Property Rights without Transfer Rights: A Study of Indian Land Allotment[*]

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Abstract

Governments often institute transferability restrictions over property rights to protect owners and communities, but these restrictions impose costs: lowering property values, limiting investment, and increasing transaction costs. We study the long-run impacts of transferability restrictions using a natural experiment affecting millions of acres of Native American reservation land, by comparing non-transferable allotted-trust parcels with transferable fee-simple parcels. We use satellite imagery to study differences in land use across tenure types by leveraging fine-grained fixed effects to compare immediate neighbors. We find that fee-simple plots are 13% more likely to be developed and have 35% more land in cultivation.

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1 Introduction

Government programs that formalize the property rights of the poor often include provisions that limit the ability to transfer or alienate property for fear that property owners may sell their property under value or against their own long-term interest or the interests of their community. Legally speaking, owners enjoy *usufruct* rights, i.e. they can use their property and enjoy its "fruits," but they cannot transfer or alienate their property (Rose-Ackerman 1985, Ellickson 1993, Alston, Alston, Mueller, and Nonnenmacher 2018, ch2-3). Such transfer restrictions may be well-intentioned but they may also come at a heavy price: they can undermine a property's collateralizability, reduce the incentive to invest in improvements and, in our setting, interact with inheritance laws to cause ownership fractionation that raises coordination costs.

To investigate the long-run consequences of limits on transfer rights, we study a natural experiment that resulted from the policy of "Indian allotment" on American Indian reservations in the early 20th century. This policy generated a patchwork of land titles on reservations, with some Native households owning their land in non-transferable "allotted trust" and other immediately adjacent Native households owning land under full fee-simple property rights (Taylor, 1980; Carlson, 1981). Allotment ended with the *Indian Reorganization Act* (IRA) of 1934, creating a checkerboard of land tenures on reservations that persists to the present day.[1] To compare economic activity on plots with different title, we map the universe of historic land allotments recently digitized by the *Bureau of Land Management* (BLM) to the *Public Land Survey System* (PLSS) grid, and to high-resolution satellite data from the *National Wall-to-Wall Land Use Trends Database* (NWALT).

Endogeneity problems in the comparison of non-transferable allotted-trust and fee-simple lands on reservations arise from the fact that allotments may have been selectively converted into fee simple. The primary concern is selection on land characteristics: plots with certain characteristics may have been converted at a higher rate. Our primary approach to addressing these concerns is to rely on the fine-grained nature of our spatial data, which we leverage in both a high-resolution spatial fixed effect strategy and a series of matching estimators, comparing individual fee-simple plots to adjacent allotted-trust plots with very similar characteristics.

[1] Subsequent to 1934, moving land out of trust status remains a theoretical possibility but requires special approval from the Secretary of the Interior (Shoemaker, 2003; C.F.R.150.1-150.11, 1981).

The pattern of results suggests that comparing adjacent plots substantially undercuts potential endogeneity problems. With coarse reservation-level fixed effects, estimated coefficients change considerably with the inclusion of more controls and with the use of a matching estimator. In contrast, once we move to the smallest possible spatial fixed effects that compare only directly adjacent plots within square-mile PLSS "sections," estimated coefficients are unaffected by either the addition of controls or by various matching estimators. In our preferred specification, transferable fee-simple status increases the share of land in agricultural cultivation by four percentage points, and increases the probability of any non-agricultural development by 1.4 percentage points. This amounts to a roughly 35 percent increase in the share of land in agricultural cultivation relative to allotted-trust land, and a 13 percent increase in the probability of development. Our findings are robust to various forms of spatial correlation, including clustering by PLSS township, reservation, or spatial HAC standard errors proposed by Conley (1999, 2008). Spurious effects are ruled out using random inference permutation tests.

A separate potential selection concern is that *allottees* with certain characteristics (such as a higher proclivity for farming) may have had their plots converted at a higher rate before the policy ended in 1934, and that such characteristics continue to matter today. We address this concern by including (reservation-specific) family-name fixed effects in our baseline estimation. Family-name fixed effects add considerable explanatory power to the regressions, but their effect on our core estimates is marginal. Using a selection-on-observables exercise, we find it is highly unlikely that remaining unobservables could explain away our core results (Altonji, Elder, and Taber, 2005; Oster, 2019). This exercise also underscores the point that the scope for selection on unobservables is dramatically curtailed by the inclusion of PLSS section fixed effects.

Turning to mechanisms, we begin with a discussion of plausible channels (see Section 3). We place this discussion in the context of the broader literature on the security and transferability of property rights to show that the two main follow-on consequences of transfer restrictions on U.S. reservations are that (*i*) trust land cannot be collateralized for credit because it cannot be foreclosed on or sold outside of the tribe, and that (*ii*) tenancy-in-common inheritance rules lead to a proliferation of heirs with partial claims on land, i.e. the fractionation of ownership, which in turn causes major coordination and incentive problems that compound over time.

To get at the first channel, we leverage the fact that the NWALT satellite data exist in five decadal waves (1974, 1982, 1992, 2002, and 2012) for a panel analysis that leverages exogenous changes in credit supply driven by the deregulation of branch banking within and between states. In this exercise, we compare the impact of bank deregulation on allotted-trust vs. fee-simple plots, based on the logic that the benefits of expanded access to credit markets are muted on allotted trust land that cannot be used for collateral. We find that deregulation increased the impact of fee-simple ownership on development by over 60%, and had no impact on developed land use on allotted trust land. We do not find any impact of deregulation on cultivated land use, consistent with the fact that agricultural cultivation is comparatively not very capital-intensive.

To get at the second channel, we need to measure fractionation at the plot level, but we cannot do so directly because land ownership records are kept confidential by the BIA.[2] We overcome this by constructing measures of a plot's *latent potential* for fractionation. For our first measure, we digitize a special Census of American Indians from the mid-1930s, where we can match individuals to allotment numbers from the land records. We then assign an indicator to a plot for whether the original allottees were deceased by the 1930s, to proxy for the process of fractionation starting earlier. For our second measure, we use a plot's allotment year as a proxy for the extent of fractionation, based on the logic that plots allotted early had more time to fractionate as the original owners and subsequent heirs passed away. We validate these measures by aggregating them and correlating them to historical and modern reservation-level fractionation statistics. We find that both measures of a plot's *latent potential* for fractionation accentuate the negative effects of land being held in trust for cultivated land use, but that neither has an effect on fee-simple. This is consistent with fractionation amplifying the coordination problems that arise from a plot's trust status.

Our paper complements a large literature on land tenure and economic development (Alston, Libecap, and Mueller, 2000; Banerjee, Gertler, and Ghatak, 2002; Besley and Ghatak, 2010; Hornbeck, 2010). This literature has tended to focus more on property rights *security* than on *transferability* as the primary source of assurance, collateralizability, and realizability problems (De Soto, 2000; Goldstein and Udry, 2008; Besley, Burchardi, and Ghatak, 2012). In this literature, identifying the effect of non-transferability separately from insecurity has proved challenging because the two are typically bundled in the context of informal institutions that give the local chief or com-

[2]These records are managed by the BIA through the so-called *Trust Asset Accounting Management System*.

munity (village or lineage group) the ability to prevent transfers *and* revoke or reassign use rights after an owner's passing (Migot-Adholla, Hazell, Blarel, and Place, 1991; Besley, 1995; Fenske, 2011). In contrast, tenure is completely secure in our context, so that our results isolate the effects of transfer-restrictions from the effect of insecurity (with the caveat that the effects of transfer-restrictions are themselves potentially quite context-specific).

Our paper also speaks to a literature on indigenous economic development. Non-transferable property rights are particularly common among indigenous peoples, who historically had limited bargaining power in shaping their property rights. Examples include indigenous land rights in Mexico until recent *Procede* land reforms (De Janvry, Emerick, Gonzalez-Navarro, and Sadoulet, 2015), historical restrictions of Alaska Natives' transfer rights over their reindeer herds (Massey and Carlos, 2019), and many Native American households and tribes who historically did not, and today often still do not have transfer rights over their land. A number of studies suggest that more complete property rights could improve economic outcomes in indigenous communities (Trosper, 1978; Johnson and Libecap, 1980; Libecap and Johnson, 1980; Anderson, 1995; Alcantara, 2007; Dippel, 2014; Leonard, Parker, and Anderson, 2020). Having said that, our paper should not be misconstrued as implying that land-markets on reservations should simply be privatized without restrictions. There are clearly other concerns at play, including a desire by tribes to retain control over their land at least at the community-level.

Our study contributes to these by providing plausibly causal estimates of the cost of non-transferable land rights, using highly disaggregated spatial units of analysis. By including the near-universe of allotted reservations, we provide the average treatment effect on land use more broadly to complement a number of case studies comparing specific outcomes on trust-land and fee-simple land on specific reservations, including housing and business investment on Agua Caliente in California (Akee, 2009; Akee and Jorgensen, 2014), oil and gas development on Fort Berthold in North Dakota (Leonard and Parker, 2021), and irrigation on Uintah and Ouray in Utah (Ge, Edwards, and Akhundjanov, 2019). Our paper also relates to Aragón and Kessler (2020), who find that Canada's "certificates of possession," which are similar to allotted-trust rights, fall short of generating the benefits of full property rights.

2 Background

Following the establishment of the reservation system, *"Friends of the Indian"* reformers viewed assimilation as necessary for Native American survival (Carlson, 1981, p80).[3] Private property was seen as a key component in the path towards assimilation, and reformers viewed land allotment as the best way to introduce real property to Indians (Otis 2014).[4] The government concurred, and in 1886 Henry Dawes introduced an allotment bill to the Senate. On February 8, 1887, President Grover Cleveland signed the Dawes General Allotment Act into law. The Dawes Act authorized the president, through the *Office of Indian Affairs* (the BIA's precursor), to survey and allot reservation lands (Banner, 2009). Heads of household received 160 acres, and single persons over the age of 18, as well as orphans, received 80 acres. The policy put the land into an "allotted trust" until the reservation's local BIA agent determined that the allottee had acquired sufficient experience ("competence" was the word used) with private property, at which time they were granted full (i.e. fee simple) right to their land. As long as land was in trust, it could not be transferred or alienated.

On an allotted reservation, allotments were mandatory. There was no explicit policy about selecting land for allotment. Allottees could select a plot, but often did not, in which case the *allotting agents* determined the assignment of allotments (Banner, 2009; Otis, 2014; Carlson, 1981). *Allotting agents* often did not know much about the quality of the land because they were typically distinct from the reservation's resident BIA agent, and as such they only visited the reservations for the specific task of allotment (Bureau of Indian Affairs , 1887–1926). The process was characterized as follows:

> *The original allotments of land to the Indians were generally made more or less mechanically. Some Indians exercise their privilege of making their own selections [...]; others failing to exercise this right were assigned land. Often Indians who exercise the privilege made selections on the basis of the utility of the land as a means of continuing their primitive mode of existence. Nearness to the customary domestic water supply, availability of firewood, or the presence of some native wild food were common motives. Few [...] selected land on the basis of its pro-*

[3] The two main reformist groups were the *Indian Rights Association* and the *National Indian Defense Association*, respectively formed in 1882 and 1885.

[4] Most tribes had norms of private property, and the majority of tribes viewed their land as their tribal property, but no tribe had traditionally had private property rights over land (Demsetz, 1967).

ductivity when used as the white man used it. The allotting work was done too fast and on too wholesale a basis for the representative of the government to advise and lead [allottees] (Meriam, 1928, p470)

Even if there was indeed little evidence that allotted land was selectively different from unallotted land, it is a separate (and, for us, more important) question whether allottees were *selectively* declared competent and whether land was thus *selectively* converted into fee simple. It seems almost certain that this was the case, given that selection on "competence" was part of the policy design. Because our study's core comparison is between those allotted plots that were converted to fee simple and those allotted plots that were not, our empirical analysis will pay careful attention to selection into fee simple.

The allotment period began in 1887 and accelerated with the 1906 Burke Act. Between 1887 and 1934, but especially between 1906 and 1924, millions of acres of reservation lands were converted into allotments, and about half of these were converted into fee simple after some time. Appendix-Figure A1 plots the time series of both events. However, by the late 1920s, sentiment within the BIA had turned against allotment. One reason may have been the failures of allotment reported in the 1928 Meriam report.[5] Another reason may have been that the BIA did not want to release allotted lands from its control as the trustee of the land (McChesney, 1990). Whatever the motivations behind the government's and the BIA's about-turn, in 1934 the Commissioner of Indian Affairs, John Collier, introduced the Indian Reorganization Act (IRA), which ended allotment. As a consequence of the IRA, reservations that the BIA had not yet managed to survey by 1934 were never allotted (unallotted reservations play no role in our empirics); the IRA froze allotted-trust land in its trusteeship status indefinitely; already-converted fee-simple land remained fee simple; and unallotted lands remained under tribal ownership. The IRA's legacy was to create a patchwork land tenure pattern on reservations of (*i*) individually owned allotted-trust plots, (*ii*) individually owned fee-simple plots, and (*iii*) tribally owned plots. This patchwork persists to the present day.

[5] Meriam's report was written for the Institute of Governmental Research, a precursor of Brookings Institution. The report was concerned with the socio-economic conditions on reservations, with special attention to allotment.

3 Land Tenure Issues on Allotted-Trust Land

How do the issues facing allotted trust land fit into the context of the existing literature on incomplete property rights over land? This literature, see e.g. Place and Swallow (2000); De Soto (2000); Goldstein and Udry (2008); Fenske (2011); Besley et al. (2012), has tended to focus on three primary impacts of incomplete property rights: an inability to borrow against the value of the land one owns, a reduced incentive to invest in the land if more productive land is more likely to be expropriated, and an inability to "realize" the full value of the property through exchange, which can be thought of as second reason for reduced incentives to invest in the land. Besley (1995) calls this trifecta of channels "collateral," "security," and "gains-from-trade." Brasselle, Gaspart, and Platteau (2002) refer to these alternatively as "collateralizability," "assurance," and "realizability."

