# The Long-Term Outcomes of Restoring Indigenous Property Rights to Water*

Leslie Sanchez† Bryan Leonard‡ Eric C. Edwards§

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**Abstract**

Water rights were allocated in the Western United States over 1850—1900 without regard for pre-existing Indigenous claims, but a 1908 U.S. Supreme Court Ruling (Winters v. United States) created a framework for restoring tribal rights. To date, reservations in the Colorado River Basin have obtained rights to 2.8 million acre-feet of water, and unsettled tribal rights may exceed the annual flow of the Colorado River. However, various institutional barriers including land tenure, credit access, and bureaucratic hurdles may constrain tribes' utilization of these new rights. This paper uses satellite data on land use to study the effect of Winters settlements on 26 reservations that received water rights between 1974 and 2012. Using newly developed difference-in-difference techniques, we find statistically significant, economically small differences in agricultural and developed land use. Back-of-the-envelope calculations reveal that most tribes are using a small fraction of their entitlements, foregoing billions of dollars in potential revenue. We provide evidence that land tenure and credit constraints may explain some of this shortfall.

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†Tufts University leslie.sanchez@tufts.edu

‡Arizona State University. bryan.leonard@asu.edu

§North Carolina State University eric.edwards@ncsu.edu

# 1 Introduction

Throughout the world and well into the 20th Century, natural resource commons traditionally governed by Indigenous people were enclosed, allocated, or otherwise appropriated as part of settlement and colonization. While the bounding of a resource and allocation of formal property rights can address over-exploitation, pre-existing Indigenous property rights (formal and informal) were generally extinguished in the process. In the United States, the loss of natural resources ranging from land and water to salmon or bison has been suggested as a key reason why many Indigenous communities remain underdeveloped relative to surrounding non-Indigenous populations (Carmody and Taylor, 2016; Parker et al., 2016; Feir et al., 2019; Farrell et al., 2021), as many Indigenous groups remain highly reliant on direct natural resource extraction for their livelihoods, including subsistence and for-market agriculture, fishing, and hunting.

Globally and in the United States, restoring natural resource access to historically marginalized Indigenous groups has become a policy focus. The 1974 Boldt Decision (United States v. Washington, 384 F. Supp. 312, aff'd, 520 F.2d 676) allocated substantial fishing rights to tribes in Washington State and the 2020 McGirt ruling (McGirt v. Oklahoma, 140 S. Ct. 2452) moved about half of the land in Oklahoma into the Muscogee (Creek) Nation. Internationally, restoration of natural resource property rights has occurred for Australian aboriginal land and water right claims (Mabo and Others v. Queensland (no. 2) 1992 HCA 23, Native Title Act 1993); Chilean Indigenous land and water rights (Heise, 2001; Tomaselli, 2012); and New Zealand Maori land (the Ngai Tahu and Waikato-Tainui settlements) and customary water and fishing right claims (Gibbs, 2000; Te Aho, 2010). While property rights to natural resources can increase economic value and improve ecological health (Libecap, 2007; Costello et al., 2008), causal analyses of the effects of Indigenous rights restoration have been limited. What studies been undertaken have shown ambiguous results (Parker et al., 2016; Blackman et al., 2017; Robinson et al., 2017).

In this paper, we estimate the effect of a large-scale attempt to restore Indigenous rights to natural resources: the allocation of formal property rights to water to tribal nations in the western United States. This policy, stemming from a U.S. Supreme Court ruling (Winters v. United States, 1908), has granted tribes formal title to large volumes of the West's scarce water. Reservations in the Colorado River Basin, a subset of those receiving rights, have obtained rights to 20 percent of the river (U.S. Bureau of Reclamation, 2018). These rights total 2.8 million acre-feet, enough water for the domestic use of 20 million southwest US residents or approximately one million acres of irrigated agriculture. Based on prevailing prices, tribal water rights themselves could have a market value exceeding $3.8 billion annually.[1] In addition to the nearly 3 million acre-feet worth of water rights previously restored to tribes, the potential volume of outstanding, unsettled tribal water rights that are currently being adjudicated could exceed 1.6 million acre-feet(Sanchez et al., 2020). Hence, measuring the implications of Winters settlements for land and water use is important not just for tribal policy and economic development, but for agricultural users, urban water suppliers, and policymakers across the Western United States.

In the arid western U.S., irrigation is critical to the development of agriculture (Edwards and Smith, 2018). However, water access and availability are determined by the nuances of the system for allocating property rights to water (Garrick and Aylward, 2012). Secure property rights to water helped facilitate massive investment in irrigation infrastructure to bring otherwise unusable land into agricultural production (Leonard and Libecap, 2019), but these investments also required secure rights to the land itself (Alston and Smith, 2022). Given prior work demonstrating how the definition and attributes of Indigenous land property rights create barriers to agriculture, irrigation, and development (Trosper, 1978; Anderson and Lueck, 1992; Dippel et al., 2020; Ge et al., 2020; Leonard et al., 2020), there is considerable ambiguity about the direction and magnitude of the expected effect of the restoration of tribal water rights on resource access, agricultural production, and development outcomes.

We study the effect of tribal water right settlements with a parcel-level difference-in-difference model using newly-developed estimators robust to heterogeneity in the timing of treatment effects (de Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2020). Tribal nations select into the water right adjudications, but the duration of negotiations is plausibly exogenous, ranging from 5–50 years (Sanchez et al., 2020). This allows us to obtain a causal estimate of the effect of a water rights settlement on changes to agricultural land use relative to reservations that had begun but not yet completed the settlement process. Because historic administrative data on land use and social and economic outcomes are limited on reservations, we use satellite estimates of land use available starting in 1974.

[1] Authors' calculation using data from Donohew and Libecap (2010).

Our estimates show that after obtaining formal water rights, agricultural land use on reservations increased by up to 0.36 percentage points, which translates into a 5 percent increase in agricultural land use. To our knowledge, this is the first estimate of the effect of these tribal water settlements on tribal land and water use. This finding holds across three different difference-in-difference estimators and alternative measures of agricultural land use (e.g., focusing solely on cultivated crops). We also find mixed evidence of small increases in developed land use, but these results are more sensitive to the inclusion of various reservation-level controls.

The magnitude of the estimated treatment effects, in terms of tribal water use, are small compared to the overall size of settlement allocations. One explanation for this outcome is that the restoration of Indigenous property rights changed the distribution of resource wealth but not the allocation of the resource itself, i.e. off-reservation users started paying tribes for water they had already been using. A simple water accounting estimation exercise rejects this explanation: we find that combined on-reservation use and off-reservation leasing is well below the volume of allocated water rights on the majority of reservations with Winters rights. The upshot is that Winters settlements apparently fail to deliver actual water for tribes, despite granting large quantities of "paper rights."

In this and other resource settings, tribal development via newly acquired property rights faces dual challenges related to preexisting resource users and overlapping institutional failures. In the case at hand, western water law favors *beneficial use*, and water title holders do not automatically gain control of the resource. Without building diversion infrastructure — ditches and canals — and installing irrigation systems, tribes cannot put their rights to beneficial use. Tribes are then paradoxically put at a disadvantage in negotiating the lease of the water they do not divert but hold title to. Should an agreement fail to materialize, existing users see no credible threat of losing access in the absence of tribal diversion infrastructure. Preexisting institutional barriers that limit tribal access to credit, especially associated with the utilization of land as collateral, further diminish their ability to assert *de jure* rights despite holding legal title.

We explore several mechanisms that could explain the relatively low utilization of tribal water rights revealed by our main results. First, drawing on existing literature on the impact of land tenure on resource use and economic development on reservations (Ge et al., 2020; Leonard et al., 2020; Leonard and Parker, 2021; Dippel et al., 2020), we explore the differential effects of Winter settlements across land tenure types. We find that federal trust restrictions on both tribal and individually held land constrain land use change. Consistent with prior work, we find increases in on-reservation agriculture and development after gaining property rights are confined to land without ownership constraints, relative to land that cannot be alienated or collateralized, lacks access to credit, and on which leases and changes to land use require federal government approval.

We also explore the impact of transactions costs associated with water markets more broadly (Garrick and Aylward, 2012) by comparing outcomes from tribes that lease water back to off-reservation users. We find substantially smaller increases in agricultural land use on reservations that lease their water back to off-reservation users. However, we find that tribes that lease their water see much larger increases in developed land use. Hence, it may be that income from water leasing helps tribes address preexisting credit constraints that prevent further economic development. However, even the tribes that lease substantial volumes of water have unrealized gains because some portion of their Winters allocation is still unaccounted for by on-reservation water uses and off-reservation leases.[2]

Overall, our results suggest a striking difference between the promise and reality of water right restoration. While tribal water settlements increase on-reservation agricultural development, the increases in water use pale in comparison to the magnitude of the settled water rights. This is largely consistent with other attempts to restore Indigenous rights to a particular resource without addressing broader institutional challenges facing reservations, such as the Boldt decision regarding commercial fishing in Washington State (Parker et al., 2016). More broadly, our results underscore the point that multiple institutional or market failures require multiple policy solutions (Bennear and Stavins, 2007), particularly when land rights are incomplete (Alix-Garcia et al., 2015). Hence, while restoring Indigenous rights to previously expropriated natural resources may help achieve important goals in terms of procedural justice, these reallocations are unlikely to yield material benefits for tribes if they are not accompanied by complementary reforms to other institutions that constrain investments in agriculture and economic development. In the mean time, off-reservation water users are able to continue their uncompensated use of tribal water rights.

[2]We also include tribal in-stream flow rights in our definition of "use" for these calculations.

## 2 Background

Surface water in the western United States is governed by the prior appropriation doctrine, which assigns water rights based on the chronological priority of the initial claim. This "first in time, first in right" system ensures the earliest (senior) appropriators' water access in all but the driest years, forcing juniors to curtail their usage first. States assigned the earliest appropriative rights to white settlers starting in the 1850s, and by the mid-1900s, most basins were fully appropriated. Around the same time, the federal government relegated tribes to reservations established by tribe-specific reservation treaties. Many reservations are located in the west, where rivers and streams are separated by large expanses of dry, but otherwise arable, land that requires large-scale irrigation infrastructure to support agricultural production (Hanemann, 2014; Leonard and Libecap, 2019).

While reservation treaties and successive federal policies established expectations that tribes would sustain themselves through agriculture, tribal water needs were not considered when reservations were created (Carlson, 1981). States, who have the authority to allocate water within their boarders, did not allocate water rights to tribes, and none of the Bureau of Indian Affairs (BIA) reservation irrigation projects started in the late 1800s were completed (Government Accountability Office, 2006). Today, BIA irrigation projects are largely dilapidated, inefficient, and in need of repair (Carlson, 2018). Without enforceable water rights, water availability on reservations became scarce and highly variable as nearby off-reservation water use increased. Court documents filed by tribes describe the consequent depletion of reservations streams, springs, and aquifers. For example, the Ak-Chin, Jicarilla Apache, Tohono O'odham, and Hopi tribes sought legal protection when "existing wells went dry, and irrigation had to be abandoned" due to off-reservation water use (Arizona Department of Water Resources, 2006; Ak Chin Indian Community v. United States , 1973).

A 1908 Supreme Court ruling (Winters v. United States, 1908) affirmed that while not explicitly mentioned, reservation treaties implicitly granted tribes rights to water with a priority based on the date that the treaty was signed. The ruling did not provide quantified, legal water rights. Instead, it created a legal obligation for the federal government, as a trustee of tribal resources, to remedy its neglect in filing water claims on behalf of tribes. Tribal water rights, referred to as

Winters Rights, cannot be forfeited through nonuse because they are "federally reserved." Thus, tribes have strong legal claims to high-priority water rights, but the rights themselves do not exist until they are adjudicated.

A handful of tribes acquired Winters Rights via court decree in the first 50 years following the Supreme Court ruling. However, litigation is slow and expensive, and tribes often lack the institutional support and financial capital necessary to sustain litigation or develop and use water rights once decreed. Instead, most Winters Rights are adjudicated through settlement agreements negotiated with neighboring water users, states, and the federal government. Settlements, which are ultimately enacted by Congress, provide tribes with federal funding for infrastructure and economic development. Settlement funding can help tribes to overcome capital constraints to developing their water resources for on-reservation water use.

The prevalence of agriculture relative to other economic activities on reservations prior to water settlements, as well as existing (though aging) farm infrastructure, suggests that changes to agricultural land use from a water settlement may occur more quickly than capital-intensive shifts towards non-agricultural development. Prior to the 1970s there was little non-agricultural economic development on reservations. In the 1980s several key pieces of legislation — the Indian Gaming Regulatory Act and the Indian Self-Determination Education Assistance Act — enabled tribes to diversify their economies away from agriculture (Cornell and Kalt, 2010). Administration of reservation land shifted from federal to tribal control, and overall, gaming, tourism, mining, municipal, and industrial development have increased across reservations (Lyons et al., 2007).

Still, reservation economies remain largely agricultural due to geographical remoteness, low population densities, and difficulties achieving economies of scale, while capital credit constraints further limit investments in housing, infrastructure, and business development (Mauer, 2017; Ak Chin Indian Community v. United States , 1973). Such barriers potentially limit how tribes can develop their water rights for non-agricultural activities. While settlements often include Congressional funding for reservation water infrastructure and economic development, tribes face countless hurdles to actually obtaining funding (Western States Water Council & Native American Rights Fund, 2014).

Once enacted, settlement implementation can take years. Water rights must be reallocated from existing appropriators and legally transferred to tribes; federal agencies must allocate funding in their annual budgets to meet settlement obligations, and water infrastructure must be rehabilitated or constructed anew. Tribes then enact water codes that standardize rules for approving, conditioning, and revoking water use permits on-reservation (Termyn, 2018). Typically, any individual (Indian or non-Indian) on a reservation can apply for water use permits, which are approved by a tribal water authority (Breckenridge, 2006). The upshot is that tribes are most likely to use their newly acquired Winters rights for agriculture.

However, in the absence of physical diversion of water by the tribes, off-reservation users may continue to use and benefit from water that they no longer legally own. This mismatch between "paper" and "wet" water arises from the long tradition of favoring "beneficial use" within the prior appropriation doctrine. Diverting water for productive use in the arid West is a capital-intensive endeavor, even in agriculture (Hanemann, 2014; Ge et al., 2020). Given the various constraints to development on reservations (Dippel et al., 2020; Leonard and Parker, 2021; Dippel et al., 2021), the magnitude of realized changes in land and water use for tribes following a Winters settlement is uncertain.

# 3 Data & Empirical Strategy

## 3.1 Data

A persistent challenge to conducting empirical research on reservations is the lack of fine-scale, longitudinal data. Previous analyses rely on US Census data, for which variables, such as income and farm sales, are aggregated at the reservation-level, only collected for some reservations, and available from 1980 onward. We overcome these limitations by combining several novel data sources: i) fine-scale measures of land tenure on reservations assembled by Dippel et al. (2020), ii) information on tribal water settlements compiled from adjudication filings, court records, and settlement texts by Sanchez et al. (2020), and iii) time-varying, parcel-level measures of land use and other reservation characteristics over five decades collected for this study.

