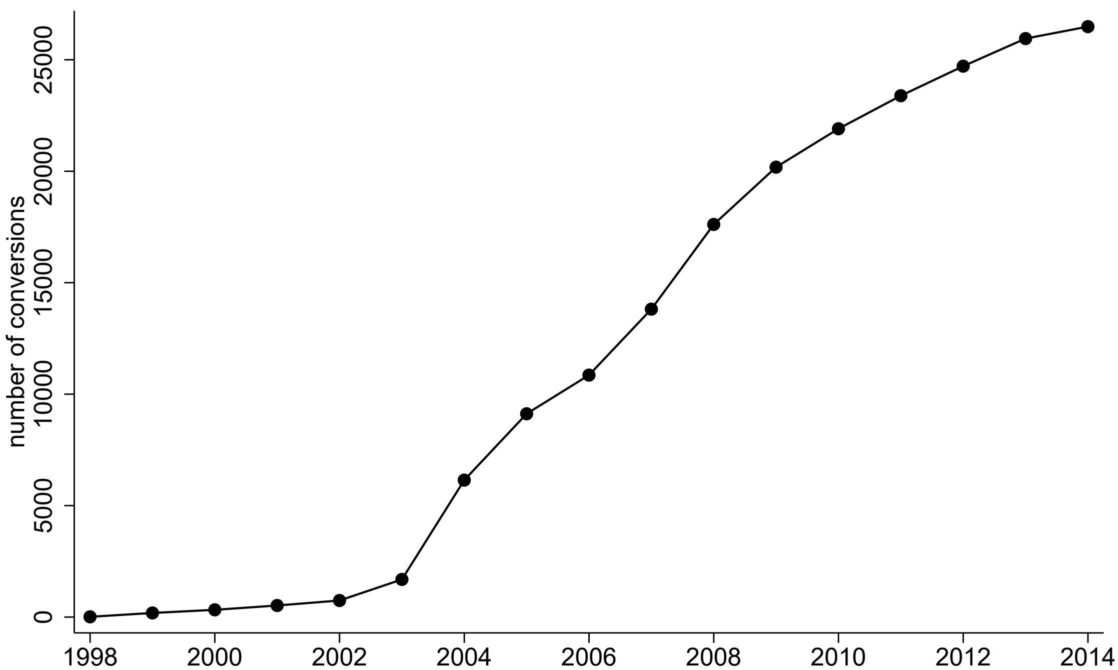


Figure 2: Cumulative number of conversions over time performed under the Cash-for-Grass program (single-family parcels that converted only once).

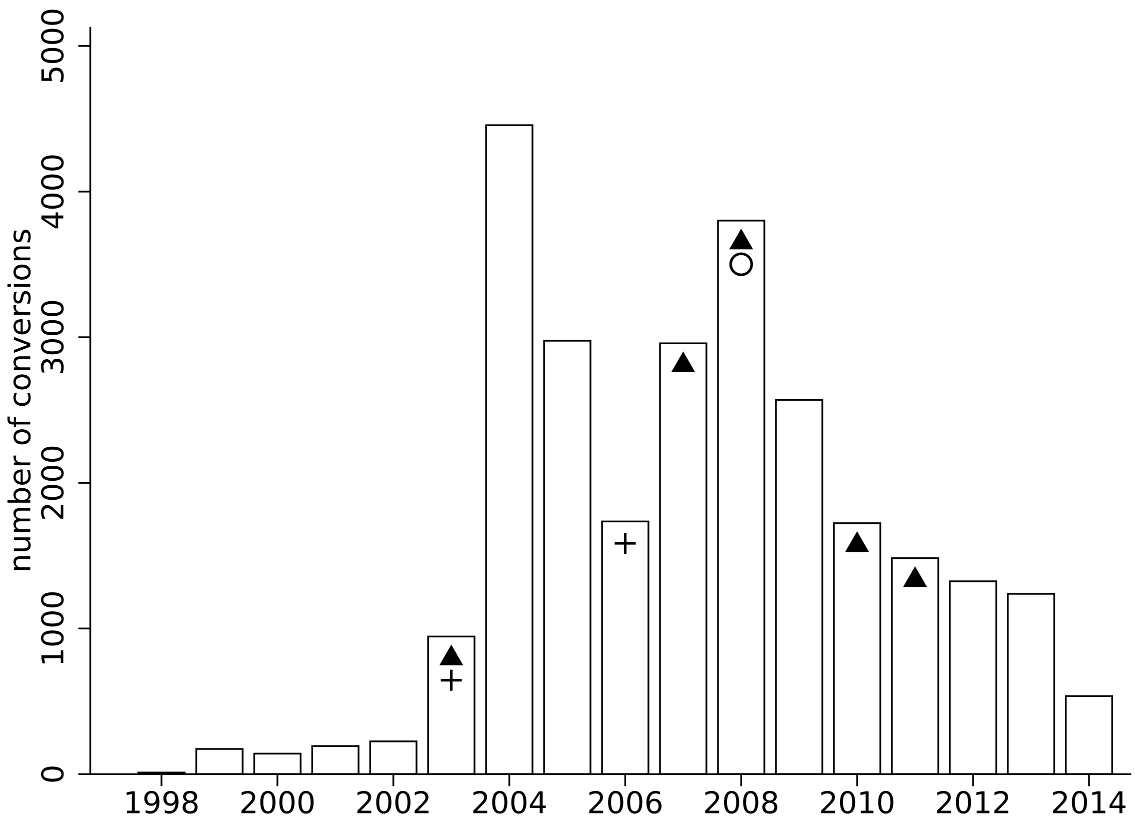


Figure 3: Number of conversions by year performed under the Cash-for-Grass program (single-family parcels that converted only once). 'Plus' signs indicate increases in the subsidy level; the hollow circle indicates a decrease in the subsidy level; solid triangles indicate dates of major increases to a customer's average water bill. 2014 participation data only run through June 12.

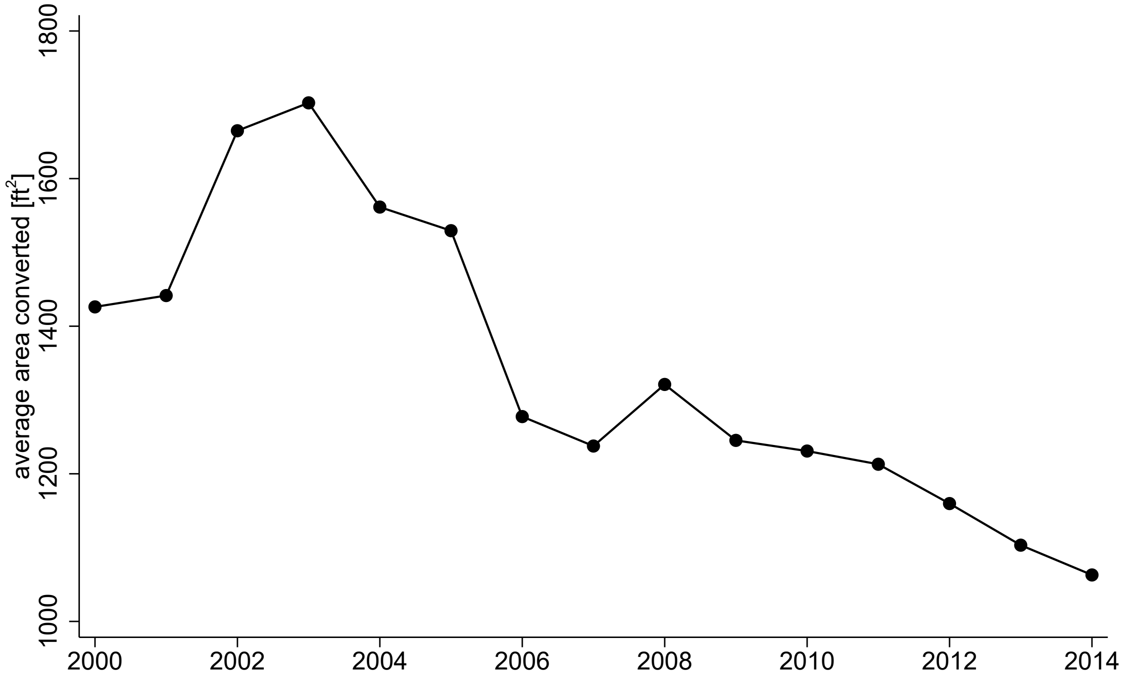


Figure 4: Average size of conversions in each year of the Cash-for-Grass program. No averages are shown before 2000 since only one conversion has a recorded area in 1998 and none of the conversions undertaken in 1999 record the area converted (averages derived from single-family parcels that converted only once).

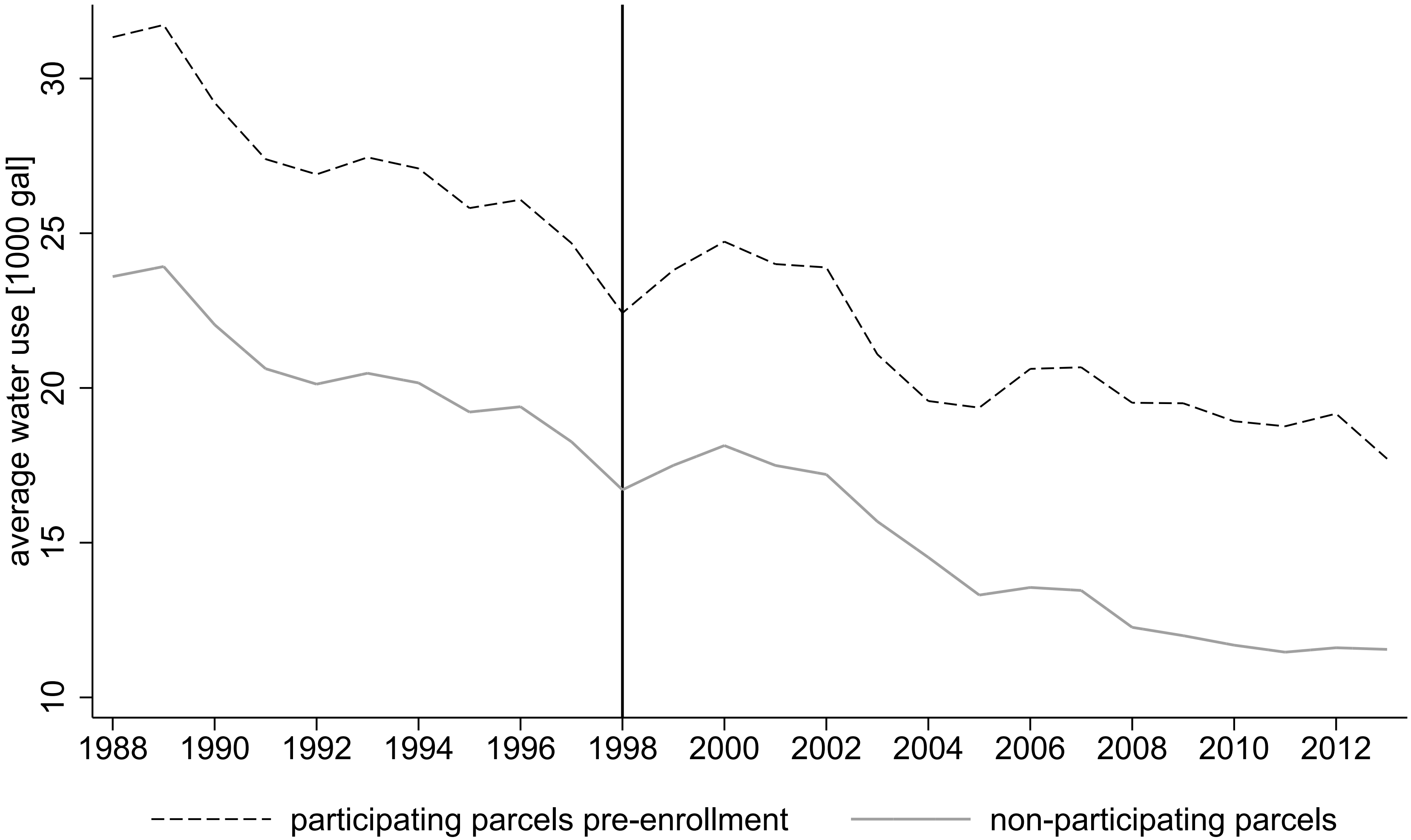


Figure 5: Average annual water use from 1988 through 2013 (the last full year in my panel) for participating parcels prior to Cash-for-Grass program enrollment (dashed black), and non-participating parcels (solid gray). Participating parcels drawn from program participants classified as single-family households who undertook one conversion. Sample sizes change over time as new homes add to the service population, or as pre-enrollment participating parcels enroll (occasionally homes exit the service population as well).

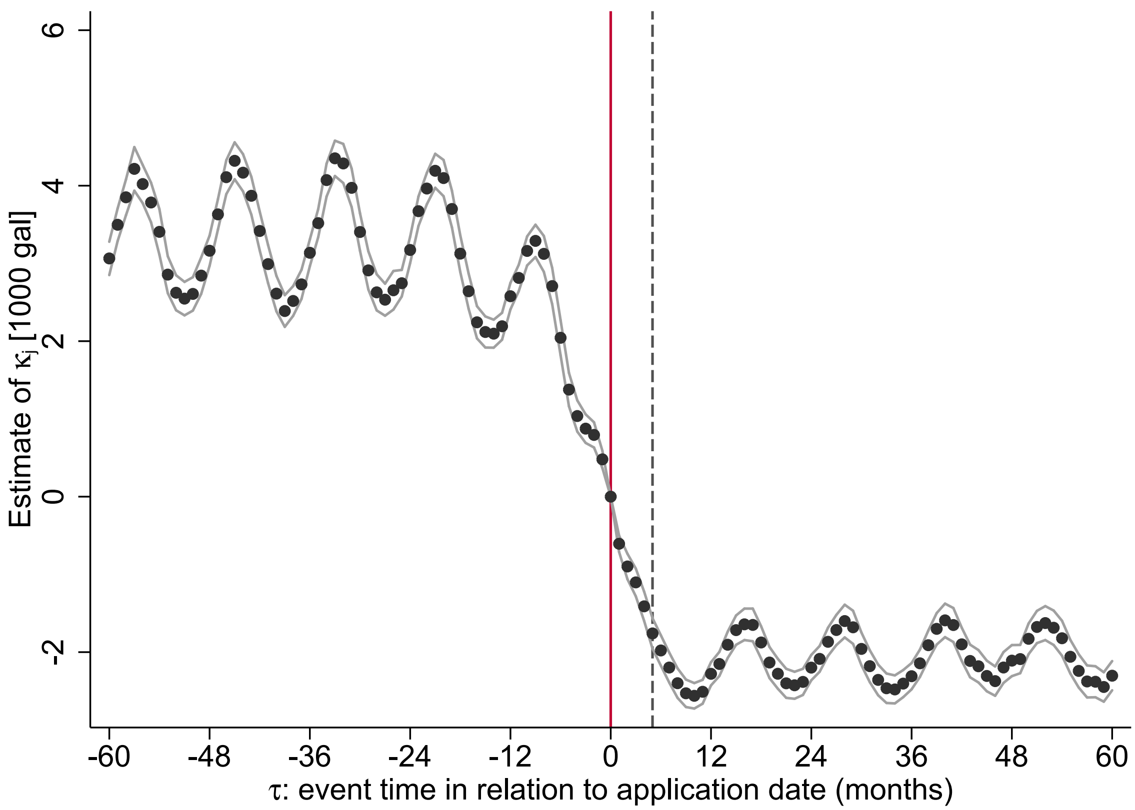


Figure 6: Event study, illustrating water savings from the Cash-for-Grass program. Point estimates and 95 percent confidence intervals of κ_j's are derived from estimating Eq. (1). Standard errors are clustered at the parcel level. The omitted category is $\kappa_0 = 0$. Observations are limited to single-family participating parcels that converted only once and all non-participants. Participating parcel observations are further restricted to a five-year window around the month of application; that is $-60 \leq \kappa \leq 60$. The vertical solid line indicates the month of application. The vertical dashed line indicates the average month of enrollment, five months after the application date.

