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“When the well is dry, we know the worth of water”.

— Benjamin Franklin, *Poor Richard’s Almanac*, 1746.

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ABSTRACT

Ensuring the efficient allocation of water resources among end users has become crucial in light of increasing climate variability and the high capital and environmental costs of developing new supply. However, within the two largest sectors of water consumption — agricultural users and residential users — the different nature of water use and governing institutions gives rise to different challenges in allocating water across competing demands. This dissertation comprises two essays, both case studies evaluating policies to improve water management in each sector respectively. Informed by different settings, I use novel data and methods to estimate impacts of the distinct reforms. The two chapters provide lessons about how policymakers in either sector can improve water management in the future.

Chapter 1 measures the monetary value created by clarifying property rights for water within the agricultural sector, through a legal process of 'water right adjudication.' Between 1987 and 2014, the Snake River Basin Adjudication determined who had legal rights to use water and what trades would be hydrologically permissible, covering 139,000 water rights and 90% of Idaho's water use. Using differences in the timing of adjudication between different sub-basins, I identify the medium-run impacts of this adjudication. I find that adjudication caused a 140% increase in the frequency of water right trading, that these trades moved water to relatively more productive parcels of land, and that adjudication increased total crop acreage by 3.9%. To evaluate whether these benefits justify the $94 million Idaho spent in legal proceedings during the adjudication, I use a revealed preference framework and exploit quasi-random variation in federally-administered crop insurance prices to monetize the value of changes in crop choice after adjudication. I find that the one-time adjudication of the Snake River Basin increased the value of Idaho's agricultural output by $250 million per year.

Chapter 2 studies policies adopted by a large Californian municipal water utility to

achieve their State-mandated target of reducing residential water use by 25% during the recent 2011 to 2017 California drought. Because the municipality and State simultaneously adopted many different reforms to reduce water use, it is unclear which particular policies drove the observed conservation. My co-authors and I use hourly micro-data from over 86,000 single family households between 2013 and 2016 to disentangle the impacts residential water use of these different policies. First, we find that a 10% increase in marginal rates is associated with a decrease in household water use of 20 gal/day. Over our sample period, these rate changes are responsible for saving 25 gal/day. Second, reducing the number of days households are allowed to use water outdoors results in a substitution of water use from banned days to the remaining non-banned days. However, there is also a persistent decrease in water use by 6% (30 gal/day) after this policy change, particularly during hours when outdoor use was never permitted, suggesting the policy change might have increased compliance with the regulation. Thirdly, water use declines by 74 and 44 gallons/day after the announcement of a *State of Emergency* and *Mandatory Water-Use Regulations*, respectively. These major State-level announcements appear to induce interest in the drought, as measured by Google searches. A mediation analysis shows that our measure of drought awareness is highly correlated with water use, but, after controlling for city and state policies, this correlation disappears. Finally, we find that adoption of city-funded rebates for water-efficient toilets and lawn replacement leads to substantial water savings (both 55 gal/day); however, the aggregate impacts of the program are negligible due to low-take up rates.

CHAPTER 1

THE VALUE OF CLARIFYING PROPERTY RIGHTS FOR WATER

1.1 Introduction

As first identified by Coase (1960), transaction costs are a crucial determinant of allocative efficiency in resource markets. Reforms that reduce transaction costs will increase the number of potential Pareto-efficiency improving trades, which will lead to more efficient ex-post resource allocation.

Transaction costs might be an especially important institutional barrier to efficient allocation in the context of water rights, because patterns of water allocation reflect the historical development of water resources, rather than the distribution that might be optimal today. Furthermore, because water availability poses a binding constraint on agricultural activity in arid regions, allocating water more efficiently potentially has high returns. As climate change and urban growth cause patterns of water demand to change, institutions must be sufficiently flexible to ensure that water continues to be allocated efficiently.

For example, in the Western United States, water use is governed by the legal doctrine of 'prior appropriation.' Under this doctrine, historical patterns of water use give rise to de-facto property rights. Specifically, if an individual has historically diverted water and put it to beneficial use, then they gain a legal right to continue diverting water for beneficial use in the future. Water rights exist detached from the land on which they are used, and so in principle can be bought and sold. Because of this many economists argue that this could give rise to a flexible market for water rights.[1] However, the development of *markets for water* has been hampered by a lack of State support. Although water rights exist as legal

1. Anderson and Libecap (2014) advocates for this. Burness and Quirk (1979) build a model which highlights the conditions under which 'Prior Appropriation' leads to efficient allocation.

entitlements, States have historically spent few resources attempting to verify or document these rights systematically.[2] Furthermore, enforcement of rights has typically involved time-consuming litigation, which is not necessarily responsive to the immediate needs of irrigators during times of drought.

This lack of documentation of is significant because it has made the trading of water rights difficult for several reasons: Firstly, before trading, potential sellers must to prove to the State that they have a legitimate claim. This is costly because it requires systematically documenting historic use, and it also puts potential sellers at risk of losing their water if the State fails to endorse their right. Second, the State requires that proposed trades of water rights have no adverse impacts on third parties, such as other water right holders, environmental users or other stakeholders. Mitigation plans are typically required to alleviate third-party impacts before trades are approved. However third party are particularly difficult to identify, especially if it is not documented who the potential third parties are. For example, demonstrating a proposed trade has no third party impacts might require hydrological modeling, or designing mitigation plan to offset third party impacts, which requires engineering or legal expertise. When considered through an economic lens, these are examples of what Coase (1960) would describe as transaction costs.[3]

Transaction costs entrench status quo of water use, which reflects historical patterns of water resource development, rather than matching how water might be most efficient allocated today [4]. Typically disputes have been resolved on an ad-hoc basis; when a problem arises, a judge will determine the water rights of the parties involved. However, over the past thirty years, it has become common for States to try to minimize disputes by undertaking a process of *general basin adjudication. General basin adjudication* is a process in which

2. Colorado is the only State in the West that has systematically documented and administered its water rights continuously since the founding of the State. As a result, today Colorado has the most functional water markets in the West.

3. Colby (1990) discusses what he calls ‘Policy Induced Transaction Costs’ in water markets.

4. Leonard and Libecap (2016) systematically document this fact in Colorado

a court formally verifies which users within a watershed have valid rights, and what their entitlements are.

However, because of their scale and legal complexity, large scale adjudications often take decades to complete and involve significant litigation costs. Initially, experts thought that large adjudications could be completed within a few years. However, as litigation caused the process to drag on for decades, the cost to taxpayers became increasingly steep, leading some legal scholars to question the efficacy of *general basin adjudication* (Tarlock, 2009; Macdonnell, 2015).

In *general basin adjudication*, the State pays a large up-front fixed cost in order to reduce the marginal cost that right holders will pay every whenever they trade water rights in the future. In terms of up-front costs, Idaho spent over $94 million on the legal process of adjudicating and documenting water rights. Furthermore, that number doesn't include the private costs incurred by holders of water rights who had to defend their claims. On the other hand, adjudication increases the number of potential Pareto-efficiency improving water right trades by reducing the variable costs associated with each trade. This, in turn, will lead to more trading and a more efficient ex-post allocation. For the benefits of adjudication process to exceed the costs in the medium run, the efficiency gains in the new ex-post allocations and water use must outweigh the legal costs spent on adjudication. However, to date no research has attempted to quantify the impact of a large-scale adjudication, a gap in the literature that this paper aims to fill.[5,6]

This paper asks two questions: First, what impact does adjudicating water rights have

5. Adjudication might also have long-term benefits. For example having a more flexible system of water rights might allow for more flexible response to future pressures such as urban growth and climate change. Furthermore systematically documenting water rights will enable planners to make more rational planning decisions in the future. Although hugely significant, these benefits are beyond the scope of this paper, which will focus on identifying only the medium-run benefits of adjudication. However, because of the potential for these long-term benefits, my results should be interpreted as a lower bound on the true benefits of adjudication.

6. Debaere and Li (2017) is a recent exception.

on patterns of trading and leasing of rights? And second, what impact does adjudication have on patterns of farm production and investment?

I study these questions in the setting of the adjudication of the Snake River Basin in Idaho. Between 1987 and 2014, The State of Idaho build a large bureaucracy to undertake the largest water right adjudication ever completed to date in the Western United States[7]. The adjudication covered 87 percent of Idaho's land area and 90 percent of its water use. The adjudication covered 139,000 individual water rights, which corresponds to the court settling approximately one case every 90 minutes for 24 years.[8] Put another way, the adjudication settled one court case for every ten people in Idaho. During this process, water courts clarified individual property rights, reformed water law, and streamlined the institutions which manage water and approve water right transfers within the State.

In addition to interesting policy variation, this setting also allows me to compile a novel and comprehensive linked individual-level panel dataset of water rights, water right transactions, water consumption and crop choice. In particular, I observe the following data: Firstly, a comprehensive database of water rights, which I link to the land parcels on which they are used. Second, all transfers and trades of water rights within in the State before and after reform.[9] Third, a remotely-sensed measure of water consumption from evapotranspiration which I use to estimate the extent to which each right's water is applied to parcels of land. Fourthly, remotely-sensed crop classification data, which I use to estimate the value of the output farmers produce with their rights.

I identify the medium-run impacts of adjudication using panel variation in the timing of adjudication between sub-basins[10]. Figure 1.1 is a map of Idaho showing the boundaries

7. Only the total number of rights involved in the ongoing adjudication of the state of Montana is larger.

8. Supreme Court Justice Antony Scalia made this observation during remarks he gave at the conclusion of the adjudication (Vonde et al., 2016).

9. Two limitations of this data are that (1) I do not have an extensive pre-adjudication series for rights adjudicated early on and (2) I do not observe the price at which rights are traded.

10. the Snake River Basin was divided into 35 sub-basins, and the adjudication took place on a quasi-

of each sub-basin, which formed the basic unit for the adjudication process. In a panel-framework, estimates based on variation in the timing of adjudication between sub-basins are unbiased because there are parallel trends in observed outcomes between basins that are adjudicated earlier relative to later. Furthermore, because every sub-basin in the Snake River eventually becomes adjudicated, there is no selection into adjudication which might bias our estimates[11].

I present my empirical analysis in three parts: First, I test for reduced form evidence that adjudication leads to an increase in the rate at which water rights are traded. After adjudication, I find a 140 percent increase in the rate at which water rights are traded. This increase is robust to a variety of specifications.

Second, I use remotely sensed data to determine whether these paper transfers of water rights lead to real changes in water use. I find that on average, buyers of water rights hold land with higher productivity soil characteristics than their respective sellers. Furthermore, after a trade of water rights takes place, buyers of water rights expand their crop acreage relative to the sellers. This evidence suggests an aggregate increase in economic output after adjudication as a result of increased trading.[12]

Third, I estimate the value generated by the adjudication reforms. In the environmental economics literature, a hedonic approach is typically taken to estimating such a value, regressing a post-reform dummy on land values which are thought to capitalize all benefits of the reform[13]. I attempt this using data from the census of agriculture; however, the data is not sufficiently high resolution to draw strong conclusions. Instead, I propose a novel method

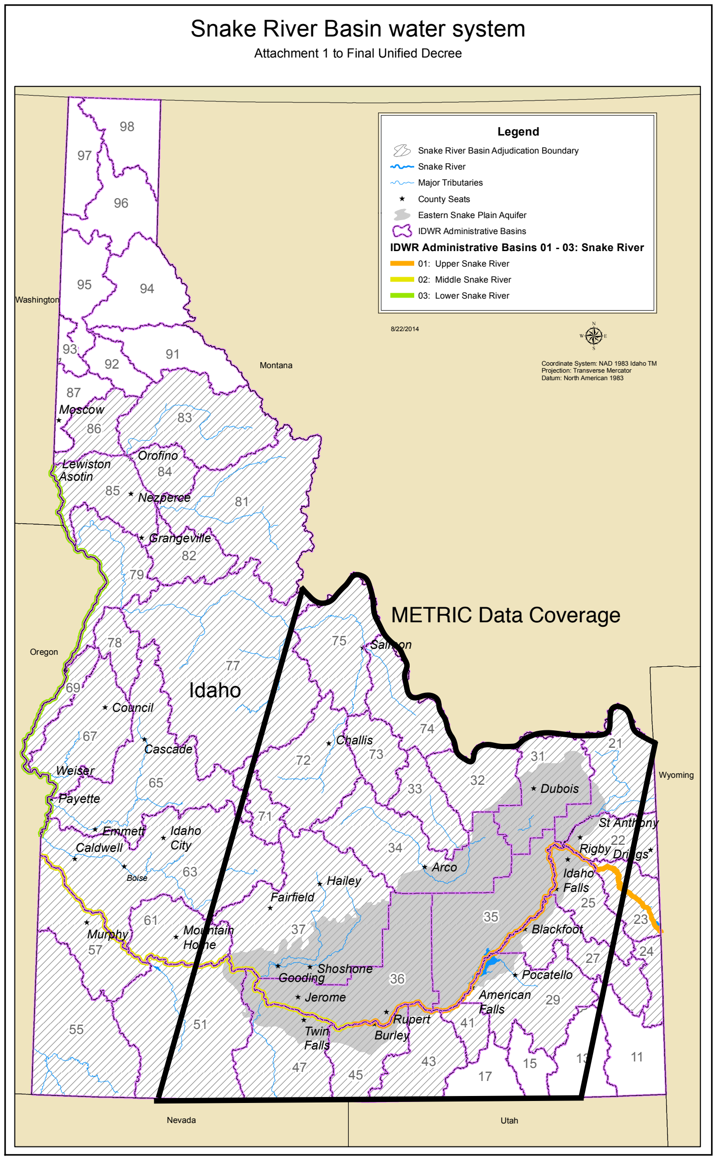
sequential sub-basin by sub-basin basis.

11. This is unlike most adjudications, which occur only locally and on a small scale. Estimating the value of such small-scale adjudications could lead to a selection effect if adjudication only occurs in basins which have significant water right disputes, and those basins are also the ones which have the highest returns to adjudication.

12. Although, I cannot detect a significant difference in water consumption after adjudication.

13. for example, Buck, Auffhammer and Sunding (2014).

Figure 1.1: Idaho's Administrative Sub-Basins



This map shows Idaho's administrative sub-basins, the unit at which adjudications took place. Coverage of METRIC evapo-transpiration data is outlined in thick black line. Source: Attachment 1 to Snake River Basin Adjudication Final Unified Decree *http://srba.idaho.gov/finaldecree.htm* to estimate the value created by adjudication; using a revealed preference framework based on farmer's crop choices. Based on the relative profitability of each choice, and given individual characteristics and time specific shocks, farmers choose either between planting different crops or leaving their land as pasture in order to maximize their profit. The adjudication reform changes the relative profitability of each choice. Estimating either (1.) a multinomial logit model (between the six largest crops grown in Idaho, or pastureland, or fallow-land) or (2.) a binomial logit model (cropland vs. pasture); I identify up to a scalar how the change in relative utility of planting each crop changes after adjudication. However, in order to interpret this change in dollar terms, I need to divide the coefficient through by a regression coefficient that is itself interpretable in dollar terms. In particular, I monetize my estimates using demand responses to exogenous changes in federal crop insurance prices. Although my point estimates are noisy, I find that adjudication increased the value of Idaho's annual agricultural output by $250 million. My results suggest that at least 20% of this effect can be explained by increased acreage among individuals who traded water rights, which is suggestive of realized gains from trade.

This paper contributes to the understanding of the economic impact of variations in property rights. There is a growing empirical literature which examines quasi-random variation in property rights generated from various sources, for example: political reform (Alston, Libecap and Mueller, 2000; Libecap and Lueck, 2011; Galiani and Schargrodsky, 2010), political connectedness (Goldstein and Udry, 2008), or technological change (Hornbeck, 2010). In particular, this paper measures how institutional changes reduce transaction costs, as theoretically motivated by Coase (1960) and Demsetz (1967). Recent authors have argued that this area is understudied within environmental economics (Libecap, 2016; Wang, 2007).[14]

In the context of water rights, a few recent papers have examined similar questions; Debaere and Li (2017) estimates the long-term impact of the Rio-Grande Adjudication us-

14. Some examples of applications within environmental economics are pollution markets (Stavins, 1995; Gangadharan, 2000) and fisheries (Costello, Gaines and Lynham, 2008; Grainger and Costello, 2014).

ing aggregate data from the Census of Agriculture. Ayres, Edwards and Libecap (2017) and Ayres, Meng and Plantinga (2017) study the role transaction costs play management of groundwater in California. Rimsaite (2017) assesses the efficiency of water right markets by comparing prices from medium-term leases to permanent transfers. Donna and Espín-Sánchez (2018) studies a historic Spanish water market which switched from a quota to a market system and argues that market failures arising from credit constraints made the quota system superior.

There is also a literature which seeks to measure the economic value of water. Often an hedonic framework is used to value water rights, for example, Mukherjee and Schwabe (2014) in California, Faux and Perry (1999) in Oregon, and Petrie and Taylor (2007) in Georgia, or Brent (2016) in Washington State. This paper instead uses a framework based on revealed preferences for farm production decisions. My approach is similar to Hansen, Lowe and Xu (2014) who infers the value of water by matching water rights to remotely=sensed crop choice data and Ji and Cobourn (2018), who estimates the value created by irrigation districts using a similar approach.

The rest of this paper is organized as follows; Section 1.2 gives further background on water rights and The Snake River Basin Adjudication. Sections 1.3 and 1.4 respectively describe the data and identification strategy. In Section 1.5, I show that adjudication increases the frequency of water right trading, and in Section 1.6, I demonstrate that this reflects a more valuable use of water. Section 1.7 uses a revealed preference method to estimate the value created by adjudication. Section 1.8 looks for evidence of aggregate impacts of adjudication in the Census of Agriculture. Section 1.9 concludes.

1.2 Background

1.2.1 What are Water Rights?

A water-right is a legal entitlement to divert and use water from a particular source - typically a lake, river or aquifer - under certain conditions. Water rights are 'usufructuary' rights; they are rights to use rather than own water. The legal owners of all water in the U.S.A. are the States, who have the directive to manage that water for the 'maximal benefit' of their citizens.[15]

In particular, In the Western U.S.A, surface water use is governed by the legal doctrine of 'Prior Appropriation'.[16] Historically under this doctrine, water rights are 'appropriative' in the sense that they arise from exercising water for beneficial use.[17,18] The 'Prior Appropriation' doctrine has two key components; first, individuals who divert water and put it to 'beneficial use' gain the right to continue using that water in the future, so long as they continue to make 'beneficial use' of it[19,20]. The second component is the 'priority' principle; during droughts or times when there is insufficient water to satisfy all competing demands, the first users who begin diverting water historically - the so-called 'senior appropriators' - have the right to use water before more 'junior' appropriators who began diverting more

15. However, Reserved tribal water rights preempt states water rights.

16. The Prior Appropriation Doctrine is followed in states West of the 100^{th} meridian running from Texas to North Dakota. In particular: California, Oregon, Washington, Idaho, Nevada, Montana, Wyoming, Colorado, New Mexico, Utah, Arizona, Texas, Oklahoma, Nebraska, South Dakota, North Dakota.

17. New diversions today in Idaho and most other states do require a permit before they are acknowledged, however historic rights are typically remain undocumented.

18. Whereas under the regulated versions of the 'Riparian' system in the eastern U.S.A water rights are created and granted only by regulation.

19. Initially the term 'beneficial use' was interpreted to include only economically productive uses of diverted water, such as mining, agriculture or residential consumption. However over time legal interpretation beneficial use has expanded to recognize storage, recreation, and environmental preservation as valid 'beneficial uses'.

20. However if a user of a water right fails to put its water right to beneficial use in a particular year, then they can lose that water right due to non-use.

recently.

The Prior Appropriation doctrine was created to incentivize investment in irrigation infrastructure, such as diversion canals and drainage ditches. It protects the water supply of the first individuals to build by giving them legal protection against reductions in flow caused by later diversions upstream which might reduce their supply, stranding the downstream investments. However today, this dynamic is unimportant because water rights are completely-allocated in almost all basins thorough-out the Western United States. As a result, prior appropriation instead generates a hierarchy of property rights which determines the order in which water is rationed during drought. Given this hierarchy, the efficiency of water allocation depends on the extent to which water rights matched with the parcels of land where they are most productive, conditional on the probability that a water right is realized in a particular year.

Legal enforcement of water rights varies by state, but typically if a rights holder does not have enough water to satisfy his 'right' in a particular year he can make a 'call' asking upstream 'junior' users to curtail this water use. Either he must make this call directly to the junior users himself and come to an agreement, or he places the 'call' to the state water department or a local water master who is in charge of enforcing priority. If upstream 'junior' rights do not respond by curtailing their water use, the 'call' can be enforced either through the State Courts or the State Water Department. However, enforcing a water right through litigation can often be a lengthy process which might not be sufficiently responsive to the immediate needs of irrigated crops during a growing season. This lack of enforcement leads farmers to engage in unproductive mitigating behaviour - such as choosing to grow less water intensive crops - which may reduce the value of having a system of water rights.

1.2.2 Why is it Difficult to Trade Water?

In principle, under the 'Prior Appropriation Doctrine' water rights are transferable between parcels of land and can be changed along with other dimensions such as ownership, nature of use or diversion point.[21] In principle, this could give rise to fungible 'markets for water', which would increase the efficiency of water use by allowing it to be moved to the parcels of land on which it is most productive. However, the trading of water rights is thin, a fact typically attributed to transaction costs.

This paper focuses specifically on transaction costs imposed by poorly defined property rights and state policies for approving trades, because these are the transaction costs which can be mitigated through adjudication. The state must approve all changes to water rights, and they often have a costly and time-consuming bureaucratic process to approve such transfers[22]. The State will want to verify the water rights before it permits a trade; this is costly in the absence of adjudication. Historical and legal evidence is needed firstly on when the water right was first established by diverting water and, secondly, of continuous beneficial use of the water-right since that date. Another key requirement for a water right trade to be approved is that it does not have adverse effects on any third-party. Establishing that there are no third party impacts from a proposed transaction is often challenging when water rights have not been adjudicated, and there is no systematic documentation of everybody's rights or who the potential third parties are. Not only is true for other water users whose rights are not known, but it also holds true for environmental flows which might not be quantified prior to adjudication.

21. Note that Agricultural Water Rights usually are transferred along with the land when a farm is bought or sold. In this paper, when I refer to water right 'transfer','trades' or 'leases' in this paper I am referring specifically to the case where the matching of a Water Right with the parcel of land on which it is used changes.

22. Even across 'Prior Appropriation' States there is significant heterogeneity in the laws, Szeptycki et al. (2015) gives a State-by-State review of water transfer and the degree to which they facilitate water markets. Some ditch companies and irrigation districts impose transfer restrictions water right transfers. Ghimire and Griffin (2014). In California some counties impose restrictions on transfers outside of the county Hanak (2003).

There are also other transaction costs in the water market that might not be affected by adjudication. For example, there is the cost of matching buyers with sellers, which typically happens via word of mouth networks and private brokers of water rights. As a result, there is potential for significant information asymmetry. There are also physical transaction costs, such as conveyance losses when transferring water from one place to another. As a result of these costs, it might still be impossible to reach an 'optimal allocation' even when property rights are very well defined.

1.2.3 What is a General Basin Adjudication?

In a 'General Basin Adjudication,' every claim to water use within a watershed is verified, the independence of each water right determined and a hierarchy of water rights established in a binding legal manner. When General Basin Adjudication became popular in the 1970's, it was thought that paying a one-time upfront cost to determine all water rights within a basin would be cheaper and easier than repeatedly resolving water right disputes on an ad-hoc basis, which were becoming increasingly common. A General Basin Adjudication involves the following steps: First the State creates a water court and appoints a judge responsible for overseeing the adjudication. Second, all water users are expected to submit their claimed water rights and supporting evidence to the court. Third, the court audits all claims and determines each individuals water right, including the quantity and nature of the water rights and the priority order with which their water is rationed during drought.

Aside from the Snake River Basin Adjudication described in this paper, several other large adjudications have taken place or have completed in the Western USA. In Texas, the 1971 Rio Grande Basin Adjudication was one the first recent large-scale adjudications to be completed. The Kalmath in Oregon, The Big Horn in Wyoming, and The Yakima in Washington are other notable examples, along with many smaller adjudications across all Prior Appropriation States. Adjudications focusing solely on groundwater basins are also common (Ayres, Edwards and Libecap, 2017; Ayres, Meng and Plantinga, 2017).

1.2.4 How did Adjudication Make it Easier to Trade Water in Idaho?

Adjudication could increase the efficiency of water use in the Snake River Basin for several reasons: Firstly, as a result of universal documentation of water rights, owners do not need to prove the validity of their claims to the State before they could trade them. This reduces the burden of trying to demonstrate continuous use of a particular water resource since the claimed first date of appropriation. Documentation also reduces the risk that the State rejects the legitimacy of their right, which would both deny their ability to trade now and also jeopardize the future validity of their right.

Second, during adjudication Idaho streamlined the process by which they approve transfers of water rights. For example, the burden of proof was previously on water right owners to demonstrate there were no adverse third party impacts before trading of water rights could occur. However, today in Idaho, The Idaho Department of Water Resources (IDWR) - who are now responsible for administering these right - can simulate proposed water right transfers through their State-sanctioned hydrological models to determine if there were any third party impacts. If they find there are adverse impacts to third parties, then these parties can be directly notified. Prior to adjudication there was a requirement that any proposed water right transfer had to be advertised in the local newspaper for 2 weeks before it could be approved. IDWR did not remove this requirement after adjudication; however, it is no longer the main primary way information is shared.

