The Effect of Land Allotment on Native American Households During the Assimilation Era*

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Abstract

In the early twentieth century, the U.S. government broke up millions of acres of communally owned reservation lands and allotted them to individual Native American households. Households initially received land allotments with limited property rights ('in trust'), and were incentivized to prove themselves "competent" in order to obtain full legal title ('fee simple') after a set period. Indian allotment thus had elements of a conditional transfer program aimed at assimilation. The policy was ended suddenly in 1934, locking in-trust land into its status in perpetuity. We link land allotment information to the universe of Native American households in the 1940 U.S. Census. We exploit quasi-random variation in being allotted as well as in securing the allotment in fee simple. Obtaining an allotment significantly increased the likelihood of living on a farm but not of working as a farmer, indicating that allottees leased out their land. Allotments also impacted wages and occupational rank. Surprisingly, allotment most significantly impacted educational attainment. We interpret education as a way of signalling "competency" to BIA agents. Obtaining the land in fee simple was associated with decreased likelihood of living on a farm and owning one's home, evidence that many allottees sold their land once they were deemed competent and obtained title. The fee-simple effects were more pronounced within tribes whose ancestral tribal norms emphasized private over communal property, indicating a cultural determinant in how the wealth transfer was utilized. Consistent with this, households in tribes with traditions of private property also engaged in more signalling of their assimilation.

Keywords: Indigenous Economic Development, Culture, Assimilation, Property Rights, Conditional Transfer Programs, Record Linkage

JEL Codes: N10, O1, Z1

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> "Each Indian who was to receive a patent [in fee] and citizenship stepped from a tepee and shot an arrow to signify that he was leaving behind his Indian way of life. He placed his hands on a plow to show that he had chosen to live the farming life of a white man, with sweat and hard work. The secretary of the interior then handed the Indian a purse as a reminder that he must save what he earned." — McDonnell (1980, p26), describing allotments in 1916 at the Yankton Reservation

1 Introduction

Toward the end of the 19th century, with the end of the Indian Wars and the closing of the frontier, the U.S. government turned its attention towards the cultural assimilation of Native Americans, the overwhelming majority of whom were living on the reservations created during the previous half-century. Assimilation efforts were centered on policies that broke tribally owned reservation lands into individually owned land allotments. Bureau of Indian Affairs (BIA) Commissioner Thomas Morgan noted in his annual report of 1891 that "if there were no other reason [for allotment], the fact that individual ownership of property is the universal custom among civilized people of this country would be a sufficient reason for urging the handful of Indians to adopt it." Indian allotment was the cornerstone of federal Indian policy from the passing of the General Allotment Act (or 'Dawes Act') in 1887 to the passing of the Indian Reorganization Act (the IRA, or 'Howard-Wheeler Act') in 1934 (Carlson, 1981, p18).[1]

Indian allotments were first placed in a trust managed by the BIA's local superintendents in charge of reservations (the 'Indian Agents'). In-trust status limited allottees' title to the land and did not grant the full property rights needed to sell or collateralize an allotment. Following a time-window of being held in trust, the local BIA agent could declare allottees "competent," upon which they were eligible to convert their land into fee-simple, and coupled with this at the same time became citizens (Banner, 2009; Otis, 2014). It soon became clear that many allottees chose to sell their land upon obtaining full legal title, resulting in worries about the erosion of the remaining Indian land base, (Carlson, 1981, p13). By the late 1920s, concerns also increased that the system was liable to abuse and that it failed to improve the economic position of Indians. These

[1] While allotment had been practiced in some places even before 1887, our new data show that it really began in earnest with the passing of the Burke Act in 1906, which explicitly linked landownership to citizenship.

concerns were summarized in an influential 1928 report titled 'The Problem of Indian Administration' (Meriam, 1928). This report led to a fundamental re-think of federal Indian policy, and in 1934, the IRA abruptly ended the issuance of new allotments as well as the transferring of land into fee simple. Allotted lands that had not been transferred into fee simple by 1934 effectively became frozen into its trust status.[2]

The universe of Indian allotments has been digitized by the *Bureau of Land Management* (BLM). These data include for all allotments their date of issuance, their exact geo-location and the date when (if ever) they were transferred into fee simple. Wealth, income, labor market status and education of Native American individuals and households can be observed in the *Full Count Population Census*, with over 300,000 Native Americans in 1940. There is no information on allotments in the Population Census, and name data in the BLM data is so poorly recorded that it is impossible to link it to the Population Census directly. Fortunately, the BIA collected its own complete census of all Native Americans in the late 1920s and early 1930s, the so-called *Indian Census Rolls* (ICR). The ICR has been published in books which have been scanned and from these scans we have constructed a separate dataset that is a near-complete separate census of Indians on reservations.[3] Unlike the Population Census, the ICR did not record socio-economic outcomes, and were intended only to give a complete listing of people. They contain names, ages, household relations, blood quantum, and—critically—allotment numbers. We can directly link individual records in the ICR to the BLM data by allotment number. Linking these treatment data to the outcome data in the Population Census is more difficult, requiring us to move beyond standard linkage approaches because the main linkage variables (names and birth years) are recorded with higher-than-average noise in all Native American records at this time.[4]

We therefore develop a new record linkage algorithm that emphasizes the similarity of *households*, adjusting individual record similarity scores up or down to account for the similarity of individuals' households across data sets. After running this code, we identify questionable links based on a number of criteria (for example, if a husband and wife in a Census household are a

[2] Transfers into fee simple became a bit more common again after 1950, but no new allotments were issued after 1934.

[3] At this time, the universe of Native Americans on reservations is almost the entire universe of Native Americans.

[4] For one, census enumerators made frequent errors in spelling Indian names. For another, Native Americans were frequently anglicizing their names or re-arranging first and last names in this period. (For example, one individual called 'Little Running Bear' in 1930 had become 'Jim Bear' in 1940.) Stated birth years also varied considerably from data source to data source, especially for older people.

husband and wife in two different households in the ICR), and manually check several thousand thus flagged households, hard-coding 'true positives' and 'true negatives'. These hard-coded links then constrain the data when the linkage algorithm is run another time. Previous to the manual step, we link roughly 7,000 Native American households from the Census to the ICR.[5]

Allotment as a policy was effectively a package of two treatments, an unconditional and a conditional transfer program: The first treatment, receiving an allotment, gave the allottee the unconditional right to use the land for their own purposes, as well as the right to leasing rents. Annual proceeds from leasing out an allotment averaged between 150 and 300 dollars in our data, relative to an average per capita income between 100 and 240 dollars.[6] To be eligible to receive the second treatment—obtaining the land in 'fee simple'— the allottee had to be declared "competent" by the local BIA agent. This second treatment constituted a major wealth transfer. The average proceeds from selling the typical 160 acre allotment were 2,800–3,800 dollars in our data, and many, possibly even most allottees chose to sell their land once they had full rights to it (Carlson, 1981, p8,13). It was clear—given the policy's objective of cultural assimilation— that improving one's own education and one's children's education, farming the allotted land, and attending church were all actions that marked one out as competent in the BIA agent's eyes (Carlson 1981, ch4, Golenko 2010).

The Dawes Act applied to all reservations equally, and every household on allotted reservations was to receive an allotment.[7] However, allotment was phased out sequentially across reservations because it required significant investments into surveying and administration on the BIA's part (Carlson, 1981, p41). As a result, only half of all reservations had been allotted by 1934. The challenge with identifying the effect of allotment is therefore that the BIA phased out allotment in a non-random order across reservations. Two considerations appear to have dominated the government's thinking in this regard: It prioritized allotting reservations of tribes that had shown more organized opposition to the federal government in the past, and it prioritized allotting reserva-

[5] In addition to having individual-level linkage scores above a certain threshold, these households do not throw up any flags along a number of criteria used to identify links that are potentially logically inconsistent. We also link the 1930 Population Census to both the ICR and the 1940 Population Census.

[6] See authors' calculations in Table 1.

