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Leslie Sanchez
Tufts University

Bryan Leonard
Arizona State University

Eric C. Edwards
North Carolina State University

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Leslie Sanchez† Bryan Leonard‡ Eric C. Edwards§

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Abstract

Restoring natural resource access lost during colonization has become an important component of formalizing sovereignty and providing economic opportunity for Indigenous groups. This paper uses satellite data on land use to study the effect of property right settlements for surface water on reservations in the western United States between 1974 and 2012 with robust difference-in-difference methods. We find statistically significant increases in agricultural land use and no change in developed land use. Despite this, back-of-the-envelope calculations reveal that most tribes are using a small fraction of their entitlements, potentially forgoing as much as $1.6 billion in annual production or leasing revenue. We provide evidence that this underutilization may be driven by the inability to construct the necessary infrastructure. Our findings indicate that restoring formal property rights to a single resource is unlikely to provide significant economic benefits unless existing institutional constraints and barriers to development are also addressed.

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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 resources traditionally governed by Indigenous people were enclosed, allocated, or otherwise appropriated as part of settlement and colonization. Preexisting Indigenous property rights (formal and informal) were generally extinguished in the process, often without any compensation. In the United States, the loss of natural resources ranging from land and water to salmon and bison has been suggested as a key reason why many Indigenous communities fare worse on a variety of margins than 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 U.S., 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) led to roughly half of the land in Oklahoma coming under the jurisdiction of the Muscogee (Creek) Nation. Internationally, restoration of natural resource rights has occurred for Australian aboriginal land and water claims (Mabo and Others v. Queensland (no. 2) 1992 HCA 23, Native Title Act 1993); Chilean Indigenous land and water (Heise, 2001; Tomaselli, 2012); and New Zealand Maori land (the Ngai Tahu and Waikato-Tainui settlements) and customary water and fishing 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 have 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 a culturally and economically significant natural resource: water. Specifically, we study the economic impacts of the allocation of formal property rights to water for tribal nations in the western United States. This policy, which stems from a U.S. Supreme Court ruling (Winters v. United States, 1908) and has granted tribes formal title to large volumes of the West's scarce water based on historical treaty rights, requires re-allocating water from existing uses. 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 U.S. residents or approximately 1 million acres of irrigated agriculture.

Based on prevailing prices, tribal water rights could have a market value exceeding $1.6 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.

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 five to 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 (Falcone, 2015).

Our estimates show that after obtaining formal water rights, agricultural land use on reservations increased by up to 0.61 percentage points, which translates into a 8.7 percent increase relative to mean agricultural land use. To our knowledge, this is the first estimate of the effect of Winters settlements on tribal land and water use. This finding holds across three different difference-in-difference estimators and alternative measures of agricultural land use (for example, focusing solely on cultivated crops). We do not find evidence of increases in developed land use following a settlement.

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 disparity is that the

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

restoration of Indigenous property rights may have changed the distribution of resource wealth but not the allocation of the resource itself, that is, 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. Winters settlements apparently fail to deliver actual water for tribes, despite granting large quantities of "paper rights."

We explore several mechanisms that could explain the relatively low utilization of tribal water rights revealed by our main results. The first potential barrier to accessing and developing settlement water is that tribal governments often lack capital necessary for large, up-front investments in in large-scale water infrastructure. To assess infrastructure-related water use barriers at the reservation-level, we examine the differential effects of Winters settlements on reservations with pre-existing Bureau of Indian Affairs (BIA) irrigation infrastructure projects. We find that post-settlement agriculture (though not development) is increasing almost exclusively on reservations where BIA infrastructure already exists and has the capacity to make settlement water available for immediate use.

Second, we examine within-reservation heterogeneity in treatment effects. Many reservation lands are held in trust with the federal government on behalf of tribes or individuals. These trust lands cannot be sold or consolidated, cannot be used as collateral to access credit, and entail costly approval processes for leases or major changes to land use that have been found to hamper development across a variety of resources and contexts (Ge et al., 2020; Leonard et al., 2020; Leonard and Parker, 2021; Dippel et al., 2020). Consistent with prior work, we that find increases in on-reservation agriculture and development after gaining water rights are concentrated on lands held in "fee simple" that are not subject to these ownership constraints.

Finally, we explore the impact of transactions costs associated with water rights themselves (Garrick and Aylward, 2012) by comparing outcomes from tribes that lease water back to off-reservation users, which requires special approval by the U.S. Congress. We find mixed evidence that reservations that lease their water back to off-reservation users see smaller increases in agricultural land use, but these results are not precisely estimated and vary with the inclusion of controls. Similarly, we find mixed evidence that tribes that lease their water may see much larger increases in developed land use. Hence, our results provide suggestive evidence that leasing may help tribes address preexisting credit constraints that previously prevented economic development. Still, many of 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 rights restoration. While tribal water settlements increase on-reservation agricultural development, the increases in water use are small 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 re-allocations 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.[3]

2 Background

Surface water in the western U.S. 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 early-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 costly, large-scale irrigation

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

[3] And, to the extent that off-reservation users are still appropriating the water, settlements are unlikely to yield benefits associated with the cultural or spiritual significance of water for tribes.

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, which have the authority to allocate water within their borders, largely did not allocate water rights to tribes, and none of the reservation irrigation projects started by the Bureau of Indian Affairs (BIA) in the late 1800s were completed (Government Accountability Office, 2006).[4] 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 reservation 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 was 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 reserve water rights for tribes 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 initially 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 in a *de facto* sense until they are adjudicated (Sanchez et al., 2020).

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 (Sanchez et al., 2020). Settlement funding can help tribes to overcome capital constraints to developing their water resources for on-reservation water use.

[4]Today, many BIA irrigation projects are inefficient and in need of repair (Carlson, 2018).

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 toward 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 some 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 for development, 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 use 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). Moreover, reservations with greater farming capacity (measured as "practicably irrigable acreage") prior to an adjudication tend to receive larger water entitlements and higher levels of federal funding (Sanchez et al., 2020).

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 property rights to land 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), it is unclear whether the restoration of tribal water rights, by itself, could meaningfully improve resource access, agricultural production, and development outcomes on reservations.

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 holding title to a water right does not automatically secure 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 that they legally own but do not (and cannot) divert. Should an agreement fail to materialize, existing users see no credible threat of losing access in the absence of tribal diversion infrastructure. Hence, preexisting institutional barriers that limit tribal access to credit to finance large-scale infrastructure further diminish tribes' ability to assert de facto ownership despite holding legal title.

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).

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

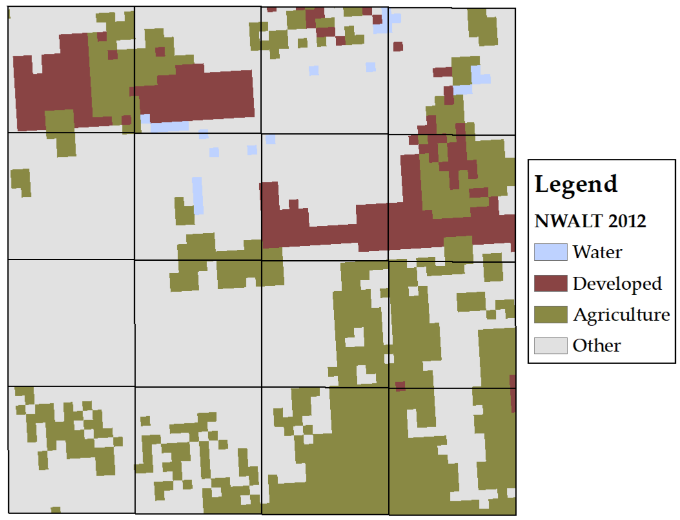
A persistent challenge for conducting empirical research on reservations is the lack of fine-scale, longitudinal data. Previous analyses rely on the U.S. Census, which aggregates variables such as income and farm sales to the reservation level, only collects data for some reservations, and is largely only available from 1980 onward. We overcome these limitations by combining several novel data sources: i) fine-scale measures of land tenure on and land use 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) important, time-varying 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 onward (Falcone, 2015), matched to reservation parcels by Dippel et al. (2020). These data allow 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 U.S. 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 NWALT categories: crops or hay/pasture (Spangler et al., 2020). We also measure developed land use. NWALT includes a variety of developed land uses including 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).

Our unit of analysis is a Public Land Survey System (PLSS) quarter section. The PLSS is a

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.

rectangular grid devised by Bureau of Land Management to divide most of the U.S. into 6×6-mile townships. Townships are then divided into 36 1-mile×1-mile sections. Each section is divided into four quarter sections that are $\frac{1}{2}$-mile×$\frac{1}{2}$-mile squares (see Figure A2). These 160-acre quarter sections correspond to the typical size of an ownership tract on a reservation due to historical land titling policies, and conform to reservation boundaries (Leonard et al., 2020; Dippel et al., 2020).[5] For brevity, we refer to quarter sections as "parcels," but we note that some quarter sections may contain multiple land parcels.[6] 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 U.S. west that have asserted water right claims for agricultural and domestic water use (Figure A1). We define our treatment group (n=144,933) as parcels located on 24 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 adjudi-

[5] Some sections are divided into varying numbers of "government lots" rather than quarter sections, and this was especially true of Indian Land Patents issued under the Dawes Act. These government lots are typically 40 acres rather than 160. The upshot is that actual parcel sizes vary within the data to some degree, though the majority are 160 acres.

[6] We lack data on individual land parcels, but previous research utilizing parcel level data for other reservations (Leonard and Parker, 2021) has shown a high degree of correspondence between PLSS units and actual parcels.

cating their water rights when total reservation area, arable reservation acreage, and measures of water scarcity are increasing (Sanchez et al., 2020). To ensure that our group of untreated parcels is 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 reservations. Our untreated group is comprised of 112,254 parcels located on 33 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 of the reservation's water settlement and zero prior to settlement.[7]

To assess potential differences between the treated and untreated groups, we also develop parcel-level measures of land quality and terrain. We use 30-meter$\times$30-meter resolution data from the 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 parcel 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

[7]In almost all cases, a tribe's water rights, as specified in a settlement agreement signed between negotiating parties, are formally defined when a water settlement is enacted by Congress. However, a handful of water settlements, such as the San Luis Rey settlement in Southern California, were enacted by Congress prior to negotiating parties reaching an agreement about how a tribe's water right would be satisfied. In these instances, we consider the later settlement agreement rather than the settlement act as the functional date of settlement completion.

casino websites. Data identifying tribal lending institutions is available from the Federal Reserve Bank of Minneapolis. 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 U.S. Census data to estimate the off-reservation population in counties overlying or adjacent to reservations in each year of the sample.[8]

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 that some reservations have begun the negotiation process but have yet to settle their rights. 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:

$$y_{irt} = \beta_{TWFE} PostSettlement_{rt} + \beta_2 X_{rt} + \vec{\lambda}_i + \vec{\tau}_t + \varepsilon_{irt} \tag{1}$$

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 an indicator for access to tribal lending institutions), λ_i is a vector of parcel fixed effects, and τ_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 β_{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 β_{TWFE} is a weighted average of all 2×2 comparisons of "switchers" to "non-switchers"

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

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 β_{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).[9]

de Chaisemartin and d'Haultfoeuille (2020) and Callaway and Sant'Anna (2020) both describe the problems that can cause β_{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 versus late-treated comparison. de Chaisemartin and d'Haultfoeuille (2020)'s method provides time-specific ATTs for each k period since treatment that are averaged across different cohorts that are treated at different times, whereas Callaway and Sant'Anna (2020) construct group-time-specific ATTs (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 that is averaged across all post-treatment periods.