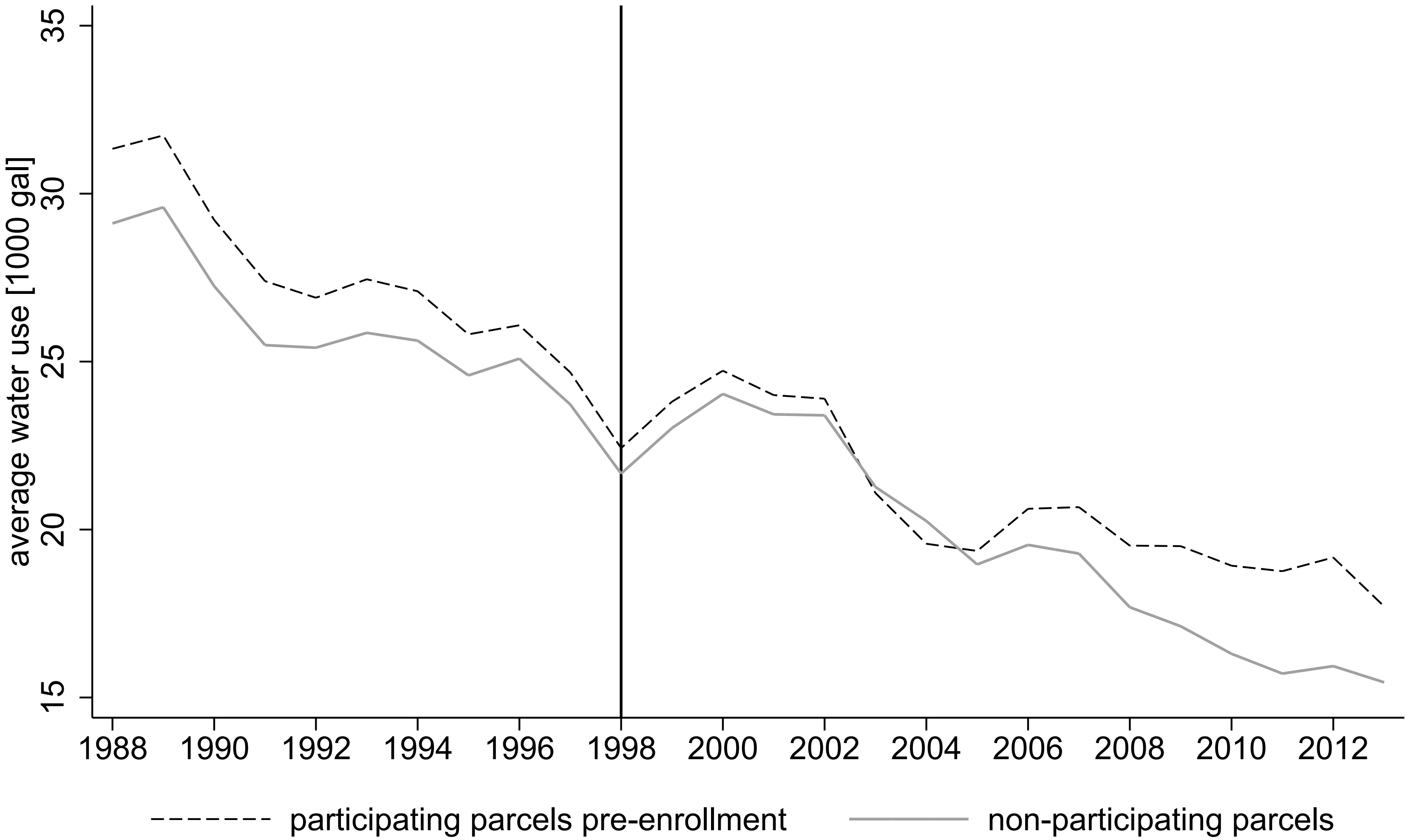


Figure 7: Average annual water use from 1988 through 2013 (the last full year in my panel) for participating parcels prior to Cash-for-Grass program enrollment (dashed black), and non-participating parcels that applied for the program, but never become enrolled (solid gray), i.e. the DNF control sample. Participating parcels drawn from program participants classified as single-family households who undertook one conversion. Sample sizes change over time as new homes add to the service population, or as pre-enrollment participating parcels enroll (occasionally homes exit the service population as well).

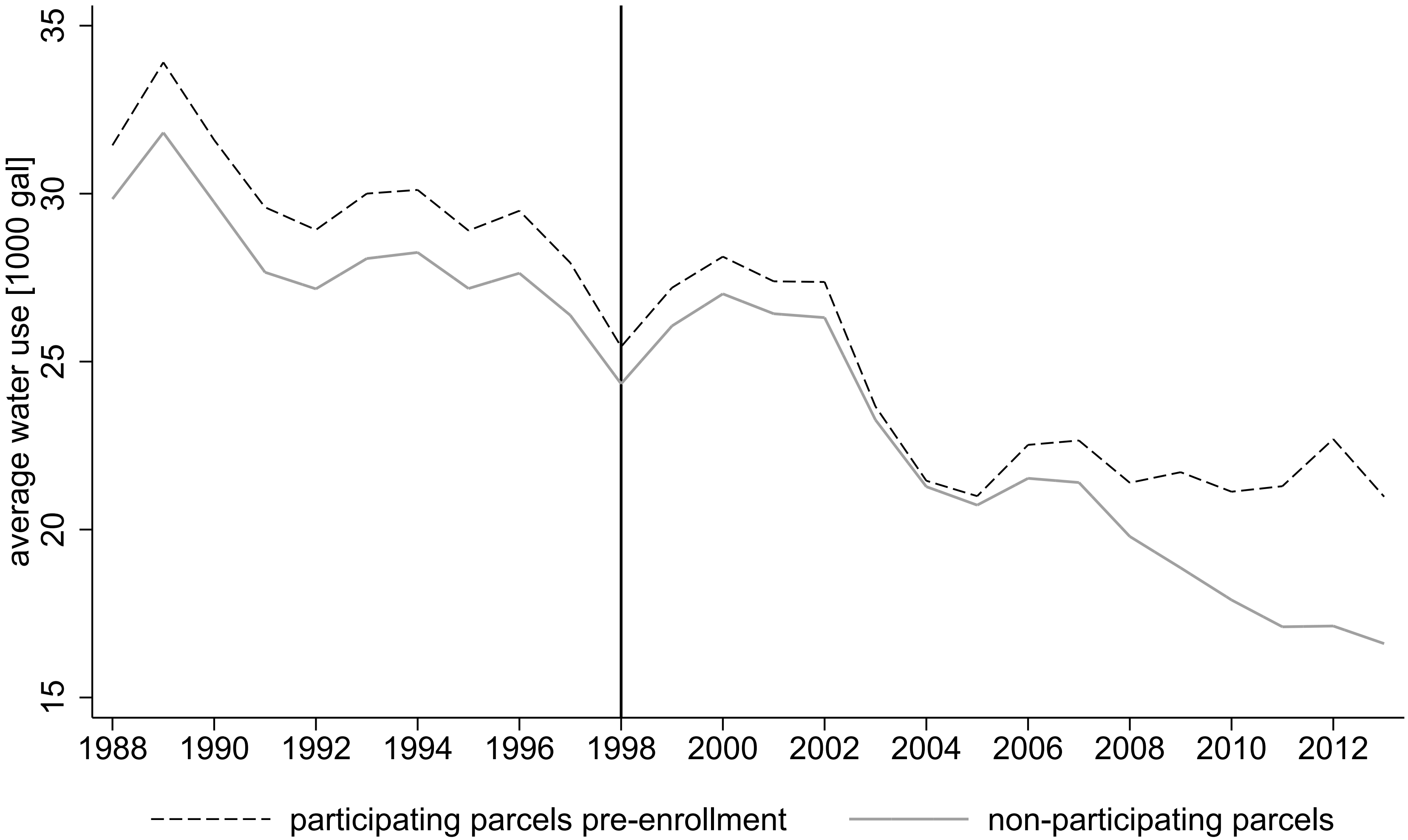


Figure 8: Average annual water use from 1988 through 2013 (the last full year in my panel) for participating parcels prior to Cash-for-Grass program enrollment (dashed black), and non-participating parcels that match with participating parcels on lot size and pre-enrollment July water use (solid gray), i.e. the matched control sample. Participating parcels drawn from program participants classified as single-family households who undertook one conversion. Sample sizes change over time as new homes add to the service population, or as pre-enrollment participating parcels enroll (occasionally homes exit the service population as well).

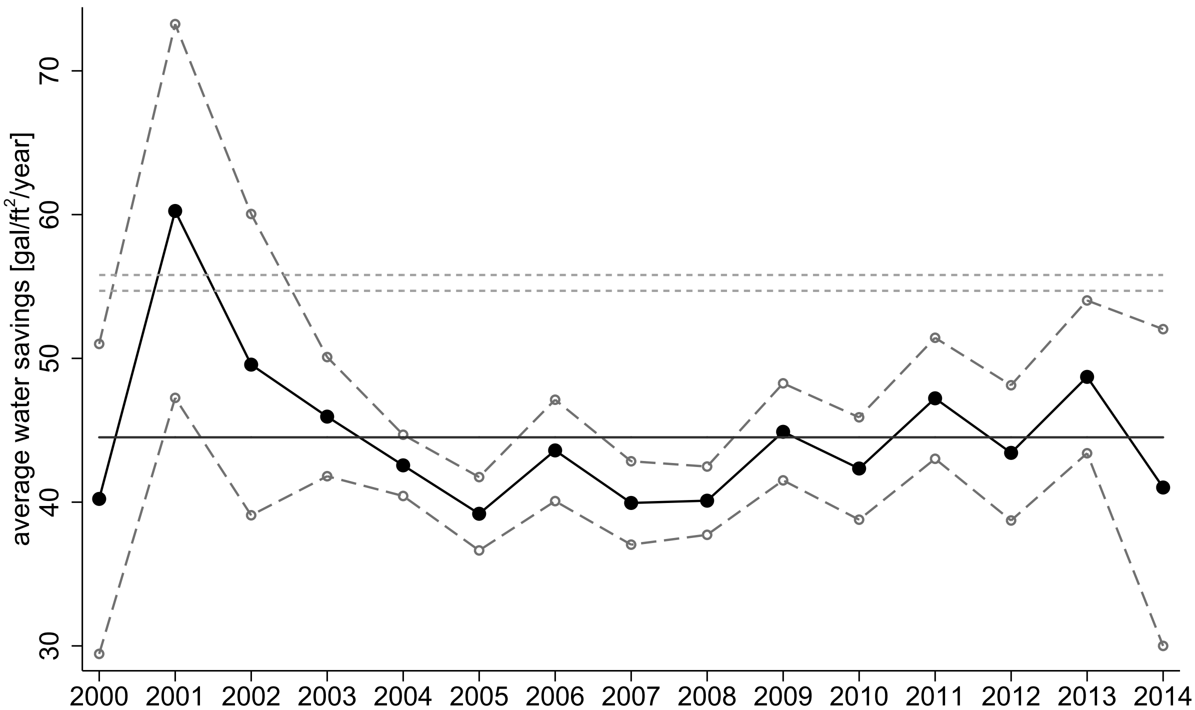


Figure 9: Average water savings achieved by participants in each year of the program (derived from Eq. (4)) and normalized by the corresponding average annual conversion area (see Figure 4). 95 percent confidence intervals are derived considering average converted area a fixed parameter. Horizontal gray dotted lines show the water authority's two point estimates of normalized savings: 55.8 and 54.7 gal/ft^2/year. The horizontal solid line represents my estimate of 5,000 gal/month normalized by the overall average converted area, 1,348 ft^2, and then scaled up to gal/ft^2/year.

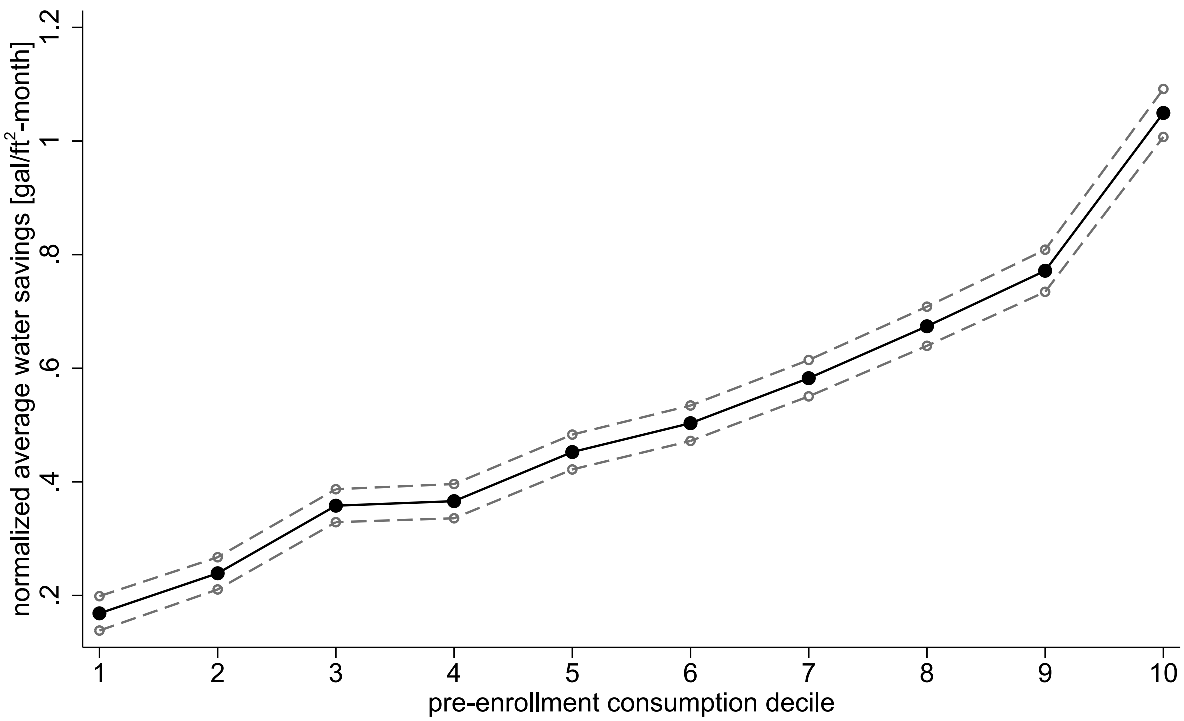


Figure 10: Average water savings per square foot of lot size achieved within each pre-enrollment water consumption decile. Point estimates and 95 percent confidence intervals are derived from a model based on Eq. (4) with the dependent variable, Q_{it}, normalized by 1000 ft^2 of lot size. Pre-enrollment consumption deciles are defined based on a 12-month average of water use for participating parcels (normalized by lot size) beginning 24 months prior to the month of enrollment.