Thirdly, the litigation involved in the adjudication process led to the development of a substantial body of case law which clarifies the functioning of water rights, institutions which manage rationing during droughts, the condition under which water rights can be transferred and when they are approved.

Fourthly, adjudication led to the development of a more formal process for ensuring the prompt enforcement of water rights. The State took a more active role, appointing local water masters who were responsible for management of water rights within specific sub-basin. Idaho changed how they issued water calls - orders for upstream junior users to - using hydrological models to determine which users should curtail their water use. They also began using remotely sensed data on evapotranspiration to determine when curtailment orders were being violated.

Finally, Adjudication brought surface water and groundwater management together under a system of ‘conjunctive management’. This meant that both types of rights were managed under the same system of priority. If a groundwater users are reducing flow to senior downstream surface water rights, the State can order groundwater users to develop a plan to mitigate these impacts. This arrangement also opened up the possibility of trading water between groundwater and surface water users, an option that did not exist previously.

1.2.5 A Brief History of the Snake River Basin Adjudication

Idaho is a largely rural State, and agriculture constitutes an even larger share of water consumption than other western states (85% compared to 80% in California, Colorado or Utah). Consequently, the value of water in Idaho is not set by the opportunity cost of water use in a growing urban sector as is the case in California and Colorado.[23]

Geographically, 85% of Idaho’s land area and 90% of it’s water supply lie within a single watershed; the Snake River Basin. Idaho’s major groundwater fed agricultural region, The Upper Snake River Plain forms a fertile crescent across the southern third of Idaho.

The origins of the basin’s adjudication lie in the reduction in surface water flows in the lower Snake River as a result of groundwater pumping on the Upper Snake Plain.[24] During

23. Idaho law firm and water broker WestWater Research LLC. argues this is the case in their market reports (WestWater Research Inc, 2015)

24. Stapilus and The Idaho State Bar Water Law Section (2014) gives a very detailed background how the SRBA was organized. Vonde et al. (2016) discusses all of the legal issues that arose from adjudication.

the 1950's newly available groundwater technology enabled water to be pumped out of the Upper Snake Plain Aquifer; this led to a dramatic expansion of agricultural activity in the area. The expansion of agricultural activity reduced groundwater seepage downstream back into the snake river, which in turn reduced flows to downstream hydro-electric generators at Hell's Canyon owned by public utility Idaho Power. The utility's surface water rights were senior to upstream groundwater users since they arrived and began pumping water earlier. In 1977, a ratepayers interest group successfully sued Idaho Power arguing it had failed to protect the public interest by not enforcing it's senior water rights against upstream groundwater users with junior rights. In a 1983, the Idaho Supreme Court found that the senior water rights Idaho Power had not lost it's water rights due to non-use, and that it had a public duty to protect it's water rights in future years. As a result, Idaho Power was forced to file curtailment notices against every upstream groundwater user in the State, including over 7,500 farmers.

These curtailment notices were obviously politically unpopular, and highlighted the inadequacies of Idaho's water right management as this case would involve almost every water user in the middle and upper snake river for many of whom had not had their water rights documented by the State. A political solution was found in 1987 when the State settled the Swan Falls case; paying to buy out some of the Idaho Power's rights and also requiring that the entire Snake River Basin be adjudicated to ensure that a similar conflict could not happen again.

Over the subsequent 27 years, the State of Idaho would build a large bureaucracy to undertake the most complex water right adjudication every undertaken in the United States. Because of a limited bureaucratic capacity to process water right claims, the adjudication was broken up into 35 sub-basins. Three teams based out of 3 regional field offices in Boise, Idaho Falls and Twin Falls would each adjudicate one basin at a time in their local area. This differences in the timing of adjudication in otherwise nearby similar sub-basins is what forms the basis of my identification strategy in this paper.

Idaho began collecting the first water rights claims in 1988 in 3 test basins as part of the adjudication process. In 1992, the directors released reports containing their preliminary findings and recommendations were released for these first 3 test basins, and the teams moved onto the next set of basins. These preliminary findings were subject to multiple legal challenges which held up the adjudication process for several years as it worked through Idaho's court system. As a result, the first irrigation water rights were not officially decreed until 1999.[25] After a basin is adjudicated the judge issues a 'partial-decree' announcing the water rights in that basin, and these water rights are legally binding as of the date of the decree.[26,27] Almost all irrigation rights within Idaho were decreed between 1999 and 2012. The final unified decree of all water rights in the Snake River Basin was made in August 2014.[28]

1.3 Data

This section describes the different data sources used in this paper. This information is also summarized in Appendix Table A1.1. My analysis focuses primarily on the agricultural sector for two reasons: First, agriculture accounts for the majority of both Idaho's consumptive water use (85%) and water right trading (87% of transfers are between agricultural users).

25. Issues included litigation over the constitutionality of adjudication, the conditions under which a water right is forfeited due to non-use, the meaning of Idaho water rights ensuring water use in 'Public Trust', How water connections should be determined within the Snake River Basin and how Federal and Indian rights should be quantified.

26. This was explicit determined in a supreme court ruling prior to the first partial-decree and appears in the text of the final decree.

27. However many smaller 'De Minimis' Domestic and Stock-water rights were decreed much earlier than this. The vast majority of the water right decrees which will make up the empirical analysis in this paper occurred between 1999 and 2013.

28. Idaho has two regions outside of the Snake River Basin, although they constitute a small share of the State's water resources; Idaho's Northern Panhandle and the Bear River Basin near Utah. Adjudication of the Northern Idaho Basins began in 2008 and is ongoing, with the first preliminary directors reports issued in 2014. The Adjudication of the Bear River is expected to begin in 2017 after the conclusion of the Northern Idaho Adjudications.

The second reason is that individual level outcomes are easily observed within agriculture, both on how water is used (from crop classification and evapotranspiraiton data), and the likely productivity of water use (from soil and climate characteristics). I construct a 'land parcel by year' data panel by merging the soil and climate characteristics of each parcel with the water rights attached to each parcel, and each parcel's annual measured evapotranspiration and crop classification. The dataset contains the universe of parcels that are ever associated with a water right for the duration of the panel (from 1990 to 2016).[29]

I also construct a more aggregate panel of 'county by year' data which I use to assess the impact of adjudication on land value, and on-farm investment, variables I only observe at the county-level from the Census of Agriculture. [30]

1.3.1 Water Right Data

Of 162,000 water rights: 92% (139,000) lie within the River Snake River Basin, which becomes adjudicated during the period of the data. Water rights classified for agricultural water use make up 27% of rights by number (44,500) but 85% of volume.[31] Although a minority in number, agricultural rights account for majority of consumptive water use in Idaho. The vast majority of the remaining rights (104,000) rights are too small to be actively managed by IDWR an are used for either filling stock water ponds or domestic water supply. The rest of these water rights either allow reservoir storage for later use (4,500 rights),[32] or they

29. Potentially irrigable lands which have never been associated with a water right are omitted from my data, if anything this would lead me to understate the returns to adjudication.

30. Some outcome variables in my panel are incomplete due to differences in data availability across data-sets. The second panel in Figure A1.1 highlights which data sources are available in which years as adjudication progresses. Because data availability is differs across outcome variables, the identifying variation also differs across my different estimates.

31. Most water rights do not explicit specify their volume, so the volume of the right is instead estimated by multiplying the permitted flow rate of the water right by the length of the irrigation season.

32. Depending on whether the reservoir was build by the US Bureau of Reclamation (USBR) or the Army Corps of Engineers, the management of water might either be in the hands of the federal government or a local water master, typically these institutions manage this local water use.

have a variety of other large water uses such as municipal, commercial, power generation recreation, environmental uses (4,500 rights) .

I also observe every application made to The Idaho Department pf Water Resources (IDWR) to modify a water right, and whether that application was approved. I observe this data between 1999 and 2016.[33,34] Although water rights can be modified in a number of ways,[35] in this paper I shall restrict my definition of *water right trades* or *transfers* to only include approved applications to change the *place of use* of a water right from one parcel of land to another. This allows me to follow water as trades move it between parcels and to ask whether adjudication leads water rights became more frequently matched to more productive parcels of land[36].

Table 1.1 contains summary statistics for the water rights in my dataset. This table has a several key takeaways: Firstly the distribution of the volume of water rights is right skewed. There is a very small number of water rights account for a large share of the volume of water. This is true both between water uses and within irrigation water rights. 42% of irrigation rights have a groundwater source, however a much larger share of water right transfers involve irrigation water rights. On average a water right irrigates around 191 acres of land. Traded water rights tend to be far larger than average. Approximately 18% of irrigation water rights are ever traded.

33. applications and approvals exist prior to these dates, and that data would strength my analysis. However IDWR had not digitized their records prior to this date.

34. Although I observe when transactions take place and how they move water, I do not observe prices paid.

35. Water rights can be modified in 5 ways: 1) change of place of use, 2) change of owner, 3) change / add point of diversion, 4) change of nature of use 5) change period of use.

36. I do not count changes in the ownership of water rights that result from sale of the underlying land. I also do include applications to move water rights from one parcel of land to another even if the owner of the water right does not change.

Table 1.1: Summary Statistics - Water-Right Data

	Water rights		Transaction data
	All rights	Irrigation rights	Traded rights
Observations	163,259	46,838	24,938
Median priority date	1952	1952	1959
Median adjudication date	2000	2007	2002
Groundwater source	54%	42%	67%
Irrigation use	27%	100%	87%
Within Snake River Basin	92%	89%	97%
Adjudicated prior to 2015	87%	82%	93%
Median volume (cfs)	0.04	0.49	0.99
Max permitted irrigation acres		191	479
Water right ever traded	6%	18%	100%
Median transaction year			2009
Adjudicated at time of transaction			82%
Within basin transactions			97.8%

This table includes all known water rights in both adjudicated and unadjudicated basins. However, a small number of omitted rights might exist that have never been documented in unadjudicated basins. Priority denotes the year water was first diverted creating the legal right; this determines the order of rationing in shortages. The adjudication date is the date on which the water right was adjudicated. In calculating median adjudication date, unadjudicated rights are imputed as $+\infty$. In all other calculations missing data is omitted, these omissions typically only occur in not-yet-adjudicated rights which were historically incompletely documented. The median volume of a water right is implicitly defined by its flow rate - measured in cubic feet per second - since total volume is often not quantified.

1.3.2 Crop Data Layer

To infer what crops are being grown on irrigated lands, I use data from USDA's Cropland Data Layer (CDL) dataset.[37] This dataset is generated from Landsat satellite data which classifies patterns of land use, particularly among agricultural crops. The CDL dataset is measured at a 30x30m resolution throughout the United States. In Idaho, complete CDL data is available between the years 2007 and 2015.[38] By matching the shape file of each

37. Available at: *https://nassgeodata.gmu.edu/CropScape/*

38. In 2005 only partial data is available but this data covers all of the major agricultural areas in the Snake River Basin.

water right with the crop data layer, I identify land within each parcel allocated to six major crops of the region: alfalfa, barley, corn, potato, sugarbeet and wheat, or other crops, fallow and pasture land. I remove all non-agricultural land from my sample.

Table 1.2 contains summary statistics for the largest agricultural land uses in by acreage. These can conceptually be broken down in three groups: 1.) high-value, water-intensive crops, such as potatoes, corn, sugarbeet and a number of smaller specialty crops. 2.) Medium-value crops which are either more drought-resistant (wheat and barley) or flexible in their water requirements (alfalfa). 3.) Finally there is a significant amount low value uses such as growing pasture and fallowed land.

The first row of Table 1.2 shows the total acreage in agricultural sector in 2007 as calculated from the CDL. Almost half the acreage is in pasture. Medium value crops (alfalfa, wheat and barley) together constitute another 33.7%, while high value crops only make up 12.8% of cropland.

Second row shows the market value of each different crop. This is calculated from USDA's quick-stat database, and shows the average price received for each crop from the USDA's quick-stat database in 2007. Low value uses are worth less than $300 per acre. Medium are worth between $300-$600 per acre. High value uses are worth greater than $1000 per acre. However, these prices cannot be interpreted as the marginal product of water on a given piece of land because these values do not reflect the costs paid for other unobserved inputs into production. If this were the case we would expect to see almost the entire state in planted in the highest value crops, either potatoes or specialty crops such as hops. However because of the unobserved factors in production, when I estimate the value created by adjudication I prefer to rely on a revealed preference approach rather than market prices.

Table 1.2: Summary Statistics - Crop Acreage in 2010

	Low Value Uses		Medium Value Crops			High Value Crops			
	Fallow	Pasture	Alfalfa	Wheat	Barley	Potatoes	Corn	Sugarbeets	Other
Area - (000 Acres)	488	4,908	1,575	1,218	621	323	388	168	419
(% of total)	(4.83%)	(48.5%)	(15.6%)	(12.0%)	(6.1%)	(3.2%)	(3.8%)	(1.6%)	(4.2%)
Avg. market value of crops ($/acre)	$0	$242	$567	$374	$555	$2,859	$1,032	$1,849	$47,000
Avg. evapotranspiration rate (mm/day)	1.25	2.47	3.71	3.83	3.43	3.62	3.76	4.4	3.4
(std. dev.)	(0.58)	(0.90)	(0.80)	(0.60)	(0.67)	(0.42)	(0.43)	(0.50)	(0.67)
Insurance price ($/acre)	.	.	$19.2	$11.4	$6.5	$37.7	$7.6	$10.2	$29.4
(share insured)			(2%)	(67%)	(59%)	(70%)	(31%)	(70%)	(36%)

Acreage calculated from Crop Data Layer. Market value estimates from USDA QuickStat ('other' crop value is for dry beans only). Average evapotranspiration is calculated by matching Metric evapotranspiration data with Crop Data Layer classifications. Insurance data is taken from USDA-RMA Summary of Business reports. Insurance price is for baseline 65% revenue insurance. Insurance shares are calculated over all insurance policies by comparing insured acres in Summary of Business to total acreage in Crop Data Layer. In descending order, the ten next largest crops that constitute the majority of the 'other' column are: dry beans, peas, lentils, safflower, canola, triticale, oats, herbs, onions, and mustard.

1.3.3 Metric Evapotranspiration Data

I use evapotranspiration (ET) data data from the METRIC dataset to estimate how patterns of water use change after adjudication. METRIC was developed in partnership with IDWR by Allen, Tasumi and Trezza (2007) as a tool to help the state manage Idaho's water. Metric uses surface energy balance algorithm to estimate the mass is lost to evaporation over every parcel of land. The data is calibrated to a series of ground based meters to ensure accuracy. Developed in Idaho the tool has been applied in many other regions.

METRIC data is considered sufficiently credible that it has been use by IDWR for many administrative applications. For example, in 2009 Idaho courts accepted METRIC data a "legal finding of fact" that A&B irrigation district had been illegally using water after a call had been made requiring them to curtail their use. A finding which subsequently upheld in the Idaho supreme court. METRIC data was also used to calibrate the aquifer models which quantify relations between surface and groundwater in the Upper Snake Plain Aquifer.

The third row of Table 1.2 calculates the average ET for each crop. This is calculated by matching METRIC data with the CDL data and shows the average levels of evapotranspiration for each crop. I find a significant difference in the rate of ET observed between Pasture (2.7 mm/day) and the other crops (from 3.4 to 4.4 mm/day). However the difference in ET rates within medium - high value crops is much smaller (1-2mm per day).[39]

1.3.4 Soil and Climate Data

Soil data is obtained from the SSURGO database, a soil database developed by USDA-NRCS. I also include common soil quality indicators in our model, such as irrigated and non-irrigated soil capacity class, percent of clay, percent of slopes, and k-factor [40].

39. Notably, scientific crop studies, such as Allen and Robison (2007), often find larger ET differences between these crops in Idaho than I measure here.

40. SSURGO also contains a much larger set of interesting variables related to soil quality, unfortunately many of these have highly incomplete coverage throughout Idaho, and so I have omitted them from my

Weather data is obtained from the PRISM climate dataset developed by Oregon State University, which provide small-scale climate maps and estimates. From this dataset I get daily estimates of precipitation and temperature at a high resolution for all years of our sample.

1.4 Identification

This paper estimates the impact the adjudication of the Snake River Basin had on various economic outcomes. I identify this effect using the differences in the timing of adjudication between sub-basins within Idaho. Figure 1.2 highlights this variation in repeated maps, each showing the share of each sub-basin adjudicated at different points of time.

My goal is to estimate the impact (δ) of a change in adjudication status ($\text{Post\_Adjudication}_{i,t}$) in parcel i at time t on some outcome of interest, $y_{i,t}$. This outcome might be whether or not a water right is traded, whether or not a farmer is growing a particular crop, or measured rates of evapotranspiration.

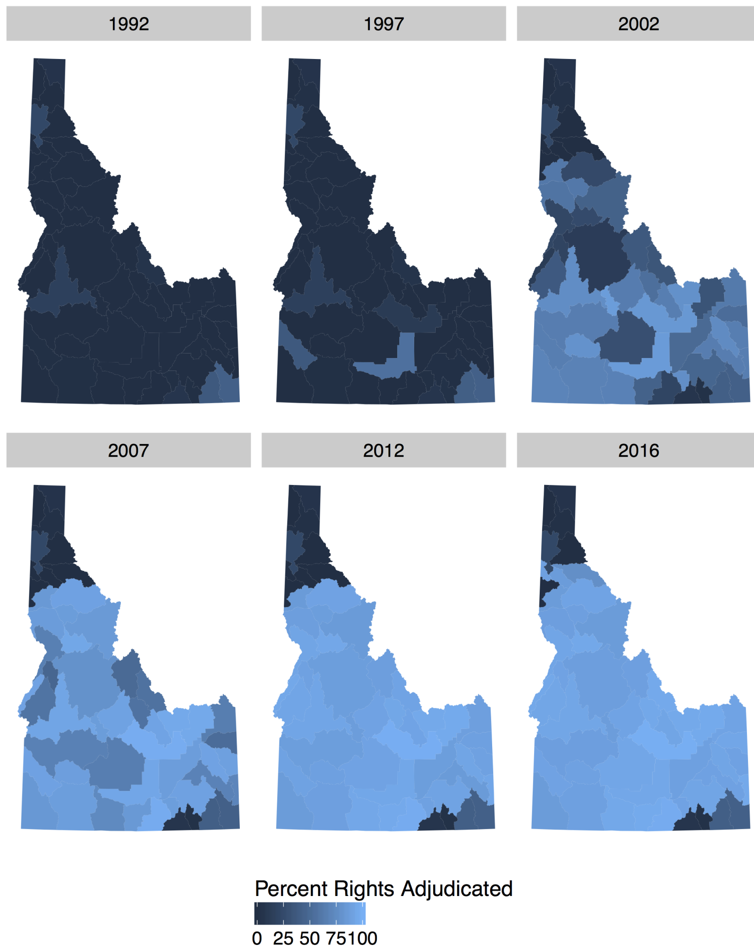
In the baseline, I estimate this in a panel structure using both fixed effects for each parcel of land $\gamma_{\text{water\_right}}$ and fixed effects for each year of time γ_{year}. In this specification the impacts of adjudication is learned by comparing the relative changes in outcomes between parcels that changed in adjudication status from unadjudicated ($\text{Post\_Adjudication}_{i,t} = 0$) to adjudicated ($\text{Post\_Adjudication}_{i,t} = 1$), with changes in other similar parcels whose adjudication status did not change at that time. In this case, the regression equation will be:

$$y_{i,t} = \delta \text{Post\_Adjudication}_{i,t} + \gamma_{\text{year}} + \gamma_{\text{water\_right}} + \varepsilon_{it}$$

In some specifications, I replace either the individual or year fixed effects with observable

analysis.

Figure 1.2: Spatial Heterogeneity of Adjudication Status



This figure shows the progress of the Snake River Basin Adjudication. In each figure, the color of each sub-basin shows the share of water rights that had been adjudicated before or during that year. This spatial variation in the patterns of adjudication identifies the estimates in this paper. Spatial heterogeneity in adjudication status suggests that adjudication status might not systematically vary with omitted spatial variables. By 2012, almost all water rights in the Snake River Basin have been adjudicated. Additional sub-basins in the Northern Panhandle become adjudicated by 2016.

characteristics of individual water rights rather than fixed effects $X_{\text{water\_right}}$. In this case, I estimate:

$$y_{it} = \delta \text{Post\_Adjudication}_{it} + X_{water\_right}\beta^{1} + \gamma_{\text{sub\_basin}} + \gamma_{\text{year}} + \varepsilon_{it}$$

In these regressions, identification comes from relative changes over time in the rate at which water rights are traded between water rights that get adjudicated relatively sooner, and those that adjudicated relatively later.

A sufficient, but not necessary condition for these regressions to produce an unbiased estimates is for the timing of adjudication to be as-though it was randomly assigned. To test if this is the case, I break the water rights into four groups based on the order in which they were adjudicated and plot the average characteristics of each group. The results are shown in Table A1.2. Unfortunately, the table shows large differences in observable characteristics between groups. However, these differences do not appear to have a discernible trend from early or late adjudication.

The levels of these differences are not important for identification, identification in a panel setting relies on an assumption of parallel trends. In Figure 1.3, I test the parallel trends assumption using data on evapotranspiration and cropping patterns. For evapotranspiration rates, I have individual level pre-adjudication data in the years 1985, 86, 92 and 96. I sort parcels into four groups based on the adjudication date of individual water rights and then plot the trends in evapotranspiration rates. For cropping data, I have a longer series of annual data, but this data is only at the county level. I sort counties into four groups based on the median adjudication date of water rights within that county, and then plot the market shares of the five largest crops. The trends in these figures provide suggestive evidence in favor of the parallel trends assumption. In principle, a similar test could assess whether the parallel trend assumption holds in the frequency of water trading; but unfortunately, I only observe trading data after 1999, making such a test impossible.[41]

If the parallel trends assumption holds, then the order in which basins were adjudicated is unimportant, and we do not need to worry about weather there was selection into the order that basins were adjudicated. However, if the parallel trends assumption is violated, then selection on the timing of adjudication will bias our estimated average treatment effect and the direction of this bias will depend on how that selection occurred.

For example, IDWR intentionally selected the first three basins to be adjudicated for their complex legal disputes; the State reasoned that by adjudicating the most challenging cases first, that they could develop the case law necessary to allow the remainder of the adjudication to proceed quickly. If we believed the presence of these complex disputes implies that water right trading in the first three sub-basins would have grown at a faster than in sub-basins in the rest of the state, then we would conclude that including the first three basins would lead us to overestimate our treatment effects. To allay this fear, I estimate a specification of my results on the rate of water right trading omitting these basins and I find identical results.

For the remainder of the basins, the order of adjudication was arbitrary.[42] IDWR split the remaining sub-basins into three contiguous groups, and assigned three teams each based out of a regional IDWR office to collect adjudication claims sequentially sub-basin by sub-basin.

There is some heterogeneity in the timing of adjudication within basins. Disputed might take longer to reach a final decree than those that are not. This could bias my results; if the rate at which disputed water rights trade would have grown faster than those not disputed, then I would underestimate the treatment effect. To address this concern one of

41. Trading and trading data does exist before this date. However it has not been digitized, and I have been unable to obtain copies from IDWR.

42. Anecdotally this appears to be the case based on the oral history of Stapilus and The Idaho State Bar Water Law Section (2014).

Figure 1.3: Pre-Trends in County Acreage by Adjudication Timing

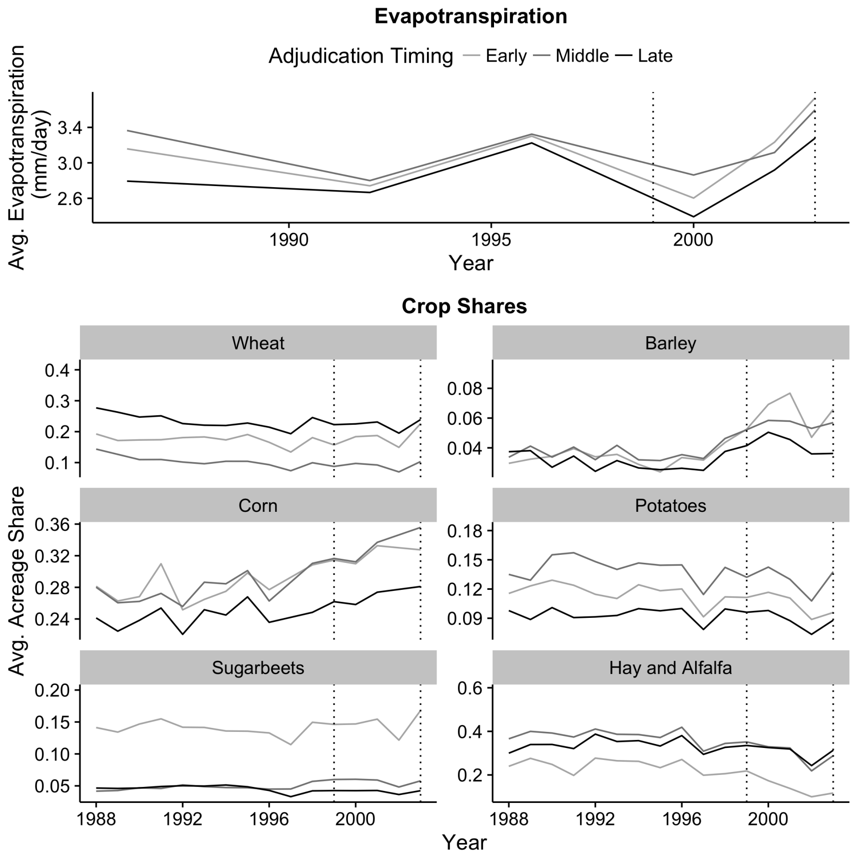


Figure notes:

- This figure tests the parallel pre-trends assumption necessary to draw inference from panel data.
- ‘Early’ basins were adjudicated between 1999-2002, ‘Middle’ basins between 2003-2006, and ‘Late’ basins between 2006-2015. The dashed lines denote the beginning of the ‘Early’ and ‘Middle’ adjudication periods.
- The top panel shows parallel trends in METRIC Evapotranspiration Rates between ‘Early’, ‘Middle’ and ‘Late’ adjudicated basins. This series is based of matching observations from individual parcels.
- The second panel shows parallel trends for crop shares for the 6 largest crops from the USDA quick-stat database. Because Hay and Alfalfa are classified differently to in Cropscape data, these numbers are not necessarily consistent with the data later in the paper. Furthermore this data is at the county level, and counties are classified into ‘Early’,‘Middle’ or ‘Late’ adjudication based on the median adjudication date in the county.

my specifications estimates the impact of adjudication using the earliest release date of the preliminary sub-basin reports as the treatment date, and I find the same results as in my other specifications.