[7] In practice, because we don't have inheritance information and in some places allotment had been rolled out much earlier, we cannot simply assume every household on an allotted reservation in 1940 was "treated", and need to ICR to identify treatment. In the ICR, we find that about ten percent of households on allotted reservations are unallotted. We omit these from the analysis.

tions with more attractive land, primarily because "surplus" tribal lands left after allotment could be opened for white settlement (Carlson 1981, ch1, Leonard, Parker, and Anderson 2018).[8] Our approach to this identification challenge is two-fold: First, we condition all estimations on tribe and state fixed effects, with the reservation-to-tribe mapping being many-to-one.[9] This sweeps out unobserved cultural factors that made the government target certain tribes for allotment. Second, to condition out reservation characteristics that could have influenced both the allotment process and household outcomes in 1940, we include all controls for reservations' land quality from Leonard et al. (2018), and a range of additional controls for the socio-economic environment.

When an Indian was declared competent, their allotment was transferred into fee simple, giving them full property rights. To identify the causal effect of this second treatment, we develop an instrumental variable (IV) strategy based on the exogenous rotation of BIA agents across reservations over time. Using a newly constructed reservation-year panel of BIA agents from 1887–1934, we observe considerable variation in agents' propensity to transfer land into fee simple, and interact this with allotment-specific time-variation in the eligibility for transfer: Agents with high transfer propensity would still only transfer land into fee simple after it had been held in trust for a sufficient period of time, and only once the allottee had come of age. Based on this logic, we construct for each allotment a cumulative probability that it had been transferred into fee simple by 1934.[10]

Within a tribe, we find that allotted households were about thirty percent more likely to live on a farm than households of the same tribe on unallotted reservations. Interestingly, there is no corresponding increase in the likelihood of working as a farmer. This indicates that many allottees leased out their allotments instead of working their land, as already argued in Carlson (1981). We find no effects on labor force participation or employment, but wages were 4 percent higher for household heads with allotments, and they were in significantly better occupations, as measured

[8] Online Appendix Figure 1 is a sales ad for such surplus land.

[9] Dippel (2014) and Feir, Gillezeau, and Jones (2019) use tribe fixed effects to identify reservation treatments. Similarly, Akee (2019) compares one allotted and one unallotted reservation of the Anishinaabe tribe in Minnesota.

[10] Our use of agents' exogenous rotation is akin to the 'judge fixed effect' literature. The majority of papers in this literature gains identification from the raw-data probability that a specific judge makes a certain decision in a static setup, e.g. in applications ranging from criminal sentencing to patent office decisions (Kling, 2006; Di Tella and Schargrodsky, 2013; Galasso and Schankerman, 2014; Aizer and Doyle Jr, 2015; Melero, Palomeras, and Wehrheim, 2017; Dobbie, Goldin, and Yang, 2018; Frandsen, Lefgren, and Leslie, 2019). Our approach differs somewhat in that the decision to transfer land from trust status to fee simple occurred in a repeated duration setup. We first construct year-specific probabilities of transfer into fee simple, and then aggregate these into a cumulative probability.

by a range of occupational rank measures. The wage and occupation effects are not explained by a household's farm status, in fact they get stronger when farm status is controlled for. This suggests that rents earned from allotments may have impacted the occupations that households choose.

Most surprising is that we find very strong effects of being allotted on educational attainment. Mothers, sons and daughters have an average of about 2 years of extra schooling relative to those on unallotted reservations of the same tribe. We interpret this education effect as an effort by allottees to improve their prospects of being declared competent by their local BIA agents. One piece of evidence consistent with the 'education as a signal for assimilation' interpretation is that households who ended up obtained their land in fee simple had higher educational attainment than allotted households whose land remained in trust. To provide further evidence on this channel, we construct a repeated cross-section of the entire Native American population in the four Population Census waves 1910–1940, and study schooling choices in this data.[11] We find that school attendance increased within reservations over time with the expansion of allotments (as did farming), conditional on either flexible age polynomials or age fixed effects. One may be concerned that the education effects we find are supply-driven, i.e. if the government pursued education-based assimilation efforts in parallel to land allotments. Several facts speak against this: while the federal government had tried assimilationist educational policies in the form of off-reservation boarding schools, their emergence in 1879 pre-dated the thrust of allotment by almost three decades (Gregg, 2018). As early as 1891, parents had to give their "full consent" for a child to be sent to a boarding school (Adams 1995, 65, Lomawaima 1995, 36), and by around 1910 primary and middle school were fully provided on reservations, while it was a parental choice whether to send their kids to high school, be it a local (mostly non-Indian) public one, or an off-reservation Indian boarding school (Hoxie, 2001, 208). Lastly, the pattern we find likely explains a large part of the secular increase in school attendance documented in the BIA Annual Reports from around 1910 on, and this increase turns out to be entirely driven by local non-boarding schools (Gregg, 2018, Fig1).

Next, we investigate the effect of the policy's second treatment arm, the additional wealth transfer from having one's allotment transferred to fee simple. This effect is studied conditional on reservation fixed effect, so that we compare households who obtained their land in fee simple

[11] Educational attainment is only reported in the 1940 Census so that we can only consider school attendance in previous years.

only to households on the same reservation whose allotments had remained in trust. We find that obtaining full (i.e. 'fee simple') property rights to their allotment, reduced the likelihood of living on a farm or owning one's residence, evidence that many households sold their land when they could. Home values (conditional on owning) also increased significantly. We also find that the fee simple treatment increased the likelihood of working for wages, and led to occupational upgrading, suggesting individuals who sold their land also changed the type of work they did subsequently. Children of households with fee simple land attained higher levels of education, beyond what was explained by their parents' education. As well, they had significantly higher wages once out of school.

We also investigate the interaction between allotment and the ancestral cultural norms governing individual property rights in a tribe.[12] We find that the effects of fee simple land on wealth (measured by home values), on parents' occupational upgrading, and on children's wages interact strongly with having ancestral traditions of private property. This suggests that a culture of private property rights allowed individuals to better utilize the income shock from obtaining their land in fee simple.

Our paper studies the consequences of the federal government's policy of 'Indian allotment' on affected households. The first treatment arm of this policy was to unconditionally grant each household on an allotted reservation and allotment that could be used for farming or leased out. The second arm was to grant the much more valuable full property rights over this land, conditional on allottees "proving themselves competent". The unconditional component of Indian allotment relates our paper to a large literature on the effects of household income on various outcomes, primarily children's health and education. Existing evidence suggests that the causal effect of parents' income on children's health and schooling is generally small in developed economies with universal public good provision, but that it can be large for poor households (Akee, Copeland, Keeler, Angold, and Costello, 2010; Aizer, Eli, Ferrie, and Lleras-Muney, 2016; Cesarini, Lindqvist, Östling, and Wallace, 2016; Bleakley and Ferrie, 2016; Haushofer and Shapiro, 2016; Akee, Copeland, Costello, and Simeonova, 2018). In our setting, treated parents were poor on average, and we find sizable effects of parental wealth on children's education and wages. The

[12] Tribes' traditions of private property are coded in the *Ethnographic Atlas* (EA). As an aside, the seminal paper on 'property rights' in the economics literature used the varying property rights among different Native American tribes as an illustration of how such rights may be endogenized (Demsetz, 1967).

conditional component of Indian allotment relates our paper to a large literature on conditional cash programs, to which our setting provides an interesting point of comparison. Modern day programs typically involve ongoing smaller cash payments to families conditional on children's attend school (Schultz, 2004; Barrera-Osorio, Bertrand, Linden, and Perez-Calle, 2011; Behrman, Parker, and Todd, 2011; Parker and Todd, 2017; Barrera-Osorio, Linden, and Saavedra, 2019). By contrast, being declared competent in our setting translated into a large one-time wealth transfer, but this transfer could lie far in the future and was only implicitly linked to educational attainment. Nonetheless, it appears to have provided a powerful incentive for educational attainment in the setting we study.

Two quantitative studies have investigated the effects of Indian allotment on Indian farming.[13] Carlson (1981) uses data from BIA Annual Reports to show that allotment in fact reduced aggregate Indian farm output. Our findings are consistent with this insofar as we find clear evidence of leasing-out of allotments, which would have reduced agricultural output from Indian-operated farms. Closely related to ours is a recent study by Akee (2019) who compares households on the allotted White Earth reservation to households on the unallotted Red Lake reservation —both of the Anishinaabe tribe in Minnesota— and finds significantly lower rates of farming and home ownership on White Earth. In contrast, while we find that fee simple land reduced farming and home ownership relative to in-trust allotted households (on the second treatment arm), we do not find zero-effects of allotted relative to unallotted households. There is good reason to believe that the effect of allotment on farming and home ownership may have been more pronounced on White Earth than elsewhere. White Earth specifically was allotted through its own act (the 'Nelson Act' of 1889), that provided for allotment of White Earth "in advance of the Dawes Act's normal operation" (Carlson, 1981, p11). The government's desire to free up Indian lands was higher than average on White Earth. Consistent with this, the BLM data reveal that over 90 percent of allotments on White Earth were transferred into fee simple so that the effect of the second treatment arm may have dominated on White Earth.