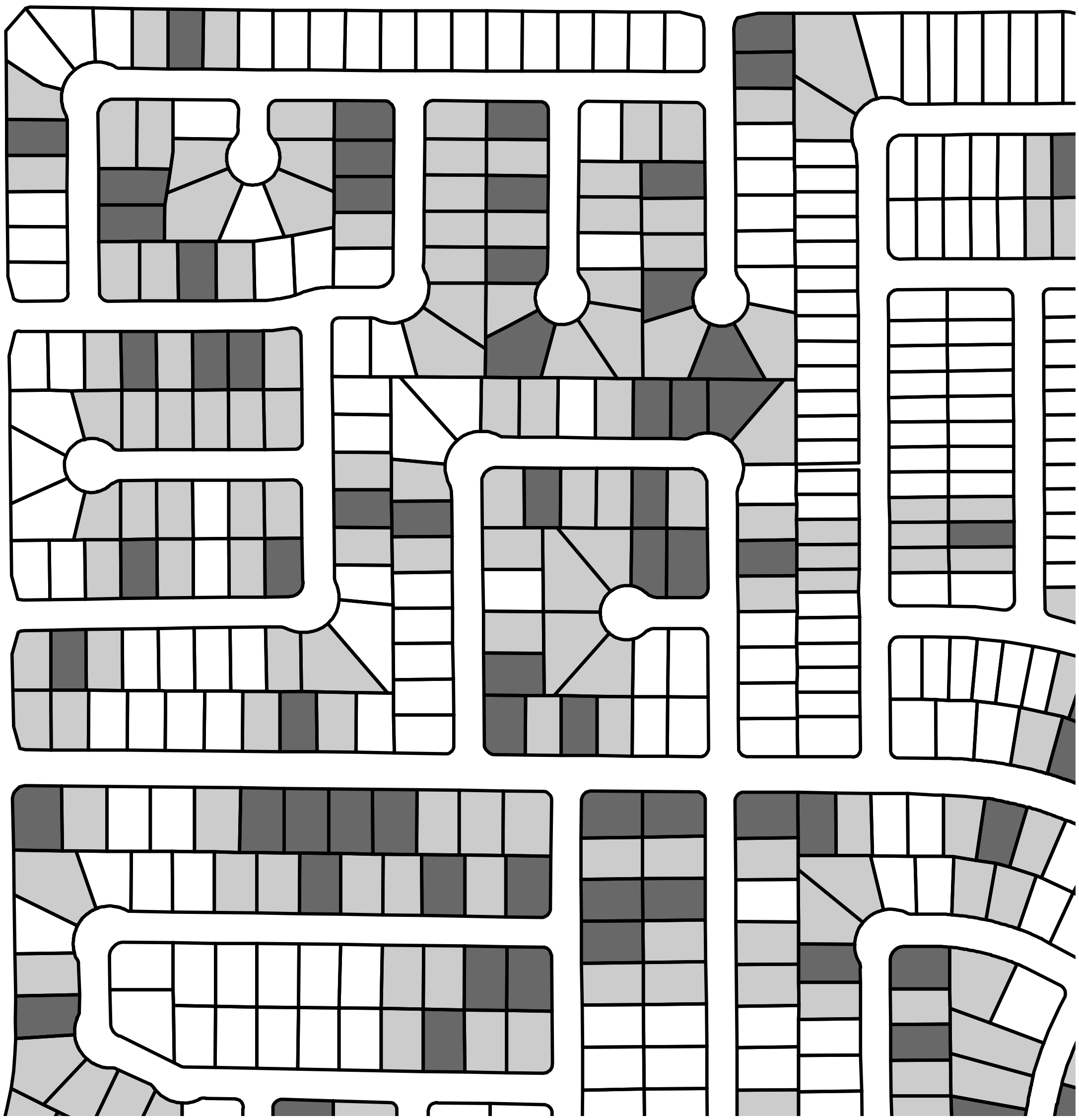


Figure 11: Illustration of neighboring parcels. Dark shaded parcels eventually participate in the Cash-for-Grass program. Lightly shaded parcels neighbor participating parcels. Empty parcels are neither participating parcels nor neighbors, and thus make up my control sample. Note that neighboring participating parcels are neighbors of the other, participating parcel.

Tables

Table 1: Summary of water consumption panel.

	All parcels	Participating parcels	Non-participating parcels
Monthly water use obs. (N)	64,135,652	6,580,788	57,554,864
Number of parcels	309,608	26,488	283,120
Mean water use (kgal/mo)	15.7	23.8†	15.1

† Derived from pre-enrollment water consumption observations.

Table 2: Water savings from converting to desert landscape (results from main specification).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
post-enroll	-4.92	-4.97	-5.01	-4.90	-5.19	-5.16	-5.17
	(0.06)***	(0.07)***	(0.07)***	(0.12)***	(0.14)***	(0.06)***	(0.06)***
post-enroll$\times T_{it}$						0.005	0.006
						(0.001)***	(0.002)***
post-enroll$\times T_{it}^2$							-5.4e-06
							(1.7e-05)
Sample	full	1yr window	2yr window	balanced	no zero	full	full
Fixed-effects							
μ_{im}	yes	yes	yes	yes	yes	yes	yes
δ_t	-	-	-	yes	-	-	-
δ_{tc}	yes	yes	yes	-	yes	yes	yes
adj. R^2	0.30	0.30	0.30	0.53	0.71	0.30	0.30
Parcels	309,201	309,198	309,193	81,476	47,771	309,201	309,201
Observations	64,120,344	63,472,767	62,867,660	25,746,416	13,775,144	64,120,344	64,120,344

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Parcel clustered standard errors (reported in parentheses).

μ_{im} parcel by month-of-calendar year fixed-effects.

δ_t, δ_{tc} month-of-sample and month-of-sample by cohort fixed-effects.

Cohorts based on parcel's first year in sample: 88-89, 90-94, 95-99, 00-04, 05-14.

Xyr window: drop participant observations within X years around enrollment.

balanced: includes parcels with non-missing water consumption in each month from Jan. 1988 to Apr. 2014.

no zero: includes parcels with all positive consumption only.

Table 3: Water savings from converting to desert landscape (results from alternative control samples).

	(1)	(2)	(3)	(4)
post-enroll	-4.92	-5.22	-4.15	-4.11
	(0.06)***	(0.08)***	(0.09)***	(0.14)***
Sample	full	participants	DNF	match
Fixed-effects				
μ_{im}	yes	yes	yes	yes
δ_t	-	-	-	yes
δ_{tc}	yes	yes	yes	-
adj. R^2	0.30	0.64	0.33	0.70
Parcels	309,201	26,414	32,368	16,774
Observations	64,120,344	6,579,892	8,022,520	5,568,552

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Parcel clustered standard errors (reported in parentheses).

μ_{im} parcel by month-of-calendar year fixed-effects.

δ_t, δ_{tc} month-of-sample and month-of-sample by cohort fixed-effects.

Cohorts based on parcel's first year in sample: 88-89, 90-94, 95-99, 00-04, 05-14.

participants: includes only rebate program participants.

DNF: uses a control sample of non-participants that apply, but do not finish.

match: uses a control sample of matched non-participating parcels.

Table 4: Summary statistics for hedonic panel: sale years 1996 - 2014

(a) **All sales**		*Participant*			*Neighbor*		
	Full	$P = 0$	$P = 1$	p-val	$N = 0$	$N = 1$	p-val
sale price	309,077	309,823	268,839	0.00	311,356	274,165	0.00
age	6.3	6.1	17.3	0.00	5.6	16.6	0.00
lot sqft	5,755	5,725	7,361	0.00	5,662	7,181	0.00
home sqft	2,042	2,042	2,017	0.04	2,042	2,035	0.32
bedrooms	3.39	3.39	3.43	0.00	3.39	3.45	0.00
full bath	2.26	2.26	2.26	0.65	2.26	2.27	0.20
half bath	0.53	0.53	0.36	0.00	0.54	0.37	0.00
pool	0.19	0.19	0.33	0.00	0.19	0.32	0.00
garage sqft	465	464	505	0.00	463	498	0.00
Observations	199,037	195,410	3,627		186,840	12,197	

(b) **Repeat sales**		*Participant*			*Neighbor*		
	Full	$P = 0$	$P = 1$	p-val	$N = 0$	$N = 1$	p-val
sale price	295,867	297,054	239,803	0.00	299,783	241,908	0.00
age	5.9	5.7	15.1	0.00	5.3	14.5	0.00
lot sqft	5,072	5,037	6,743	0.00	4,970	6,484	0.00
home sqft	2,047	2,047	2,055	0.75	2,047	2,057	0.44
bedrooms	3.36	3.36	3.46	0.00	3.35	3.45	0.00
full bath	2.26	2.25	2.30	0.02	2.25	2.27	0.06
half bath	0.63	0.63	0.42	0.00	0.64	0.44	0.00
pool	0.16	0.16	0.33	0.00	0.16	0.29	0.00
garage sqft	452	450	500	0.00	449	487	0.00
Observations	40,755	39,910	845		37,998	2,757	

Summary statistics in panel (a) reflect the sample used in estimating Eq. (6). Panel (b) reflect summary statistics from estimating Eq. (6), additionally including parcel fixed-effects (and dropping time-invariant property specific controls). Prices adjusted to 2014 dollars and further restricted to the 1^{st} and 99^{th} percentiles of the sale price distribution. I further drop parcels meeting criteria for undertaking additions.

Table 5: Summary statistics for hedonic panel: sale years 1996 - 2006.

All sales		*Participant*			*Neighbor*		
	Full	$P = 0$	$P = 1$	p-val	$N = 0$	$N = 1$	p-val
sale price	344,873	344,449	399,708	0.00	343,415	393,853	0.00
age	5.3	5.2	13.8	0.00	5.0	13.9	0.00
lot sqft	5,959	5,949	7,219	0.00	5,920	7,275	0.00
home sqft	2,024	2,025	1,910	0.00	2,027	1,939	0.00
bedrooms	3.40	3.40	3.37	0.22	3.40	3.41	0.86
full bath	2.26	2.26	2.20	0.00	2.26	2.21	0.00
half bath	0.48	0.48	0.34	0.00	0.49	0.34	0.00
pool	0.21	0.21	0.30	0.00	0.21	0.31	0.00
garage sqft	467	467	491	0.00	466	487	0.00
Observations	124,742	123,785	957		121,136	3,606	

Summary statistics reflect the sample used in estimating Eq. (6). Insufficient observations preclude estimating a model with parcel fixed-effects. Prices adjusted to 2014 dollars and further restricted to the 1^{st} and 99^{th} percentiles of the sale price distribution. I further drop parcels meeting criteria for undertaking additions.

Table 6: Regression results for the effect of conversion to desert landscape on home property values.

	(1)	(2)	(3)	(4)	(5)
DP (ever converts)	0.0048	0.0040		0.0027	
	(0.0013)***	(0.0013)***		(0.0019)	
Direct effect	0.012	0.018	0.021	0.015	0.014
	(0.0034)***	(0.0040)***	(0.012)*	(0.0044)***	(0.025)
DN (neighbors DP)	0.0016	0.0019		0.0012	
	(0.0010)	(0.0010)*		(0.0022)	
Spillover effect	-0.0018	0.00040	-0.011	-0.0018	-0.0040
	(0.0022)	(0.0026)	(0.0072)	(0.0031)	(0.026)
age (years)	-0.0090	-0.0042		-0.0069	
	(0.00070)***	(0.00055)***		(0.00093)***	
parcel sqft	1.7e-05	1.5e-05		1.4e-05	
	(7.6e-07)***	(6.9e-07)***		(8.3e-07)***	
house sqft	2.5e-04	2.3e-04		2.5e-04	
	(4.0e-06)***	(3.7e-06)***		(4.8e-06)***	
bedrooms	0.0041	0.0054		-0.00072	
	(0.0016)**	(0.0015)***		(0.0020)	
full bath	-0.018	-0.0091		-0.020	
	(0.0028)***	(0.0026)***		(0.0050)***	
half bath	-0.0064	0.0043		0.0037	
	(0.0027)**	(0.0024)*		(0.0036)	
pool sqft	1.1e-04	9.1e-05		1.2e-04	
	(3.2e-06)***	(3.1e-06)***		(5.0e-06)***	
garage sqft	2.0e-04	1.9e-04		1.6e-04	
	(1.3e-05)***	(1.5e-05)***		(1.5e-05)***	
Control group	All	All	All	DNF	DNF
Sale years	1996-2014	1996-2006	1996-2014	1996-2014	1996-2014
Fixed-effects					
quarter-block	yes	yes	yes	yes	yes
parcel	-	-	yes	-	yes
adj. R^2	0.95	0.95	0.93	0.94	0.73
Observations	199,037	124,742	40,755	36,608	2,639

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. 2014 adjusted sale prices trimmed at the 1^{st} and 99^{th} percentiles. Fixed-effects: 2010 U.S. Census Blocks (blocks) by quarter of sample (e.g. 1^{st} quarter of 1997 is quarter 5). Sample excludes parcels undertaking additions (see section 4.2). Block clustered standard errors.

Table 7: Regression results for the effect (in percentage terms) of an extra square-foot of conversion to desert landscape on home property values conditional on the presence of desert landscape.

	(1)	(2)	(3)
DP (ever converts)	0.0049	0.0040	
	(0.0013)***	(0.0013)***	
Direct effect	0.016	0.029	0.018
	(0.0053)***	(0.0074)***	(0.015)
Direct×area effect	-4.3e-06	-8.3e-06	5.2e-06
	(4.1e-06)	(5.3e-06)	(1.7e-05)
DN (neighbors DP)	0.0015	0.0018	
	(0.0010)	(0.0010)*	
Spillover effect	0.0030	0.0071	-0.015
	(0.0031)	(0.0039)*	(0.0093)
Spillover×area effect	-3.7e-06	-4.8e-06	5.6e-06
	(1.9e-06)*	(2.6e-06)*	(8.3e-06)
Sale years	1996-2014	1996-2006	1996-2014
Fixed-effects			
quarter-block	yes	yes	yes
parcel	-	-	yes
adj. R^2	0.95	0.95	0.93
Observations	198,519	124,375	40,685

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Standard errors clustered at the block level.

blocks: 2010 United States Census Block boundaries.

quarter: quarter of sample (e.g. 1^{st} quarter of 1997 is quarter 5).

2014 adjusted sale prices trimmed at the 1^{st} and 99^{th} percentiles.

Sample excludes parcels undertaking additions (see section 4.2).

Appendix A Volumetric and water bill savings calculation details

I calculate baseline water use from the average water use for all participants converting once prior to enrollment, or 23,818 gallons per month. Dividing my savings estimate of 5,000 gal/month by the baseline value yields the 21 percent reduction reported in section 3.

I calculate water bill savings for an average LVVWD customer in 2013 that experiences a constant 5,000 gal/month savings throughout each month of the year. Water charges depend upon the meter size. In 2013, over 99 percent of single-family LVVWD customers in my panel have a 1 inch, 3/4 inch or 5/8 inch meter. Of these customers, 4 percent have a 1 inch meter, 47 percent have a 3/4 inch meter, and the remaining 49 percent have a 5/8 inch meter. Using a bill calculator provided by the water authority, I estimate the annual water bill for each meter size (1”, 3/4” and 5/8”) for average monthly water use in 2013. I also calculate the water bill for each meter size using average water use net of the 5,000 gal/month savings estimate. Finally, I calculate a weighted average water bill, with weights defined by the share of customers associated with each meter size.

The average customer in 2013 pays a $501.03 water bill. Saving 5,000 gal/month reduces the water bill by $150.44 (about 30 percent) to $350.59. The present discounted value of an infinite stream of these savings equals $3,159 assuming a 5 percent discount rate.

I likely overstate water bill savings. My water use data demonstrate a high degree of variability in demand over the calendar year, and to the extent that most of the increase in summer water use arises from outdoor landscape irrigation, applying average savings evenly throughout the year will likely understate water savings in the summer and overstate water savings in the winter.[73] Since LVVWD customers experience block pricing, I likely underestimate summer bill savings more than I overestimate winter bill savings, producing a net underestimate in the annual water bill. The water bill savings should be interpreted with this caveat in mind, however I do not believe this bias to grossly distort the predicted water bill savings.

[73] Sovocool *et al.* (2006) find that much of the savings due to conversion to desert landscape comes from savings in the summer.

