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PEER EFFECTS IN WATER CONSERVATION:
EVIDENCE FROM CONSUMER MIGRATION

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ABSTRACT

Social interactions are widely understood to influence consumer decisions in many choice settings. This paper identifies causal peer effects in water conservation during the growing season, utilizing variation from consumer migration. We use machine learning to classify high-resolution remote sensing images to provide evidence that conversion to dry landscaping underpins the peer effects in water consumption. We also provide evidence that without a price signal, peer effects are muted, demonstrating a complementarity between information transmission and prices. These results inform water use policy in many areas of the world threatened by recurring drought conditions.

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A data appendix is available at http://www.nber.org/data-appendix/w24812

1 Introduction

Social interactions have been shown to play a pivotal role in the diffusion of many new technologies and practices, and undergird classic economic models of technology diffusion (Griliches 1957; Bass 1969; Rogers 1995). The idea that individuals learn from their peers, neighbors, or friends to adopt behaviors or technologies has been explored in settings ranging from agriculture (Foster and Rosenzweig 1995; Conley and Udry 2010) to foreclosures (Towe and Lawley 2013) and schooling (Sacerdote 2001; Graham 2008). In the environmental realm, such 'peer effects' have been shown in the adoption of solar photovoltaic panels (Bollinger and Gillingham 2012; Graziano and Gillingham 2015) and hybrid vehicles (Narayanan and Nair 2013; Heutel and Muehlegger 2015).

This paper is the first to identify causal peer effects in water consumption. Our identification strategy relies on quasi-experimental variation from consumer migration in which new households move into peer groups and make water consumption and landscape changes. We use water billing and housing transaction data from over 300,000 households in Phoenix, Arizona to identify households that make substantial and persistent reductions in growing season water consumption that are consistent with a switch to dry landscaping. We find that a 10% increase in the fraction of peer group households who substantially reduce their growing season water consumption from one year to the next increases the probability that a focal household will also reduce their growing season water consumption by 3%.[1] We use a machine learning approach on remote sensing data to classify the greenness of a household's landscaping and provide evidence that the peer effects in water consumption are underpinned by a switch to dry landscaping. We further demonstrate a complementarity between economic incentives and peer effects by exploiting a natural experiment to show that a non-negligible price is necessary for these peer effects to influence consumer behavior.

Access to water is a major issue in many locations around the world. Over the 20th

[1]Peer group households in our primary specification are defined as other households with a 500′ radius of the focal household.

century, global water use experienced a sixfold increase and by 2025, the U.N. Environmental Program estimates that over two-thirds of the world's population will live in water-stressed regions. Concerns about water availability are equally important in many regions of the United States. For example, the California droughts of 2012 and 2015 led to combined economic losses of approximately $5.2 billion, and only a few years earlier, droughts in the Southwest and Midwest led to losses of $20 billion.[2] The U.S. Environmental Protection Agency predicts up to a 40% decrease in snow runoff and soil moisture in parts of the Western United States by 2050, further exacerbating concerns about droughts.[3]

Residential water demand is of further interest due to rapid changes in both technologies and practices. For example, between 1990 and 2010, the diffusion of higher water efficiency appliances and a shift towards dry landscaping reduced per capita residential water use by roughly 10%.[4] This decline in per capita water usage is a positive development for concerns about water availability, but can pose challenges for municipalities that designed water systems for greater demand. Thus, understanding residential water demand and the diffusion of technologies and practices that influence demand is of major interest to policymakers.

Causal peer effects have the potential to be a major influencer of the diffusion of water-saving practices, but it is well understood that these effects are challenging to identify. Challenges include simultaneity, endogenous group formation leading to issues of self-selection of peers, and correlated unobservables (Manski 1993; Brock and Durlaf 2001; Moffitt 2001; Hartmann et al. 2008). This study addresses simultaneity by using a predetermined variable for peer group decisions: switches in peer water consumption that occurred in the previous year. We address concerns of endogenous group formation and correlated unobservables using an instrumental variables strategy to exploit a source of

[2] See https://www.ama.org/publications/eNewsletters/Marketing-News-Weekly/Pages/economic-loss-us-droughts.aspx and http://www.cnbc.com/2015/03/03/california-drought-seen-having-worsening-3-billion-economic-impact-in-2015.html

[3] See https://19january2017snapshot.epa.gov/climate-impacts/climate-impacts-southwest_.html

[4] See https://water.usgs.gov/watuse/data/2010/index.html

quasi-random variation in peer group decisions. New residents to a particular house often change the landscaping of that property within a short period of time after the move. At the same time, there is a broader trend towards dry landscaping occurring in Phoenix, which we document. Accordingly, we observe a much higher probability of a switch from green to dry landscaping just after a housing transaction. Our identification strategy therefore uses shocks in consumer migration as an instrument for changes in the water consumption of a household's peer group during the growing season, while focusing on the variation remaining after inclusion of trends in housing prices, new construction, and a fine level of time-varying geographic fixed effects.

This work contributes to several literatures. Most directly, it adds new, well-identified evidence to the large and growing literature on peer effects in the diffusion of consumer behaviors. In addition, it adds to the literature on how information transmission, in our case through social learning, can influence consumer decisions about energy and water use. Several papers explore how social norm-based messages aimed at energy conservation can reduce energy use (e.g., Allcott 2011; Allcott and Rogers 2014; Ayres et al. 2012; Costa and Kahn 2013; Dolan and Metcalfe 2015; Tsvetanov and Gillingham 2018; Bollinger and Gillingham 2018) or how prosocial appeals influence energy use relative to economic incentives (e.g., Reiss and White 2008; Ito et al. 2017; Burkhardt et al. 2017).

Our paper is the first to explore how the effect of social interactions can be directly influenced by economic incentives. Jessoe and Rapson (2014) show that households are three standard deviations more responsive to temporary price increases when provided with high frequency information on electricity usage, and Dolan and Metcalfe (2015) show that the effect of financial incentives can disappear when information on social norms is provided. In the context of residential water demand, Ferraro and Price (2013) show that social comparison messages are the most effective among the least price sensitive households. However, in these studies, information transmission is exogenously manipulated. In contrast to these previous findings, we show that social interactions–a phenomenon involving *endogenous* information transmission–is instead muted in the absence of a non-negligible price signal.

Our findings contribute to active policy discussions. Water districts make large up-front infrastructure investments and face continual planning challenges. Understanding the speed and pattern of the diffusion of water conservation activities, such as a transition from green to dry landscaping, is immediately valuable for water planning purposes. An understanding of such diffusion can also enable better targeting of policies. Neighborhoods that begin with some landscape transitions in a given year may be expected to experience contagion from these first transitions, and observe a greater number of transitions in the future relative to other neighborhoods. Policymakers may be interested in one-time targeted information campaigns or subsidies for dry landscaping for fast-growing areas that are straining the water system, while such efforts may be of less interest for areas that have too much water system capacity.

The remainder of the paper is organized as follows. In the next section we provide some background on water use in Arizona and describe our unique dataset, which combines water bills, remote sensing images, and housing transaction data. Section 3 presents the model and discusses our identification strategy. Section 4 describes the results and mechanisms underlying the peer effect. Section 5 concludes.