Our findings speak to a literature on the effects of cultural assimilation policies, which are typically implemented through schooling (Sakalli, 2017; Bandiera, Mohnen, Rasul, and Viarengo, 2018; Fouka, 2019). This is no less true in Indian country, where Indian boarding schools were

[13] Leonard et al. (2018) is more concerned with the causes than the consequences of Indian allotment.

the primary assimilationist policy in the pre-allotment era.[14] Gregg (2018) shows that reservations that historically sent a higher share of children to boarding schools have more assimilated populations today. At a more micro level, Feir (2016b) studies Canadian residential (boarding) schools, and —using an identification strategy that addresses selection into attending these— shows that they increased the educational attainment and later labor force participation of students. Assimilationist effects show up in a higher likelihood of living off-reserve, and a lower likelihood of speaking an indigenous language. In a follow-up, Feir (2016a) finds broadly negative intergenerational impacts of mothers' residential schooling experience on children's school experiences, possibly reflecting a longer-run form of cultural "backlash" that has also been documented with other assimilationist policies (Sakalli, 2017; Fouka, 2019). In contrast to the assimilationist policies studied in these papers, allotment did not use education to assimilate. Instead, its design as a wealth transfer that was made conditional on signalling one's cultural assimilation appears to have incentivized families indirectly to increase school attendance.

Our paper also contributes to a growing literature emphasizing the importance of cultural norms as drivers of economic outcomes and decision making (Algan and Cahuc, 2010; Fernández, 2011; Nunn, 2012; Costa-Font, Giuliano, and Ozcan, 2018; Dohmen, Enke, Falk, Huffman, and Sunde, 2018; Enke, 2019).[15] We contribute to this literature by showing that ancestral cultural norms of private property were an important mediating channel that determined how successfully households navigated the transition in the property rights regime imposed by the policy of Indian allotment.[16] Giuliano and Nunn (2017) argue that societies that have historically lived in more stable conditions evolved norms that place greater value on tradition. When the conditions drastically change, adherence to tradition can become costly for societies if the traditional cultural norms are ill-suited to dealing with the new conditions. We provide concrete evidence that this was the case. It is clear that Native Americans had norms that placed great value on tradition, and that they faced drastic 'environmental changes' during the reservation and assimilation eras. We

[14] In Canada, where no parallel to allotment was tried, and Indian residential schools continued into the late twentieth century, schooling remained the main assimilationist policy throughout.

[15] This literature has also brought to light the tight empirical connection between present-day and ancestral cultural norms. Cultural norms evolve over the long run to optimally regulate a groups' economic and social lives, and they have been shown to be remarkably persistent over time in a variety of contexts (Spolaore and Wacziarg, 2013; Galor and Özak, 2016; Becker, Enke, and Falk, 2018). Certainly, the EA's 'ancestral tribal norms' would have still been very much just the tribes' culture at the time we investigate them between 1906 and 1940.

[16] To some extent this is the flipside of Ostrom (1990) who argued for the importance of culture in making collective property management function without having it deteriorate into the tragedy of the commons.

show that the cultural groups that had traditions of individual property rights were better able to navigate these changes.[17]

Before proceeding, a comment is in order on what our results imply about the aggregate welfare consequences of Indian allotment. Although we estimate positive effects of allotment on educational attainment and on farming, and positive effects of obtaining land in fee simple on wealth and occupational upgrading, these findings should not be interpreted as saying that tribes were better off with allotment, even in a narrow economic sense.[18] There is no doubt that allotment is viewed negatively in Indian country today. One obvious reason for this is that it was an externally imposed colonial policy aimed at the cultural assimilation of Indians, and at undermining tribes as polities. A second reason is that the wealth that was transferred to individual tribal members through allotment was taken from the tribe itself. With the 1934, tribal governments were strengthened and it is entirely possible that the value of counterfactual revenues that a tribe could have earned of the land and paid to its members in annuities and transfers could have exceeded those earned from individual allotments. A third reason is that—regardless of the lawmakers intentions—there was plenty of corruption in the on-the-ground implementation of the law, and land speculators clearly tried to influence the allotment process for personal gain. A fourth reason has to do with how in-trust land was handled after 1934. Under the ill-conceived inheritance rules determined by the BIA, in-trust land had to be bequeathed to all of an allottee's heirs in *multiple, undivided interests*. Over time, this led to a steady increase in claimants on these lands, which in turn has created large inefficiencies in land use for later generations of Native Americans (Leonard and Parker, 2017; Dippel, Frye, and Leonard, 2019). While this problem is clearly a legacy of Indian allotment, it has more to do with not transferring all land into fee simple than with the policy of allotment per se.

[17] A related question that we do not explore is whether some tribes' (or even households') cultural norms in relation to private property may have changed in response Indian allotment. Di Tella, Galiant, and Schargrodsky (2007) provide evidence for such a mechanism, exploiting quasi-random variation in giving legal title to squatters in Buenos Aires to identify the effect of property rights on beliefs and norms related to the market.

[18] On the one hand, the seminal work in Ostrom (1990) clearly delineates many successful cases in which communal (tribal) land- or resource ownership can work well. On the other hand, it is not clear that tribal governance over a commonly owned reservation land actually was more consistent with many Native American tribes' traditions and customs. In fact, a number of papers demonstrate that economic under-development on reservations today is partly created by political control over reservation resources by tribal leadership (Anderson and Parker, 2008; Dippel, 2014). Consistent with this, tribal governments on many reservations today are in the process of changing institutions to reduce communal tribal control over resources in order to promote economic development (Regan and Anderson, 2016; Jorgensen, 2007).

2 Background

2.1 Land Allotment on Reservations

Following the establishment of the reservation system, American Indian reformers considered land allotment as a requisite element in the assimilation of American Indians (Otis, 2014).

The driving force of assimilation was to "be the enlightened self interest of the individual Indian. Freed from the binds of tribal customs and authoritarian chiefs, the individual would soon want to accumulate wealth and property and, as he progressed economically, would acquire the habits and customs of Christian society. The key was to be private property" (Carlson, 1981, p80). Early allotment efforts were negotiated into treaties, but legislation implementing a general allotment act stalled over the issues of citizenship, jurisdiction, and whether to immediately grant allotted Indians title to their land. In 1886, Henry Dawes introduced a modified allotment bill to the Senate. The bill quickly passed before moving to the House, where it passed after the addition of several amendments. 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, to survey and allot reservation lands deemed appropriate (Banner, 2009).[19] Heads of household received 160 acres, single persons over 18 received 80 acres, orphans under 18 received 80 acres. The Dawes Act was amended in 1891 to grant 80 acres to every adult, instead of 160 acres to heads of households. If the land was only suitable for grazing the allotment amounts doubled. If a prior treaty specified larger allotments, the prior treaty acreages were applied. Allotments were mandatory and anyone not selecting an allotment within the first four years, would be assigned a parcel by the Indian Agent. Unallotted reservation land was designated as surplus and made available for outside settlement. The law required tribal approval of ceded surplus land, but tribes were rarely in a position to negotiate (Carlson, 1981).[20] Proceeds from the sales of surplus land were held in trust and appropriated at the discretion of Congress (Banner, 2009).

Once selected, allotments were approved by the Secretary of Interior and each Indian was issued a trust patent. This patent held the allotted land in trust for a trust period, during which the Indian or their heirs were the beneficiary of the allotment. Land held in trust could not be

[19] Tribes in New York and Indian Territory were temporarily exempted from the Dawes Act.

[20] By 1903, tribal approval was no longer necessary.

alienated or leased and was not subject to state or local taxes. At the end of trust period, the allotment would be transferred to the owner as fee-simple. In 1906, the Burke Act granted the Commissioner of Indian Affairs the authority to shorten or lengthen the 25-year trust period for individual allotments. Shorter trust periods were often at the recommendation of the Indian Agent (Carlson, 1981). The Burke Act gave agents considerable authority over the process of converting land from trust status to fee-simple.