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. [10] We prefer the de Chaisemartin and d'Haultfoeuille estimator for two 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

[9] 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.

[10] Sun and Abraham (2020) and Borusyak et al. (2021) also propose related estimators, but their focus is on dynamic TWFE designs (event studies). We do not use those estimators for several reasons. First, we do not pursue an event study as our primary design due to the nature of our data and outcomes of interest. Our data occur at low frequency (once per decade) and we only observe five periods, limiting the insight that can be gleaned from an event study design. Moreover, some of the dynamic lead and lag coefficient would be identified off of a single reservation. Second, the Sun and Abraham estimator is similar in practice to the Callaway and Sant'Anna (2020) estimator, except that it excludes non-yet-treated units from the control group. Finally, the Borusyak et al. estimator requires relatively stringent assumptions for identification and may be more subject to bias than our preferred estimators if the parallel trends assumption does not strictly hold (de Chaisemartin and D'Haultfœuille, 2021).

the de Chaisemartin and d'Haultfoeuille estimator is that it allows the researcher to include non-parametric time trends for different groups. We discuss below why this is an important consideration in our setting.

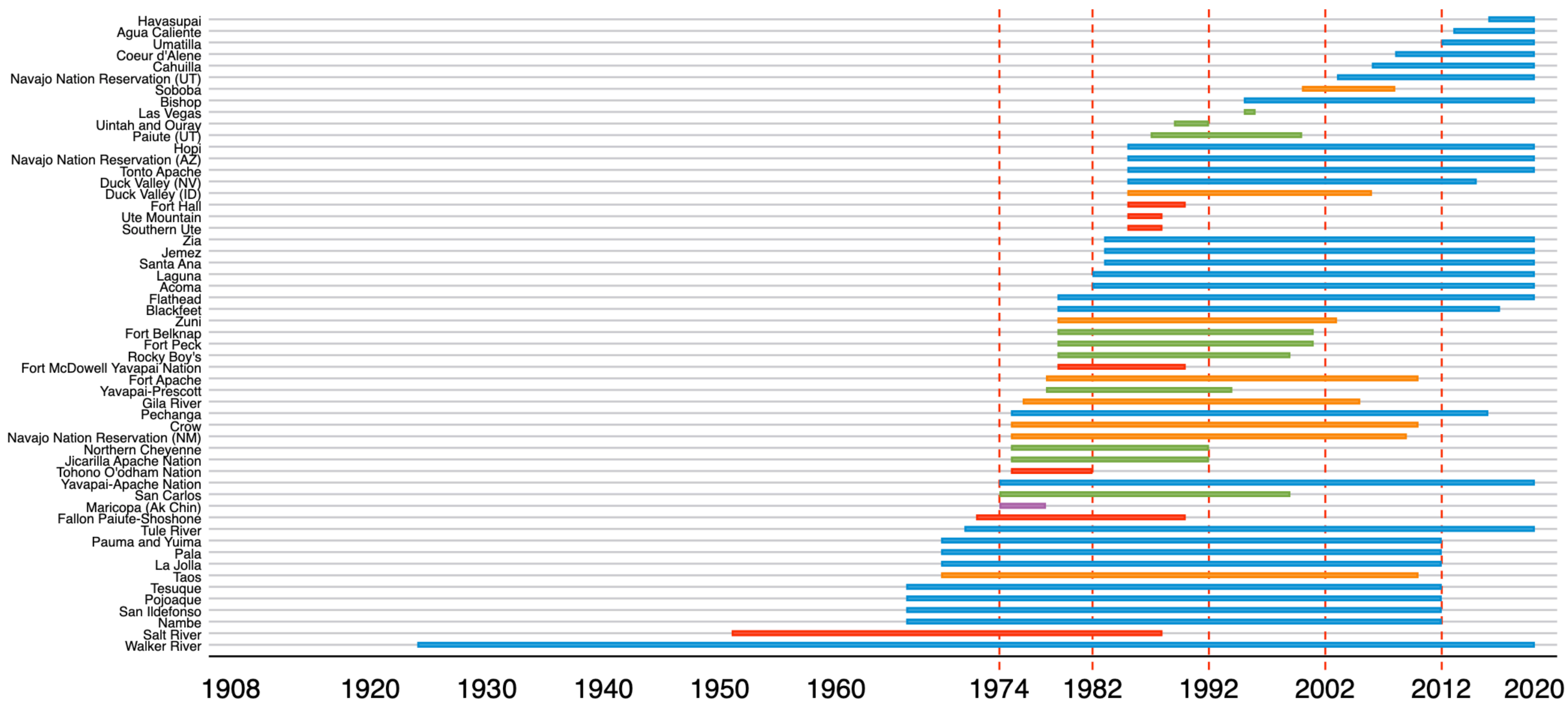
3.2.2 Identification

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.[11] 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 assumptions 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 24 reservations that settled Winters rights between 1974 and 2012, another 33 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, rather than level differences.

[11] 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.

Figure 2: 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 2 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 adjudications *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 is 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).[12]

Despite the largely exogenous nature of congressional actions to finalize Winters settlements, reservation or state-specific shocks could violate the identifying assumptions if they are correlated with the timing of treatment (settlement) and with changes to reservation land use. 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 time trends to capture shocks to water resources and water demand, as well as potential changes in state water policy that could affect the outcome of Winters negotiations when we use the de Chaisemartin and d'Haultfoeuille estimator.[13] 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.

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.

[12]This interpretation is also consistent with our conversations with various policy stakeholders who have been involved in the adjudication process.

[13]We replace these with state-by-year fixed effects when using the the TWFE estimator.

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.[14] 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 parallel trends assumptions. Table A2 shows the evolution of these variables over time and reveals differences between reservations that never receive treatment versus 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 focus is on agriculture, 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

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

include, on average, 144 quarter sections each, as is common in studies of agricultural land and water use (Ge et al., 2020; Hagerty, 2021).[15]

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. The relevant comparison for identification purposes requires focusing on trends in the untreated group relative to a treated reservation *at the time of treatment*, i.e., 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 versus after treatment. Our NWALT data contain a total of five periods. Because settlements are staggered over time, we are able to report a symmetric window that includes three periods prior to treatment and three 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.[16]

Figure 3 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 effects and state-specific non-parametric trends, but no time-varying reservation controls.[17] All coefficients are relative to the difference between treated and untreated parcels in the period just prior to treatment, which is normalized to zero. The coefficients for periods $t-2$ and $t-3$ are near zero and statistically insignificant. The period $t-3$ coefficient is near to significance, but this suggests a *decreasing* trend in agricultural land use on treated reservations prior to receiving a water right.

From period $t = 0$ onward, there is a statistically significant (and increasing) difference between treated and untreated parcels. Appendix Figure A3 shows that this finding is robust to including time varying controls for off-reservation population, casino presence, and credit access. Adding these controls reduces the magnitude and precision of the $t-3$ coefficient. This provides

[15] 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).

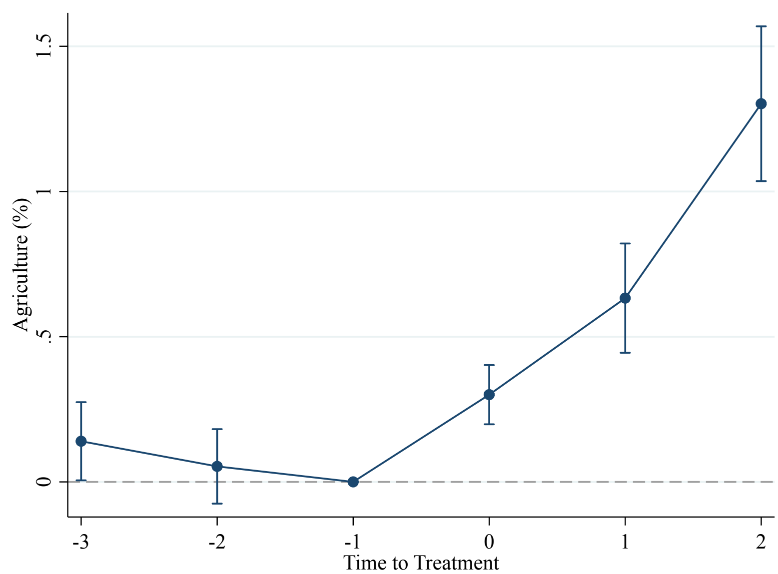
[16] We have only one reservation, Maricopa (Ak-Chin), for which we observe three time periods of data after period "0." Accordingly, we focus our event window on period 0 plus two years.

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

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 estimates obtained using the classic TWFE approach.[18] 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.[19]

Figure 3: 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.

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,

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

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

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 versus 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 those tribes with the institutional capacity to pursue casinos and lending institutions may fare better in Winters negotiations. The coefficients are also fairly consistent across all three estimators. The TWFE and Callaway and Sant'Anna (2020) estimates are quite similar, whereas the de Chaisemartin and d'Haultfoeuille (2020) tend to be somewhat larger.