2 Background and Data

2.1 Background on Water Use in Arizona

Residential water in Phoenix comes from three sources: wells owned by individuals or the city, the Salt and Verde rivers, and the Colorado river.[5] These sources are constrained and access to them is politically contentious. Moreover, between 1980 and 2010, the population of Phoenix increased by approximately 83%, leading to an increase in residential water use of 23%.[6]

[5] Phoenix receives only 8 inches of rain a year on average. For details see http://archive.azcentral.com/members/Blog/ShaunMcKinnon/123051

[6] See https://www.biggestuscities.com/city/phoenix-arizona

Fortunately for Phoenix and many arid regions of the United States, while water use may be increasing, water use per household has been trending downwards over time. Figure 1 shows the average water use per household in Phoenix between 1986 and 2012.[7] While there was a slight increase in the late 1990's, there is a clear downward trend throughout the entire time period. The broader decline in per household water use is primarily attributed to improved water efficiency of appliances, such as low-flow toilets and shower heads or water-efficient washers/driers and dishwashers. Yet there is a growing consensus among practitioners that the low hanging fruit in improvements in indoor water efficiency has mostly been picked (CUWA 2017).

More recently, water districts have thus been focusing on behavior change and outdoor water use. Water conservation campaigns have become popular (Ferraro and Price 2013; Brelsford and De Bacco 2018), and some areas have changed water pricing schedules. Promoting a shift to dry landscaping, with less grass and more drought-tolerant plants, is another option that has been adopted in numerous arid water districts, including several in the Phoenix metro area. Furthermore, in some cases this transition to dry landscaping is occurring without policy action. As we will see shortly, Phoenix has seen just such a trend towards dry landscaping in recent years.

2.2 Data

The foundation of our data set is monthly water billing data from the City of Phoenix water department for all single-family households served by the water department between 2004 and 2012 (308,529 households in the raw data). These contain the address of the land parcel, allowing us to geocode all parcels. They also contain the total monthly water consumption for each household, with the exception of parcels that are eligible to receive a defined amount of extremely reduced-cost flood irrigation water from the Salt River Project (SRP), a relic from previous agricultural uses whose boundaries are based on historic water rights. Roughly 43% of households in the city of Phoenix Water District

[7] A similar trend exists in landscaping. See Figure A.1 in the Appendix.

are within the boundaries of the SRP, and the general topography and climate is the same everywhere within the Phoenix basin.

SRP-eligible households only pay a small fee for the water delivery; currently, this fee is on the order of $5 per month for most households and has not appreciably changed in recent years. Relative to the cost of city water, this SRP water is nearly free. However, the SRP flood irrigation water is only suitable for outdoor uses and households must construct berms to direct the water.[8] For this reason, not all eligible houses use SRP water, and eligible houses must supplement SRP water with municipal water. We do not observe the consumption of SRP irrigation water, but from conversations with the water department we were able to verify that SRP water provision did not change over the time frame of our study.

We complement the water billing data with remote sensing images provided by the City of Phoenix in order to develop a measure of landscape greenness. These images, taken once a year in the fall, have resolutions ranging from 0.3 to 0.8 feet. All years are available at high resolution except 2010, which is only available at a lower resolution. Therefore, our remote sensing dataset spans 2004-2012, excluding 2010. Each image has three color bands (red, green, and blue). There are standard software packages for developing vegetation indices, such as the Normalized Difference Vegetation Index, but our remote sensing images lack the infrared band needed for such standard packages. Therefore, we used a machine learning approach to train the computer to find green pixels through a series of iterations. The approach we used is a supervised maximum likelihood classification routine developed by the company Imagine Software, Inc. To verify the algorithm, we compared the machine learning results to hand-coded results[9] and found that the machine learning approach provided the same coding as the hand-coded pixels in 85-90% of the parcels. The mean greenness based on the machine learning and hand-

[8]SRP water is provided to households based on plot size and is distributed in 45 minute increments. We find that annually, off-project houses use approximately 12 cubic feet of municipal water more than on-project houses on average, but this difference is not statistically significant once lot size is controlled for.

[9]Specifically, a group of interns visually estimated the percentage of turf and greenery in 30,293 parcels and we consider the coding to be accurate if it is within 1-2 percentage points.

coding are not statistically different from one another using a simple t-test of differences in means.

Figure 2 provides an example of the output of the classification process. The photo on the left shows several randomly chosen parcels while the photo on the right shows the same parcels with the pixels the computer designates as green landscaping, including tree tops, grasses, and other vegetation, highlighted in green. As is seen in the photo, trees and bright green lawn are coded green. Dry grass is coded (correctly) as not green. It may be hard to see in the photo, but succulents (like cacti) are not coded as green. Figure 3 shows a map of Phoenix with the water department territory, SRP territory, and the remote sensing subsample of 71,477 parcels.

Our next data source is the Maricopa County Assessor's Office, which provided data on housing sales and other other physical housing characteristics including pool size, lot size, construction date, home size, and garage size. Importantly, we observe the date on which the housing transactions occur and the address of the parcel, allowing us to match these data with the previous data sets. Summary statistics for physical house characteristics are presented in Table A.1 in the Appendix.

For both the water billing data and the landscape data, we geocode all addresses. To create a measure of peer decisions, we create a radius around the center of each household's parcel. In our primary specifications, we use a radius of 500 feet, but we also explore different radii in our robustness checks. Using the migration data, we also create a variable for the fraction of households sold within that radius and during or prior to that year. As we are interested in water consumption and outdoor water use is very important in a dry climate like Phoenix, we focus on the growing season water consumption, where the growing season as defined as April-September. Likewise, the non-growing season is defined as October-March.

In this study, we are especially interested in substantial *changes* in water consumption that would be consistent with a switch to dry landscaping. We explore a variety of specifications, but in our preferred specification we consider a substantial change to be roughly half of the average difference between growing season and non-growing season water consumption. The average difference between water consumption in the growing season and non-growing season is 5.7 ccf (one ccf equals 748 cubic feet), so half of this is 2.83 ccf and this is the threshold we use. This also happens to be the 25th percentile of all changes in water use.[10] We thus define a switch in water consumption as a decrease of at least 2.83 ccf in monthly water consumption during the growing season that is persistent through at least the following year. For example, the dummy variable for a switch is equal to one for household if the household exhibited a decrease in consumption between growing season $t-1$ and growing season t that was greater than 2.85 ccf and persisted for at least one more season. We similarly define our peer decision variable as the average fraction of houses in $i's$ peer group that made a switch between the $t-2$ and $t-1$ growing seasons. Finally, we create a variable for the fraction of houses in $i's$ peer group that were sold between the growing and non-growing seasons of $t-1$ and $t-2$ (including the non-growing season of $t-1$).

Table 1 presents the summary statistics for the water consumption data, landscape data, and moving data. It shows that the average house consumes 17.3 ccf (about 13,000 gallons) of water during the growing season and only 12.5 ccf (about 9,300 gallons) of water during the non-growing season. Approximately 27% of households observe a switch in landscaping in a given year t. The average number of switches by the peers is about 28%. The mean fraction of landscape greenness is 37% (with the mean taken over parcels). Further details on the data cleaning process used to develop this final dataset are included in the Appendix.

[10]For example, the median (50th percentile) change in water use between growing season t and $t-1$ would be 0. Below median changes in water use are negative while above median changes in water use are positive.