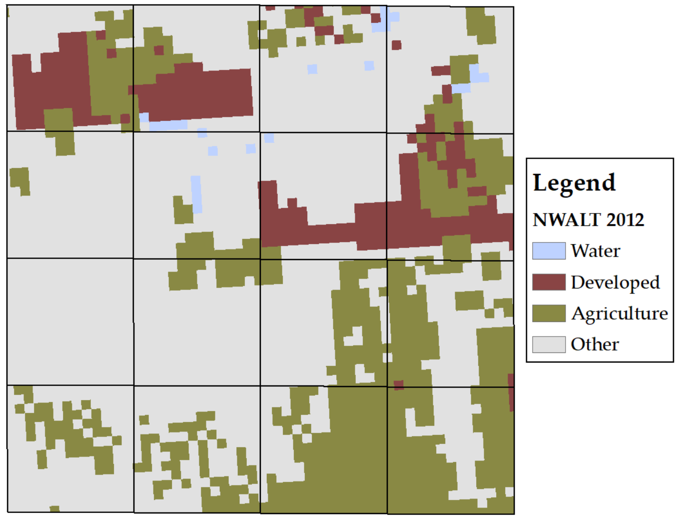
To measure the effects of water right security on land use, we use high-resolution satellite imagery from the U.S. Conterminous Wall-to-Wall Anthropogenic Land Use Trends (NWALT) geospatial dataset available from 1974 onwards (Falcone, 2015), which allows for water right allocation outcomes to be observed over a span of 40 years. NWALT data provide estimates of 19 categories of land use at a 60×60m resolution for five time periods — 1974, 1982, 1992, 2002, and 2012 — and have been cross-checked and validated using county-level USDA and US census data, and numerous other federal land use and geospatial databases.

We focus on changes to agricultural land use resulting from Winters rights for two main reasons. First, the majority of water use on and off reservations is still associated with agricultural land use (Brewer et al., 2008). Second, as discussed in Section 2, reservation economies are still largely agricultural and Winters settlements themselves are focused on the acquisition of water for agriculture. We categorize land use as agricultural if it falls into one of two categories of agricultural land use: crops or hay/pasture (Spangler et al., 2020). We also measure developed land use. NWALT codes "developed" land as major transportation, commercial services, industrial and military, recreation, high density residential, low-medium density residential, and other developed land. We categorize land use as "developed" if it falls into any of these development classes (Medalie et al., 2020). Developed land use reflects capital investments in the construction of physical structures, such those for housing, gaming, manufacturing, and recreation.

Our unit of analysis is a Public Land Survey System (PLSS) quarter section. The PLSS is a rectangular grid devised by Bureau of Land Management to divide most of the US into 6×6-mile townships. Townships are then divided into 36 $1 \times 1$-mile sections. Each section is divided into four quarter sections that are $\frac{1}{2}$-mile$\times\frac{1}{2}$-mile squares (see Figure A3). These 160-acre quarter sections correspond to the typical size of an ownership tract on a reservation due to historical land titling policies (Leonard et al., 2020; Dippel et al., 2020). For brevity, we refer to quarter sections as "parcels," but we note that some quarter sections actually contain multiple land parcels. Our primary outcomes of interest — agricultural and developed land use — are calculated as percentages of total usable parcel area, which excludes water and wetlands. Figure 1 shows the NWALT data across a sample of 16 adjacent quarter sections.

Our sample includes a panel of 257,187 parcels on 57 federally recognized reservations in the US West that have asserted water right claims for agricultural and domestic water use (Figure A2). We define our treatment group ($n$=144,933) as parcels located on 26 reservations that adjudicated their water rights via negotiated settlement between 1974 and 2012 (years of land use data availability). Previous research finds a tribe is more likely to begin the process of adjudicating their water rights when total reservation area, arable reservation acreage, and measures of water scarcity

Figure 1: NWALT Land Use Data




**Notes:** This figure depicts our outcome measure of agricultural and and developed land in the NWALT data. The figure depicts 16 quarter-sections of 160 acres each. The quarter-section is our unit of analysis. Light blue color shading indicates water, which we omit from the denominator when calculating the percentage of each parcel that is devoted to agriculture or development.

are increasing (Sanchez et al., 2020). To ensure that our group of untreated parcels are a plausible counterfactual for treatment parcels on reservations that achieve a water settlement, we restrict our untreated group to parcels on reservations that have self-selected into the adjudication process but have not yet secured legal titles to water. We also exclude reservations that primarily pursue instream flow claims, as major changes to land use are less likely to occur after settlements on these reservation. Our untreated group is comprised of 112,254 parcels located on 31 reservations that have initiated but not completed the adjudication process.

To determine treatment status for each reservation in each year, we use primary data collected by Sanchez et al. (2020) on the status of tribal water right adjudications from settlement agreement texts housed at the University of New Mexico's Native American Water Rights Settlement Project, and from state, appellate, and district court documents detailing ongoing, but unresolved, water right adjudications. We define a water settlement dummy variable, $PostSettlemt_{rt}$, where each parcel on reservation $r$ is assigned a value of 1 for each year, $t$, following the enactment the reservation's water settlement and zero prior to settlement.

To assess potential differences between the treated and untreated groups, we also develop parcel-level measures of land quality and terrain. We use 30x30 meter resolution data from National Elevation Dataset to estimate each parcel's mean elevation and standard deviation of elevation as a measure of ruggedness (Ascione et al., 2008). We use the Schaetzl et al. (2012) soil productivity index (PI) as a time-invariant, ordinal estimate of mean soil quality on each parcel. The soil productivity index is ranked categorically from 0-21 with values greater than 10 representing highly productive soils. Finally, we include information on land tenure for each quarter section from Dippel et al. (2020), which we describe in Section 5.

We also construct two time-varying dummy variables: casino operation and presence of a tribal lending institution. For each indicator, parcel $i$ on reservation $r$ is assigned a value of one in year $t$ if a casino/lending institution was in operation in that year, and zero otherwise. We collected data on casino operation from the National Indian Gaming Commission and individual casino websites. Data identifying tribal lending institutions is available from the Minneapolis Federal Reserve. We collected supplemental data on tribes served by each institution, including dates of operation, from the institutions' individual websites. Sanchez et al. (2020) find that tribal water entitlements and settlement funding are correlated with off-reservation county population, a measure of competing demand. We use US Census data to estimate the off-reservation population in counties overlying or adjacent to reservations each year of the sample.[3]

### 3.2 Empirical Strategy

#### 3.2.1 Difference-in-Differences

We use a difference-in-difference methodology to estimate parcel-level changes to land use before and after a water right settlement on treated versus untreated parcels, taking advantage of the fact that different reservations settled their water rights at different times, and some reservations have yet to settle their rights (despite having begun the negotiation process). The typical approach for recovering difference-in-difference estimates of the average treatment effect on the treated (ATT) in our setting would be to use a two-way fixed effects estimator (TWFE) of the form:

$$y_{irt} = \beta_{TWFE} PostSettlement_{rt} + \beta_2 X_r t + \vec{\lambda}_i + \vec{\tau}_t + \varepsilon_{irt} \quad (1)$$

where $Y_{irt}$ is the outcome for parcel $i$ on reservation $r$ in year $t$. $X_{rt}$ is a set of time-varying reservation characteristics (adjacent-county population, an indicator for casino development, and

[3]We use the closest available census year to each of the five NWALT waves (1974, 1982, 1992, 2002, and 2012).

an indicator for access to tribal lending institutions), $\lambda_i$ is a vector of parcel fixed effects, and $\tau_t$ is a vector of year fixed effects.

The coefficient on $PostSettlement_{rt}$ has traditionally been interpreted as the difference-in-difference coefficient, but recent work has revealed potential problems with this interpretation. The core issue is that $\beta_{TWFE}$ can deliver biased estimates of the true ATT when different cohorts (in our case, reservations) are treated at different times if there is substantial heterogeneity in the treatment effects over time or between cohorts (de Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2020; Goodman-Bacon, 2021; Wooldridge, 2021). This bias arises because $\beta_{TWFE}$ is a weighted average of all 2×2 comparisons of "switchers" to non-"switchers" that appear in the data, which includes: i) comparisons of switchers to never-treated parcels, ii) comparisons of early switchers to non-yet-treated parcels, and iii) comparisons of late switchers to already-treated parcels (Goodman-Bacon, 2021). The third comparison, where already-treated parcels act as a control group for late-treated parcels, can lead to negative weights in the weighted average represented by $\beta_{TWFE}$, resulting in a downward bias or even a negative coefficient when all underlying ATTs are in fact positive (de Chaisemartin and d'Haultfoeuille, 2020).[4]

de Chaisemartin and d'Haultfoeuille (2020) and Callaway and Sant'Anna (2020) both describe the problems that can cause $\beta_{TWFE}$ to deliver a biased estimate of the ATT and propose alternative DiD estimators that are robust to heterogeneous treatment effects across time and/or cohorts. To briefly summarize, both estimators are similar in that they use only never-treated or not-yet treated units as control groups, eliminating the already-treated vs. late-treated comparison. de Chaisemartin and d'Haultfoeuille (2020)'s method provides time-specific ATTs for each $k$ periods since treatment that are averaged across different cohort groups, whereas Callaway and Sant'Anna (2020) construct group-time-specific ATTs (e.g., a separate ATT for each cohort in each of the $k$ periods since treatment). Both estimators also include methods for aggregating ATTs across time/groups to deliver either event-study coefficients or an overall ATT.

We use de Chaisemartin and d'Haultfoeuille (2020)'s estimator as our preferred approach, but we show that the results are similar using either the Callaway and Sant'Anna (2020) estimator or the traditional TWFE approach. We prefer the de Chaisemartin and d'Haultfoeuille estimator

[4]These problems are more likely to arise as treatment effects become more heterogenous either across time or between treatment cohorts. See de Chaisemartin and d'Haultfoeuille (2020) and Callaway and Sant'Anna (2020) for additional details.

for three reasons. First, in practice, the Callaway and Sant'Anna estimator treats all covariates as time-constant, using only base-period covariates in the estimation, whereas de Chaisemartin and d'Haultfoeuille allow for time-varying covariate controls. A second, related advantage of the de Chaisemartin and d'Haultfoeuille estimator is that it allows the researcher to include group-specific non-parametric trends. We discuss below why this as an important consideration in our setting. Third, de Chaisemartin and d'Haultfoeuille provide guidance for estimating the weights associated with $\beta_{TWFE}$ and using them to diagnose the extent of potential bias.

### 3.2.2 Identification

Identification of the ATT associated with settling Winters rights using the de Chaisemartin and d'Haultfoeuille (2020) estimator requires several assumptions. In addition to some regularity conditions, we must assume that both the untreated and treated *potential* outcomes for the treated and untreated groups follow parallel trends, and that any shocks affecting the potential outcomes for either group are uncorrelated with treatment.[5] Examination of event study estimates from the de Chaisemartin and d'Haultfoeuille approach can provide some suggestive evidence in support of these assumptions, but ultimately, they are not testable.

Our first step in trying to justify the assumption necessary for identification is to select an untreated group of reservations that is likely to be similar to the treatment group. Hence, our sample only includes reservations that have at least started the adjudication process. As Sanchez et al. (2020) show, a variety of reservation-specific characteristics including irrigable acreage and water scarcity affect the probability that a tribe initiates an adjudication. In addition to the 26 reservations that settled Winters rights between 1974 and 2012, another 31 initiated a claim but had not settled by 2012. We include these latter reservations as our untreated group. Although Table A1 indicates some baseline differences between these groups in 1974, identification relies on the comparison of *trends* and *shocks* across these two groups that may be correlated with the timing of treatment.

Figure A1 depicts the start and end dates of each reservation's adjudication, along with dashed red lines for the years in which we observe land use. Reservations are stacked by when their ad-

[5]The regularity conditions include: i) there is a balanced panel of *groups*, ii) treatment is sharp (binary), iii) groups are independent, and iv) there exists a group of non-switchers for each set of switchers in the data.

judications *starts*, and color-coded based on when they enter the treated group in our data (the *ending of their adjudication*). As the figure indicates, the length of adjudications are highly variable: many reservations that begin adjudicating at the same time nevertheless settle at different times, whereas some reservations that begin the process at different times settle simultaneously (through a single act of congress). Importantly, Sanchez et al. (2020) find that the speed with which settlement occurs after a tribe initiates the adjudication process is a function of several factors that are largely exogenous to the reservations, such as the majority party in Congress and the number of off-reservation parties included in the adjudication (Sanchez et al., 2020).

Despite the largely exogenous nature of Congressional actions to finalize Winters settlements, reservation or state-specific shocks could violate the identifying assumptions. For instance, an unobserved shock to a reservation's development potential could alter their bargaining power or their incentives to end negotiations quickly and therefore be correlated with treatment. Similarly, because negotiation occurs in state courts, changes to state water policy could affect the duration or outcomes of negotiations. We take several steps to address these concerns and diagnose their likely importance for our results.

We include state-specific non-parametric trends to capture sharp changes in state water policy that could affect the outcome of Winters negotiations in the context of the de Chaisemartin and d'Haultfoeuille estimator and state-by-year fixed effects with the TWFE estimator. There were a variety of state-level changes to water policy during our study period that may have affected negotiations. Some examples include the state-by-state adoption of in-stream flow rights (Boyd, 2003), the construction of the Central Arizona Project (Glennon, 1995), and new groundwater regulations (Jacobs and Glennon, 1992). In the absence of flexible controls for year effects that vary by state, these events may compromise identification. Unfortunately, the Stata implementation of the Callaway and Sant'Anna estimator does not allow for time trends or year effects that vary by groups.

We also include several time-varying reservation-level controls that could influence the evolution of land use on reservations. The first control is off-reservation population in adjacent counties. While this is unlikely to directly affect reservation land use, it may be correlated with treatment: Sanchez et al. (2020) show that the number and heterogeneity of off-reservation parties affects the timing of settlement. Second, we include a dummy variable that is equal to one if a reservation has an active casino. This variable is equal to zero for all reservations before 1992, but it varies by reservation thereafter.[6] Finally, we include a dummy variable that is equal to one if a reservation has a tribal lending institution, which also varies over time. In addition to being common controls in the literature on Native American development (Dippel, 2014; Frye, 2014; Leonard et al., 2020; Dippel et al., 2021), these variables help capture time-varying differences in reservations economic development that may otherwise violate the common trends assumptions. Table A2 shows the evolution of these variables over time and reveals differences between reservations that never receive treatment vs. those that settle between 1974 and 2012. In the next section, we report results with and without the inclusion of these controls to better determine the extent to which they affect our estimates.