Appendix B Additional water savings results

B.1 Fixed-effects, early exits, and program changes

In this section I present savings estimates that explore the impact of various fixed-effects, parcels that exit before the end of the sample, and two program policy changes.

In July, 2000, the water authority began rebating participants based on the size of their conversion; prior to this the water authority determined rebates based on how much water the participant saved relative to participant specific past average monthly water use. In March, 2004, the water authority relaxed a 400 square foot minimum conversion requirement, allowing conversions less than 400 square feet provided the conversion comprised an entire front or back yard.[74] To study the impact of these program changes, I estimate Eq. (8) and Eq. (9), where I interact my post-enrollment indicator with indicators for enrollment dates corresponding to dates on or after the change in determining the rebate amount, 'sqft', and relaxing the 400 ft^2 minimum, 'min conv'.

$$Q_{it} = \alpha[\text{pre-period}]_{it} + \beta_1[\text{post-enroll}]_{it} + \beta_2[\text{post-enroll}]_{it} \times [\text{sqft}]_i + \mu_{im} + \delta_{tc} + \epsilon_{it} \tag{8}$$

$$Q_{it} = \alpha[\text{pre-period}]_{it} + \beta_1[\text{post-enroll}]_{it} + \beta_2[\text{post-enroll}]_{it} \times [\text{min conv}]_i + \mu_{im} + \delta_{tc} + \epsilon_{it} \tag{9}$$

Table 8 displays results. Compared to the results in column 1, including either month-of-sample by cohort fixed-effects (column 2) or parcel by month-of-calendar year fixed-effects (column 3) reduces the estimate of savings compared to the specification that includes only parcel and month-of-sample fixed-effects (column 1). These results suggest that variation in seasonal fluctuations as well as time-varying characteristics among different aged houses affect results. I therefore control for both in my main specification (column 4).[75] In column 5 I eliminate from my main specification any parcels that drop out before the end of the sample (April, 2014). Dropping early exits has little impact on savings. In column 6 and 7 I explore the impact of the two program administrative changes discussed above. The negative point estimate associated with the effect of the subsidy

[74]This restriction appears to have constrained participants. Histograms of converted areas for single-family participants (not reported here) show a sharp cutoff at 400 square feet for conversions taking place prior to March 2004. After March 2004, a similar histogram shows a more continuous distribution around 400 square feet.

[75]In addition, results from a falsification test show that a proxy policy defined as a proxy enrollment 5 years prior to actual enrollment has no effect only for the fixed-effect specifications represented in columns 2 and 4. This further validates the importance of controlling for cohort effects.

Table 8: Additional water savings results: effect of fixed-effects, early exits, and administrative changes.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
post-enroll	-5.16	-5.00	-5.07	-4.92	-4.93	-4.91	-6.32
	(0.06)***	(0.06)***	(0.06)***	(0.06)***	(0.06)***	(0.61)***	(0.23)***
post-enroll×sqft						-0.02	
						(0.61)	
post-enroll×min conv							1.54
							(0.24)***
Sample	full	full	full	full	no early exits	full	full
Fixed-effects							
μ_i	yes	yes	-	-	-	-	-
μ_{im}	-	-	yes	yes	yes	yes	yes
δ_t	yes	-	yes	-	-	-	-
δ_{tc}	-	yes	-	yes	yes	yes	yes
adj. R^2	0.25	0.26	0.30	0.30	0.30	0.30	0.30
Parcels	309,608	309,608	309,201	309,201	305,068	309,201	309,201
Observations	64,135,652	64,135,652	64,120,344	64,120,344	63,356,472	64,120,344	64,120,344

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Parcel clustered standard errors (reported in parentheses).

μ_i, μ_{im} parcel and parcel by month-of-calendar year fixed-effects.

δ_t, δ_{tc} month-of-sample and month-of-sample by cohort fixed-effects.

Cohorts based on parcel's first year in sample: 88-89, 90-94, 95-99, 00-04, 05-14.

Early exit parcels drop out of sample prior to sample's end.

sqft: indicator for enrollment after June 2000.

min conv: indicator for enrollment after Feb. 2004.

remuneration method (column 6, row 2) suggests that rebating customers based on the size of their conversion may have increased savings, however I cannot statistically distinguish the effect from zero. The rebate remuneration method therefore likely had little effect.[76] Allowing conversions under 400 square feet, however, appears to have decreased savings by about 1,500 gallons per month per average conversion (column 7, row 3). As more participants convert areas less than 400 square feet, one expects average savings per conversion to fall, since the average conversion area falls. In terms of savings per square foot of converted area, 6,300 gallons per month corresponds to about 46 gal/ft^2/year (the mean conversion area for conversions prior to March, 2004 equals 1,653 square feet). After relaxing the minimum conversion requirement, savings become 4,800 gallons per month ($-6.320 + 1.54 = -4.78$) which corresponds to 43 gal/ft^2/year (the mean conversion area throughout the life of the program equals 1,348 ft^2), however, this value falls within the 95 percent confidence interval of the pre-March, 2004 estimate. Relaxing the minimum conversion requirement does not appear to have affected savings efficiency.

B.2 Additional event studies

Figure 12 illustrates results of an event study derived from Eq. (1), but defining τ with respect to the enrollment date rather than the application date. The solid vertical line indicates the date of enrollment, while the dashed vertical line indicates the average month of application, 5 months prior to the date of enrollment. These results exhibit less oscillations prior to the enrollment date compared to Figure 6, however overall Figure 6 and Figure 12 lead to the same conclusion; there appears to be an absence of pre-trends prior to the application-enrollment period, savings remain relatively stable after enrollment, and there exists some transient behavior at least one year prior to the enrollment date.[77]

I also implement an alternative event study using my matched sample. In particular, I calculate the difference in water use in each month-of-sample between individual participating parcels and their respective matched non-participating pairs. Then for each participating parcel, I associate

[76] I am unable to determine the impact on savings per square foot of converted area since the water authority did not record converted area for conversions prior to 2000.

[77] Though not shown, event studies derived from the three alternative control samples (participants only, DNF, and matched control samples) display largely similar patterns as in Figure 6 and Figure 12, however they show greater instability prior to conversion and exhibit strikingly more pronounced erosion in savings after enrollment. Also, the event studies derived from the DNF and matched control samples illustrate savings closer to 4,000 gal/month, consistent with the estimates shown in Table 3.

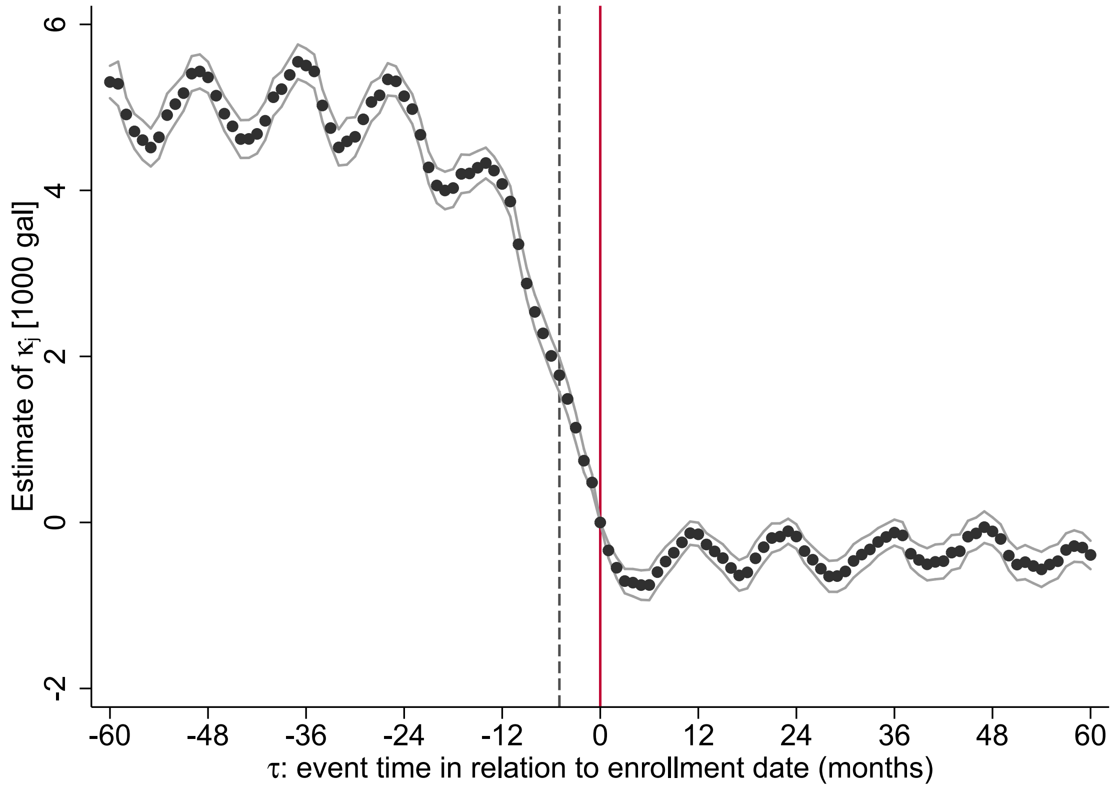
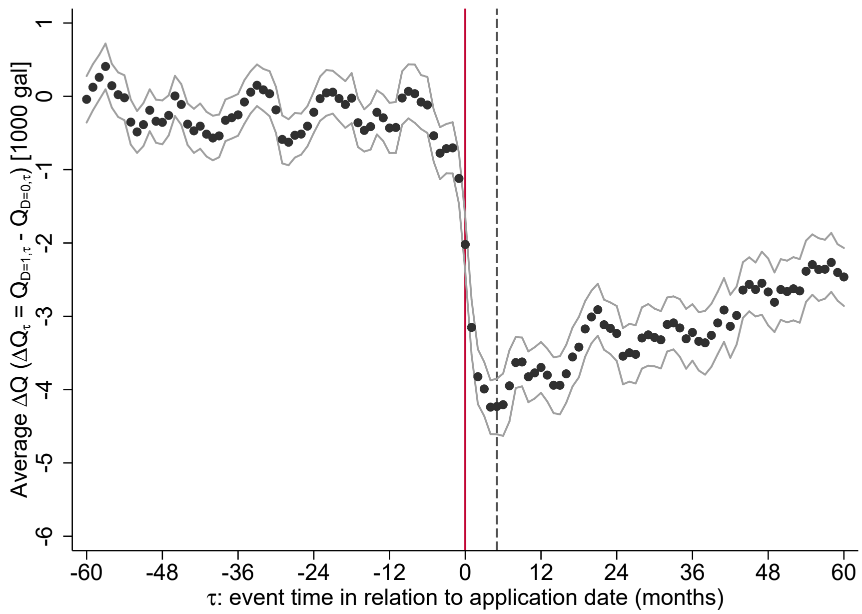


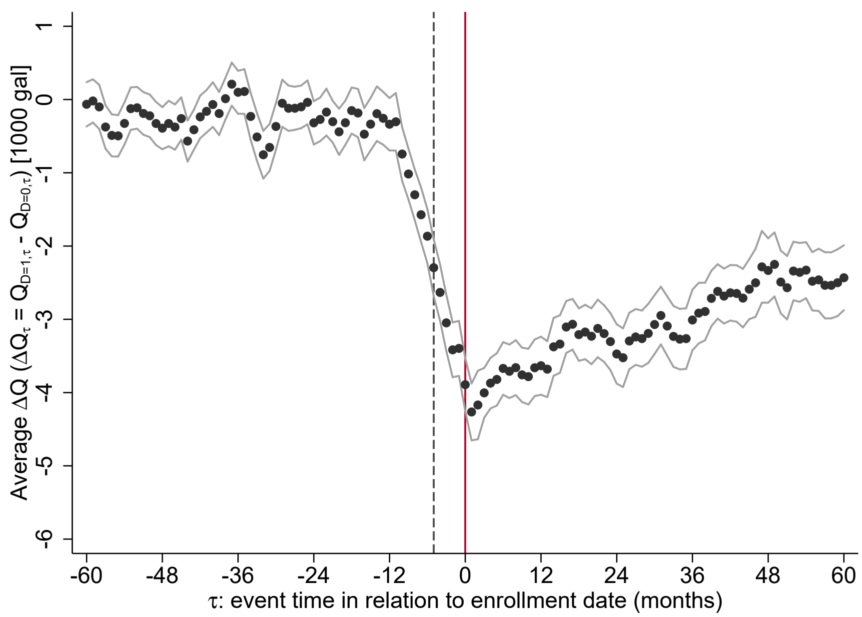
Figure 12: Event study, illustrating water savings from the Cash-for-Grass program. Point estimates and 95 percent confidence intervals of κ_j's are derived from estimating Eq. (1), with event time defined with respect to the month of enrollment. Standard errors are clustered at the parcel level. The omitted category is $\kappa_0 = 0$. Observations are limited to single-family participating parcels that converted only once and all non-participants. Participating parcel observations are further restricted to a five-year window around the month of enrollment; that is $-60 \leq \kappa \leq 60$. The vertical solid line indicates the month of enrollment. The vertical dashed line indicates the average month of application, five months prior to the enrollment date.

the month-of-sample with event time. Finally, I calculate the average of the pairwise differences in water use in each event month.[78] Figure 13 illustrates results of this procedure, defining event time with respect to application (panel a) and enrollment (panel b). Both figures illustrate an absence of pre-trends prior to the application-enrollment period, and some transient behavior at least one year prior to the enrollment date. In contrast to Figure 6 and Figure 12, however, Figure 13 illustrates a more pronounced erosion in savings after conversion.

[78]In particular, I regress my 'difference' variable on a constant, and cluster standard errors at the parcel level.



(a) Event study w.r.t. application date



(b) Event study w.r.t. enrollment date

Figure 13: Event study based on the average difference in water use between treated and matched pairs, defining event time with respect to application date (a) and enrollment date (b). The vertical solid line indicates the month of application (a) or enrollment (b). The vertical dashed line indicates the average month of enrollment (a) or application (b).

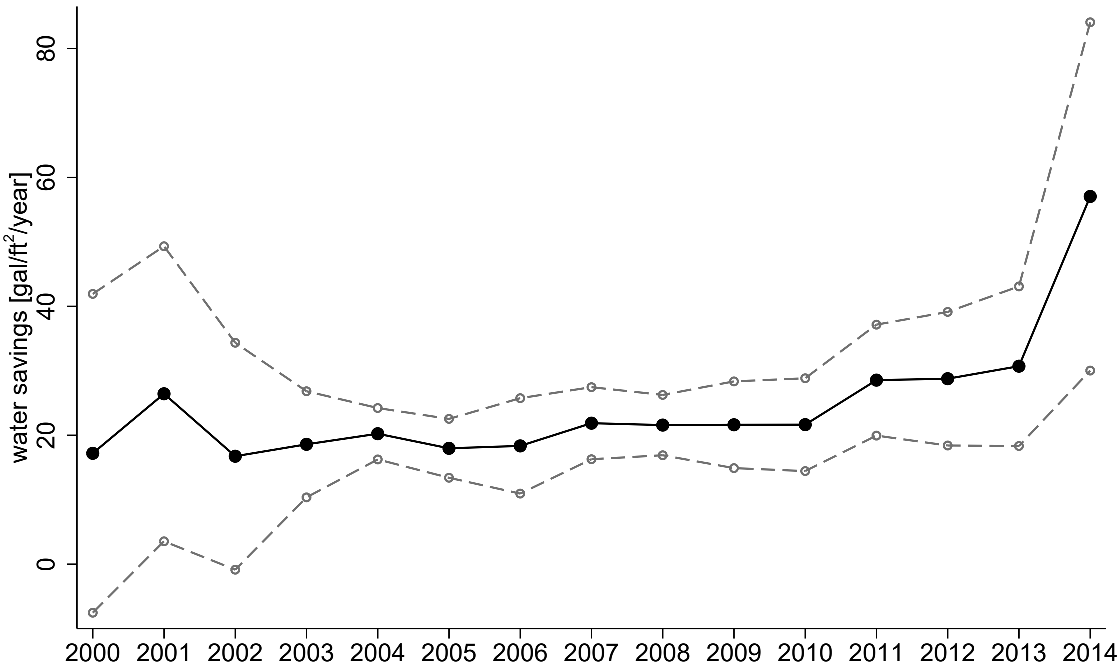


Figure 14: Average water savings achieved in each year of the program derived from my main specification model, Eq. (2), run separately for each enrollment year. In each year, my sample is comprised from pooling all participating parcels in that year and their respective matched non-participating parcels. Results are further normalized by the corresponding average annual conversion area. I derive 95 percent confidence intervals considering average converted area a fixed parameter.