One major empirical challenge in the study settings above is that limits on transferability are bundled with the issues of property insecurity. In Besley (1995), for example, communal land tenure systems in sub-Saharan Africa return property rights to the chief after a tenant passes away. In this case, non-transferability causes insecurity in an intergenerational sense. However, this system also gives the chief the ability to claim a tenant's land at *any* time they see fit (Goldstein and Udry, 2008). Non-transferable property rights will have some direct negative effect on collateralizability (even if credit markets are informal) and on the incentive to invest (if the investments are long-run or inter-generational, e.g. planting new trees). However, the non-transferability of property rights is bundled with insecure property rights. Hence, it is often difficult to separately identify the effects of insecurity from non-transferability. One approach, taken by Huntington and Shenoy (2021), is to use RCTs and experimental variation to isolate the effect of one channel, such as transferability.

Another approach is to carefully unpack the transaction costs related to land tenure in the specific context in which they occur, acknowledging that, ultimately, *any* form of incomplete property rights will interact with the surrounding institutional environment in shaping the *de facto* property regime that actually becomes salient in a given setting. This is the approach we take in this paper. Our setting isolates the effect of non-transferability from the effect if property rights insecurity, because property rights security is unequivocal on reservations.[6]

[6] On allotted-trust plots, the owners hold the *beneficial title* to their land, and their land automatically bequeathes to their heirs.

However, the consequences of non-transferability in our context are quite specific and relate to the fractionation of land-ownership. The reason for this is that on reservations, non-transferability has interacted with states' legal rules governing inheritance: the court presumption in the U.S. is *tenancy in common*: in the absence of a will stating otherwise, heirs receive equal interests in the land, which remains physically undivided, giving rise to common ownership.[7] With full transfer rights, this issue of undivided claims on a common property is easily resolved through the courts' probate systems because heirs either sell the inherited property and divide the proceeds, or one heir takes out a mortgage on the property to buy out the others. Historically, well-developed capital markets in the rural U.S. have allowed this mechanism to keep American farms at a larger scale relative to European farms (Libecap and Alter, 1982; Alston and Ferrie, 2012). On allotted-trust land, however, this path is closed because interests in trust property are non-transferable.[8]

As a result, non-transferability on reservations has interacted with heirship law to create, over generations, massive problems of fractionated ownership over land, with each inheritance amplifying fractionation because each heir may have multiple heirs themselves (Russ and Stratmann, 2014). Today, the average allotted-trust plot has 13 claimants, but there are many instances of trust plots with hundreds of claimants on them (Department of Interior, 2013). Shoemaker (2003, p746) cites a 1987 report prepared for the Supreme Court that provides a compelling example: "*Tract 1305 (on the Sisseton-Wahpeton Lake Traverse Sioux reservation) is 40 acres. [...] It has 439 owners, one-third of whom receive less than $0.05 in annual rent and two-thirds of whom receive less than $1. The largest interest holder receives $82.85 annually.*"

This is not to say that non-transferable property rights do not also directly undermine an owner's ability to borrow against their property (the collateral channel), and therefore their ability to invest. In fact, there are studies that show this happens (Treuer, 2012; Feir and Cattaneo, 2020). And in a counterfactual world where each generation had only had one single heir, the collateral channel and the resulting constraints on investment might have ended up being the only major

[7]Tenancy in common is *partible heirship into equal undivided* claims. According to Habakkuk (1955), the intention of an *impartible heirship* court presumption is to keep the family property intact. In contrast, the intention of an *partible heirship* into equal divided claims is to keep the family intact. The result is a fracturing of the original family property into many neighboring farm sizes connected by family relations. This was the case in most of continental Europe in the 19th century, and is the case in India today, where farm sizes are often too small to operate at efficient scale (Libecap and Alter, 1982; Foster and Rosenzweig, 2011, 2022).

[8]This issue is amplified by the fact that will-writing was uncommon among Native Americans in the early parts of the twentieth century; and was apparently actively discouraged by the BIA, resulting in widespread use of the default tenancy in common rule (Stainbrook 2016, p2, Shumway 2017, p648).

land management issue on reservations. However, in practice there typically were multiple heirs to a plot of trust land, and fractionation therefore emerged as a separate major land management issue that created its own set of coordination problems.

4 Data

Allotment data: Following approval from the President, each patent issued on a reservation was filed with the General Land Office (GLO). These patents—subsequently digitized by the Bureau of Land Management (BLM)—record the transfer of land titles from the federal government to individuals. Each patent contains information regarding the patentee's name, the specific location of the parcel(s), the official signature date, total acreage, and the type of patent issued. These patents include—among other things—cash sales, homestead entries, and Indian allotments. These Indian allotments include a unique federally issued allotment number. An important feature of the GLO data is that we can see the date on which each allotment was issued and the date on which it was converted into fee simple, if ever. This ability to follow the individual allotments and when they were converted to fee simple allows us to identify them as either allotted-trust or fee-simple lands today. Appendix-Figure A1 depicts the aggregate annual flow of allotments issued and converted into fee simple from 1887–1934.

The Public Land Survey System: The GLO allotment data describe the location of each land allotment within the Public Land Survey System (PLSS), a rectilinear grid that divides (most of) the United States into 6×6-mile townships, each with a unique identifier.[9] Each township is composed of 36 square-mile sections numbered 1 to 36. Hence, any individual square mile of land within the PLSS can be referenced using the township identifier and section number. These numbered sections, which are 640 acres, were often divided into smaller "aliquot parts" when transferred to private ownership. The most common division is the quarter section, which is a 160-acre, $\frac{1}{2} \times \frac{1}{2}$-mile square referenced by a direction within a section (e.g., NE refers to the northeast corner of the section). Land could be further subdivided smaller than a quarter section, but the relevant quarter section can still be extracted from the aliquot part listed in the BLM allotment. For example, an allotment with an aliquot part of SW$\frac{1}{4}$NW is the southwest quarter of the northwest

[9] Each township is referenced by a township number and direction that indicate its North-South position and a range number and direction that identifies its East-West position relative a prime meridian.

quarter-section.

We focus on 160-acre quarter sections, which we refer to as *plots*, as the basic unit of analysis because quarter sections were the size of a standard Indian allotment and because quarter-sections are a standard unit of analysis that has been used previously in the literature to analyze land use decisions with satellite data (see, e.g., Holmes and Lee 2012; Allen and Leonard 2021).[10] Of the universe of allotments with a potentially matchable aliquot part variable in our data, we successfully matched over 95% to quarter sections in the PLSS using an aggregated shapefile from individual state BLM offices.[11] Appendix-Figure A2 depicts the location of all allotted plots across the Western United States. In most cases, these clusters of allotments trace out the boundaries of present-day reservations. In some rare cases, clusters of allotments trace out the boundaries of former reservations or rancherias that were subsequently terminated. This is true, for example, of the more dispersed looking clouds of allotments in Central and Northern California. Oklahoma, which is in fact densely covered by allotments, is the only gap in our spatial allotment data.[12]

Once allotments are geo-located, we track the history of BIA transactions associated with each allotment to code whether it was converted from allotted trust to fee simple. Figure 1 depicts an example of our data on the Pine Ridge Reservation in South Dakota.[13] Dark/orange plots are still in allotted-trust status, whereas light/grey plots have been converted to fee simple. The larger square outlines are the boundaries of 6×6-mile PLSS townships (nearly 150 can be seen on Pine Ridge). Unshaded areas correspond to quarter sections that are not associated with an allotment and therefore do not play a role in our analysis. There are three types of quarter sections for which this is true: (*i*) land was never allotted and is thus tribally owned; (*ii*) surplus land that was made

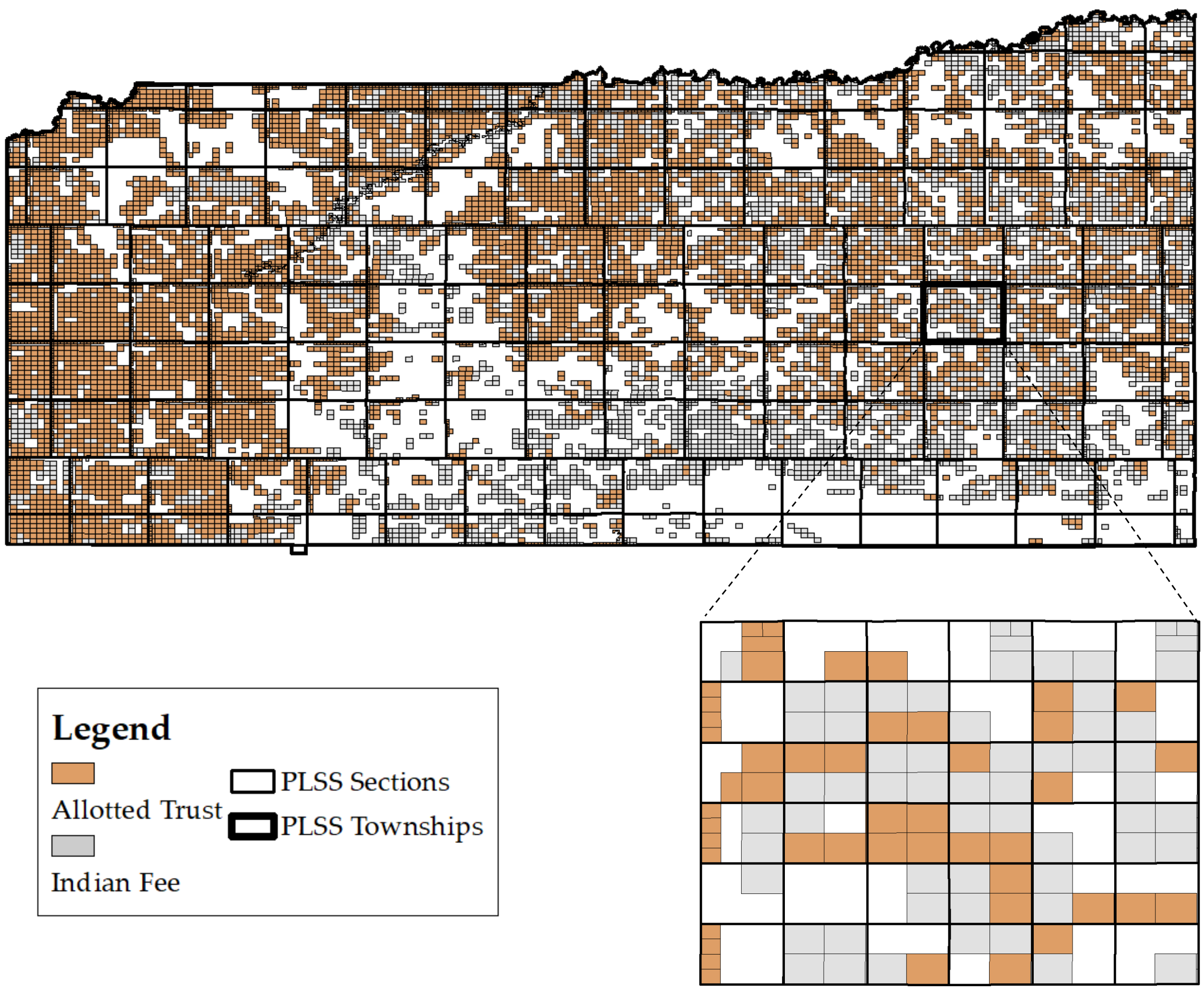
[10]While our typical feature is the 160-acre quarter section. The full PLSS contains other features types, like government lots, and smaller Indian Parcels. In our empirical approach we account for these differences by including fixed effects for PLSS feature type.

[11]Our match rate is above 99% for most states, with notably lower match rates for New Mexico (where the PLSS grid is less cleanly defined) and Wisconsin. In some cases the aliquot part is either missing, corrupted, or not not formatted in a way that allows matching to quarter-sections. Some quarter sections in our data are associated with more than one allotment, but we only use quarter sections that are mapped to a unique land tenure type.

[12]Eastern Oklahoma was covered by reservations for the 'Five Civilized Tribes' (the Cherokee, Chickasaw, Choctaw, Creek, and Seminole) who had been relocated there in the 1830s. These tribes were fully allotted under an alternative allotment agreement however, these allotments were not filed with the General Land Office because the land was already owned in fee-simple by the tribes at the time of allotment. We exclude the Western Oklahoma and the Osage reservation because allotments outside of the Five Civilized Tribes were not consistently categorized as 'Indian Patents' by the BLM.

[13]To simplify the analysis, we focus on plots which are matched to either all fee simple or all allotted trust, but not a mix. We also omit observations that converted from allotted-trust to fee-simple title after 1934, a rare occurrence that required special approval from the Secretary of the Interior. (See footnote 1).

Figure 1: Checkerboard Pattern of Land Tenure on the Pine Ridge Reservation



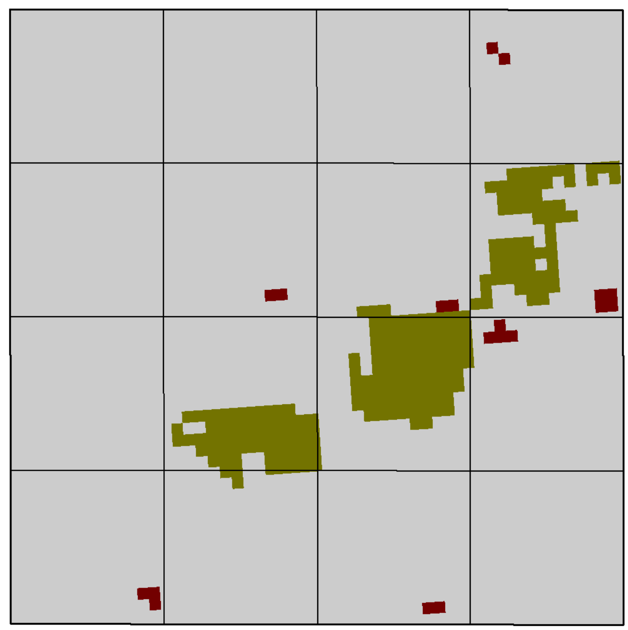
Notes: Distribution of Land tenure on the Pine Ridge reservation by allotment parcel (quarter-section) in the GLO data. Overlaying the reservation is a township grid. Each township is 36 square miles and contained in each are 144 ($= 36 \times 4$) quarter sections, each of which is 160 acres (one-quarter of a square mile) large. The figure depicts only the allotted quarter sections. Appendix-Figure A3 depicts a more detailed map of all land ownership types.

available to white settlers;[14] (*iii*) quarter sections which we are unable to match to the BLM data.[15] Appendix-Figure A3 shows a version of Figure 1 that separately identifies these other land types.