Finally, we note that we do not anticipate spillover effects of water right adjudications across reservations. Reservations are generally spatially dispersed enough to prevent a downstream reservation from benefiting from return flows from an upstream reservation's water use. Likewise, land use change in anticipation of settlement is unlikely, as a tribe's water rights must be clearly defined before the tribe can enforce water deliveries or lease that water to others. Moreover, many tribes lack the physical diversion infrastructure (or the capital to develop it rapidly) to begin diverting and using water in anticipation of a settlement (Government Accountability Office, 2006)

## 4 Main Results

This section presents the main results of our estimates of the impact of Winters rights settlement on reservation land use. Our main focus is on agricultural land use, but we also report estimates for developed land use. We also provide back-of-the-envelope calculations for what our estimates imply in terms of actual *water* use under a variety of assumptions about water use per acre. In all the results that follow, we cluster standard errors by PLSS townships, which are arbitrary 6×6-mile squares that include, on average, 144 quarter sections each, as is common in studies of agricultural land and water use (Ge et al., 2020; Hagerty, 2021).[7]

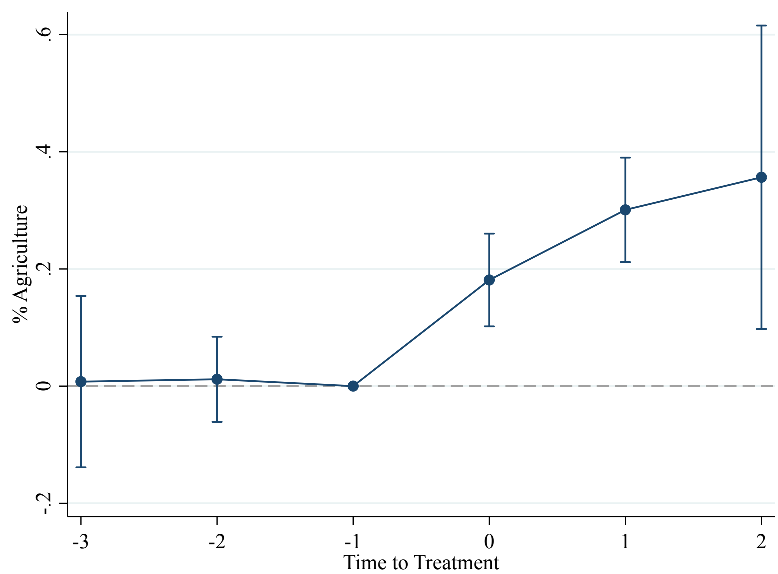
[6] The passage of the Indian Gaming Regulatory Act in 1988 is what allowed tribes to begin operating casinos.

[7] Clustering at a higher level, such as reservation, is not feasible because of the small number of reservations in our sample and our inability to combine the novel DiD estimators with techniques for valid inference with low numbers of clusters, such as the wild cluster bootstrap (MacKinnon and Webb, 2017).

### 4.1 The Effect of Winters Rights on Agricultural Land Use

We begin by presenting event study estimates to provide evidence for whether the necessary parallel trends assumptions are likely to hold in our setting. Although Table A2 reveals in the evolution of time-varying controls across treated vs. never-treated reservations, the relevant comparison for identification purposes requires focusing on trends in the untreated group relative to a treated reservation *at the time of treatment* (e.g., an event study). de Chaisemartin and d'Haultfoeuille (2020)'s estimator allows the researcher to estimate the effect of treatment in each of the $k$ periods before vs. after treatment. Our NWALT data contain a total of five periods, so we report a symmetric window that includes 2 periods prior to treatment and two years after treatment, with period "0" defined as the first year in which treatment begins. Sizing the event window in this way ensures that dynamic leads or lags are not being identified by a single reservation.[8]

Figure 2: Agricultural Land Use Event Study




**Notes:** This figure depicts event study estimates using the estimator developed by de Chaisemartin and d'Haultfoeuille (2020), implemented with the `did_multiplegt` package in Stata. The model corresponds to the specification in column 1 of Panel A of Table 1, which includes parcel fixed effects and state-by-year fixed effects. The difference between treated and untreated groups is normalized to zero in period $t-1$, the final period before treatment. Period 0 denotes the first period in which parcels are exposed to treatment.

Figure 2 presents the results of the event study estimates using the estimator proposed by de Chaisemartin and d'Haultfoeuille (2020) for our baseline specification that includes parcel fixed

[8]We have only one reservation, Maricopa (Ak Chin), for which we observe 3 years of data after period "0." Accordingly, we focus our event window on period 0 plus two years.

effects and state-specific non-parametric trends, but no time-varying reservation controls.[9] All coefficients are relative to the difference between treated and untreated parcels in the period just prior to treatment, which is normalized to 0. The results show no evidence of a pre-trend: the coefficients for periods $t-2$ and $t-3$ are near zero and statistically insignificant. From period $t=0$ onward, there is a statistically significant (and increasing) difference between the treated and untreated. Appendix Figure A4 shows that this finding is robust to including time varying controls for off-reservation population, casino presence, and credit access. Though not conclusive, this finding provides support for the common trends and exogeneity assumptions necessary for identification.

Our main estimates for the effect of Winters rights on agricultural land use are presented in Table 1. The baseline model in Column 1 does not include any time-varying reservation controls. column 2 controls for off-reservation population growth, column 3 controls for casino presence, column 4 controls for credit access, and column 5 includes all three controls. Panel A reports estimates from de Chaisemartin and d'Haultfoeuille (2020)'s method, Panel B reports estimates using the Callaway and Sant'Anna (2020) estimator, and Panel C reports estimators from the classic TWFE estimator.[10] Panel A includes state-specific non-parametric trends and Panel C includes state-by-year fixed effects, but Panel B includes only year fixed effects.[11]

The coefficient estimates in Table 1 are fairly stable across specifications and different estimators. The dependent variable is the percentage of a quarter section devoted to agricultural land use (ranging from 0 to 100). Controlling for time-varying reservation covariates tends to increase the estimated effect of Winters settlements, especially when all three controls are included. Hence, although inclusion of time-varying controls does not appear critical for the parallel trends assumption, it may nonetheless lead to a more credible comparison between reservations in the treated vs. untreated groups that were on similar land use trajectories. We do note, however, that controls for casino presence and tribal lending institutions may be endogenous because that tribes with the institutional capacity to pursue casinos and lend institutions may fare better in Winters negotiations.

[9] Implemented with the `did_multiplegt` package in Stata.

[10] Panel A estimates are derived using with the `did_multiplegt` package in Stata. Panel B estimates are derived using the `csdid` package in Stata.

[11] The Callaway and Sant'Anna (2020) estimator does not have an option for including group-varying time effects.

Table 1: The Impact of Winters Settlements on Agricultural Land Use

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Y = % Agriculture | | | | |
| *Panel A:* | | | | | |
| | *de Chaisemartin & D'Haultfoeuille (2020)* | | | | |
| Post Settlement | 0.2433*** | 0.2933*** | 0.2873*** | 0.2631*** | 0.3293*** |
| | (0.0330) | (0.0396) | (0.0324) | (0.0340) | (0.0367) |
| *Panel B:* | | | | | |
| | *Callaway & Sant'Anna (2020)* | | | | |
| Post Settlement | 0.2982*** | 0.3466*** | 0.3056*** | 0.3094*** | 0.3552*** |
| | (0.0398) | (0.1252) | (0.0404) | (0.0519) | (0.1347) |
| *Panel C:* | | | | | |
| | *Two-Way Fixed Effects* | | | | |
| Post Settlement | 0.2341*** | 0.2946*** | 0.3376*** | 0.2493*** | 0.3576*** |
| | (0.0394) | (0.0446) | (0.0446) | (0.0418) | (0.0471) |
| Observations | 1,285,935 | 1,285,934 | 1,285,935 | 1,285,935 | 1,285,934 |
| Adjusted R-squared (TWFE) | 0.986 | 0.986 | 0.986 | 0.986 | 0.986 |
| Parcel Fixed Effects | ✓ | ✓ | ✓ | ✓ | ✓ |
| Off-Reservation Population | | ✓ | | | ✓ |
| 1(Casino) | | | ✓ | | ✓ |
| 1(Tribal Lending Institution) | | | | ✓ | ✓ |

**Notes:** This table presents difference-in-difference estimates for the effect of Winters settlements based on the model in Equation 1 using several estimators. Panel A uses the estimator proposed by de Chaisemartin and d'Haultfoeuille (2020) and implemented with the `did_multiplegt` Stata package with two leads and two lags of treatment. Panel B uses the estimator proposed by Callaway and Sant'Anna (2020) and implemented with the `csdid` package in Stata. Panel C presents traditional TWFE estimates obtained via OLS. Panels A and C include state-by-year fixed effects, whereas Panel B uses pooled year fixed effects due to limitations of the `csdid` package. Standard errors are clustered by PLSS township (a 6×6-mile square containing 144 parcels) and reported in parentheses* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The coefficients are also quite consistent across all three estimators. In any given column, the differences between the three estimators are small, and they become even more similar once all of the controls are included. The diagnostics suggested by de Chaisemartin and d'Haultfoeuille (2020) provide some intuition for why this is the case. When heterogeneity creates severe bias in TWFE, it is because many of the weights emebbed in the TWFE are negative. Hence, a simple diagnostic involves estimating these weights and examining them. Table A3 reports the results of using the `twowayfeweights` package in Stata. Out of 16,469 weights, only 5 are negative, suggesting that the TWFE estimator is unlikely to be biased.[12] This is consistent with the stability of coefficient estimates presented in Table 1.

Although we find a robust and precisely estimated increase in agricultural land use after Winters settlements, the magnitude of the effect is quite small. Focusing on the largest of the coefficients in Panel A of Table 1, column 5 suggests a 0.32% increase in agricultural land use due to

[12]In the applications examined by de Chaisemartin and d'Haultfoeuille (2020) as examples of bias in TWFE, as many as half of the weights are negative.

a Winters settlement (the dependent variable ranges from 0 to 100). The average quarter section on untreated reservations is 6.08% agriculture, implying that settlement leads to a 5% increase in agriculture relative to the mean. Given the massive volumes of water associated with these settlements, this a strikingly small number.

In part, the low mean (6.08%) for % agriculture is driven by a large number of zeroes in the data. For parcels that do have at least some agriculture, the mean is 51% agricultural land use. In Table A4, we switch to a linear probability model where the dependent variable is equal to one if a parcel has any agricultural land use, and zero otherwise. We find statistically significant increases in the probability of agriculture on parcel after settlement, but the effect is still quite small. The Panel A, column 5 coefficient in Table A4 indicates that settlement increases the probability of agriculture on a parcel by 0.74 percentage points, which is a 6% increase relative to the mean of 12% on parcels in the untreated group. Finally, Table A5 focuses on cultivated crops only, excluding hay/pasture, yielding even smaller results: the 0.0694% increase in the percentage of land on a parcel devoted to cultivated crops is just a 1.4% increase relative to the mean of 4.91%.

The upshot of our results thus far is that Winters settlements lead to a statistically significant increase in agricultural land use that is robust to a variety of specifications, alternative estimators, and alternative ways of measuring agricultural land use. However, the effects are much smaller than one might anticipate given the size of the typical settlement. One possible explanation is that, contrary to our discussion in Section 2, tribes may be using their newly acquired water to support additional residential development rather than agriculture. Next, we assess this possibility.

### 4.2 The Effect of Winters Rights on Developed Land Use

We now turn our attention to developed land use on reservations. NWALT's categorization of developed land use includes essentially any "hard" infrastructure or other physical buildings. Hence, increases in development could reflect new infrastructure, new residential housing, or urbanization. Table 2 presents the results, with panels and columns defined as in Table 1. Appendix Figures A5 and A6 show the associated event study coefficients.

Unlike the agricultural land use results, the magnitudes (and signs) of the coefficient estimates in Table 2 vary based on the inclusion of controls, and to differing degrees across estimators.[13] The

[13]The same pattern is evidence in the event studies in Figure A6.

de Chaisemartin and d'Haultfoeuille (2020) and Callaway and Sant'Anna (2020) always agree in sign, if not in magnitude. For our interpretation of the results, we disregard the TWFE estimates, which diverge considerably from the robust DiD estimators.

Table 2: The Impact of Winters Settlements on Developed Land Use

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Y = % Development | | | | |
| *Panel A:* | | | | | |
| | *de Chaisemartin & D'Haultfoeuille (2020)* | | | | |
| Post Settlement | 0.0317*** | -0.0365* | 0.0272*** | 0.0318*** | -0.0378* |
| | (0.0092) | (0.0205) | (0.0099) | (0.0092) | (0.0209) |
| *Panel B:* | | | | | |
| | *Callaway & Sant'Anna (2020)* | | | | |
| Post Settlement | 0.0263*** | -0.6430*** | 0.0209** | 0.0010 | -0.5756*** |
| | (0.0114) | (0.2249) | (0.0125) | (0.172) | (0.2296) |
| *Panel C:* | | | | | |
| | *Two-Way Fixed Effects* | | | | |
| Post Settlement | 0.0389*** | 0.0079 | 0.0217 | 0.0379*** | 0.0035 |
| | (0.3709) | (0.0111) | (0.0135) | (0.0078) | (0.0119) |
| Observations | 1,285,935 | 1,285,934 | 1,285,935 | 1,285,935 | 1,285,934 |
| Adjusted R-squared (TWFE) | 0.923 | 0.924 | 0.924 | 0.923 | 0.924 |
| Parcel Fixed Effects | ✓ | ✓ | ✓ | ✓ | ✓ |
| Off-Reservation Population | | ✓ | | | ✓ |
| 1(Casino) | | | ✓ | | ✓ |
| 1(Tribal Lending Institution) | | | | ✓ | ✓ |

**Notes:** This table presents difference-in-difference estimates for the effect of Winters settlements based on the model in Equation 1 using several estimators. Panel A uses the estimator proposed by de Chaisemartin and d'Haultfoeuille (2020) and implemented with the `did_multiplegt` Stata package with two leads and two lags of treatment. Panel B uses the estimator proposed by Callaway and Sant'Anna (2020) and implemented with the `csdid` package in Stata. Panel C presents traditional TWFE estimates obtained via OLS. Panels A and C include state-by-year fixed effects, whereas Panel B uses pooled year fixed effects due to limitations of the `csdid` package. Standard errors are clustered by PLSS township (a 6×6-mile square containing 144 parcels) and reported in parentheses* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Two patterns stand out from the results in Table 2. First, the inclusion of off-reservation population controls causes the estimated effect of Winters settlements on developed land use to become negative. Second, the Callaway and Sant'Anna estimates are similar in magnitude to the de Chaisemartin and d'Haultfoeuille estimates in some specifications, but they are larger by an order of magnitude when off-reservation population controls are included. Unlike agriculture, accounting for trends in off-reservation population growth appears to be critical for assessing the effect of Winters rights on developed land use.

The changing sign from column 1 to 2 in Panels A & B of Table 2 implies that off-reservation population growth is correlated with on-reservation development. The robust DiD estimates do not report coefficient estimates for control variables, but we do find that off-reservation population growth is positively correlated with on-reservation development in supplemental tests. This suggests that failing to account for off-reservation population trends would cause us to mis-attribute the effect of off-reservation population growth to Winters settlements.