3 Empirical Specification

3.1 Challenges in Identifying Peer Effects

Peer effects are notoriously challenging to identify. A first question in any model of social interactions is how to define the peer group. Defining the peer group membership too broadly could pick up sufficient heterogeneity in the group that it leads to spurious correlations. Indeed, a careful definition of the peer group is central to identification in some studies (e.g., Bertrand et al. 2000). In our setting, we are interested in how peer effects influence the choice of water usage. While water usage itself may not be visible, landscaping is usually highly visible. This lends itself to a definition of the peer group based on spatial proximity to the focal household parcel. In a similar setting, Towe and Lawley (2013) define neighbors as the nearest 13 and nearest 25 neighbors by distance. Because there is variation in parcel sizes, we prefer a measure based on the radius around the focal household parcel (our 500′ radius includes 25.3 neighbors on average). Using a geographic peer group is also common in the literature (Topa 2001; Arzaghi and Henderson 2007; Bell and Song 2007; Manchanda et al. 2008; McShane et al. 2012; Narayanan and Nair 2013). Households can naturally also be expected to have other social groups as well, such as those relating to family, friends, schools, and jobs. So we view our measure as a minimal measure of the social group relevant to water and landscape decisions.

Identification of peer effects also relies on addressing the aforementioned list of common concerns: simultaneity, self-selection of peers, and correlated unobservables. Simultaneity (sometimes called 'reflection') refers to the concern that just as peers may influence the focal household, the focal household may influence peers. At the extreme this can lead to mathematical non-identification of peer effects. Simultaneity in this context can be thought of as an endogeneity issue analogous to the classic simultaneity of supply and demand. In the supply/demand context, the standard instrumental variables approach requires an exclusion restriction, such as a supply shifter to identify demand or a demand shifter to identify supply. Empirical peer effects studies often use predetermined (lagged) values of the peer group variable.

The challenge of self-selection of peers (sometimes described as 'homophily') can be a concern for identifying the relationship between an individual's decision and the average decisions by the peer group because individuals endogenously choose their social networks. In the case of housing choice, consumers with similar preferences can be expected to sort into neighborhoods. Empirical studies often address this issue with a rich set of fixed effects that remove variation stemming from endogenous selection of the peer group, controls for important correlated observables, or instrumental variables strategies.

Correlated unobservables refer to the many other factors that may influence both the individual and peers. For example, if there is an economic downturn facing all households in a neighborhood, their decisions may appear to be aligned, but this alignment is due to the conditions faced by the households, rather than peer effects. Likewise, gentrification may influence whether households change their landscaping to potentially raise the resale value of the home. Studies address this issue in various ways, including time-varying fixed effects or an instrument that provides a source of exogenous variation that shifts the peer characteristics but does not shift the individual characteristics.

For all three of the issues, the fundamental challenge is one of research design. Angrist (2014) clearly points out the ideal characteristics of a well-identified peer effects study: "Research designs that manipulate peer characteristics in a manner unrelated to individual characteristics provide the most compelling evidence on the nature of social spillovers." Our research design aims to manipulate peer decisions in a manner unrelated to individual decisions by exploiting plausibly exogenous shocks.

3.2 Empirical Strategy

Our empirical specification is inspired by a long line of literature that models the likelihood of a transition from one state to another as affected by social interactions (Brock and Durlaf 2001; Sirakaya 2006; Towe and Lawley 2013). In our context, we are interested in how transitions in outdoor water consumption can influence peers. Thus, we examine how social interactions influence the probability that an individual household makes a transition in water consumption during the growing season. Consider a specification that models a transition in water consumption during the growing season by household i in year t as a function of the peer group's aggregate choices in $t-1$, peer group housing attributes, time-invariant household characteristics, and time-varying characteristics of the local neighborhood or Census block b:

$$1(\Delta w_{i,t}) = \theta \overline{\Delta w}_{i,t-1} + \delta H_{i,t} + \eta_i + \phi_{t,b} + \epsilon_{i,t}. \tag{1}$$

The term $1(\Delta w_{i,t})$ is a dummy for a persistent switch in growing season water consumption. If we denote household i's peer group as the set P_i, then $\overline{\Delta w}_{i,t-1} = \frac{1}{|P_i|}\sum_{i' \in P_i} 1(\Delta w_{i',t-1})$ is the fraction of household i's peers that complete a major transition in the previous growing season, not including household i. $H_{i,t}$ is a vector that includes the average house price in the peer group in t, the change in the average house price in the peer group between t and $t-1$, and the fraction of homes in the peer group that are new construction. η_i contains time-invariant household characteristics, which we model as a household fixed effect (i.e., a fixed effect for each parcel x owner combination, so that there is a different fixed effect after a sale). $\phi_{t,b}$ captures time-varying factors such as localized economic shocks, gentrification, or major new development in a neighborhood, and we model this with Census block x year fixed effects.

Identification in this model rests on addressing the three key issues described above: simultaneity, self-selection of peers, and correlated unobservables. Our research design addresses simultaneity by using recent, but not contemporaneous, decisions by peers. This use of predetermined peer group decisions to overcome reflection follows several papers in the recent literature (e.g., Towe and Lawley 2013; Bollinger and Gillingham 2012). Endogenous group formation or self-selection of peers is addressed in several ways. First, we estimate the model *only* on the sample of households that do not move in the previous year (this includes moves during the growing season of year t, the non-growing season of year t, and the growing season of $t-1$). This may be important if exit from the neighborhood is due to self-selection or neighborhood sorting, and we explore its importance in our robustness checks.

Second, we use household fixed effects. As long as households and their neighbors remain in the same dwelling, these fixed effects capture the preferences of the household and surrounding neighbors. Third, we include Census block-by-year fixed effects to capture time-varying factors at a fine level of geographic disaggregation. In our sample, there are 12,485 Census blocks, and each Census block has on average 37 households. These controls are particularly useful because there are frictions in the housing market, such that homebuyers may be able to choose a given broad neighborhood, but are not typically going to be able to choose the exact location of the purchase. Thus, as long as any time-varying sorting into the neighborhood occurs at the Census block level or a greater level of aggregation, then the block-by-year fixed effects nonparametrically control for such sorting. Bayer et al. (2008) make a similar but stronger assumption, arguing that a neighborhood corresponds to a Census block, but that the housing market works at an even higher level of aggregation–at the Census tract level. Our approach allows for sorting at the Census block level, but like Bayer et al. (2008), we rely on frictions in the housing market to rule out sorting within our level of geographic aggregation (i.e., within Census block in our case or within Census tract in the case of Bayer et al. (2008)).

Correlated unobservables that are at a finer geographic level than the Census block group may still be a remaining issue. To address this, our research design is based on an instrumental variables identification strategy. We instrument for $\overline{\Delta w}_{i,t-1}$ using the fraction of households in the peer group that move into that peer group in $t-1$. Differences in annual home sales across neighborhoods in Phoenix may be driven by a number of factors including urbanization, gentrification, housing prices, changes in local amenities (e.g., revitalization of an old park), and new construction. A concern about using this instrument is that some of these factors may be correlated with major changes in water consumption, such as changes in the housing stock that possibly relate to gentrification. We address this concern in several ways. Most importantly, by including both household and Census block-by-year fixed effects, we focus inference on the variation in our instrument that results from hyper-local variation in which house is for sale. This a strategy employed by Bayer et al. (2016) to study housing investment decisions, Towe and Lawley (2013) to study foreclosure decisions in Maryland, and McCartney and Shah (2018) to study the decision to refinance.[11]

In these aforementioned papers, the key assumption for identification is that conditional on controls, shocks to peer group decisions (foreclosure, investments, or refinancing) and the focal household's decision are not correlated. Our dataset allows us to take a different approach and instrument for peer group reductions in water consumption with consumer migration decisions. Fundamentally, our approach relies on the assumption that housing market frictions rule out any sorting that could relate to water consumption within a Census block (recall that a block has only 37 parcels on average), which is a more mild assumption than in the previous literature. Furthermore, there is support for it. McCartney and Shah (2018) provide survey evidence indicating that realtors do not field housing requests at the block level, with the exception of new construction of the most expensive homes. Thus, the remaining variation in the fraction of movers into and out of the peer group must be due to individual shocks, such as peer moves for family reasons or a job. These shocks can be considered plausibly random with respect to major changes in a focal household's water consumption. Census data from 2014 indicate that roughly 50% of moves are for family or job reasons and it is this idiosyncratic variation we are exploiting.[12]

Should there still be sorting into a 500′ radii peer group that happens to be correlated with preferences for switching to dry landscaping, we could still have biased results. Graham (2018) describes the bias that may result if sorting occurs on correlated unobservables that remain even after the extensive use of control variables. In this discussion

[11]For comparison, these papers also use predetermined peer group variables, and in terms of the granularity of the fixed effects and controls, Bayer et al. (2016) uses zip code fixed effects and other controls at larger geographic aggregation, Towe and Lawley (2013) includes controls for housing prices as the Census tract level and county fixed effects, while McCartney and Shah (2018) uses Census block fixed effects.