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	0.526***	0.588***	0.582***	0.526***	0.614***
	(0.053)	(0.065)	(0.061)	(0.066)	(0.066)
Panel B:					
			Callaway & Sant'Anna (2020)		
Post Settlement	0.393***	0.299***	0.409***	0.375***	0.328**
	(0.062)	(0.138)	(0.060)	(0.099)	(0.143)
Panel C:					
			Two-Way Fixed Effects		
Post Settlement	0.209***	0.292***	0.348***	0.201***	0.360***
	(0.068)	(0.068)	(0.065)	(0.067)	(0.064)
Observations	1,410,185	1,410,185	1,410,185	1,410,185	1,410,185
Adjusted R-squared (TWFE)	0.979	0.979	0.979	0.979	0.979
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* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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 0.614 percentage point increase in agricultural land use due to a Winters settlement. The average untreated parcel is 7.04 percent agriculture, implying that settlement leads to a 8.7 percent 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 (7.04 percent) for agricultural land use is driven by a large number of zeroes in the data. For parcels that do have at least some agriculture, the mean is 55 percent agricultural land use. In Table A3, 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 A3 indicates that settlement increases the probability of agriculture on a parcel by 0.019 percentage points, which is a 14.9 percent increase relative to the mean of 12.7 percent for untreated parcels. Finally, Table A4 focuses on cultivated crops only, excluding hay/pasture, yielding even smaller results: the 0.226 percentage point increase in the percentage of land on a parcel devoted to cultivated crops is just a 4 percent increase relative to the mean of 5.61 percent.

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. The effects are much smaller, however, than one might anticipate given the size of the typical settlement. This is not to say that tribes will not expand agricultural land use in the future. The event study in Figure 3 indicates that the rate at which agricultural land use expands is increasing over time, so it may be that full utilization of Winters is yet to come. Another 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.

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 A4 and A5 show the associated event study coefficients.

Unlike the agricultural land use results, the developed coefficient estimates in Table 2 are statistically insignificant across all models and estimates. The estimates are also a full order of magnitude smaller than the agricultural land use estimators. Although these effects are clearly not precisely estimated zeroes, we fail to reject the null that Winters' settlements have no effect on developed land use. When interpreting how these results affect total tribal water use, it is important to remember that agricultural water use per-acre is much higher than residential and urban water use per-acre. Hence, the remaining analysis focuses primarily on agricultural land use.

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	-0.042	-0.085	-0.037	-0.042	-0.081
	(0.101)	(0.089)	(0.090)	(0.100)	(0.085)
Panel B:					
	Callaway & Sant'Anna (2020)				
Post Settlement	0.095	0.027	0.069	0.045	-0.022
	(0.142)	(0.143)	(0.149)	(0.156)	(0.166)
Panel C:					
	Two-Way Fixed Effects				
Post Settlement	-0.059	-0.131	-0.067	-0.037	-0.081
	(0.158)	(0.155)	(0.116)	(0.153)	(0.114)
Observations	1,410,185	1,410,185	1,410,185	1,410,185	1,410,185
Adjusted R-squared (TWFE)	0.887	0.887	0.887	0.887	0.887
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* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

4.3 Water Use Estimates

Next, we use our agricultural land use estimates to develop i) predicted changes in water use due to settlement and ii) total water use on each reservation in 2012. This allows us to characterize the proportion of settlement water being used by each tribe, and how much of this use is attributable to post-settlement changes in land use associated with our estimates.

We use the estimates from column 5 in Panel A of Tables 1 and 2 to calculate the share of total reservation water use that is attributable to changes in land use associated with the settlement of a Winters right.[20] To do so, we take the average predicted change in land use for a parcel and multiply by the average parcel size and number of parcels on each reservation. We multiply this figure by varying levels of water use ranging from 2 to 5 acre-feet per acre (AFA).[21] We estimate the predicted change in reservation water use according to the following calculation:

$$\Delta \hat{U}se_r = \frac{\hat{\beta}_{ag}}{100} \times AFA_{ag} \times \overline{ParcelAcres}_r \times N_r^P \tag{2}$$

where N_r^P is the number of parcels on reservation r, and $\overline{ParcelAcres}_r$ is the average size of parcels on reservation r. $\hat{\beta}_{ag}$ is the de Chaisemartin and d'Haultfoeuille (2020) coefficient estimating changes to agriculture land use from column 5 of Panel A in Table 1.

We estimate that the average settlement-induced changes in on-reservation water use across reservations account for 5–12% of tribes' total water entitlements, depending on assumptions about water use per acre. When we include off-reservation leasing, our estimates of post-settlement changes to water use increase to an average of 8–16%. 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.

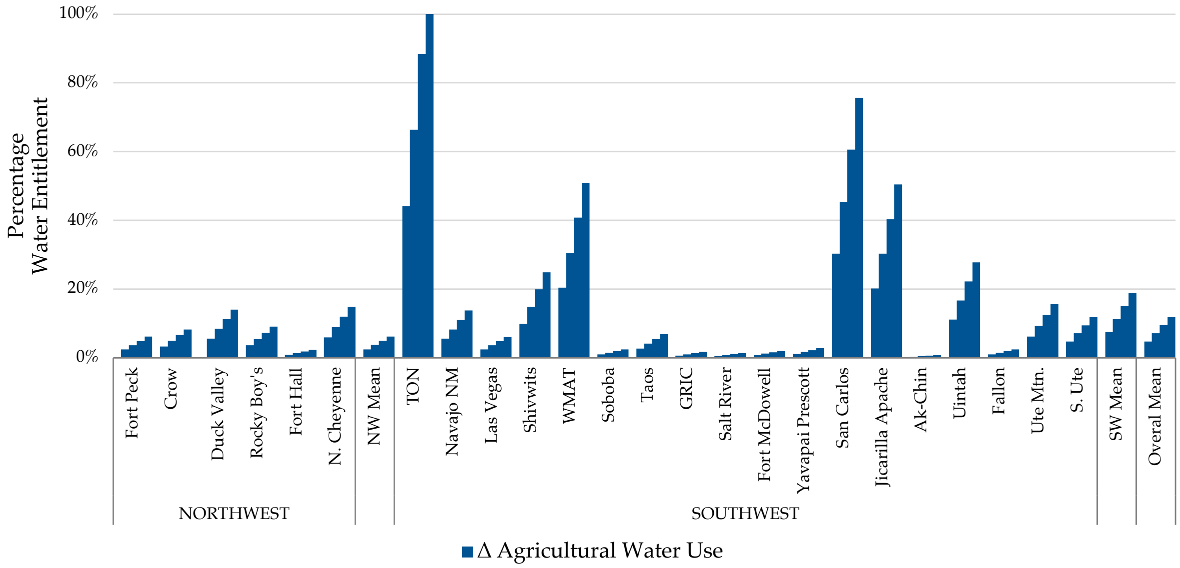
The results for each reservation are depicted in Figure 4, which shows predicted changes to on-reservation water use. For each reservation, we present four scenarios corresponding to an assumed 2, 3, 4, or 5 AFA agricultural water use, from left to right. The striking heterogeneity in predicted water use impacts across reservations is driven to a large degree by differences in reservation acreage relative to settlement amounts. For example, the San Carlos and Tohono O'odham reservations are both quite large, and hence see large predicted increase in agricultural land use. The figure also underscores the impact of assumptions about water use per-acre. The predicted effects on Tohono O'odham vary from just over 40% with 2 AFA, to 100% of its entitlement with 5 AFA.

Next, we estimate *total* water use in 2012 for each treated reservation to better understand

[20] We use the column 5 coefficients to generate the largest (most optimistic) predicted increase in water use.

[21] 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.

Figure 4: Estimated Change in Water Use Relative to Entitlements



Notes: This figure depicts estimated change in water use each treated reservation in our sample using calculation in Equation 2 using the coefficient from column 5 of Panel A in Table 1. 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.

plausible water use scenarios. We separately sum 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.[22] We estimate the share of settlement water use on reservation r in 2012 as:

$$\hat{Use}_r = AgAcres_r \times AFA_{ag} + DevAcres_r \times 0.25 + Leased_r \tag{3}$$

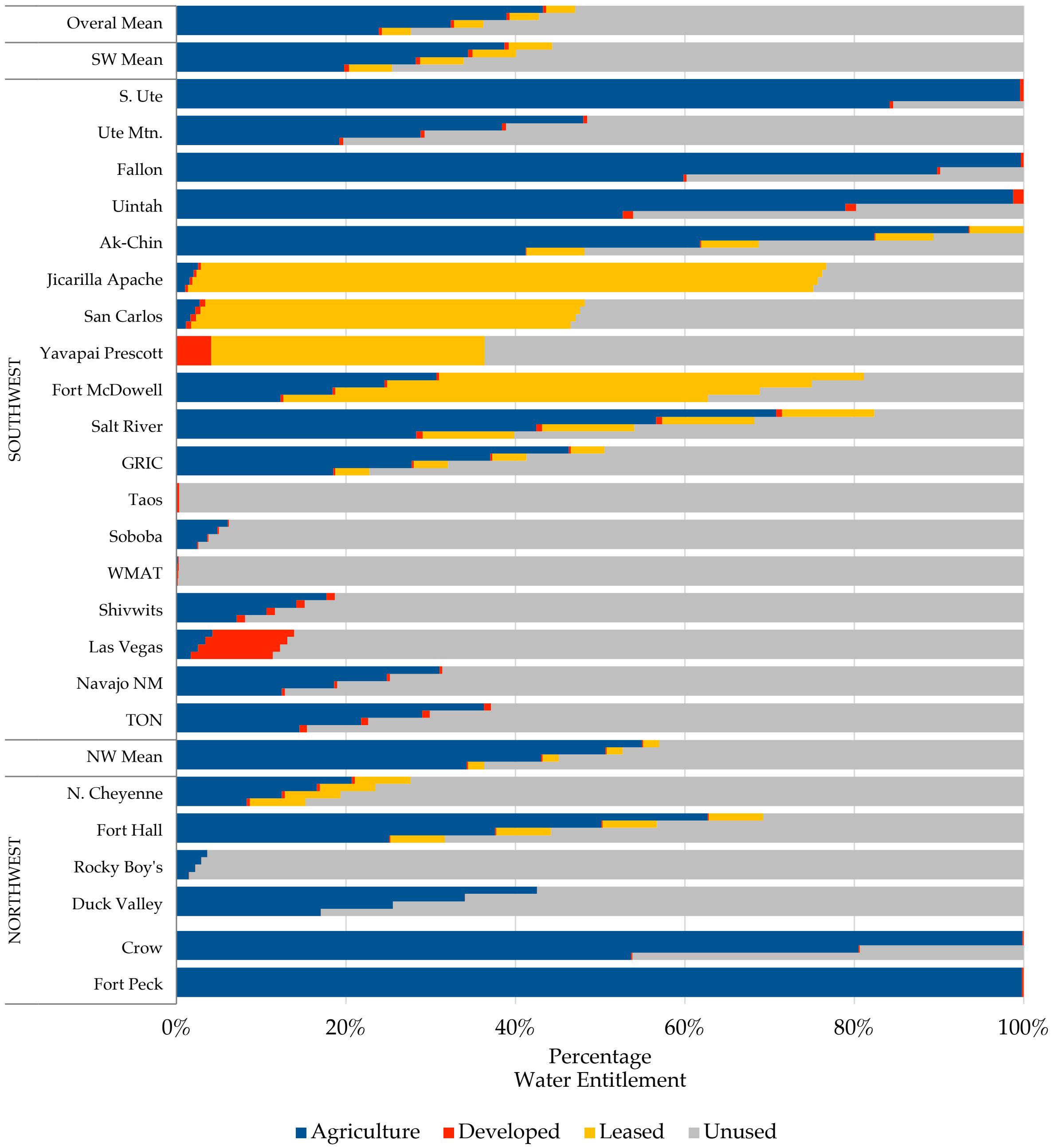
where and $Leased_r$ is water being leased to off-reservation users by reservation r in 2012, which we obtain from settlement agreements and water transaction records for each state. We censor our estimate at 100 percent in cases where predicted water use exceeds available water.