B.3 Additional discussion regarding impact of time and pre-enrollment consumption characteristics

Because estimating Eq. (4) essentially compares participants in a given year with participants in all years, one may be concerned that higher water demand early in the program biases estimates of later year annual program savings. To test robustness of my results to this concern, I estimate Eq. (2) separately for each enrollment year with a pooled sample of parcels enrolling in that year and their matched non-participating parcel pairs (with any duplicate non-participating parcels weighted accordingly by frequency). As in my main results, Figure 14 shows that savings across years exhibit a 'U'-shaped pattern over time, though less pronounced.[79]

One may be similarly concerned that in deriving savings as a function of pre-enrollment con-

[79]Of course, because of the wide confidence intervals, I cannot rule out the possibility of initially low savings, producing instead of a 'U'-shaped pattern a steady increase in savings across years.

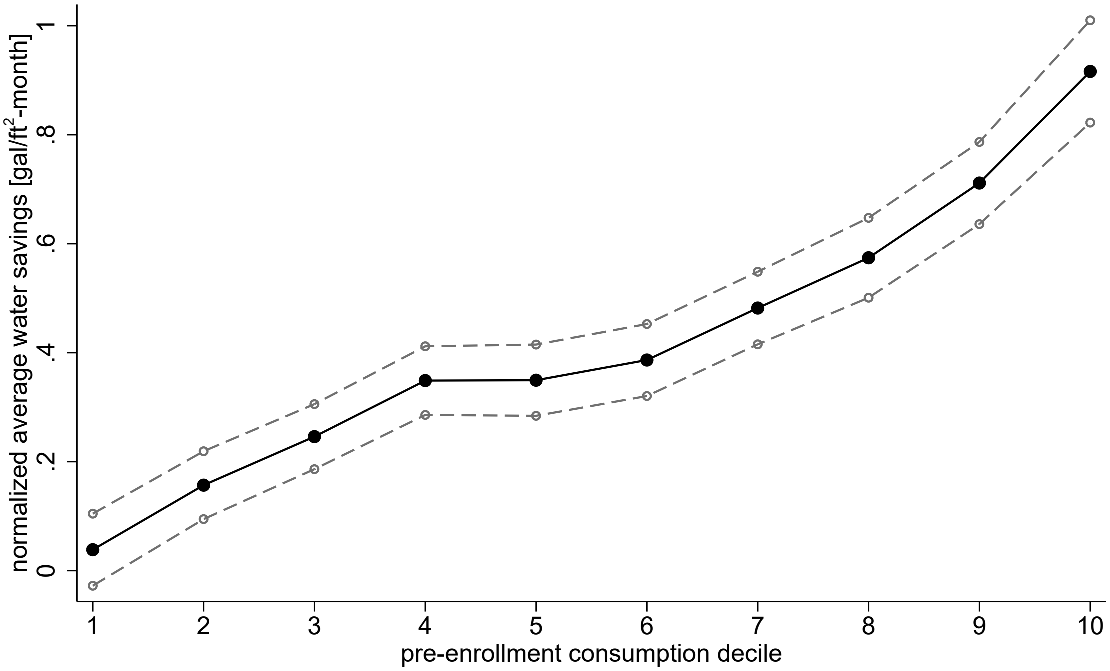


Figure 15: Average water savings per square foot of lot size achieved within each pre-enrollment water consumption decile. I derive point estimates and 95 percent confidence intervals by pooling the participating parcels with their matched non-participating parcels, and then estimating Eq. (2) separately for each decile. I normalize the dependent variable, Q_{it}, by 1000 ft^2 of lot size. Pre-enrollment consumption deciles are defined based on a 12-month average of water use per lot size beginning 24 months prior to the month of enrollment.

sumption decile (Figure 10), comparing high (or low) decile converters with everyone else may bias results. Ideally, one would want to compare high decile converters with comparable non-participants. In Figure 15, I implement such a procedure. I first derive pre-enrollment consumption deciles (based on a 12-month pre-enrollment average of water use normalized by lot size), and then for each decile, pool the participating parcels with their matched non-participating parcels. With this pooled sample, I run Eq. (2) separately for each decile. Like my main results, Figure 15 illustrates a positive relationship between savings and pre-enrollment consumption decile.[80]

[80]This result is robust to whether I define pre-enrollment consumption deciles with a 12, 24, 36, or 48-month average, and whether I estimate Eq. (2) with water use normalized by lot size, or simply water use, and then post-estimation normalizing by average conversion area for each given pre-enrollment consumption decile.

Appendix C Additional hedonic results and robustness checks

C.1 Effect of two policy changes

In 2004, Las Vegas communities restricted new home construction from planting a front lawn.[81] This policy may increase the prevalence of desert landscape after 2004, and thereby effect how the market values properties that participated in the Cash-for-Grass program and/or the neighbors of these properties. I estimate the effect of the 2004 prohibition on front yards with Eq. (10), including an indicator for homes built in or after 2004 and interacting this indicator with P_{it} and N_{it}.

$$\begin{aligned} \ln p_{it} = \psi[\text{post-2004}]_i + \alpha_1 DP_i + \beta_1 P_{it} + \beta_{1p04} P_{it} \times [\text{post-2004}]_i \\ + \alpha_2 DN_i + \beta_2 N_{it} + \beta_{2p04} N_{it} \times [\text{post-2004}]_i + \delta Z_i + b_{iq} + \epsilon_{it} \end{aligned} \tag{10}$$

Since June, 2009, rebate recipients must agree to maintain their conversions in perpetuity. Prior to this date, customers agreed to maintain the conversion for five, then ten years, with the agreement voided upon transfer of ownership.[82] By removing the option value that prospective buyers previously had regarding a pre-existing conversion, the June, 2009 policy may reduce the value of conversions. To explore the effect of this policy, I estimate Eq. (11), interacting indicators for enrollments and neighboring enrollments from June, 2009 onward with P_{it} and N_{it}, respectively.

$$\begin{aligned} \ln p_{it} = \alpha_1 DP_i + \beta_1 P_{it} + \beta_{1jun09} P_{it} \times [\text{post-June '09}]_i \\ + \alpha_2 DN_i + \beta_2 N_{it} + \beta_{2jun09} N_{it} \times [\text{nbr post-June '09}]_i + \delta Z_i + b_{iq} + \epsilon_{it} \end{aligned} \tag{11}$$

Table 9 shows that neither policy had any additional impact on the direct or spillover effect of conversion to desert landscape. In columns 1 and 2 I present results from estimating Eq. (10). Both columns include the full sample, and column 2 additionally includes parcel fixed-effects. I cluster standard errors at the Census block level. The statistically insignificant estimates on the

[81] pers. comm. SNWA staff, March 14, 2016 and May 8, 2017.

[82] The November 2008 application states: "The converted area must remain in compliance with all program conditions for a period of ten years. This requirement is void upon transfer of ownership. You agree to return the incentive payment if this requirement is violated." The February 2012 application states: "Rebate is subject to owner's grant of a conservation easement that restricts certain uses of the conversion project areas in perpetuity." The specific language in the applications since June 2009 has changed slightly. The June 2009 and September 2010 applications refer to the agreement as a "restrictive covenant" rather than a conservation easement.

Table 9: Regression results for the direct and spillover effect of conversion to desert landscape on home property values and the additional effects of policy changes.

	(1)	(2)	(3)	(4)
post-2004	-0.046			
	(0.0093)***			
DP (ever converts)	0.0043		0.0048	
	(0.0013)***		(0.0013)***	
Direct effect	0.011	0.017	0.010	0.014
	(0.0035)***	(0.013)	(0.0035)***	(0.014)
Direct×post-2004	-0.0040	0.034		
	(0.015)	(0.028)		
Direct×post-June '09			0.013	0.030
			(0.0095)	(0.024)
DN (neighbors DP)	0.0011		0.0016	
	(0.0010)		(0.0010)	
Spillover effect	-0.0021	-0.016	-0.0016	-0.0067
	(0.0023)	(0.0084)*	(0.0024)	(0.0087)
Spillover×post-2004	-0.010	0.024		
	(0.0095)	(0.020)		
Spillover×nbr post-June '09			-0.0016	-0.014
			(0.0055)	(0.013)
Sale years	1996-2014	1996-2014	1996-2014	1996-2014
Fixed-effects				
quarter-block	yes	yes	yes	yes
parcel	-	yes	-	yes
adj. R^2	0.95	0.93	0.95	0.93
Observations	199,037	40,755	199,037	40,755

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Standard errors clustered at the block level.

blocks: 2010 United States Census Block boundaries.

quarter: quarter of sample (e.g. 1^{st} quarter of 1997 is quarter 5).

2014 adjusted sale prices trimmed at the 1^{st} and 99^{th} percentiles.

Sample excludes parcels undertaking additions (see section 4.2).

interaction of the direct and spillover effect with the post-2004 indicator demonstrate that the front lawn restriction does not affect the direct or spillover value of desert landscape, though the restriction does appear to reduce the overall value of a home by about 5 percent (row 1). In columns 3 and 4 I present results from estimating Eq. (11). Both columns include the full sample, and column 4 additionally includes parcel fixed-effects. I again cluster standard errors at the Census block level. The statistically insignificant estimates on the interaction of the direct and spillover effect with their respective indicators for enrollments (or neighboring enrollments) after June 2009 demonstrate that the requirement to keep the conversion in place in perpetuity does not affect the direct or spillover value of desert landscape.[83] And while not the primary coefficient of interest in this exercise, the estimates of the direct and spillover effect demonstrate consistency with those presented in Table 6. The one exception is the negative and statistically significant estimate of the spillover effect in column 2. This result raises the possibility that negative spillovers from desert landscape do exist, however the weight of the evidence presented in my analysis points to no spillover effects.

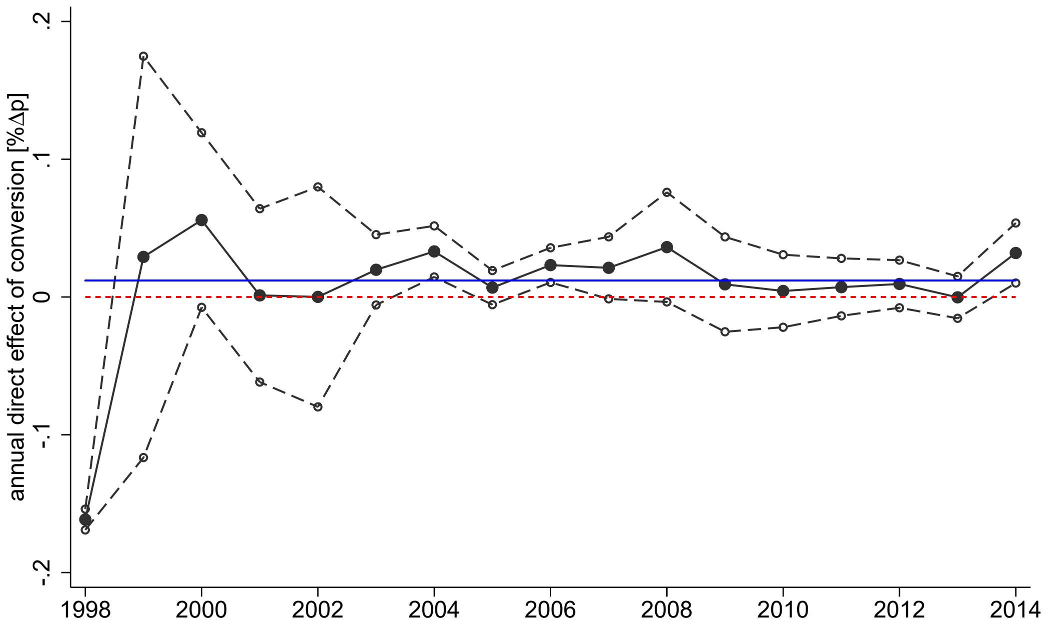
C.2 Heterogeneous effects across time

The Cash-for-Grass program has been in place for nearly 20 years, making it reasonable to expect that characteristics of residents changed in ways that impact the value of desert landscape. To estimate the impact of changing consumer preferences, I interact year of sale indicators (yos_j) with P_{it} and N_{it}, as shown in Eq. (12). The vector of β_{1j}'s and β_{2j}'s describe the *annual* direct or spillover effect of conversion to desert landscape, respectively.

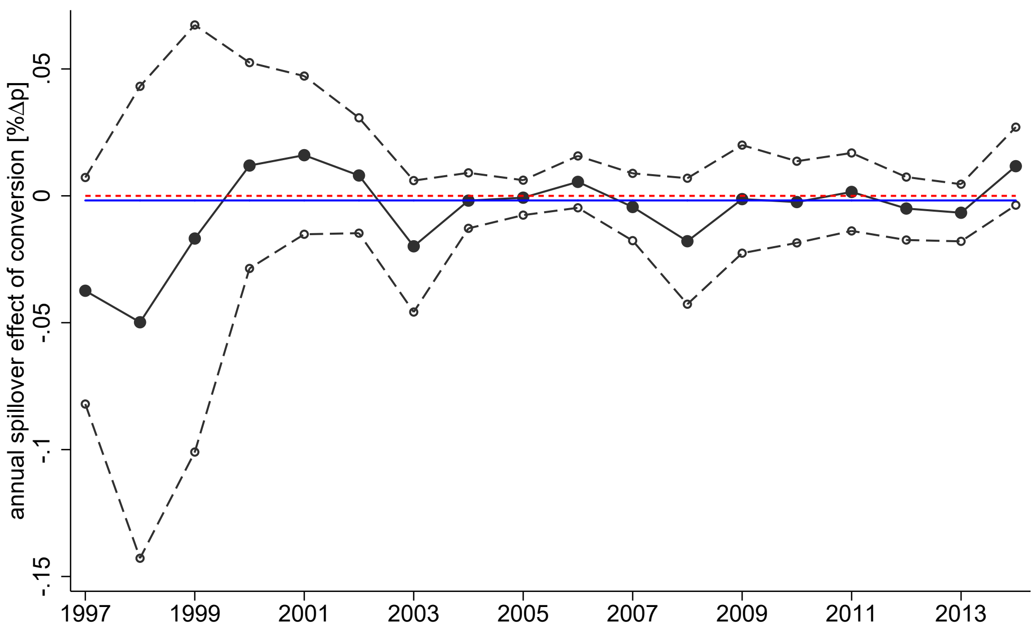
$$\begin{aligned} \ln p_{it} = \alpha_1 DP_i + \sum_{j=1996}^{2014} \beta_{1j}(yos_j \times P_{it}) \\ + \alpha_2 DN_i + \sum_{j=1996}^{2014} \beta_{2j}(yos_j \times N_{it}) + \delta Z_i + b_{iq} + \epsilon_{it} \end{aligned} \tag{12}$$

Figure 16 illustrates the direct and spillover effect of Cash-for-Grass subsidized conversions to

[83]While not shown, the coefficient estimates on each covariate effect housing prices in expected ways. The two exceptions involve negative coefficient estimates on full bathrooms in columns 1 and 3, significant at the 1 percent level, and the negative coefficient on half bathrooms in column 1 and 3, significant at the 5 percent level. Toilets make up the largest share of indoor water use (Bennear *et al.*, 2013), and the negative coefficient on bathrooms may reflect consumers' recognition of higher water bills associated with an increased number of water-intensive fixtures.