[12]See https://www.census.gov/prod/2014pubs/p20-574.pdf.

he also points out that "sorting into neighborhoods is mediated by the housing market, for which we observe a price." Thus, to address any potential correlated unobservables relating to the housing market, we also include peer-group measures of housing prices, changes in housing prices, and new construction in our vector $H_{i,t}$. These additional controls directly address the possibility of a bias from sorting of households due to gentrification or the construction of new dwellings at this finer level of geography. For there to be a remaining threat to the validity of our instrument, one must believe that people disproportionally move to the radius around a household in a particular year–relative to the rest of that Census block–for factors that *both* directly influence the focal household's water consumption and are not already captured by our controls for gentrification, new home construction, focal household fixed effects, and Census block-by-year fixed effects.[13] We find this difficult to imagine.

Of course, for our research strategy to work, the instrument cannot be weak. Fortunately, we find a strong relationship between the fraction of movers into the peer group and the peer group's decision (see Table A.4 in Appendix C for full first-stage results). The F-statistic for the first stage is 3,075. The economic explanation for this relationship is that when peers move into a new house, they are likely to use the opportunity to change things about the house, including the landscaping. We find that when there is a greater fraction of sold homes in the peer group, the changes in water use and landscape greenness are negative, suggesting that peer households that move into the peer group tend to transition to drier landscapes. This is consistent with the broader trend towards drier landscapes in the Southwest, and consumer migration provides the exogenous shock to spur the transitions by peer group members.

[13]The common assumption that this does not hold is stated formally in Graham (2018) as the conditions for no sorting or matching on unobservables conditional on predetermined attributes. Graham (2018) further states that such approaches "... have a meaningful role to play in neighborhood-effects research."

4 Results

4.1 Peer Effects in Water Consumption

We begin by estimating our primary specification examining peer effects in water consumption (equation (1)). Table 2 presents ordinary least squares (OLS) estimates in columns 1 and 2, and instrumental variables (IV) estimates in columns 3 and 4. Columns 1 and 3 include household and subdivision-by-year fixed effects while columns 2 and 4 include household and Census block-by-year fixed effects. All specifications include the controls for housing prices and new construction and omit households who moved that year. The IV estimations instrument for the fraction of households in the peer group that make a switch in water consumption with the fraction of parcels in the peer group that observe a housing transaction in the previous year.

Our preferred specification is in column 4, which indicates that if the fraction of peers who made a switch in the previous year increases by 10 percentage points, then the probability that the focal household will make a switch in this growing season increases by 3%. A switch in water consumption under our definition translates to a savings of 2,117 gallons per month out of an average monthly consumption of 12,532 gallons. For reference, a typical load of laundry uses 30 gallons of water. Thus, if even 1% of the households in the Phoenix water district make a switch in their water consumption, total residential monthly water use would decrease by 6.6 million gallons, which is equivalent to the average monthly water consumption of approximately 525 households. For comparison, if we use the -0.33 water demand elasticity from Olmstead et al. (2007), water prices would have to change by 5.15% or $3.13 per month to achieve a similar change in water consumption.

One question that may arise in interpreting these results is whether we are actually estimating demand-side peer effects. It is possible that there are also supply-side factors that influence the decision to change water use through landscaping decisions. Landscaping firms may implement focused localized marketing campaigns, such as door-to-door canvassing. However, any localized marketing campaign is likely to be at the block level, rather than just around the radius of a single parcel. Moreover, we are examining the effect of predetermined peer decisions on the household's decision, and any door-to-door canvassing marketing campaign that induces peers to change water use would be typically over by the next year. For supply-side factors to play a role in our estimation, these factors would have to be persistent for over a year and truly focused on marketing to an incredibly narrow set of customers. One example of this might be a yard sign put up by the landscaping firm that a household in the peer group uses, with the goal of increasing the visibility of the landscape change to the surrounding peers. Such efforts can be viewed are firms attempting to leverage demand-side peer effects to increase sales, and are certainly possible. Thus, we interpret our results as an the combined effect of demand-side peer effects and any hyper-localized persistent marketing designed to leverage such peer effects.

To provide further evidence that our results are well-identified, we run a set of placebo tests. In our first and most important placebo test, we switch the ordering of the timing. In our specifications above, we examined the effect of switches in water consumption by peer group households in the previous growing season ($t-1$) on switches by the focal household this growing season (t). In our placebo test, we examine the effect of switches in water consumption by peer group households today (t) on switches by the focal household during the previous growing season ($t-1$). The only reason that we should find statistically significant results from the effect of peer decisions in t on the household decision in $t-1$ is if there are correlated trends that are influencing both the peers and the focal household. Indeed, it would be physically impossible for such a relationship to be due to peer effects. Thus, if we find a statistically significant effect in our placebo test, that would raise questions about whether there are other unobservable trends influencing our results, rather than actual peer effects.

Table 3 presents the results of our placebo tests. Just as in Table 2, the first two columns present the OLS results, while the second two present the IV results. The first two columns indicate a statistically significant relationship between peer group water consumption switches in t and the household's decision to switch in $t-1$. This immediately raises concerns about the identification of peer effects in the OLS specifications, even with the rich set of fixed effects. We view this result as indicating that there is a severe endogeneity issue, likely due to trends that affect both peer group water consumption and the household's water consumption. On the other hand, the IV results show no statistically significant relationship. Indeed, the coefficients themselves are much closer to zero than the coefficients in the same columns in Table 2. While this result alone cannot rule out all possible identification concerns, it shows that there is no evidence of unobservable trends confounding identification, further supporting the validity of our primary results. It also highlights the great extent to which an instrumental variables strategy leveraging conditionally exogenous variation might be essential in identifying peer effects, consistent with the arguments made by Angrist (2014).

To provide more evidence regarding the validity of our instrument, we run an additional placebo-type test relating to the concern that households may be sorting in a way that might be due to highly localized shocks. If the variation in our instrument–the fraction of households that move in the peer group–is due to highly localized trends that lead to sorting on preferences, we would expect new moves to be clustered. Thus, we use the full sample (including houses that are sold) and regress a dummy for whether the focal house is sold in that year on the fraction of households that moved in the peer group in the previous year, as well as household fixed effects and block-by-year fixed effects. We find a small and statistically insignificant coefficient, which indicates that, after including our controls, the moving process (entry or exit from the neighborhood) does not appear to show clustering.[14] We view this as further suggestive evidence that localized trends leading to sorting is unlikely to be a confounding factor in our empirical design.