Figure 5 depicts estimates of the share of used, leased, and unused settlement water for each treated reservation in the sample. Our estimates again show considerable heterogeneity across reservations. For instance, Fort Peck is using its entire entitlement under all four scenarios, as are several other reservations under high water-use scenarios. Others, like Soboba or Taos, were using little to none of their settlement by 2012.

Given the growing deficit between water supply and demand in the West, and consequent

[22]We view this as an upper-bound assumption as 48 percent of reservation households lack access to water and sanitation infrastructure (Democratic Staff of the House Committee on Natural Resources, 2016).

Figure 5: Total 2012 Water Use Estimates



Notes: This figure depicts estimated water use in 2012 for each treated reservation in our sample using calculation in Equation 3. 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 unused in 2012.

increases in the value of water in the region, tribes' collective acquisition of titles to 5.3 million AF should be an economic windfall. Yet, our estimates show that most tribes are not capturing the full value of their water rights either through on-reservation water use or by leasing water off-reservation. Water use estimates suggest that tribes are missing out on significant economic gains associated with a resource that they own on paper but not in practice. Should tribes fully use their remaining entitlements entirely for agriculture, under a 2 acre-foot per acre water duty, they could irrigate an additional 1.3 million acres.[23] Using state-specific water market pricing data from the Water Transactions Dataset (Donohew and Libecap, 2010), we estimate the total adjusted (2020$) value of water forgone by tribes in 2012 to be between $900 million $1.6 billion. This amounts to between $4,500 and $8,000 for each person residing on these reservations.[24]

The results also reveal that some reservations such as Crow and Fort Peck have a small predicted effect of settlement in Figure 4, but nevertheless have high overall water use in 2012. This suggests that some tribes may pursue Winters settlements to solidify rights to water they are already using, rather than to acquire new water. It is possible that these differences are suppressing the overall treatment effect we estimate in Table 1. To explore this possibility, we estimate the share of each reservation that is already in agricultural land use at the beginning of the sample 1974. Figure A6 depicts a histogram of 1974 agricultural land use, and indicates that most reservations were using less than 20% of their land for agriculture in 1974. We test for heterogeneity along this margin by interacting the post-treatment indicator with an indicator for whether a reservation had greater than 5, 10, or 20% agricultural land use in 1974. Table A10 presents the results, and indicates that the effect of settlement is not statistically different based on the share of a reservation in agriculture in 1974.

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 instream flows.

Another possibility is that tribes use newly acquired water rights to intensify irrigation by

[23] Authors' calculation based on $\approx$ 2.1 million AF remaining under a 2 AFA scenario. Accordingly, 2.1 million AF $\div$ 2 AFA = 1.05 million acres of additional agriculture

[24] Per capita value of unused water is calculated by authors using 2010 U.S. Census Data

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 A4 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 5 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 underutilized 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 the myriad of institutional barriers that tribes face in putting water to productive use on reservations. Even though tribal water rights 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 divert or lease their water rights, creating a gap between paper vs. "wet" water rights for tribes.

We consider several factors that may explain the limited increase in agricultural land use observed after Winters' settlements. First, we examine the importance of irrigation infrastructure at the reservation level. Second, we analyze the importance of land tenure constraints at the parcel level. Finally, we explore whether leasing water to off-reservation users can provide a revenue source for overcoming the first two barriers to enable additional development on reservations.

5.1 Irrigation Infrastructure on Reservations

Surface water irrigation relies on large-scale infrastructure to store, divert, and convey water from where it is typically found—in drainages too rugged for farming—to where it can be used on flat, arable lands (Hanemann, 2014; Leonard and Libecap, 2019; Edwards and Smith, 2018). Developing this infrastructure is costly. Off of reservations, private financing of ditches and other small-scale infrastructure in the late 19th century gave way to larger projects funded by the federal Bureau of Reclamation in the early 20th century (Hanemann, 2014). Without similar infrastructure, reservations face physical and logistical hurdles to using their water (Water & Tribes Initiative, 2021).

Financing new infrastructure may prove difficult for tribes, especially in the wake of costly water right settlements. 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; Water & Tribes Initiative, 2021). Legal challenges to water settlements have delayed their implementation (and funding). Moreover, 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).

To assess the potential importance of limited large-scale infrastructure in explaining the small impacts of Winters settlements, we exploit the fact the the Bureau of Indian Affairs constructed irrigation infrastructure projects on some reservations in the early 20th century. Although many BIA projects are sorely in need of repair today (Carlson, 2018), the presence of an existing project nevertheless provides a major logistical advantage for the tribes that have them. Within our sample, seven treated reservations and two untreated reservations have BIA projects.[25]

To test whether the presence of a pre-existing BIA project facilitates additional water use after a Winters settlement, we estimate the following difference-in-difference-in-difference (DDD) model:

$$y_{irt} = \beta_1 PostSettlement_{rt} + \beta_B PostSettlement_{rt} \times BIA_r + +\beta_2 X_r t + \vec{\lambda}_i + \vec{\tau}_t + \varepsilon_{irt} \qquad (4)$$

where BIA_r is an indicator that is equal to one for parcels on reservations with a BIA project. β_1 is the estimated effect of Winters rights on reservations without a project, the omitted group, and β_B reports the difference in this effect for parcels on reservations with a project. All other parameters are defined as in Equation 1. Importantly, our model includes parcel fixed effects that absorb all time-invariant differences between parcels on reservations with and without BIA projects, 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, we believe these TWFE estimates are

[25]The treated reservations with projects are: Crow, Duck Valley (ID), Fort Hall, Fort Peck, Gila River, Southern Ute (CO), and Uintah Ouray. The untreated reservations with projects are Fort Belknap and Southern Ute (NM).

reliable, especially for agricultural land use. Moreover, by allowing for different treatment effects across different groups of reservations, we are flexibly incorporating heterogeneity into the model, reducing the likelihood that remaining heterogeneity will bias the TWFE estimates (Wooldridge, 2021). We present robustness checks using robust DiD estimators estimated separately for reservations with vs. without BIA projects in Table A6.

The results in Table 3 indicate that the effects of Winters settlements differ substantially based on the presence of a BIA infrastructure project. Across four of five columns, the baseline effect on reservations with no project is not statistically different from zero, and the column 5 coefficient is small in magnitude and marginally significant. In contrast, the coefficient on β_B is significant across all five specifications and is roughly twice the magnitude of the baseline average effect in Table 1. Table A5 indicates that there is still no effect on developed land use for either class of reservation. These results suggest that a lack of existing irrigation infrastructure is a major factor constraining tribes' ability to utilize their Winters rights. Pre-existing BIA projects have the capacity to make settlement water available for immediate use. Next, we explore whether variation in land rights within reservations also plays a role.

Table 3: Differential Impacts for Reservations with BIA Projects

	(1)	(2)	(3)	(4)	(5)
			Y = % Agriculture		
Post Settlement	-0.0932	-0.00985	0.106	-0.0934	0.122*
	(0.069)	(0.069)	(0.071)	(0.071)	(0.074)
Post Settlement X BIA Project	0.615***	0.572***	0.439***	0.616***	0.449***
	(0.114)	(0.115)	(0.126)	(0.117)	(0.126)
Observations	1,410,185	1,410,185	1,410,185	1,410,185	1,410,185
Adjusted R-squared	0.979	0.979	0.979	0.979	0.979
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 reservations with no BIA project. Standard errors are clustered by township and reported in parentheses* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

5.2 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).

Trusteeship on allotted and tribal land may prevent land use changes via a complex nexus of transaction costs, credit constraints, and bureaucratic hurdles. The non-transferability 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 the use if settlement water on fee simple land is still within the jurisdiction of the tribal government.

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 General Land Office (GLO). Each patent contains the parcel location, as indicated by the BLM's Public Land System Survey (PLSS), the Indian allotment number, the date it was initially issued in trust, and the date when it was converted from trust to fee simple ownership (if ever). To focus on fixed differences in land tenure, we limit our sample to parcels that have not changed in land tenure status since 1974 (the first year of land use data availability).[26] Parcels are categorized into three discrete land tenure groups: fee simple, tribal trust, and allotted trust land.[27] The last three rows of Table A1 show the share of each type

[26]In practice, this is the vast majority of parcels. We exclude parcels that have changed tenure status due to special circumstances so that our results are not confounded by factors that cause changes in tenure status, such as special acts of Congress.

[27]To deliver precise estimates of the effect of tenure, we exclude parcels that have a mix of land tenure associated with them. This would include allotted parcels where only a subset of the acreage was converted to fee ownership as well as parcels that were only partially allotted to begin with.

of ownership on treated versus 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:

$$\begin{aligned} y_{irt} = \beta_F PostSettlement_{rt} + \beta_A PostSettlement_{rt} \times Allotted_i + ... \\ ... + \beta_T PostSettlement_{rt} \times Tribal_i + \beta_2 X_r t + \vec{\lambda}_i + \vec{\tau}_t + \varepsilon_{irt} \end{aligned} \tag{5}$$

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. β_F is the estimated effect of Winters rights on fee simple parcels, the omitted group, and β_A and β_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 4 presents our estimates of the parameters in Equation 5 for agricultural land use using TWFE. We also report separate DiD estimates for each land tenure class using both robust DiD estimators in Table A7.[28]

The results in Table 4 reveal substantial differences in the impact of Winters settlements across land tenure classes. On fee simple land, settlement increases agricultural land use by roughly 0.95 percentage points, a 50 percent increase relative to the pooled coefficient in Table 1. This is a 13.5 percent increase relative to mean agricultural land use on untreated parcels. The coefficient estimates for allotted parcels indicate that increases in agricultural land use are almost completely negated on allotted and tribal trust land. 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 are not statistically different from zero in most cases. The results in Table A7 lead to similar conclusions. Tables A8 and A9 present the tenure-specific results for developed land use. We find no effect of Winters settlements on developed land use across any of the land tenure classes or estimators.