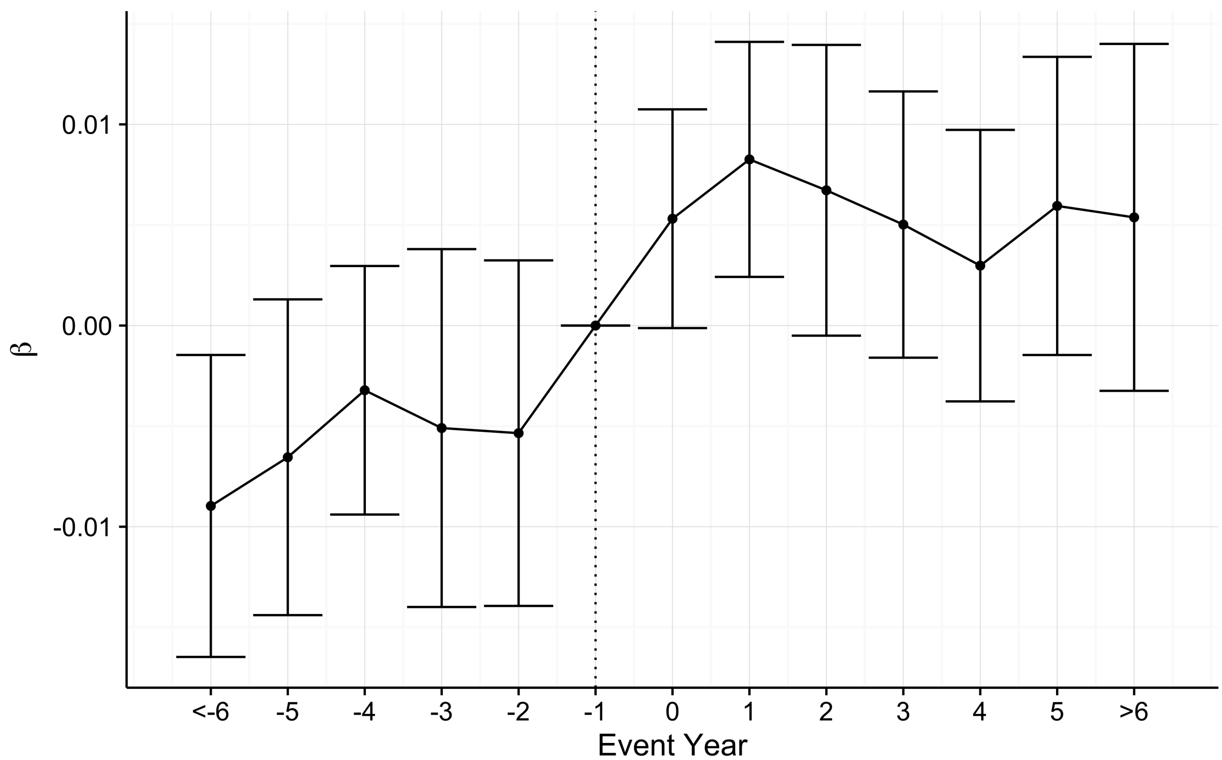
1.5 Impact of Adjudication on Trading

This section presents my primary piece of evidence that adjudication reduces transaction costs; I estimate the impact that a sub-basin becoming adjudicated has on the rate at which water rights are traded within that sub-basin. I show that after adjudication the probability that a water right is traded in a particular year increases by one percentage point.

Figure 1.4 is an event study figure. It shows the coefficients of an OLS regression, where the left-hand side is a binary variable that indicates whether a water right was traded in a particular year, and the right-hand side is a set of binary variables representing the number of years before or after the right was formally adjudicated by a court. The regression also controls for time-specific effects with year dummies, and for differences in the propensity of different farms to buy or sell water rights with farm-specific fixed effects. I restrict the sample to the years in which I observe whether water rights are traded for at least six years before and after adjudication. The standard errors are clustering two ways at the adjudication sub-basin and year levels. This conservative approach accounts for the fact that our identifying instrument (adjudications by the State) varies on a basin-by-basin basis and for the fact that there might be common factors affecting the productivity of farms within each basin or each year.

The takeaway from Figure 1.4 is that water rights are traded at a persistently higher rate after adjudication compared to baseline before. This result is consistent with adjudication reducing transaction costs. The estimated change in the coefficients β post-adjudication compared to before is around 0.01. Interpreting this as a linear probability implies that a

Figure 1.4: Event Study - The Impact of Adjudication on the Rate of Trading



This figure shows how the rate at which water rights are traded changes after water rights are legally adjudicated. It plots the coefficients of the following 'event-study' regression :

$$y_{it} = \delta_t \text{Years After Adjudication}_{it} + \gamma_{\text{year}} + \gamma_{\text{water right}} + \varepsilon_{it}$$

The sample is restricted to data with at least six years of observations before and after adjudication. The omitted factor in the regression is $t = -1$, the year before adjudication. Standard errors are robust and clustered two ways at the Basin and Year level. Confidence intervals are at the 95% level.

water right is about 1% more likely to be traded in years post-adjudication[43]. Although standard error bands overlap in this figure, the change in level after adjudication is statistically significant when year dummies are pooled into a post-adjudication dummy as we see below.

Table 1.3 presents six different specifications for regressions estimating the impact of adjudication on water right trading[44]. Each column estimates that specifications on three different sub-samples of the data: 1.) All water rights in Idaho, 2.) All irrigation water rights in Idaho and 3.) The subset of water rights for which I have matching evapotranspiration data. This third sub-sample is included to match the cross-section of rights used in the later analysis of evapotranspiration data and crop choice. My results are consistent across the three sub-samples, the numbers quoted in the text below are from the second column, which includes all irrigation rights.

Specification 1 is my preferred specification, a standard panel regression with individual and time specific fixed effects. The estimated coefficient of 0.010 (s.e 0.003) consistent with the Figure 1.4 discussed above. Given our preferred estimate of $\beta = 0.01$, this would imply that the rate at which water rights are traded increases by 140%, from a baseline of 0.7% of all water rights being traded each year to 1.7% of all water rights. This estimate implies 443 additional water right transfers each year among agricultural users as a result of adjudication. Although only a small percentage of water rights change hands every year, over time this compounds such that 18% of water rights are traded between 1999 and 2016.

Specification 2 addresses a concern that time specific fixed effects might be too conservative. In particular, specification 1 does not capture aspects the adjudication process which affects all water rights holders simultaneously outside of basin-specific adjudications.

43. the linear probability interpretation can be theoretically problematic (for example it might predict negative probabilities). However, these results are robust to employing a logistic specification. Later in the paper, we use logistic regression on crop choice to avoid similar problems.

44. Note that Appendix Table A1.3 reproduces the event study Table 1.4 for each of the alternative specifications and samples discussed below.

Table 1.3: Impact of Adjudication on Water-Right Trading

	Sample: All Idaho Water Rights	All Irrigation Water Rights	Evapotranspiration Sample Only
	Dependent Variable: Water Right Traded in Year		
	Specification 1: Year FE & Water Right FE		
Post Adjudication	0.010***	0.010***	0.008***
	(0.003)	(0.003)	(0.003)
R^2	0.089	0.086	0.071
	Specification 2: Year Cubic + Crop Price Series + Weather Series & Water Right FE		
Post Adjudication	0.013***	0.013***	0.009***
	(0.003)	(0.004)	(0.003)
R^2	0.099	0.094	0.078
	Specification 3: Year FE & Basin FE + Soil Controls + Climate Controls		
Post Adjudication		0.010***	0.007***
		(0.003)	(0.002)
R^2		0.013	0.005
	Specification 4: Weighted by Water Right Volume (Year FE & Water Right FE)		
Post Adjudication	0.018	0.058	0.012***
	(0.013)	(0.039)	(0.005)
R^2	0.161	0.172	0.100
	Specification 5: Weighted By Acreage Irrigated (Year FE & Water Right FE)		
Post Adjudication		0.050	0.013***
		(0.034)	(0.005)
R^2		0.097	0.013
	Specification 6: Alternative Adjudication Dates (Year FE & Water Right FE)		
Post Adjudication	0.009***	0.006*	0.007***
	(0.002)	(0.004)	(0.002)
R^2	0.089	0.086	0.071
N	165104	44353	10136
T	17	17	17
Observations	2,806,768	754,001	172,312
Note:			*p<0.1; **p<0.05; ***p<0.01

This table presents different specifications of the following regression:

$$y_{i,t} = \delta \text{Post\_Adjudication}_{i,t} + X_i\beta_i + X_t\beta_t + \varepsilon_{i,t}$$

Where $y_{i,t}$, a dummy variable indicating where a water right is traded in a particular year is regressed on $\text{post\_adjudication}_{i,t}$, another dummy variable indicating whether the water right has been adjudicated.
The three columns correspond to different, increasingly narrow samples. The first column contains all water rights. The second contains only water rights which are specified for irrigation use. The third contains only irrigation water rights for which we have matched evapotranspiration data (see Figure 1 for region).

- Specification (1) is a standard panel regression with individual and year fixed effects.
- Specification (2) accounts for components of the treatment effect which might be co-linear with year-fixed effects, such as changes in policy which affect the entire state simultaneously. This specification includes a polynomial trend in time as well as controls for average annual crop prices (for potato, wheat, and corn) and weather controls (annual precipitation and average temperature).
- Specification (3) accounts for the fact that because most water rights are never traded, the panel specification may be over-controlled. It replaces individual fixed effects with basin fixed effects and controls for the irrigation capacity class of the soil.
- Specification (4) weighs water rights by their permitted volume rather than weighing each right equally.
- Specification (5) weighs water rights by the acreage those rights are permitted to irrigate.
- Specification (6) replaces the adjudication date with the release date of preliminary basin reports. These reports were the first signal water right claimants received about the likely outcome of adjudication. On average the preliminary basin report date was released 1.3 years before the adjudicated water right was formally decreed.

Robust standard errors, clustered two-ways at the basin and year level are presented in parentheses.

For example during adjudication, the supreme court made a number of significant decisions which developed substantial case law about how water rights should operate[45]. IDWR also made significant changes in the process of water right transfers which affected all basins simultaneously. Arguably, these effects reflect as much of the benefits of adjudication as the impact when a particular basin's rights were signed off as adjudicated. However, in Specification 1 these effects will be captured in year specific fixed effects rather than the estimated coefficient on adjudication. Specification 2 corrects by replacing basin specific fixed effects with a cubic polynomial time trend in calendar year and series controls for crop prices and weather realizations. This specification results in substantially larger estimates - 1.3% vs. 1.0% - suggesting that up to up to a quarter of the estimated impact of adjudication could be coming from these simultaneous effects of the policy.

Specification 3 addresses a concern that individual specific fixed effects might throw out too much information. The probability that a water right is traded in a particular year is low, and most water rights are never traded. Because of this most of the fixed effects in my estimate will be equal to zero, significantly narrowing the identifying variation. This specification addresses this concern by replacing individual-specific fixed effects with basin-specific fixed effects and also controls for parcel soil and climate characteristics to control for differences within basins. The estimates are effectively the same as specification 1.

Specifications 4 and 5 address a concern that the relevant unit of observation in this exercise is not the individual water right but rather the volume of water traded, or the acres of land that traded water irrigates. These specifications re-estimate specification 1 where each observation is weighted by its volume or irrigated acreage. Because the distribution of water right volumes and acreage is strongly left-skewed, these estimates have much larger

45. Vonde et al. (2016) has an extensive discussion of all of the Basin-wide Idaho Court rulings relating to the Snake River Basin Adjudication which developed substantial case law about how water was to be managed affecting all water users simultaneously. Issues included: what volume of rights can be transferred, when adversely affected third-parties can object to a transfer, the conditions under which a right is forfeited for non-use and whether private parties can own in-stream water rights, how storage is administered and many other things.

standard errors, and consequently are not statistically significant in some specifications. However, the estimate treatment effects are larger than estimated in specification 1 (0.05 vs 0.01) and consistent with my previous results. The larger point estimates might reflect that if transaction costs are fixed per transaction and don't vary with volume, then it is easier for holders of relatively larger water rights to overcome this fixed cost, since they will face a smaller fixed cost per unit volume.

Specification 6 addresses a concern that the 'treatment date' might be incorrectly specified. In Figure 1.4 it appears that half the treatment effect occurs at the year before adjudication $t = -1$, rather than the year of adjudication $t = 0$. This might occur because during adjudication, water courts released their preliminary sub-basin report - which detailed proposed allocations of water rights - approximately a year before the final allocations are officially decreed by a judge as having been 'adjudicated'. Although some water users disputed the results of the preliminary adjudication, the vast majority of preliminary findings are not disputed and were eventually confirmed as water rights. If the preliminary report resolved the uncertainty around the vast majority of water rights, and this is the mechanism which increases trading, then we might expect the jump in transfers to occur in $t = -1$ rather than at $t = 0$. Specification 6 replaces the 'adjudication date' which appears on each water right with the date on which the preliminary basin report was released in each basin. Although the pooled estimates are somewhat smaller under this approach (0.006 vs. 0.01) which reflects the fact there are more unadjudicated rights in the post-period, the results are again consistent with previous findings.

Finally, Figure A1.2 addresses one last concern about my baseline estimate: That there may be manipulation around the treatment date. Because adjudication is long anticipated, firms may choose to defer their planned water right trades until after adjudication has taken place. If this were to happen, we would expect to observe that there is a spike in the rate at which water rights are traded immediately after adjudication and that this subsequently dies down and the rate at which rate rates are traded later returns to the rate at which it was previously. Because of the limited length of my panel, it is difficult to address this concern. One approach is to limit my sample to the years where I only have at least ten years of post-adjudication data and reproduce the event study plot.

1.6 Impact of Trading on Farm Outcomes

The previous section established that after adjudication water rights are traded at an increased rate. This section shows that these water right transfers are associated with more productive use of water. In particular, I show that: 1.) transfers move water rights to parcels of land with on average more productive soil and climate characteristics, 2.) parcels of land which bought water rights observe increased evapotranspiration after adjudication, and 3.) both after adjudication generally, and after water right transfers specifically, cropping patterns change towards increased acreage and higher value crops.

These findings demonstrate that adjudication is not just a 'paper phenomenon', only generating work for lawyers and that transfers occur only on paper, but instead that it had real-world effects on the decisions farmers and irrigation districts make, which are reflected in actual changes in water use.

1.6.1 Water Right Buyers have More Productive Land

Table 1.4 shows the average differences in parcel characteristics between buyers and sellers of water rights. Transfers of water rights typically move water within the same basin, and over relatively short distances - the average distance between the parcel of a seller and buyer is only 8 miles. Because of this average differences are small; however, the fact that such differences exist and are statistically significant suggests that the process of water right transfers is systematically moving water to pieces of land where it will be more productive.

On average the buyers of water rights live on parcels of land which are warmer and have higher average precipitation. They have a lower elevation, implying that on average water rights move water downstream.[46] Buyers of water rights have a higher k-factor (and so are more resistant to erosion) and a higher clay percentage. The land is flatter and has more frost-free days.

The bottom three rows of Table 1.4 use USGS estimates of the agricultural potential of each parcel of land for a given crop under both irrigated and non-irrigated agriculture. I merge this data with the average price of different crops in the base year (2000) to estimate the average expected per acre revenue for each parcel of land. Under this measure, the parcels of land who buy water rights are $34 per acre more productive when irrigated. Buyers and sellers land are equally productive when not irrigated. These results suggest that transactions are moving water from parcels of land with comparative advantage in non-irrigated agriculture to parcels of land with comparative advantage in irrigated agriculture.[47]

1.6.2 Impact of Adjudication and Trading on Evapotranspiration Rates

Table 1.5 shows the impact of adjudication and water right transfers on patterns of satellite evapotranspiration.[48] Specifications (1) and (3) are regressions of post-adjudication dummies on evapotranspiration rates, using a similar empirical framework to Table 1.3. The estimates are not significant, reflecting the fact that total water use did not change after adjudication. This is expected because water rights were already exhaustively allocated before adjudication, and adjudication neither created nor destroyed additional water.

Specifications (2) and (4) show the impact that individuals buying or selling water rights has on measured evapotranspiration rates on that farmer's land. I estimate a 'within' linear

46. Unlike upstream transfers, downstream transfers will not reduce in-stream flow between the buyer and seller, making third-party objections less likely.

47. Of course, just because water moves to parcels with more productive characteristics does not mean water is more productively used.

48. The estimates I find are quite noisy, perhaps reflecting significant spatial smoothing which might go into the construction of the evapotranspiration data.

Table 1.4: Soil and Climate Differences Between Water-Right Buyers and Sellers

Outcome	Difference	C.I
Distance		
Average Annual Air Temp	0.0753	(0.0169,0.134)
Average Annual Precipitation	0.459	(-1.11,2.02)
Elevation	-7.43	(-13.8,-1.08)
K-factor	.085	(0.0215,0.149)
Clay Percentage	3.25	(2.19,4.28)
Frost Free Days	1.06	(0.515,1.61)
Land Slope	0.0564	(-0.274,0.387)
Expected Rev/Acre Irrigated	34.2	(4.98,63.4)
Expected Rev/Acre Non-Irrigated	-2.05	(-5.39,1.29)
Expected ValueAdded/Acre by Irrigation	34.6	(5.68,63.6)

This table shows the acreage weighted average differences for each water right transaction between the soil characteristics of the water right buyer and seller. Soil quality data is from the USGS. Confidence Interval is from a one-sided paired t-test of hypothesis that the difference is equal to zero. Expected Revenue is calculated from the USGS Soil Productivity Estimates for that parcel's most productive crop multiplied by that years market price.

regression on 'Post-Buying Water Right' and 'Post Selling Water Right' dummies for buyers and sellers of water rights respectively. The estimates are small and not statistically significant, implying that I cannot rule out that there was no change in evapotranspiration after users trade water rights. However, the signs of the point estimates in specification (4) are consistent with buyers of water rights using more water after adjudication and sellers using less.

The standard errors are sufficiently small to rule out massive changes to cropping patterns after adjudication. For example, these estimates are inconsistent with all buyers of water rights converting their entire parcels from low-value pasture or fallow land before buying rights to medium-high value crops after, because this would require an average increase evapotranspiration rates of between 1 to 3 mm/day. The reverse change can also be ruled out for sellers of water rights.

However, these estimates are still consistent with farmers making marginal changes in the share of land allocated to pasture vs. crops or in substitutions between different high and medium value crops after adjudication. For example, these estimates would be consistent with buyers of water rights substituting up to 10% of their acreage from pasture to higher value crops - a result in the next section.

The second panel of Table 1.5 multiplies through the estimated treatment effects by the volumes of the water rights associated with each parcel. The total volumes traded per year are relatively small in these estimates.

One way to assess the magnitude of these estimates is to compare them to the total volume of water rights transferred. The total volume of water rights involved in trades is approximately 1,461,000 AF / yr sold.[49,50]. This number is larger than the evapotranspiration estimates. However, if you assume 90% return flow and 50% irrigation efficiency, then these estimates are consistent with the total paper value of market activity

1.6.3 Impact of Adjudication and Trading on Crop Choice

This sub-section describes the impact adjudication and trading has on the acreage shares of different crops. I estimate a simple panel linear regression for each of the 8 crop groups to estimate the impact of adjudication on crop shares:

$$\text{Share}_{i,t}^{crop} = \delta \text{Post\_Adjudication}_{i,t} + \gamma_i + \gamma_t + \varepsilon_{i,t}$$

The results are presented in the first panel of Table 1.6. After adjudication, we see that there is a substitution away about 2.9% of land away from Pasture. This suggests that the share of all higher value-added crops must have increased by 2.9%.[51] Although the point

49. Slightly less - 1,389,000 AF / yr are bought because of mitigation requirements.

50. Some imputations and assumptions go into making this estimate: when the volume of water transferred is not recorded it is inferred by multiplying through the flow rate of the water right by the duration of the irrigation season.

51. Although the shares in these regressions add up to one, because of the linear structure and separate

Table 1.5: The Impact of Adjudication and Trading on Rates of Evapo-Transpiration

	Dependent variable:			
	Change in Evapotranspiration Rate (mm / Acre day)			
	Specification 1:		*Specification 2:*	
	Individual and Year Fixed Effects		Basin and Year Fixed Effects + Soil and Climate Controls	
	(1)	(2)	(3)	(4)
Post-Adjudication	−0.153		0.036	
	(0.102)		(0.069)	
Post-Buying Water Right		0.054		0.140*
		(0.079)		(0.075)
Post-Selling Water Right		0.046		−0.124
		(0.084)		(0.078)
Observations	93,860	93,860	93,860	93,860
R^2	0.640	0.640	0.332	0.332

	Total Volume of Water (AF/year)			
	(1)	(2)	(3)	(4)
Post-Adjudication	−204,517		48,405	
	(136,506)		(91,658)	
Post-Buying Water Right		18,303		46,964*
		(26,733)		(25,261)
Post-Selling Water Right		14,147		−38,213
		(25,941)		(24,190)

Note: *p<0.1; **p<0.05; ***p<0.01

This table estimates the impact of adjudication and water right trading on evapo-transpiration rates.
Columns (1) and (3) estimate the equation: $\text{ET}_{i,t} = \delta \text{Post-Adjudication}_{it} + X_i\beta_i + \gamma_t + \varepsilon_{i,t}$
Columns (2) and (4) estimate the equation:
$\text{ET}_{i,t} = \delta_1 \text{Post-Buying Water Right}_{it} + \delta_2 \text{Post-Selling Water Right}_{it} + X_i\beta_i + \gamma_t + \varepsilon_{i,t}$
The estimates are noisy - although they are consistent with both (1) the claim that there was no change in total water use after adjudication (2) that there was some change in measured ET when water rights were transferred from buyers to sellers - however the effects are too small to claim that on average sellers completely dry their land after selling rights.
The second panel multiplies through the estimates in panel 1 by the volumes of each water right traded. If you assume 90% return flow and 50% irrigation efficiency then the estimates are consistent with the total paper value of total market activity: 1,461,348 AF / yr sold (1,389,268 AF / yr bought).

estimates indicate a modest increase in the acreage of alfalfa, wheat, barley, potatoes and corn that corresponds to the decrease in pasture acreage, we do not have the power to claim these increases are statistically significant, except for potatoes.

Panel 2 of Table 1.6 estimates how the shares of different crops change after adjudication by estimating the following regression:

$$\text{Share}_{i,t}^{crop} = \delta^{buy}\text{Post-Buys\_WaterRight}_{i,t} + \delta^{sell}\text{Post-Sells\_WaterRight}_{i,t} + \gamma_i + \gamma_t + \varepsilon_{i,t}$$

where Post-Buys_WaterRight$_{i,t}$ and Post-Sells_WaterRight$_{i,t}$ are dummy variables that become true after a parcel of land either buys or sells a water right that was attached to it. The estimates show that both buyers and sellers of water rights reduce their acreage of other crops after adjudication. However, buyers of water rights have a larger reduction in pasture area which is offset by increases wheat and potatoes acreage. Buyers of water rights have a smaller reduction in pasture acreage, and it is unclear how this is offset, perhaps by fallow land, alfalfa, and wheat.

These tables suggest that the primary margin of substitution after both adjudication and trading is on the extensive margin, moving pasture into cropland, and not on the intensive margin between different varieties of crops.[52] Since I do not appear to have the statistical power to detect large changes in relatively small crop shares, I simplify my analysis in the next section by estimating my results as a binary choice between ”Pasture and Fallow” versus ”Crops” rather than a choice between the many different crops farmers choose.

regressions, the treatment effects do not necessarily add up.

52. This is a slightly different result to other parts of the literature, for example, Debaere and Li (2017). This is consistent with the findings of Hornbeck and Keskin (2015) in the short run, but not in the long-run.