In our empirical analysis, we focus on fine spatial variation and primarily compare plots of different tenure regimes within a section (with 36 such sections in the township zoom-in on the bottom right of the figure). It is therefore important to note that land tenure regimes vary within close proximity of one another in Figure 1; i.e. most allotted-trust plots have at least one fee-simple direct neighbor and vice versa. This pattern is representative of most reservations.

Land use satellite imagery data: Our main outcome data on land use come from the *National Wall-to-Wall Land Use Trends* Database (NWALT). A collection of federal agencies known as the *Multi-Resolution Land Characteristics Consortium* produces the NWALT by combining satellite images from the LandSat database with remote processing techniques. The resulting database provides estimates of land cover at a 60×60-meter resolution for 1974, 1982, 1992, 2002, and 2012. We focus our attention on two main land cover classes in the NWALT: development and cultivated crops.[16] These two measures—development and cultivation—comprise the majority of "productive" uses of land that may be affected by restrictions on transferability.[17] Developed pixels in NWALT reflect capital invest-

Figure 2: NWALT Land Use Data



Notes: This figure depicts our outcome measure of cultivated and developed land in the NWALT data. The figure depicts 16 quarter-sections of 160 acres each. A one quarter-section plot is our primary unit of analysis (compare figure-notes in Figure 1).

[14]The vast majority of surplus lands is outside of reservations, because it was ceded from reservations as large tracts. We always omit surplus land inside modern reservation boundaries from our analysis. Proceeds from the sales of the surplus land were held in trust and appropriated at the discretion of Congress for "education and civilization (Banner, 2009).

[15]Reservation-level data obtained from Anderson and Parker (2008) indicates that 32% of the land on Pine Ridge is held in tribal trust and therefore falls into category (*i*).

[16]Pixels coded as cultivated by the NWALT include annual crop production, orchard crops, and any land that is being tilled. The NWALT also codes a variety of other land cover types including pasture, scrub/brush, forests, wetlands, perennial snow/ice, water, and "barren" land comprised of bedrock, talus, or sand dunes. For our land use measures, we exclude water from a plot's area: the denominator of each parcel's share-variable is land only.

[17]Another productive land use is extraction of natural resources such as coal or oil, but this is highly dependent on

ments in the construction of durable structures that may be associated with manufacturing, commercial activity, or private residences, and other scholars have used similar measures to study economic activity and growth at a fine spatial scale (Burchfield, Overman, Puga, and Turner, 2006; Saiz, 2010).

Figure 2 depicts our coding of land use from the NWALT data on a subset of the Fort Berthold reservation in North Dakota. The figure depicts four sections comprised of sixteen individual quarter-section plots. The larger green areas are land under agricultural cultivation, the small red areas are developed land. We express land use as a share of total usable parcel area, and define this denominator as the total number of pixels in a parcel excluding water and perennial snow/ice.[18]

Plot-level covariates: As controls, we construct a variety of variables for each plot. First, we measure the *geographic characteristics* of each plot. We use 30×30-meter elevation data from the *National Elevation Dataset* (NED) to measure the mean and standard deviation of elevation in each plot. We define the variable ruggedness as the standard deviation of elevation, a commonly-used measure of terrain ruggedness (Ascione, Cinque, Miccadei, Villani, and Berti, 2008). We use the soil productivity index developed by Schaetzl, Krist Jr, and Miller (2012) and estimate the average of the soil index within each plot. The soil productivity index ranges from 0 to 21, with soil index values greater than 10 representing highly productive soils (Schaetzl et al., 2012). We measure average annual precipitation and temperature using the 1980–2010 "climate normal" from the PRISM climate data set (Prism Climate Group, 2014). Finally, we calculate the distance from the centroid of each plot to the nearest perennial stream using the National Hydrography Dataset (U.S. Geological Survey , 2019).

In addition to the geographic controls listed above, we also estimate distances from each plot to *historically significant features* that may have affected conversion to fee simple. These include the nearest historical railroad completed by 1911 from Atack (2016), the nearest battlefield site from the Indian Wars (McConnell, 1919), the nearest exploratory route in the early 1800s (McConnell, 1919), and the nearest reservation boundary. We also calculate the latitude and longitude of each plot's centroid as a way of proxying for unobservables that are systematically correlated across

the location of valuable deposits.

[18]We recognize that the NWALT data only allow us to capture changes in land use at the *extensive* margin; i.e, bringing new land into agricultural production or development. Improvements such as intensifying irrigation and agricultural use, or buildings constructed over an empty parking lot are not measured. As a result, our estimates will likely understate the full effect of non-transferability.

space. Finally, we calculate the acreage of the PLSS polygon associated with each plot.[19] Feature size does not vary a lot. Broadly, every parcel in the Public Land Survey System was initially demarcated as a rectangular unit. However, there are some exceptions, such as Spanish land grants in California, Arizona, and New Mexico. Generally, plots have tended to stay rectangular in shape but in some cases there were clear benefits to less regular demarcation, such as in very rugged terrain.

5 The Effect of Transferable Property Rights

This section presents our baseline identification strategy and results, where we leverage both a fine-grained spatial fixed effect strategy and a series of matching estimators to compare individual fee-simple plots to only allotted-trust plots of very similar spatial characteristics. While we view geographic selection as the primary identification threat, we are also concerned about selection on allottees' characteristics, and we address this concern by conditioning our results on (reservation-specific) allottee last names to absorb potential confounding variation from unobservable family-traits within a reservation.

We estimate the effect of non-transferability on land utilization, using the following linear regression model

$$y_{ij} = \theta \times \text{FeeSimple}_i + \kappa_j + \lambda' X_{ij} + \varepsilon_{ij}, \tag{1}$$

where y_{ij} is the outcome of interest on plot i in spatial region j in 2012. FeeSimple_i is an indicator equal to 1 if a plot is under fee-simple ownership, and the outcome y_{ij} is a measure of land use. The coefficient of interest is θ, which represents the average difference in land use for fee simple versus nearby allotted-trust plots within the same spatial neighborhood κ_j. The vector X_{ij} includes various controls, as discussed below. Standard errors are clustered at the reservation level to account for potential spatial correlation.

For land use outcomes y_{ij}, we focus on developed land and cultivation. For developed land use, y_{ij} is an indicator equal to one if a plot contains *any* developed pixels. For cultivation, y_{ij} is the share of pixels on a plot that are cultivated. This decision is motivated by the nature of

[19] Our empirical approach also includes fixed effects for different PLSS features types, like aliquots, government lots, and smaller Indian Parcels.

the variation in our measures of developed and cultivated land use. As Appendix-Figure A4 illustrates, the primary variation in developed land use is at the *extensive* margin: 79% of plots with any development have less than 10% of pixels developed. In contrast, there is much greater *intensive*-margin variation for cultivation, where just 16% of plots have less than 10% of pixels developed, reflecting the fact that share cultivated is much more uniformly distributed from 0 to 100%.

One concern with the comparison in equation (1), which we discuss in Section 2, is that the geographic characteristics of a plot could have played a role in BIA agents' historical decision to convert it from allotted-trust to fee simple, and could have at the same time influenced contemporary land utilization directly.

Columns 1–2 of Table 1 display mean and standard deviations of geographic characteristics on allotted-trust and fee-simple land. Columns 3–4 report differences between fee-simple and allotted-trust land, conditioning on reservation fixed effects in column 3, and conditioning on section fixed effects in column 4.[20] The unconditional differences reported in column 3 of Table 1 suggest that when all data are pooled, and we only compare within a reservation, higher-quality lands were more likely to transition out of allotted-trust status: fee simple plots are at lower elevation, are less rugged (by about a standard deviation), and have higher soil quality (by half a standard deviation).[21] This is consistent with previous findings by Leonard et al. (2020). They are also warmer, closer to water sources, and more connected to the outside world (as proxied by proximity to historical railway lines). These differences become noticeably less pronounced in column 4, when we condition on fine-grained section fixed effects: the number of differences that are statistically significant shrinks, and among the differences that do remain statistically significant, the magnitude of the difference is reduced by a factor of 10 for most variables.[22] While differences

[20] According to the Office of Indian Affairs (1935), there were 119,000 allotments made in Oklahoma. which is home to several relocated tribes. As we discuss in Section 4, Oklahoma is not included in the data because its allotments were administered separately (through the so-called *Dawes Rolls*), and as a result *every single allotment* was converted to fee simple, so that Oklahoma allotments would not contribute to the allotted trust vs fee simple comparison.

[21] Elevation and ruggedness are expressed in 1,000s of meters in our regression models.

[22] Crucially, there is still significant residual variation in land ownership patterns within PLSS sections. We confirm that there is broad coverage across the sample in within-section identifying variation in the appendix. To do this, we regress the Fee Simple indicator on section and feature-type fixed effects alone and obtain the residuals. We calculate the share of non-zero residuals on each reservation in the sample (e.g., the share of plots on a reservation where land tenure is not completely explained by the fixed effects) and plot the results by region in Appendix-Figure A5. This figure makes it clear that we have numerous reservations with significant residual identifying variation in Fee Simple across most regions in the sample. The region with the least residual variation is the Pacific, where reservations are quite small. Ultimately, this region represents less than 1% of our main estimating sample.

remain statistically significant as we add finer spatial fixed effects, they arguably become economically insignificant: for example, within sections, fee simple plots are only 15 meters closer to the nearest railroad line.

Table 1: Baseline Differences and Variable Selection Model

	(1)	(2)	(3)	(4)	(5)	(6)
			Difference: Fee - Trust		Variable Selection	
	Trust	Fee	Res FE	Sect FE	Res FE	Sect FE
Elevation	0.926	0.652	-0.014	-0.001	0.083	-0.137
	(0.487)	(0.301)	[0.013]	[0.001]	[0.151]	[0.097]
Ruggedness	8.144	4.540	-2.026***	-0.299***	-0.006***	-0.002***
	(9.206)	(5.731)	[0.638]	[0.107]	[0.002]	[0.000]
Soil	9.490	11.527	1.409***	0.089**	0.013***	0.003***
	(4.518)	(4.004)	[0.263]	[0.035]	[0.002]	[0.001]
Precipitation	480.048	564.524	-3.542	-0.260		
	(281.052)	(223.368)	[5.240]	[0.243]		
Temperature	7.942	6.947	0.019	0.001	0.023	
	(2.917)	(2.019)	[0.046]	[0.001]	[0.022]	
Dist. Streams (kms)	6.006	4.133	-0.107	-0.008	-0.002	
	(7.306)	(4.200)	[0.316]	[0.012]	[0.002]	
Dist. RRs (kms)	22.839	15.799	-2.843***	-0.015*	-0.003**	-0.006**
	(17.717)	(15.438)	[0.667]	[0.009]	[0.001]	[0.003]
Dist. Wars (kms)	110.495	138.064	1.917	0.006	0.001	
	(85.484)	(77.591)	[2.464]	[0.009]	[0.000]	
Dist. Trails (kms)	18.757	21.273	0.756	-0.007	0.002*	
	(15.727)	(17.651)	[1.169]	[0.009]	[0.001]	
Dist. Boundary (kms)	10.622	9.817	-0.508	-0.003	-0.002**	
	(8.941)	(8.777)	[0.352]	[0.010]	[0.001]	
Longitude	-106.364	-101.778	0.092	0.000	0.079**	
	(6.620)	(7.420)	[0.061]	[0.000]	[0.036]	
Latitude	44.535	45.674	0.010	-0.000	0.048	
	(3.651)	(2.021)	[0.015]	[0.000]	[0.074]	
Feature Size	105.439	100.060	4.963*	4.354***	0.000**	0.000***
	(60.213)	(61.147)	[2.747]	[1.315]	[0.000]	[0.000]
Observations	65,228	26,212	91,433	85,491	91,425	85,488

Notes: This table reports on baseline differences in land characteristics. Columns 1–2 present mean and standard deviations by land tenure. Column 3 reports differences conditional on reservations fixed effects, column 4 reports differences conditional on section fixed effects. Columns 5–6 apply a variable selection model to the same tuple of fixed effects. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

To get a better handle on the residual differences, i.e. those differences that remain when we condition on all other differences, we also run a variable-selection model to tell us agnostically which land characteristics are most different across the two land-types in residual variation. Variable selection models are commonly used to discipline the set of control variables by determining which variables are actually predictive of the main regressor of interest in a multivariate regression. For an application, see for example Dippel and Heblich (2021). In columns 5–6, we use *Akaike's information* criterion to select the set of control variables that really matter, conditional on either reservation or section fixed effects. For the mechanics of variable selection models, see for instance Lindsey, Sheather, et al. 2010.

Variable selection models tend to reduce observed differences, by virtue of conditioning them on other observed differences which are often correlated. This is true in column 5, and becomes even more true in column 6, where the model selects only five land characteristics as being meaningfully different between fee and trust once we condition on section fixed effects in the last column. Again, those differences are also significantly compressed in magnitude relative to column 3. Nonetheless, some imbalance obviously remains even in column 6, and this fact informs our empirical approach.

Table 2 reports the results of estimating equation 1 for the two outcomes of interest. In both panels of this table, columns 1–2 use OLS, while columns 3–6 use different matching estimators.[23] Column 1 estimates OLS with only spatial fixed effects, and column 2 adds the controls selected by the variable selection model in Table 1.[24] For the finer section fixed effect, we run a series of matching estimators. Column 3 runs a propensity-score matching estimator that matches on all controls selected by the variable selection model in Table 1. The remaining columns report on additional matching-estimator variations, first broadening the set of allowed matches that satisfy the common support restriction (col 4), and then using multi-dimensional matching through Mahalanobis distance matching on the same set of variables in columns 5 and 6 (Rosenbaum and

[23]Every specification also includes feature-type fixed effects.

[24]Appendix-Table A2 uses the specification from column 4 to assess the robustness of our results to potential spillovers from fee-simple land to trust land. Panel A demonstrates that trust plots with more fee simple neighbors do not have a higher probability of development, but they do have more cultivation, suggesting positive spillovers in agriculture that may bias our main estimates toward zero. Panel B confirms that our core results are robust to controlling for the number of neighboring fee parcels, a proxy for large spillovers.

Rubin, 1985). Across columns, we cluster standard errors at the reservation level.