[28] These estimates include all land tenure classes in the untreated group, focusing only only changes in a given tenure class relative to the untreated group after treatment.

Table 4: Differential Impacts by Land Tenure Class: Agriculture

	(1)	(2)	(3)	(4)	(5)
	Y = % Agriculture				
Post Settlement (Fee Simple)	0.908***	0.928***	0.947***	0.905***	0.953***
	(0.148)	(0.148)	(0.146)	(0.147)	(0.146)
Post Settlement X Allotted	-0.727***	-0.728***	-0.705***	-0.725***	-0.712***
	(0.161)	(0.161)	(0.161)	(0.159)	(0.159)
Post Settlement X Tribal	-0.905***	-0.887***	-0.814***	-0.903***	-0.824***
	(0.148)	(0.150)	(0.154)	(0.151)	(0.155)
Observations	1,244,285	1,244,285	1,244,285	1,244,285	1,244,285
Adjusted R-squared	0.980	0.980	0.980	0.980	0.980
p-value (Fee + Allotted)	0.196	0.146	0.0743	0.198	0.0765
p-value (Fee + Tribal)	0.965	0.536	0.0278	0.966	0.0421
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 5 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* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

These results are 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 within a single reservation. 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 are similar, though we do not find differences in the effect on development across fee simple versus allotted trust parcels.

Our results imply that agricultural land use constraints on both allotted and tribal trust lands limit the ability of tribes to utilize their full water entitlements. We construct a counterfactual of changes to land use as the result of a settlement to understand the extent to which under-utilization of settlement water is the result of land tenure constraints. We assume that all parcels experienced the same increase in land use as observed on fee simple parcels:

$$\begin{aligned}\Delta \tilde{U}se_r = &\left(\frac{-\hat{\beta}^A_{ag}}{100} \times AFA_{ag}\right) \times \overline{ParcelAcres}^A_r \times N^A_r \\ &+ \left(\frac{-\hat{\beta}^T_{ag}}{100} \times AFA_{ag}\right) \times \overline{ParcelAcres}^T_r \times N^T_r \end{aligned} \tag{6}$$

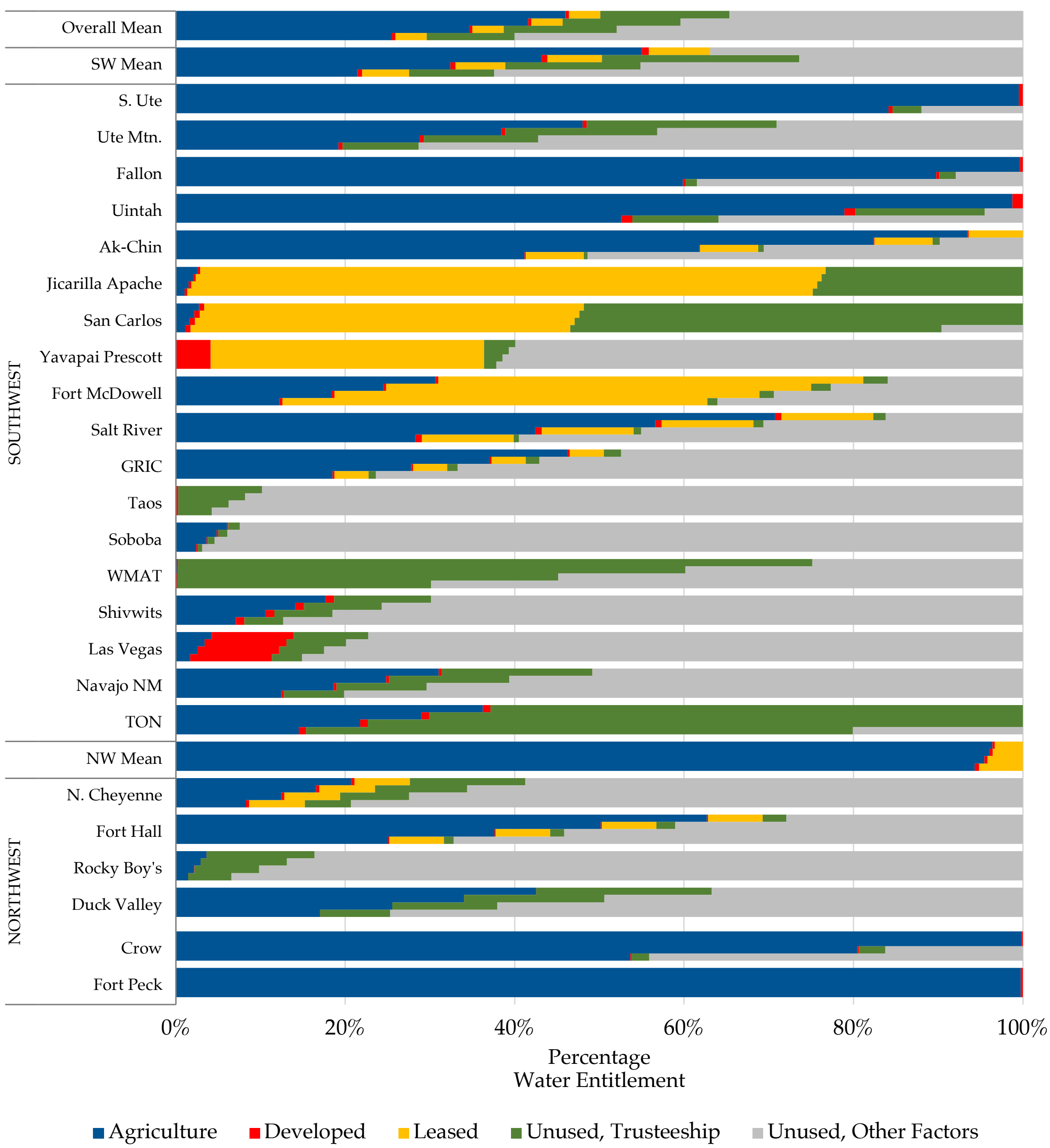
where $ParcelAcres_r^A$ and $ParcelAcres_r^T$ are the average size of allotted trust and tribal trust parcels, respectively, and N_r^A and N_r^T are the number of allotted trust or tribal trust parcels on reservation r. $\hat{\beta}_{ag}^A$ and $\hat{\beta}_{ag}^T$ are coefficients from column 5 of Table 4.

Essentially, Equation 6 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. Next, we take the estimated counterfactual changes to water use and add them to our estimates of *actual* total water use from Equation 3 (and Figure 5) to construct a counterfactual estimate of what water use on each reservation would have been in 2012 in the absence of land tenure constraints: $\tilde{Use}_r = \hat{Use}_r + \Delta\tilde{U}se_r$.

As a final step, we estimate the portion of unused water in the counterfactual 2012 scenario as $\hat{Unused}_r = Settlement_r – \tilde{Use}_r = Settlement_r – (\Delta\tilde{U}se_r + \hat{Use}_r)$. We express $\hat{Unused}_r$, $\tilde{Use}_r$ and $\Delta\tilde{U}se_r$ as shares of $Settlement_r$. The results are depicted in Figure 6. The blue shading indicates the estimated share of a settlement actually used in agriculture in 2012, whereas the red shading indicates the estimated share used in development in 2012. Yellow shading indicates leased water. The green shading corresponds to $\Delta\tilde{U}se_r$ 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.

The results in Figure 6 indicate that constraints on land tenure are a meaningful barrier to expanding water use on some reservations but are less consequential on others. Differences across reservations appear to be driven by a combination of the amount of non-fee land on a reservation and 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. Our estimates may understate the importance of land tenure because they focus only on own-parcel impacts but not on the effects on neighboring parcels. Spillovers across parcels and larger mosaics of fee simple, tribal, and allotted trust parcels may further constrain land use beyond the own-parcel effects (Leonard and Parker, 2021). Still, there appear to be important factors beyond land tenure constraining water use on some reservations.

Figure 6: 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 6 to Equation 3. 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.

5.3 Off-Reservation Water Leasing

The results in the previous two sections suggest that various factors that vary between and within reservations create major barriers to the utilization of Winters rights. Tribal water right settlements may yield limited changes in land use in the presence of these other frictions. One possible avenue for tribes to benefit from Winters settlements, even in the presence of these on-reservation barriers, is to lease water to off-reservation users.

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 with off-reservation users (Bovee, 2015; Nyberg, 2014). Prior academic work 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). Trading/leasing 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 transaction costs associated with leasing water rights (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 potential markets to specific water sources, or to certain municipalities, which reduces opportunities for trade 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 vs. 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:

$$y_{irt} = \beta_N PostSettlement_{rt} + \beta_L PostSettlement_{rt} \times Lease_{rt} + \beta_2 X_r t + \vec{\lambda}_i + \vec{\tau}_t + \varepsilon_{irt} \qquad (7)$$

where $Lease_{rt}$ is equal to one 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, β_N represents the effect of a Winters settlement on non-leasing reservations (the omitted group) and β_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.[29]

Table 5 presents the results of estimating Equation 7. 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 provides mixed evidence on the effect of leasing on agricultural land use. The coefficients on β_L in columns 1 and 2 suggest that reservations that lease their rights have much lower increases in agricultural land use, but these coefficients are only marginally significant and not robust to the inclusion of additional controls. On net, this suggests that tribes may be able to lease their water *and* develop agriculture, which is consistent with the fact that some reservations clearly do both in Figure 5.

Panel B of Table 5 presents the results for developed land use. Again, the effect of a settlement on developed land use is not conclusive. In columns 1–3, there is suggestive evidence that reservations that lease may see increases in developed land use, and these coefficients are much larger than the developed land use coefficients in any of our other models. However, these effects are marginally significant and not robust to the inclusion of controls for lending institutions. Hence, there is some limited evidence that tribes that lease their water *may* use the revenue to support additional development. Such an interpretation of the results would also require concluding that

[29] Because $Lease_{rt}$ varies over time and is only non-zero for treated reservations, we are not able to construct separate robust DiD estimates for leasing vs. non-leasing reservations was we did for BIA projects and land tenure.

this comes at the the cost of some agricultural land use, based on the marginally significant coefficients in columns 1 and 2 in Panel A.