2.2 Administration on the Ground

Implementing the Dawes Act on an individual reservation was a complicated process. First, the BIA agent in charge of the reservation was tasked with determining the list of eligible tribal members entitled to an allotment and the household structure for every household within the reservation (Banner, 2009). These agents were also tasked with managing the surveying of the reservation and its division into parcels.

Indians could select a parcel, but usually did not, in which case the local BIA agent determined the assignment of allotments (Banner, 2009; Otis, 2014; Carlson, 1981). The Meriam Report characterized the process 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 where 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 were sufficiently far sighted to select land on the basis of its productivity when used as the white man used it. The Indians were not sufficiently advanced generally to make their selections on this basis and the allotting work was done too fast and on too wholesale a basis for the representative of the government to advise and lead them to sound selections" (Meriam, 1928, p470).[21] Each allotment was given an allotment number and a patent was filed with the Government Land Office upon approval by the President. These official patents specified the trustee, the specific plot location, the date, and the unique allotment number. Reservations were either allotted all at once or over a period of several

[21] Unsurprisingly, there are also accounts of outside settlers influencing agents to set aside the highest quality land for surplus (Otis, 2014, p145).

years.

The implementation of the Dawes Act moved quickly on the extensive margin, where nearly four reservations per year were designated for allotment over the first two decades (Otis, 2014). However, constraints over personal and resources slowed surveying and the allocation of allotments. Following the introduction of the Burke Act, the pace of allotting accelerated considerably.

Concerns about the lack of development of Indian farmers, expansions in leasing, and sales of Indian land to settlers led to a change in public opinion regarding allotment culminating in a review of the current social and economic conditions on reservations by Lewis Meriam of the Institute of Governmental Research. The Meriam Report, published in 1928, was critical of the support provided to Indians by the Office of Indian Affairs (Meriam, 1928). This report led to a shift in federal Indian policy, brought to fruition by President Roosevelt's new Commissioner of Indian Affairs, John Collier. Collier introduced a bill that fundamentally changed Indian policy. In 1934, the Indian Reorganization Act (IRA) ended the allotment of Indian reservations. The IRA returned unallotted lands back to tribal ownership and froze allotted trust land in it's trust status, creating a patchwork of land tenures within Indian reservations.

2.3 Legacy of the Allotment Era

Table 1: Leasing Rates and Sales Prices of Indian Allotments

	Leasing Revenue per Acreage				Sales Price per Acreage				Annual Income		
Year	#Leases	Area in Acres	Income	Annual Lease Price per Acre in $	Area	Income	Sale Price per Acre in $	Sales Price / Lease Revenue	Pop.	Total Income	annual per cap income
1920	23,550	2,254,632	4,406,308	$1.95	147,047	3,566,816	$24.26	12.51	306,539	72,696,431	$237
1919	22,658	2,198,753	3,809,291	$1.73	57,947	1,224,823	$21.14	12.55	304,974	53,994,859	$177
1918	19,073	2,145,553	3,067,875	$1.43	74,126	1,541,178	$20.79	14.78	306,755	42,056,070	$137
1917	20,567	2,023,788	2,615,639	$1.29	69,849	1,040,202	$14.89	13.77	309,409	35,867,696	$116
1916	22,612	2,357,542	2,603,498	$1.10	54,959	969,611	$17.64	18.65	307,797	26,489,948	$86
1915	16,500	2,415,794	2,117,166	$0.88	34,429	584,724	$16.98	19.38	309,911	23,193,046	$75

Notes: Leasing Rates and Reservation Incomes Data come from Tables 12 and 11 of the 1920 Annual Report of the Commissioner of Indian Affairs. (The level of detail in these reports was dramatically reduced after 1920, and no longer includes leasing revenues.) Sales prices come from United States National Resources Planning Board 1935 Report *Indian Land Tenure, Economic Status, and Population Trends*

In total, the government extended the Dawes Act to 118 reservations and issued over 245,000 patents covering nearly 41 million acres (Office of Indian Affairs, 1935). On the one hand, Indian allotment resulted in a substantial transfer of revenues that previously would have gone to tribes, and now went to individual tribal members. Table 1 shows that a 160 acre allotment would have generated annual leasing proceeds of between 150 and 300 dollars in our data (e.g., $160 \times 1.73 = 270$ in 1919), relative to average per capita incomes from all sources between 100 and 240 dollars during the same time. At the same time, the average proceeds from selling a 160 acre allotment that was obtained in 'fee simple' would have been between 2,800 and 3,800 dollars in our data (e.g., $160 \times 21.14 = 3382$ in 1919).

On the other hand, Indian allotment resulted in a substantial transfer of land out of Native and into non-Native hands. Prior to the Dawes Act, Indians controlled over 138 million acres of lands within their reservations. By 1934, Native land holdings had fallen to 52 million acres. Of this 85 million acre reduction, nearly 60 million acres had been ceded as surplus and the remaining were sold as fee-simple or alienated by the Secretary of Interior (Office of Indian Affairs, 1935). Within reservations, the Dawes Act created considerable variation in the status of land tenure between land held in individual trust, and land held in fee simple. Parcels of differing tenure types are often adjacent to one another, creating a checkerboard pattern of land tenure on many reservations. Figure 1 illustrates this pattern on the Pine Ridge Reservation, using the data discussed in Section 3.1.

3 Data Sources

3.1 Allotments in the BLM Land Data

Following approval from the President, each patent issued on the reservation was filed with Government Land Office and was digitized by the BLM. These patents 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. Patent types include cash sales, homestead entries, and Indian allotments. The patent also includes the Indian allotment number associated with the transaction. A nice feature of the BLM data is that we can see exactly the date on which each patent was issued (in trust) and the date on which it transferred into fee simple, if ever.[22]

3.2 Measuring Outcomes: The Full-Count Population Census

The 'rule of 72' dictates that non-anonymized individual-level Census information can be made available 72 years after a Census was published. The 1930 and 1940 Full-Count US Censuses (FCC) each include around 300,000 individual Native Americans living in roughly 80,000 households (with some intermarriage). The FCC data provides us with measures of individuals' and households' incomes, occupational rank, property ownership, and a range of other outcomes. Critically for our purposes, there is no way to directly link the FCC to the BLM land information: the allottee name information in the BLM data is inconsistent, and the FCC contains no information at all related to allotment. Section 3.3 introduces newly collected data that allow us to build a bridge between the BLM data and the FCC.

3.3 Measuring Treatment: The Indian Census Rolls

To be able to link BLM land allotment information to the FCC, we need to link both the BLM and the FCC to the *Indian Census Rolls* (ICR). The ICR were special censuses that were unrelated to the FCC and were collected on reservations consistently from the 1880s to the late 1930s. The ICC did not record any economic outcome data; instead they are simply complete enumerations of all Native American households. Online Appendix Figure 2 depicts a sample page from the ICR. They include only individuals' names, their ages, and their relations within the household (e.g. spouse or son). Critically, they also report whether individuals received land allotments, including a complete listing of unique allotment numbers by individuals. We digitized the entirety of the ICR from the mid-1930s.

The first of two steps is to link the ICR data match uniquely to the BLM data via allotment number. We match about 85% of the allotments in the ICR to the BLM data. The remaining 15% are likely mis-recorded in the ICR. This is illustrated Figure 1 for the the Pine Ridge Reservation. We omit households with unmatched allotment numbers from our analysis. The second step is to link the ICR to the FCC, using record linkage methods described in Section 4.

[22] This ability to "follow the land" and the ability to distinguish parcels that were converted to fee simple is of separate interest because of the large amount of allotted land that became "trapped" in trust in 1934. This topic is the focus of Dippel et al. (2019).

3.4 The need for linking to the ICR

Given that allotment was designed such that all households on an allotted reservation were allotted, one may conjecture that the link to the ICR is not strictly needed to investigate the effects of allotment per se. There are two main reasons that one cannot simply infer treatment in the Population Census based on being on an allotted reservation. One reason is that in some places allotment had been rolled out much earlier than 1940, so that many of the original allottees had died. In those cases, some descendants of an allottee may not be treated if the allotment was bequeathed to only one descendants, or if it was bequeathed at all if, for example, descendants were born out of wedlock. A second reason is that the Population Census does not actually contain tribe or reservation information.[23] Instead, the only spatial information is county. We use county to predict a person's most likely reservation, which helps us with the linkage to the ICR (Section 4), but the relation between county and reservation is not sharp enough to directly measure a person record's reservation based on county.