Table 1.6: The Impact of Adjudication and Trading on Crop Acreage Shares

	Panel 1: Impact of Adjudication on Crop Shares *Dependent variable:*								
	Fallow	Pasture	Alfalfa	Wheat	Barley	Potatoes	Corn	Sugarbeets	Other
Post-Adjudication	−0.002	−0.029*	0.013	0.004	0.008	0.002**	0.003	−0.002	0.003
	(0.012)	(0.016)	(0.020)	(0.009)	(0.006)	(0.001)	(0.004)	(0.002)	(0.002)
R^2	0.395	0.795	0.564	0.437	0.488	0.440	0.567	0.309	0.381

	Panel 2: Impact of Trading on Crop Shares *Dependent variable:*								
	Fallow	Pasture	Alfalfa	Wheat	Barley	Corn	Potatoes	Sugarbeets	Other
Post-Buying Water Rights	0.007	−0.039***	0.002	0.035***	0.002	−0.002	0.008***	−0.002	−0.004
	(0.009)	(0.012)	(0.015)	(0.013)	(0.011)	(0.005)	(0.003)	(0.002)	(0.003)
Post-Selling Water Rights	0.010*	−0.029***	0.010	0.012	0.003	0.005	0.001	−0.001	0.002
	(0.006)	(0.010)	(0.012)	(0.014)	(0.005)	(0.004)	(0.001)	(0.001)	(0.002)
R^2	0.395	0.795	0.564	0.440	0.488	0.442	0.365	0.309	0.381
T	11	11	11	11	11	11	11	11	11
N	19159	19159	19159	19159	19159	19159	19159	19159	19159
Observations	186,711	186,711	186,711	186,711	186,711	186,711	186,711	186,711	186,711

Note: *p<0.1; **p<0.05; ***p<0.01

Panel 1 estimates the following for each of 9 crop classifications:
$\text{Share}_{i,t}^{crop} = \delta \text{Post\_Adjudication}_{i,t} + \gamma_i + \gamma_t + \varepsilon_{i,t}$
where $\text{Share}_{i,t}^{crop}$ is the fraction of the land parcel which is planted in that specific crop as measured by the Crop Data Layer.
$\text{Post\_Adjudication}_{i,t}$ is an indicator variables that become true after the adjudication of all water rights attached to that parcel of land is complete.
Panel 2 estimates:
$\text{Share}_{i,t}^{crop} = \delta^{buy}\text{Post-Buys\_WaterRight}_{i,t} + \delta^{sell}\text{Post-Sells\_WaterRight}_{i,t} + \gamma_i + \gamma_t + \varepsilon_{i,t}$
where $\text{Post-Buys\_WaterRight}_{i,t}$ and $\text{Post-Sells\_WaterRight}_{i,t}$ are indicator variables that become true after a parcel of land either buys or sells a water rights attached to it.
In both specifications, the regressions are panel 'within' estimators with time fixed effects γ_t and parcel fixed effects γ_i.
Standard errors are two-way clustered at the basin and year level.

1.7 Estimating Value Created by Adjudication

The evidence in previous sections suggests that adjudication created significant value through increased trading and expanded acreage. However, this does not necessarily imply that the benefits of adjudication outweigh the considerable investment of over $94 million Idaho made in the adjudication process. This section answers this question, using a novel revealed preference approach to estimate the value generated by adjudication.

1.7.1 Revealed Preference Framework

When farmers make decisions about which crops to plant, they consider the relative productivity of the different crops under the prevailing system of property rights, and the different input and output prices for producing each crop. Using a logit framework, the choice of crops planted by farmers reveals their preferences or 'average utilities' for each crop, and changes in cropping patterns after adjudication reveal how adjudication changes these 'average utilities.' However, 'utility' is only identified in relative terms between crops; to obtain monetary estimates of the value-added from growing a particular crop, I need to normalize utility by a demand elasticity with respect to some exogenously varying price. Then, I can estimate the value created post-adjudication by adding up the changes in the expected maximum value of crop choices across all individuals.

I estimate this model in a panel framework, with group and year fixed effects. As a result, to identify a price elasticity, the candidate price needs to vary in a panel manner - by having relative changes over time across individuals between crops[53] and it needs to vary in a plausibly exogenous manner.

Crop insurance is an excellent candidate; crop insurance prices are federally regulated and set by an algorithm which is known and transparently understood. There is panel

53. This rules out all variations in prices which are purely cross-sectional - such as travel time. It also rules out purely time series price variation - such a commodity prices.

variation in crop prices; baseline crop insurance prices are set at the county level, with further adjustments made for individual soil characteristics. The setting of prices is designed to be actuarial fair conditional on the empirical distribution past crop losses. As a result, the price of insurance varies exogenously with the realization of past weather shocks. This variation is what I use to identify the impact of changes in insurance prices, while using year and sub-basin and county fixed effects to partial out other confounding effects.

Suppose the value $\pi^c_{i,t}$ to a farmer i residing in county b at time t of growing a particular crop c on a piece of soil with characteristics X_i and facing a price of crop insurance $p_{b,t}$ is given by the production function:

$$\pi^c_{i,t} = \delta^c \times \text{PostAdju}_{i,t} + \alpha \times \text{InsPrice}^c_{i,t} + X_i\beta + \gamma_t + \gamma_b + \varepsilon^c_{i,t}$$

In this framework, adjudication changes the relative profitability of each crop δ^c. The coefficient α is the demand response to the crop specific insurance price. Assuming that adjudication does not change the profitability of the outside option - pasture in this application - we can estimate the change in the profitability of any particular crop by computing $-\delta^c/\alpha$[54].

The advantage of this approach over alternative methods which calculate the value added by computing the expected revenue of each acre of cropland (perhaps by multiplying acreage times average yield times crop price) is that farmer's choices implicitly account for all unobserved input quantities and input costs (monetary and otherwise) and other factors that might influence the relative profitability of different crops, but are unobserved by the econometrician.

54. since 23% of pasture is irrigated, and it might be expected that adjudication increased the profitability of irrigated pasture by allowing more efficient water use, then we would underestimate the effect of adjudication.

1.7.2 *Insurance Prices*

Crop insurance is regulated by the federal government who restrict offerings to a uniform set of prices and products. These prices are set by the USDA's Risk Management Agency (RMA). The full details about how insurance prices are set are complex. The RMA sets insurance with the aim of achieving an expected loss ratio of 1. To price these plans, the RMA determines a "base rate" each year for each county by crop combination. These rates are calculated for a 65% coverage level from that county's insured individual's loss experiences going back to 1975. The insurance pricing formula calculates a "reference yield" which estimates the expected yield for all producers of a crop within a county. Recent fluctuations around the trend of historical yield are used to construct an empirical distribution of losses.[55] There is little room for discretion in setting insurance prices; the formulaic approach is applied evenly across all counties.[56]

After controlling for county averages and time trends, the residual variation year-to-year in the relative price of crop insurance between counties is driven by weather shocks which can be considered plausibly exogenous. For example, consider two counties which receive different weather shocks in a year, these counties have different losses, changing the empirically observed distribution of historic losses, in turn, this changes the price of insurance in subsequent years. The changes in the price of crop insurance change the relative profitability of planting different crops and lead farmers to different patterns of crop choice. These changes in crop choice are what I used to estimate the demand elasticity of crop insurance with respect to these weather shocks. As a result, estimating a demand parameter on the price of crop insurance after controlling for year and county fixed effects is likely to

55. Unlike the county base rate, the reference yield is constructed using NASS data which covers both insured and uninsured farmers.

56. However, because sometimes this does lead to outliers, and there are rules in place to handle such exceptions. For example, the rate that producers play for the same coverage cannot change by more than 20% between years. In extreme cases, usually involving poor data quality expert underwriters are brought in to determine rates.

yield an unbiased estimate of the demand for crop insurance.[57]

1.7.3 Estimation

Following standard logit results, if we assume that $\varepsilon^c_{i,t}$ is distributed I.I.D on a Type 1 Extreme Value Distribution then the probability that farmer chooses crop c will be:

$$\Pr_{i,t}(c) = \frac{\exp(\pi^c_{i,t}(\mathbf{X}_{i,t}|\theta))}{\sum_{c'=1}^{C} \exp(\pi^{c'}_{i,t}(\mathbf{X}_{i,t}|\theta))}$$

Where $\theta = (\alpha, \beta, \gamma_t, \gamma_b)$ and $\mathbf{X}_{i,t} = (\text{PostAdju}_{i,t}, \text{InsPrice}_{i,t}, X_i)$. We can then estimate this using standard maximum likelihood:

$$\theta^* = \text{argmax}_{\theta^*} \mathcal{L}(\theta|\mathbf{X}_{i,t})$$

Where

$$\begin{aligned}\mathcal{L}(\theta) &= \sum_{i=1}^{N}\sum_{i=t}^{T} \ln[l_{i,t}(\theta|\mathbf{X}_{i,t})] \\ &= \sum_{i=1}^{N}\sum_{i=t}^{T}\sum_{c=1}^{C} y^c_{i,t} \ln(Pr^c_{i,t}(\theta|\mathbf{X}_{i,t}) \\ &= \sum_{i=1}^{N}\sum_{i=t}^{T}\sum_{c=1}^{C} y^c_{i,t} \ln\left[\frac{\exp(\pi^c(\theta|\mathbf{X}_{i,t}))}{\sum_{c'=1}^{C}\exp(\pi(\theta|\mathbf{X}_{i,t}))}\right]\end{aligned}$$

The next sub-section presents the results to a simplified estimate of the model; I reduce the problem to a bi-variate logit where farmers choose between either planting a high-value crops or leaving the land as pasture/fallow. In Appendix Tables A1.3 and A1.4, I include a multivariate estimate of this model where farmers choose between all nine crops described in the previous section, however the results are substantively similar because the main margin

57. In the future I plan to take this approach further by explicitly instrumenting the price of crop insurance with historic losses and estimating both the instrumenting equation and the logit via a joint maximum likelihood approach.

of substitution is between pasture and crops.[58]

After estimating the model, I calculate two statistics which I use to evaluate the value of adjudication. First I compare the acreage in crops in 2016, to the counterfactual acreage that would have been planted had adjudication never taken place:

$$\%\Delta\text{Acreage} = \sum_i \frac{Area_i}{\sum_j Area_j}\Big[Pr(y_{i,2016}|\text{PostAdju}_{i,t} = \text{PostAdju}_{i,2016}) \\ - (Pr(y_{i,2016}|\text{PostAdju} = 0)\Big]$$

Secondly I calculate the total profit generated by adjudication :

$$\text{Total Value} = \sum_i Area_i\Big[E(\max_{c=1,\ldots,C}(\pi_{i,2016}(\text{PostAdju} = \text{PostAdju}_{i,2016}))) \\ - E(\max(\pi_{i,2016}(\text{PostAdju} = 0)))\Big]$$

where $E(\max_{c=1,\ldots,C}(\pi_{i,t})) = \frac{1}{\alpha}\ln(\sum_{c=1,\ldots,C}(\exp X_i\beta^c)$

Next, I narrow my focus on water right trading as the specific mechanism by which adjudication gives rise to changes in crop acreage. To do this I estimate the following model:

$$y_{i,t} = \delta \times \text{PostBuyingRight}_{i,t} + \text{PostSellingRight}_{i,t} + \alpha \times \text{InsPrice}_{i,t} + X_i\beta + \gamma_t + \gamma_b + \varepsilon_{i,t}$$

I calculate acreage changes and profits attributable to these trades in an analogous manner to what was previously described. I calculate the change in the value of output as a result of water right trading. Finally, I calculate the change in output as a result of marginal water right trades which were induced by adjudication by multiplying the .

58. Another benefit of the bi-variate estimate is that it abstracts from modelling patterns of substitution between crops and from the dynamics of crop rotation.

1.7.4 Results

Results are presented in Table 1.7. There are two sets of control specifications; The first specification contains county and year fixed effects; while the second contains additional soil and climate controls to account for heterogeneity within the county. The text below describes the results from the second specification which utilizes a richer set of controls

The raw coefficients are between 0.4-0.6. The scale of this number is arbitrary, so it cannot be easily interpreted. For each control specification, I present a regression omitting crop insurance price. This omission does not dramatically change the point estimates on $Post\_Adjudication_{i,t}$, indicating that changes in crop insurance prices appear to be orthogonal to adjudication status.

By normalizing the $Post\_Adjudication_{i,t}$ coefficient by the coefficient on insurance price, I find that adjudication increased the relative profitability of growing crop by \$84-\$114 dollars per acre. This increase in profitability led to a 3.9% increase in the acreage of crops. These two factors lead to an increase in the total profitability of crop production in Idaho by \$257 million, this is a central result of this paper. The estimates do not vary significantly across the two specifications and are statistically significant; however, the standard errors are substantial and as such the results should be interpreted cautiously.

This result does not necessarily rely on normalizing our estimate with respect to insurance price demand. The coefficient on post-adjudication implies that total utility is 8% higher after adjudication. I can check this value against other potential normalizations; for example, if we interpret 'total utility' as farm revenues, then we can normalize the change in profitability after adjudication against the total market value of Idaho's crop production (\$3.1 billion according to 2012 census of agriculture). If we do this then the value created by adjudication to be \$231 million, an estimate of a similar magnitude. However, this estimate does not account for differences in the unobserved costs farmers must pay to produce different crops.

Table 1.8 estimates the impact that water right trading had on crop choices, and whether the increase in trading we documented in Section 1.5 rationalizes the increase in crop acreage we observed in Table 1.7.

After buying water rights, I estimate the relative profitability of a farmer planting crops over pasture increases by $119 per acre and buyers increase the share of their land planted in crops by 10% from 67% to 77%. On the other hand, after selling water rights, my point estimate implies that the relative profitability of planting crops also increases $35, but point estimate is not statistically significant. Sellers increase their share of their land planted in crops by 5% from 45% to 50%. Taken together, both the post-transaction increase in the profitability of buyers and sellers, as well as the corresponding increase in crop acreage imply that the total surplus generated from transfers of water rights between 1999-2017 is $98 million. However many of these transactions might have occurred anyway in the absence of adjudication, some even happen before adjudication. To account for this, I adjust the value created by each trade with the probability that trade was induced by adjudication. After this adjustment, I estimate the surplus adjudication generates through the particular channel of increased water trading is $56 million.[59]

1.8 Aggregate Impacts of Adjudication

In this section, I attempt to detect aggregate impacts of adjudication using data from the Census of Agriculture. In particular, I estimate the impact of adjudication on-farm investment decisions, farm revenues, and land value. Although the census of agriculture offers a

59. This result implies that trading of water rights accounts for less than 20% of the profitability changes after adjudication. There are two potential reasons for this: Firstly, If I might have overestimated the value of adjudication due to a violation of the parallel trends assumption. In the next section, I test this by using aggregate data from the census of agricultural to estimate the impact of adjudication on the value of agricultural production and the capitalization of this value into land prices. Secondly, there might be other mechanisms through which adjudication increases the value of planting crops. For example, an increase in the security of water rights might have led farmers to make larger and longer-term on-farm capital investments, increasing the profitability of growing crops. I also test this using the census of agriculture data.

Table 1.7: Logit Model of Decision to Plant Crops After Water-Rights Adjudicated

	Dependent variable:			
	Choice of Crop Type			
	Specification 1:		*Specification 2:*	
	Basin and Year Fixed Effects		Basin and Year Fixed Effects + Soil and Climate Controls	
	(1)	(2)	(3)	(4)
	Estimated Coefficients:			
Insurance Price		-0.006^{**}		-0.005^{**}
		(0.002)		(0.002)
Post Adjudication	0.630^{***}	0.672^{***}	0.630^{***}	0.411^{***}
	(0.199)	(0.196)	(0.199)	(0.150)
	Monetized Estimates: ($/ Acre)			
Post Adjudication		114.2		84.1
		[41.6, 423.7]		[29.0, 297.9]
	Acreage change in 2016 resulting from adjudication (%)			
		7.3		3.9
		[2.6, 27.1]		[1.4, 14.7]
	Total Value of Adjudication: ($ 000,000)			
		320		257
		[126, 915]		[95, 1254]
Observations	136,298	136,298	136,298	136,298
R^2	0.096	0.115	0.096	0.133

Note: *p<0.1; **p<0.05; ***p<0.01

- The first panel of this table shows the estimate coefficients from the fractional logistic regression:

$$y_{i,t} = \delta \times \text{PostAdju}_{i,t} + \alpha \times \text{InsPrice}_{i,t} + X_i\beta + \gamma_t + \gamma_b + \varepsilon_{i,t}$$

  Where $y_{i,t}$ is the share of the basin planted in crops (wheat, alfalfa, barley, corn, sugarbeets, potatoes and other) rather than pastureland or fallow land.
  Fixed effects are estimated with dummy variables. Standard errors are clustered two ways at the basin and year level.
- The second panel shows the monetized estimates of the change in value of crops relative to pasture after adjudication: $-\frac{\text{PostAdju}_{i,t}}{\text{InsPrice}_{i,t}}$.
  Square brackets contain 95% confidence interval constructed via cluster bootstrap at basin $\times$ year level with sample of 1000.
- The third panel estimates the change in acreage attributable to adjudication:
  $\%\Delta\text{Acreage} = \sum_i \frac{Area_i}{\sum_j Area_j}\left[Pr(y_{i,2016}|\text{PostAdju}_{i,t} = \text{PostAdju}_{i,2016}) - (Pr(y_{i,2016}|\text{PostAdju} = 0)\right]$
- The fourth section estimates the expected maximum profit
  $\text{Total Value} = \sum_i Area_i\left[E(\max_{c=1,\ldots,C}(\pi_{i,2016}(\text{PostAdju} = \text{PostAdju}_{i,2016}))) - E(\max(\pi_{i,2016}(\text{PostAdju} = 0)))\right]$
  where $E(\max_{c=1,\ldots,C}(\pi_{i,t})) = \frac{1}{\alpha}\ln(\sum_{c=1,\ldots,C}(\exp X_i\beta^c))$

Table 1.8: Logit Model of Decision to Plant Crops After Water-Rights are Bought or Sold

	Dependent variable:			
	Choice of Crop Type			
	Specification 1:		*Specification 2:*	
	Basin and Year Fixed Effects		Basin and Year Fixed Effects + Soil and Climate Controls	
	(1)	(2)	(3)	(4)
Insurance Price		−0.005**		−0.004**
		(0.002)		(0.002)
Post Buying Water Right	0.691***	0.667***	0.691***	0.536***
	(0.139)	(0.128)	(0.139)	(0.127)
Post Selling Water Right	0.396	0.380	0.396	0.156
	(0.291)	(0.288)	(0.291)	(0.245)
	Monetized Estimates ($/Acre):			
Post Buying Water Right		128.5		119.0
		[52.8, 468.1]		[27.2, 371.3]
Post Selling Water Right		73.2		34.6
		[23.5, 248.1]		[-13.2, 113.0]
	Average Acreage Change after Transfer:			
Post Buying Water Right		8.9		10.2
		[3.1, 33.0]		[2.5, 36.7]
Post Selling Water Right		3.2		5.5
		[0.7, 12.1]		[1.9, 20.4]
	Surplus from transfers ($ 000,000):			
Surplus from all transfers		79		98
		[31, 355]		[40, 483]
Surplus from transfers induced by adjudication		45		56
		[17, 202]		[23, 276]
Observations	136,298	136,298	136,298	136,298
R^2	0.087	0.105	0.093	0.130

Note: *p<0.1; **p<0.05; ***p<0.01

- The first panel of this table shows the estimate coefficients from the fractional logistic regression:

$$y_{i,t} = \delta \times \text{PostBuyingRight}_{i,t} + \text{PostSellingRight}_{i,t} + \alpha \times \text{InsPrice}_{i,t} + X_i\beta + \gamma_t + \gamma_b + \varepsilon_{i,t}$$

  where $y_{i,t}$ is the share of sub-basin planted in any crop rather than pasture or fallow land.
  Fixed effects are estimated with dummy variables. Standard errors are clustered two ways at the basin and year level.
- The second panel shows the monetized estimates of the change in value of crops relative to pasture after adjudication: $-\frac{\text{PostBuyingRight}_{i,t}}{\text{InsPrice}_{i,t}}$ and $-\frac{\text{PostSellingRight}_{i,t}}{\text{InsPrice}_{i,t}}$. Square brackets contain 95% confidence interval constructed via cluster bootstrap at basin × year level with sample of 1000.
- The third panel estimates the change in acreage in Idaho attributable to adjudication by estimating $\%\Delta\text{Acreage} = \sum_i \frac{Area_i}{\sum_j Area_j}\left[Pr(y_{i,2016}|\text{PostBuyingRight}_{i,2016}, \text{PostSellingRight}_{i,2016}) - (Pr(y_{i,2016}|0,0)\right]$
- The fourth section estimates the expected maximum profit
  $\text{Total Value} = \sum_i Area_i \left[E(\max_{c=1,\ldots,C}(\text{PostBuyingRight}_{i,2016}, \text{PostSellingRight}_{i,2016}))) - E(\max(\pi_{i,2016}(0,0)))\right]$
  where $E(\max_{c=1,\ldots,C}(\pi_{i,t})) = \frac{1}{\alpha}\ln(\sum_{c=1,\ldots,C}(\exp X_i\beta^c))$

rich set of outcome variables, it is limiting for two reasons, firstly the Census is only measured every five years, and as a result, there are only have three post-adjudication observations (in 2002, 2007, 2012). Secondly, observations in the census are at the county-level, and the 44 counties in Idaho to not precisely match the 35 sub-basins units which are the level at which the adjudication was conducted, reducing potentially useful variation. As a result, this analysis is statistically underpowered, and the results are sensitive to specification.

My results are presented in Table 1.9. For each outcome variable, I present two specifications, one where the outcome is in levels and another where the outcome is in logs. I present each specification with two sets of controls, one with County Fixed Effects and parametric time trends (specifically a quadratic polynomial time trends and growing degree days and precipitation). The other specification has a traditional panel structure with year and county fixed effects.

In the first two columns of Table 1.9 the outcome variable is land value. If adjudication actually increased the value of farm incomes, as I estimated in the previous section, then I would expect the value created by adjudication is at least partially capitalized in land values. I find mixed results. When the outcome variable is measured in levels, the value of agricultural land increases by \$200,000 per farm. But when I measure the outcome variable in logs, I find the value of agricultural land declines by 18%

In the second two columns of Table 1.9 the outcome variable is the value of farm equipment and machinery. Aside from reducing transaction costs, one alternative channel through which adjudication could impact agricultural productivity is that the reduction in uncertainty created by clarifying water rights might lead farms to invest in more equipment and machinery. In this specification, the results are consistent between the level-specification and log-specifications. When measured in levels, on-farm investment increases by \$36,000 per farm. This is consistent with the 22% increase in the value of on-farm machinery found when the outcome is measured in logs. However, the result in logs is not statistically significant.

It would appear that adjudication generated an increase in on-farm investment.

In the final two columns of Table 1.9 the outcome variable is net farm income. In the level-specification, adjudication increased net farm income by $50,000, although the effect is not significant. In the log-specification, adjudication increased net farm income by 120%, although this estimate is statistically significant, I find it too large to be plausible.

Table 1.9: The Impact of Adjudication on Aggregate Economic Outcomes

	Panel 1 - Dependent variable in Levels					
	Value of Land		Value of On-Farm Equipment and Machinery		Net Cash Income	
	(1)	(2)	(3)	(4)	(5)	(6)
Share of County Adjudicated	205,104*	210,031*	36,988**	35,428**	51,529	51,048*
	(107,518)	(116,755)	(14,751)	(15,545)	(33,376)	(26,442)
R^2	0.882	0.882	0.720	0.712	0.730	0.707
	Panel 2 - Dependent variable in Logs					
	Log(Value of Land)		Log(Value of On-Farm Equipment and Machinery)		Log(Net Cash Income)	
	(1)	(2)	(3)	(4)	(5)	(6)
Share of County Adjudicated	−0.210**	−0.189***	0.190	0.205	0.787**	0.811**
	(0.086)	(0.072)	(0.130)	(0.139)	(0.313)	(0.318)
R^2	0.963	0.962	0.774	0.769	0.863	0.859
	Specification					
Parametric Time Trends	X		X		X	
Year FE		X		X		X
County FE	X	X	X	X	X	X
T	7	7	7	7	7	7
N	44	44	44	44	44	44
Observations	308	308	308	308	308	308

Note: *p<0.1; **p<0.05; ***p<0.01

The two panels on this table estimate the impact of adjudication on economic outcomes from the Census of Agriculture. In the top panel outcome variables are measured in levels in the bottom panel variables are measured in logs. Observations are at the county level and contain all 44 counties in Idaho. Observations are included from seven 5-yearly Census of Agriculture between 1982-2012.

The estimating equation for specifications with county FE and year FE is: $y_{it} = \delta \text{Share Adjudicated}_{it} + \gamma_i + \gamma_t + \varepsilon_{it}$

The estimating equation for specifications with parametric trends is: $y_{it} = \delta \text{Share Adjudicated}_{it} + \gamma_i + f(t) + X_{it}\beta + \varepsilon_{it}$

Where $f(t)$ is a quadratic polynomial in calendar year, and X_{it} includes Idaho Producer Price Indices and Farm Cost Indices from FRED, and county growing degree days and precipitation from the PRISM climate database. Standard Errors are robust clustered two ways at the County and Year Level.

1.9 Conclusion

This paper has studied the adjudication of water rights in the Snake River Basin, Idaho. By exploiting differences in the timing of the adjudication across sub-basins, I show that adjudication leads to a 140% increase in the rate at which water rights are traded. I also find that these trades move water to more productive pieces of land and that buyers of water increase their crop acreage. Taken together these findings suggest that adjudication reduced transaction costs, and led to more Pareto improving trades and a more efficient resource allocation.

I use a revealed preference approach to estimate the value generated by an expansion of crop acreage after adjudication. I use demand responses to plausibly exogenous changes in crop insurance prices to monetize my estimates of the value by adjudication. I find that adjudication increased the annual value of Idaho's agricultural production by $250 million per year. This $250 million annual return significantly exceeds the $94 million one-time cost Idaho spent on adjudication.

However, only 20% of this value was generated among farmers who traded their water rights as a result of adjudication. Although this is clearly an important mechanism, it suggests that much of the value of adjudication is created through other channels. For example, I also find suggestive evidence that reduction in uncertainty after adjudication lead to an increase in on-farm investment in machinery and equipment.