Why is it that off-reservation trends appear so consequential for development, but not agriculture? One possible explanation is that on-reservation housing development may be indirectly affected by off-reservation housing markets. Many tribal members leave near their reservations but not on them. Hence, changes in nearby off-reservation housing markets could affect the demand for housing on the reservation if, for example, tribal members get priced out of off-reservation markets and seek to return to the reservation. Given the negative signs in columns 3 and 5 of Table 2, it may be that tribes that invest in Winters adjudications do so at the expense of other development opportunities. Unfortunately, we lack the data to test this hypothesis.

Although the developed land use results are somewhat ambiguous, it is important to remember that agricultural water use per-acre dwarfs residential and urban water use per-acre. Our primary goal is to understand the implications of Winters rights for actual water use. For this purpose, agricultural land use change is the most important driver.

### 4.3 Water Use Estimates

Next, we use our land use estimates to develop back-of-the-envelope estimates of total water use on each reservation in 2012. We then compare these estimates to i) predicted changes in water use due to settlement and ii) actual settlement amounts. This allows us to characterize how much of their settlement each tribe is using, and how much of this use is attributable to post-settlement changes in land use associated with our estimates.

We sum total agricultural acres and developed acres from the 2012 NWALT data by reservation and multiply by conversion factors for water use per acre. We assume that developed land uses an average of 0.25 acre-feet (AF) per acre. This is a generous assumption considering that 48 percent of reservation households lack access to water and sanitation infrastructure (Democratic Staff of the House Committee on Natural Resources, 2016). For agricultural land, we construct estimates based on varying levels of water use ranging from 2 to 5 acre-feet per acre (AFA).[14] We estimate

[14] This covers the range of water use per acre for the most common crops grown in the U.S. West (Johnson and Cody, 2015), which varies from 0.6 AFA for berries to 5 AFA for sugar beets.

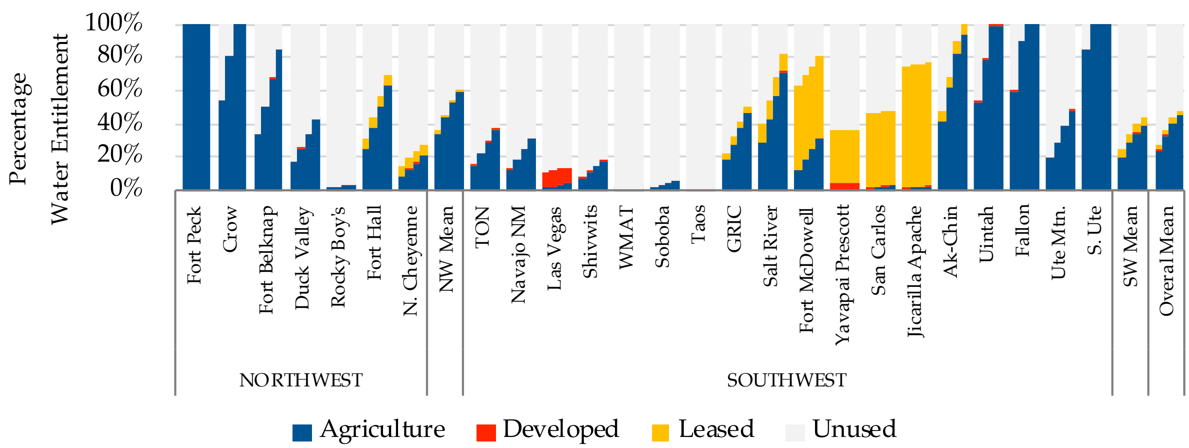
the share of water use on reservation $r$ in 2012 as:

$$\hat{Use_r} = \frac{AgAcres_r \times AFA_{ag} + DevAcres_r \times 0.25 + Leased_r}{Settlement_r} \tag{2}$$

where and $Leased_r$ is water being leased to off-reservation users by reservation $r$ in 2012. We censor our estimate at 100 percent in cases where predicted water use exceeds available water.

Figure 3 depicts estimates of the share of unused settlement water for each treated reservation in the sample. For each reservation, we present four scenarios corresponding to an assumed 2, 3, 4, or 5 AFA in agricultural water use, from left to right. Our estimates suggest that Fort Peck is using its entire entitlement under all four scenarios and that several other reservations may be using their full entitlement under high water-use scenarios. Others, like Soboba or Taos, were using little to none of their settlement by 2012.

Figure 3: 2012 Water Use Estimates




**Notes:** This figure depicts estimated water use in 2012 for each treated reservation in our sample using calculation in Equation 2. The estimates include water for agriculture, developed land use, and off-reservation water leasing. We assume 0.25 AF/acre for developed land use. For each reservation, water use estimates depicted by each bar, from left to right, assume agricultural water use estimates of 2, 3, 4, and 5 AF/acre. The gray area represents the share of a reservation's water settlement that we estimate is used in 2012.

Next, we use the estimates from column 1 in Panel A of Tables 1 and 2 to calculate the share of $\hat{Use_r}$ that is attributable to changes in land use associated with the settlement of a Winters right.[15] To do so, we take the average predicted change in land use for a 160-acre quarter section and multiply by the number of usable quarter sections on each reservation. Some reservation lands are likely to be unsuitable for agriculture, even with abundant water rights, because many reservations lack quality farmland in significant quantities (Leonard et al., 2020). To account for this, we

[15]We use the column 1 coefficients to generate the largest (most optimistic) predicted increase in water use.

only utilize quarter sections from 6x6-mile townships where we observe at least 160 acres worth of agricultural land use. After excluding non-usable areas, we estimate the predicted change in water use according to the following calculation:

$$\Delta \hat{U}se_r = \left( \frac{\hat{\beta}_{ag}}{100} \times AFA_{ag} + \frac{\hat{\beta}_{dev}}{100} \times 0.25 \right) \times \overline{ParcelAcres}_r \times N_r^P \tag{3}$$

where $N_r^P$ is the number of usable quarter sections on reservation $r$, and $\overline{ParcelAcres}_r$ is the average size of quarter sections on reservation $r$ (in practice, this is almost always 160 but does vary in some cases). $\hat{\beta}_{ag}$ and $\hat{\beta}_{dev}$ are the de Chaisemartin and d'Haultfoeuille (2020) coefficients estimating changes to agriculture and developed land use.

The results are depicted in Figure A7, which shows predicted changes to on-reservation water use attributable to settlement for each AF/acre scenario. We estimate that settlement induced changes in on-reservation water use across reservations, account for less than 2 percent of tribes' total water entitlements. When we include off-reservation leasing, our estimates of post-settlement changes to water use increase to an average of 3-5 percent. The extent to which tribes lease water rights off-reservation is highly variable, with a few reservations in the Southwest leasing large portions of their water entitlements.

Our results are striking. Given the growing deficit between water supply and demand in the west, and consequent increases in the value of water in the region, tribes' collective acquisition of titles to 5.57 million AF should be an economic windfall. Yet, our estimates show that tribes are not capturing the full value of their water rights either through on-reservation water use or by transferring water off-reservation. Water use estimates suggest that tribes are missing out on significant economic gains associated with a resources that they own on paper but not in practice. Should tribes fully use their remaining entitlements entirely for agriculture, under a two acre-foot per acre water duty, they could irrigate an additional 1.3 million acres. Using year and state-specific water market pricing data from the Water Transactions Dataset (Donohew and Libecap, 2010), we estimate the total value of water foregone by tribes in 2012. Assuming 3 AF/year water use for agriculture to estimate what tribes *are* using, we estimate that the market value forgone in unused water is approximately $1.4 billion.

## 5 Mechanisms

One possible explanation for the divergence between water entitlements and use is that our measures of water use do not fully capture the diverse priorities and goals that tribes have for water. For instance, many reservations in the Northwest use large portions of their water rights to maintain streamflow. However, legal streamflow protections in Southwestern states are limited, and none of the water settlements included in our study specifically allocated water for streamflow.

Another possibility is that tribes use newly acquired water rights to intensify irrigation by shifting from low-value hay and pasture to higher-value crop production without expanding farmed acreage. The results for increases in cropped acreage in Table A5 are small relative to those in overall agricultural land in Table 1, suggesting that this is not the case. It is also possible that reservation farmers are switching to more water-intensive crops after settlement. However, the 4 and 5 AFA estimates presented in Figure A7 indicate that most reservations would still not be using their full allocation, even if they switched entirely to the most water-intensive crops such as rice, alfalfa, and sugar beets (Johnson and Cody, 2015).

The most likely explanation, based on the magnitude of unexercised tribal water rights, is that the majority of tribal water rights are still being used off-reservation and without compensation. One reason for this may be tribes' inability to put water to productive use on reservations. Even though tribal water rights they cannot be forfeited through non-use, they may nevertheless be insecure and open to continued appropriation by off-reservation users (who no longer own the water) so long as tribes lack the ability to use them. A key barrier to productive on-reservation land uses identified in previous work is land tenure. Using recently developed parcel-level measures of land tenure, we explore whether the ownership regimes prevalent on many reservations constrain land use change when tribes receive water rights.

There are also broader obstacles to tribal control over their water rights. Because federal funding allocated through water settlements is often discretionary rather than mandatory, tribes have struggled to secure annual payments to support water infrastructure projects (Stern, 2017). Legal challenges to water settlements have delayed their implementation, while tribal infrastructure projects, unlike those constructed off-reservation for non-tribal farmers in the early 1900s, are subject to Endangered Species Act, National Environmental Protection Act, and state environmental regulations (Blumm et al., 2006). We lack the data to analyze the specific empirical magnitudes of these barriers. We can, however, assess whether tribes that engage in off-reservation leasing are able to finance additional agricultural or other development via this new revenue source.

### 5.1 Land Tenure as a Barrier to Land Use Change

Previous literature has found that land tenure presents significant barriers to development on Native American Reservations (Anderson and Lueck, 1992; Ge et al., 2020; Leonard et al., 2020; Dippel et al., 2020). Beginning in 1887, the Dawes General Allotment Act authorized the Office of Indian Affairs to allocate tribal land to individual Native American households. These allotments were typically held in trust by the federal government for 25 years until the allottee was deemed "competent" to hold fee simple title. Allotted trust lands could not be transferred or included in an individual's will. The allotment process abruptly ended in 1934, resulting in a a complicated mosaic on many reservations consisting of of fee-simple parcels, allotted trust parcels owned by individuals but held in trust with the federal government, and tribal parcels that were never allotted (Carlson, 1981; Leonard et al., 2020).

The land tenure mosaic found on many reservations creates several barriers to productive land use. The non-transferrability of allotted trust lands precludes their use as collateral for accessing credit, prevents land assembly to efficient farm sizes, and has led to fractionated ownership due to common heirship, wherein a single trust parcel can be shared by over 100 owners who must agree to any changes in land use (Dippel et al., 2020). Tribal land avoids many of these pitfalls, but tribes must confront federal regulatory hurdles not present on private land due to federal trusteeship (Leonard and Parker, 2021). Fee simple land entails far fewer constraints on land use, though it is still within the jurisdiction of the tribal government. The upshot is that trusteeship on allotted and tribal and may prevent land use changes via a complex nexus of transaction costs, credit constraints, and bureaucratic hurdles.

We use land tenure data developed by Dippel et al. (2020) from BLM digital records documenting changes in land ownership on reservations. Land patents issued on each reservation during the 1877-1934 Allotment Era were filed with the Government Land Office (GLO). Each patent contains the parcel location, as indicated by the BLM's Public Land System Survey (PLSS), the Indian allotment number, date it was issued in trust, and the date when it was converted from trust to fee simple ownership. By tracking parcel ownership over time, we limit our sample to parcels that have not changed in land tenure status since 1974 (the first year of land use data availability). Parcels are categorized into three discrete land tenure groups: fee-simple, tribal trust, and allotted trust land. The last three rows of Table A1 show the share of each type of ownership on treated vs. untreated reservations. Overall, treated reservations have a larger share of fee simple and allotted trust land, and a lower share of tribal trust land.

To explore the impact of land tenure on the ability to change land use in response to a Winters settlement, we estimate a difference-in-difference-in-difference (DDD) model that allows the effect of Winters settlements to vary by land tenure class. Our estimating equation is:

$$y_{irt} = \beta_F PostSettlement_{rt} + \beta_A PostSettlement_{rt} \times Allotted_i + ...$$
$$... + \beta_T PostSettlement_{rt} \times Tribal_i + \beta_2 X_r t + \vec{\lambda}_i + \vec{\tau}_t + \varepsilon_{irt} \tag{4}$$

where $Allotted_i$ is an indicator that is equal to one for allotted trust parcels and $Tribal_i$ is an indicator that is equal to one for tribal trust parcels. $\beta_F$ is the estimated effect of Winters rights on fee simple parcels, the omitted group, and $\beta_A$ and $\beta_T$ report the difference in this effect for allotted and tribal parcels, respectively. All other parameters are defined as in Equation 1. Importantly, our model includes parcel fixed effects that absorb all time-invariant differences between parcels in each land tenure class, such as soil quality, terrain, and proximity to water.

Table 3 presents our estimates of the parameters in Equation 4 for agricultural land use using TWFE. Unfortunately, the estimators proposed by de Chaisemartin and d'Haultfoeuille (2020) and Callaway and Sant'Anna (2020) cannot be used to estimate a difference-in-difference-in-difference model. Given the similarity of the estimators in Table 1 and the small number of negative weights reported in Table A3, we believe these TWFE estimates are reliable, especially for agricultural land use. Moreover, by allowing for different treatment effects by land tenure class, we are embedding flexibility in the model, reducing the likelihood that remaining heterogeneity will bias the TWFE estimates (Wooldridge, 2021). We also report separate DiD estimates for each land tenure class using both robust DiD estimators in Tables A6 and A7.

The results in Table 3 reveal substantial differences in the impact of Winters settlements across land tenure classes. On fee simple land, settlement increases agricultural land use by roughly 0.83%, over twice the pooled coefficient in Table 1. This is a 13.7% increase relative to mean agricultural land use on untreated parcels. The coefficient estimates for allotted parcels indicate that increases in agricultural land use are roughly half has large on allotted parcels, and changes to tribal land use are even smaller. The p-value for the sum of the allotted interaction term and the baseline fee simple effect, reported in the bottom of the table, indicates that increases on allotted and tribal parcels, though much smaller than fee parcels, are still statistically different from zero.