Table 4 illustrates the robustness of our results to a variety of checks. In columns 1 and 2, we present the same IV specifications as columns 3 and 4 of Table 2, only we add zip code-specific time trends as further controls to address potentially localized unobserved

[14]Results shown in Table A.5 in the Appendix.

trends in water consumption. These do not significantly change the results, which may not be surprising given the highly disaggregated Census block x year fixed effects already included. Columns 3 and 4 are the same as the same columns in Table 2 only instead of dropping all parcel that had a recent sale, we include the parcels that were sold in the current growing season, previous non-growing season, or previous growing season and control for whether there was a transaction with a dummy. Again, the coefficients of interest barely change. These results underscore the robustness of our results to both the potential for unobserved trends and the modeling decision we made to exclude parcels that observed a transaction. In the Appendix, we provide further robustness checks, including examining different thresholds for determining a 'switch' in water consumption and examining different definitions of the peer group (Tables A.6 and A.7 respectively). Notably, we find no substantial difference in estimated peer effects for larger radii through 700 feet. However, the peer effects become statistically insignificant around 1,000 feet.

4.2 Are the Peer Effects Due to Dry Landscaping?

Our primary results provide strong support for peer effects in major decreases in water consumption and we defined a major decrease in water consumption based on the amount of water savings that would be expected to occur with a typical switch to dry landscaping. In this section, we examine whether dry landscaping is a primary driver of these results.

Table 5 presents the results of several analyses. First, if landscape changes are the primary driver of the peer effects we observed above, we would expect to see little or no effect in non-growing season water consumption. To test this hypothesis, we created a new dependent variable: a major decrease in water consumption in the non-growing season months of the year (October-March). We similarly created a variable for the fraction of the peer group that make a major decrease in water consumption in the non-growing season months. For both variables we use the same definition of a major decrease in water consumption (i.e., same ccf reduction) as was used in the variables in our preferred specification. In column 1 of Table 5, we see that the coefficient on the peer group variable is not statistically significant. This finding, along with our primary result, indicates that the peer effects in water consumption occur due to growing season water consumption, which is more likely to be outdoor water consumption.

To further explore whether a change to dry landscaping is a driving force behind our results, we next re-create our water consumption variables using *increases* instead of decreases in water consumption in the growing season. Thus, our dependent variable is a major increase in water consumption in the growing season of year t, and our variable of interest is the fraction of households in the peer group with a major increase in water consumption in the growing season of $t-1$. Again, we use the same definition of a major increase: an increase of 2.83 ccf. Column 2 of Table 5 shows no statistically significant relationship between the peer group increases in water consumption in the previous year and the household decision to increase water consumption.[15] This finding suggests that it is a shift towards dry landscaping that is underpinning our results, rather than broader peer effects in water consumption.

Both of these results are strongly suggestive that the peer effects in our primary results are due to a shift toward dry landscaping. Next, we shift our attention to the subsample for which we also have remote sensing landscape data. The landscape data can be useful in several ways. First, we can examine a descriptive regression of the log of the landscape greenness in a given year on the log of the household water consumption by month of the year. A strong relationship between landscape greenness and water consumption in the growing season would suggest that water consumption is being used for outdoor water use to increase the greenness of the landscape. Indeed, we find a statistically significant positive relationship between landscape greenness and water consumption in all months of the year, with the largest coefficients in the growing season, when outdoor water use is essential to keep green plants alive. This confirms our intuition that there is a clear link between water consumption and landscape greenness, which is even stronger in the growing season, as one would expect.

[15]This result does not appear to be sensitive to the definition of a major increase in water consumption.

Second, we can examine how our primary results change if we add a new covariate for a decrease in landscape greenness by the peer group into our preferred IV specification. If the channel through which the peer effect operates is a switch to dry landscaping, then when we include both landscape and water consumption peer group variables, we should expect to find a statistically significant and meaningful coefficient on the change in the peer group landscape greenness, but not on the change in the peer group water consumption. Column 3 of Table 5 presents the results of this estimation that includes both variables. We find that the coefficient on the peer group water consumption variable is statistically insignificant, while the coefficient on the the change in household landscape greenness is statistically significant and indicates a negative relationship between changes in greenness and the probability of a major reduction in water consumption as expected.[16] This confirms our hypothesis and provides further evidence that the primary mechanism underpinning the peer effects in water consumption is a switch to dry landscaping.

4.3 Role of Economic Incentives

We use the Salt River Project's provision of heavily discounted irrigation water for outdoor use during part of the year as a novel opportunity to test whether the price signal for water influences the strength of the peer effect. Recall, as mentioned above, that the boundaries of SRP are based on historic water rights boundaries. More specifically, they tend to follow the path of canals built in the late 1800s, which themselves follow the paths of ancient canals built by the Hohokam Indians. These paths were designed to bring Salt River water to irrigated farmland and were based on convenience and historic land rights based on where settlers created ranches in the 1800s. Importantly, the areas covered by SRP do not include lands that are of any different quality or climate than the rest of the Phoenix basin. Also, it is important to note that today's residential development in Phoenix is not influenced by the SRP boundaries.[17] Thus, we use this historical artifact to

[16]See Table A.2 in the Appendix for details on the differences in the landscape subsample and broader water sample.

[17]For more on the history of SRP, see https://www.srpnet.com/about/history/StoryofSRP_HistoryBook.pdf.

provide a natural experiment to explore whether SRP-eligible households are less influenced by peer water consumption and landscape choices than non-eligible households. Jessoe and Rapson (2014) and Bollinger and Hartmann (2018) show that in the electricity context, consumers reduce consumption simply from being exposed to price information, and here we examine whether consumers respond more to peers when there is a stronger price signal in landscape decisions.

For this to be a valid analysis, we must be confident that non-eligible households are a useful control group for SRP-eligible households. As there are modest differences between SRP and non-SRP households (see Appendix B), we view a matching approach as the most appropriate to assure that we have a valid control group. In our preferred specification, we match each SRP household with a nearest neighbor based on the Mahalanobis distance (Rubin 1980). We match based on key observables that might influence water consumption: previous water consumption in the growing season of $t-1$, the lot size, the house square footage, the number of bathroom fixtures, a dummy for whether the household has a pool, and the median Census tract income.[18]

In Table A.3, we show the balance of observables between the SRP households and the matched non-SRP households. The table illustrates that on all of the observables, the match is very good. For all but one of the variables, the differences are not statistically significant using a simple t-test for differences in means. For the median Census tract income, the difference is only $292 out of over $45,000, which is well within the range of normal variations in income. While one can never fully rule out unobservables, this success in matching suggests that we are comparing very similar households, some of whom are eligible for nearly free water for outdoor use and others who are not. Moreover, because the assignment into the SRP is due to a historical artifact that has no relation to the economy in Phoenix today, we find it highly unlikely that there are other unobservables that both influence water consumption and assignment to SRP eligibility.

Our hypothesis is that households that receive heavily discounted outdoor water will appear less susceptible to peer effects. We do not observe the SRP outdoor water con-

[18] We also explore other matching approaches and find similar results.

sumption, but it is important to note that this consumption is limited both in quantity and in the times it is available (which are not always predictable). Thus, to keep green plants alive, SRP-eligible households must typically use a combination of city water and SRP-provided water. We observe this in our data, as city water consumption is much higher in the growing season than in the non-growing season for SRP households (17.9 ccf versus 12.4 ccf), just as for non-SRP households. Thus, examining how SRP-eligible households respond differently than non-SRP households in their water consumption, can shed light on the role of economic incentives. We cannot rule out that SRP-eligible households exhibit peer effects in SRP water, but we can observe whether the peer effects exist in the consumption of city water that SRP-eligible households have to use to keep their plants alive during times when SRP water is unavailable.