Furthermore, there are several reasons to believe this study might give only a lower bound on the true value of adjudication: Firstly, this analysis focused solely on the agriculture and has not attempted to estimate any value adjudication generated in the urban or electricity sectors. Secondly, my results estimate only the economic value that is identified from relative difference in the timing of adjudication between basins. As a result, it is difficult to quantify the impact of aspects of adjudication that affected all users of water rights simultaneously, such as unilateral changes in policy by IDWR or case law developed through Idaho's courts.

Furthermore, my estimates come from only a relatively short time horizon, and consequently reflect only the short-term benefits of adjudication, and only those arising from increases in trading and acreage immediately after adjudication.

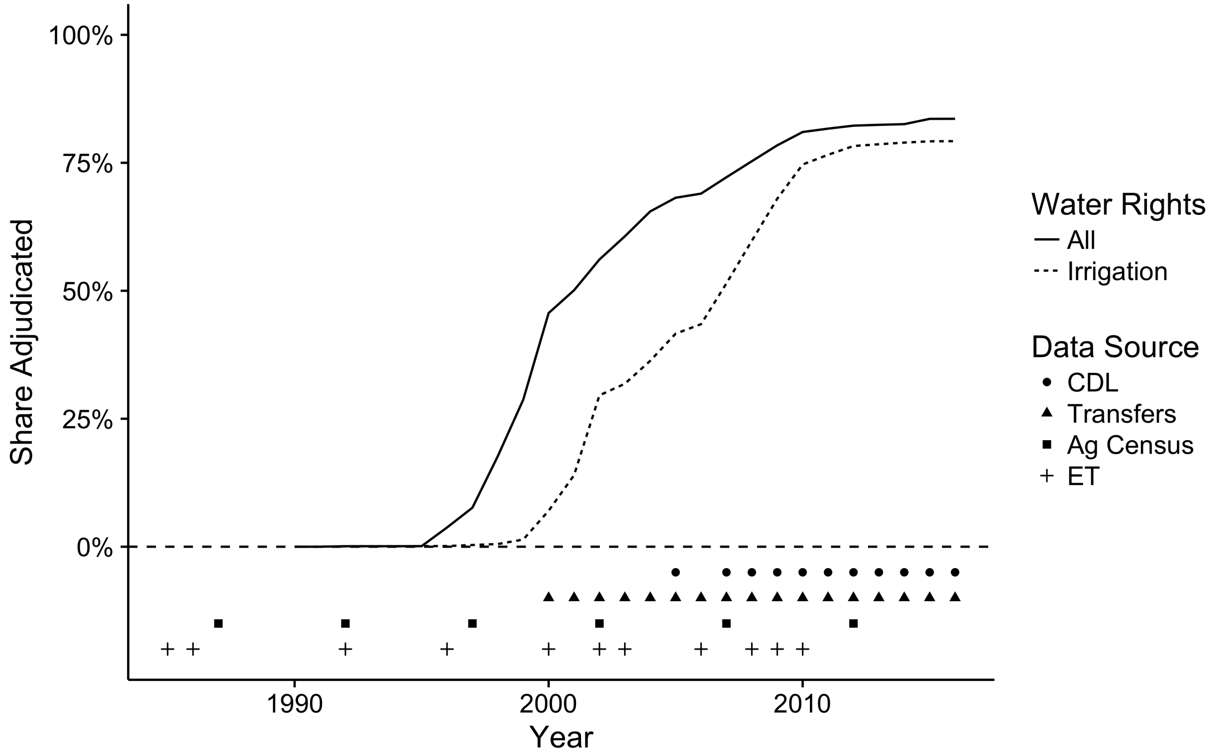
In the long-term benefits of adjudication are likely to be even greater than in the short-term, as farmers make more long-term investments and adjust over longer time horizons to systemic changes. Finally, the benefits could be even greater still if adjudication reduces the cost of adapting to future changes in demand for water use, such as those expected as a result of climate change and urban growth.

A1.1 *Appendix Tables and Figures*

Table A1.1: List of Data Sources

Data	Source	Resolution	Years Available	Fields
Water Right Database	IDWR	Individual Water Right - GIS Place of Use and Point of Diversion	NA - Cross Section	Adjudication Date, Priority Date, Nature of Use, Water Source, Area, Maximum Rate of Diversion, Maximum Permitted Irrigable Area, Associated Water Districts, Number of Water Sources
Water Right Trades	IDWR	Individual Water Right Level - GIS Place of Use and Point of Diversion	Annual 2000-2016	Date, Approval, Parcel of WR Seller, Parcel of WR Buyer
Farm Crop Choice	USDA NASS Crop Data Layer	30x30m Raster	2005, Annual 2007-2016	Crop Classifications
Soil Data	USGS SSUURGO	30x30m Raster	NA - Cross Section	Irrigation Capacity Class, Mean Anual Temperature, Average Annual Precipitation, Average Annual Frost Free Days, Elevation, K-factor, Clay Percentage, Land Slope
Climate Data	PRISM	County Level	Annual 1990-2016	Annual Temp, Annual Precip, Annual Growing Degree Days
Crop Insurance Data	USDA RMA Summary of Business Data	County Level	Annual 1990-2016	Type of Insurance, Acreage and Average Price by Crop
Evapotranspiration Data	UIdaho METRIC Model calibrated from Landsat Satellite Data	30x30m Raster	1985, 1986, 1992, 1996, 2000, 2002, 2003, 2006, 2008, 2009, 2010	Average Daily ET calculated from Season Average ET
Census of Agriculture	USDA	County Level	1972, 1978, 1982, 1987, 1992, 1997, 2002, 2007, 2012	On-farm Investment & Property Values

Figure A1.1: Share of Water Rights Adjudicated



Solid series shows the share of all water rights in Idaho which have been adjudicated since 1990 as a result of the SRBA and other adjudications. The dashed line shows share adjudicated for only the subset of irrigation water rights. The difference in timing between 'all rights' and 'irrigation rights' is due to a different administrative process being used to adjudicate domestic and stock-water rights. The shapes below show the years in which data is available from various sources.

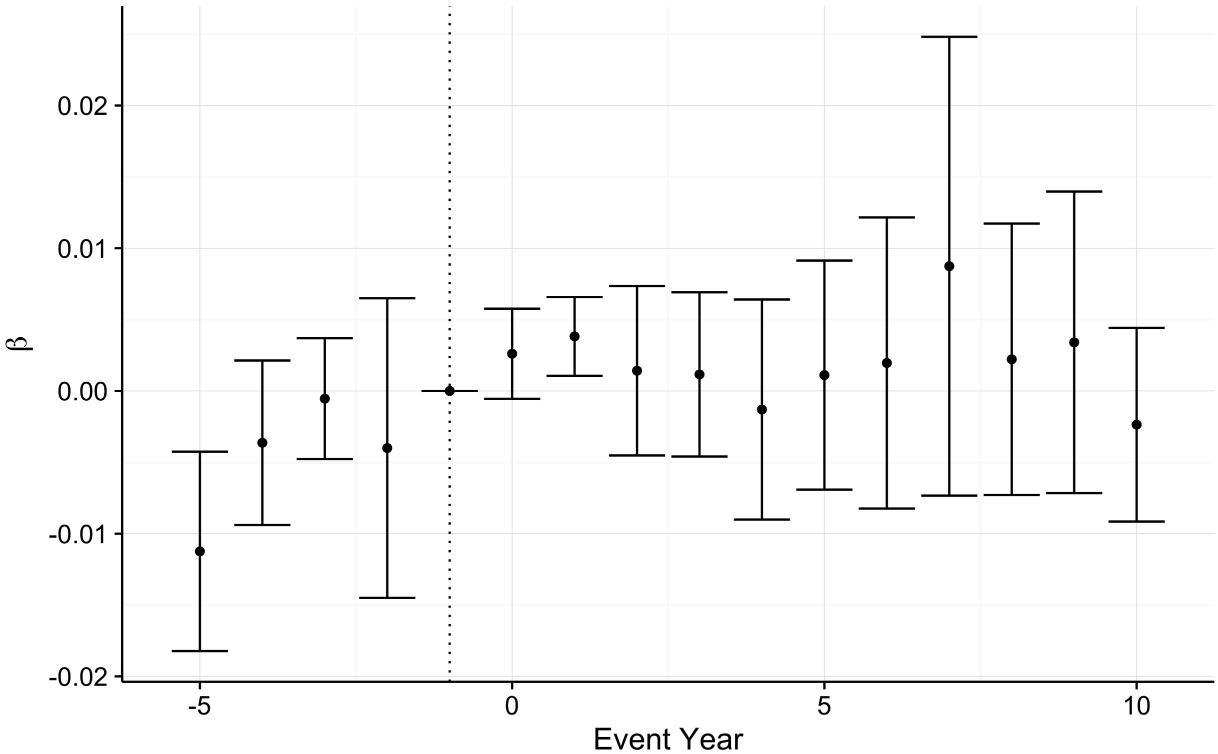
Table A1.2: Average Characteristics of Water-Rights by Adjudication Timing

Adjudication Date	Early 1999-2002	Middle 2003-2006	Late 2007-2015	Unadjudicated
Number of Observations	6148	10165	18738	9295
Irrigation Value Added ($)	900	1051	1003	711
Groundwater Source (%)	42	59	29	46
Priority Date	1934	1943	1928	1962
Number of Water Sources	1.03	1.02	1.04	1.03
Large Place of Use (%)	4.5	3.0	7.7	6.8
Acres permitted to Irrigate	44	76	130	41
Maximum Flow Rate (cfs)	1.69	2.10	4.24	1.97
Elevation (m)	1427	1398	1350	1184
Avg Air Temp (°C)	7.0	7.5	7.6	7.6
Avg Annual Precip (mm)	297	295	326	462

Table contains the average characteristic of water rights and the land they irrigate, binned by their date of adjudication. If adjudication was quasi-randomly assigned then characteristics should be similar across time. Unfortunately there are significant differences between bins. This motivates a panel approach to estimation which instead relies on an assumption parallel trends.

Characteristics are those associated with the water rights at the end of the sample. The soil characteristics are from gSSURGO. The climate characteristics are from PRISM averaged over the previous 50 years. The water rights unadjudicated at the end of 2017 are outside of the Snake River Basin, either in Northern Idaho Basins or the Bear River Basin.

Figure A1.2: Event Study - The Impact of Adjudication on the Rate of Trading with Extended Post-Period

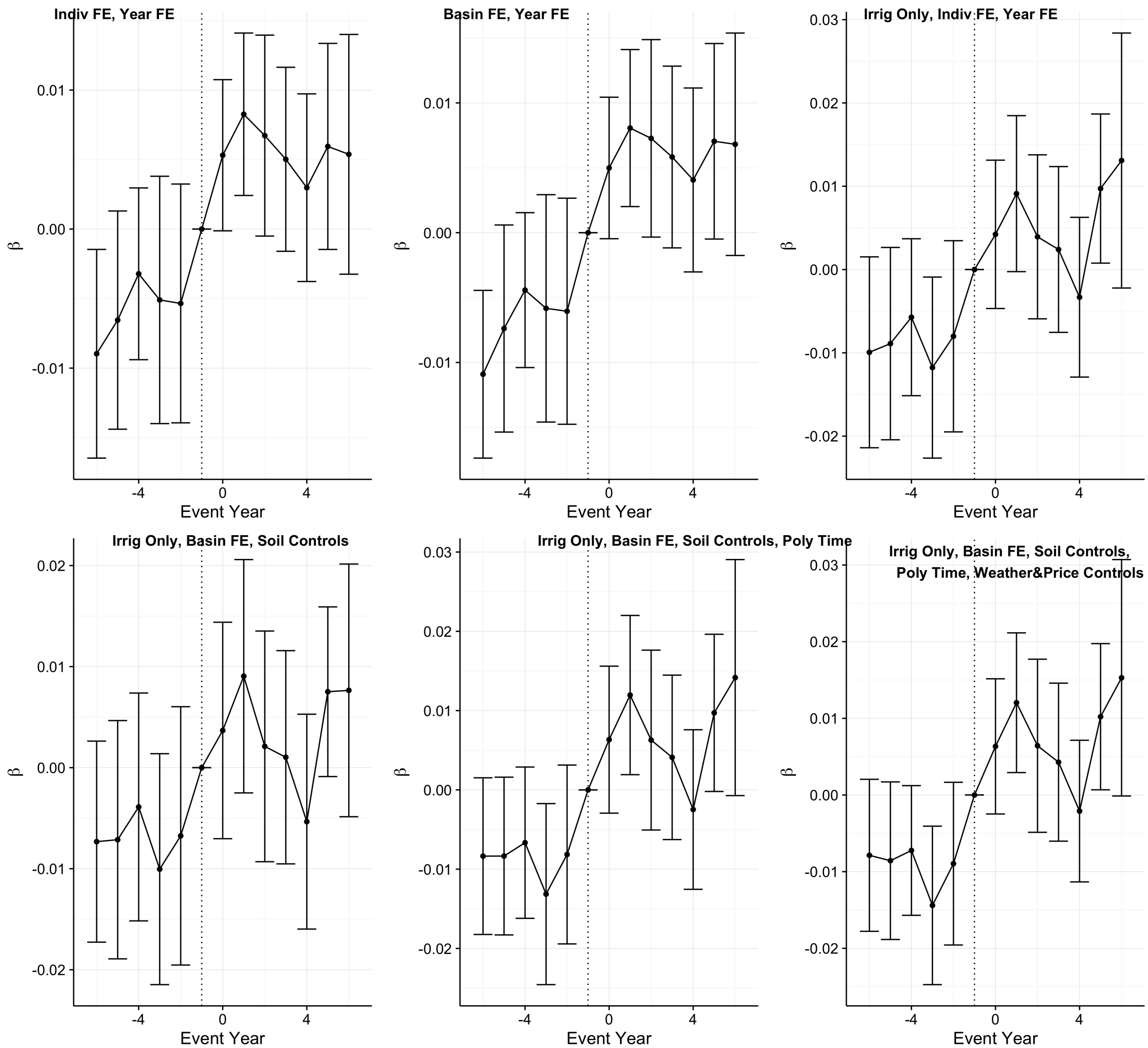


This figure addresses concerns about manipulation around the treatment date. Since adjudication is anticipated, firms might strategically defer trades before adjudication until after. In this case our estimate would not affect the persistent effects of adjudication. However if the effect is persistent over time, then we might expect to attribute the changes to permanent increases in the rate of transfers, perhaps driven by a permanent reduction in transaction costs.
This figure estimates the same equation as Figure 5 :

$$y_{it} = \delta_t \text{Years After Adjudication}_{it} + \gamma_{\text{year}} + \gamma_{\text{water right}} + \varepsilon_{it}$$

However this table instead restricts the sample the three years (2004-2006) in which there at least 10 observations available after adjudication and 4 before.
Although the results are noisy, they are suggestive of a permanent increase in the rate of transactions.

Figure A1.3: Event Study - The Impact of Adjudication on the Rate of Trading with Robustness Checks



This figure replicates Figure 5 for different specifications show in Table 5. In particular we estimate some version of

$$y_{it} = \delta_t \text{Years After Adjudication}_{it} + f(t, i, X_{i,t}) + \varepsilon_{it}$$

The sample is restricted to data with at least six years of observations before and after adjudication. The omitted factor in the regression is $t = -1$, the year before adjudication. Standard errors are robust and clustered two ways at the Basin and Year level. Confidence intervals are at the 95% level. See Table 5 for more details on specifications

Table A1.3: Multinomial Model of Crop Choice Changes after Water-Rights are Adjudicated

	Specification 1: Basin FE + Year FE							
Insurance Price				-0.008^{***}				
				(0.003)				
	Fallow	Alfalfa	Wheat	Barley	Potatoes	Corn	Sugarbeets	Other
Post Adjudication × Crop	0.185	0.465^{***}	0.441^{**}	0.545	0.408	0.279	0.672^{***}	-0.804^{**}
	(0.208)	(0.100)	(0.201)	(0.338)	(0.301)	(0.292)	(0.236)	(0.367)
Post Adjudication × Crop (Monetized Estimates)	24.6	61.9^{**}	58.8	72.6	54.3	37.2	89.5^{**}	-107.1^{*}
	(25.4)	(28.9)	(37.1)	(54.1)	(44.2)	(43.3)	(44.5)	(55.9)
Average Partial Value Added ($ / Acre)				22.4				
				(56.1)				
Total Value of Adjudication ($ 000 000)				62.8				
				(149.2)				
Observations				106,078				
R^2				0.082				

	Specification 2: Basin FE + Year FE + Soil & Climate Controls							
Insurance Price				-0.004^{***}				
				(0.001)				
	Fallow	Alfalfa	Wheat	Barley	Potatoes	Corn	Sugarbeets	Other
Post Adjudication × Crop	0.158	0.400^{***}	0.207	0.390	0.144	0.236^{*}	0.526^{*}	-0.583^{***}
	(0.225)	(0.106)	(0.169)	(0.239)	(0.254)	(0.135)	(0.297)	(0.210)
Post Adjudication × Crop (Monetized Estimates)	38.1	96.9^{**}	50.0	94.2	34.8	57.0	127.3	-140.9^{***}
	(49.6)	(45.2)	(47.6)	(71.6)	(63.8)	(43.1)	(91.0)	(48.1)
Average Partial Value Added ($ / Acre)				28.1				
				(63.1)				
Total Value of Adjudication ($ 000 000)				79.3				
				(189.7)				
Observations				106,078				
R^2				0.139				

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

- The first row of this table shows the estimate coefficients from the fractional logistic regression: $y_{i,t} = \delta \times \text{PostAdju}_{i,t} + \alpha \times \text{InsPrice}_{i,t} + X_i\beta + \gamma_t + \gamma_b + \varepsilon_{i,t}$
  Where $y_{i,t}$ is the share of the basin planted in crops (wheat, alfalfa, barley, corn, sugarbeets, potatoes and other) rather than pastureland or fallow land.
  Fixed effects are estimated with dummy variables. Standard errors are clustered two ways at the basin and year level.
- The second row shows the monetized estimates of the change in value of crops relative to pasture after adjudication: $-\frac{\text{PostAdju}_{i,t}}{\text{InsPrice}_{i,t}}$.
- The third row estimates the average per acre value added of adjudication:
  $\text{Average Partial Value Added} = \frac{1}{\sum_j Area_j} \sum_i Area_i \left[E(\max_{c=1,\dots,C}(\pi_{i,2016}(\text{PostAdju} = \text{PostAdju}_{i,2016}))) - E(\max(\pi_{i,2016}(\text{PostAdju} = 0)))\right]$
  where $E(\max_{c=1,\dots,C}(\pi_{i,t})) = \frac{1}{\alpha}\ln(\sum_{c=1,\dots,C}(\exp X_i\beta^c))$
- The fourth row estimates the total value added from adjudication
  $\text{Total Value} = \sum_i Area_i \left[E(\max_{c=1,\dots,C}(\pi_{i,2016}(\text{PostAdju} = \text{PostAdju}_{i,2016}))) - E(\max(\pi_{i,2016}(\text{PostAdju} = 0)))\right]$
  where $E(\max_{c=1,\dots,C}(\pi_{i,t})) = \frac{1}{\alpha}\ln(\sum_{c=1,\dots,C}(\exp X_i\beta^c))$

Table A1.4: Multinomial Model of Crop Choice Changes after Water-Rights are Bought or Sold

	Specification 1: Basin FE + Year FE							
Insurance Price				-0.007^{***}				
				(0.000)				
	Fallow	Alfalfa	Wheat	Barley	Potatoes	Corn	Sugarbeets	Other
After Buying Rights × Crop	0.74^{***}	0.72^{***}	0.68^{***}	0.90^{***}	0.65^{**}	0.80^{***}	0.30	0.45
	(0.21)	(0.15)	(0.22)	(0.16)	(0.26)	(0.19)	(0.18)	(0.38)
After Selling Rights × Crop	0.91^{***}	0.04	0.25	0.12	0.15	0.10	0.07	-0.79^{**}
	(0.27)	(0.17)	(0.18)	(0.36)	(0.18)	(0.20)	(0.23)	(0.37)
				Monetised Estimates				
After Buying Rights × Crop	100.8^{**}	97.3^{**}	92.3^{**}	122.5^{**}	88.4^{**}	108.8^{**}	40.5	60.6
	(51.51)	(42.46)	(50.06)	(51.00)	(47.11)	(45.03)	(30.23)	(53.23)
After Selling Rights × Crop	123.8^{*}	5.2	33.4	16.7	20.3	14.0	9.7	-107.6^{*}
	(69.45)	(23.17)	(27.66)	(50.07)	(25.75)	(27.96)	(30.60)	(56.18)
Average Partial Value Added of Transfer				84.2				
($ / Acre)				(53.3)				
Total Value of Adjudication Induced Transfers				17.1				
($ 000 000)				(10.2)				
Observations				106,078				
R^2				0.098				

	Specification 2: Basin FE + Year FE + Soil & Climate Controls							
Insurance Price				-0.004^{***}				
				(0.001)				
	Fallow	Alfalfa	Wheat	Barley	Potatoes	Corn	Sugarbeets	Other
After Selling Rights × Crop	0.64^{***}	0.63^{***}	0.54^{***}	0.73^{***}	0.51^{**}	0.91^{***}	0.20^{***}	0.58^{*}
	(0.18)	(0.13)	(0.19)	(0.17)	(0.23)	(0.19)	(0.22)	(0.34)
After Buying Rights × Crop	0.72^{***}	−0.06	−0.10	0.01	−0.22	0.02	−0.24	-0.83^{***}
	(0.21)	(0.15)	(0.13)	(0.22)	(0.17)	(0.22)	(0.22)	(0.35)
				Monetised Estimates				
After Selling Rights × Crop	161.4^{**}	157.7^{***}	136.3^{*}	183.0^{**}	127.7^{*}	228.7^{*}	50.9	145.0
	(66.74)	(61.20)	(73.95)	(73.25)	(68.97)	(79.90)	(61.67)	(94.61)
After Buying Rights × Crop	181.2^{*}	−15.0	−24.6	2.9	−54.7	5.3	−61.2	-209.5^{**}
	(93.14)	(37.12)	(32.45)	(55.31)	(42.58)	(54.62)	(56.98)	(104.97)
Average Partial Value Added of Transfer				139.2^{*}				
($ / Acre)				(67.5)				
Total Value of Adjudication Induced Transfers				30.1^{*}				
($ 000 000)				(13.2)				
Observations				106,078				
R^2				0.159				

Note: *p<0.1; **p<0.05; ***p<0.01

This Table extends binomial logistic the results in Table 10 to the multinomial case where farmers choose between different varieties of crops. This table estimates how the value of planting different crops changes after each water right transfer.

- The first set of rows in this panel of this table shows the estimate coefficients from the fractional multinomial logistic regression: $y_{i,t} = \delta \times \text{PostBuyingRight}_{i,t} + \text{PostSellingRight}_{i,t} + \alpha \times \text{InsPrice}_{i,t} + X_i\beta + \gamma_t + \gamma_b + \varepsilon_{i,t}$ where $y_{i,t}$ is the share of sub-basin planted in any crop rather than pasture or fallow land. Fixed effects are estimated with dummy variables. Standard errors are clustered two ways at the basin and year level.
- The second set of rows in each panel shows the monetized estimates of the change in value of crops relative to pasture after adjudication: $-\frac{\text{PostBuyingRight}_{i,t}}{\text{InsPrice}_{i,t}}$ and $-\frac{\text{PostSellingRight}_{i,t}}{\text{InsPrice}_{i,t}}$.
- The third row estimates the average per acre value added of adjudication:
- The fourth row estimates the total value added from adjudication

CHAPTER 2

THE DETERMINANTS OF RESIDENTIAL WATER CONSERVATION DURING DROUGHT

This chapter was co-authored with Michael Greenstone and Ludovica Gazze.

2.1 Introduction

The recent drought in California, between 2011 and 2017, was unprecedented in State's history. One measure of soil moisture deficit estimated it to be the worst in over 1,200 years (Griffin and Anchukaitis, 2014). Figure 2.1 shows a different measure, the USDA's drought monitor which put a record 78% of the state in one of the two most intense drought levels, either in 'Extreme' and 'Exceptional' drought.

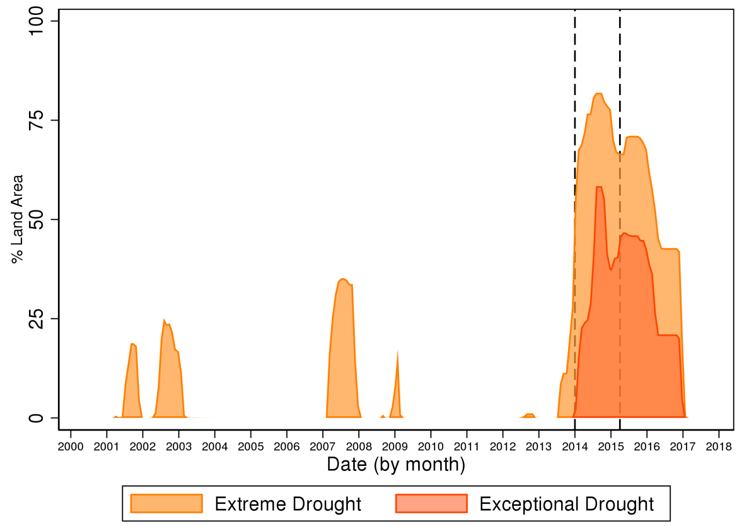
In January 2014, California Governor Jerry Brown declared a state of emergency, mandating that all users in the State reduce water use by 25% relative to a 2013 baseline. Yet, even before this announcement, there was an ongoing policy push by municipal utilities to reduce water consumption through a range of policies. These included tightening outdoor watering use restrictions, increasing rebates on water-efficient appliances, increasing rates to encourage conservation and instituting a variety of public awareness campaigns through a variety of media. Remarkably, more than half of all utilities in the state achieved their mandated goal, to reduce their residential water use by over 25%, representing a surprising public policy success.