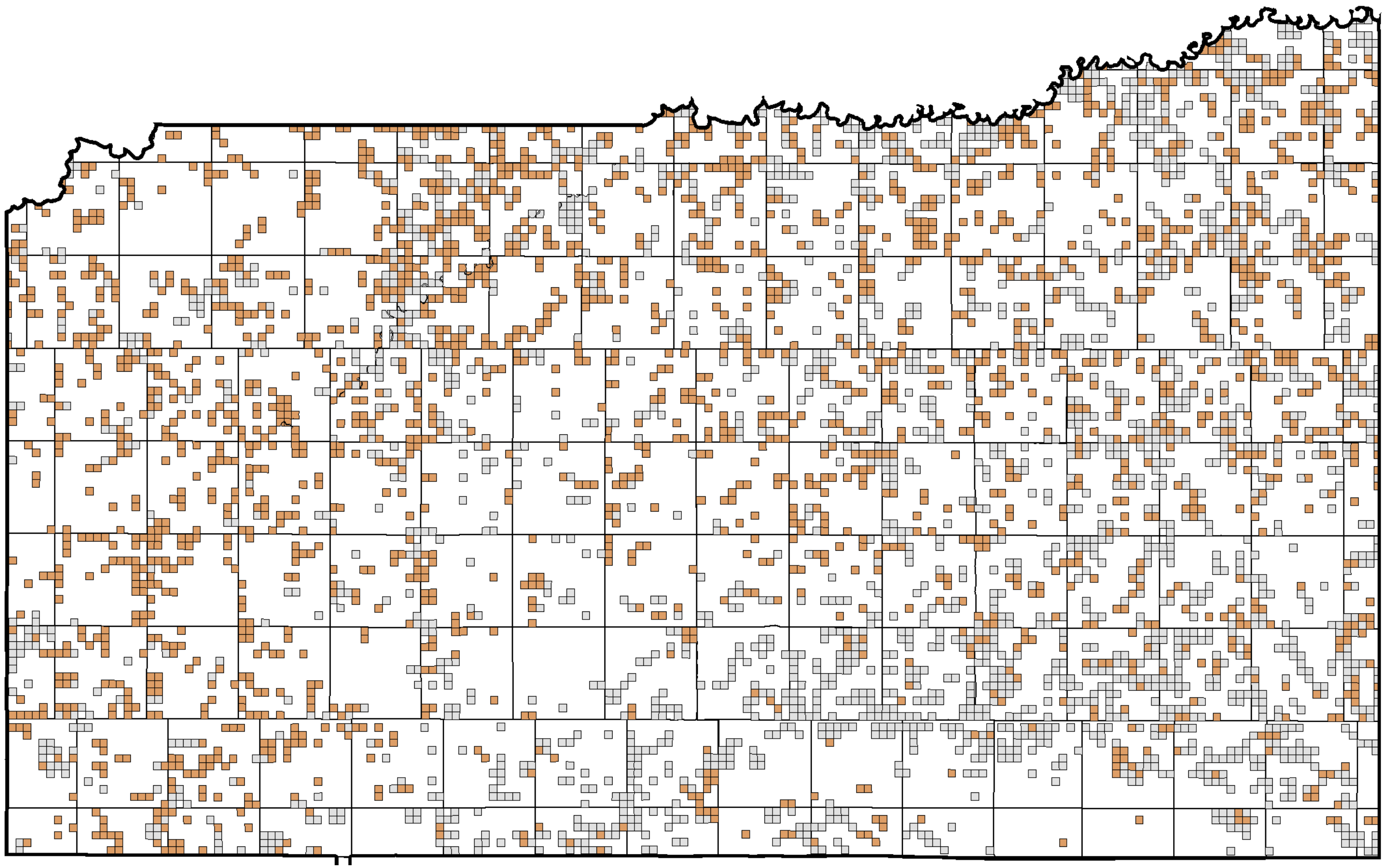
3.5 The Indian Agents

To gain identification on the the causal effect of obtaining land in fee simple, we pursue a strategy based on the exogenous rotation of Indian Agents across reservations, and their varying propensity to transfer land into fee simple. To operationalize this, we construct a complete reservation-year panel of Indian Agents from 1879–1940. Our primary source of agent information is from the Department of Interior employment rosters recorded in the Official Register of the United States (1932).[24] The records provide agent name, birthplace, position title, and annual pay. Each agent is listed by agency and city, which we link to reservations. We supplement these records with two additional resources produced by the Bureau of Indian Affairs. First, we use the agent narratives included in the Bureau of Indian Affairs Reports published annually from 1879 to 1907. Each agent was required to produce an annual summary of agency events. We recorded each agents name from the end of the summary. Second, we use the ICR data, which included the name of the agent

[23] This is a statement about the information currently included in the historical Full Count Censuses. While variables are added to these over time, they always contain far fewer variables than the 1% IPUMS micro-samples, since adding them is obviously 100 times as expensive. The reader may be aware that the 1% IPUMS 1930 micro-sample has (relatively coarse) tribe information. See, for example, Giuliano and Nunn (2017) for a paper using this variation.

[24] The Official Registers were published biennially from 1879–1940.

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 BLM data described in Section 3.1. The reservation is divided into 36-square mile townships. A township itself consists of 36 one-square-mile sections, and a quarter-section corresponds to a 160-acre allotment. Each quarter-section is either an allotment transferred into fee simple (dark/orange), an allotment that remained in trust (light-grey), or else it is tribal land that was never allotted or an allotment that had no match in the ICR (blank).

in charge of taking the census from the beginning of each census year.

3.6 Ancestral Tribal Norms

The data on ancestral cultural norms comes from the Murdock (1967) *Ethnographic Atlas* (EA). Among many other measures, the EA has tribal information on property rights norms. In fact, the EA has separate measures for two different types of property, movable and real property, i.e. land. We will be interacting the effects of land allotment with the types of property rights separately.

To the best of our knowledge, there is currently no paper in the 'cultural economics' literature that has focused on the effects of property rights traditions. To rule out spurious correlations of property rights norms with other tribal characteristics, we will also look at two other dimensions of traditional norms that have received attention in the cultural economics literature, namely norms of having nuclear family and norms of having a bride price, i.e. of the groom's family paying a material transfer to the bride's parents.

4 Record Linkage

Measuring treatment and constructing the instrument requires either only the BLM data, or [if allottee characteristics are included in the zero-stage equation (7)] the BLM data and the ICR data. These two data sets are straightforwardly linked by allotment number. Once treatment and instrument are attached, however, the ICR needs to be linked to the Full Count Census (FCC) data for the second-stage analysis, and this record-linkage is difficult. The main individual linkage variables at our disposal in both ICR and FCC are first and last name, birth-year, location, and gender. Fortunately for us, spatial mobility among Native Americans in this period of time was very low, with the vast majority of Native Americans living on reservations or former reservation lands in both 1930 and 1940.[25] We can therefore block the record linkage on location. In our case, because some reservations in the Southwest straddle state borders, we create 40 meta-states so that each reservation is uniquely contained in one, and then block on these. Secondly, we also block on gender.[26]

[25] Substantial off-reservation mobility really took hold in the 1950s during the 'Termination Era', in which the federal government attempted to dissolve reservations.

[26] The FCC does not report reservation or tribe, but county of residence, which is sufficient to uniquely place most individuals in a reservation.

Within this blocking, individuals are linked by name and birth-year. The challenge in our data is that both of these linkage variables are recorded with a lot of noise. For names, this is partly driven by the fact that Census enumerators made more frequent errors in recording Native American names because they were unfamiliar with them. It is further driven by the fact that there was a strong trend of anglicization of names in this period so that names changed over time. For birth-years, noise is introduced into the records by the fact that most Native Americans at that time did not have birth certificates, and were thus more likely than other populations to revise their stated birth-year as time passed. These challenges make it impossible to obtain a large number of unambiguous individual record links in these data based on only individuals' information. To make progress in this direction, and ultimately enable us to perform the second stage analysis, we had to move beyond existing individual-based record linkage methods and develop a linkage scheme that incorporates the similarity of households' structure across data sets.[27;28]

Table 2: Household-Based Linkage Example

ICR [master dataset]							FCC [using dataset]				
hhid	birthyr	namelast	namefrst	relate	links	score	hhid	birthyr	namelast	namefrst	relate
35545638	1908	SESSPOOCH	WAUN	Head	2	6.3	79055	1907	CESSPOOCH	JUAN	Head
35545638	1901	SESSPOOCH	ELLEN	Wife	2	11.9	79055	1902	CESSPOOCH	ELLEN	Wife
35545638	1934	SESSPOOCH	LOUIS	Son	5	11.2	79055	1933	CESSPOOCH	LEWIS	Son
35545638	1937	SESSPOOCH	DEBOIA	Daughter	3	6.3	79055	1936	CESSPOOCH	DOVELIA	Daughter

Notes: This table shows how household-information can increase the confidence of individual level record linkages. No individual link looks very compelling as not a single person is a perfect match on age and name. Yet, viewed in combination, these two households are clearly the same.