(a) Annual direct effect



(b) Annual spillover effect

Figure 16: Annual effect of conversion to desert landscape. Point estimates and 95 % confidence intervals for the direct effect (panel a) and spillover effect (panel b) of Cash-for-Grass subsidized conversion to desert landscape in each year. Results derived from estimating Eq. (12). The solid blue line represents the point estimate from Table 6, and the dotted red line highlights zero on the y-axis (i.e. no effect). The coefficients for 1996 and 1997 in panel (a) and for 1996 in panel (b) are dropped due to collinearity.

desert landscape in each year. Though imprecise, the estimates show little noticeable pattern over time, generally fluctuating around the point estimate (solid horizontal line) for the overall average direct or spillover effect shown in Table 6. Furthermore, the point estimate of the overall average effect generally falls within the confidence intervals of the annual estimates. These results suggest that the effect of conversion to desert landscape under the Cash-for-Grass rebate program has remained stable over time.[84]

C.3 Overlap of covariate distributions

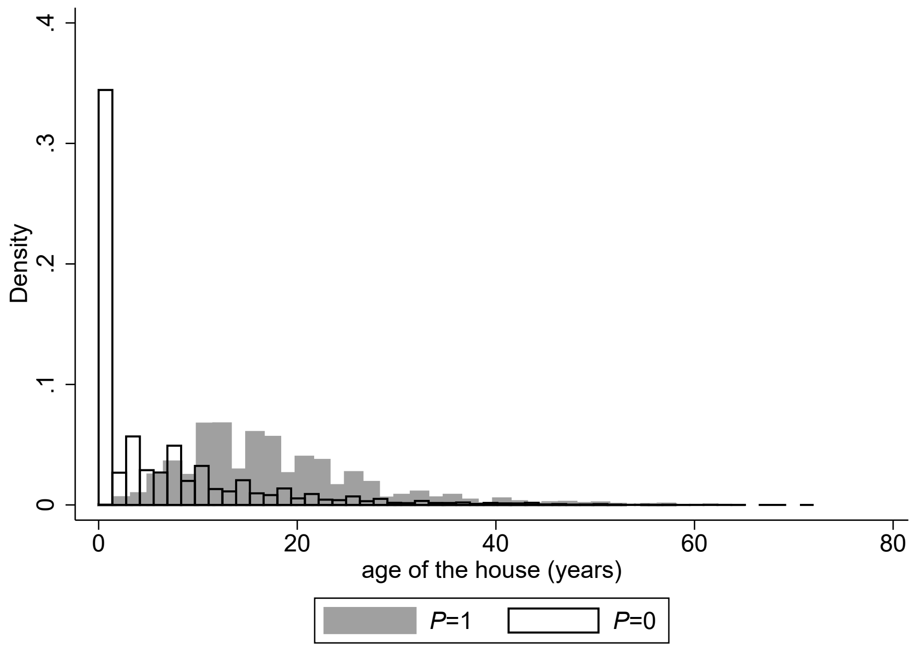
Table 4 shows very different mean age and lot size between participating or neighboring parcels prior to sale ($P = 1$ and $N = 1$) and parcels not participating or neighboring a conversion prior to sale ($P = 0$ and $N = 0$). The imbalance raises a concern regarding distributional overlap. In the following figures, I illustrate the distributional overlap for age, lot size, and the outcome variable, price. I conclude that the distributions overlap for a substantial portion of each variables' domain.

Age Figure 17 demonstrates that new homes comprise the majority of non-participating or non-neighboring parcels. The distributions of participating and non-participating parcels, and neighboring and non-neighboring parcels, however, overlap throughout the majority of the distributions' domain. While not shown, I observe a similar distributional overlap for the repeat sales model and the model that restricts sales to pre-2007.

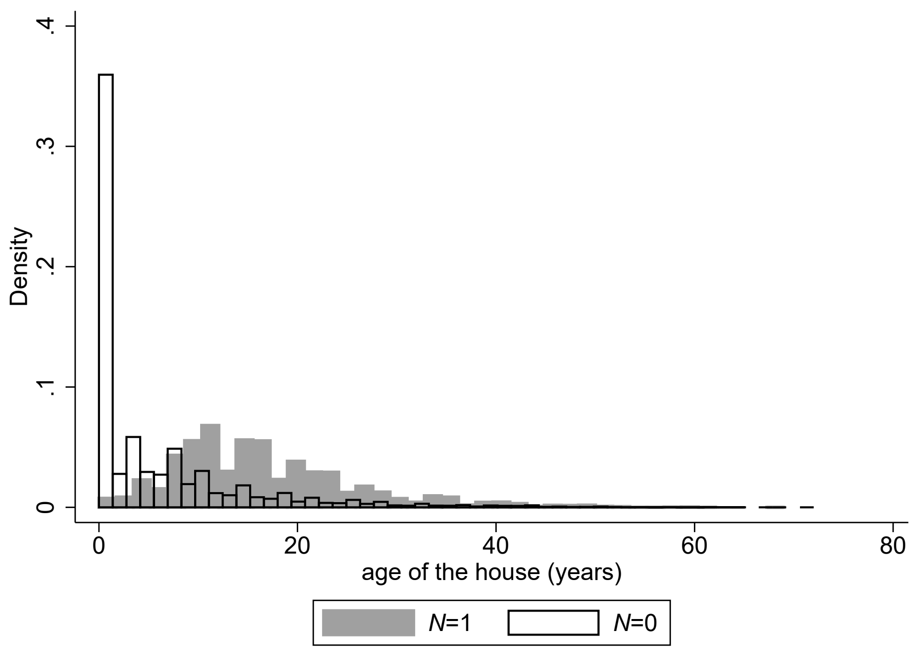
Lot Area Figure 18 shows a fairly high degree of overlap between the lot size distributions of participants and non-participants, and between the lot size distributions of neighbors and non-neighbors. This conclusion holds for the repeat sales model and the model that restricts sales to pre-2007.

Price Finally, I compare the distribution of the sale price (in levels) for participants and non-participants, as well as for neighbors and non-neighbors. Figure 19 illustrates substantial distributional overlap. Though not shown, the same can be said of the repeat sales model and the model that restricts sales to pre-2007.

[84]Though not shown, figures derived from estimating Eq. (12) with parcel fixed-effects show a similar pattern.

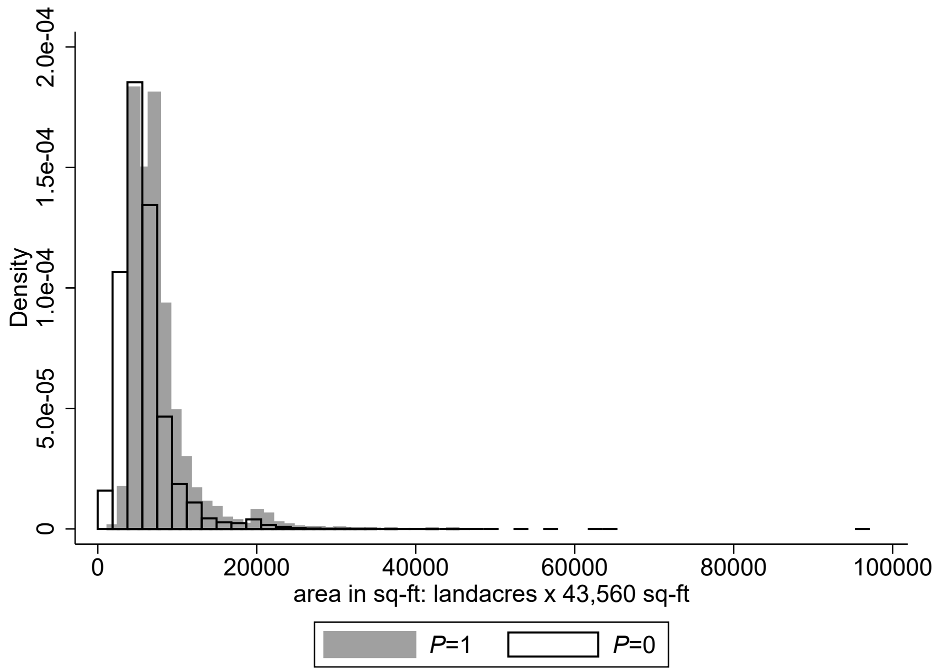


(a) Participants vs. non-participants

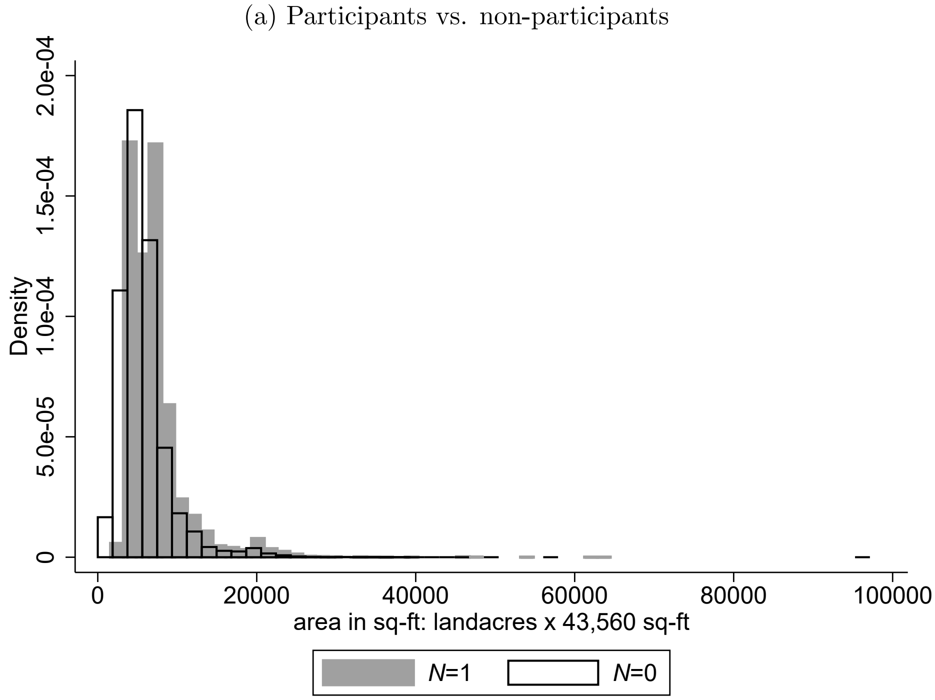


(b) Neighbors vs. non-neighbors

Figure 17: Density distribution of home age for the model including quarter-block fixed-effects and sale years 1996-2014.

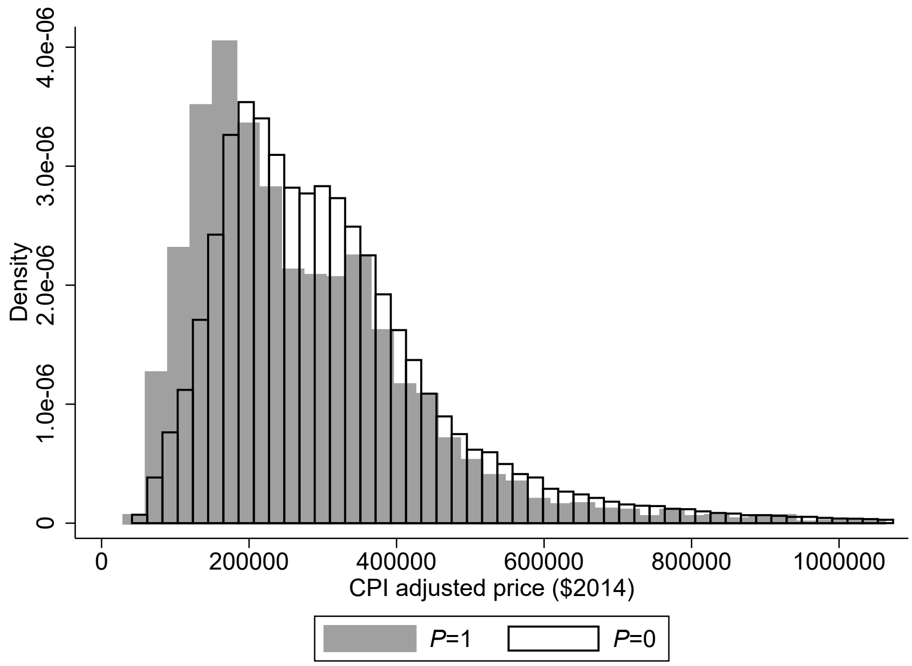


(a) Participants vs. non-participants

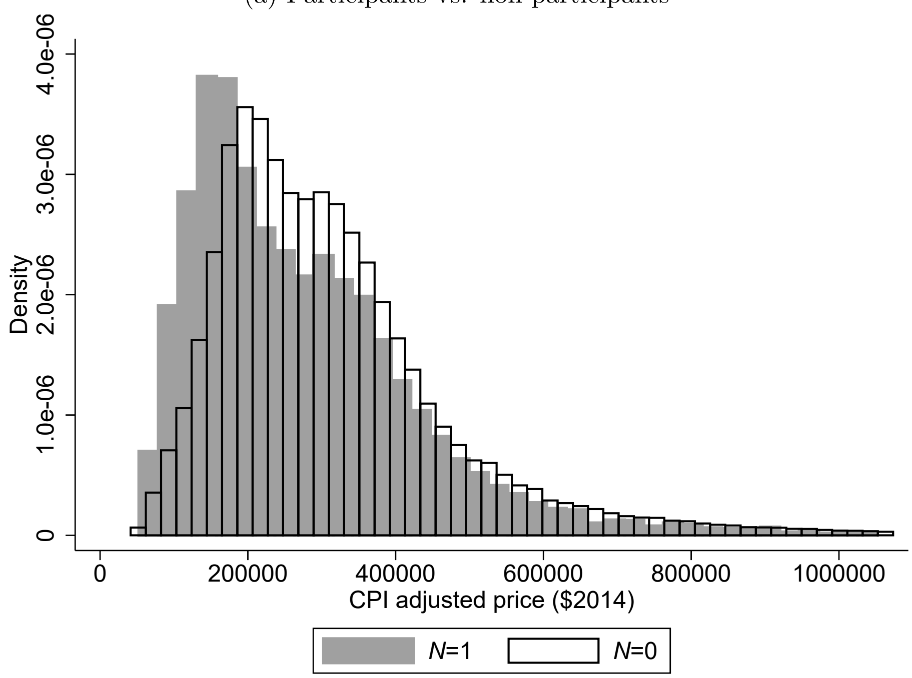


(b) Neighbors vs. non-neighbors

Figure 18: Density distribution of lot size for the model including quarter-block fixed-effects and sale years 1996-2014.



(a) Participants vs. non-participants



(b) Neighbors vs. non-neighbors

Figure 19: Density distribution of sale price for the model including quarter-block fixed-effects and sale years 1996-2014.