Climate change is expected to increase the frequency and severity of droughts in California, as in many other arid regions worldwide. In light of this, as well as growing populations and the increased costs of developing new supply, it is vital to understand which of the many policy tools they have at their hands to incentivize water conservation work, and which do not. This paper seeks to answer this question, by disentangling the impacts different state and municipal conservation policies had on residential water use using a case study from a single large utility, which successfully met it's conservation goals during the recent drought.

We use a universal micro-data set from over 86,000 single family households between 2013 and 2016 from a large Californian city. During our sample period, this city implemented a suite of water conservation policies, including six rate changes, reducing the number of days that households could use water outdoors, as well as offering rebates to both purchasers of water-efficient appliances and to households replacing their lawn with drought-resistant landscaping. Simultaneously, the State of California made two large-scale regulatory announcements related to the drought; First, in January 2014, declaring a 'State of Emergency' and second, in April 2015, requiring all utilities to reduce water use by 25% relative to a 2013 baseline. The dataset we study is particularly interesting because it is of the few large cities in the U.S. to have universal adoption of Advanced Metering Infrastructure, so-called 'smart meters.' Each household's meter communicates continuously with the utility, allowing us to observe water consumption at hourly intervals. Using this high-resolution data, we draw inference about water use patterns at a high frequency and estimate the differential impacts of policies across different times of the day. We merge this household water-use data with assessor parcel data on building and land characteristics, and also block-group level census data to provide a detailed set demographic controls.

Figure 2.2 summarizes the aggregate level data in this paper. The series shows weekly average water use between 2013 and 2016. Water use is highly seasonal with a peak in summer as a result of increased use for outdoor irrigation. From 2013 to 2015 water use decreases year on year. However water-use rebounded as the drought eased in 2016. The table beneath the series summarizes the timing of the many policy changes studied in the paper. There are six rate changes, the two aforementioned State-wide regulatory announcements, and changes regulated outdoor water use schedule from one day in winter, to three days in summer prior to August 2014, changing to only two days after. With all of these policies

Figure 2.1: California Drought Severity, 2000 to 2018



Data from the United States Drought Monitor. Figure shows the percent of California in moderate to severe drought from January 2000 to February 2018.

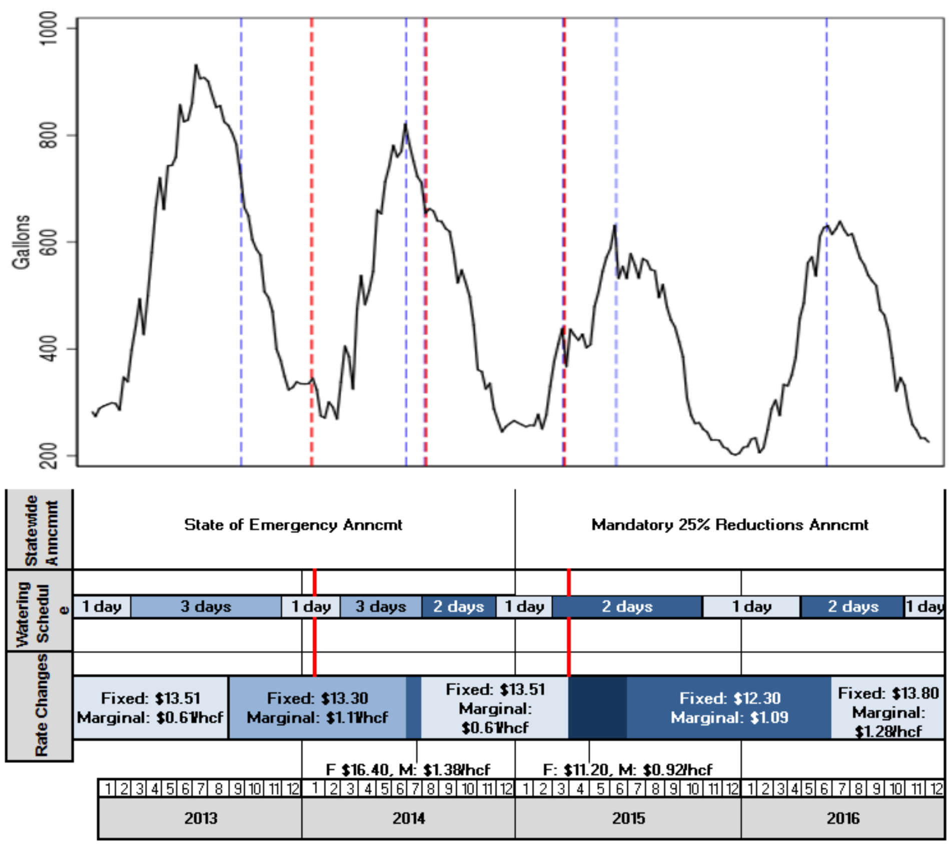
changing simultaneously, and for all households in the city, the challenge in this paper is to disentangle their impacts.

In our empirical analysis, we control for long-term trends using year fixed effects and for seasonality using 'week of year' fixed effects and weather controls. These fixed effects may absorb part of the impact of these policies, making our approach relatively conservative. Nonetheless, we still find that state and local policies had significant impacts on water use.

We find the following results: First, a 10% increase in marginal rates is associated with a decrease of 20 gallons per day in average water use. Overall, we find that rate changes during our sample period account for a 5% decrease in use (25 gallons/day).

Second, we study a change in the outdoor water schedule regulations that reduced the number of days water-use was permitted outdoors. After this policy, households substitute water use gallon-for-gallon from banned days to still-permitted days. However, water-use persistently decreases by 6% (30 gal/day) after the schedule change. Puzzlingly, this reduction is concentrated during hours when outdoor use was never permitted; perhaps suggesting

Figure 2.2: Policy Changes and Average Water Use, 2013 to 2016



Series shows average daily single family residential water use. Red lines correspond to the dates of State-wide drought announcements. Blue lines corresponds to the dates of rate change occurred.

that households might have reacted to the announcement of the schedule change by conserving water, rather than the schedule change itself.

This reaction is consistent with our findings the impacts of State-wide regulatory announcements: we observe reductions in average daily water use of 74 and 44 gallons/day after the "State of Emergency" and "Mandatory Restrictions" announcements, respectively. Together, these policies represent a 22% decrease in water-use. To interpret this result, we observe that these major announcements appear to induce interest in the California drought as measured by a Google search index for the keyword "drought." A mediation analysis shows that our measure of drought awareness is highly correlated with water use, but, after controlling for city and state policies, the effect disappears.

Finally, households who install water-efficient toilets and drought-resistant lawns through a rebate program see substantial reductions in water use (55 gallons/day) from toilet and lawn rebates. However, due to low-take up rates, the aggregate impacts of these policies are negligible.

This paper is the first to use micro-level data to study the impact of the most recent 2011 to 2017 drought on residential water consumption. However, there is research on household responses to past droughts in California, particularly the drought between 1986 and 1992 (Pint, 1999; Berk et al., 1993);

There is also already a sizable literature which discusses separately the impacts of many of the policies covered in this paper; however, this is the first study to look at the effect of all of these policies has simultaneously. Our findings also replicate many results from past papers; for example, Jorgensen, Graymore and O'Toole (2009) show that high-income households use water more than those of lower income, and that this fact is primarily a result of higher outdoor water use.

Bennear, Lee and Taylor (2011); Lee, Tansel and Balbin (2011, 2013) show that there is a rebound effect for water-efficient appliances, for example after installation, some papers find consumers respond to a perceived lack of effectiveness of high-efficiency devices by using them for longer or more frequently, dampening the conservation benefits. Attari (2014) also shows that consumers often underestimate potential savings from efficient appliances. Like much of the previous literature, we find that rate changes have a significant impact on water consumption. However, some studies have found that customers respond to average rather than marginal prices (Ito, 2013) and others have argued that similar levels of conservation can be achieved in a more equitable way using non-price instruments (Olmstead and Stavins, 2009). However, Wolak (2016) shows that if rates can be individualized, they can be chosen to maximize revenue and address equity concerns.

Mandatory, rather than voluntary restrictions appear more effective in reducing water-us e(Kenney, Klein and Clark, 2004); however, (Castledine et al., 2014) show that allowing customers the flexibility to choose the days they use their allowance can be more effective than rigid scheduling .

Increasing public enforcement of regulations, like those studied in this paper, may act as a deterrence and increase compliance. However, there is no direct evidence of this deterrence effect in the water sector. Although, there are similar findings in similar areas such as speed limit enforcement (de Waard and Rooijers, 1994) and tax audits (Slemrod, Blumenthal and Christian, 2001). Finally, providing consumers with comparisons to the water use of their neighbors is an effective and lasting method of reducing water-use (Ferraro, Miranda and Price, 2011).

The rest of this paper proceeds as follows: Section 2.2 describes the data. Section 2.3 studies each candidate policy individually and looks for micro-evidence that this policy had an impact on patterns of water use. Section 2.4 shows that 'drought' related Google searches are highly correlated with patterns of water use and examines whether these searches could be an indirect channel by which the aforementioned policies affect water use. In Section 2.5, we simultaneously estimate the impact of the previously discussed policies using a regression framework. Finally, Section 2.6 conducts an accounting exercise, which adds up all of the estimated policy impacts and tests to what extent they explain the observed changes in water use over the course of our sample. Section 2.7 concludes.

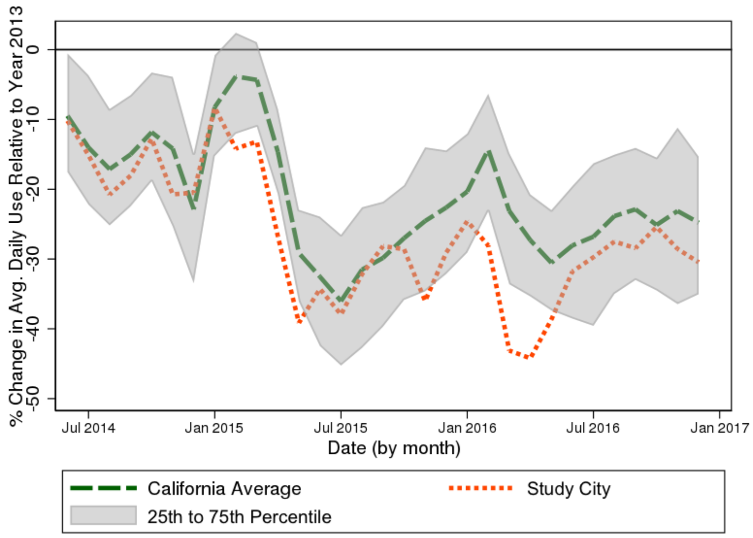
2.2 Data

We observe hourly water use for the universe of single family households in one of the five largest cities in California. In cleaning data, we drop all newly constructed houses, and all households where the account changes address between the beginning and end of the data. We also drop all hours where frequent smart-meter transmission malfunctions lead to implausible estimates of water use more than 5 standard deviations from the households average. This leaves us with 26,280 hourly observations for 86,000 households. We merged this water-use data with address-level house and land parcel characteristics data from assessor files. We also merge the data with census-block level household characteristics.

Figure 2.3 shows the percentage change in water use, relative to a 2013 baseline, in both the city we study and the state of California as a whole. The figure uses data reported to California's State Water Conservation Board as part of mandatory reporting regulations beginning in State in July 2014. The shaded grey area denotes the savings between the 25th and 75th percentiles of all utilities. The city we study persistently had larger than average conservation, and the level of conservation for much of 2016 was even above the 75th percentile of all utilities in California. In both 2015 and 2016, more than half of all cities in California achieved their mandated goal of a 25% reduction in water use on a 2013 baseline.

Appendix table A2.1 shows how average water use and savings breaks down by different household demographics. Households with higher baseline water use tend to have larger savings in response to conservation policies. Furthermore, these households also tend to be larger homes, with large yards, in richer, whiter and more educated neighborhoods.

Figure 2.3: Water Conservation in Study City compared to California



The two series compare the rates of water conservation within our study city and the State of California as a whole. Monthly conservation is calculated relative to a 2013 baseline. Shaded grey area corresponds to 25-75th percentile of conservation rates within California utilities.

We approximate whether a household pays their own water bill by using data on whether households have their water bill sent to the same address as their meter. Using this proxy, we find that households who pay their own water bills both use more water in the baseline, and have higher savings. All of these results are consistent with findings in past literature such as Berk et al. (1993); Jorgensen, Graymore and O'Toole (2009); Willis et al. (2013); Wolak (2016).

We also merge climate and precipitation variables into our regressions. In particular, in any specification described as 'including climate controls' we include dummies variables for weeks of the year with high temperatures over 95 and over 100 degrees Fahrenheit respectively, and we add dummy variables for weeks with any measured precipitation, and with totals greater than 2mm and 5mm respectively. In general, changing the specification of these climate controls does not significantly change results.

2.3 Candidate Policies Driving Water Conservation

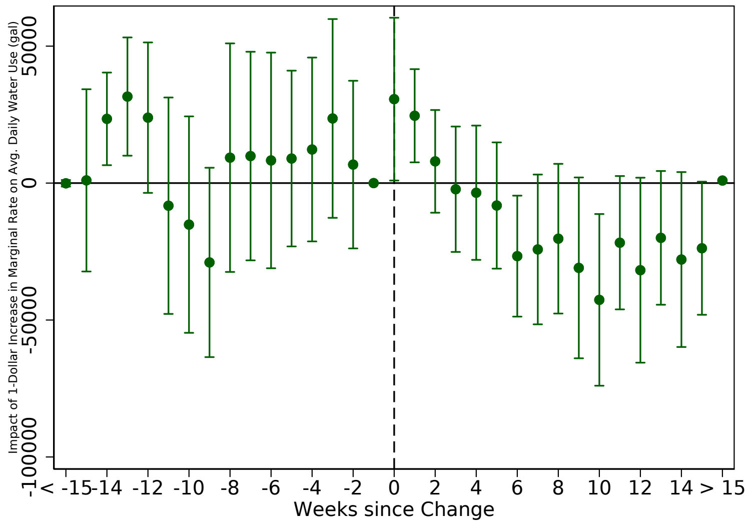
The following sub-sections discuss, the impact of each candidate conservation policy on household water-use. We harness high-frequency nature of the data to look for evidence of the impacts of each of these policies. We take different empirical approaches to each policy depending on the identifying variation. Typically we use an event-study framework, using week-of-year and year fixed effects to partial out time series variation; and when available we use a difference-in-difference between households to partial out other idiosyncratic variation. We study the impact of the following policies in turn: rate changes, reducing the number of outdoor watering days, increasing enforcement of outdoor watering restrictions, rebates for water efficient appliances and state-wide regulatory announcements. Later, in section 2.5, we pull all of these policies together to estimate their simultaneous impact.

2.3.1 Rate Changes

First, we estimate the impact of rate changes on household water-use. Because the rate changes will impact all households in the city simultaneously, the estimates are identified entirely off of time series variation. However, because there are six different rate changes at different times of year between 2013 and 2016, the estimates are identified off the differences in water use, before and after a rate change, compared to a similar year in which there was no such change. The magnitude and timing of all of these rate changes are indicated in Appendix Figure A2.1. On average marginal rates increased over the period of study; however, there are instances of both marginal and fixed rates falling, most notably in August 2014 when a rate increase was rescinded after local political pressure.

To determine the impact of these rate changes on water consumption, we estimate the following equation:

Figure 2.4: Event study - The Impact of *Marginal* Price on Water-Use



Event-study figure illustrating the impact on water use of all changes in marginal water prices that took place over the sample period. The event-study includes weather controls, a control for whether summer watering schedule is in place and fixed effects in year and week of the year. Standard errors are two-way clustered at household-week level.

$$y_{c,i,t} = \sum_{t=-15}^{15} \beta_t \frac{\mathbb{I}_{c,t}^{\text{Weeks Post-Change}}}{\Delta_c^{\text{Marginal Rates}}} + \gamma_{woy} + \gamma_{yr} + \gamma_i + X_t\theta + \varepsilon_{c,i,t}$$

Where for every individual i and for every rate change they face c: $\mathbb{I}_{c,t}^{\text{Weeks Post-Change}}$ is a dummy variable that is true if it is t weeks since the policy was enacted, $\Delta_c^{\text{Marginal Rates}}$ normalizes the estimate by the size of the marginal change, so that the coefficients β_t can be interpreted in terms of the impact of a \$1 increase in marginal rates. Finally, $\gamma_y r$ and γ_{woy} are fixed effects that control for year and week-of-year, X_t are controls for concurrent policy changes (the ones discussed later in this section) and for temperature and precipitation.

Figure 2.4 is an event-study figure that plots every coefficient β to show how patterns of water use changed in the weeks leading up to and following a rate change. It appears that water use decreases after a rate increase; however, there is not enough power to statistically distinguish the impact from zero.

In this paper, we focus on demand responses to marginal price, which strong basis in economic consumer theory, and not average price. However, there is an empirical literature which finds that customers are more likely to respond to average price; for example Ito (2013). In the appendix table A2.2, we do replicate figure 2.4 using average prices and find results that are similar. In this analysis, average prices are calculated for the entire period using that household's baseline water use as the quantity. However, it should be noted that changes in average price are largely reflected by changes in marginal price because the changes in fixed costs are reasonably small. Furthermore, because we do not study a city with an increasing block-rate structure, there are no cross-sectional discontinuities in average price we can exploit.

Despite relying entirely on time series variation, we feel comfortable that these estimates identify the impact of rate changes on water-use for several reasons. Firstly, we have a relatively large number of rate changes, which occur in different parts of the year and some occur in a plausibly exogenous manner, reducing the likelihood that our estimate arises only because of some idiosyncratic shock. Second, in Appendix Table A2.2, we interact our demand estimates with demographic characteristics such as household income and baseline water use. We find that demand elasticity is much higher for high-income households and the most prolific baseline water users, which is precisely the result we would expect to see if our coefficient estimates do in-fact represent demand responses to increased rates.

2.3.2 Reducing Number of Permitted Outdoor-Watering Days

The second set of policies we evaluate are regulations that restrict the 'time-of-day' and 'day-of-week' that households are permitted to use water outdoors. These regulations are ubiquitous throughout California and other drought-prone States and typically target turf (lawns) irrigation, which are the single largest end-use of residential water. Even before the State of California made such restrictions mandatory in April 2015, 70% of Californians already lived within utilities who applying some restrictions on how water could be used outdoors[1]. Typically, these policies restrict outdoor water use to only nights and evenings, when rates of evaporative losses are lowest, and also restrict the number of days each week households can use water outdoors. Many utilities require odd and even numbered houses to use water outdoors on different days of the week, to reduce the load on the storm-water system.

Advocates argue these regulations nudge households to irrigate their lawns in the most efficient ways by preventing them from over-irrigation or losing water to evaporation (Kenney, Klein and Clark, 2004). However, if households can freely substitute water use between hours or days, then it is not clear whether that tightening these regulations will reduce aggregate water use Caselli and Coleman (2013).

This subsection focuses on policy change in August 2014, which reduced from three to two the number of days households were permitted to use outdoors during the summer. The exact policy before and after this change is described in Appendix Table A2.3. Prior to August 2014 in the city we study, during the summers even numbered houses were permitted to use water outdoors on Wednesday, Friday and Sunday and odd numbered houses were permitted to use water outdoors on Tuesday, Thursday and Saturday. On Mondays, all households were banned from using water outdoors regardless of their house number. After August 2014, Thursday and Friday also became banned days for outdoor water use for odd and even numbered households respectively.

In our analysis, we use the differences in water use between even- and odd-numbered households as a proxy for outdoor water use. Even-numbered houses make natural control groups for odd-numbered houses on the days that odd-numbered houses are permitted to use water outdoors and vice-versa for even numbered houses. Because even- and odd-numbered households are uniformly distributed throughout the city, they are balanced on observable characteristics and presumably also on unobservable characteristics.

1. Authors' calculation based on water conservation reporting data from California's State Water Resource Control Board.

Figure 2.5: Substitution After Outdoor-Watering Banned on Thursdays and Fridays

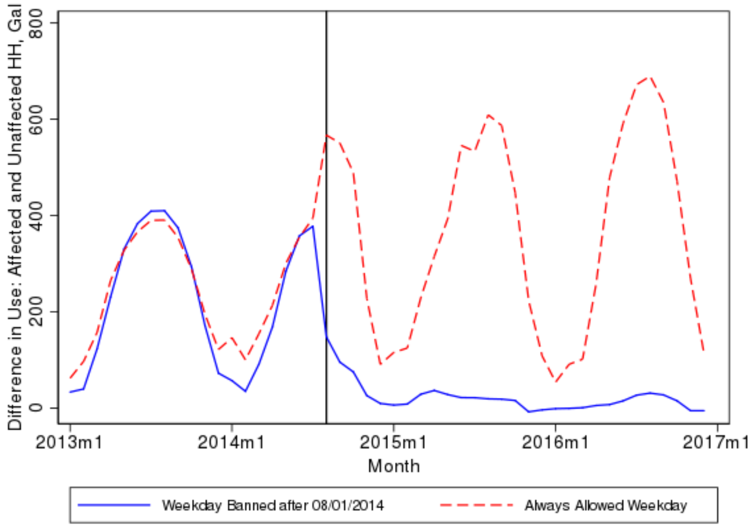


Figure plots the raw difference in water use between even- and odd-numbered households on (1.) Fridays (blue), the weekday that became banned for even-numbered households (it was always allowed for even-numbers but banned for odd-numbers) and (2.) Wednesday, a day that remained allowed for even-numbers throughout and banned for odd-numbers throughout.
Blue line shows a drop in differential water use consistent with even numbers ceasing outdoor water use after the schedule change. However, red line shows an increase in water use after the schedule change indicating substitution of water use from Friday to Wednesday.

If we assume that all households comply with this regulation, then the difference between even-numbered and odd-numbered household water use is entirely accounted for by water use outside. In practice, some households will not comply with this regulation, instead they will use water outdoors on days they are not permitted to leading us to understate true outdoor water use[2].

Figure 2.5 plots the difference in water use between even and odd numbered households on Wednesdays and Fridays from 2013 to 2016. Before August 2014, even-numbered houses were permitted to use water outdoors on both of these days while odd-numbers were not,

2. This claim requires there to be more households who 'always' use water outdoors regardless of their schedule than their are 'dissenting' households who always follow the opposite of their schedule.

thus the difference between even and odd water use reflect the outdoor water use of the even-numbered households. However, after the ban on outdoor water use in August 2014, the blue line falls to zero as even-numbered houses stop using water on Fridays. Simultaneously, there is a substantial increase in water use on Wednesdays as households substitute their outdoor water-use towards other days of the week. Outdoor water use on the two remaining permitted days of the week increases by approximately 50%, and therefore appears to completely offset the savings from banning outdoor water use on the third day.

We can utilize the hourly resolution of our data to unpack further the impact of tightening these outdoor water restrictions had on water-use, for each type of day, and at each hour of the day. To do this, we restrict our data to the summer months when outdoor water restrictions are in place. Then we stratify our data into the 24 hours of the day and estimate the following equation for each hour of the day:

$$\begin{aligned} y_{i,t} =& \beta_0 + \beta_1 \text{BannedDay} + \beta_2 \text{AlwaysPermitted} \\ &+ \beta_3 \text{PostBan} + \beta_4 \text{BannedDay} \times \text{PostBan} + \beta_5 \text{AlwaysPermitted} \times \text{PostBan} \\ &+ \gamma_i + \gamma_{dow} + \gamma_{woy} + \gamma_{yr} + \varepsilon_{it} \end{aligned}$$

In a difference in difference framework, the coefficient β_3 is interpreted as the impact of the ban on water use during days when outdoor use was never permitted[3]. $\beta_3 + \beta_4$ is the effect of banning on outdoor use on days that become banned [4]. And $\beta_3 + \beta_5$ is the effect of banning on outdoor use on days that are always permitted[5]. The specification also contains individual fixed effects as well as a rich set of fixed effects for the day-of-week, week-of-year, and year. As such, the relative variation identifying coefficients is as follows: The difference

3. Never Permitted is Monday, Wednesday, Friday, and Sunday for odd-numbers and Monday, Tuesday, Thursday, and Saturday for even-numbers.

4. Eventually Banned is Thursdays for odd-numbers and Friday for even-numbers.

5. Never Banned is Tuesdays and Saturdays for odd-numbers and Wednesday and Sunday for even-numbers.

Figure 2.6: Substitution After Outdoor-Watering Ban - Stratified by 'Hour-of-Day'

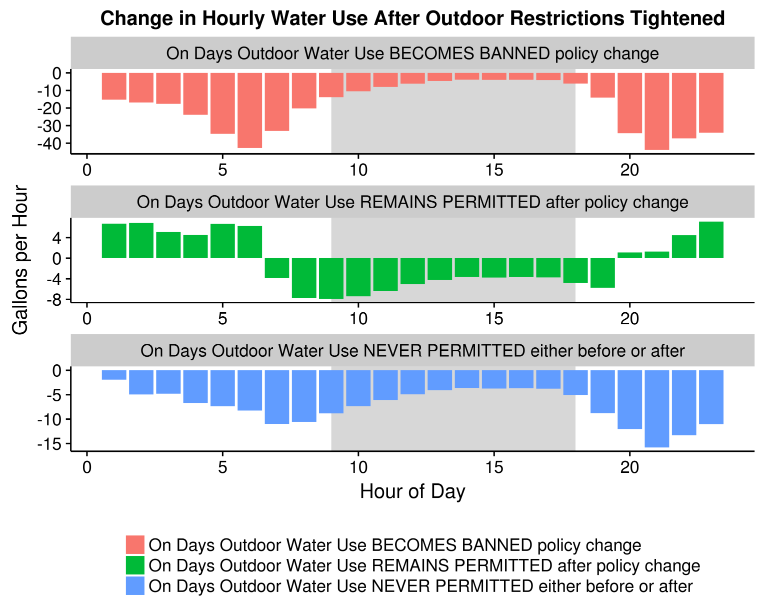


Figure estimates the change in water use after moving from a three-day to two-day watering schedule. Regression is estimated separately for each type of day and every day of week. Outdoor water-use is always banned during daytime hours shaded grey.

in water-use, before and after the ban, between an even-numbered house that is permitted to use water outdoors before but becomes banned after, and the similar odd-number house that is always banned; and vice versa.