Because the water consumption for outdoor use would be expected to be smaller for SRP-eligible households, we perform our analysis using several different thresholds for how we define a major change in water consumption. Specifically, we use thresholds that correspond to 1.5 ccf (low), 2.83 ccf (mid; our preferred), and 7.17 ccf (high). These also align with the 35th, 25th, and 10th percentiles of all changes in water consumption. Table 7 presents our primary IV specifications run separately for the SRP-eligble households (columns 1-3) and matched non-SRP households (columns 4-6) at each of the thresholds.

Regardless of the threshold used, the results reveal no statistically significant peer effects coefficients for the SRP households. In contrast, we see a statistically significant peer effect for all three thresholds when we estimate the model on the matched non-SRP sample, similar to our primarily results, although somewhat larger in magnitude. The estimated coefficient using the central case (2.83 ccf) estimate is statistically significantly different across the two samples. Because SRP households only use city water to complement the SRP water, a more appropriate comparison may be to compare the high change (7.17 ccf) in the non-SRP sample with the central case in the SRP sample (2.83 ccf). This comparison also yields a statistically significant difference across the two samples. One can make a similar comparison between the central case in the non-SRP sample and the low case in the SRP sample, and again we find statistically significant differences.

These results suggest that economic incentives to reduce outdoor water use are important for the operation of peer effects in water consumption. In neighborhoods where green landscaping is costly, neighbors may discuss xeriscaping (landscaping with slow-growing, drought-tolerant plants) as a money-saving tool. Economic incentives may affect the peer effects because households are more susceptible to peer effects when they are looking to save money on their water bill. Alternatively, neighbors may be more likely to discuss and share information about dry landscaping when there is an obvious monetary benefit. There may also be a combination of both channels.

4.4 Implications for Policy and Targeted Interventions

Our primary results provide strong evidence that water consumption decisions made by peers influence the household decision to make a major change in water consumption. We provided further evidence that switches to dry landscaping appear to be a primary driver underpinning the observed peer effects. While we are able to show that economic incentives appear to be necessary for the peer effects, we cannot further distinguish between information channels, social norm channels, supply-side channels, or combinations of all three. For example, word-of-mouth could provide information transmission that is facilitated by a changing social norm and landscapers could put signs up on parcels where they just worked (and these signs could remain up for a year). As mentioned previously, focused neighborhood-wide marketing campaigns, such as door-to-door marketing, cannot be an explanation for our results given that we are examining the effect of the peer decisions in the previous year on decisions today (and block x year fixed effects also capture the effect of one-time campaigns). But long-term persistent efforts by landscapers to leverage peer effects would most certainly be included in our estimates.

From a water district policymaker perspective, it is most useful to have the result of these factors combined. It does not matter whether landscapers are leveraging peer effects or peer effects stem entirely from word-of-mouth; in both cases the causal peer effects leading to the diffusion of lower outdoor water consumption would occur in a spatial pattern that has ramifications for water provision. Of course, the welfare effects of our findings on consumers and firms may be different, but this is a secondary consideration to the water district policymaker who is primarily focused on water provision and how the diffusion process of lower outdoor water use occurs.

Our results suggest that policies to promote dry landscaping may have substantial spillovers due to peer effects. Consider a dry landscaping subsidy that increases the probability of a household making a switch. To calculate the total impact of such a subsidy, we also need to calculate the effect on the household's peers. Using our primary specification in Table 2, we first divide our estimated peer effect by the average number of houses in the peer group (25.3) to calculate the 'susceptibility' of each household to the actions of a single peer. This can be interpreted as the expected change in the probability of a switch by the household in response to a switch by one of the peers.

We calculate the average susceptibility of households in our data from a switch by a single peer, by increasing the peer group switch variable by 4 percentage points (1/25.3). Our primary results (Table 2, column 4) imply that a 4 percentage point increase in the fraction of households making a switch will increase the probability a household switching by 1.2%. Each such switch saves 2.83 ccf (2,117 gallons) of water per month. This yields an average water savings for that household of 25.6 gallons per month. This is roughly one tenth of the savings from the original intervention–a substantial spillover from peer effects alone. Extrapolating, if 1,000 randomly distributed households across Phoenix were influenced by policy to make a switch to dry landscaping, reducing their water use accordingly, in addition to the monthly growing season reduction by the targeted households of 2,117 thousand gallons, the associated peer effects would lead to an additional reduction of approximately 25.6 thousand gallons–the equivalent of the average monthly water consumption of 2 additional households.

Of course, these estimates deserve several caveats. Most notably, they are an estimate of the average effect, but as we saw in the section above, economic incentives matter.

Policymakers may not want to target a dry landscaping program to SRP households, as our results suggest no spillovers for those households. Similarly, the SRP result suggests that targeting households that have greater economic incentives may lead to even larger spillovers. Another caveat is that our estimates show the magnitude of causal peer effects in equilibrium over our sample period. If firms or consumers respond differently in a different empirical setting, the estimated spillovers would of course be different. However, Phoenix is not only a large desert city itself, but it also has similar water concerns and a similar diffusion of dry landscaping as many other desert cities, such as San Diego, Las Vegas, Tucson, Albuquerque, etc.

5 Conclusions

In this paper, we estimate causal peer effects in residential water consumption using a unique IV strategy that leverages consumer migration into the peer group. Specifically, we study substantial reductions in growing season water usage consistent with (often observable) dry landscape transitions. To identify the effects of interest, we exploit within household and within Census block-by-year variation after controlling for housing prices and new construction; our key identifying assumption is that the remaining variation in peer housing transactions serves as an exogenous shock to the individual's peer group. We further perform a series of placebo tests that uniformly support the contention that our IV strategy addresses all three known issues in identifying peer effects: homophily, correlated unobservables, and simultaneity.

Our primary result is that a 10 percentage point change in the fraction of the peer group that have a major change in their growing season water consumption results in a 3% change in the individual household's probability of making a similar switch. We then use machine learning techniques on high resolution remote sensing data to develop a robust measure of landscape greenness. Using this measure and several other diagnostics, we provide evidence indicative of a close relationship between outdoor water use, dry landscape adoption, and switches in water consumption. This evidence supports dry landscape adoption as a primary factor underpinning our water consumption peer effects results.

One novel finding of this study is that the economic incentives complement the effect of social interactions. By exploiting a natural experiment offered by the existence of the Salt River Project's provision of heavily discounted irrigation water and a matching approach, we show that the peer effect in city water is close to zero and not statistically significant for households eligible for the discounted water. This provides further evidence that the water consumption peer effect is driven by outdoor water use and is also suggestive of the power of economic incentives on the influence of peers.

Our results have clear policy implications. The presence of a social spillover suggests that policies may have disproportionately larger indirect effects. For example, a dry landscaping subsidy that causes 1,000 distributed households within Phoenix to adopt dry landscaping would lead to a social spillover effect equivalent to the average monthly water use of two households. We anticipate that targeted policies may be even more effective, such as a policy that avoids SRP households and focuses on households in areas with a greater economic incentive to reduce water use. Of course, policymakers may be most interested in such targeted policies when there is a strain on parts of the water system from increasing demand or reduced supply. Thus, optimal policy design will inherently involve a consideration of both water district constraints and the potential spillovers possible in the target audience.