Table 2 provides an example for the potential power of using household information in data like these. The table shows two four-person families (one in the ICR, and one in the 1940 FCC). The noteworthy feature of the table is that not a single person is a perfect match on age and name. The shared last name is different in the two data-sets, all birth-years are off by one year, and only one of four first names is an exact match. Nonetheless, viewing the four records in combination gives a high degree of confidence in this being the same household in the two data sets. This example

[27] Existing linkage methods are almost entirely focused on individual linkage. The one exception that uses household structure for record linkage that we are aware of is Price, Buckles, Riley, and Van Leeuwen (2019).

[28] Methods of historical record linkage are fast evolving, driven by advances in the access to historical individual level data, computational power needed for linkage algorithms, the ability to scale up manual linkage through online job platforms, and machine learning capabilities. For a review of the historical record linkage literature, see Online Appendix A.2.

illustrates the potential usefulness of household structure in determining individual links, when individual linkage variables are measured with a lot of noise. In the following we describe our step-by-step approach to record-linkage:

1) We first use standard individual record linkage methods to establish for each individual in a master data-set (say, the ICR), the set of all potentially linked individuals in a using data-set (say, the 1930 FCC). This can be any of the commonly used approaches discussed in Bailey, Cole, Henderson, and Massey (2017), Abramitzky, Boustan, Eriksson, Feigenbaum, and Pérez (2019), and Online Appendix A.2. We pursue a two-step approach: we first apply bigram-indexation to first and last name to define the set of individuals in the using data with similar names.[29] We are 'generous' in this set, in the sense that we set the string-similarity cutoff low enough that each person in the master data has an average of 8 links in the using data.

   We then trim that set to include only potential matches that deviate by at most 4 years in birth-year for master records born after 1910, and by at most 6 years for master records born before 1910.

2) Each individual in the master data and each individual in the using data is associated with a unique household id. Consider a pair of households ICR-HH A in master and FCC-HH B in using. Our approach is to calculate the number of individuals in ICR-HH A who are linked to FCC-HH B in stage 1. This gives a metric of household similarity. We can then upward-adjust the similarity-scores of individual links between individuals in ICR-HH A and FCC-HH B by a percentage for each marginal increase in our metric of household similarity. We also re-generate adjusted similarity-scores between all non-linked individuals in ICR-HH A and FCC-HH B to potentially find new links. The idea is that these new links did not meet the match-threshold in the individual-level linkage in stage 1, but do meet the threshold with the upward-adjusting. Household similarity therefore brings new individual links into the fold.

   The researcher has three levers to control the adjustment process in stage 2: One, household

[29] We use the `stringsim()` function in `R`'s `RecordLinkage`, and block on gender and on 'meta-state' and gender. (A 'meta-state' is a set of two states whose boundaries are straddled by reservations. This occurs, e.g. in New Mexico and Arizona. We formed 40 meta-states.

similarity can be calculated in different ways. For example, we use the absolute number of stage 1 individual links as our metric. This inherently creates more adjustments in larger households than in smaller ones. One could alternatively use the share of household members who are linked. As another alternative, one could also create a series of household-pair-specific dummies for binary measures such as whether there is both a male and female head of household, whether a household is a nuclear or an extended family, etc.

Two, the gradient of how individual similarity is adjusted to household similarity can be controlled. With more aggressive upward-adjusting, the researcher puts more weight on household-similarity. With less aggressive upward-adjusting, the researcher puts more weight on individual similarity.

Three, one could define a series of explicit rules based on household types in a pair. For example, if the only individual that is linked across ICR-HH A and FCC-HH B, is a 'son' in the ICR, and a 'head' in the FCC, then two households are likely to be otherwise completely disjoint sets, and the researcher might want to prevent the one exiting individual link's similarity score to be downward-adjusted (in relative terms) for the lack of household-similarity.

To what extent these levers are used will be dictated by the structure of the specific dataset as well as by computational concerns.

3) Then a "best-off" gridsearch is applied on the adjusted similarity scores created in stage 2. The grid-search maximizes the sum of similarity-scores of all links, with the constraint that each individual record in the master data is linked to at most one record in the using data, and each record in the using data is linked to at most one record in the master data.

4) Relative to individual linkage methods, steps 2 and 3 above have the potential to create additional links to the extent that household similarity can lead individual similarity scores to be scaled up. This is evident in Table 2 where no individual link looks particularly compelling when viewed in isolation, but the links look very compelling when viewed in combination.

   More importantly, the household structure also suggests a range of criteria to generate flags for potentially problematic links, based on reported family relations. For example, while it is reasonable for the husband and the wife in a household in the (later) master data to be linked to records in the (earlier) using data where they are recorded as a son and a daughter, they should then not share a household in the using data, lest they be brother and sister.[30] The final linked data-set will invariably be improved by adding a fourth stage of manually checking links that that throw up flags for family relations that are either logically consistent or unlikely.

Manual linkage after stage 4 is greatly aided by the fact that stages 2–3 have identified the most likely individual link for each member of a given "anchor household" in the master data, so that one can view all members in the anchor household together with all person records belonging to households with any links to that anchor household. In other words, manual links can at this stage be confirmed or changed with all relevant household relations in view. Table 3 shows two examples of how our research assistants manually check flagged households in practice. The flagged household is the "anchor household," and associated with it is "network" of direct and indirect links to person-records in other datasets. In the top-panel, that anchor household has id 15426032 in the 1930 Full Count Census. In the bottom-panel, that anchor household has id 10 in the ICR. A network consists of all individuals that belong to any household linked to the anchor household. For instance, in the top-network, 1930-household 15426032 has one link to an individual in 1940-household 19002788, so that all five members of that household are included in the bloc. Household id, the number of people in a household, and individual id's are reported by data-source; columns 1–3 report on the 1940 FCC, columns 4–6 report on the 1930 FCC, and columns 7–9 report on the ICR. One observations is one person-record. If a person in a 1930-household is individually linked to persons in the 1940 Census and the ICR after stage 2, then there are entries in columns 1–3, 4–6, and 7–9 of that person record.

Columns 16–33 report on six variables that represent the relevant person- and household-information, again separately for each of three data sources. These variables are a person's first and last name, birth-year, gender, their marital status, and their relation to the household head. Based on this information, we can assess whether existing links are correct, and make manual ad-

[30] Of course, if master and using data are collected in the same year, we should expect family relations to be exactly identical in the two datasets. Any time gaps between the years in which master and using data were recorded will imply some differences in household structure between the data-sets. For example, the ICR was collected between 1930 and 1937 and is therefore likely to contain more households whose nucleus is unchanged. By contrast, when linking two decadal Census waves like the 1930 and 1940 FCC, a sizeable share of 1930 children will have formed new households in 1940, and become heads or spouses. As well, more 1930 household heads and spouses will have passed away, and two-parent households will thus have turned into single-head households.

justments in columns 10–15, which then get hardcoded into the data before re-running stages 2–3 of the program above. Columns 10-12 are 'true positive' links. A 'true positive' link can confirm an existing link, or it can over-ride an existing link. Columns 13-15 are 'true negative' un-links, which declare that an existing link is wrong. To avoid clutter for illustrative purposes, Table 3 omits entries in column 10–15 that merely confirm existing links, and only includes entries for records where at least one link gets manually over-ridden. In the top-record for example, columns 11-12 confirm the existing link between the 1930 FCC and the ICR, but column 13 unlinks these from their link to a person-record in 1940. In the fourth row, columns 11-12 establish a new link between a 1930-record called Josephine Rose and an ICR-record called Josephine Rose who differs by 11 years in birth-year but shares an ICR household with three others who are linked to 1930-records in Josephine Rose's 1930 household. Column 15 in the same row lists the unlinked previous link.[31] In the bottom-bloc, columns 11-12 link four previously unlinked individuals in the 1930 census to four previously unlinked individuals in the ICR. The records were not linked by the algorithm because of unstable last names ('Big' in the ICR and 'Big Knife' in the 1930 FCC) and first names ('Dorothy Ann' in the ICR, and 'Pretty Woman' in the 1930 FCC).