Table 2: Transfer Restrictions and Land Use Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Any Development						
FeeSimple	0.018**	0.014**	0.018***	0.014**	0.014**	0.015***
	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)
Panel B: Share Cultivated						
FeeSimple	4.558***	4.389***	3.753***	3.852***	3.702***	3.944***
	(1.053)	(1.037)	(0.979)	(0.972)	(0.979)	(0.958)
Fixed Effects	Section	Section	Section	Section	Section	Section
Method	OLS	OLS	PSM	PSM	MD	MD
Neighbors	NA	NA	1	Any	1	Any
Match Vars	NA	NA	VSelect	VSelect	VSelect	VSelect
Covariates	None	VSelect	None	None	None	None
Observations	85,488	85,488	91,440	91,440	91,440	91,440

Notes: Both panels of this table report on six variations of estimating equation 1, for two separate outcomes. Every specification includes section fixed effects and feature-type fixed effects. Column 1 estimates OLS with only spatial fixed effects, and column 2 adds the controls selected by the variable selection model in Table 1. In columns 3 – 6, we run a series of matching estimators. Column 3 runs a propensity-score matching estimator that matches on all controls selected by the variable selection model in Table 1. The remaining columns report on additional matching-estimator variations, first broadening the set of allowed matches that satisfy the common support restriction (col 4), and then using multi-dimensional matching through Mahalanobis distance matching on the same set of variables in columns 5 and 6 (Rosenbaum and Rubin, 1985). Across columns, we cluster standard errors at the reservation level. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

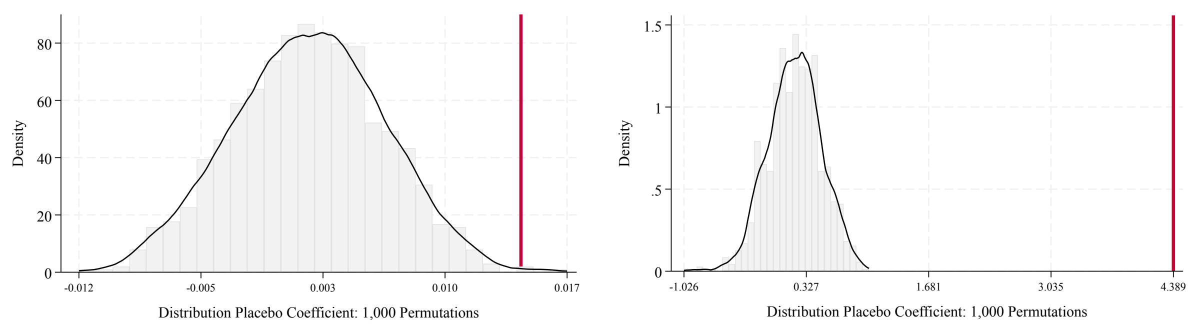
Table 2 demonstrates that the estimated coefficients are unaffected by both the addition of controls and by various matching estimators. Panel (a) suggests that transferable fee-simple status increases the probability of there being any non-agricultural development by 1.4 percentage points, and Panel (b) suggests that fee-simple status increases the share of land in agricultural cultivation by four percentage points.[25] Appendix-Table A1 helps contextualize the magnitude of these results. The baseline untreated mean for the probability of development on trust land is 0.106, and the baseline mean share of cultivated land on trust land is 11. These baseline numbers imply that transferability leads to a 13 percent increase ($0.014/0.106$) in the probability of development, and a 35 percent increase ($3.89/11$) in the share of cultivated land relative to the mean on allotted-trust plots.

As an additional robustness check, we use randomization inference to rule out spuriously cor-

[25]The NWALT data make it difficult to make intensive-margin comparisons within different land use classes (e.g., different forms of development or agriculture), especially in a relatively rural setting like ours. However, in Appendix-Table A3, we provide some additional evidence of differences in agricultural outcomes for fee-simple plots by comparing Net Primary Production (NPP) and crop choice using the same specifications presented in Table 2. See Appendix C for an extended discussion.

related effects through a permutation test. For this purpose, we replace the actual over $26,000$ fee-simple plots with an equal number of randomly drawn plots (from all plots), and then re-estimate our preferred specification from column 2 above with selected controls and section fixed effects. We repeat this experiment until we obtain 1,000 permutations that mimic the spatial correlation of fee-simple plots in the real data, and then compare the distribution of the estimated placebo effects to the fee-simple effect.[26] Figure 3 shows the result of this permutation exercise: the permuted distribution is centered around a mean of zero, and even the 99-th percentile of the distribution is far to the left of the actual estimates from column 2 of Table 2.

Figure 3: Randomization Inference



(a) Any Development

(b) Share Cultivated

Notes: The figure shows the distribution of 1,000 coefficients from randomization inference estimations where we replace the actual fee-simple plots with an equal number of randomly drawn plots. In contrast to the distribution, the vertical line shows the magnitude of the actual estimated coefficients from column 2 of Table 2.

Selection on allottees A remaining challenge that is not addressed by our identification strategy so far is that allottees' characteristics (or actions) could have played a role in the BIA agents' historical decision to convert trust land into fee simple, and that these same characteristics or actions could have had some independent long-run effects on the their heirs' future land utilization. We address this concern by conditioning equation (1) on (reservation-specific) fixed effects for al-

[26]We achieve this permuting the data one township at a time. For each township, we first calculate the Moran's I of the Fee Simple variable. Next, we randomly draw Fee Simple plots across all plots within a township and re-calculate Moran's I on the permuted data. We repeat this exercise until we obtain 10 permutations of each township where the where we fail to reject the null hypothesis that the permuted Moran's I is equal to the true Moran's I at a 5% significance level. Finally, we randomly sample from these permuted townships, appending them to construct a permutation of the full data set and re-run our main regression. We then repeat this final exercise 1,000 times.

lottees' last names. This is common way of addressing unobservable productivity differences in the literature on land tenure and productivity; see e.g. Deininger and Ali (2008). This approach is particularly salient in an environment with strong family social ties, or where characteristics or decisions are clustered at the level of the family.

Our setting includes many large reservations, such that individuals with a common last name may not be directly related, or may be associated with physically distant and dissimilar plots. Indeed, the average number of observations per reservation-by-family-name group is 35. Therefore, as a final refinement we include family name-by-township fixed effects that identify the effect of fee simple property rights by comparing individuals with the same last name who received plots within the same 6×6-mile neighborhood. These finer fixed effects include an average of 5 observations per group.

The results of including family fixed effects are presented in Table 3. Columns 1 and 5 report the results with baseline section fixed effects. Columns 2 and 6 introduce controls from the variable selection model. Columns 3 and 7 add the reservation level family-name fixed effects. Columns 4 and 8 include the family name-by-township fixed effects. Within each outcome, the Adjusted R^2 suggests that family-name fixed effects add considerable explanatory power to the regressions, but their effect on our core estimates is marginal, as the reported coefficients are qualitatively very similar to those in Table 1.[27]

We formalize this intuition using the approach proposed by (Altonji et al., 2005; Oster, 2019): we report the estimated Oster δ for each model that includes control variables. This parameter corresponds to the proportional degree of selection on *unobservables* that would have to be present for $\hat{\theta}$ to be zero, based on the selection on *observables* estimated from the data. The estimated values suggest that selection on unobservables would have to be 13–27 times the estimated selection on our observed covariates to undermine the estimated effect for development (23–71 for cultivation).[28] The magnitude of the Oster δs suggest that selection on unobservables would need to be severe to explain away the estimated effects. Given these results, we believe that our focus on difference in land quality and allottee family characteristics sufficiently addresses the selection

[27]The number of observations drops because of unique names among allottees within reservations.

[28]Following Oster (2019), we use a value of "rmax" that is 1.3 times the observed R^2 in the controlled regression where we condition on PLSS feature type and spatial fixed effects.

criteria that agents used when converting land to fee simple.

Table 3: Adding Fixed Effects for Allottees' Family Names

	Any Development				Share Cultivated			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
FeeSimple	0.018**	0.014**	0.015**	0.019**	4.558***	4.389***	4.515***	4.174***
	(0.007)	(0.007)	(0.008)	(0.009)	(1.053)	(1.037)	(1.263)	(1.232)
Oster δ		13.022	14.741	27.814		23.830	29.929	71.258
HAC SEs (25 kms)	(0.005)	(0.005)	(0.006)	(0.008)	(0.585)	(0.581)	(0.666)	(0.842)
HAC SEs (100 kms)	(0.006)	(0.005)	(0.006)	(0.007)	(0.782)	(0.774)	(0.903)	(1.013)
Adj. R^2	0.3539	0.3594	0.3928	0.4072	0.7697	0.7717	0.7933	0.8117
Observations	85,488	85,488	77,834	67,309	85,488	85,488	77,834	67,309
FE Type	Section	Section	Section	Section	Section	Section	Section	Section
# Spatial FEs	21,553	21,553	19,917	17,415	21,553	21,553	19,917	17,415
# Name FEs	0	0	9,509	17,209	0	0	9,509	17,209
Covariates	None	VSelect	VSelect	VSelect	None	VSelect	VSelect	VSelect

Notes: This table extends the OLS columns from Table 2, where columns 3 and 7 add reservation-specific fixed effects for allottees' last names and columns 4 and 8 replace these with PLSS township-specific fixed effects for allottees' last names. Standard errors are clustered at the reservation level and significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Lastly, we provide evidence that our inference is not sensitive to our treatment of the standard errors. Our preferred approach clusters by reservation, allowing all plots within a reservation to be arbitrarily correlated. However, clustering in this manner may not match the extent of spatial spillovers, particularly among adjacent reservations, like those in Washington State and the Southwest, where there are many neighboring reservations. In these instances, clustering may insufficiently address spatial correlation (Kelly, 2019, 2020). As an alternative, we report spatial HAC standard errors following Conley (2008) and Hsiang (2010) under two alternative distance thresholds. The first uses a 25 km threshold, that allows for narrow spillovers in space, while the second applies a 100 km threshold that allow for spillovers across a larger area (Smith, 2022). This larger threshold allows the error terms to be correlated across nearby but distinct reservations.

6 Mechanisms

As discussed in Section 3, the two main follow-on consequences of transfer-restrictions on U.S. reservations are that (*i*) land cannot be collateralized for credit, and that (*ii*) tenancy-in-common inheritance rules lead to a proliferation of heirs with partial claims on land, i.e. the fractionation of ownership, which in turn causes coordination and incentive problems over time. Even without inter-generational fractionation, non-transferability would affect development through the collateral channel. And indeed, collateralizability has long been viewed as a significant impediment to investment on reservations (Community Development Financial Institutions Fund, 2001).[29]

Collateral Channel: To get at the collateral channel, we leverage the fact that the NWALT satellite data exist in five decadal waves (1974, 1982, 1992, 2002, and 2012). This allows us to perform a panel analysis, in which we exploit exogenous changes in the supply of credit driven by the wave of state-level banking deregulation in the 1980s and 1990s. Historically, states imposed restrictions on branch banking both within states (intrastate branching) and between states (interstate branching) to create local banking monopolies from which they could extract rents (Kroszner and Strahan, 1999). During the 1980s and 1990s, most states gradually permitted intrastate branching through mergers and acquisitions and/or the opening of new ("de novo") branches. The Reigle-Neal Interstate Banking and Branching Efficiency Act permitted interstate branching on a nationwide basis, though many states had already passed legislation to allow this, or adopted the federal measures earlier (Demyanyk, Ostergaard, and Sørensen, 2007).

Previous research has used the timing of state-level reforms as an exogenous source of variation to study the impact of changes in the credit supply on a variety of outcomes including housing markets (Favara and Imbs, 2015), entrepreneurship (Black and Strahan, 2002; Kerr and Nanda, 2009), economic development (Jayaratne and Strahan, 1996; Huang, 2008), business cycles (Morgan, Rime, and Strahan, 2004; Hoffmann and Shcherbakova-Stewen, 2011; Demyanyk et al., 2007), personal bankruptcy (Dick and Lehnert, 2010), and education investments (Levine and Rubinstein, 2013; Sun and Yannelis, 2016).

The upshot is that deregulation of both interstate and intrastate branching in the 1980s and 1990s—driven by statewide political-economic factors—is an established source of exogenous variation in the supply of credit for consumers and businesses. Perhaps most importantly for our purposes, Sun and Yannelis (2016) demonstrate that deregulation increased access to credit at the intensive margin by documenting increases in total private loan volume combined with reduced bank fees and mortgage loan interest rates. Deregulation also expanded the supply of credit at the extensive margin—making credit available to individuals who previously lacked access—by

[29]It also creates distortions. For example, Native Americans have by far the highest rate of mobile-home ownership in the U.S. because mobile homes can be repossessed whereas permanent structures built on trust land cannot be repossessed any more than the land itself (Treuer, 2012; Feir and Cattaneo, 2020).

spurring new branching in unbanked communities (Amel and Liang, 1992; Wirtz, 2005; Oberfield, Rossi-Hansberg, Trachter, and Wenning, 2024), and by allowing banks to extend loans to riskier individuals thanks to greater diversification (Dick and Lehnert, 2010).

If the inability to collateralize allotted trust land is an important mechanism behind our results, then increases in the supply of credit to a reservation—either through the opening of new banks or through intensive-margin changes at existing institutions—should have a smaller (or zero) impact on allotted trust land than on fee simple land. First, we verify that state banking deregulation did affect the supply and utilization of credit on reservations. Appendix-Table A4 demonstrates that the number of banks on reservations in treated states increased after deregulation. The only way to get a measure of lending on reservations is from the Home Mortgage Disclosure Act (HMDA) data (Parker, 2012). We use the HMDA data in Appendix-Table A5 to demonstrate that census tracts on reservations with more banks issue more loans and have greater total loan value, both in terms of actual loans (Panel A) and loan applications (Panel B).[30]

We use a canonical difference-in-difference-in-difference model to compare the impact of bank branching deregulation before vs. after these state-level reforms (first difference) in regulated vs. de-regulated states (second difference) on fee-simple vs. allotted trust plots (third difference). We estimate the following equation:

$$y_{ijt} = \theta \times \text{FeeSimple}_i + \delta \times \text{Deregulation}_{it} + \gamma \times \text{Deregulation}_{it} \times \text{FeeSimple}_i + \psi_j + \tau_t + \lambda' X_i + \varepsilon_{ijt}, \quad (2)$$

where y_{ij} is the outcome of interest on plot i in spatial region j in year t, ψ_j is a spatial fixed effect, and τ_t is a year fixed effect. $Deregulation_{it}$ = 1 for parcels on reservations where branch banking has been deregulated, and 0 otherwise.[31] The parameter θ measures the difference between fee-simple and allotted trust parcels without deregulation, δ measures the impact of deregulation on allotted trust parcels, and the DDD parameter γ measures the differential impact of deregulation

[30] The HMDA data give the census tract of loans, but not the precise geographic location. Hence, we cannot directly compare lending to fee-simple vs. allotted trust plots.