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Tables & Figures

Table 1: Summary Statistics

Variable	Mean	Std. Dev.	Min.	Max.
Panel A: Water Consumption (N=1,823,150; 265,789 households)				
growing season consumption (ccf/yr)	17.3	11.6	0	100
non-growing season consumption (ccf/yr)	12.5	8.2	0	99.3
1(household switch in t)	0.27	0.44	0	1
fraction of peer switches in $t-1$	0.28	0.11	0	1
1(housing transaction)	0.05	0.22	0	1
fraction of houses sold within 500′ in $t-1$	0.05	0.05	0	1
1(SRP-eligible)	0.43	0.5	0	1
Panel B: Landscape (N=531,650; 71,477 households)				
fraction of green landscape	0.37	0.10	0.19	0.68
1(housing transaction)	0.04	0.20	0	1
fraction of households sold within 500′ in $t-1$	0.04	0.05	0	1
1(SRP-eligible)	0.38	0.49	0	1
lot size (ft^2)	9,706	570	1,537	299,200

Notes: An observation is a household-year. Sample is sample from preferred specification. SRP refers to the Salt River Project

Table 2: Peer Effects in Water Consumption

	(1) OLS	(2) OLS	(3) IV	(4) IV
fraction of peer switches in $t-1$	0.13***	0.24***	0.33***	0.29***
	(0.006)	(0.006)	(0.12)	(0.13)
peer sales prices (1000's\$/100ft^2)	-0.30	-0.23	-0.02	-0.18***
	(0.80)	(0.90)	(0.82)	(0.91)
Δ peer sales prices (1000's\$/100ft^2)	0.64	1.01	0.34	0.96
	(0.97)	(1.07)	(0.99)	(1.08)
fraction peer new construction	-0.35**	-0.25	-0.35**	-0.26***
	(0.16)	(0.19)	(0.16)	(0.19)
Household Fixed Effects	Y	Y	Y	Y
Subdivision x Year Fixed Effects	Y	N	Y	N
Census Block x Year Fixed Effects	N	Y	N	Y
R-squared	0.178	0.211	0.177	0.211
N	1,823,150	1,823,150	1,823,150	1,823,150

Notes: The dependent variable is 1(household switch in water consumption in t), where a switch is defined as a reduction in a growing season month of at least half the difference between the growing season and non-growing season consumption that does not return. An observation is a household parcel-year. The peer group is defined here as all houses within a 500′ radius of the household and on average, there are 25.3 houses within a 500′ radius of any household in our study. The 'fraction of peer switches' refers to the fraction of households in the peer group that make a switch in water consumption in the previous growing season. Column 1 and 2 present OLS peer effect results. Columns 3 and 4 instrument for the fraction of peer switches using the fraction of parcels with housing transactions within 500′ in the previous year. Standard errors are clustered at the subdivision level. *** denotes significance at 1% level, ** at 5% level, * at 10% level.

Table 3: Placebo Tests

	(1) OLS	(2) OLS	(3) IV	(4) IV
fraction of peer switches in t	0.13***	0.24***	0.04	0.11
	(0.006)	(0.006)	(0.10)	(0.11)
peer sales prices (1000's\$/100ft^2)	-0.52	0.008	-0.52	-0.03
	(0.77)	(0.97)	(0.77)	(0.97)
Δ peer sales prices (1000's\$/100ft^2)	0.72	0.60	0.64	0.55
	(0.94)	(1.13)	(0.94)	(1.12)
fraction peer new construction	-0.20	-0.16	-0.21	-0.17
	(0.18)	(0.18)	(0.18)	(0.18)
Household Fixed Effects	Y	Y	Y	Y
Subdivision x Year Fixed Effects	Y	N	Y	N
Census Block x Year Fixed Effects	N	Y	N	Y
R-squared	0.176	0.210	0.176	0.210
N	1,823,150	1,823,150	1,823,150	1,823,150

Notes: The dependent variable is 1(household switch in water consumption in $t-1$), where a switch is defined as a reduction in a growing season month of at least half the difference between the growing season and non-growing season consumption that does not return. An observation is a household parcel-year. The peer group is defined here as all houses within a 500′ radius of the household and on average, there are 25.3 houses within a 500′ radius of any household in our study. The 'fraction of peer switches' refers to the fraction of households in the peer group that make a switch in water consumption in the growing season t. Column 1 and 2 present OLS peer effect results. Columns 3 and 4 instrument for the fraction of peer switches using the fraction of parcels with housing transactions within 500′ in year t (to align with the fraction of peer switches variable). Standard errors are clustered at the subdivision level. *** denotes significance at 1% level, ** at 5% level, * at 10% level.

Table 4: Robustness Checks

	(1)	(2)	(3)	(4)
	Time Trends		Include Sold Homes	
fraction of peer switches in $t-1$	0.32***	0.29**	0.34***	0.41***
	(0.12)	(0.13)	(0.12)	(0.13)
peer sales prices (1000's\$/100ft^2)	-0.003	-0.156	-0.387	-0.855
	(0.819)	(0.907)	(0.740)	(0.828)
Δ peer sales prices (1000's\$/100ft^2)	0.321	0.928	0.842	1.816*
	(0.996)	(1.076)	(0.919)	(0.989)
fraction peer new construction	-0.349**	-0.259	-0.371**	-0.272
	(0.164)	(0.194)	(0.163)	(0.189)
Household Fixed Effects	Y	Y	Y	Y
Subdivision x Year Fixed Effects	Y	N	Y	N
Census Block x Year Fixed Effects	N	Y	N	Y
Zip code-specific Time Trends	Y	Y	N	N
R-squared	0.177	0.211	0.169	0.201
N	1,823,150	1,823,150	1,979,932	1,979,932

Notes: The dependent variable is 1(household switch in growing season water consumption in t). All specifications instrument for the fraction of peer switches using the fraction of parcels with housing transactions within 500′ in the previous year. The specifications that include sold homes include a dummy for whether the parcel had a transaction in the last year. An observation is a household parcel-year. All variable definitions are the same as in Table 2. Standard errors are clustered at the subdivision level. *** denotes significance at 1% level, ** at 5% level, * at 10% level.

Table 5: Evidence that Landscaping Decisions Underpin Water Results

	(1) Off-season	(2) Increase in use	(3) Landscape added
fraction of peer water switches in $t-1$	0.09	-0.26	0.04
	(0.09)	(0.46)	(0.37)
change in average peer greenness			-27.78***
			(0.00)
Housing price controls	Y	Y	Y
New construction controls	Y	Y	Y
Household Fixed Effects	Y	Y	Y
Census Block x Year Fixed Effects	Y	Y	Y
R-squared	0.23	0.20	0.43
N	1,823,150	1,823,150	288,006

Notes: Column 1 uses downward switches in water consumption in the non-growing season for both the dependent variable and the peer group variable. Column 2 uses increases in water consumption (upward switches) for both the dependent variable and the peer group variable. Column 3 is identical to Column 4 in Table 2, only with the new covariate, the average change in the landscape greenness in the peer group. All specifications instrument for the fraction of peer switches using the fraction of parcels with housing transactions within 500′ in the previous year. An observation is a household parcel-year. Standard errors are clustered at the subdivision level. *** denotes significance at 1% level, ** at 5% level, * at 10% level.

Table 6: Table of Balance for Matched Households

	(1) SRP	(2) Matched	(3) Diff	(4) t-stat
lot size (ft^2)	7,952	7,950	1.9	0.10
water consumption (ccf)	19.81	19.81	-0.004	0.09
house size (ft^2)	1,557	1557	-0.2	0.07
# bath fixtures	6.09	6.09	0.001	-0.10
1(has pool)	0.193	0.193	0.000	-0.03
median HH income ($)	45,663	45,955	-292	-4.72***

Notes: Column 1 reports means for SRP households in the water consumption data with standard deviations in parentheses. Column 2 reports means for the matched non-SRP households, using Mahalanobis matching, in the water consumption data with standard deviations in parentheses. Column 3 reports the difference in means, while column 4 shows the t-statistic for a two-sided test of differences in means. Median HH income refers to the median household income at the Census tract level. *** denotes significance at 1% level, ** at 5% level, * at 10% level.