Table 3: Differential Impacts by Land Tenure Class: Agriculture

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | | | Y = % Agriculture | | |
| Post Settlement (Fee Simple) | 0.820*** | 0.836*** | 0.836*** | 0.819*** | 0.837*** |
| | (0.127) | (0.127) | (0.128) | (0.127) | (0.128) |
| Post Settlement X Allotted | -0.439*** | -0.434*** | -0.402*** | -0.435*** | -0.402*** |
| | (0.150) | (0.150) | (0.151) | (0.149) | (0.151) |
| Post Settlement X Tribal | -0.750*** | -0.731*** | -0.662*** | -0.740*** | -0.661*** |
| | (0.128) | (0.129) | (0.133) | (0.128) | (0.133) |
| Observations | 1,266,645 | 1,266,644 | 1,266,645 | 1,266,645 | 1,266,644 |
| Adjusted R-squared | 0.986 | 0.986 | 0.986 | 0.986 | 0.986 |
| N Clusters | 3254 | 3254 | 3254 | 3254 | 3254 |
| p-value (Fee + Allotted) | 0.00266 | 0.00147 | 0.000602 | 0.00232 | 0.000579 |
| p-value (Fee + Tribal) | 0.0428 | 0.00944 | 0.0000649 | 0.0425 | 0.000137 |
| Parcel Fixed Effects | ✓ | ✓ | ✓ | ✓ | ✓ |
| Off-Reservation Population | | ✓ | | | ✓ |
| 1(Casino) | | | ✓ | | ✓ |
| 1(Tribal Lending Institution) | | | | ✓ | ✓ |

**Notes:** This table presents estimates of the difference-in-difference-in-difference model presented in Equation 4 using TWFE. The omitted category for the baseline difference is fee simple land tenure. Table A6 presents alternative DiD estimates for each group separately using the methods proposed by de Chaisemartin and d'Haultfoeuille (2020) and Callaway and Sant'Anna (2020), which cannot be used directly to estimate a DDD model. Standard errors are clustered by township and reported in parentheses* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 presents the tenure-specific results for developed land use. As with agriculture, positive coefficients for fee simple parcels are much larger than the grouped coefficients in Table 2. Unlike the grouped results in Table 2, the positive effect of settlements on development on fee simple land is not sensitive to the inclusion of time-varying reservation controls. The differences in development effects on allotted trust parcels are not statistically significant (though they are all negative). Tribal trust land is significantly different in all specifications: the results suggest that Winters settlements even lead to a net negative effect on development after controlling for off-reservation population trends. Hence, the puzzling results in Table 2 appear to be driven by differing responses by land tenure types, namely development patterns on tribal lands that are correlated with off-reservation population growth.

Table 4: Differential Impacts by Land Tenure Class: Development

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | | | Y = % Development | | |
| Post Settlement (Fee Simple) | 0.204*** | 0.184*** | 0.199*** | 0.204*** | 0.183*** |
| | (0.0485) | (0.0461) | (0.0477) | (0.0486) | (0.0461) |
| Post Settlement X Allotted | -0.0667 | -0.0724 | -0.0783 | -0.0682 | -0.0779 |
| | (0.0604) | (0.0590) | (0.0594) | (0.0598) | (0.0586) |
| Post Settlement X Tribal | -0.202*** | -0.226*** | -0.230*** | -0.206*** | -0.238*** |
| | (0.0495) | (0.0519) | (0.0524) | (0.0505) | (0.0537) |
| Observations | 1,266,645 | 1,266,644 | 1,266,645 | 1,266,645 | 1,266,644 |
| Adjusted R-squared | 0.923 | 0.923 | 0.923 | 0.923 | 0.923 |
| N Clusters | 3254 | 3254 | 3254 | 3254 | 3254 |
| p-value (Fee + Allotted) | 0.0273 | 0.0489 | 0.0428 | 0.0266 | 0.0608 |
| p-value (Fee + Tribal) | 0.895 | 0.00269 | 0.0275 | 0.911 | 0.000768 |
| Parcel Fixed Effects | ✓ | ✓ | ✓ | ✓ | ✓ |
| Off-Reservation Population | | ✓ | | | ✓ |
| 1(Casino) | | | ✓ | | ✓ |
| 1(Tribal Lending Institution) | | | | ✓ | ✓ |

**Notes:** This table presents estimates of the difference-in-difference-in-difference model presented in Equation 4 using TWFE. The omitted category for the baseline difference is fee simple land tenure. Table A7 presents alternative DiD estimates for each group separately using the methods proposed by de Chaisemartin and d'Haultfoeuille (2020) and Callaway and Sant'Anna (2020), which cannot be used directly to estimate a DDD model. Standard errors are clustered by township and reported in parentheses* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Taken as a whole, the results in Tables 3 and 4 are quite consistent with the existing literature on reservation land tenure. Ge et al. (2020) find significantly less irrigation investment on tribal land relative to fee simple. Similarly, we find that tribal lands see the smallest increase in agricultural land use in response to Winters settlements. Dippel et al. (2020) find a rank ordering of fee simple parcels, then tribal parcels, followed by allotted trust parcels in terms of both agricultural and developed land use. Our results for agriculture match their rank ordering, though we do not find differences in the effect on development across fee-simple vs. allotted trust parcels.[16] However, given the larger water intensity of agricultural land use, our results imply that both allotted and tribal trust lands limit the ability of tribes to utilize their full water entitlements.

[16]This finding may be explained by Leonard and Parker (2021), who find that the relative merits of allotted trust (or fee simple) tenure vs. tribal tenure depends on the scale of a development/resource project, with the transaction costs of allotted trust land rising as projects grow large and must include multiple parcels. This is consistent with our findings because agriculture tends occur at much larger scales than development, suggesting larger costs associated with allotted trust parcels in one land use than the other.

## 5.2 Land Tenure and Water Use

To better understand the extent to which constraints on land tenure affect tribes' under-utilization of settlement water, we next explore water use across tenure classes. First, we construct a counterfactual of changes to land use as the result of a settlement under the assumption that all parcels experienced the same increase in land use as observed on fee land:

$$\Delta\tilde{U}se_r = \left(\frac{-\hat{\beta}^F_{ag}}{100} \times AFA_{ag}\right) \times \overline{ParcelAcres}^A_r \times N^A_r + ...$$
$$\left(\frac{-\hat{\beta}^T_{ag}}{100} \times AFA_{ag} + \frac{-\hat{\beta}^T_{dev}}{100} \times 0.25\right) \times \overline{ParcelAcres}^T_r \times N^T_r \tag{5}$$

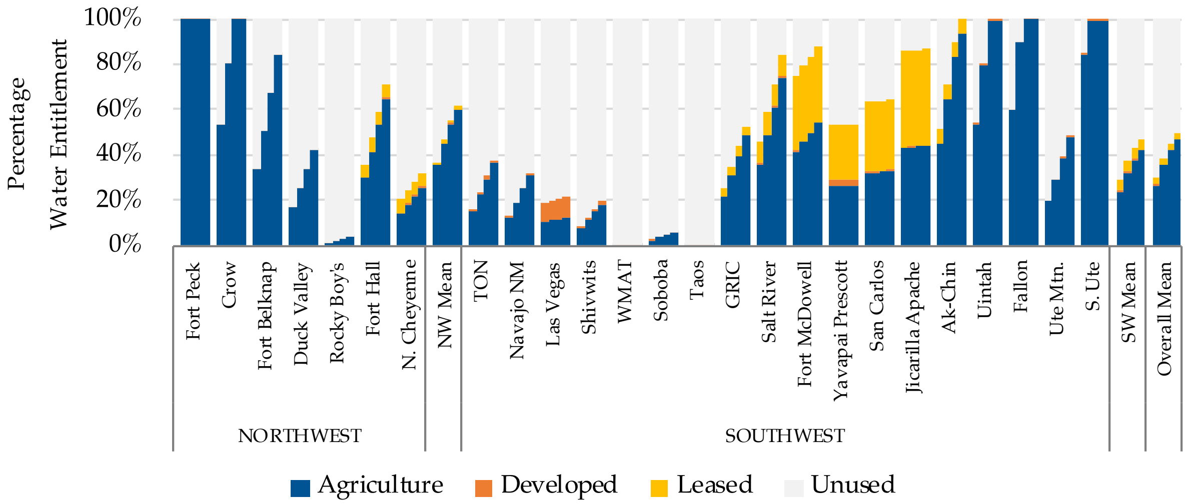
where $ParcelAcres^A_r$ and $ParcelAcres^T_r$ are the average size of allotted trust and tribal trust parcels, respectively, and $N^A_r$ and $N^T_r$ are the number of allotted trust or tribal trust parcels on reservation $r$. Essentially, Equation 5 removes the negative allotted effect from allotted parcels and the negative tribal effect from tribal parcels to predict overall changes in water use if all restrictions associated with land tenure were removed. Appendix Figure A8 depicts the results.

Next, we take the estimated counterfactual changes to water use and add them to our estimates of *actual* total water use from Equation 2 (and Figure 3) to construct a counterfactual estimate of what water use on each reservation would have been in 2012 in the absence of land tenure constraints. Figure 4 presents the results. Under counterfactual water use predictions, a far greater number of reservations use their full entitlement, or close to it. However, a number of reservations still fall far short of full utilization.

As a final step, we decompose the estimates presented in Figure 4 in an attempt to isolate the effects of land tenure restrictions on water use from the residual component associated with other factors. To do this, we estimate the portion of unused water in 2012 as $Un\hat{u}sed_r = Settlement_r – U\hat{s}e_r$. We express $Un\hat{u}sed_r$, $U\hat{s}e_r$ and $\Delta\tilde{U}se_r$ as shares of $Settlement_r$. The results are depicted in Figure 5. The blue shading indicates the estimated share of a settlement actually used in 2012. The orange shading corresponds to $U\hat{s}e_r$ and indicates how much more water would be used if the reservation were entirely fee simple land. The grey shading corresponds to water that is unused even in the counterfactual scenario, and hence attributable to factors other than land tenure.

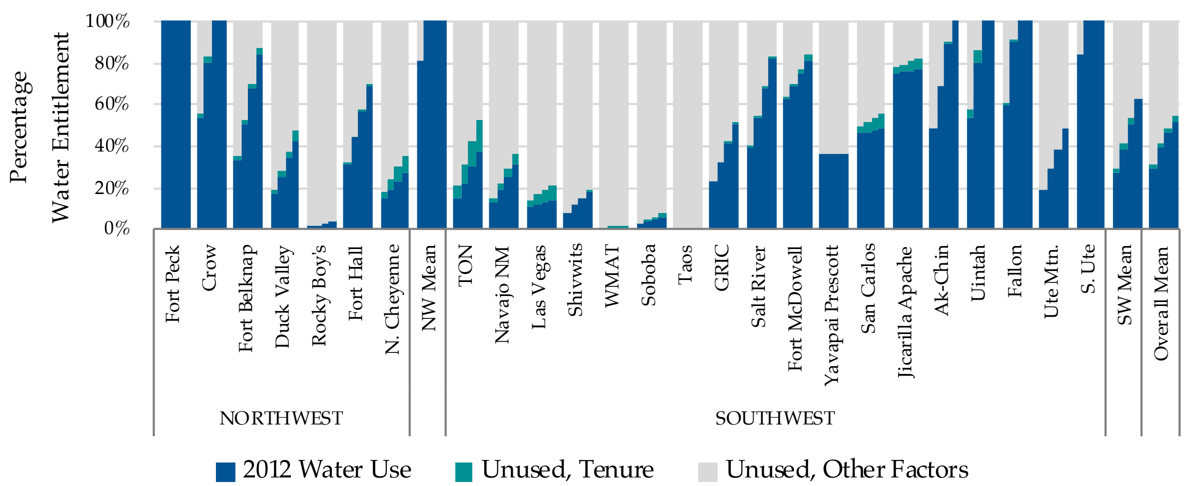
Results indicate that constraints on land tenure are a meaningful barrier to expanding water

Figure 4: 2012 Counterfactual Water Use Estimates




**Notes:** This figure depicts our estimates of counterfactual 2012 water use if the barriers associated with allotted trust and tribal trust land were removed. The estimates are obtained by adding the results of Equation 5 to Equation 2. We assume 0.25 AF/acre for developed land use. For each reservation, water use estimates depicted by each bar, from left to right, assume agricultural water use estimates of 2, 3, 4, and 5 AF/acre.

Figure 5: Decomposition of Barriers to Water Use




**Notes:** This figure depicts the share of each reservation's entitlement that we estimated to be used (blue), unused due to land tenure constraints (green), and unused due to other factors (grey). To do this, we estimate the portion of unused water in 2012 as $Un\hat{used}_r = Settlement_r - \hat{Use}_r$. We express $Un\hat{used}_r$, $\hat{Use}_r$ and $\Delta\hat{U}se_r$ and shares of $Settlement_r$. We assume 0.25 AF/acre for developed land use. For each reservation, water use estimates depicted by each bar, from left to right, assume agricultural water use estimates of 2, 3, 4, and 5 AF/acre.

use on some reservations but are less consequential on others. Differences appear to be driven by a combination of a) the amount of non-fee land on a reservation, and b) the timing of a settlement. All else equal, reservations with large areas of tribal and allotted trust land stand to gain more in a counterfactual scenario where those parcels are free from trust land constraints.

### 5.3 Off-Reservation Water Leasing

Through negotiated settlement agreements, many tribes have secured Congressional authorization to lease their water off-reservation. Leasing can provide tribes with relatively swift economic return on their water entitlements — particularly given costs and delays associated with building infrastructure for on-reservation water use — while also mitigating conflicts over limited water supplies with off-reservation users (Bovee, 2015; Nyberg, 2014). Water markets literature demonstrates that when barriers to water marketing are sufficiently low, scarce water is transferred from relatively low value water use, such as agriculture, to more efficient, higher value urban and environmental uses (Brewer et al., 2008). Marketing also increases tribes' flexibility to use water to meet reservation needs and priorities. For instance, Gila River Indian Community deposits leasing revenue in an endowment fund to subsidize water for reservation farmers and reinvest in irrigation system maintenance, while nearby Fort Apache Reservation leases the majority of its water to Arizona cities and largely eschews farming (Arizona Water Banking Authority, 2019; Amended and Restated White Mountain Apache Tribe Water Rights Quantification Agreement, 2012).

However, existing literature also highlights high costs associated with locating willing lessees, agreeing on leasing terms, verifying that downstream water users will not be harmed in a transaction, and conveying water to (Edwards and Libecap, 2015; Womble and Hanemann, 2020; Leonard et al., 2020). Beyond requiring express authorization from Congress to lease water off-reservation, tribes face additional leasing constraints. Most settlements require the Secretary of Interior to approve individual leases (Nyberg, 2014). In the Southwest, settlements have limited marketing to specific water sources, or to certain municipalities, which reduces marketing opportunities and potentially reduces leasing revenue (Royster, 2013). The extent to which a reservation leases water rights off-reservation potentially influences land and water use decisions, particularly as differences in the marginal value of water use on- and off-reservation increase. High transaction costs paired with diminishing gains from trade may limit the extent to which tribes pursue and ultimately benefit from water leasing.