5) Manual changes made in stage 4 are the 'true positives' and 'true negatives' in columns 10–15 of Table 3.

   Hard-coded 'true positives pairs' mean that any other individual links with either record in a pair get deleted. Hard-coded 'true negative pairs' mean that this particular link gets deleted from the stage 1 output.[32] These become hard-coded and are fed into the data-output generated at the end of stage 1.

The researcher can choose the decision rules that determine which records to manually check and link by hand. In our case, we defined a number of different flags for households with inconsistent linkages between the 1940 FCC and the ICR. Possible linkages and their consistency are visualized in Table 4. The side of the table content to the left of the arrow ⇒ depicts the 1940 households for which we can study outcomes. Across columns 1–5, the right side of the table content depicts possible ways in which a 1940 household can be linked to ICR households. The top-two

[31] This particular column 15 entry is redundant because columns 11-12 already hard-code a unique 'true positive' link between these records, thus precluding any other possible links.

[32] These need to get deleted a second time at the end of stage 2 in case that any are re-introduced during stage 2 by the similarity-score adjustment.

Table 3: Household-Based Linkage Example

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)	(25)	(26)	(27)	(28)	(29)	(30)	(31)	(32)	(33)
1940			1930			ICR			true positives			true negatives			1940						1930						ICR					
hhid	#	pid	hhid	#	pid	hhid	#	pid	pid 1940	pid 1930	pid ICR	pid 1940	pid 1930	pid ICR	LN	FN	Birth Y	Sex	Married	Rel	LN	FN	Birth Y	Sex	Married	Rel	LN	FN	Birth Y	Sex	Married	Rel
Anchor-HH : hhid=15426032 in 1930																																
19002788	4	19002788-1	15426032	4	15426032-1	68934	5	9-806-11		15426032-1	9-806-11	19002788-1			RACE	WILLIAM	1891	M	M	Head	ROSE	WILLIAM	1891	M	M	Head	ROSE	WILLIAM	1890	M	M	Head
19051844	5	19051844-2	15426032	4	15426032-4	68934	5	9-807-2							ROCE	FOREST	1925	M		gr-ch	ROSE	FORREST	1925	M		child	ROSE	IAM FORREST	1924	M		child
19051844	5	19051844-3	15426032	4	15426032-3	68934	5	9-807-1							ROCE	JAUNITA	1922	F		gr-ch	ROSE	WANETA	1923	F		child	ROSE	JUANITA	1922	F		child
			15426032	4	15426032-2	67409	3	86-10-2		15426032-2	9-806-12			86-10-2							ROSE	JOSEPHINE	1890	F	M	Sp	SHANE	JOSEPHINE	1891	F	M	Sp
19051844	5	19051844-5	15463552	8	15463552-7	68452	16	9-674-17							REDFOX	CYNTHA	1922	F		gr-ch	RED FOX	CYNTHIA	1922	F		step-ch	RED FOX	CYNTHIA	1921	F		step-ch
19051844	5	19051844-1	15463565	2	15463565-2	68933	2	9-806-10							ROCE	AGNES	1861	F	W	Head	ROSE	AGNES	1862	F		Sp	ROSE	AGNES	1860	F	M	step-ch
19066701	9	19066701-5	15477934	7	15477934-6	67409	3	86-10-3							BEAS CUB	ARCHIE	1929	M		child	BEARCUB	ARCHIE	1928	M		child	EARGROUND	ARCHIBALD	1930	M		Sp
19002788	4	19002788-4				68258	12	9-627-12							BEARDEAU	MELVIN	1928	M		step-ch							BURDEAU	MELVIN JOE	1927	M		child
19002788	4	19002788-3				68258	12	9-627-11							BEARDEAU	JAMES	1926	M		step-ch							BURDEAU	AMES ADAMS	1924	M		child
19002788	4	19002788-2													RACE	MAGGIE	1890	F	M	Sp												
19062368	11	19062368-5				68934	5	9-807-3							LA ROGUE	DOROTHY	1930	F		child							ROSE	DOROTHY	1930	F		child
						67409	3	86-10-1																			ES TO OTHER	GROUND	1870	M	M	Head
						68934	5	9-806-12																			ROSE	OSEPHINE N S	1901	F	M	Sp
Anchor-HH : hhid=10 in ICR																																
19009442	3	19009442-1	15429377	8	15429377-6	10	9	426-617-4							BIG DOY	OLD WOMAN	1890	F	W	Head	BIG DOG	OLD WOMAN	1868	F	W	Parent	BIG DOG	OLD WOMAN	1868	F	W	Head
19009442	3	19009442-2	15429377	8	15429377-7	10	9	426-617-5							BIG DOY	RUTHY LODGE	1910	F		child	BIG DOG	PRETTY LODGE	1909	F		Sibling	BIG DOG	PRETTY LODGE	1908	F		child
			15429377	8	15429377-8	10	9	426-617-6													BIG DOG	ALBERT	1912	M		adop-ch	BIG DOG	ALBERT	1913	M		neph
						10	9	426-617-10																			BIG KNIFE	JOHN	1928	M		child
						10	9	426-617-9																			BIG KNIFE	DOROTHY ANN	1926	F		child
						10	9	426-617-11																			BIG KNIFE	JOSEPH	1933	M		child
						10	9	426-617-8																			BIG KNIFE	FLORA	1907	F	M	Sp
						10	9	426-617-7																			BIG KNIFE		1900	M	M	Head
						10	9	426-617-12																			BIG KNIFE	DOROTHY	1935	F		child
19009442	3	19009442-3													FOUR	COLORE GAESTOTHE	1887	F	W	child												
			15429377	8	15429377-1					15429377-1	426-617-7										BIG	KINFE	1901	M	M	Head						
			15429377	8	15429377-4																BIG KNIFE	LEVEL WALING	1928	F		child						
			15429377	8	15429377-3					15429377-3	426-617-9										BIG	PRETTY WOMAN	1926	F		child						
			15429377	8	15429377-2					15429377-2	426-617-8										BIG	FLORA S	1908	F	M	Sp						
			15429377	8	15429377-5					15429377-5	426-617-10										BIG KNIFE	HAD LIGHTENI NG	1929	M		child						

Notes: (*a*) Columns 20, 26, 31: M=married, W=widowed. Columns 21, 27, 32: SP=spouse, step-ch=step-child, adop-child=adopted child, gr-child=grand-child. (*b*) This table reports on two "household networks" Each household network has one "anchor household." In the top-panel, that anchor household has id 15426032 in the 1930 Full Count Census. In the bottom-panel, that anchor household has id 10 in the ICR. Household id, the number of people in a household, and individual id's are reported by data-source; columns 1–3 report on the 1940 FCC, columns 4–6 report on the 1930 FCC, and columns 7–9 report on the ICR. One observations is one person-record. If a person in a 1930-household is individually linked to persons in the 1940 Census and the ICR after stage 2, then there are entries in columns 1–3, 4–6, and 7–9 of that person record. Columns 10-12 are 'true positive' links. A 'true positive' link can confirm an existing link, or it can over-ride an existing link. Columns 13-15 are 'true negative' un-links, which declare that an existing link is wrong. To avoid clutter for illustrative purposes, Table 3 omits entries in column 10–15 that merely confirm existing links, and only includes entries for records where at least one link gets manually over-ridden.