The results of all these regressions are presented in Figure 2.6. The gray shading in this figure covers daytime hours (9 a.m. till 7 p.m.) when outdoor use has never been permitted. The first panel in this figure shows a decrease in water use across all hours of the day during days that become banned; as expected, these decreases are concentrated during nighttime hours when turf irrigation previously took place. However, the second panel shows these reductions are partially offset by increases in water use during on days where outdoor water use remained permitted during similar night time hours, consistent with our daily results.

The third panel is most puzzling, it shows a substantial and unexpected reduction in water use, especially during night-time hours on days that were banned both before and after the outdoor water restrictions were tightened. One possible interpretation of this result is that there is increased compliance with outdoor use regulations after the schedule change. Another interpretation might be that the schedule change led to increased public-awareness of the drought, which led households to conserve water in other ways.

2.3.3 Strengthening Enforcement of Outdoor-Watering Restrictions

In addition to tightening outdoor water-use restrictions, the city we study also cracked down on the enforcement of these restrictions. Individuals who were caught in violation of the law, by using water outdoors outside of their permitted schedule, or who violated city the regulations in other ways received a $45 fine. These regulations were enforced by a team of city water representatives who patrolled the city, often at night, issuing notices to individuals violating water-use regulations. The particular city we study issued more fines than almost any other in the State of California. Violators who received their first notice were given the option of having their fine waived if they agreed to a household water audit. Typically, a crucial component of these audits was to have the representative help the household reset the timers on their automated lawn sprinklers systems. Mis-set sprinkler timers are thought to be a key driver of water wasted water in the City. In our dataset, we observe 12,915 fines, and 4,117 household water audits between January 2013 and December 2016[6]. In this section, we estimate the impact that receiving one of these notices has on water consumption.

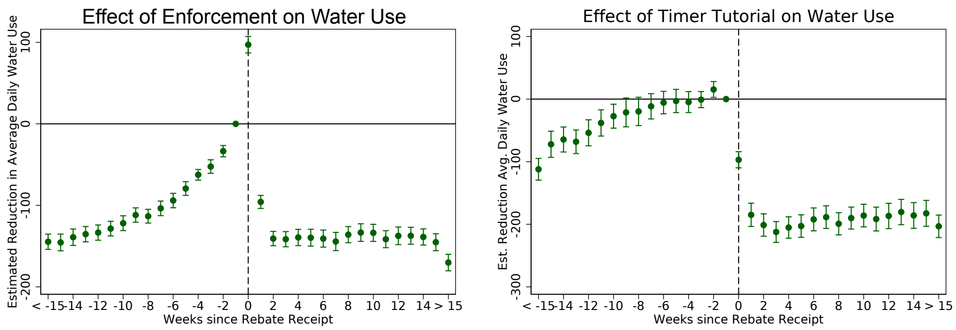
Figure 2.7 estimates the following event study equation to estimate the impact that receiving a fine or a household water audit has on subsequent water use[7]:

$$y_{f,i,t} = \sum_{t=-15}^{15} \beta_t \mathbb{I}_{f,t}^{\text{Weeks Post-Enforcement}} + \gamma_{woy} + \gamma_{yr} + \gamma_i + X_t\theta + \varepsilon_{f,i,t}$$

6. In the event-study analysis, violations in the first and last 15 weeks of the dataset are dropped leaving observations for 11,086 fines and 3,658 audits/timer tutorials.

7. The notation is the same as previously defined: For every individual i and for every fine they face f, $\mathbb{I}_{c,t}^{\text{Weeks Post-Enforcement}}$ is a dummy variable that is true if it is t weeks since the policy was enacted. $\gamma_y r$ and γ_{woy} are fixed effects that control for year and week of year, X_t are controls for concurrent policy changes (the ones discussed elsewhere in this section) and for temperature and precipitation.

Figure 2.7: Event Study - The Impact of Receiving a Fine or Timer Reset on Water-Use



Left panel is an event-study which estimates the impact of receiving a fine for violating outdoor water-use restrictions on average daily water use. Right panel estimates them impact of receiving a household water audit and sprinkler timer tutorial. The event-study includes weather controls, a control for whether summer watering schedule is in place and fixed effects in year and week of the year. Standard errors are clustered at household-week level.

To produce Figure 2.7, we restrict the data sample to only households who ever receive fines or household audits respectively. In this analysis, the hypothetical control group is households who later or earlier received a notice, but did not in that particular period. Unfortunately, the sharp upward pre-trend in these figures suggests that houses the hypothetical control-group had systematically differing trends in water use in the weeks leading up to the fine. These pre-trends occur because households who receive a fine may develop a pattern of non-compliance in the weeks leading up to their eventual notice. However, this is not observed concurrently among households who did not receive a fine at that time, but potentially developed such a pattern of violations later on. Because of these pre-trends, we estimate the long-term impact of these fines to be much smaller than the observed discontinuity in Figures 2.7 around event-time zero.

Receiving a fine is associated with a short-term 250-gallon/day decline in water use within two weeks. However, the long-term reduction in water use is much lower, closer to 40 gallons. Similarly, accepting a water audit and timer tutorial leads to a short-term 200-gallon/day decline in water use within two weeks. However, the long-term effect reduction is closer to

100 gallons per day. The long-term impact of receiving timer tutorials is more persistent than the impact of receiving a fine. This might occur because a timer-reset is a one-time intervention that will mechanically lead to persistent water savings so long as the timer is not reset again, whereas other types of conservation might require more effort to create a systematic pattern of behavior change to reduce water-use.

2.3.4 Water-efficient Appliance Rebates

The fourth policy lever the city used to incentives water conservation was to offer rebates for households who installed water-efficient appliances. The city's rebate program began in 2006 and offers residential households rebates for $2 per square foot for lawn replacement, $50 per unit for water-efficient toilets and 100 $ per unit for water-efficient clothes washers[8]

These rebates can be redeemed simultaneously with another rebate through California's State-wide 'Save Our Water' program. Anecdotally most households do this, although we do not have data from that program to back up this claim. The Save Our Water rebates are worth $100 per water-efficient toilets and $2 per acre for lawn replacement. There is no corresponding rebate for water-efficient clothes washers. Typically customers receive both sets of rebate forms from either the vendor or installing contractor who will allow them to fill out and submit the rebates at the time of installation. Entry-level water efficient toilet costs between $100 to $300, so taken together these rebates could cover the entire cost of the toilet. However, buyers of a toilet still need to pay for the labor required to install the toiled (around $250 to $300 for a straightforward install, and potentially significantly more depending on any complexities in the job). Clothes washers sell for between $500 to $1000, so the rebate is a much smaller fraction of the sticker price; however, they can also typically be installed the assistance of a plumber. Lawn replacements are highly heterogeneous in

8. The city also offers rebates for a variety of other products; however, too few of these rebates were taken up to allow a statistical analysis. These rebates include rain sensors, sprinkler nozzles, pool covers, moisture sensors recirculating hot water pumps, micro-irrigation conversion, evaporative coolers. Furthermore, there is also a commercial and industrial rebate program, in addition to the residential one.

terms of costs and effectiveness, depending both on the nature and topography of the lawn, and also the type of landscape that replaces it. Consequently, the change in data on changes in water-use after a lawn replacement is extremely noisy, making it challenging to evaluate their effectiveness.

Rebate program began in 2006 and has grown every year since then. For the period of our study (2013-2016) we observe 585 toilet rebates 2,034 clothes washer rebates and 231 turf replacements[9].

Figure 2.8 is another, which estimates the impact that receiving a rebate for either installing a toilet, clothes washer or turf replacement has on subsequent water use[10]:

$$y_{r,i,t} = \sum_{t=-15}^{15} \beta_t \mathbb{I}_{r,t}^{\text{Weeks Post-Rebate}} + \gamma_{woy} + \gamma_{yr} + \gamma_i + X_t\theta + \varepsilon_{r,i,t}$$

The event-study results are presented in Figure 2.8. Toilet rebates result in a persistent reduction of water use of around 50 gallons per day[11]. However, we cannot detect a significant change in water use after the install of water-efficient clothes washers. For turf-replacement, the point estimates suggest a reduction in water use of around 40 gal/day for households, however due to a limited sample, heterogeneous outcomes and pre-trends the results are difficult to interpret.

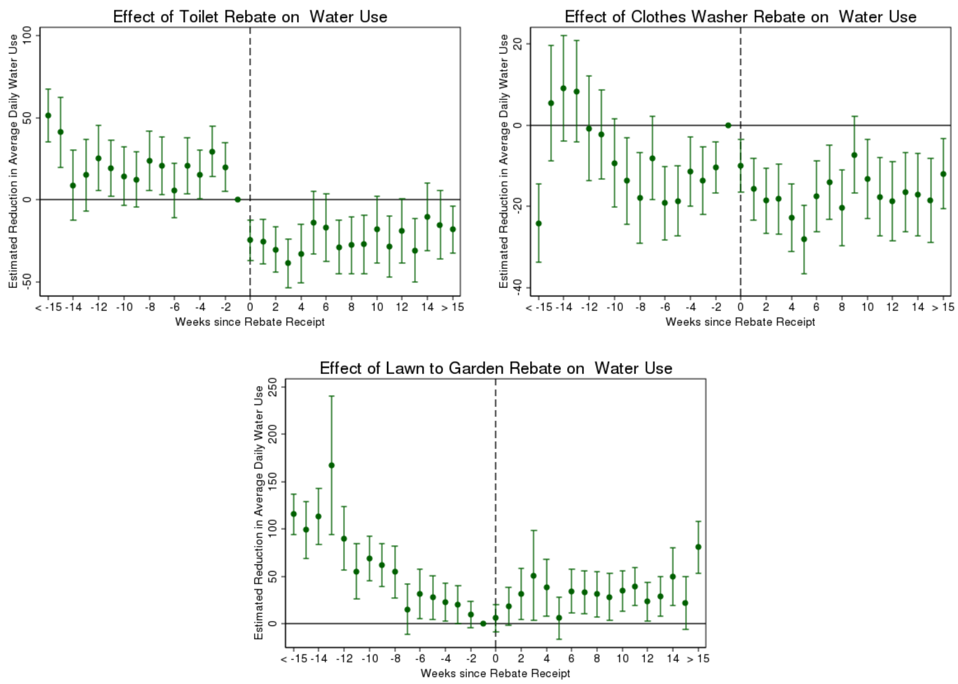
Unfortunately, these estimates can only tell us whether or not installing a water-efficient appliance as part of a rebate program will lead to a reduction in water use. They cannot tell us whether the money spent on rebates was effective at incentivizing households to take-

9. In the event-study analysis, rebates in the first and last 15 weeks of the dataset are dropped leaving observations for 1,516 washers, 508 toiled rebates and 191 lawn rebates.

10. The notation is the same as previously defined: For every individual i and for every rebate they receive r, $\mathbb{I}_{r,t}^{\text{Weeks Post-Rebate}}$ is a dummy variable that is true if it is t weeks since the policy was enacted. $\gamma_y r$ and γ_{woy} are fixed effects that control for year and week of year, X_t are controls for concurrent policy changes (the ones discussed elsewhere in this section) and for temperature and precipitation.

11. The data on the rebate forms is highly incomplete; however, if we assume that these rebates reduced the per flush water use from 10 gal/flush to 4 gal/flush this would imply the toilets are being flushed about 8 times per day

Figure 2.8: Event study - The Impact of Receiving a Water-Efficient Appliance Rebate on Water-Use



Top-left panel is an event-study which shows the impact of receiving a water-efficient toilet rebate on daily water use. Top-right panel for receiving an efficient clothes-washer rebate. Bottom-center panel is for recieving a Lawn to garden conversion rebate. The event-study includes weather controls, a control for whether summer watering schedule is in place and fixed effects in year and week of the year. Standard errors are clustered at household-week level.

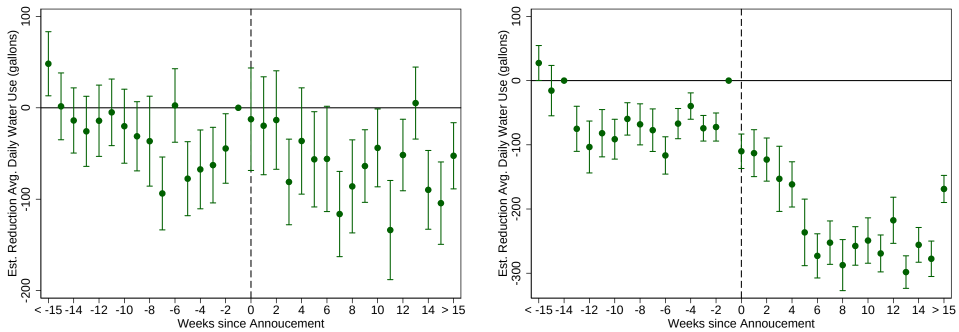
up rebates. If all households who took rebates would have chosen to install the appliances anyway in the absence of the program, then the rebate money might not be well spent. To estimate a demand elasticity for the price of rebates, we would need price variation, which does not exist in our sample. However, if we assume that all households that take up the rebates were infra-marginal, which is to say that they would not have installed the appliance in the absence of the subsidy and if we assume that the marginal price paid by consumers full reflects the social cost of developing new water supply, then we can show that toilet rebates have a positive rate of return of 11%. On the other hand, if we assume that only 20% of customers are 'marginal' in the sense that they only 20$ were induced to purchase an appliance because of the rebates, then the corresponding rate of return for toilet rebates only 3.1%.

2.3.5 Announcement of State-wide Conservation Regulations

In additional to policies adopted at the municipal level, two key statewide policies helped shape California's response to the drought at a state level. Firstly on January 17, 2014, Governor Jerry Brown announced that he placed the entirety of California in a 'State of Emergency' as a result of the drought. The 'State of Emergency' allowed the State to access federal disaster relief funds and gave the State additional jurisdiction over local water institutions to manage water supply. However, as we will show, one of the key impacts of the announcement was to increase public awareness of the drought. The 'State of Emergency' persisted until April 7, 2017, for most of the State, although it remained longer for some counties.

The second statewide policy announcement came on April 1, 2015. This announcement gave rise to the first-ever State-wide mandatory water-use restrictions and gave the California unprecedented authority to regulate local water utilities. All utilities were compelled to report their water use every month to the State and were required to reduce their water use

Figure 2.9: Event study - The Impact of Announcement of State-Wide Regulations on Water-Use



Left panel is an event-study for the *State of Emergency* declaration. Right panel is for the *Mandatory Conservation Regulations*. The event-study includes weather controls, a control for whether summer watering schedule is in place and fixed effects in year and week of the year. Standard errors are clustered at household-week level.

by 25% relative to their 2013 Baseline [12]. In 2015, 57% of the utilities in California achieved this goal[13].

These policies were one-time announcements and simultaneously affected all households in our data, which implies that our estimates are identified entirely by time series variation. Notwithstanding this, We estimate the impact of these using the same empirical framework as in the previous subsections[14]:

$$y_{a,i,t} = \sum_{t=-15}^{15} \beta_t \mathbb{I}_{a,t}^{\text{Weeks Post-Announcement}} + \gamma_{woy} + \gamma_{yr} + \gamma_i + X_t\theta + \varepsilon_{a,i,t}$$

12. In addition to this, this regulation also directed the creation of a temporary, statewide consumer rebate program to replace old appliances with more water and energy efficient models; Required campuses, golf courses, cemeteries and other large landscapes to make significant cuts in water use; Prohibit new home developments from irrigating with potable water; Banned watering of ornamental grass on public street medians; Prohibited the serving of water in restaurants to customers who had not specifically requested it, and a spate of other regulations.

13. Authors' calculation based on State Water Resource Control Board data on Conservation Reporting

14. The notation is still the same as previously defined: For every individual i and for every announcement a, $\mathbb{I}_{a,t}^{\text{Weeks Post-Announcement}}$ is a dummy variable that is true if it is t weeks since the announcement. $\gamma_y r$ and γ_{woy} are fixed effects that control for year and week of year, X_t are controls for concurrent policy changes (the ones discussed elsewhere in this section) and for temperature and precipitation.

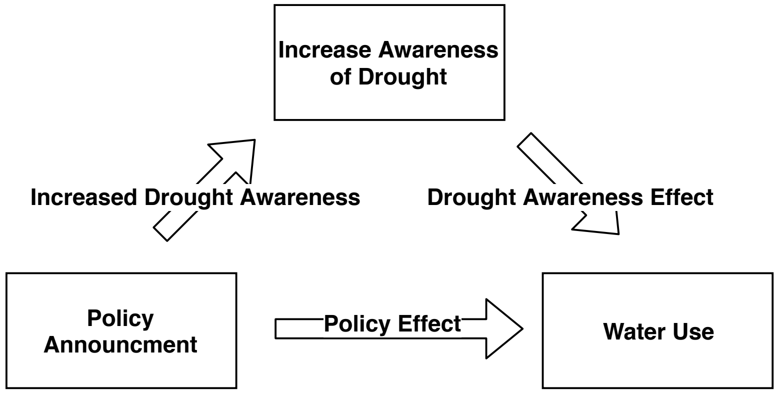
The event-study results are presented in Figure 2.9. Although the results are noisy, it appears that these policy announcements were associated with reductions of water use between 50 and 100 gallons per day. The Mandatory Conservation Measures seem to both have a more substantial impact than the State of Emergency announcement and also to be cleanly estimated. These announcements were effectively a large bundle of state-wide policies, and although they significantly affected household water use, it is not clear by which mechanism they had an effect. We attempt to unpack this in the next section, by looking this impact was mediated by public interest in the drought.

2.4 Does Public Awareness Mediate Policy Impacts?

Public interest in an ongoing drought is likely to be a key driver of conservation behavior. However, drought can generate public interest for multiple reasons: Firstly, general news and media coverage of drought can increase public interest, or secondly, interest can increase because of concerted public information campaigns on behalf of state and local governments. Furthermore, because elevated media attention and public-awareness campaigns and other policies all occur simultaneously, it is difficult to disentangle how specific policies drive reductions in water-use. Do they lead customers to change their behaviors in the manner intended? Or is the observed concurrent conservation when a policy is adopted merely a response to heightened public awareness? To highlight this problem Figure 2.10 shows the two possible channels through which policy announcements could reduce water use. Either a policy can reduce water use because it has it's intended 'policy effect' directly on water use, or alternatively, the policy announcement might increase drought awareness and lead to an indirect 'drought awareness effect' as a result of other conservation actions customers take as a result of heightened drought awareness.

To explore this idea further, we construct a measure of interest in the drought using Google Trends to create a weekly index of the number of searches within our city containing

Figure 2.10: Mediation Diagram



Conceptual diagram for mediation analysis.

the word "Drought". In Figure 2.11, we remove seasonal trends and then plot this measure of drought-interest against water-use. As shown in the figure there appears to be a strong negative correlation (0.5) between average water use and interest in the drought.

We conduct a mediation analysis to examine whether changes in drought interest could be the mechanism through which the policy changes we previously studied affected policy. First, to explore whether our policies are correlated with increased interest in drought, we estimate the following the regression:

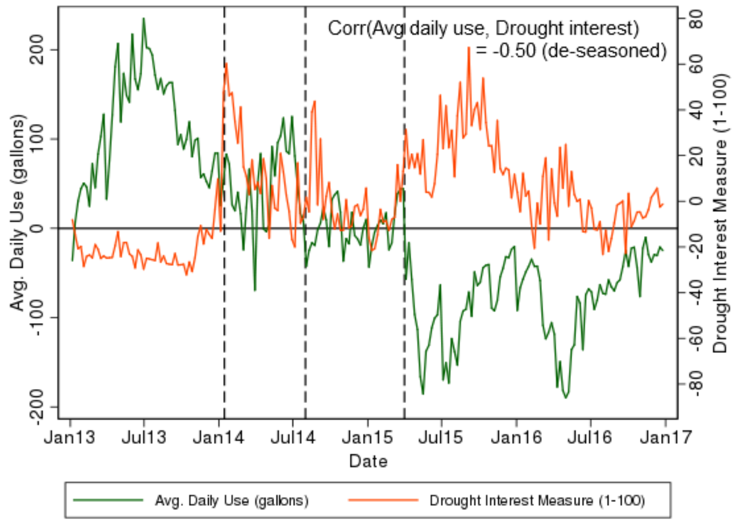
$$\text{Drought Interest}_{i,t} = \theta \text{Policies}_{i,t} + \gamma_{woy} + \gamma_{yr} + \gamma_i + X_t\theta + \varepsilon_{i,t}$$

Second, we explore whether including drought interests in our regression changes the direct impact of the policies on water use.

$$y_{i,t} = \beta^{\text{Direct}}\text{Policies}_{i,t} + \beta^{\text{Awareness}}\text{Drought Interest}_{i,t} + \gamma_i + \gamma_{woy} + \gamma_{yr} + \varepsilon_{i,t}$$

To interpret the direct effect β^{Direct}, and the indirect effect $\theta \times \beta^{\text{Awareness}}$ in a causal manner, we need to make very strong assumptions which probably don't hold in this context. However, this mediation exercise is still useful to think about the extent to which policy

Figure 2.11: Drought Interest and Water-Use Series (Deseasoned)



Green line is de-sesoned and average daily water use, Orange line is de-seasoned Google Trends index of for 'Drought'. These two series are negatively correlated, with a correlation coefficient of -0.5

outcomes are correlated with drought interest[15].

Table 2.1 presents the results of the mediation analysis. In column 1, we see that all of the policy variables studied in this paper; rate changes, changes in watering schedule and statewide emergency announcements are significantly correlated with our search measure of drought interest. In fact, changes in these policies explain about 73% of the variation in the drought interest measure. In column 2, we see that the drought interest measure is also strongly correlated with average daily water use. However column 4, shows that once we

15. In particular, Imai, Keele and Yamamoto (2010) show that outcomes can be interpreted as causal if

$$Y_i(policy, droughtinterest), \text{DroughtInterest}_i(policies) \perp \text{Policies}_i | \text{CONTROLS}i_{=}control$$

and

$$Y_i(policy, droughtinterest) \perp \text{DroughtInterest}_i(policies) | \text{CONTROLS}i_{=}control, \text{Policies}_i = policy$$

, which is to say that (1.) the relationship between water use and policies and also the relationship between drought awareness and policies are uncorrelated with unobserved factors which influence both policies and outcomes and (2.) that the relationship between water use and drought awareness is uncorrelated with unobserved factors affecting both drought awareness and water use.

Table 2.1: Mediation Analysis: Drought Interest as Mediator for Policy Variables

	Drought Interest	Average Daily Water Use (gallons)		
	(1)	(2)	(3)	(4)
Drought Interest		-0.917***		0.0770
		(0.189)		(0.242)
Log of Marginal Rate per Gallon	21.76***		-39.99**	-41.67**
	(3.841)		(13.19)	(14.48)
Log of Fixed Monthly Rate	-36.44		218.1**	220.9**
	(18.99)		(69.44)	(69.01)
Switch to 2-Day Outdoor Use	7.275**		-65.62***	-66.18***
	(2.774)		(10.17)	(10.87)
California Mandated Reductions	25.55***		-44.13*	-46.10*
	(5.201)		(20.19)	(21.94)
Emergency State	30.14***		-73.83**	-76.15**
	(3.823)		(23.60)	(24.97)
Observations	16745193	16745193	16745193	16745193
R^2	0.734	0.472	0.475	0.475
Adjusted R^2	0.732	0.470	0.473	0.473

Standard errors in parentheses are two-way clustered at household-week level.

All regressions include weather controls and week and year fixed effects.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Column 1 regresses Google Trends ‘drought interest’ measure on policy variables. Column 2 regresses average daily water-use on ‘drought interest’ measure. Column 3 regresses average daily water-use on policy variables. Column 4 regresses average daily water-use on both ‘drought interest’ measure and policy variable.

control for all of the other policies, that the drought interest measure is no longer correlated with average daily water use. This suggests that the correlation of drought interest with outdoor water use operates largely through the policies we controlled for. Finally, comparing columns 3 and 4 which regress all of the previously discuss policies on average daily water use, we see that including the drought interest measure does not significantly change the magnitude of our other coefficients.

Our interpretation of these results is that drought interest has a significant impact on water use to the extent that it is correlated with the policies that interest us. However, it appears to have very little power explaining water use outside of these policies. Perhaps the polices we study in this paper are responsible for driving most of the observed changes, however once controlled for this drought interest is not very significant for actual water consumption.