To test how water leasing shapes changes in land use, we collect primary data on whether a reservation leases water rights from settlement texts, federal water project reports, and state water right databases. We examine the effect of leasing using a DDD model of the form:

$$y_{irt} = \beta_N PostSettlement_{rt} + \beta_L PostSettlement_{rt} \times Lease_{rt} + \beta_2 X_r t + \vec{\lambda}_i + \vec{\tau}_t + \varepsilon_{irt} \tag{6}$$

where $Lease_{rt}$ is equal to one on in post-settlement year if a parcel lies on a reservation that leases some or all of its settlement water (and a value of 0 for parcels on reservations that have not settled yet or do not lease settlement water). All other parameters and variables are defined as in Table 1. In this framework, $\beta_N$ represents the effect of a Winters settlement on non-leasing reservations (the omitted group) and $\beta_L$ represents the difference in the effect of settlement for reservations that lease some portion of their water rights back to off-reservation users. As before, we estimate the model using TWFE because the robust DiD estimators do not allow for a third difference.

Table 5 presents the results of estimating Equation 6. Panel A reports results for agricultural land use and Panel B reports results for developed land use. The specifications vary across columns as in Table 1. Panel A indicates that non-leasing reservations see increases in agricultural land use from Winters settlements. These increases are larger than the pooled estimated in Table1, but not dramatically so. Panel A also indicates that reservations that lease a portion of their water rights tend to have smaller increases in agricultural land use. In fact, in three of five specifications, we fail to reject the null that there is no net increase in agricultural land use for reservations that lease out a portion of their entitlement. This may reflect a selection effect, where reservations that lack the institutional capacity to develop the necessary infrastructure for irrigation agriculture instead pursue their ability to lease water to other others.

Panel B of Table 5 presents the results for developed land use. Across all five specifications, there is no evidence of a statistically significant increase in development for non-leasing reservations. As in Table 2, controlling for off-reservation population leads to the conclusion that a Winters settlement actually *decreases* the growth in developed land use relative to parcels on untreated reservations. In contrast, the interaction term with the leasing term is positive and significant

Table 5: Differential Impacts by Leasing Status

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| *Panel A:* | | | | | |
| | Y = % Agriculture | | | | |
| Post Settlement | 0.297*** | 0.382*** | 0.379*** | 0.305*** | 0.414*** |
| | (0.0448) | (0.0531) | (0.0481) | (0.0463) | (0.0531) |
| Post Settlement X Lease | -0.284*** | -0.344*** | -0.214*** | -0.262*** | -0.253*** |
| | (0.0731) | (0.0775) | (0.0739) | (0.0714) | (0.0804) |
| Adjusted R-squared | 0.986 | 0.986 | 0.986 | 0.986 | 0.986 |
| p-value (Post Settlement + Post Settlement X Lease) | 0.840 | 0.541 | 0.0191 | 0.507 | 0.0233 |
| *Panel B:* | | | | | |
| | Y = % Development | | | | |
| Post Settlement | 0.00708 | -0.0363** | -0.00404 | 0.00739 | -0.0373** |
| | (0.0109) | (0.0165) | (0.0122) | (0.0110) | (0.0167) |
| Post Settlement X Lease | 0.144*** | 0.174*** | 0.134*** | 0.145*** | 0.184*** |
| | (0.0501) | (0.0568) | (0.0483) | (0.0497) | (0.0598) |
| Adjusted R-squared | 0.924 | 0.924 | 0.924 | 0.924 | 0.924 |
| p-value (Post Settlement + Post Settlement X Lease) | 0.00275 | 0.00290 | 0.00583 | 0.00231 | 0.00319 |
| Observations | 1,285,935 | 1,285,934 | 1,285,935 | 1,285,935 | 1,285,934 |
| N Clusters | 3293 | 3293 | 3293 | 3293 | 3293 |
| Parcel Fixed Effects | ✓ | ✓ | ✓ | ✓ | ✓ |
| Off-Reservation Population | | ✓ | | | ✓ |
| 1(Casino) | | | ✓ | | ✓ |
| 1(Tribal Lending Institution) | | | | ✓ | ✓ |

**Notes:** This table presents estimates of the difference-in-difference-in-difference model presented in Equation 6 using TWFE. The omitted category for the baseline difference is reservations that do not lease any water. Standard errors are clustered by township and reported in parentheses* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

across all specifications. Moreover, the p-value on the sum of both terms indicates that reservations that do lease a portion of their entitlement see statistically significant increases in developed land use, even after controlling for off-reservation population.

The leasing results suggest that tribes may face financial hurdles to development in addition to the land tenure issues previously discussed. There is cross-sectional heterogeneity in the extent of irrigation infrastructure on reservations prior to Winters settlements, thanks to historical legacies of both allotment and BIA water projects. There is also heterogeneity in tribes' access to lending institutions and their ability to attract capital investments from off-reservation Dippel et al. (2021). Hence, some tribes are more poised than others to begin making using of their new water entitlements. The Panel A results in Table 5 imply that tribes are not using leasing revenues to develop irrigation infrastructure, as those tribes that do lease water see smaller increases (if any) in agricultural land use. On the other hand, tribes that lease their water exhibit a much stronger and more robust development response than tribes that do not. This suggests that leasing water to off-reservation users may provide an income stream for tribes to overcome financial barriers that previously constrained development.

# 6 Conclusion

Water right security has been fundamental to agricultural and economic development across the western United States (Hanemann, 2014; Leonard and Libecap, 2019), where water systems are fully appropriated (Grantham and Viers, 2014) and even small shifts in the distribution of water entitlements and water use can impact other water users. Consistent with this received wisdom, we show that tribal water settlements lead to an expansion of agricultural land use on reservations. This runs counter to regional water use trends showing declining agricultural water use as market-based transactions redirect water away from low-value agriculture to higher value municipal, industrial, and environmental water uses (Brewer et al., 2008; Dieter, 2018).

Ultimately though, our findings indicate that most tribes are using a small fraction of their water entitlements, potentially leaving as much as $1.4 billion in annual revenue on the table. This finding is consistent with surveys find that even after reservations that have secured Winters rights, nearly 48 percent of households continue to lack indoor plumbing, sanitation infrastructure, and potable drinking water (Crepelle, 2019; Rodriguez-Lonebear et al., 2020; Democratic Staff of the House Committee on Natural Resources, 2016).

Given large and growing tribal water allocations, understanding tribal water use priorities and obstacles is critical to shaping regional drought adaptation strategies and to addressing economic underdevelopment on reservations. For instance, the gradual expansion of relatively low-value agriculture provides opportunities to compensate tribes for conserved water by financing irrigation efficiency improvements, addressing barriers to water leasing, or negotiating water sharing agreements that meet shared basin priorities.

Tribes included in this study have secured rights to 5.8 million acre-feet of water — a volume that greatly exceeds the 3-5 percent change in the volume of water that tribes actually use on-reservation and lease off-reservation. The fact that water withdrawals in the Colorado River Basin regularly exceed supply by 1 million AF implies that there are lucrative opportunities to market water to willing lessees. While agriculture expands following settlement more in the southwest than on northwest reservations, and a greater percentage of entitlements are leased, even under the most optimistic scenario, southwest reservations are only using two-thirds of their entitlements.

More broadly, under-utilization of reservation land can be explained by a myriad of factors that prevent land use change necessary to fully utilize water rights. Many reservations suffer from lack of access to credit, unclear commercial codes, and legacies of destabilizing events (Dippel, 2014; Dippel et al., 2021; Frye, 2014). Reservations tend to be located in geographically remote areas, where low population densities hinder economies of scale, while critical infrastructure, such as roads, electricity are underdeveloped on many reservations (Mauer, 2017). Combined, these challenges raise the costs of increasing both agriculture and non-agricultural development on reservations.

This is not to say that tribal water use is limited to agriculture or leasing, or that tribal water use will not increase over time. The Gila River Indian Community in Arizona, for example, banks a portion of its water entitlement as groundwater to recharge the aquifer underlying the reservation and to restore wetlands, and continues to expand irrigated agriculture and commercial development on the reservation since its 2005 water settlement. In recent years, Colorado River Basin tribes have increased the volume of water leased to maintain water levels in Lake Mead (Arizona Water Banking Authority, 2019). Yet, the gap between water use and water entitlements that exist on paper is remarkable given regional water constraints.

Legally enforceable water rights should mitigate conflict over water by reducing uncertainty over its ownership, facilitate accurate accounting of regional water supply and demand, and allow tribes to capture the value of their water. However, because tribes lack flexibility in how they can exercise and benefit from legally secure water rights, these benefits have been limited. Incentives embedded in the adjudication process, high transaction costs associated with transferring water rights, and institutional constraints that increase costs of land use mean that junior appropriators have continued to benefit from unused tribal water without compensation.

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# Appendix

Table A1: Pre-Settlement Parcel Summary Statistics (1974)

| | (1) Untreated Group | (2) Settlement Parcels | (3) Settlement - Untreated |
|---|---|---|---|
| % Agriculture | 4.209 | 8.198 | 3.989*** |
| | (18.014) | (24.167) | (0.728) |
| % Development | 0.329 | 0.320 | -0.010 |
| | (2.654) | (2.686) | (0.039) |
| Avg. Soil PI | 6.321 | 8.384 | 2.062*** |
| | (4.380) | (4.562) | (0.181) |
| Avg. Elevation | 1,670.449 | 1,507.905 | -162.544*** |
| | (366.804) | (667.274) | (22.996) |
| Ruggedness | 17.893 | 19.184 | 1.291* |
| | (23.618) | (25.924) | (0.729) |
| Distance to Stream | 18,082.471 | 14,779.769 | -3,302.703*** |
| | (16,865.688) | (18,709.156) | (796.651) |
| Fee Simple | 0.102 | 0.122 | 0.020** |
| | (0.303) | (0.327) | (0.009) |
| Allotted Trust | 0.034 | 0.113 | 0.079*** |
| | (0.182) | (0.316) | (0.007) |
| Tribal Trust | 0.864 | 0.765 | -0.099*** |
| | (0.343) | (0.424) | (0.013) |
| Observations | 112,254 | 144,933 | 257,187 |

**Notes:** This table presents baseline (1974) summary statistics for parcels that are always untreated (column 1), or eventually treated (column 2), and the difference between the two (column 3). Standard errors are clustered by township and reported in parentheses* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Time-Varying Summary Statistics

| | | (1) | (2) | (3) | (4) |
|---|---|---|---|---|---|
| | | Unteated Group | Settlement Parcels | Settlement - Untreated | |
| 1974 | 1(Post Settlement) | 0.000 | 0.000 | 0.000 | 0.000 |
| | | (0.000) | (0.000) | (0.000) | (0.000) |
| | Off-Res. Pop. | 131781.625 | 355422.500 | 223640.891*** | 395061.219*** |
| | | (98,222.953) | (430322.688) | (13,215.014) | (20,857.697) |
| | 1(Has Casino) | 0.000 | 0.000 | 0.000 | 0.000 |
| | | (0.000) | (0.000) | (0.000) | (0.000) |
| | 1(Tribal Lending Institution) | 0.633 | 0.224 | -0.410*** | -0.357*** |
| | | (0.482) | (0.417) | (0.020) | (0.024) |
| 1982 | 1(Post Settlement) | 0.000 | 0.001 | 0.001* | 0.002* |
| | | (0.000) | (0.033) | (0.001) | (0.001) |
| | Off-Res. Pop. | 149540.094 | 515927.344 | 366387.250*** | 622835.875*** |
| | | (134557.062) | (661741.812) | (20,217.451) | (32,657.465) |
| | 1(Has Casino) | 0.000 | 0.000 | 0.000 | 0.000 |
| | | (0.000) | (0.000) | (0.000) | (0.000) |
| | 1(Tribal Lending Institution) | 0.706 | 0.224 | -0.482*** | -0.473*** |
| | | (0.456) | (0.417) | (0.019) | (0.024) |
| 1992 | 1(Post Settlement) | 0.000 | 0.199 | 0.199*** | 0.270*** |
| | | (0.000) | (0.399) | (0.012) | (0.017) |
| | Off-Res. Pop. | 175705.016 | 660230.875 | 484525.844*** | 848846.250*** |
| | | (177401.203) | (917286.875) | (27,886.803) | (45,209.508) |
| | 1(Has Casino) | 0.023 | 0.142 | 0.119*** | 0.066*** |
| | | (0.148) | (0.349) | (0.011) | (0.012) |
| | 1(Tribal Lending Institution) | 0.814 | 0.253 | -0.561*** | -0.626*** |
| | | (0.389) | (0.435) | (0.018) | (0.020) |
| 2002 | 1(Post Settlement) | 0.000 | 0.688 | 0.688*** | 0.639*** |
| | | (0.000) | (0.463) | (0.014) | (0.017) |
| | Off-Res. Pop. | 214475.922 | 893793.438 | 679317.562*** | 1.196e+06*** |
| | | (210617.953) | (1.304e+06) | (39,451.172) | (64,609.008) |
| | 1(Has Casino) | 0.164 | 0.553 | 0.389*** | 0.603*** |
| | | (0.370) | (0.497) | (0.019) | (0.023) |
| | 1(Tribal Lending Institution) | 0.814 | 0.402 | -0.412*** | -0.469*** |
| | | (0.389) | (0.490) | (0.019) | (0.020) |
| 2012 | 1(Post Settlement) | 0.000 | 1.000 | 1.000 | 1.000 |
| | | (0.000) | (0.000) | (0.000) | (0.000) |
| | Off-Res. Pop. | 250348.359 | 1.129e+06 | 878510.750*** | 1.529e+06*** |
| | | (254536.547) | (1.650e+06) | (49,862.680) | (81,714.031) |
| | 1(Has Casino) | 0.257 | 0.771 | 0.514*** | 0.597*** |
| | | (0.437) | (0.420) | (0.019) | (0.016) |
| | 1(Tribal Lending Institution) | 0.898 | 0.751 | -0.147*** | 0.018* |
| | | (0.303) | (0.433) | (0.016) | (0.011) |
| | Observations | 112,254 | 144,933 | 257,187 | 257,187 |
| State FE | | | | | ✓ |

**Notes:** This table presents year-specific summary statistics of time-varying reservation-level variables for parcels that are always untreated (column 1), or eventually treated (column 2), and the difference between the two (columns 3 and 4). Column 3 shows raw comparisons whereas column 4 shows within-state comparisons. Standard errors are clustered by township and reported in parentheses* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Weights Associated with TWFE

| | % Agriculture | % Developed |
|---|---|---|
| Number of positive weights | 16,464 | 16,464 |
| Number of negative weights | 5 | 5 |
| Sum of negative weights | -0.0003 | -0.0003 |
| Std. Dev. (Weights) | 0.177 | 0.059 |

**Notes:** This tables presents summaries of the weights associated with the TWFE estimator described in Equation 1. Weights were obtained using the `twowayfeweights` package in Stata, developed by de Chaisemartin and d'Haultfoeuille (2020).