Table 5: Differential Impacts by Leasing Status

	(1)	(2)	(3)	(4)	(5)
Panel A:					
	Y = % Agriculture				
Post Settlement	0.276***	0.364***	0.392***	0.263***	0.413***
	(0.068)	(0.076)	(0.066)	(0.064)	(0.069)
Post Settlement X Lease	-0.272*	-0.285*	-0.197	-0.245	-0.203
	(0.156)	(0.156)	(0.152)	(0.152)	(0.156)
Adjusted R-squared	0.979	0.979	0.979	0.979	0.979
p-value (Post Settlement + Post Settlement X Lease)	0.976	0.57	0.182	0.902	0.137
Panel B:					
	Y = % Development				
Post Settlement	-0.158	-0.239*	-0.160	-0.121	-0.178*
	(0.141)	(0.144)	(0.104)	(0.127)	(0.101)
Post Settlement X Lease	0.428*	0.441*	0.427*	0.351	0.390
	(0.239)	(0.239)	(0.232)	(0.245)	(0.240)
Adjusted R-squared	0.888	0.888	0.888	0.888	0.888
p-value (Post Settlement + Post Settlement X Lease)	0.367	0.481	0.300	0.454	0.411
Observations	1,504,140	1,504,140	1,504,140	1,504,140	1,504,140
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 7 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* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

6 Conclusion

Water right security has been fundamental to agricultural and economic development across the western U.S. (Hanemann, 2014; Leonard and Libecap, 2019), where water resources 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. We show that tribal water settlements, which can result in large changes in water right entitlements, 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 or leasing a small fraction of their water entitlements, potentially leaving as much as $1.6 billion in annual production or leasing revenue on the table. This finding is consistent with surveys that find nearly 48 percent of reservation households continue to lack indoor plumbing, sanitation infrastructure, and potable drinking water, even after reservations secure Winters rights (Crepelle, 2019; Rodriguez-Lonebear et al., 2020; Democratic Staff of the House Committee on Natural Resources, 2016). Darryl Vigil, the Chairman of the Ten Tribes Partnership in the Colorado River Basin testified before a Senate subcommittee that "the Ten Tribes are very concerned that while they struggle to put their water to use, others with far more political clout are relying on unused tribal water rights and will seek to curtail future tribal use to protect their own uses" (Vigil, 2013). Given underlying mistrust that Winters rights will not be upheld and ongoing water insecurity on reservations, it is unlikely that tribes are intentionally forgoing the use of settlement water.

While changes to reservation land and water use can evolve over generations, the (up to) five decades of land use change analyzed in this study highlights key barriers to water use: capital constraints, funding delays, and restrictions on trust land hamper settlement implementation. 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 potential 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.

Our results suggest that a lack of irrigation infrastructure at the reservation level impedes increases in agriculture, and that these infrastructure barriers are compounded by land tenure issues within reservations. While previous research has highlighted the importance of land tenure as barrier to resource use on reservations, this paper underscores the importance of considering property rights across multiple resources when attempting to reconcile historical expropriation of a particular resource, such as water. The myriad institutional barriers to accessing and using settlement water limits tribes' abilities to realize the the full economic and cultural benefits of legally enforceable water rights, and undermines tribal sovereignty over resource use decisions. Such barriers also contribute to broader water use inefficiencies across the West, as expanding relatively low-value, low-efficiency agriculture is one of the few ways for tribes to put settlement water to immediate use.

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 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, and tribes are emerging as critical players in Drought Contingency Planning for the Colorado River (Arizona Water Banking Authority, 2019). Still, the remaining gap between actual water use and water entitlements has allowed junior appropriators to continue to benefit from the use of water without compensation to tribes. Future research is necessary to explore the impact of Winters settlements on off-reservation water use to determine the extent of these uncompensated benefits.

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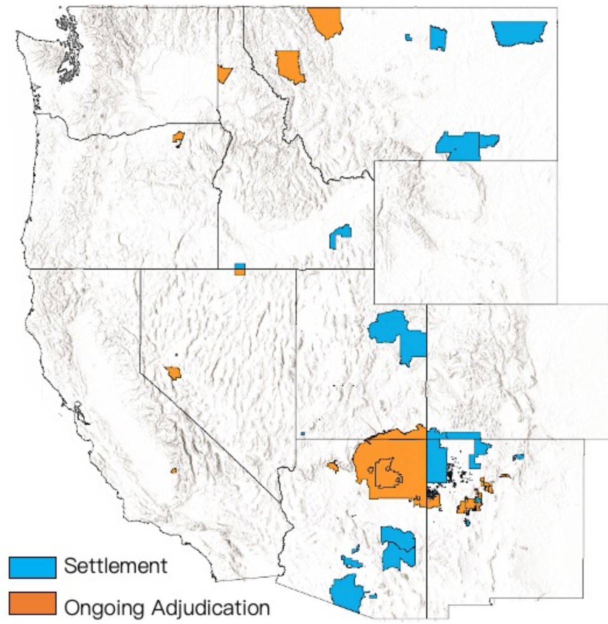
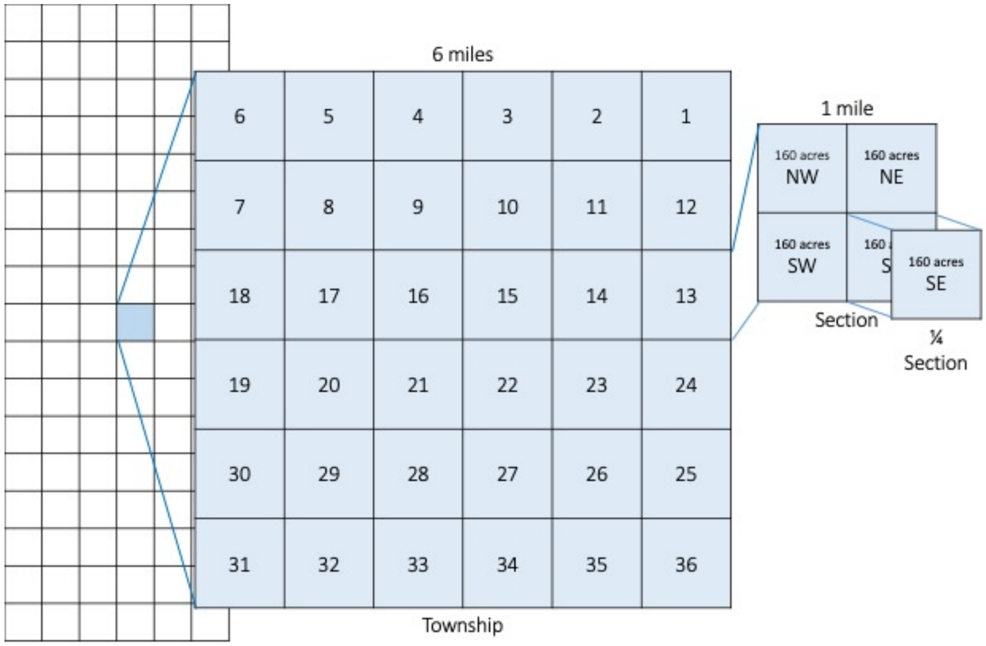
Appendix

Figure A1: 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 A2: 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).

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

	(1) Untreated Group	(2) Settlement Parcels	(3) Settlement - Untreated
% Agriculture	4.470	10.631	6.160***
	(18.716)	(27.690)	(0.904)
% Development	1.429	0.704	-0.724**
	(10.181)	(6.053)	(0.351)
Avg. Soil PI	6.568	8.046	1.479***
	(4.566)	(4.369)	(0.190)
Avg. Elevation	1,579.532	1,539.476	-40.055
	(440.810)	(697.382)	(27.057)
Ruggedness	12.526	13.952	1.427***
	(18.584)	(18.491)	(0.535)
Distance to Stream	14,903.496	9,472.705	-5,430.791***
	(14,023.704)	(12,244.000)	(627.587)
Fee Simple	0.091	0.153	0.062***
	(0.288)	(0.360)	(0.010)
Allotted Trust	0.068	0.136	0.068***
	(0.252)	(0.343)	(0.009)
Tribal Trust	0.717	0.599	-0.118***
	(0.451)	(0.490)	(0.018)
BIA Project	0.042	0.599	0.557***
	(0.200)	(0.490)	(0.018)
Observations	130,221	151,816	282,037

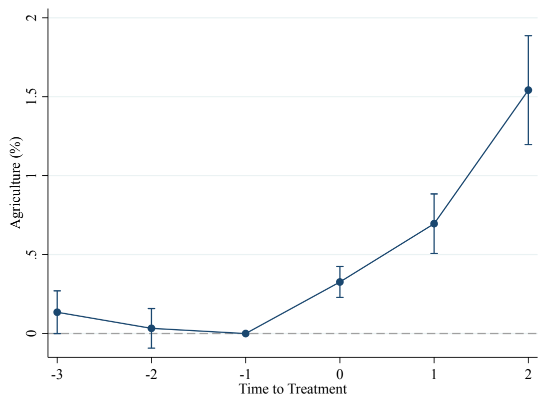
Notes: This table presents baseline (1974) summary statistics for parcels that are always untreated (column 1), or eventually treated (column 2), and the difference between the two (column 3). Standard errors are clustered by township and reported in parentheses* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Time-Varying Summary Statistics