2.5 Estimating Simultaneous Policy Impacts

This section pulls together all of the mechanisms highlighted in previous sections simultaneously, by using a single linear regression to estimate the impact of each of the previously discussed policies in the presence of all other policies. In particular, we estimate the following equation:

$$
\begin{aligned}
y_{it} = & \beta^1 \log(\text{Price}^{\text{marg}})_t + \beta^2 \log(\text{Price}^{\text{fixed}})_t \\
& + \beta^3 \text{PostScheduleChange} \times \text{Summer} \\
& + \beta^4 \text{PostAnnounce}_t^{\text{Emergency}} + \beta^5 \text{PostAnnounced}_t^{\text{MandatedReductions}} \\
& + \beta^6 \text{PostEnforcement}_{i,t}^{\text{Ticket}} + \beta^7 \text{PostEnforcement}^{\text{Tutorial}} \\
& + \beta^8 \text{PostRebate}_{i,t}^{\text{Washer}} + \beta^9 \text{PostRebate}_{i,t}^{\text{Washer}} + \beta^{10} \text{PostRebate}_{i,t}^{\text{Washer}} \\
& + \gamma_i + \gamma_{woy} + \gamma_{yr} + f(\text{Weather}) + \varepsilon_{it}
\end{aligned}
$$

Table 2.2 presents our results: The first two specifications omit individual specific variables, such as rebates and fines; however, when we add these back in the third and fourth specifications, the coefficients do not change substantially. If we Compare specification (1.) with (2.) and (3.) with (4.), we drop an additional the dummy variable for "post-outdoor water restrictions" that is not interacted with the "summer schedule" variable. The estimates are highly sensitive to this dummy because the adoption of the two-day watering schedule is co-linear with a one month of extremely elevated fixed rates, highlighting the fragility of series base estimates.

In column (4.), our most preferred specification, the estimates are consistent with the event-study specification in Section 2.3. In particular, we find the following; firstly, that an increase in the marginal cost of water by 10% leads to a reduction in water use by 40 gal/day, while an increase in the fixed component of rates does not significantly affect water consumption. Second, the switch from three to two days of permitted outdoor water use led to a 65 gal/day reduction in water use. Third, the announcement of California's mandatory conservation measures on April 1, 2015, is associated with a decline in water use of 40 gal/day and the State-of-Emergency announcement on Jan 17, 2014, is associated with 70 gal/day reduction in water use. Fourth, Receiving a ticket for violating outdoor watering restrictions

Table 2.2: Simultaneous Impacts of Policies, Rebates and Enforcement on Water-Use

	Average Daily Use (gallons)			
	(1)	(2)	(3)	(4)
Log of Marginal Rate per Gallon	-201.9***	-39.99**	-202.7***	-40.02**
	(19.03)	(13.19)	(19.04)	(13.21)
Log of Fixed Monthly Rate	-15.86	218.1**	-15.92	219.1**
	(67.46)	(69.44)	(67.39)	(69.54)
Emergency State	-47.28	-73.83**	-47.30	-73.96**
	(26.34)	(23.60)	(26.25)	(23.51)
Switch to 2-Day Outdoor Use	-219.4***		-220.5***	
	(24.48)		(24.44)	
Switch to 2-Day Outdoor Use	-33.49**	-65.62***	-33.31**	-65.62***
× Summer	(10.08)	(10.17)	(10.08)	(10.17)
California Mandated Reductions	-41.32*	-44.13*	-41.31*	-44.15*
	(18.61)	(20.19)	(18.59)	(20.19)
After Ticket Received			-16.23*	-13.18
			(6.890)	(6.887)
After Timer Tutorial Received			-104.7***	-104.4***
			(9.645)	(9.638)
After Clothes Washer Rebate Received			-0.385	-1.243
			(5.572)	(5.598)
After Toilet Rebate Received			-55.28***	-55.47***
			(11.16)	(11.13)
After Lawn Rebate Received			-60.71***	-54.45***
			(12.73)	(12.77)
Observations	16745193	16745193	16745193	16745193
R^2	0.477	0.475	0.477	0.476
Adjusted R^2	0.474	0.473	0.475	0.473

Standard errors in parentheses are two-way clustered at household-week level.

All regressions include weather controls and fixed effects in year and week of year.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Columns 1 regresses average daily water use all city-wide policy changes. Column 2 drops an indicator for the 2-day schedule change during winter. Columns 3 and 4 add in individual specific policy interventions. Column 4 again drops the indicator for the 2-day schedule change during winter; This is our preferred specification and contains the main results described in the text.

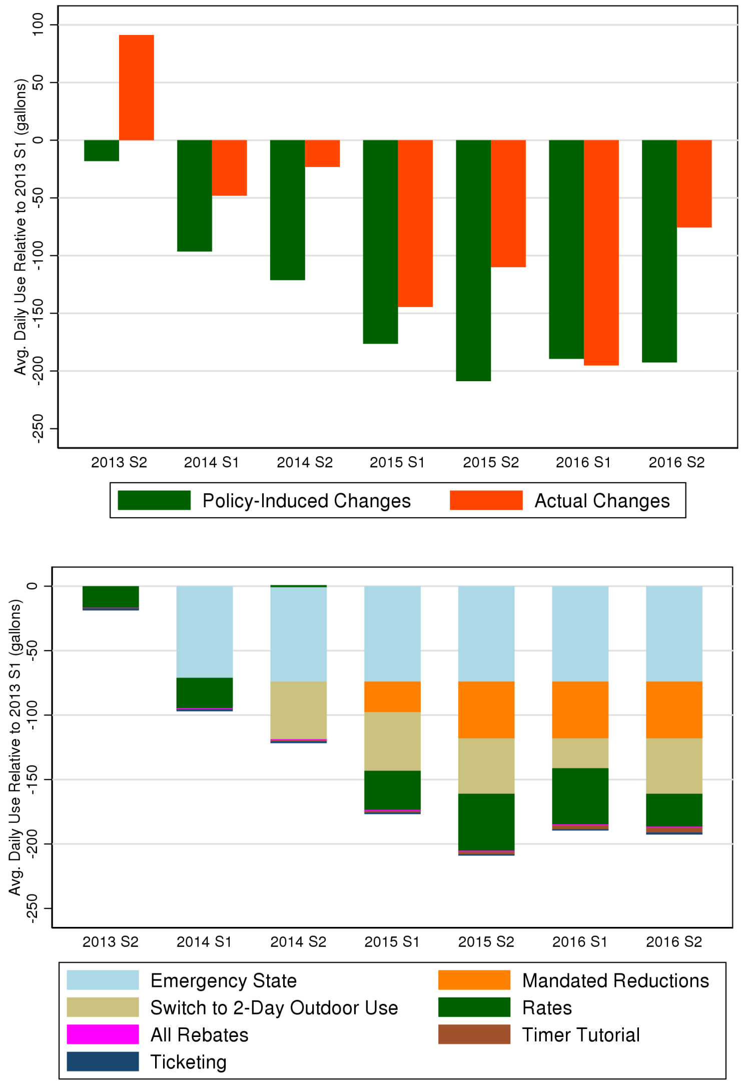
is associated with a long run 13 gal/day reduction in water use, although the coefficient is not significant. Fifth, Receiving a tutorial to set your sprinkler timer is associated with a 105 gal/day reduction in water use. Finally, receiving a toilet or lawn rebates are both associated with 55 gal/day reductions in daily water use, while rebates for a water-efficient clothes washers do not appear to significantly affect water-use.

2.6 Accounting for Total Water Conservation

In this final section, we will put the regression estimates from the previous section in context, by decomposing the total water savings estimated over our entire study into components that are attributable to each of the policies previously discussed. We start with our preferred specification column 4 of table 2.2. Then, for every 6-month period, we calculate average water use across all individuals and weeks after removing all variation attributable to weather effects, and seasonal and annual trends. We calculate the difference between the de-seasoned and de-weathered water use estimate in the first semester of 2013 and each subsequent semester. This data is presented in the "Actual Reduction" columns in the top panel of figure 2.12.

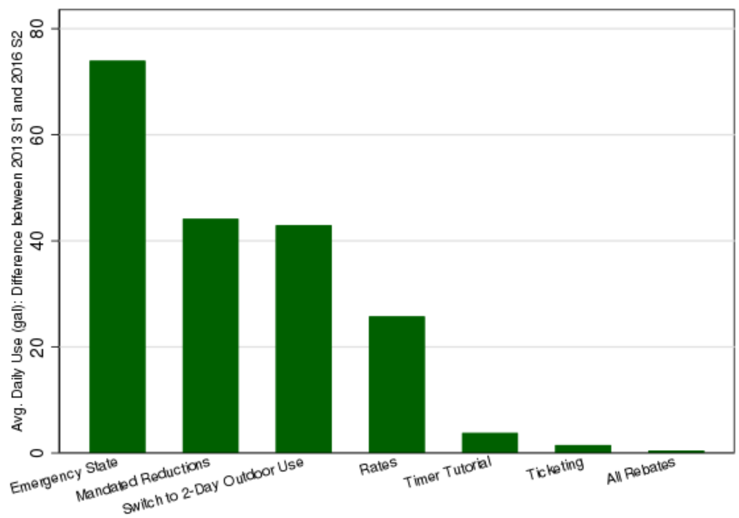
Then, we repeat the same exercise, except using predicted water use from our regression model; we remove all weather, seasonal and annual effects to give a predicted regression estimate reflecting only the coefficients on policy changes. Then, for every 6-month period, we calculate average water use across all individuals and weeks. And, again calculate the difference in predicted water-use between the first semester of 2013 and all subsequent semesters. This data is presented in the "Policy Induced Reduction" columns of the top panel of figure 2.12. "Policy-Induced Reductions" closely matches the "Actual Reductions", implying the regression model accurately predicts total water savings. Figure 2.12 shows how as more policies are adopted, total policy-induced savings increase. To summarize, we calculate the following:

Figure 2.12: Contributions of Each Policy Change to Water Conservation, 2013 to 2016



Average Daily Water-Use Relative to Year 2013 First Semester. Top Panel: Water-Use Reduction Accounted for by Each Policy. Bottom Panel: Water-Use Reduction Accounted for by All Policies vs. Actual Water-Use Reduction. See text for estimation details.

Figure 2.13: Total Policy Contributions to Water Conservation: Semester 1, 2013 vs. Semester 2, 2016



Average daily water conservation explained by each policy between Semester 2, 2016 and Semester 1, 2013.

$$\text{De-seasoned Actual Reduction} = \left(\overline{y_0 - f(Weather_0) - \gamma_0^{woy} - \gamma_0^{yr}}\right) - \left(\overline{y_T - f(Weather_T) - \gamma_T^{woy} - \gamma_T^{yr}}\right)$$

$$\text{Policy Induced Reduction} = \left(\overline{\hat{y_0} - f(Weather_0) - \gamma_0^{woy} - \gamma_0^{yr}}\right) - \left(\overline{\hat{y_T} - f(Weather_T) - \gamma_T^{woy} - \gamma_T^{yr}}\right)$$
$$= \beta(\overline{\text{Policies}_0} - \overline{\text{Policies}_T})$$

Figure 2.13 shows final column of Figure 2.12, except it is split into individual components for legibility. It shows each policy's contributions to the change in average water use between the second half of 2016 and the first half of 2013.

Based on these estimates, the State-wide drought announcements account for the most substantial observed savings; State of Emergency Announcement and the Mandated Water Regulations account for average savings of 70 and 45 gal/day respectively.

The switch from three to two days of permitted outdoor water-use reduced daily outdoor water use by 42 gal/day. In total, all the rate changes were responsible for a reduction in outdoor water use of around 23 gal/day between the beginning and end of the period. These estimates are smaller than those for the impact of state-wide announcements, however, unlike these estimates, they are identified of more than a single event-study, and so should be considered more credible. By the nature of these treatments, they are also more likely to persist over time as they are not one-time announcement highly correlated with public drought awareness.

Finally, although rebates, ticketing and timer tutorials have substantial impacts on each treated individuals water use of up to 100 gal/day. Because their penetration is low, overall they have a minimal impact on total water use, each being responsible for a per-captia reduction in water use of less than 4 gal/day.

2.7 Conclusion and Policy Discussion

As climate change bites, California is likely to experience worse and more frequent droughts in the future. By disentangling the impact of different policies implemented during this most recent drought, we hope we can help policy-makers think about how best to tackle the next one.

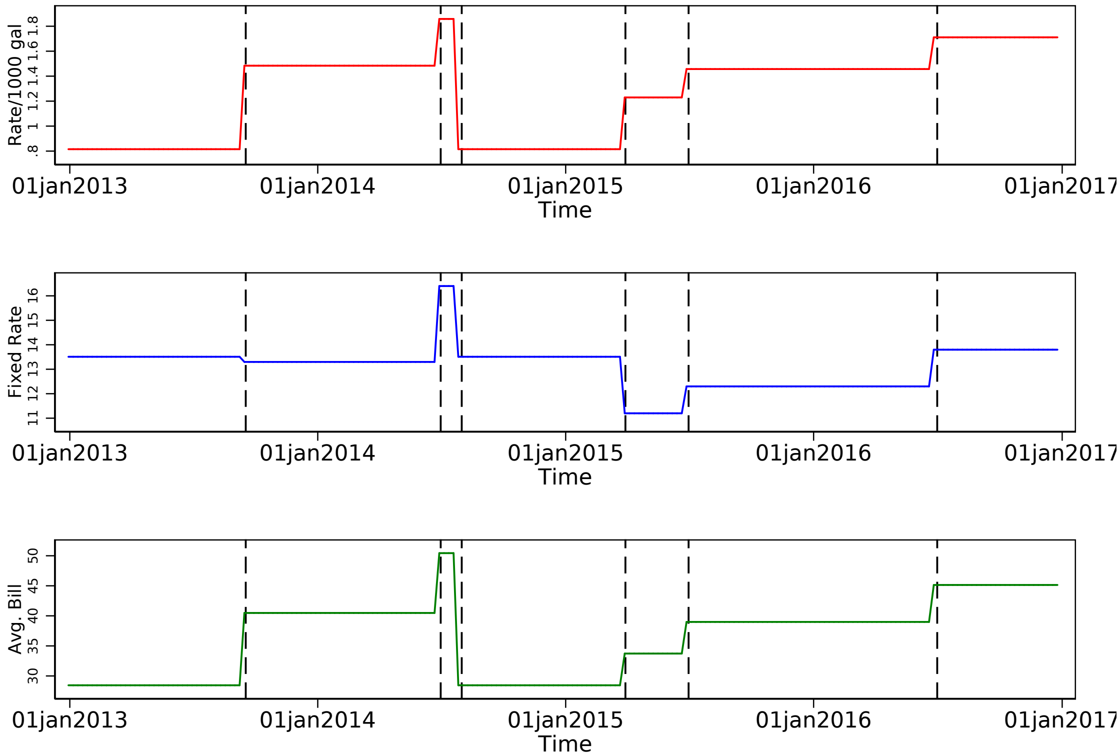
In our analysis, we find that Statewide announcements of drought policies seem to have a sizeable impact on water consumption. However, our estimates are only identified in the data off of time-series variation, so it is difficult to say how credible these estimates. Furthermore, it is uncertain whether these effects are persistent since they are highly correlated with our measure of public awareness of the drought.

On the other hand, by harnessing the unique resolution of our hourly household level micro-data, we have more tightly estimated evidence on how tightening outdoor water restrictions, installing water-efficient appliances and issuing notifications for violating water regulations can lead to persistent changes in households behavior. All of these policies seem to have had a significant impact on household behavior. However, the uptake of rebates and the issuance of fines was not sufficiently widespread to have had large impacts on aggregate water use. This is especially disappointing since water-efficient toilet rebates have a positive rate of return both for the city and customers. To the extent that these policies are feasibly scalable, Cities should consider expanding them during future droughts

Finally, Increasing the marginal prices customers pay for water can lead to persistent water conservation. Although this is a powerful tool, caution should also be advised against forever increasing marginal rates (which in many Californian cities are also increasing block schedules). Firstly, it is unclear whether customers respond to marginal or average rates, a question our paper is unable to resolve. Secondly, increasing marginal rates while reducing or holding equal fixed rates will divorce the utility's revenue structure from its costs structure, since even during drought the vast majority of the utility's costs are fixed. This can lead to a risk of a revenue shock if customers are able to conserve more water than anticipated. Finally, high marginal water-rates generate significant conservation among big houses with expansive lawns and wealthy homeowners. However, poor customers, who have more inelastic demand and who pay a higher percentage of their income towards utilities might be disproportionately burdened by rate increases, raising equity concerns (Mack and Wrase, 2017).

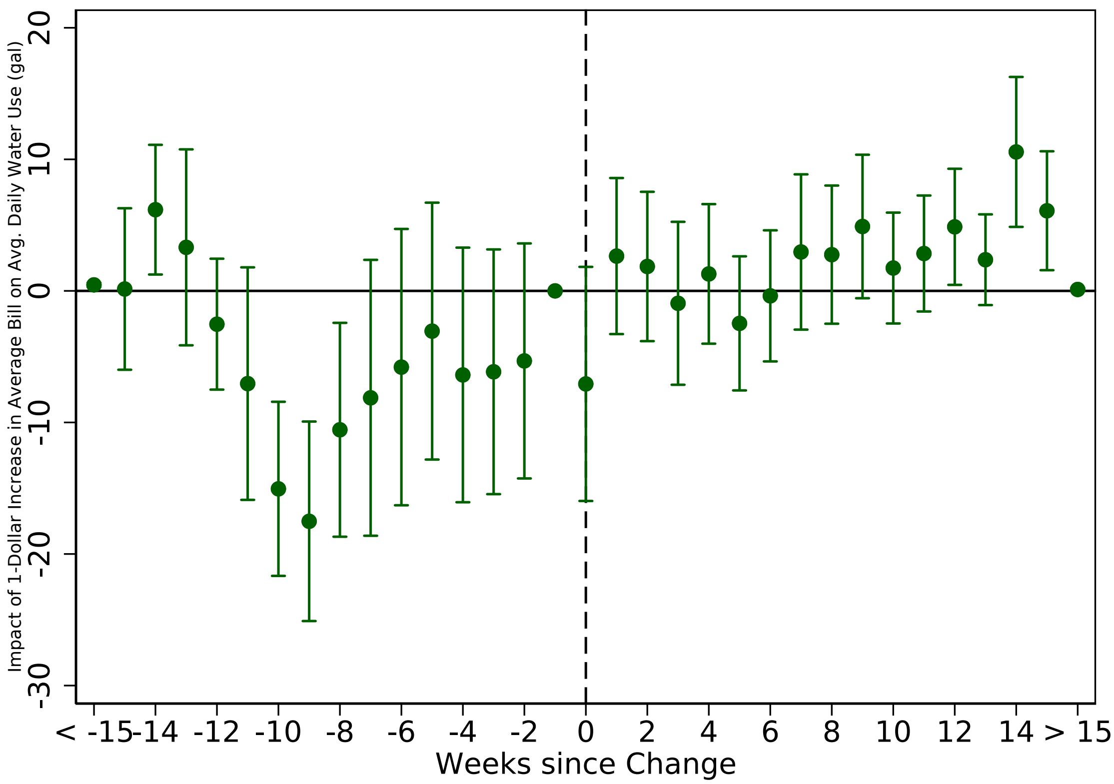
A2.1 *Appendix Tables and Figures*

Figure A2.1: Rate Changes, 2013 to 2016



This plot shows the changes in fixed and marginal costs of water over time as well as the impact on an average water user's bill. Fixed rates vary based on meter size.

Figure A2.2: Event Study - the Impact of *Average* Price on Water-Use



This figure illustrates the impact on water use of all changes in household's average bill. Average bill is evaluated at each household's baseline water use. Changes in average bill is calculated ad as "Average baseline water use ∗ Marginal rate change + Fixed rate change". The event study includes weather controls, a control for whether summer watering schedule is in place and fixed effects in year and week of the year. Standard errors are two-way clustered at household-week level.

Table A2.1: Summary Statistics - Water-Use by Household Characteristics

Characteristics	**Group**	**N**	**Average Daily Use (gal)**	**Average Daily Savings (gal)**
Baseline water use	large	27457	693 (273)	350 (342)
	medium	27457	435 (110)	144 (157)
	small	27456	286 (107)	47 (149)
Home footprint	large	27451	550 (289)	196 (260)
	medium	27451	444 (212)	173 (258)
	small	27451	421 (212)	171 (278)
Irrigable land area	large	27411	587 (305)	231 (300)
	medium	27411	441 (199)	174 (264)
	small	27410	387 (170)	136 (218)
Pays own water bill	Yes	63301	578 (257)	184 (257)
	No	19069	451 (210)	167 (293)
Blkgp income level	high	27378	509 (260)	174 (234)
	medium	27355	463 (246)	179 (264)
	low	27637	443 (230)	187 (295)
Blkgp college rate	high	27136	522 (278)	183 (236)
	medium	27885	452 (225)	175 (265)
	low	27349	442 (227)	182 (293)

Average water use over entire sample, and average change in water use between 2013 and 2016. Sample is stratified into thirds by each demographic characteristics. Demographic data is at the Census Block-group level. Parcel data is at the individual level. Standard deviations are in parentheses.

Table A2.2: Impact of Marginal Rate Changes on Water-Use, by Income and Baseline Usage

	Average Daily Use (gallons)		
	(1)	(2)	(3)
Log of Marginal Rate per Gallon	-24.92	-26.40	40.99
	(13.01)	(14.10)	(31.15)
Log of MC/gal X Pays Own Bills		1.918	2.293
		(11.19)	(8.630)
Log of MC/gal X High Baseline Use			-199.9***
			(51.85)
Log of MC/gal X Medium Baseline Use			-64.84***
			(18.27)
Log of MC/gal X High Income Blkgrp			43.01***
			(10.86)
Log of MC/gal X Median Income Blkgrp			18.49**
			(5.904)
Observations	16745193	16745193	16745193
R^2	0.471	0.471	0.475
Adjusted R^2	0.469	0.469	0.472

Standard errors in parentheses are two-way clustered at household-week level.
All regressions include weather controls, an indicator for whether summer watering schedule is in place and fixed effects in year and week of year.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This regression uses our baseline specification. Column 1 contains only a coefficient on Log Marginal Rate / gal. Column 2 interacts the Log Rate with an indicator variable measuring whether a household pays its own bills. Column 3 also interacts the Log Rate with variables indicating whether the household is in the middle or top third of baseline use, and whether it is in the middle or top third of Census Blockgroups by median household income.

Table A2.3: Outdoor Water-Use Schedule Before and After August 2014

Label	Day	Odd				Even			
		Summer		Winter		Summer		Winter	
		Before	After	Before	After	Before	After	Before	After
Always Banned	Mon	.	.	.	.	.	.	.	.
Always Allowed Summer Day	Tue	X	X	.	.	.	.	.	.
Always Allowed Summer Day	Wed	.	.	.	.	X	X	.	.
Banned after 08/01/2014	Thu	X	.	.	.	.	.	.	.
Banned after 08/01/2014	Fri	.	.	.	.	X	.	.	.
Always Allowed	Sat	X	X	X	X	.	.	.	.
Always Allowed	Sun	.	.	.	.	X	X	X	X
Total Watering Days		3	2	1	1	3	2	1	1

On Days market with an X, either Odd or Even numbered houses may use water outdoors but only before 9am in the morning or after 6pm in the evening. The designation that a day is "Always Banned" means that at no point in our sample is it an allowed watering day. The two "Always allowed summer days" are days that are allowed through out our period, but only when the summer watering schedule is in effect. The days "Banned after 08/01/2014" were previously days where households were allowed to water, only in the summer, but became banned after our City instituted the new summer watering schedule. Last, the days that are "Always Allowed" are days when households are allowed to water throughout the entire calendar year.

Table A2.4: Simultaneous Impact on Water-Use of Conservation Policies - Stratified by Household Characteristics

	Average Daily Use (gallons)			
	Pays Own Water Bill		Top 50% Baseline Water Use	
	Yes	No	Yes	No
	(1)	(2)	(3)	(4)
Log of Marginal Rate per Gallon	-36.39**	-51.39***	-69.41***	-6.650
	(13.64)	(12.06)	(18.73)	(7.985)
Log of Fixed Monthly Rate	235.4**	153.9*	273.0**	64.71
	(71.10)	(69.18)	(88.43)	(47.09)
Emergency State	-77.18**	-63.09**	-113.4***	-32.46*
	(24.38)	(20.54)	(31.54)	(14.72)
Switch to 2-Day Outdoor Use	-63.83***	-71.74***	-91.69***	-41.06***
	(10.51)	(9.273)	(13.85)	(6.704)
California Mandated Reductions	-47.62*	-34.39	-49.02	-56.15***
	(20.79)	(18.78)	(27.24)	(12.76)
After Ticket Received	-13.78	-10.33	-0.724	7.045
	(7.150)	(7.610)	(6.750)	(4.057)
After Timer Tutorial Received	-99.69***	-124.6***	-132.5***	-34.30***
	(9.865)	(16.81)	(12.57)	(5.458)
After Clothes Washer Rebate Received	-1.847	12.18	17.00*	-5.979
	(5.318)	(14.30)	(7.478)	(4.518)
After Toilet Rebate Received	-51.12***	-90.89	-57.28***	-34.25***
	(10.59)	(51.48)	(16.82)	(6.967)
After Lawn Rebate Received	-54.23***	-16.69	-59.21**	-41.81***
	(13.11)	(48.17)	(20.40)	(8.161)
Observations	12879288	3865905	8378729	8366464
R^2	0.503	0.379	0.463	0.358
Adjusted R^2	0.500	0.376	0.461	0.355

Standard errors in parentheses are two-way clustered at household-week level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This table estimates the same specification in main regression Table 2.2, except for stratified sub-samples of the data. Column 1 contains only customers who pay their own water bill; while Column 2 contains all households who do not pay their own water bill. Column 3 is estimated on a sub sample containing only households in the top 50% of water users; while Column 4 contains only households in the bottom 50%. All regressions include weather controls and fixed effects in year and week of year.

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