Table A4: The Impact of Winters Settlements on Pr(Agricultural Land Use)

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | | | Y = 1(% Agriculture > 0) | | |
| *Panel A:* | | | | | |
| | | | *de Chaisemartin & D'Haultfoeuille (2020)* | | |
| Post Settlement | 0.0052*** | 0.0064*** | 0.0065*** | 0.0057*** | 0.0074*** |
| | (0.0006) | (0.0006) | (0.0006) | (0.0006) | (0.0007) |
| *Panel B:* | | | | | |
| | | | *Callaway & Sant'Anna (2020)* | | |
| Post Settlement | 0.0076*** | 0.0082*** | 0.0078*** | 0.012*** | 0.0125*** |
| | (0.0012) | (0.0025) | (0.0011) | (0.0026) | (0.0032) |
| *Panel C:* | | | | | |
| | | | *Two-Way Fixed Effects* | | |
| Post Settlement | 0.0065*** | 0.0081*** | 0.0093*** | 0.0066*** | 0.0097*** |
| | (0.0010) | (0.0012) | (0.0012) | (0.0011) | (0.0012) |
| Observations | 1,285,935 | 1,285,934 | 1,285,935 | 1,285,935 | 1,285,934 |
| Adjusted R-squared (TWFE) | 0.954 | 0.954 | 0.954 | 0.954 | 0.954 |
| Parcel Fixed Effects | ✓ | ✓ | ✓ | ✓ | ✓ |
| Off-Reservation Population | | ✓ | | | ✓ |
| 1(Casino) | | | ✓ | | ✓ |
| 1(Tribal Lending Institution) | | | | ✓ | ✓ |

**Notes:** This table presents difference-in-difference estimates for the effect of Winters settlements based on the model in Equation 1 using several estimators. Panel A uses the estimator proposed by de Chaisemartin and d'Haultfoeuille (2020) and implemented with the `did_multiplegt` Stata package with two leads and two lags of treatment. Panel B uses the estimator proposed by Callaway and Sant'Anna (2020) and implemented with the `csdid` package in Stata. Panel C presents traditional TWFE estimates obtained via OLS. Panels A and C include state-by-year fixed effects, whereas Panel B uses pooled year fixed effects due to limitations of the `csdid` package. Here, the dependent variable is a dummy variable equal to one if a parcel has any agriculture. Standard errors are clustered by PLSS township (a 6×6-mile square containing 144 parcels) and reported in parentheses* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: The Impact of Winters Settlements on Cultivated Crops

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Y = % Cultivated Crops | | | | |
| *Panel A:* | | | | | |
| | *de Chaisemartin & D'Haultfoeuille (2020)* | | | | |
| Post Settlement | 0.0874*** | 0.0922*** | 0.0480 | 0.0816*** | 0.0694** |
| | (0.0304) | (0.0302) | (0.0324) | (0.0276) | (0.0347) |
| *Panel B:* | | | | | |
| | *Callaway & Sant'Anna (2020)* | | | | |
| Post Settlement | 0.1566*** | 0.0647 | 0.1804*** | 0.1028** | 0.0712 |
| | (0.0302) | (0.0927) | (0.0318) | (0.0442) | (01039) |
| *Panel C:* | | | | | |
| | *Two-Way Fixed Effects* | | | | |
| Post Settlement | 0.2155*** | 0.2621*** | 0.2684*** | 0..2272*** | 0.2906*** |
| | (00378) | (0.0425) | (0.0437) | (0.0401) | (0.0456) |
| Observations | 1,285,935 | 1,285,934 | 1,285,935 | 1,285,935 | 1,285,934 |
| Adjusted R-squared (TWFE) | 0.986 | 0.986 | 0.986 | 0.986 | 0.986 |
| Parcel Fixed Effects | ✓ | ✓ | ✓ | ✓ | ✓ |
| Off-Reservation Population | | ✓ | | | ✓ |
| 1(Casino) | | | ✓ | | ✓ |
| 1(Tribal Lending Institution) | | | | ✓ | ✓ |

**Notes:** This table presents difference-in-difference estimates for the effect of Winters settlements based on the model in Equation 1 using several estimators. Panel A uses the estimator proposed by de Chaisemartin and d'Haultfoeuille (2020) and implemented with the `did_multiplegt` Stata package with two leads and two lags of treatment. Panel B uses the estimator proposed by Callaway and Sant'Anna (2020) and implemented with the `csdid` package in Stata. Panel C presents traditional TWFE estimates obtained via OLS. Panels A and C include state-by-year fixed effects, whereas Panel B uses pooled year fixed effects due to limitations of the `csdid` package. Here, the dependent variable focuses only on cultivated crops, whereas the dependent variable in Table 1 also includes hay/pasture. Standard errors are clustered by PLSS township (a 6×6-mile square containing 144 parcels) and reported in parentheses* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Robust DiD Estimates by Tenure Class: Agriculture

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Y = % Agriculture | | | | |
| *Panel A:* | | | | | |
| | *de Chaisemartin & D'Haultfoeuille (2020)* | | | | |
| Post Settlement (Fee Simple) | 0.4598*** | 0.5492*** | 0.1802 | 0.4474*** | 0.2551 |
| | (0.1757) | (0.1909) | (0.2135) | (0.1840) | (0.2219) |
| Post Settlement (Allotted) | 0.1399*** | 0.1591*** | 0.1933*** | 0.1899*** | 0.2432*** |
| | (0.0162) | (0.0249) | (0.0215) | (0.0205) | (0.0305) |
| Post Settlement (Tribal) | 0.1300* | 0.1396** | 0.0369 | 0.1684*** | 0.0755 |
| | (0.0717) | (0.0707) | (0.0981) | (0.0689) | (0.0935) |
| *Panel B:* | | | | | |
| | *Callaway & Sant'Anna (2020)* | | | | |
| Post Settlement (Fee Simple) | 0.3972** | 0.3837** | 0.7574*** | 0.4818*** | 0.8533*** |
| | (0.1632) | (0.1676) | (0.1445) | (0.1664) | (0.1891) |
| Post Settlement (Allotted) | 0.2049 | 0.1971 | 0.0443 | 0.2807 | 0.7198** |
| | (0.1442) | (0.1435) | (0.2938) | (0.1753) | (0.3468) |
| Post Settlement (Tribal) | 0.1829*** | 0.3686*** | 0.1822*** | 0.2191*** | 0.3614*** |
| | (0.0274) | (0.1044) | (0.0279) | (0.0401) | (0.1091) |
| Parcel Fixed Effects | ✓ | ✓ | ✓ | ✓ | ✓ |
| Off-Reservation Population | | ✓ | | | ✓ |
| 1(Casino) | | | ✓ | | ✓ |
| 1(Tribal Lending Institution) | | | | ✓ | ✓ |

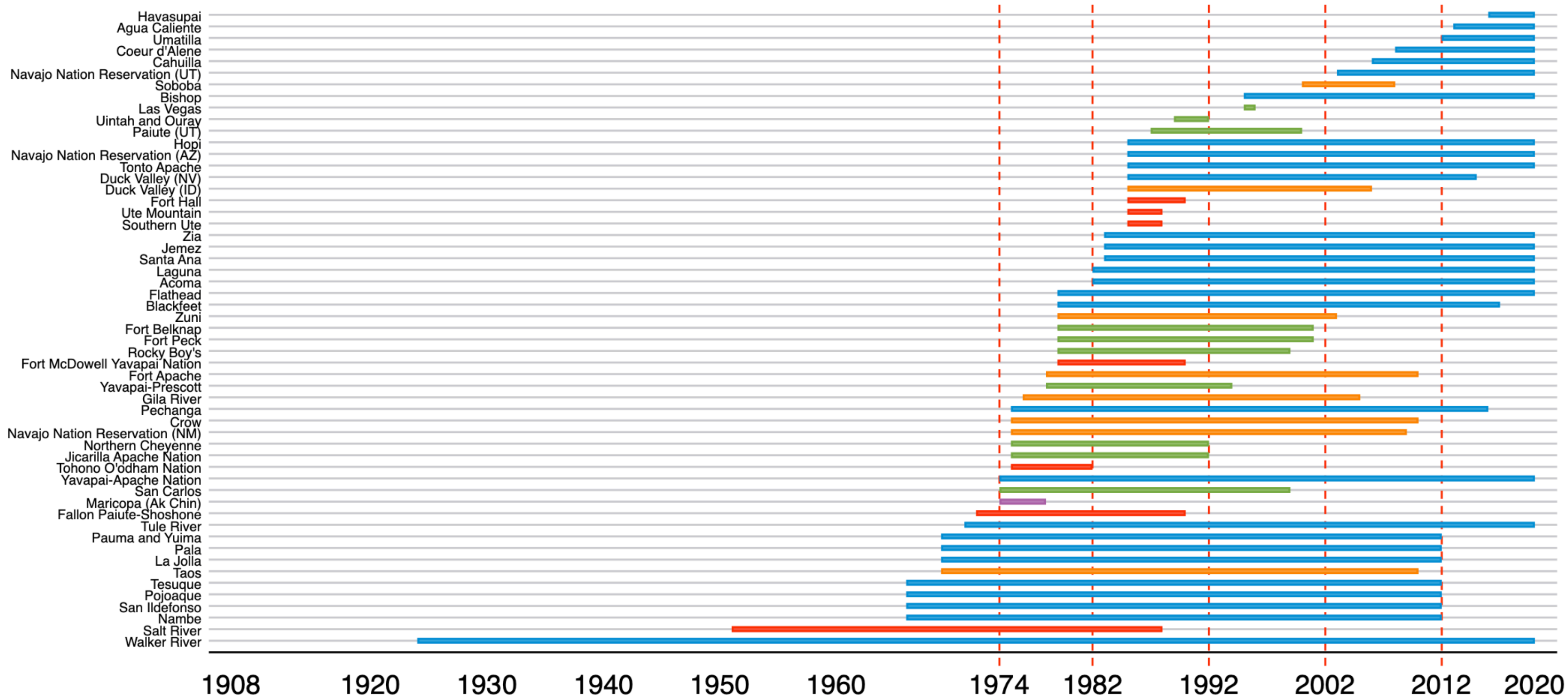
**Notes:** This table presents DiD estimates separately for each land tenure class using the methods proposed by de Chaisemartin and d'Haultfoeuille (2020) in Panel A and Callaway and Sant'Anna (2020) in Panel B, which cannot be used directly to estimate a the difference-in-difference-in-difference model specified in Equation 4. Standard errors are clustered by township and reported in parentheses* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Robust DiD Estimates by Tenure Class: Development

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Y = % Development | | | | |
| *Panel A:* | | | | | |
| | *de Chaisemartin & D'Haultfoeuille (2020)* | | | | |
| Post Settlement (Fee Simple) | 0.1263*** | -0.0573 | 0.1545*** | 0.1259*** | -0.0390 |
| | (0.0405) | (0.1135) | (0.0331) | (0.0405) | (0.1074) |
| Post Settlement (Allotted) | 0.0931** | 0.0901** | 0.0929** | 0.0940** | 0.0909** |
| | (0.0461) | (0.0449) | (0.0461) | (0.0459) | (0.0448) |
| Post Settlement (Tribal) | 0.0088* | -0.0439* | -0.0001 | 0.0077 | -0.0501** |
| | (0.0053) | (0.0241) | (0.0064) | (0.0057) | (0.0249) |
| *Panel B:* | | | | | |
| | *Callaway & Sant'Anna (2020)* | | | | |
| Post Settlement (Fee Simple) | 0.1242** | 0.0191 | 0.1521*** | 0.0992* | 0.0593 |
| | (0.0486) | (0.0669) | (0.0459) | (0.0527) | (0.0842) |
| Post Settlement (Allotted) | 0.0739 | 0.0402 | 0.0819 | 0.0507 | 0.1704 |
| | (0.0656) | (0.0755) | (0.0649) | (0.0745) | (0.2314) |
| Post Settlement (Tribal) | 0.0053 | -0.2618*** | 0.0008 | -0.0185 | -0.2461*** |
| | (0.0076) | (0.0817) | (0.0086) | (0.0148) | (0.0797) |
| Parcel Fixed Effects | ✓ | ✓ | ✓ | ✓ | ✓ |
| Off-Reservation Population | | ✓ | | | ✓ |
| 1(Casino) | | | ✓ | | ✓ |
| 1(Tribal Lending Institution) | | | | ✓ | ✓ |

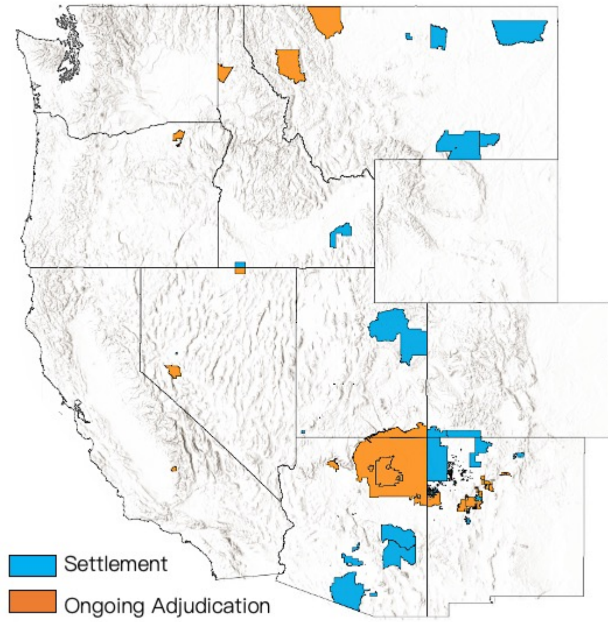
**Notes:** This table presents DiD estimates separately for each land tenure class using the methods proposed by de Chaisemartin and d'Haultfoeuille (2020) in Panel A and Callaway and Sant'Anna (2020) in Panel B, which cannot be used directly to estimate a the difference-in-difference-in-difference model specified in Equation 4. Standard errors are clustered by township and reported in parentheses* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure A1: Timeline of Winters Adjudications & Settlements



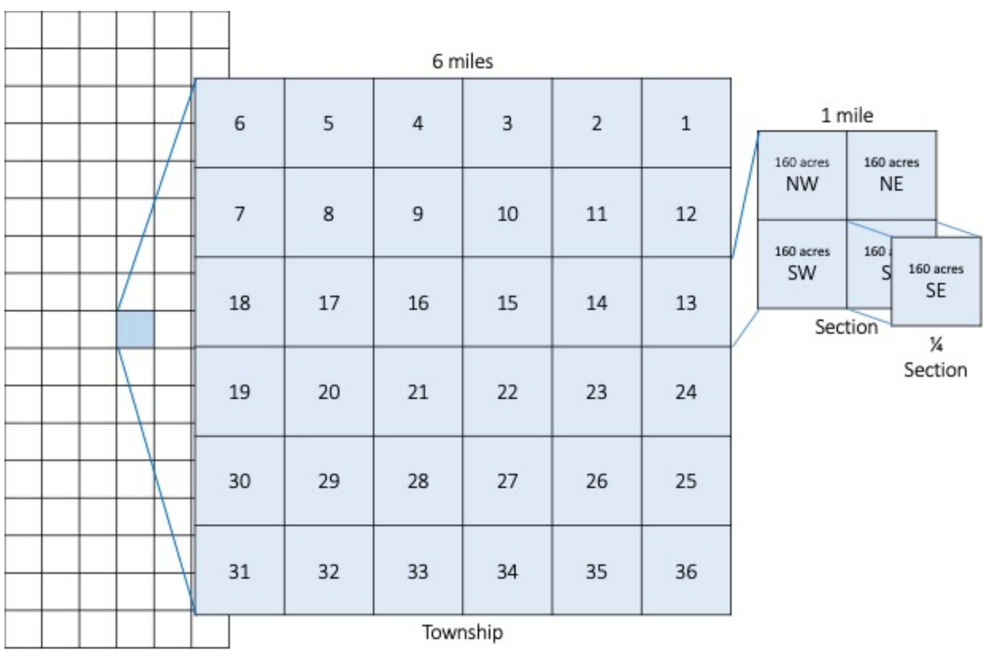

**Notes:** This figure depicts adjudication start and end dates for each reservation in our sample. Reservations are grouped into timing cohorts based on the decade when the adjudication was completed. Untreated reservations that are still undergoing the adjudication process as of 2012 act as a control group. Vertical dashed red lines indicate the years in wich we observe land use from the NWALT data.