C.4 Robustness to alternative specifications

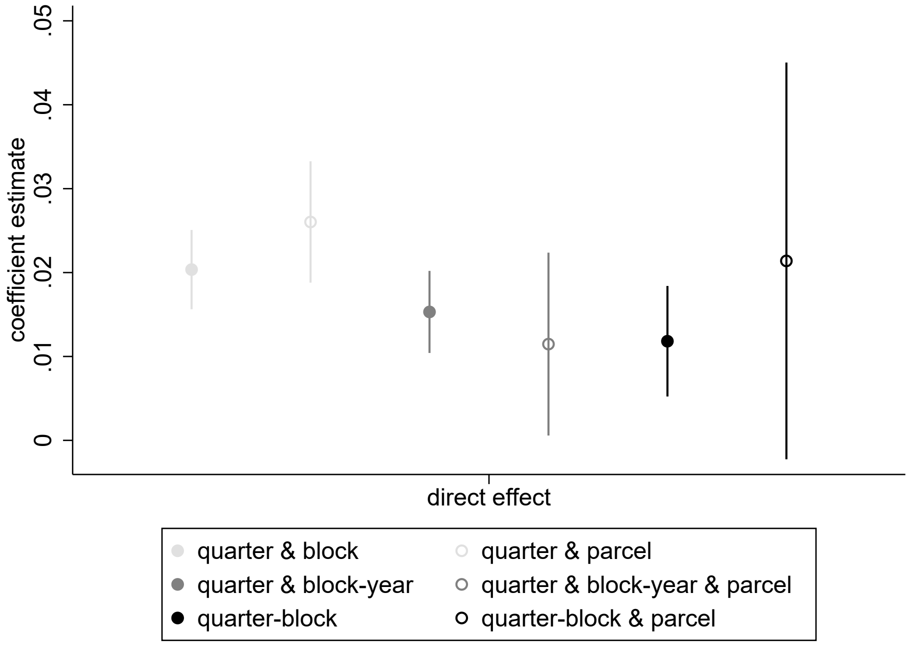
In this section, I test the sensitivity of my estimates to fixed-effect specifications and other restrictions I placed on the data in section 4. For each sensitivity analysis, I hold constant with the main specification all characteristics of the model net of the characteristic under investigation.

Robustness to fixed-effects Quarter-block and quarter-block & parcel fixed-effects control quite flexibly for fixed or varying unobserved neighborhood characteristics, but ask a lot of the data. Figure 20 illustrates the robustness of the estimates of the direct effect (panel A) and spillover effect (panel B) for the full range of sales (1996-2014) to three sets of fixed-effects. Quarter & block and quarter & parcel fixed-effects absorb average unobserved dynamics in home prices across the LVVWD, and average block or parcel effects. Quarter & block-year and quarter & block-year & parcel fixed-effects absorb average unobserved dynamics across the LVVWD, average block effects in each year of the sample, and average parcel effects for the specification that additionally includes parcel fixed-effects. Finally, quarter-block and quarter-block & parcel fixed-effects, the main specification, control the most flexibly for changes across time and space.

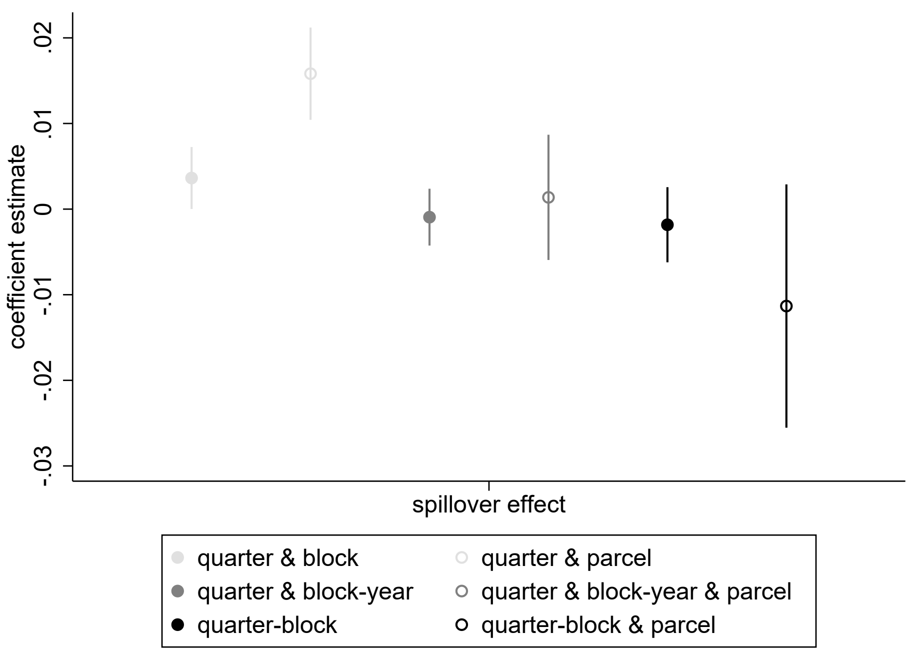
Figure 20 generally illustrates consistency across fixed-effect specifications. The two exceptions are the quarter & block and quarter & parcel fixed-effect specifications that illustrate a positive estimate for the spillover effect. As the least flexible fixed-effect specification, however, these estimates could be biased by unobserved changes across time at the block level. The weight of the evidence still suggests no net spillovers.

Robustness to additions Figure 21 shows the point estimates and 95 % confidence intervals for estimates with and without the parcels that meet my criteria for undertaking an addition. The estimates do not appear sensitive to the inclusion or exclusion of parcels undertaking additions. To the extent that my addition criteria misses parcels undergoing major structural changes, Figure 21 suggests any such missed parcels will have only a small impact on my results.

Vacant parcel sales Beginning in 2005, the assessor's office distinguishes sales by the vacancy status of the parcel. I test the sensitivity of my estimates to removing all sales not indicated as "improved" (i.e. not vacant). Dropping such parcels removes all sales of vacant properties after

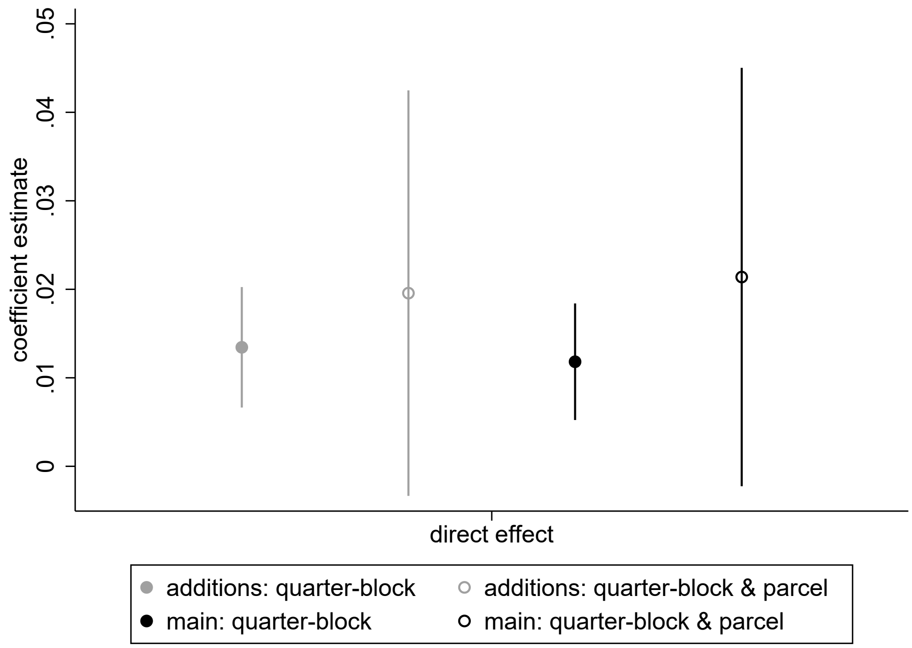


(a) Direct effect

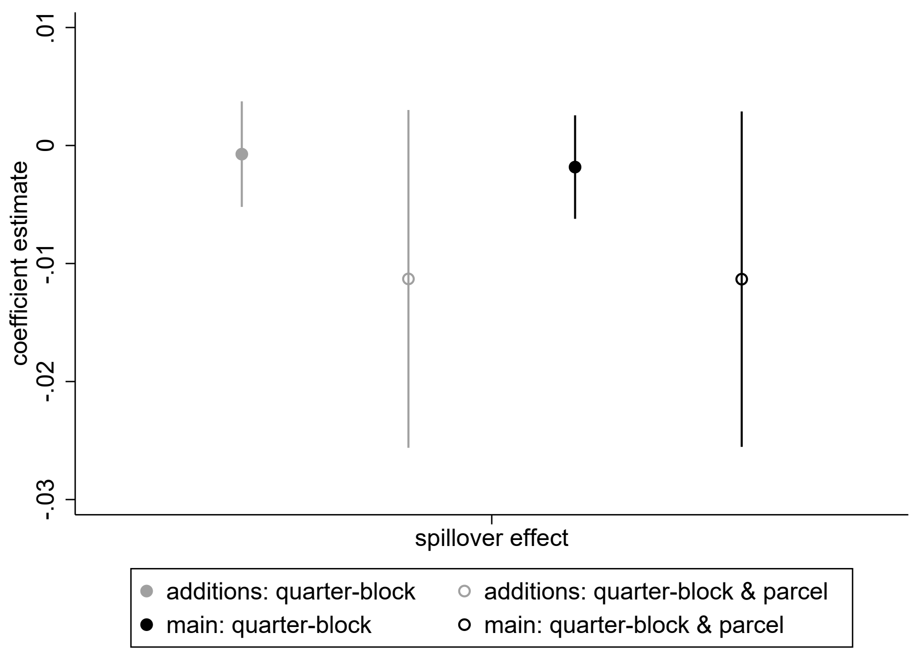


(b) Spillover effect

Figure 20: Robustness of the direct (A) and spillover (B) effect to various fixed-effect specifications. Point estimates and 95 % confidence intervals shown. Sales range from 1996 through 2014. The rightmost two points (black) represent the main specification.



(a) Direct effect



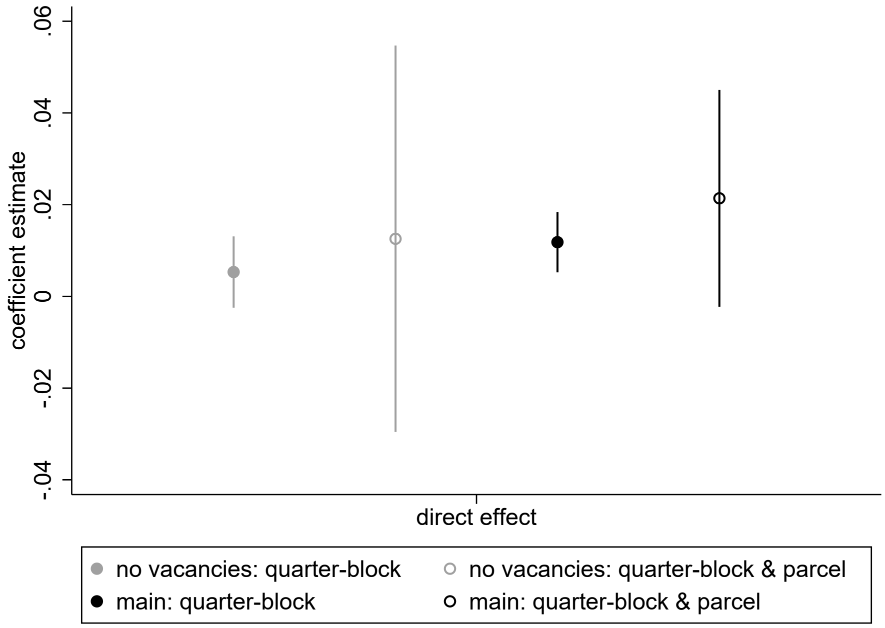
(b) Spillover effect

Figure 21: Robustness of the direct (A) and spillover (B) effect to including parcels that meet criteria for having undertook an addition. Point estimates and 95 % confidence intervals shown. Sales range from 1996 through 2014. The rightmost two points (black) represent the main specification.

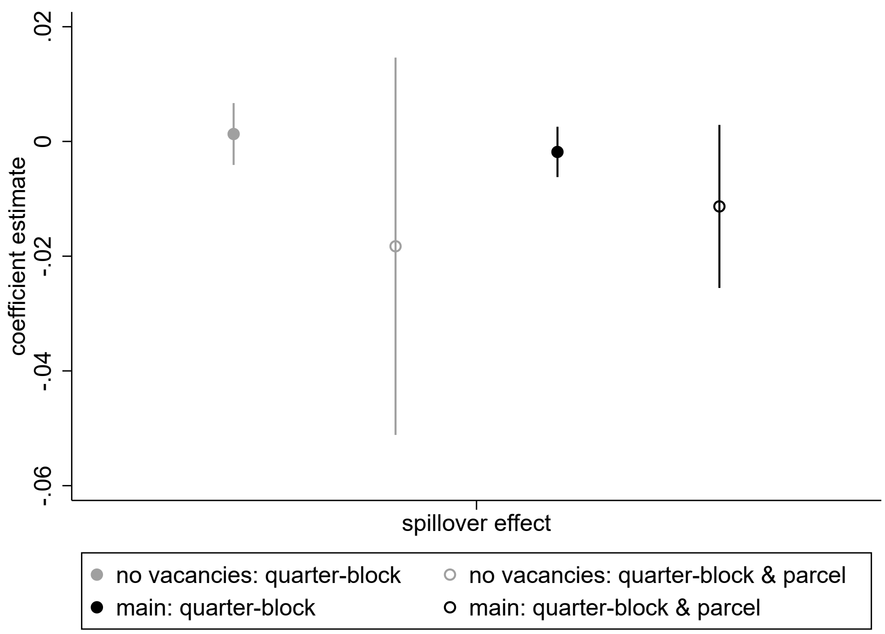
2005 and nearly all sales of all properties prior to 2005. Since the assessor data designate about 88 percent of post-2004 sales as improved, it is likely that many of my pre-2005 dropped sales are sales of non-vacant properties. However, I have no way of determining with certainty the vacancy status of these pre-2005 sales. Figure 22 shows the results from dropping all sales not designated as "improved". While I lose some precision when I keep only sales of non-vacant properties, the results appear consistent across vacancy status.

Robustness to data trimming Figure 23 illustrates the point estimates and 95 % confidence intervals for estimates with no variables trimmed, all variables trimmed (price and all control variables) at their 1^{st} and 99^{th} percentiles, and only the price variable trimmed at the 1^{st} and 99^{th} percentile (the main specification). Though I lose some precision when I do not trim the distribution of any variables, the figure illustrates that estimates are robust to the choice of data trimming. Trimming attempts to prevent outliers from driving results. My estimates here suggest that results are generally robust to outliers.

Robustness to choice of pre-Crash date The housing market crash hit Las Vegas especially hard. Figure 24 illustrates point estimates and 95 % confidence intervals of quarterly fixed-effects for a model akin to Eq. (6) that includes census block and quarter of sample fixed-effects. The figure clearly illustrates the housing bubble in the Las Vegas valley. Out of a concern that my quarter-block fixed-effects do not completely absorb all the effects of the housing crisis, in my main specification I additionally include models that limit sales to pre-housing crisis years. I choose pre-2007 sales since the most precipitous drop illustrated in Figure 24 occurs after 2006. In Figure 25, I illustrate the robustness of this selection by further estimating models that limit sales to pre-2006 and pre-2008. Specifically, Figure 25 shows the point estimates and 95 % confidence intervals for estimates derived from sale years 1996 to 2005, 1996 to 2006 (my main specification), and 1996 to 2007. I only estimate models with quarter-block fixed-effects; including parcel fixed-effects severely reduces the precision of the estimates since so few data exist in the samples limited to pre-crash years. Results appear robust to the choice of the beginning of the housing crisis.