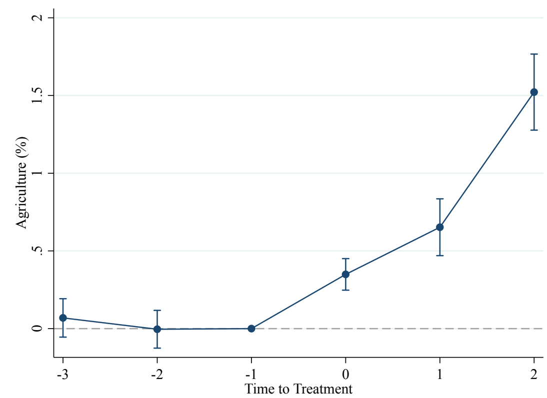
		(1) Unteated Group	(2) Settlement Parcels	(3) Settlement - Untreated	(4)
1974	1(Post Settlement)	0.000	0.000	0.000	0.000
		(0.000)	(0.000)	(0.000)	(0.000)
	Off-Res. Pop.	135263.359	312103.531	176840.156***	289751.219***
		(146437.703)	(362690.125)	(13,125.346)	(17,020.389)
	1(Has Casino)	0.000	0.000	0.000	0.000
		(0.000)	(0.000)	(0.000)	(0.000)
	1(Tribal Lending Institution)	0.498	0.266	-0.232***	-0.066***
		(0.500)	(0.442)	(0.024)	(0.025)
1982	1(Post Settlement)	0.000	0.001	0.001*	0.003*
		(0.000)	(0.037)	(0.001)	(0.001)
	Off-Res. Pop.	161187.484	447799.125	286611.625***	451829.156***
		(201334.250)	(553890.562)	(19,593.615)	(26,537.297)
	1(Has Casino)	0.000	0.000	0.000	0.000
		(0.000)	(0.000)	(0.000)	(0.000)
	1(Tribal Lending Institution)	0.581	0.266	-0.315***	-0.178***
		(0.493)	(0.442)	(0.023)	(0.026)
1992	1(Post Settlement)	0.000	0.197	0.197***	0.191***
		(0.000)	(0.398)	(0.014)	(0.015)
	Off-Res. Pop.	192360.422	564141.750	371781.312***	613581.000***
		(271260.469)	(771019.688)	(27,202.996)	(36,851.465)
	1(Has Casino)	0.030	0.187	0.157***	0.128***
		(0.171)	(0.390)	(0.015)	(0.016)
	1(Tribal Lending Institution)	0.682	0.310	-0.372***	-0.278***
		(0.466)	(0.463)	(0.023)	(0.026)
2002	1(Post Settlement)	0.000	0.675	0.675***	0.610***
		(0.000)	(0.469)	(0.016)	(0.019)
	Off-Res. Pop.	233258.828	756565.812	523306.969***	869115.062***
		(316746.750)	(1.098e+06)	(37,643.793)	(52,895.324)
	1(Has Casino)	0.208	0.486	0.278***	0.395***
		(0.406)	(0.500)	(0.023)	(0.023)
	1(Tribal Lending Institution)	0.724	0.432	-0.292***	-0.192***
		(0.447)	(0.495)	(0.024)	(0.022)
2012	1(Post Settlement)	0.000	1.000	1.000	1.000
		(0.000)	(0.000)	(0.000)	(0.000)
	Off-Res. Pop.	270889.719	954301.562	683411.812***	1.124e+06***
		(374084.844)	(1.392e+06)	(47,353.766)	(67,011.602)
	1(Has Casino)	0.331	0.732	0.401***	0.418***
		(0.471)	(0.443)	(0.023)	(0.018)
	1(Tribal Lending Institution)	0.875	0.656	-0.220***	-0.073***
		(0.330)	(0.475)	(0.021)	(0.014)
	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* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

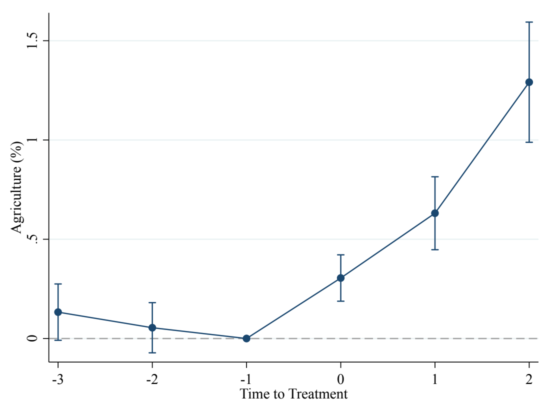
Figure A3: Agricultural Land Use Event Study — Alternative Specifications



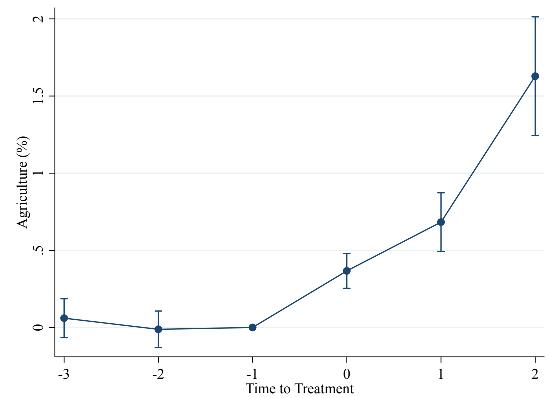
(a) Off-Res. Pop. Control



(b) Casino Control



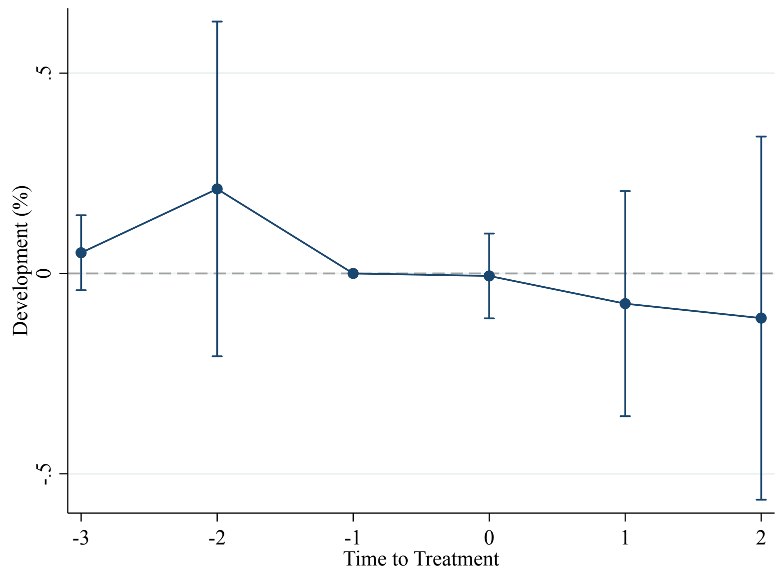
(c) Credit Control



(d) All Controls

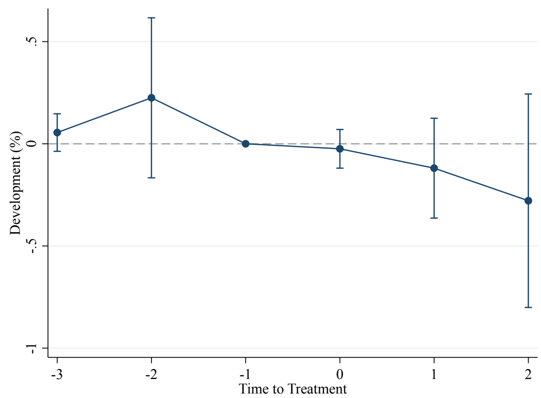
Notes: This figure depicts alternative versions of the event study estimates depicted in Figure 3 using the estimator developed by de Chaisemartin and d'Haultfoeuille (2020), implemented with the `did_multiplegt` package in Stata. The specifications in Panels (a) through (d) of the figure correspond 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 A4: Developed Land Use Event Study

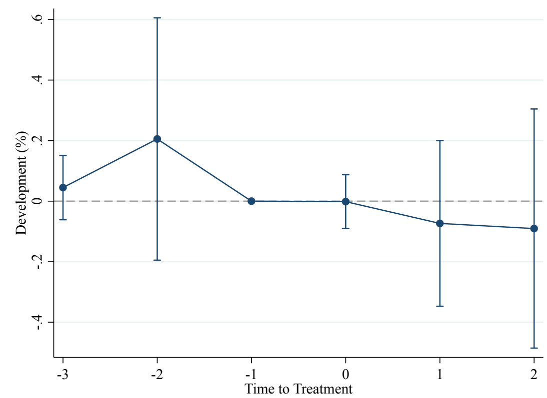


Notes: This figure depicts event study estimates using the estimator developed by de Chaisemartin and d'Haultfoeuille (2020), implemented with the `did_multiplegt` package in Stata. The model corresponds to the specification in column 1 of Panel A of Table 2, which includes parcel fixed effects and state-by-year 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.

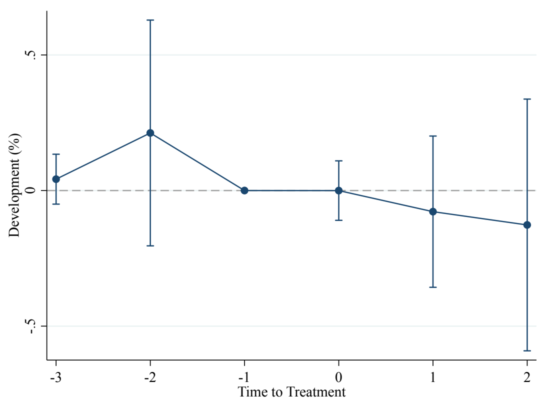
Figure A5: Developed Land Use Event Study — Alternative Specifications



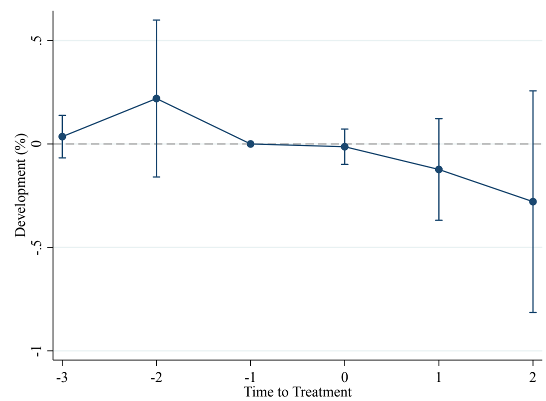
(a) Off-Res. Pop. Control



(b) Casino Control



(c) Credit Control



(d) All Controls

Notes: This figure depicts alternative versions of the event study estimates depicted in Figure A4 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.