[31] We consider a state to be "deregulated" in the first year that it allows either intrastate or interstate branching. As a practical matter, intrastate state branching deregulation preceded interstate deregulation in most states. Hence, much of the literature has focused on the effects if intrastate deregulation, which is the primary source of variation behind our Deregulation_{it} variable.The literature has also emphasized that both intrastate and interstate branching should have similar effects on the supply of credit by allowing banks to diversify geographically and by increasing competition within previously isolated markets.

on fee-simple parcels. Columns 1 and 2 focus on developed land use, whereas 3 and 4 focus on agricultural land use. In columns 1 and 3, ψ_j are section fixed effects and VSelect covariates, as in column 2 of Table 2, whereas in columns 2 and 4 take advantage of the panel structure of the data so that ψ_j are parcel fixed effects. We cluster standard errors by parcel (to account for serial correlation) and state-year (the level of treatment variation).

Table 4: Banking Deregulation and Collaterlizeability

	Any Development		Share Cultivated	
	(1)	(2)	(3)	(4)
Fee Simple	0.009*		4.330***	
	(0.005)		(0.459)	
FeeSimple × Bank Dereg.	0.006**	0.005*	-0.233	-0.239
	(0.002)	(0.003)	(0.318)	(0.271)
Bank Deregulation	-0.001	-0.001	0.387	0.411
	(0.001)	(0.002)	(0.261)	(0.250)
Adj. R^2	0.5190	0.9840	0.8251	0.9844
Observations	457,170	457,200	457,170	457,200
FE_Type	Section	Parcel	Section	Parcel
#Spatial FEs	27,499	91,440	27,499	91,440
Covariates	VSelect	None	VSelect	None

Notes: The table reports results from estimating equation 2 for the two outcomes of interest. Columns 1 and 3 include section fixed effects, whereas columns 2 and 4 introduce plot fixed effects. Across columns, we cluster standard errors by parcel and state × year. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The results in Table 4 are consistent with the hypothesis that the collateral channel explains some of the differences in developed land use on fee-simple vs. allotted trust plots. The Fee Simple coefficient in column 1 indicates that fee-simple land is more developed prior to deregulation, suggesting that collateral is not the only mechanism behind our main results. As expected, the impact of deregulation on allotted trust plots is not statistically different from zero. Importantly, the impact of deregulation on fee-simple plots is positive, significant, and roughly 2/3 the size of the baseline difference in development between fee-simple and trust plots. The coefficients in column 2 reveal that these findings are robust to the inclusion of individual parcel fixed effects that absorb all time-invariant differences between parcels—including geographic variables and differences in familial lineages.

In contrast to the developed land use results, none of the coefficients on bank deregulation or its interaction with fee-simple land are statistically different from zero in columns 3 or 4. The baseline difference in cultivated land use on fee-simple land remains in column 3, but is absorbed by the parcel fixed effects in column 4. The upshot is that deregulation appears to have had no impact on cultivated land use for either allotted trust or fee-simple land. Next, we assess whether the fractionation channel can explain some of the differences in cultivated land use.

Fractionation Channel: To get at the fractionation channel, we must overcome the fact that we cannot measure fractionation directly because land ownership records are kept confidential by the BIA.[32] Instead, we construct two proxies for fractionation. First, we develop a measure of a plot's *latent* potential for fractionation using a newly digitized historical reservation census, the mid-1930s *Indian Census Rolls* (ICR), which contains allotment information. Specifically, we assume that allotments that we cannot find in the mid-1930s *Indian Census Rolls* (ICR) belonged to allottees that had already passed away by then, implying that those allotments were more likely to become highly fractionated over time because the process of fractionation started earlier and because earlier deaths were more likely to occur without a will, as discussed in Section 2. We validate this assumption in Appendix D.2, where we show that *(i)* sequential allotment numbers are highly correlated with age within a reservation and *(ii)* it is systematically the early allotment numbers that we cannot find in the ICR. (See Appendix-Figure A6.).

Let the indicator "D(not in ICR)$_i$" denote whether plot i's allotment number can be found in the mid-1930s ICR. This indicator is similarly distributed across fee-simple and allotted-trust lands: we match 47 percent of all fee-simple plots and 40 percent of all allotted-trust plots to the ICR. Given the evidence in Appendix-Figure A6, we interpret this indicator as a measure of latent or potential fractionation, which we use in the absence of observable plot-level measures of fractionation. We gain confidence in this interpretation from relating the indicator to reported two reservation-aggregate measures of fractionation. First, we digitized a 1935 land tenure report, which included information on the number and acreage associated with deceased allottees (Office of Indian Affairs, 1935). These acreage totals are highly correlated (0.944) with our own estimated deceased acreages based our latent fractionation. Second, we obtain from a 2013 BIA report the total acreage of tracts subject to fractionation on each reservation. Appendix-Figure A7 shows that the extent of modern fractionation correlates well with our plot-level measures of latent fractionation when they are aggregated to the reservation level.

While "D(not in ICR)$_i$" proxies for whether or not a plot was likely exposed to fractionation at

[32] These records are managed by the BIA through the so-called *Trust Asset Accounting Management System*.

the *extensive* margin, it does not capture any potential variation in the degree of fractionation at the *intensive* margin (e.g., the number of owners per plot). To proxy for the intensive margin of fractionation at the plot level, we use the year in which a plot was first allotted. On trust plots exposed to latent fractionation, the year of allotment should correlate strongly with fractionation because the original and successive owners of plots that were allotted earlier would have been more likely to die earlier, leading to worse fractionation through successive waves of inheritance. We normalize allotment year to the beginning of the allotment era so that Allot Year $= 0$ is associated with 1887.

We cannot directly verify our assumption about allotment year and fractionation at the *plot* level because it is not possible to obtain plot-level fractionation data. However, we can provide some evidence in support of this assumption at the *reservation* level using the 2013 BIA report mentioned above, which reports the average number of fractionated ownership interests per tract on each reservation in 2012. Appendix-Figure A8 demonstrates that mean allotment year on trust tracts exposed to fractionation is highly correlated with the average degree of fractionation at the reservation level: reservations with earlier mean allotment years have greater degrees of fractionation.

Our proxies for fractionation have the added benefit that they can be constructed for both trust and fee-simple plots. This is useful because the fractionation channel should only be present on trust plots, where the inability to write a will triggered the use of tenancy in common. If our proxies have an effect on trust land but not on fee-simple land, this provides support for our interpretation of these proxies as capturing essential features of fractionation, rather than some unobserved factor that could also affect fee-simple land. We test for the importance of the fractionation channel with two different specifications. In the first, we focus on the extensive margin by interactig both alloted-trust and fee-simple indicators with our latent fractionation indicator to test whether they mattered on allotted- trust parcels , but not on fee-simple land (where wills could be written and inheritance could be handled in the same way as on off-reservation land). In the second, we also include a plot's allotment year, as well as the interaction between allotment year and latent fractionation for both fee-simple and trust land.

We estimate modified versions of our preferred specification of equation (1) with section fixed effects using the 2012 NWALT cross-section:

$$\begin{aligned} y_{ij} = \theta \times \text{FeeSimple}_i + \quad & \theta^T_{\text{frac}} \times \text{Trust}_i \times \text{D(Latent Frac)}_i + \\ & \theta^F_{\text{frac}} \times \text{FeeSimple}_i \times \text{D(Latent Frac)}_i + \kappa_j + \lambda' X_i + \varepsilon_{ij} \end{aligned} \tag{3}$$

$$\begin{aligned} y_{ij} = \theta \times \text{FeeSimple}_i + \quad & \theta^T_{\text{frac}} \times \text{Trust}_i \times \text{D(Latent Frac)}_i + \\ & \theta^F_{\text{frac}} \times \text{FeeSimple}_i \times \text{D(Latent Frac)}_i + \\ & \delta^T_{\text{frac}} \times \text{Trust}_i \times \text{D(Latent Frac)}_i \times \text{Allot Year}_i + \\ & \delta^F_{\text{frac}} \times \text{FeeSimple}_i \times \text{D(Latent Frac)}_i \times \text{Allot Year}_i + \\ & \delta \text{Allot Year}_i + \kappa_j + \lambda' X_i + \varepsilon_{ij}, \end{aligned} \tag{4}$$

where our hypothesis is that in $\theta^T_{\text{frac}} < 0$, and $\theta^F_{\text{frac}} = 0$ in equations in (3) and (4), because latent fractionation is much more likely to cause actual fractionation on allotted-trust plots than on fee-simple plots for the reasons discussed at the end of Section 2. Furthermore, we hypothesize that in equation (4), $\delta^F_{\text{frac}} = 0$ (allotment year does not matter on fee-simple plots, whether or not they were exposed to latent fractionation) and $\delta^T_{\text{frac}} > 0$ (being allotted later reduces the negative effect of fractionation because less fractionation occurs).

Columns 1 and 3 In Table 5 report the results of estimating equations (3). We see that our hypotheses are borne out: when y_{ij} is a plot's share of land under agricultural cultivation; $\widehat{\theta^T_{\text{frac}}} < 0$, implying that allotted-trust parcels with higher latent fractionation see less agricultural cultivation than allotted-trust parcels with lower latent fractionation (the omitted category). Moreover, $\widehat{\theta^F_{\text{frac}}} = 0$ says that the difference in latent fractionation is not important for fee-simple plots, as predicted.

On the difference between outcomes in columns 1 and 3, $\widehat{\theta^T_{\text{frac}}} < 0$ is much more strongly borne out for agriculture than for development (although it is still sign-consistent but for development). This makes sense, as it is well understood that the fractionation problem is absolutely central for agriculture because it increases the transaction costs leasing agreements as well as agreement on the various recurring decisions involved in agricultural land use, e.g., crop choice, irrigation strategies, and fallowing rotations (Russ and Stratmann, 2014). In contrast, fractionation is of second-order importance for development because all allotted-trust plots lack access to the credit needed to finance development, regardless of how fractionated they are.

Table 5: The Fractionation Channel

	Any Development		Share Cultivated	
	(1)	(2)	(3)	(4)
Fee Simple	0.021**	0.019*	4.533***	4.019***
	(0.010)	(0.010)	(1.021)	(0.895)
Trust × D(Latent Frac)	-0.001	-0.008	-0.622*	-3.146**
	(0.004)	(0.011)	(0.350)	(1.274)
Fee Simple × D(Latent Frac)	-0.010	-0.020	0.349	-2.734
	(0.013)	(0.026)	(0.749)	(3.310)
Trust × D(Latent Frac) × Allot. Year		0.000		0.086**
		(0.000)		(0.035)
Fee Simple × D(Latent Frac) × Allot. Year		0.000		0.139
		(0.001)		(0.138)
Allotment Year		-0.001		-0.160
		(0.001)		(0.123)
Adj. R^2	0.3489	0.3489	0.7818	0.7819
Observations	66,020	66,020	66,020	66,020
Fixed Effects	Section	Section	Section	Section
#Fixed-Effects	16,969	16,969	16,969	16,969
Covariates	VSelect	VSelect	VSelect	VSelect

Notes: The table reports results from estimating equations 3 and 4 for the two outcomes of interest. The indicator variable D(Latent Frac) equals one if the allotment number associated with the plot could not be found in the mid-1930s ICR. In columns 2 and 4, the variable Allotment Year is recentered so parcels allotted in 1887 have Allot Year = 0. Across columns, standard errors are clustered at the reservation level and significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Turning to the more fine-grained estimation of equations (4), columns 2 and 4 show broadly similar results when we also include our second measure of latent fractionation that is based on age. Turning to the intensive margin of fractionation, column 4 provides further support for our hypotheses. The negative effect of latent fractionation on trust plots remains, but that effect is attenuated for plots that are allotted later. A the negative effect of latent fractionaton declines by 0.086 for each year later that a plot was allotted: the effect of fractionation is much larger for a plot allotted in 1887 (Allot Year = 0) than for one allotted in say, 1907 (Allot Year = 20). As with the extensive margin, column (2) suggests that the intensive margin of fractionation is less consequential for development.

7 Conclusion

This paper estimates the long-run cost of non-transferable property rights, comparing land without transfer rights to land with full property rights on Native American reservations from 1974 to today. We leverage a natural experiment in the allocation of property rights to individual households in the early part of the 20th century that left a patchwork of different land tenures on reservations which persists to the present day. We find that the probability of developed land use is about 1.4 percentage points higher on fee-simple land than on non-transferable trust land. Similarly, we find that the share of land cultivated is about 4percentage points higher on fee-simple land than on trust land. We provide evidence that transfer restrictions affect land use through *at least* two channels: reduced access to credit and problems associated with competing ownership claims. External credit conditions accentuate the difference between allotted-trust and fee-simple plots, and this difference primarily affects development. We also find that proxies for ownership-fractionation inhibit agricultural land use on allotted-trust plots (where it cannot be resolved) but not on fee-simple plots (where it can).

It is important to be careful when considering the implications of these findings. Our results suggest that converting allotted-trust land to full fee-simple individual property rights would generate significant economic gains. However, the alternative—returning allotted-trust land to tribal control—could also deliver some efficiency gains by freeing land from individual credit constraints and by reducing fraction. There is evidence that tribal land outperforms individual trust land from previous case studies (e.g., Leonard and Parker 2021). Returning land to tribal control may also better safeguard the territorial integrity of tribes' land base that converting allotted-trust lands to fee simple. This creates tradeoffs.

From a practical standpoint, there is a workable precedent for conversion to tribal control because it is already happening on some reservations: under the 2014 'Cobell settlement', the Department of Interior (DOI) has been allocated 1.9 billion dollars to buy fractionated allotted-trust claims and return them to tribal control, in close consultation with tribes.