Figure A2: Reservations in Sample



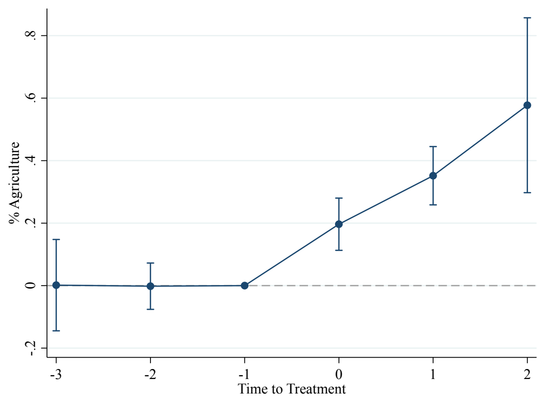

**Notes:** This figure depicts our sample of reservations across western states. Treatment parcels are located on reservations that achieved water settlements by 2012 (blue on the map), while untreated parcels are located on reservations with ongoing adjudications (orange on the map).

Figure A3: The Public Land Survey System

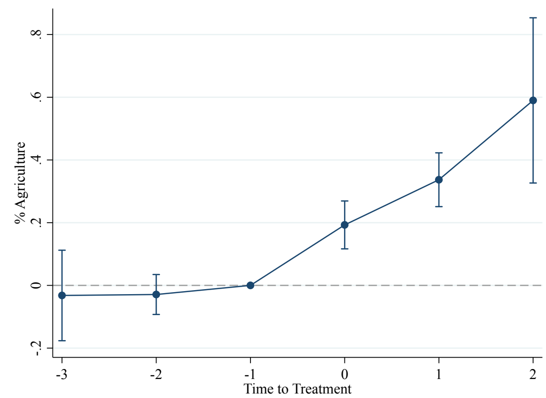



**Notes:** This figure depicts an example of a Public Land Survey System township unit and the section and quarter section units within each township. Each 36-square mile township can be divided into thirty-six 1-square mile sections. Each section is then divided into 160-acre quarter sections, which match the standard allotment assigned to Native American households under the Dawes Act over 1987–1934 (Carlson, 1981; Leonard et al., 2020; Dippel et al., 2020).

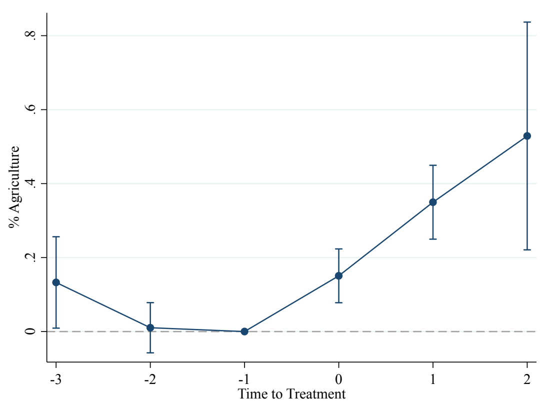
Figure A4: Agricultural Land Use Event Study — Alternative Specifications



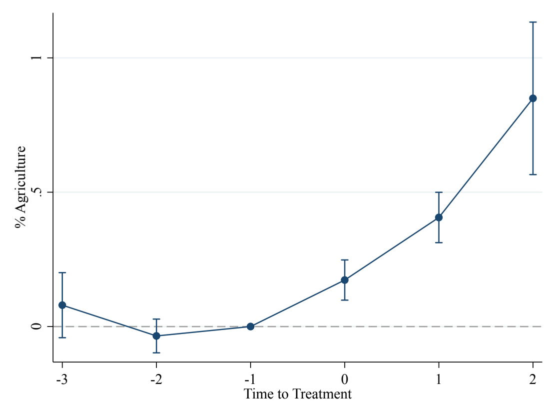

(a) Off-Res. Pop. Control




(b) Casino Control



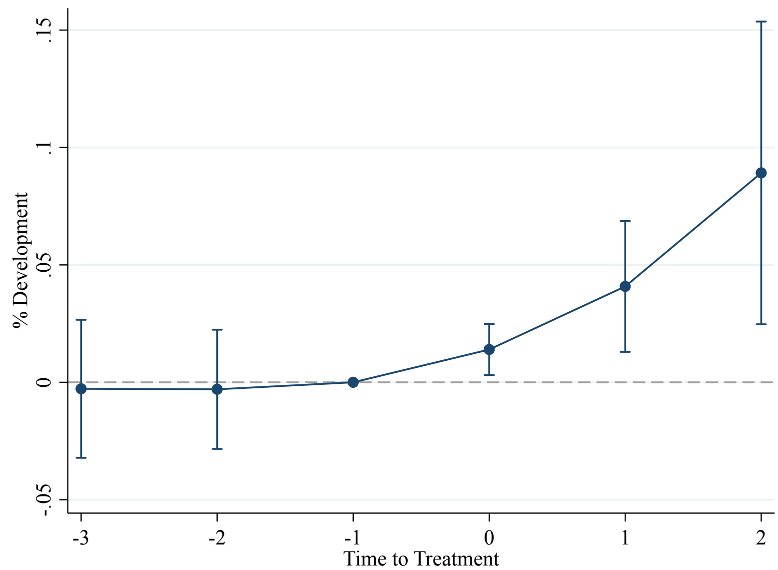

(c) Credit Control




(d) All Controls

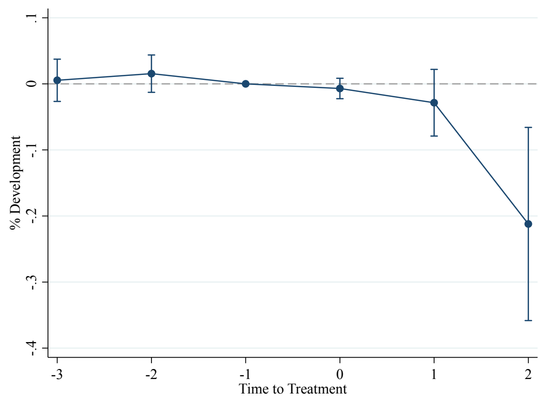
**Notes:** This figure depicts alternative versions of the event study estimates depicted in Figure 2 using the estimator developed by de Chaisemartin and d'Haultfoeuille (2020), implemented with the `did_multiplegt` package in Stata. The specifications in Panels (a) through (d) of the figure correspond to columns 2 through 5 of in Panel A of Table 1. The difference between treated and untreated groups is normalized to zero in period $t-1$, the final period before treatment. Period 0 denotes the first period in which parcels are exposed to treatment.

Figure A5: Developed Land Use Event Study

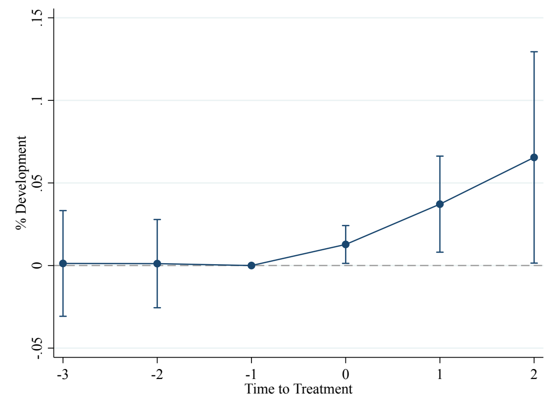



**Notes:** This figure depicts event study estimates using the estimator developed by de Chaisemartin and d'Haultfoeuille (2020), implemented with the `did_multiplegt` package in Stata. The model corresponds to the specification in column 1 of Panel A of Table 2, which includes parcel fixed effects and state-by-year fixed effects. The difference between treated and untreated groups is normalized to zero in period $t-1$, the final period before treatment. Period 0 denotes the first period in which parcels are exposed to treatment.

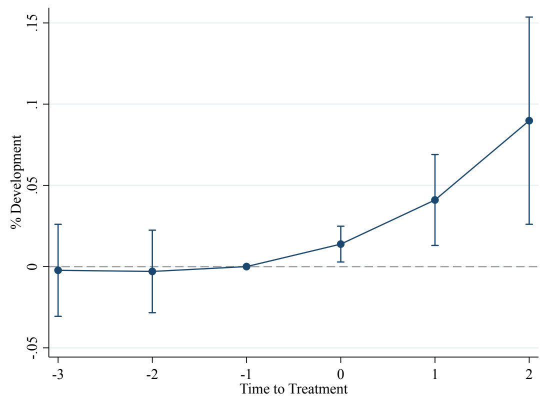
Figure A6: Developed Land Use Event Study — Alternative Specifications



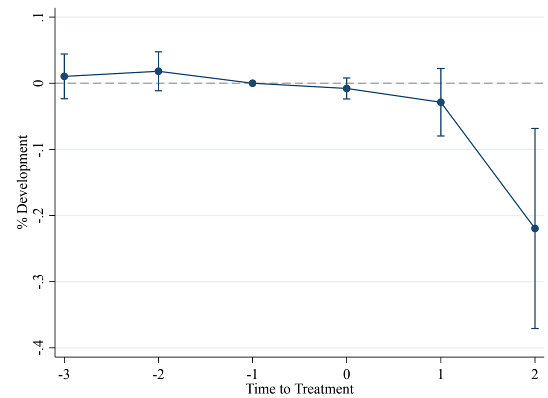

(a) Off-Res. Pop. Control




(b) Casino Control



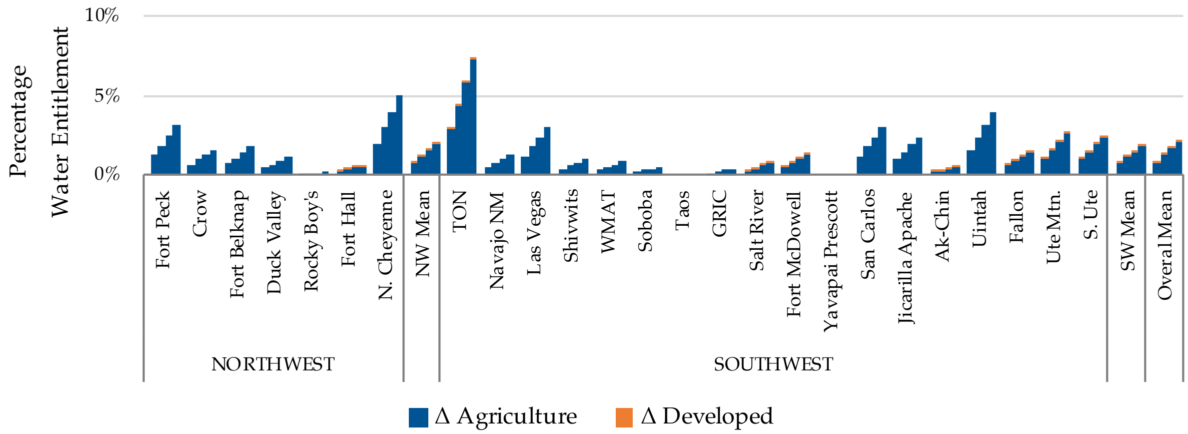

(c) Credit Control




(d) All Controls

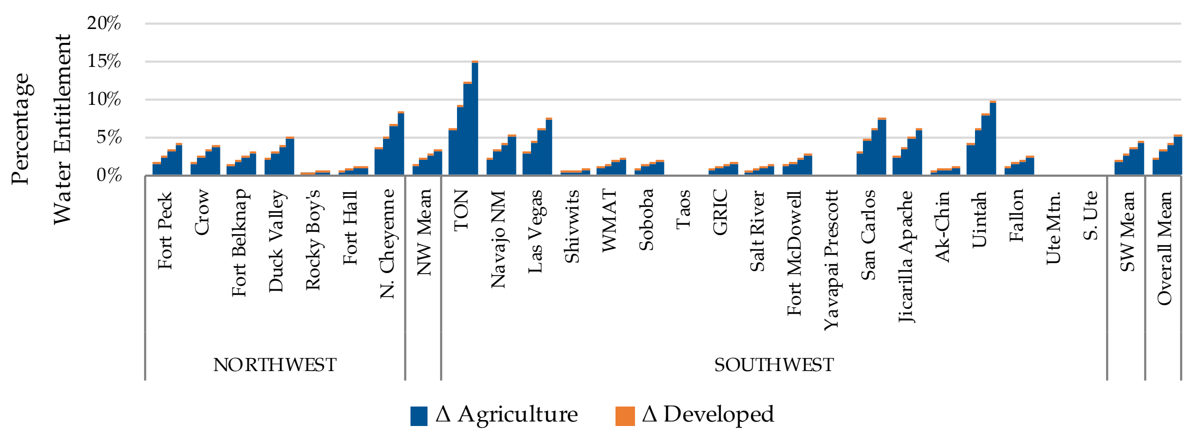
**Notes:** This figure depicts alternative versions of the event study estimates depicted in Figure A5 using the estimator developed by de Chaisemartin and d'Haultfoeuille (2020), implemented with the `did_multiplegt` package in Stata. The specifications in Panels (a) through (d) of the figure correspond to columns 2 through 5 of in Panel A of Table 2. The difference between treated and untreated groups is normalized to zero in period $t-1$, the final period before treatment. Period 0 denotes the first period in which parcels are exposed to treatment.

Figure A7: Predicted Change in Water Use due to Settlements




**Notes:** This figure depicts predicted changes in 2012 water use as the result of a water settlement based on Equation 3. Estimates presented here assume varying levels of water use for agriculture and .25 AF/acre for domestic land use. For each reservation, water use estimates depicted by each bar, from left to right, assume agricultural water use estimates of 2, 3, 4, and 5 AF/acre.

Figure A8: Additional Predicted Change in Water Use due to Settlements with No Land Tenure Constraints




**Notes:** This figure depicts additional, counterfactual predicted changes in 2012 water use as the result of a water settlement in the absence of restrictions on allotted trust and tribal trust land based on Equation 5. Estimates presented here assume varying levels of water use for agriculture and .25 AF/acre for domestic land use. For each reservation, water use estimates depicted by each bar, from left to right, assume agricultural water use estimates of 2, 3, 4, and 5 AF/acre.