Table A3: 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	0.0186***	0.0191***	0.0201***	0.0186***	0.0199***
	(0.0024)	(0.0024)	(0.0025)	(0.0025)	(0.0025)
Panel B:					
			Callaway & Sant'Anna (2020)		
Post Settlement	0.0101***	0.0098***	0.0104***	0.0137***	0.0135***
	(0.0014)	(0.0020)	(0.0014)	(0.0026)	(0.0027)
Panel C:					
			Two-Way Fixed Effects		
Post Settlement	0.0060***	0.0072***	0.0090***	0.0060***	0.0092***
	(0.0013)	(0.0015)	(0.0014)	(0.0013)	(0.0015)
Observations	1,410,185	1,410,185	1,410,185	1,410,185	1,410,185
Adjusted R-squared (TWFE)	0.948	0.948	0.948	0.948	0.948
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 A4: 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	0.222***	0.237***	0.189***	0.227***	0.226***
	(0.059)	(0.088)	(0.054)	(0.061)	(0.086)
Panel B:					
			Callaway & Sant'Anna (2020)		
Post Settlement	0.141***	0.019	0.161***	0.044	0.011
	(0.049)	(0.133)	(0.049)	(0.096)	(0.135)
Panel C:					
			Two-Way Fixed Effects		
Post Settlement	0.221***	0.314***	0.292***	0.218***	0.349***
	(0.068)	(0.072)	(0.065)	(0.066)	(0.068)
Observations	1,410,185	1,410,185	1,410,185	1,410,185	1,410,185
Adjusted R-squared (TWFE)	0.98	0.98	0.98	0.98	0.98
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 A5: Differential Development Impacts for Reservations with BIA Projects

	(1)	(2)	(3)	(4)	(5)
			Y = % Development		
Post Settlement	-0.0396	-0.141	-0.0574	-0.0866	-0.142
	(0.276)	(0.284)	(0.216)	(0.292)	(0.240)
Post Settlement X BIA Project	-0.0256	0.0276	-0.00984	0.115	0.124
	(0.244)	(0.250)	(0.199)	(0.300)	(0.249)
Observations	1,410,185	1,410,185	1,410,185	1,410,185	1,410,185
Adjusted R-squared	0.888	0.888	0.888	0.888	0.888
Parcel Fixed Effects	✓	✓	✓	✓	✓
Off-Reservation Population		✓			✓
1(Casino)			✓		✓
1(Tribal Lending Institution)				✓	✓

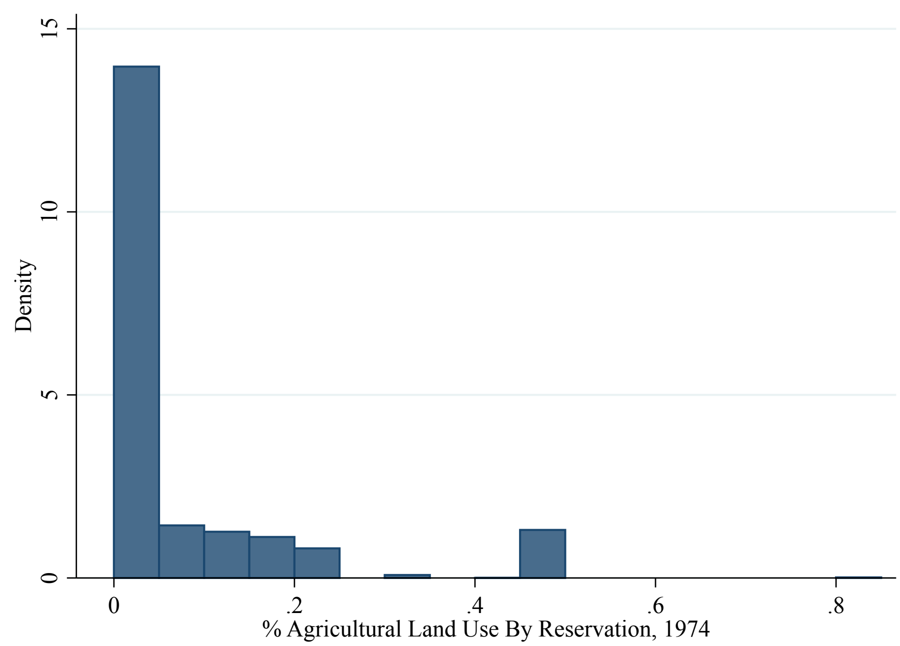
Notes: This table presents estimates of the difference-in-difference-in-difference model presented in Equation 4 using TWFE. The omitted category for the baseline difference is reservations with no BIA project. Standard errors are clustered by township and reported in parentheses* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Robust DiD Estimates by BIA Irrigation Project Status: Agriculture

	(1)	(2)	(3)	(4)	(5)
	Y = % Agriculture				
Panel A:					
	de Chaisemartin & D'Haultfoeuille (2020)				
Post Settlement (No BIA Project)	-0.036	0.072	0.196***	-0.041	0.220**
	(0.050)	(0.078)	(0.047)	(0.056)	(0.083)
Post-Settlement (BIA Project)	0.922***	0.907***	0.884***	0.925***	0.857***
	(0.069)	(0.067)	(0.078)	(0.075)	(0.085)
Panel B:					
	Callaway & Sant'Anna (2020)				
Post Settlement (No BIA Project)	-0.080	-0.279	-0.082	-0.177	-0.381
	(0.067)	(0.267)	(0.067)	(0.095)	(0.296)
Post-Settlement (BIA Project)	0.598***	0.568***	0.635***	0.524***	0.532***
	(0.086)	(0.095)	(0.084)	(0.107)	(0.107)
Parcel Fixed Effects	✓	✓	✓	✓	✓
Off-Reservation Population		✓			✓
1(Casino)			✓		✓
1(Tribal Lending Institution)				✓	✓

Notes: This table presents DiD estimates separately for parcels on reservations that do not have a BIA irrigation project, and for parcels on reservations that do have a BIA irrigation project. Panel A presents estimators specified by de Chaisemartin and d'Haultfoeuille (2020) and Panel B presents estimators specified by Callaway and Sant'Anna (2020). Standard errors are clustered by township and reported in parentheses* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure A6: Distribution of 1974 Agricultural Land Use



Notes: This figure depicts the distribution of 1974 agricultural land use by reservation. Each bin represents 5 percentage points (i.e., 5% agricultural land use).

Table A7: 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)	0.703***	0.705***	0.621***	0.681***	0.594***
	(0.141)	(0.140)	(0.141)	(0.139)	(0.136)
Post Settlement (Allotted)	0.238**	0.242**	0.211*	0.237**	0.207*
	(0.119)	(0.115)	(0.121)	(0.119)	(0.119)
Post Settlement (Tribal)	-0.161***	-0.060	-0.047	-0.152***	0.013
	(0.054)	(0.071)	(0.048)	(0.055)	(0.071)
Panel B:					
			Callaway & Sant'Anna (2020)		
Post Settlement (Fee Simple)	0.907***	0.861***	0.651***	0.895***	0.617***
	(0.129)	(0.128	(0.142)	(0.128)	(0.141
Post Settlement (Allotted)	0.283**	0.048	0.159	0.263**	0.081
	(0.116)	(0.111)	(0.113)	(0.112)	(0.08)
Post Settlement (Tribal)	-0.162***	-0.946	-0.131***	-0.318***	-0.138
	(0.046)	(0.111)	(0.045)	(0.069)	(0.117
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 5. Standard errors are clustered by township and reported in parentheses* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Differential Impacts by Land Tenure Class: Development

	(1)	(2)	(3)	(4)	(5)
	Y = % Development				
Post Settlement (Fee Simple)	-0.0255	-0.0617	-0.0239	0.0524	0.0292
	(0.1269	(0.132)	(0.112)	(0.109)	(0.098)
Post Settlement X Allotted	-0.134	-0.132	-0.133	-0.202	-0.19
	(0.112)	(0.111)	(0.104)	(0.129)	(0.123)
Post Settlement X Tribal	-0.0538	-0.088	-0.0499	-0.129	-0.126
	(0.089)	(0.092)	(0.058)	(0.118)	(0.084)
Observations	1,244,282	1,244,282	1,244,282	1,244,282	1,244,282
Adjusted R-squared	0.89	0.89	0.89	0.89	0.89
p-value (Fee + Allotted)	0.433	0.330	0.365	0.450	0.335
p-value (Fee + Tribal)	0.691	0.465	0.591	0.697	0.506
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 5 using TWFE. The omitted category for the baseline difference is fee simple land tenure. Table A9 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 A9: 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)	0.003	-0.0001	0.004	0.011	-0.002
	(0.037)	(0.037)	(0.040)	(0.038)	(0.039)
Post Settlement (Allotted)	-0.065	-0.073	-0.065	-0.065	-0.075
	(0.159)	(0.154)	(0.162)	(0.159)	(0.156)
Post Settlement (Tribal)	0.009	-0.119**	0.005	0.004	-0.119**
	(0.067)	(0.061)	(0.049)	(0.066)	(0.059)
Panel B:					
	Callaway & Sant'Anna (2020)				
Post Settlement (Fee Simple)	-0.119	-0.108	-0.132	-0.126	-0.141
	(0.073)	(0.086)	(0.064)	(0.077)	(0.081)
Post Settlement (Allotted)	-0.080	-0.073	-0.094	-0.105	-0.145
	(0.092)	(0.088)	(0.089)	(0.107)	(0.110)
Post Settlement (Tribal)	0.210	0.057	0.210	0.206	0.061
	(0.223)	(0.227)	(0.223)	(0.224)	(0.229)
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 5. Standard errors are clustered by township and reported in parentheses* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: DiD Estimates by 1974 Land Use

	(1)	(2)	(3)	(4)	(5)
			Y = % Agriculture		
Panel A:					
Post Settlement	0.160***	0.429***	0.348***	0.168***	0.366***
	(0.057)	(0.069)	(0.064)	(0.059)	(0.071)
Post Settlement X (Reservation Ag 1974 > 5%)	0.135	0.112	0.0004	0.093	-0.015
	(0.146)	(0.148)	(0.152)	(0.161)	(0.165)
Adjusted R-squared	0.979	0.979	0.979	0.979	0.979
Panel B:					
Post Settlement	0.180***	0.264***	0.348***	0.367***	0.366***
	(0.056)	(0.064)	(0.059)	(0.059)	(0.066)
Post Settlement X (Reservation Ag 1974 > 10%)	0.106	0.098	-0.001	0.039	-0.024
	(0.169)	(0.167)	(0.174)	(0.201)	(0.198)
Adjusted R-squared	0.979	0.979	0.979	0.979	0.979
Panel C:					
Post Settlement	0.157***	0.250***	0.320***	0.163***	0.341***
	(0.060)	(0.067)	(0.062)	(0.062)	(0.067)
Post Settlement X (Reservation Ag 1974 > 20%)	0.229	0.163	0.109	0.178	0.082
	(0.210)	(0.217)	(0.215)	(0.239)	(0.241)
Adjusted R-squared	0.979	0.979	0.979	0.979	0.979
Observations	1,504,140	1,504,140	1,504,140	1,504,140	1,504,140
Parcel Fixed Effects	✓	✓	✓	✓	✓
Off-Reservation Population		✓			✓
1(Casino)			✓		✓
1(Tribal Lending Institution)				✓	✓

Notes: This table presents estimates of the differential change in agricultural land use on reservations with differing levels of pre-settlement agriculture in 1974. Panel A includes an interaction term for reservations with greater than 5% of their total area devoted to agriculture in 1974–the baseline effect in Panel A is therefore the increase in agricultural land use on reservations with less than 5% agricultural land use in 1974. Panel B uses a 10% cutoff for the interaction term, whereas Panel C uses a 20% cutoff. Standard errors are clustered by township and reported in parentheses* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.