In contrast, conversion to fee simple is legally challenging under the 1934 IRA and there remains the practical difficulty of untangling the potentially hundreds of claims on some plots. Fortunately, there is a related legal precedent that is paving the way for changing this: the *Uniform Law*

Commission's *Uniform Partition of Heirs Property Act* (UPHPA) has recently been enacted into law in 14 states for the purpose of untangling fractionated claims on heir's property (Mitchell, 2019). Given the similarities between heir's property and allotted-trust land discussed in Section 3, legal statutes modeled on the UPHPA could be applied to untangling claims on reservations, and the ULC is actively working on a uniform Indian probate code to apply to reservations.

Lastly, it is worth noting that any movement away from allotted-trust land need not be a binary choice. One can imagine giving owners of trust land fully transferable property rights (thus maximizing the value from these lands) but leaving it to tribes to decide whether this transferability should extend only within the tribe or beyond. Mexico's second land reform (*Procede*) offers a useful template in this regard: from 1993–2006, indigenous farmers were given full title to the land that they had long held usufruct rights to, but it was the communities *ejidos* who then decided whether these rights would be transferable only within the ejido or whether land could also be transferred to non-ejidatarios (De Janvry et al., 2015). We see such a solution as eminently workable on American Indian reservations.

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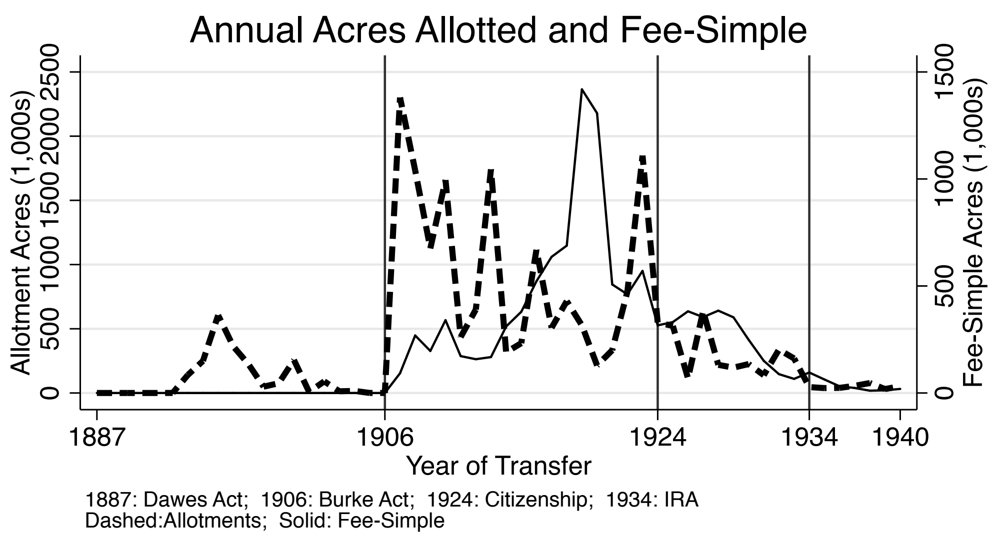
Appendix A Appendix to Section 2

Figure A1 is reproduced from Dippel, Frye, and Leonard (2022) and tracks the flow of total acres that were allotted and the flow of acres subsequently converted into fee simple in the BLM data; discussed in Section 2.

Figure A1: Flow of Allotments and Transfers into Fee Simple



Notes: This figure is reproduced from Dippel et al. (2022) and tracks the flow of total acres that were allotted and the flow of acres subsequently transferred into fee simple in the BLM data.

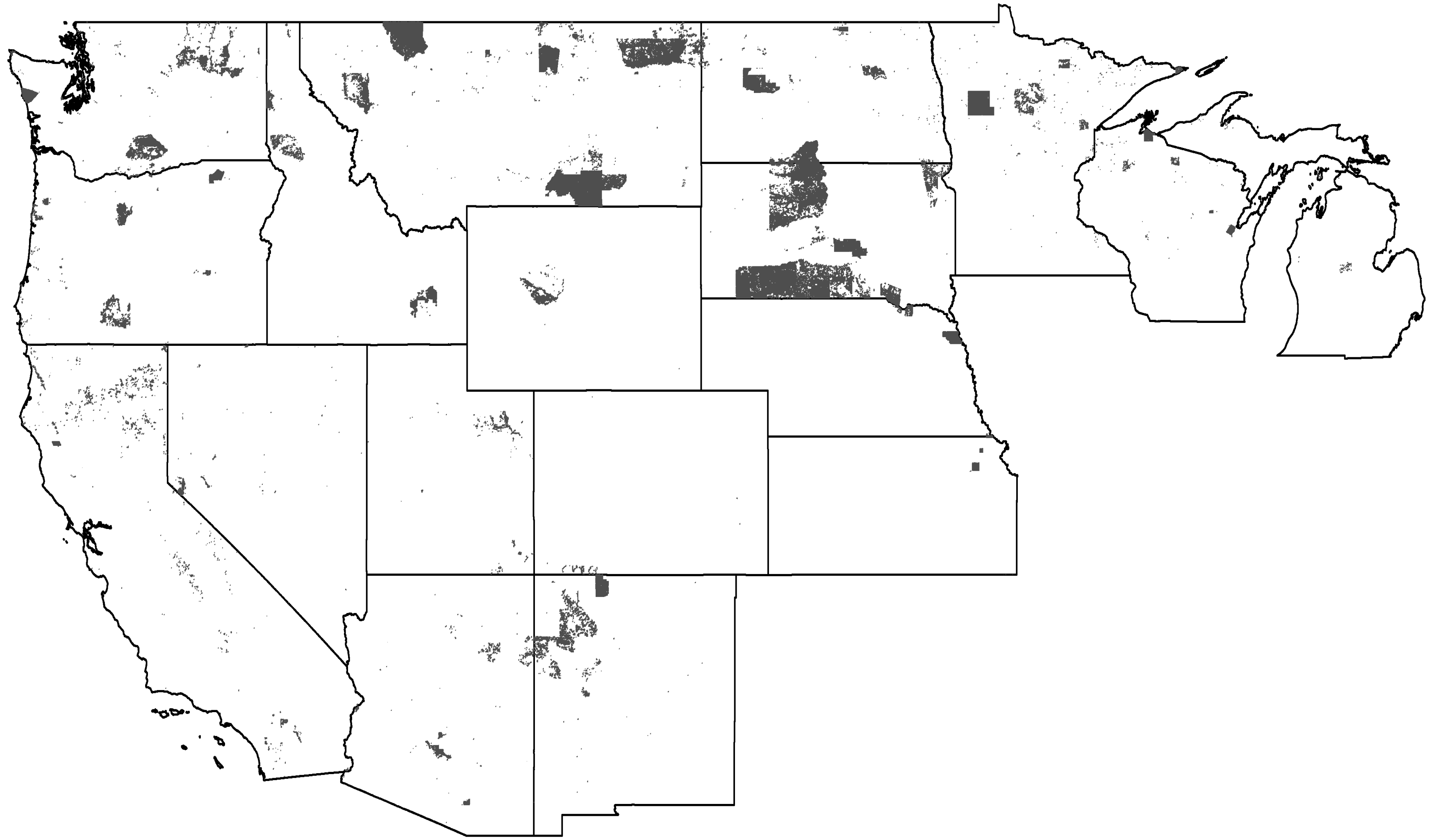
Appendix B Appendix to Section 4

Figure A2 depicts the location of allotments matched to quarter sections. In most cases, these clusters of allotments trace out the boundaries of present-day reservations (with the gaps filled in mostly by tribal lands). In some rare cases, clusters of allotments trace out the boundaries of a former reservation that was later terminated. This is true, for example, of the more dispersed looking 'clouds' of allotments in Central and Northern California. Oklahoma, which is in fact densely covered by allotments, is the only gap in our spatial allotment data.[33] Eastern Oklahoma was covered by reservations for the 'Five Civilized Tribes' (the Cherokee, Chickasaw, Choctaw, Creek, and Seminole) who had been relocated there in the 1830s. These tribes were fully allotted under an alternative allotment agreement however, these allotments were not filed with the General Land Office because the land was already owned in fee-simple by the tribes at the time of allotment. We exclude the Western Oklahoma and the Osage reservation because allotments outside of the Five Civilized Tribes were not consistently categorized as 'Indian Patents' by the BLM.

Figure A3 shows a version of Figure 1 where we separately identify surplus land inside the reservation. (The vast majority of surplus lands lies outside of reservations, because it was ceded from reservations as large tracts.) The larger black outlines are the boundaries of 6×6-mile PLSS townships.

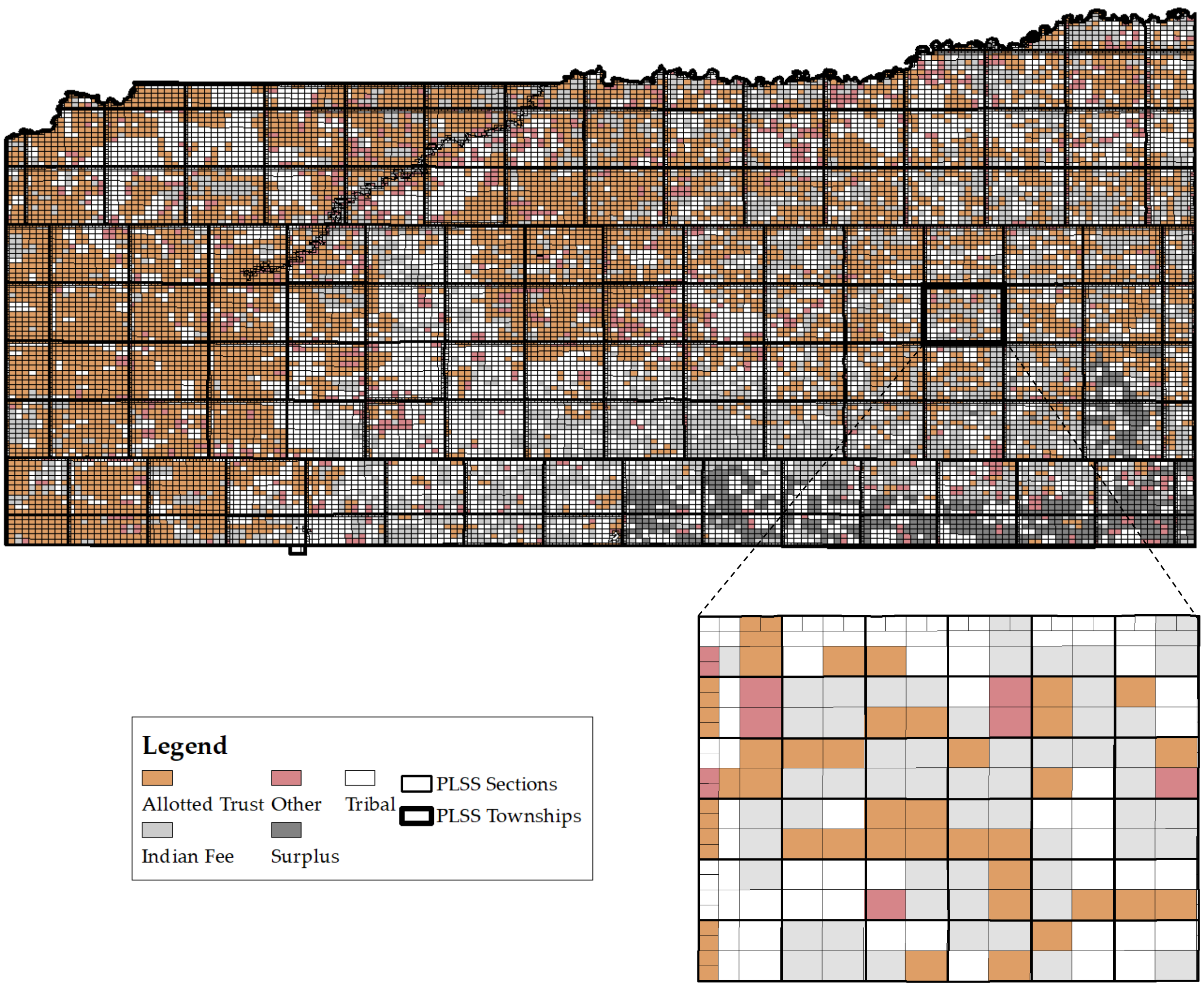
[33] Our match rate is above 99% for most states, with notably lower match rates for New Mexico (where the PLSS grid is less cleanly defined) and Wisconsin.

Figure A2: Allotted Quarter Sections



Notes: This figure depicts the location of allotments across the U.S. The main omission is Oklahoma, where the Five Civilized Tribes (and the Osage) were allotted, but their allotments where not included in the GLO data. The parcels depicted include land in allotted-trust as well as fee-simple lands.

Figure A3: Checkerboard Pattern of Land Tenure on the Pine Ridge Reservation



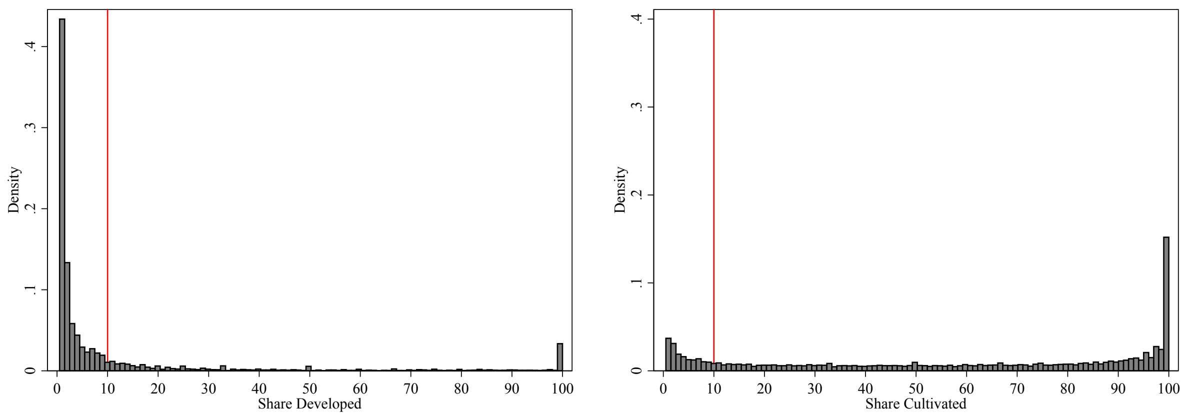
Notes: Distribution of Land tenure on the Pine Ridge reservation by allotment parcel (quarter-section) in the GLO data. Overlaying the reservation is a township grid. Each township is 36 square miles and contained in each are 144 (= 36×4) quarter sections, each of which is 160 acres (one-quarter of a square mile) large.