Table 7: Role of Economic Incentives

	(1)	(2)	(3)	(4)	(5)	(6)
		SRP-eligible			Matched non-SRP	
	low	mid	high	low	mid	high
fraction of peer switches in $t-1$	0.02	0.18	0.10	0.55**	0.45***	0.18***
	(0.24)	(0.18)	(0.15)	(0.26)	(0.08)	(0.05)
Housing price controls	Y	Y	Y	Y	Y	Y
New construction controls	Y	Y	Y	Y	Y	Y
Household Fixed Effects	Y	Y	Y	Y	Y	Y
Census Block x Year Fixed Effects	Y	Y	Y	Y	Y	Y
R-squared	0.16	0.18	0.20	0.22	0.24	0.36
N	754,578	754,578	754,578	843,330	843,330	843,330

Notes: The dependent variable is 1(household switch in growing season water consumption in $t-1$). An observation is a household parcel-year. The peer group is defined here as all houses within a 500′ radius of the household. The 'fraction of peer switches' refers to the fraction of households in the peer group that make a switch in water consumption in growing season t, but each column uses a different threshold for defining a switch. low refers to 1.5 ccf, mid 2.83 ccf, and high 7.17 ccf. All specifications instrument for the fraction of peer switches using the fraction of parcels with housing transactions within 500′ in the previous year. The 'matched non-SRP' estimations in columns 4-6 include only the subsample of matched homes, identified using a Mahalanobis matching routine. For Columns 1-3, standard errors are clustered by subdivision. For Columns 4-6, standard errors are bootstrapped clustered by subdivision. *** denotes significance at 1% level, ** at 5% level, * at 10% level.

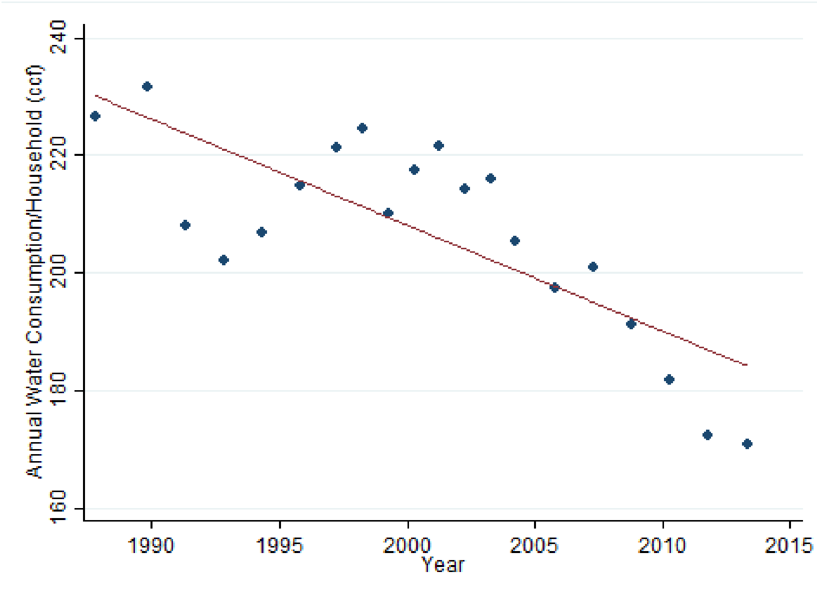

Figure 1: Binscatter plot of the annual water consumption per household in the Phoenix City Water District along with a linear best-fit trendline. Source: City of Phoenix Water Services Department.

Figure 2: Illustrative remote sensing images demonstrating the classification of green space. Panel A on the right shows what our remote sensing images look like, while Panel B on the left shows how the machine learning algorithm codes the pixels of green space.

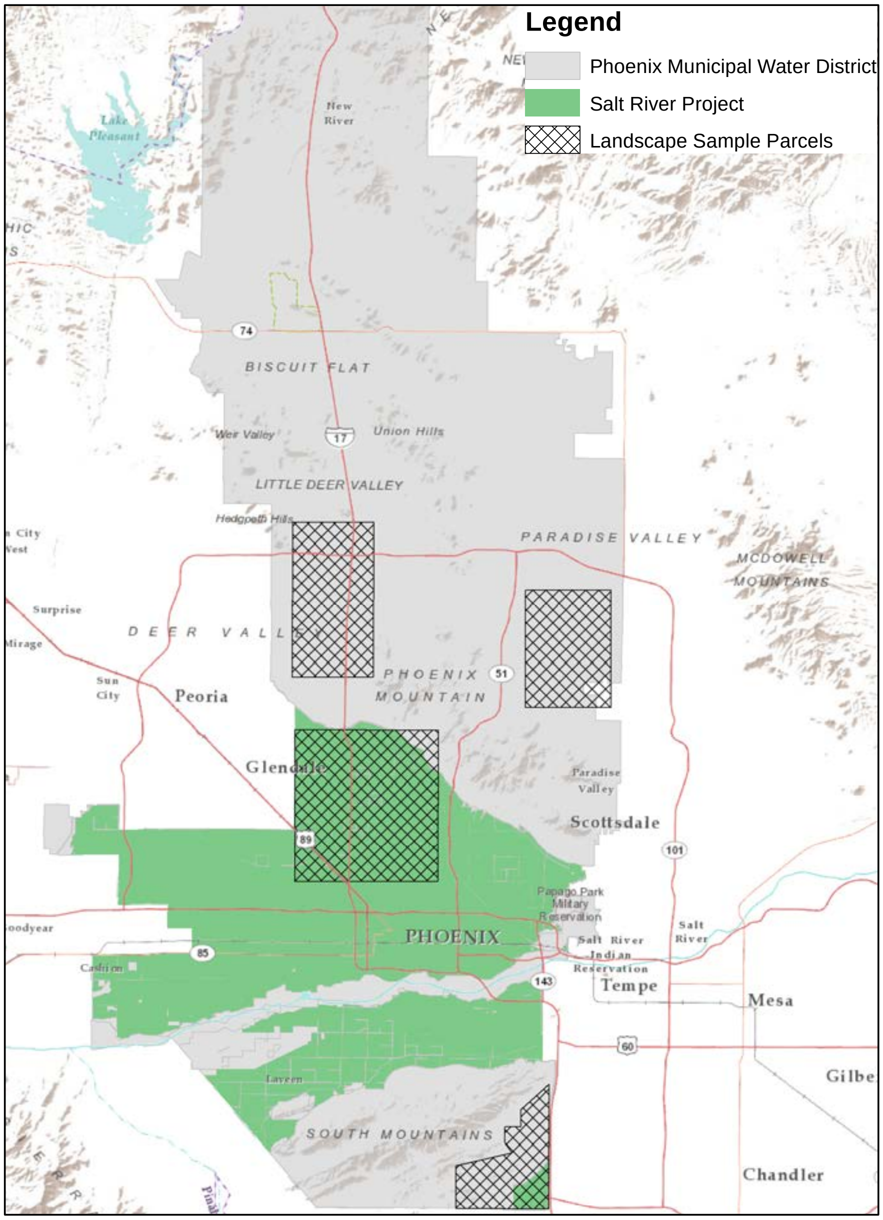

Figure 3: Phoenix water district boundary, along with identification of areas under the Salt River Project and landscape sample parcels.