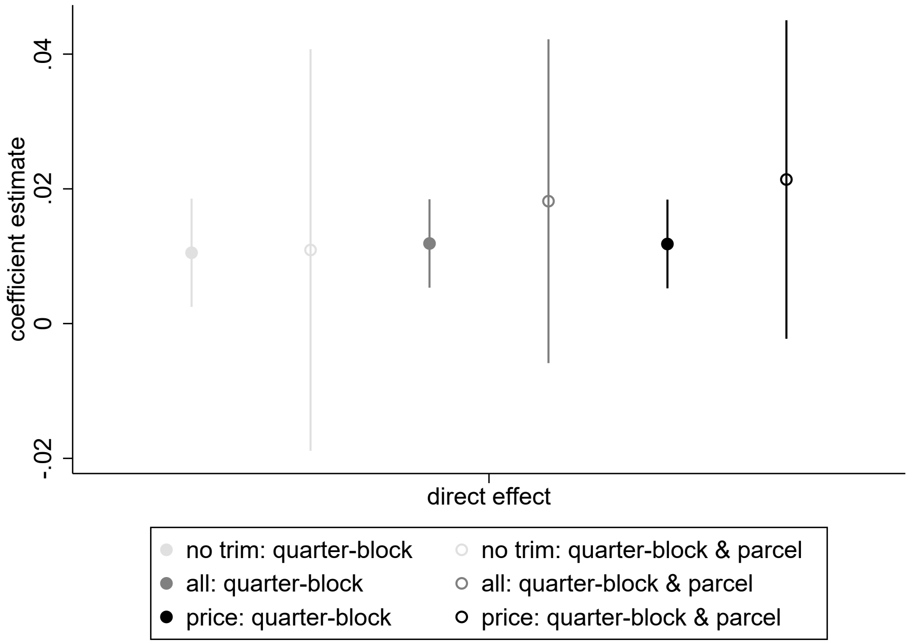


(a) Direct effect

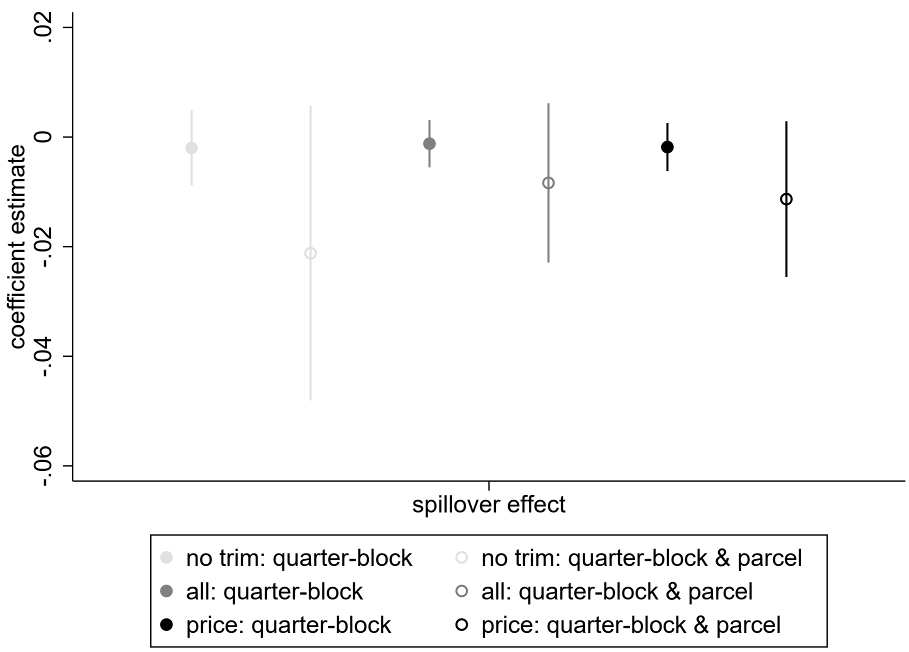


(b) Spillover effect

Figure 22: Robustness of the direct (A) and spillover (B) effect to the designation of vacant or improved parcel sales. Point estimates and 95 % confidence intervals shown. For the estimates that include only sales of improved properties, dates range from 2005 through 2014 (though a very few sales are designated as improved prior to 2005). For the estimates that include all sales regardless of the sale designation (the main specification), sales range from 1996 through 2014. The rightmost two points (black) represent the main specification.



(a) Direct effect



(b) Spillover effect

Figure 23: Robustness of the direct (A) and spillover (B) effect to how many variables in the model are trimmed at the 1^{st} and 99^{th} percentiles. Point estimates and 95 % confidence intervals shown. Sales range from 1996 through 2014. The rightmost two points (black) represent the main specification.

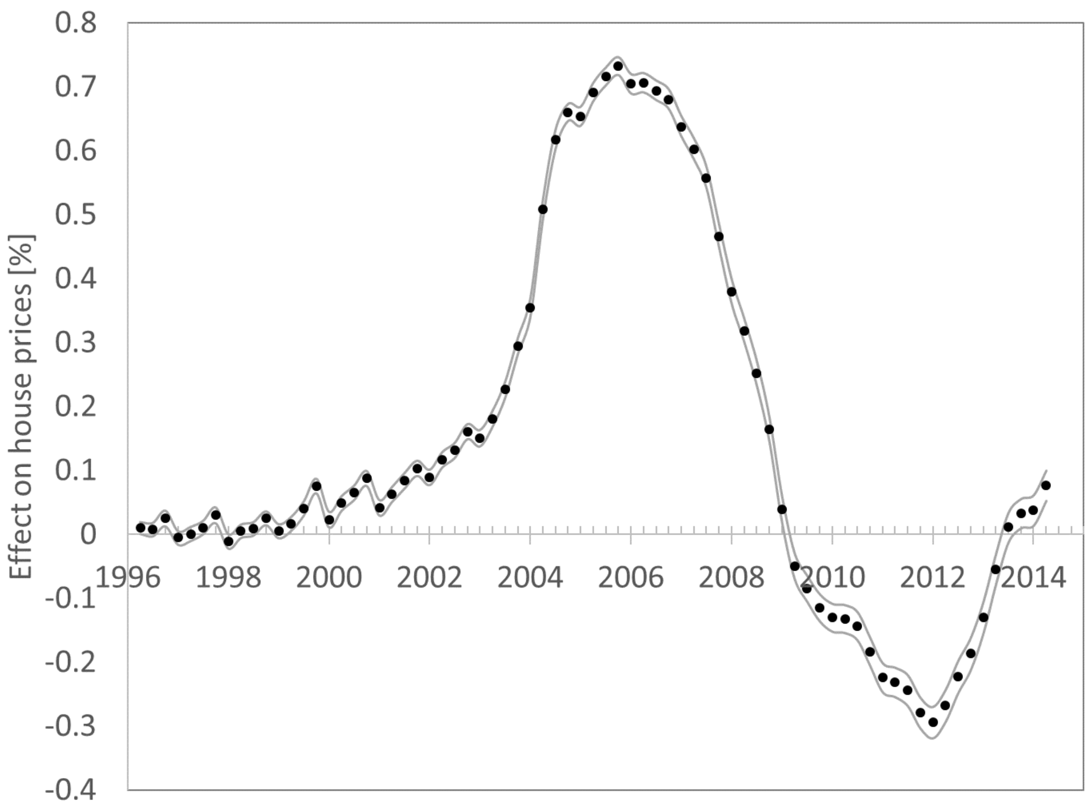
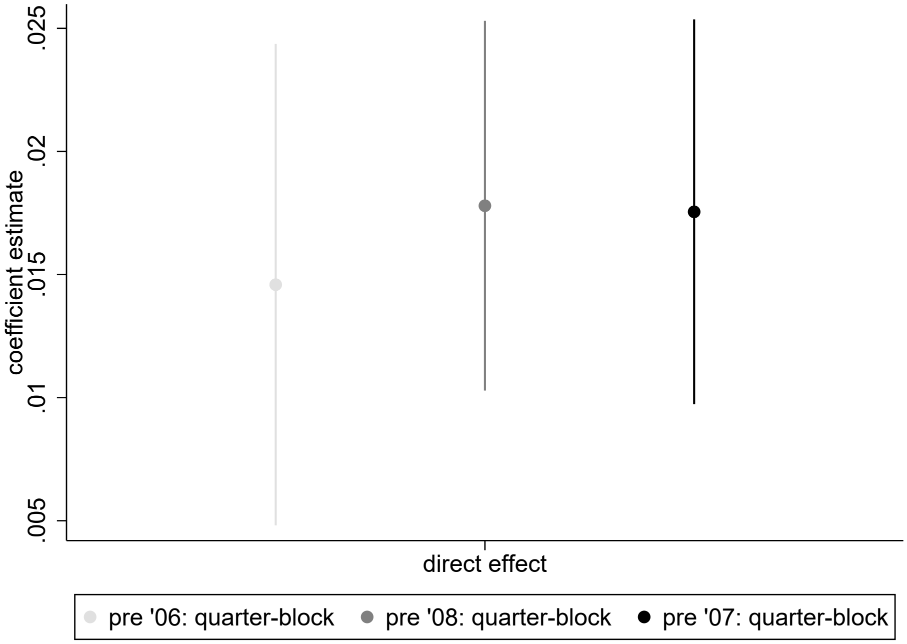
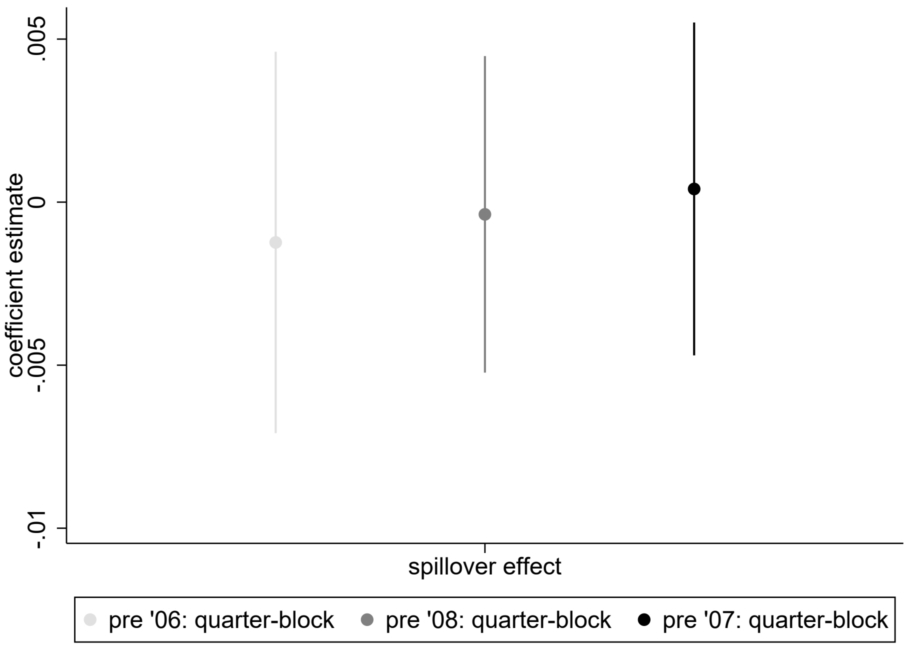


Figure 24: Plot of point estimates and 95 % confidence intervals for quarter of sample fixed-effects (qrt) estimated from the following model (where blk refers to census block fixed-effects): $\ln p_{it} = \alpha_1 DP_i + \beta_1 P_{it} + \alpha_2 DN_i + \beta_2 N_{it} + \delta Z_i + blk + qrt + \epsilon_{it}$ (which I estimate using `areg`; all other hedonic models I estimate using `reghdfe` (Correia, 2016)). Point estimates are relative to the first quarter (i.e. quarter 1 in 1996).



(a) Direct effect



(b) Spillover effect

Figure 25: Robustness of the direct (A) and spillover (B) effect to the choice of the pre-housing market crash period. Point estimates and 95 % confidence intervals shown. Sales range from 1996 through 2005, 2006 or 2007. The rightmost two points (black) represent the main specification.

Appendix D Calculation details of $/kgal-saved and net benefits

In this appendix, I provide further discussion regarding the details of my calculation of the annual cost per gallon saved and estimate of net benefits.

Additional details regarding annual cost per gallon saved

- **Total rebate outlays:** I assume rebate totals reported by the water authority are nominal to the year the rebate was granted (i.e. a rebate administered in 2007 would be recorded in 2007 dollars). I therefore adjust rebates to reflect 2014 dollars using the CPI index for all urban consumers (Bureau of Labor Statistics Series Id: CUUR0000SA0).

- **Financing costs:**[85] I ignore financing costs. Up until 2009, the rebate was funded through one-time connection charges applied to new service meters. These new connection fees would introduce little market distortion, and therefore negligible additional cost. Starting in 2009, the rebate was funded through bond measures. But the bond issue for the 2015/2016 fiscal year could be paid off entirely with only a small percentage increase in water rates. Since the water authority would pay off a bond over many years, I consider costs due to paying off bonds to be small. For these reasons, I ignore financing costs in my analysis (both in the estimate of cost per gallon saved and my estimate of net benefits).

- **Average water bill:** I calculate the water bill per 1000 gallons for an average LVVWD customer in 2013. Water charges depend upon the meter size and in 2013, over 99 percent of single-family LVVWD customers in my panel have a 1 inch, 3/4 inch or 5/8 inch meter. Using a bill calculator provided by the water authority I estimate the annual water bill per 1000 gallons for each meter size (1", 3/4" and 5/8") and calculate a weighted average water bill, with weights defined by the share of customers associated with each meter size (see Appendix A). The weighted average annual water bill equals \$3.54/kgal. Note that I assume 2013 dollars are equivalent to 2014 dollars.

- **Out-of-pocket conversion costs:** The average rebate from the 26,488 conversions that make up my water savings panel falls just under \$1,996. Since the average conversion size

[85] I am grateful to Joe Aldy for a helpful discussion on this point.

equals 1,348 ft^2, the average rebate per square-foot equals about $\$1.50/\text{ft}^2$. I assume total conversion costs equal $\$3/\text{ft}^2$, implying that the out-of-pocket expenditure for an average rebate recipient equals about $\$1.50/\text{ft}^2$. Using the average conversion size (1,348 ft^2) and the total number of conversions (26,488), I estimate that total out-of-pocket expenditures equal about \$54M. I add this to total costs to the utility reported in section 5 (\$65M), re-compute annualized cost using a 30-year time horizon and a 5 percent discount rate, and divide by total annual savings (1.6M kgal/year). The resulting program costs come to \$4.84/kgal-saved.

- **Estimate of the opportunity cost of scarce water:** Edwards and Libecap (2015) report that in the Truckee river basin, "the median price of 1,025 agriculture-to-urban water rights sales between 2002 and 2009 (2008 prices) was \$17,685/Acre Foot (AF)". I adjust this value to 2014 dollars,[86] and then divide by 325,851 gallons per acre-foot, resulting in the estimate of \$0.06/gal reported in section 5. Using sales occurring in Nevada would seem to best approximate the value of water for a Nevada water utility, and for a municipal water utility, agriculture to urban sales is a more relevant proxy for the value of water than what would be reflected in intra-agricultural sales.

Additional details regarding net benefits To be precise,[87] benefits associated with converting to desert landscape include the private benefits to the household reflected in the hedonic estimates, the scarcity value of water, as well as reduced operating and maintenance costs associated with lower delivery requirements (and any positive externalities which I ignore for the purposes of this discussion). Costs include conversion costs, and reduced revenue for the utility (which should equal the benefits consumers receive from lower water bills). However, most water utilities endeavor to price water such that revenues cover costs. Thus, the benefits from reduced operating and maintenance costs should be approximately equal to lost revenue. In my analysis in section 5, I implicitly make this assumption. But because the SNWA has other sources of revenue, it may be that operating costs exceed water bill revenue, leading me to understate net benefits.[88]

[86] http://data.bls.gov/cgi-bin/cpicalc.pl

[87] Many thanks to Nick Hagerty for the insights into net benefits contained in this discussion.

[88] To make matters more complicated, the SNWA does not actually distribute water to customers. The LVVWD and other Las Vegas area water districts supply tap water to residents' homes, buying treated water wholesale from the SNWA. In my analysis, I have assumed that the LVVWD and the SNWA are essentially one financial entity.