Appendix C Appendix to Section 5

Figure A4: Distribution of Land Use by Major Type

(a) Developed Land Use

(b) Cultivated Land Use



Notes: This figured depicts the histograms of the share of developed land use (panel a) and cultivated land use (panel b) across plots in our data. The red line in each figure indicates 10% of pixels devoted to a given land use.

Table A1: Land Use Differences in 2012

	(1)	(2)	(3)	(4)	(5)
			Difference: Fee - Trust		
	Trust	Fee	No FE	Res FE	Sect FE
Any Development	0.106	0.202	0.096***	0.077***	0.020***
	(0.308)	(0.402)	[0.018]	[0.014]	[0.007]
Share Cultivated	11.153	31.471	20.318***	15.134***	4.600***
	(27.562)	(39.426)	[2.521]	[2.486]	[1.052]
Share Developed	1.038	2.641	1.603	0.776**	0.231
	(7.360)	(12.796)	[1.090]	[0.365]	[0.149]
Any Cultivation	0.206	0.515	0.309***	0.238***	0.064***
	(0.405)	(0.500)	[0.029]	[0.032]	[0.016]
Observations	65,228	26,212	91,440	91,433	85,491

Notes: This table reports differences in land use by tenure type. Columns 1–2 present mean and standard deviations by land tenure. Column 3 reports unconditional differences, column 4 reports differences conditional on reservations fixed effects, column 5 reports differences conditional on section fixed effects. Standard errors are clustered at the reservation level and reported in brackets. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

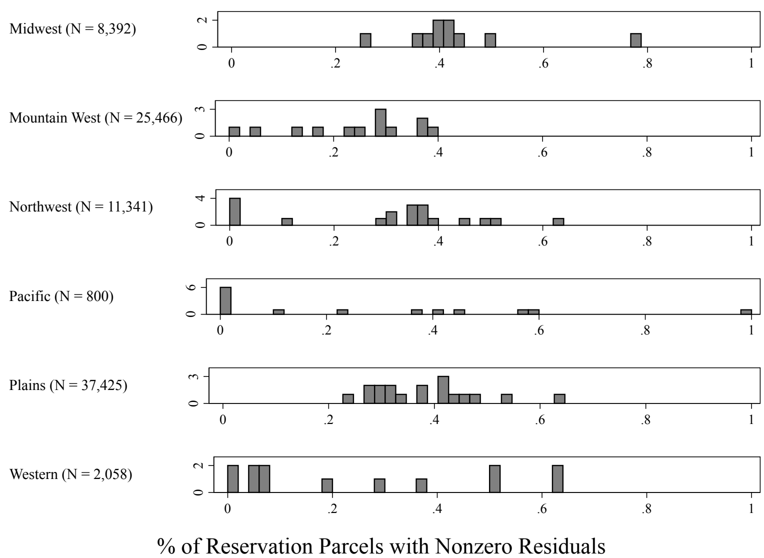
Table A2: Addressing Spillovers Between Tenure Types

	Any Development		Share Cultivated	
	(1)	(2)	(3)	(4)
Panel A: Spillovers from Fee Simple to Trust Plots				
Number Fee Neighbors	0.0006		0.3113**	
	(0.0017)		(0.1480)	
Pct. Fee Acres		0.0002		0.0332*
		(0.0002)		(0.0180)
Adj. R^2	0.3831	0.3831	0.7801	0.7800
Observations	59,149	59,149	59,149	59,149
# Spatial FEs	15,780	15,780	15,780	15,780
Panel B: Spillovers and Main Effects				
FeeSimple	0.0144**	0.0172**	4.4576***	4.6937***
	(0.0071)	(0.0071)	(1.1181)	(1.1058)
Number Fee Neighbors	0.0001		0.0888	
	(0.0019)		(0.1956)	
Pct. Fee Acres		0.0004**		0.0421***
		(0.0002)		(0.0133)
Adj. R^2	0.3594	0.3595	0.7717	0.7718
Observations	85,486	85,486	85,486	85,486
# Spatial FEs	21,553	21,553	21,553	21,553

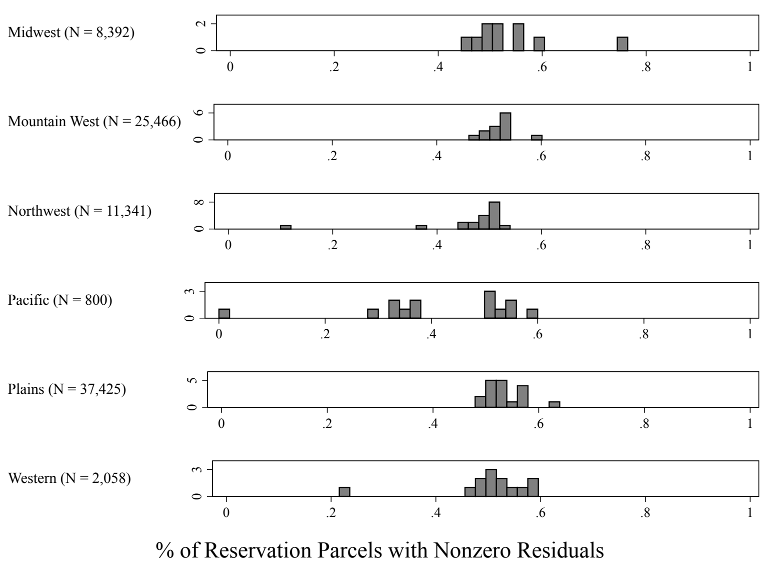
Notes: Panel A reports estimation results from a modified equation (1), where the regressor of interest measures the intensity of fee simple exposure within one mile of each trust plot. The regression estimates the effect of neighboring fee simple exposure on land use on *only* trust plots. Columns 1–2 estimate spillovers in development and columns 3–4 estimate spillovers in the share of cultivation. Odd numbered columns measure neighboring fee simple exposure through the *number* of neighboring fee simple plots, whereas even number columns measure neighboring fee simple exposure through the *share* of fee simple acreage among neighboring plots. Panel B re-estimates equation (1), introducing the two previously defined measures of neighboring fee simple exposure. The estimation sample in Panel B includes all fee simple parcels and trust parcels. The estimation results in both panels include section fixed effects, PLSS feature type fixed effects, and the full set of controls from the variable selection model. Across all columns, we cluster standard errors at the reservation level. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A5: Share of Plots with Residual Variation in FeeSimple, by Reservation

(a) Fixed Effects Only



(b) Fixed Effects & Vselect Controls



Notes: This figure depicts reservation-level frequency plots for the share of plots not explained by the fixed effects (and controls) on each reservation to give a sense of the spatial coverage of the within-section identifying variation in FeeSimple in our data. Regions correspond to BIA Regions, except that we combine the Great Plains and Southern Plains BIA Regions into "Plains", and the Navajo, Rocky Mountain, and Southwest BIA Regions into "Mountain West'.

Table A3: Transfer Restrictions and Agricultural Land Use

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: log Net Primary Production (NPP)						
FeeSimple	0.047***	0.046***	0.045***	0.044***	0.046***	0.043***
	(0.011)	(0.011)	(0.009)	(0.009)	(0.009)	(0.009)
Adj. R^2	0.8890	0.8893	.	.	.	.
Observations	84,444	84,444	90,400	90,400	90,400	90,400
Panel B: Crop Choice Similarity						
FeeSimple	0.076***	0.071***	0.063***	0.064***	0.065***	0.062***
	(0.017)	(0.017)	(0.016)	(0.015)	(0.015)	(0.016)
Adj. R^2	0.6024	0.6060	.	.	.	.
Observations	84,945	84,945	90,844	90,844	90,844	90,844
FE Type	Section	Section	Section	Section	Section	Section
Method	OLS	OLS	PSM	PSM	MD	MD
Neighbors	NA	NA	Any	1	Any	1
Match Vars	NA	NA	VSelect	VSelect	VSelect	VSelect
Covariates	None	VSelect	None	None	None	None

Notes: Both panels of this table report on six variations of estimating equation 1, for two separate outcomes. Every specification includes section fixed effects and feature-type fixed effects. Column 1 estimates OLS with only spatial fixed effects, and column 2 adds the controls selected by the variable selection model in Table 1. In columns 3 – 6, we run a series of matching estimators. Column 3 runs a propensity-score matching estimator that matches on all controls selected by the variable selection model in Table 1. The remaining columns report on additional matching-estimator variations, first broadening the set of allowed matches that satisfy the common support restriction (col 4), and then using multi-dimensional matching through Mahalanobis distance matching on the same set of variables in columns 5 and 6 (Rosenbaum and Rubin, 1985). Across columns, we cluster standard errors at the reservation level. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3 compares additional agricultural outcomes across fee-simple vs. trust plots using the same specifications as Table 2. In Panel A, the dependent variable is the natural log of net primary production (NPP) in 2012, measured at a 30m resolution and obtained from Robinson, Allred, Smith, Jones, Moreno, Erickson, Naugle, and Running (2018). NPP a satellite derived-measure of vegetative productivity that is often used as a proxy for yields in agricultural contexts where yield data are unavailable. The results in Panel A indicate that NPP is roughly 4% higher on fee-simple land. In Panel B, we use 30m data on crop choice in 2012 from CropScape (Boryan, Yang, Mueller, and Craig, 2011) to assess crop choice. Given the broad geographic coverage of our sample, the "best" crops are likely to vary by region, making a direct comparison of crop choice difficult. To derive a single that can be interpreted across the diverse geographies in our sample, we construct a comparison to nearby off-reservation crop choice. To do this, we identify the top 3 crops being grown within 10 miles of each reservation. This variable varies by reservation (e.g., for Pine Ridge the top crops are hay, wheat, and alfalfa, but for the Colorado River Reservation in Arizona they are alfalfa, cotton, and citrus). We then construct an indicator variable that is equal to one if a plot is growing one of the top 3 crops associated with nearby off-reservation lands. The results of using this indicator as a dependent variable are shown in Panel B of Table A3, indicating that fee-simple plots are about 6-7 percentage points more likely to grow crops similar to the top off reservation crops. The mean of the dependent variable is 0.39, implying a 17% increase in the probability of crop similarity. The results are quite similar if we focus on the top 1 or 5 crops instead of the top 3.

Appendix D Appendix Materials for Section 6 (Mechanisms)

Appendix D.1 Banking and Credit Access

This section provides additional evidence that branching deregulation increased the supply of credit on reservations over our sample period. First, we construct two measures of bank access, the number of banks within each reservation and the number of banks outside each reservation, but within 10 miles of each reservation boundary. We obtain data on the opening dates and precise locations of all commercial banks from the *Federal Deposit Insurance Company* (FDIC), to determine which banks opened within a reservation or outside a reservation, and when. Our resulting variables, $\textit{Banks Within Reservation}_{it}$ and $\textit{Banks 10 mi Outside of Res}_{it}$, provide counts for the number of banks associated with each reservation i in year t, where the years correspond to the available NWALT years.

Our first empirical exercise verifies that reservation bank access increased following deregulation by estimating the following equation:

$$\textit{Banks Within Reservation}_{it} = \beta \times \textit{Bank Deregulation}_{it} + \tau_t + \rho_i + \varepsilon_{it}, \tag{5}$$

where $\textit{Banks Within Reservation}_{it}$ is the total number of FDIC banks within reservation i's boundaries in each NWALT year t. The specification includes reservation fixed effects, ρ_i, and NWALT year fixed effects, τ_t. Standard errors are clustered at the State $\times$ Year level, which matches the implementation of banking deregulation. Table A4 reports results from four alternative specifications. Column (1) reports OLS estimating results for the full sample of 515 reservations from our complete parcel-level dataset. Results in column (2) use the same sample, but apply a Poisson estimator.[34] Columns (3) and (4) replicate columns (1) and (2), but trim the sample to better reflect reservations that are more represented in our parcel estimating sample.[35] The results are generally

[34]Changes to the number of observations, reservations, and the pct. of parcels from our parcel estimating sample are the results of excluding reservations that remain unbanked over time.

[35]We exclude reservations with fewer than 700 parcels, which removes several reservations but only 10 percent of overall parcels.

consistent with bank deregulation increasing the number of banks within reservations over time.

Table A4: Bank Access Within Reservations Following Deregulation

	Full Reservation Sample		Res. with at least 700 parcels	
	(1)	(2)	(3)	(4)
	OLS	Poisson	OLS	Poisson
Banking Deregulation	0.779*	0.202	0.674***	0.445***
	(0.398)	(0.234)	(0.192)	(0.140)
R^2	0.554	0.660	0.717	0.347
Observations	515	175	135	90
Reservations	103	35	27	18
Pct. Parcels Included	100	62.15	90.84	57.5

Notes: The table reports results from estimation equation 5 using OLS (cols 1 and 3) and Poisson (cols 2 and 4). The outcome of interest is the number of FDIC banks within a reservation i in NWALT year t. Across columns, we cluster standard errors at the state × year level, matching the implementation of banking deregulation. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Increased bank availability and subsequent increased competition are related potential channels through which deregulation could have expanded credit access. Unfortunately, we cannot measure the availability of credit over time, but we can explore mortgage lending in 2012 through the Home Mortgage Disclosure Act (HMDA) database and evaluate whether increased bank access is associated with more lending activity.

The HMDA database includes transaction level information regarding home loan applications and originations, which are reported by several government agencies and reflect lending activity from conventional lenders and independent mortgage companies. We include all home purchase transactions from both FDIC banks and all other lenders to capture any alternative lending sources that exist on reservations. We aggregate the transactions to the Census Tract, which is the smallest available geography, and focus on two measures of lending activity: Total Loan Value and Total Loans. Census Tract boundaries do not align with reservation boundaries, therefore we assign a Census Tract to a reservation if at least 90 percent of the tract is within a reservation's boundaries. This significantly reduces the volume of off-reservation transactions that are incorrectly assigned within a reservation, but reduces the number of reservations represented in our sample. Finally, we combine these selected tracts to the reservation level, resulting in a cross-sectional dataset of 45 reservations with positive HMDA credit activity in 2012, which we pair with our measures of bank access both within reservations and slightly outside of reservation in the same year.

We estimate the relationship between home mortgage lending activity and bank access using the following equation:

$$y_i = \beta_0 + \beta_1 Bank\ Access\ Within_i + \beta_2 Bank\ Access\ Outside_i + X_i'\theta + \varepsilon_i, \quad (6)$$

where y_i is the HMDA measure of lending activity in reservation i in 2012. Our two banking access measures of interest are *Bank Access Within*$_i$ and *Bank Access Outside*$_i$, which reflect the number of banks within reservation i and the number of banks outside reservation i, but within 10 miles of the reservation border. Additionally, the model includes several reservation level covariates that are associated with differences in economic performance across reservations.[36]

Table A5 reports results from estimating modified specifications of equation (6). We separately evaluate HMDA home loans that were issued in Panel A and HMDA home loan applications in Panel B. Within the panels, the outcome of interest vary between the first block of four columns, Ln(Total Loan Value), and the second block of four columns, Ln(Total Loans). Within each block, the first two columns only include our measure of bank access *within* the reservation, whereas the third and fourth columns include both measures of bank exposure. Odd numbered columns do not include covariates.

Among originated loans in Panel A, the results indicate that more bank availability is associated with both a larger number of loans issued and a higher total value of loans issued. The inclusion of adjacent banks does not impact this relationship, which suggests that local FDIC banks on reservations are a more important source of credit than off-reservation banks. Including a rich set of reservation level controls does not impact these findings. These patterns do not hold in Panel B, which considers HMDA loan applications. Once we condition on our covariates, we no longer find significant differences in loan applications among reservations with more bank access. Taken together, these findings suggest that increased bank availability significantly improves access to

[36]The complete list includes measures of reservation income, population, educational attainment, inequality, gaming, and mining activity, as well as distance to nearest metropolitan area, adjacent county income, and population density all from 2010. Additionally, we include longer-term institutional controls like reservation acreage, the present land tenure mix, the adoption of the Indian Reorganization Act, and the imposition of Public Law 280. These covariates are central elements in a literature that includes Anderson and Parker (2008); Frye and Parker (2021a,b); Leonard et al. (2020).

credit, but not demand for credit.

Table A5: Bank Access and HMDA Credit Use on Reservations

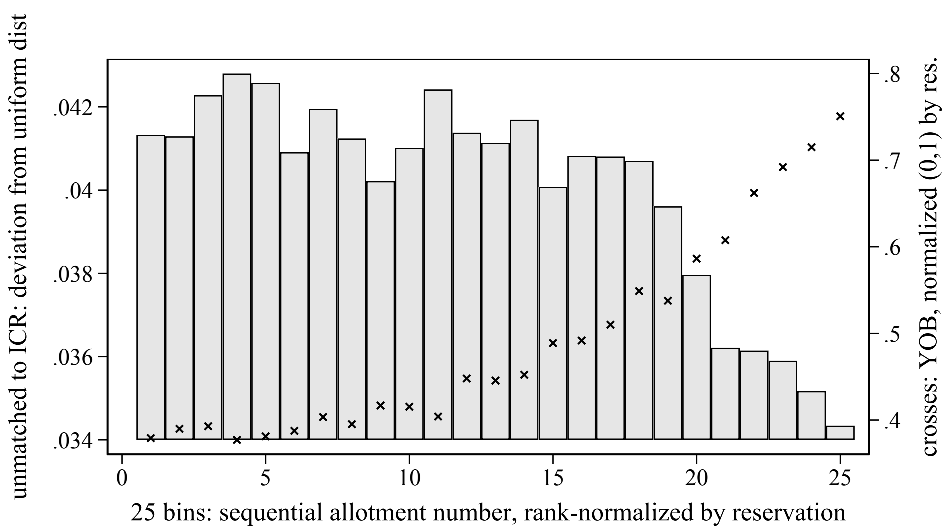
		Ln(Loan Value)				Ln(Total Loans)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: HMDA Origination Panel								
Banks Within Reservation	0.173***	0.196***	0.171***	0.196***	0.139***	0.158***	0.139***	0.158***
	(0.057)	(0.058)	(0.058)	(0.061)	(0.043)	(0.041)	(0.044)	(0.042)
Banks 10 mi Outside of Res.			-0.001	-0.005			0.000	-0.002
			(0.004)	(0.005)			(0.002)	(0.003)
Adj. R^2	0.078	0.577	0.058	0.574	0.109	0.576	0.088	0.565
Observations	45	45	45	45	45	45	45	45
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
Panel B: HMDA Application Panel								
Banks Within Reservation	0.100**	0.107	0.103**	0.107	0.076**	0.055	0.075**	0.054
	(0.042)	(0.065)	(0.041)	(0.067)	(0.033)	(0.049)	(0.033)	(0.049)
Banks 10 mi Outside of Res.			0.001	-0.003			-0.000	-0.005
			(0.003)	(0.004)			(0.002)	(0.004)
Adj. R^2	0.035	0.341	0.018	0.329	0.032	0.172	0.009	0.180
Observations	45	45	45	45	45	45	45	45
Covariates	No	Yes	No	Yes	No	Yes	No	Yes

Notes: The table reports results from estimation equation 6 across two different measures of lending activity within reservations from the HMDA. The banking measures are the number of FDIC banks within and adjacent to reservations in 2012. Robust standard errors are reported in parentheses and significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix D.2 Measuring Latent Fractionation

Our measure of latent fractionation rests on the assumption that allotments *not* matched to the ICR correspond to individuals who are decreased by 1930.[37] We can validate this assumption by leveraging the fact that allotment numbers were issued *sequentially*, which allows us to show that, *within* a reservation, smaller allotment numbers belonged to older allottees and were associated with a higher likelihood of not being recorded in the mid-1930s ICR. Figure A6 bins each reservation's rank-normalized allotment numbers into 25 bins on the horizontal axis and plots normalized birth-year by bin to show that smaller allotment numbers were associated with earlier birth-years for the allotments that we *do* match to the ICR.[38] The figure also plots the distribution of unmatched allotments to illustrate that it is skewed towards low allotment numbers, relative to a distribution of *all* allotments that is uniform by definition (because it splits the data into equal-sized bins). This is evidence that allotments that we do not find in the ICR disproportionately belonged to older individuals who where more likely to be deceased by the mid-1930s.

Figure A6: Original Allottees' Age and Sequential Allotment Number

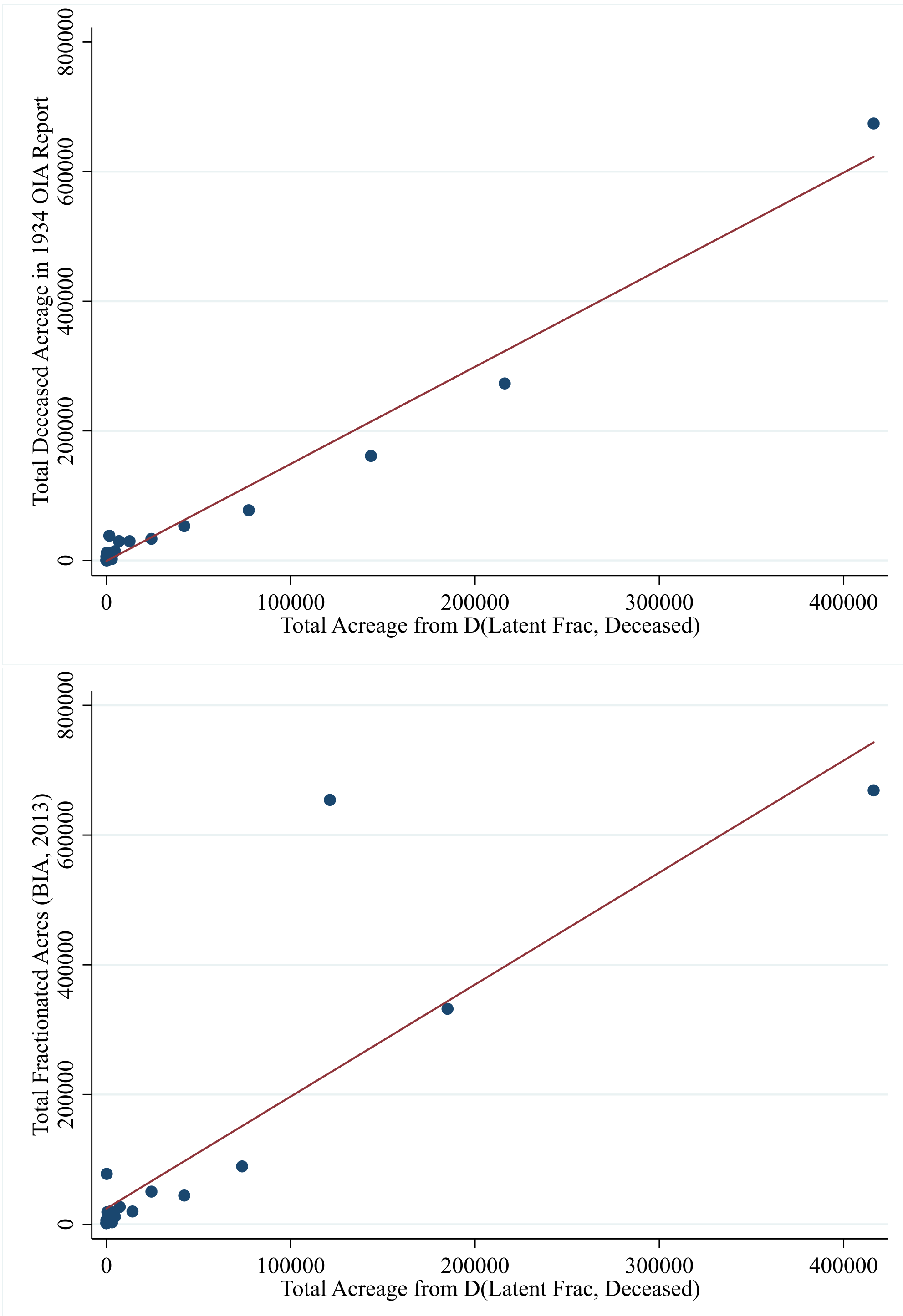


Notes: This figure shows that, *within* each reservation, smaller allotment numbers belonged to older allottees (see scatter plot) and were associated with a higher likelihood of not being recorded in the mid-1930s ICR.

[37] When linking between the ICR and BLM recorded allotment numbers we were careful to not overstate deaths due to misreporting of allotment numbers in the BLM data. We excluded any reservation where we were concerned that the number of unmatched BLM patents was too large relative to the number of unmatched ICR patents.

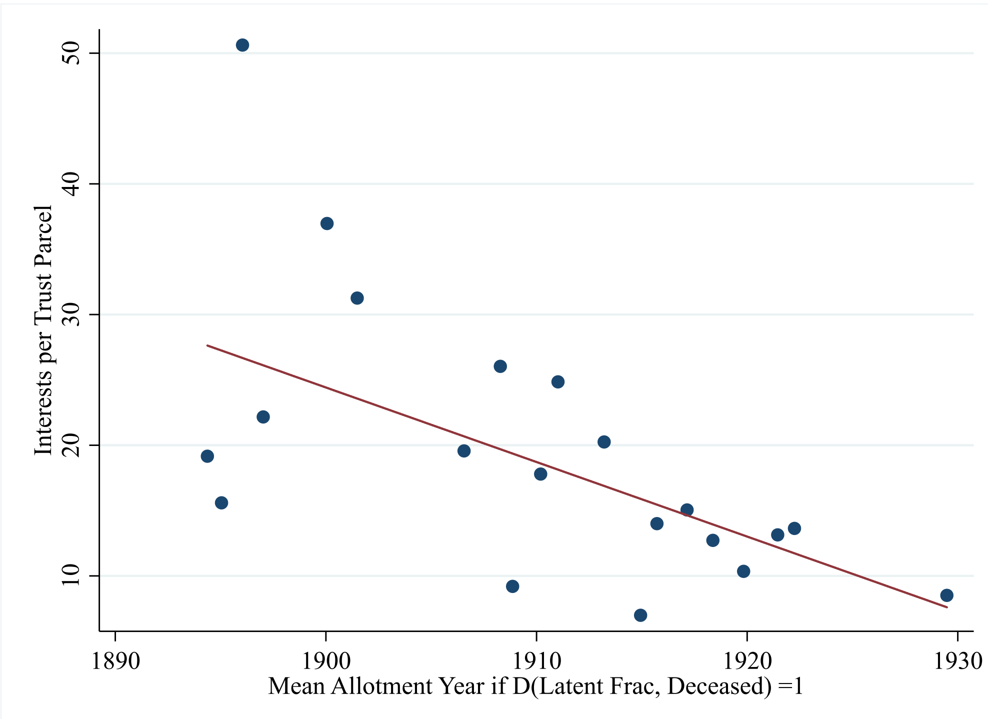
[38] Normalization (0–1) by reservation is needed because some reservations were allotted decades before others.

Figure A7: Relating Reservation Reported Fractionation to Latent Fractionation



Notes: The top graph plots the relationship between latent fractionation and the Office of Indian Affairs (1935) reported Deceased Acreage held in trust in 1935. The estimated slope coefficient ≈ 1.08 and is statistically indistinguishable from 1, supporting the relationship between our measure of latent fractionation and the true level of deceased fractionation. The bottom graph plots the relationship between latent fractionation and the acreage classified as highly fractionated in Department of Interior (2013). The estimated slope coefficient ≈ 1.03 and is statistically indistinguishable from 1. This supports that our measure of latent fractionation continues to reflect current levels of fractionation.

Figure A8: Relating Allotment Year to Fractionation Intensity



Notes: The figure plots the relationship between allotment year and fractionation at the reservation level. Mean allotment year is based on the BLM plot level data for only those parcels that we classify as fractionated. The number of ownership interests per trust parcel is reported in Department of Interior (2013). The negative relationship is consistent with our intuition that later allotted reservations have less fractionation.