Table 4: Possible Household Links, Grouped by Internal Consistency

	1940		(1)	(2)	(3)	(4)	(5)
	1940		Possible Links to ICR Households				
Acceptable Matches :							
Nuclear	H	⇨	H	non-H	non-H	•	non-H
Family	Sp	⇨	Sp	non-Sp	non-Sp	non-Sp	•
Single Head	H	⇨	H + widowed		•		H
			•		Sp + widowed		
Disregard until Manually Checked:							
Nuclear	H	⇨	H	non-H	H	•	H
Family	Sp	⇨	non-Sp	Sp	Sp	Sp	•

Notes: (*a*) The side of the table content to the left of the arrow ⇒ depicts the 1940 households for which we can study outcomes. Across columns 1–5, the right side of the table content depicts possible ways in which a 1940 household can be linked to ICR households. (*b*) The top-two panels depict linkages that are logically consistent, firstly for nuclear households with a head and a spouse, and secondly for single-head households. The bottom panel depicts linkages that appear logically inconsistent and should be checked before using these observations. (*c*) In the top panel: Column 1 indicates that both head and spouse in a 1940 FCC household are linked to a head and spouse who are in the same household in the ICR. Column 2 indicates that both 1940 head and spouse are linked to persons in the same household, and that neither of these is head or spouse in the ICR. This captures the relatively common occurrence of young couples living with one spouse's parents. Column 3 indicates that 1940 head and the spouse are linked to persons in the ICR who live in different households and are neither head nor spouse in the ICR. This captures the most common occurrence of new households being formed by young adults. Columns 4–5 capture the case where only one of 1940 head or spouse is linked to a person record in the ICR, and that record is not a head or spouse in the ICR. Column 5 is common because young wives in 1940 who were living with their parents in the ICR are less likely to be linked because of their different last names. The middle panel depicts single-head households in which the head is linked to person-record in the ICR. Such links are consistent if the ICR person-record was also a single head or was neither head nor spouse (e.g. an adolescent living with parents); they are also consistent if the ICR person-record was part of a nuclear head-spouse pair, but the 1940 person-record is widowed or divorced. In the bottom panel: Columns 1–2 indicate that both head and spouse in a 1940 FCC household are linked to person-records in the same ICR-household but that these person-records had a relation to each other that is inconsistent with their 1940 relation. Column 3 indicates that 1940 head and spouse are both linked to person-records of the same relation but in different households. Columns 4–5 indicate that only of head or spouse are linked to person-records in the ICR, but that they are already a head/spouse.

panels depict linkages that are logically consistent, firstly for nuclear households with a head and a spouse, and secondly for single-head households. In the top panel, column 1 indicates that both head and spouse in a 1940 FCC household are linked to a head and spouse who are in the same household in the ICR. Column 2 indicates that both 1940 head and spouse are linked to persons in the same household, and that neither of these is head or spouse in the ICR. This captures the relatively common occurrence of young couples living with one spouse's parents. Column 3 indicates that 1940 head and the spouse are linked to persons in the ICR who live in different households and are neither head nor spouse in the ICR. This captures the most common occurrence of new households being formed by young adults. Columns 4–5 capture the case where only one of 1940 head or spouse is linked to a person record in the ICR, and that record is not a head or spouse in the ICR. Column 5 is common because young wives in 1940 who were living with their parents in the ICR are less likely to be linked because of their different last names. The middle panel depicts single-head households in which the head is linked to person-record in the ICR. Such links are consistent if the ICR person-record was also a single head or was neither head nor spouse (e.g. an adolescent living with parents); they are also consistent if the ICR person-record was part of a nuclear head-spouse pair, but the 1940 person-record is widowed or divorced. The bottom panel depicts linkages that appear logically inconsistent. In the bottom panel, columns 1–2 indicate that both head and spouse in a 1940 FCC household are linked to person-records in the same ICR-household but that these person-records had a relation to each other that is inconsistent with their 1940 relation. Column 3 indicates that 1940 head and spouse are both linked to person-records of the same relation but in different households. Columns 4–5 indicate that only of head or spouse are linked to person-records in the ICR, but that they are already a head/spouse.

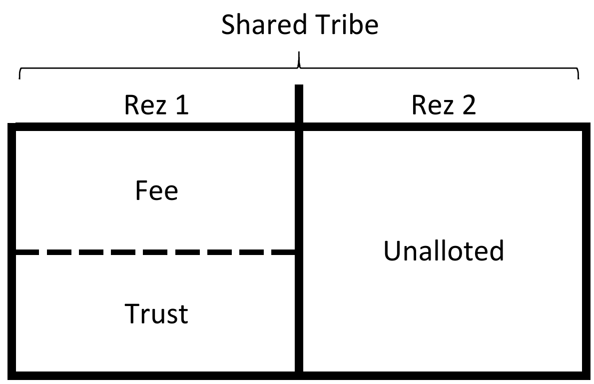
Links depicted in the bottom panel are "flagged" for potential logical inconsistency, and should be omitted from the statistical analysis until they have been manually checked and linked (or de-linked).

Previous to stages 4–5 described above, we have roughly 7,000 Native American households from the Census to the ICR that are "un-flagged", i.e. that belong to the top-two panels in Table 4.

5 Results

Indian allotment was effectively made up of two treatments, an unconditional and a conditional transfer program. Receiving an allotment, gave the allottee the unconditional right to use the land for their own purposes, as well as the right to leasing rents. The associated income stream was comparable in magnitude to average per capita incomes at this time. The second treatment—obtaining the land in 'fee simple'— was easily an order of magnitude more valuable, but to obtain it, the allottee had to be declared "competent" by the local BIA agent. Figure 2 illustrates the two treatment arms using a stylized tribe that has two reservations associated with it, one allotted and one un-allotted.

Figure 2: Treatment Arms of Indian Allotment



The first treatment arm—being allotted— is a function of reservation and tribe characteristics, but not of individual household characteristics. As such, a regression may estimate the causal effect of allotment so long as it carefully controls for tribe characteristics X_e and reservation characteristics X_r that may have influenced the decision to allot. Consider

$$Y_{i(re)} = \alpha_A^Y \cdot Allotted_i \qquad + \gamma' X_r + \beta' X_i + \mu_e + \epsilon_{i(r)}, \tag{1}$$

where $Y_{i(re)}$ denotes individual outcomes, i indexes a household, r indexes reservation and e indexes tribe. The tribe fixed effect μ_e washes out many unobservable traits that made some, for example more warlike, tribes more likely to be allotted. The inclusion of μ_e implies that tribes with a single reservation, whether allotted or no, do not contribute to the estimation of α_A^Y.[33]

[33] In equation (1), only reservations that are many-to-one mapped to tribes contribute to the estimation of α_A^Y. See

The second treatment—obtaining the land in 'fee simple'— occurs when a household was deemed "competent." As such, the second treatment is a function of household characteristics, and —given the relative dearth of household characteristics in our data— it is harder to obtain a causal estimate of it. Consider the following two estimating equations

$$Y_{i(re)} = \beta_F^Y \cdot \textit{Fee-Simple}_i + \beta_T^Y \cdot \textit{In-Trust}_i \qquad + \gamma' X_r + \beta' X_i + \mu_e + \epsilon_{i(r)} \tag{2}$$

$$Y_{i(re)} = \alpha_F^Y \cdot \textit{Fee-Simple}_i \qquad + \beta' X_i + \mu_r + \epsilon_{i(r)}, \tag{3}$$

where equation (2) includes tribe fixed effects μ_e, and equation (3) includes reservation fixed effects μ_r. Assuming—as we will in estimating equation (1)— that the reservation controls X_r mostly control for unobserved determinants of allotment, the difference in the estimated coefficients $\widehat{\beta_F^Y} - \widehat{\beta_T^Y}$ in equation (2) should be approximately equal the estimated coefficient $\widehat{\alpha_F^Y}$ in equation (3). However, neither specification resolves the endogeneity associated with which households on an allotted reservation were deemed competent and which were not. For this, we will employ an instrumental variable (IV) strategy based on the exogenous rotation of BIA agents across reservations.

5.1 Equation 1: The Effect of Being Allotted

Equation (1) estimates the effect of being allotted, comparing allotted households on allotted reservations to un-allotted households on un-allotted reservations.[34] Tribe fixed effects μ_t means we are comparing only within tribe, thus sweeping out unobserved cultural factors that may have led the government to target certain tribes for allotment. Even conditional on tribe fixed effects, however, the BIA very likely phased out allotment in a non-random order across reservations, and the characteristics that made a reservation more likely to be allotted are unlikely to have been econometrically exogenous. Fortunately, the detailed analysis of Indian allotment in Leonard et al. (2018) provides us with clear guidance on the determinants of allotment and the control variables X_r that are needed to control for selection into being allotted. Equation (3) breaks the allotment

Dippel (2014) and Feir et al. (2019) for studies that use tribe fixed effects to identify reservation treatments.

[34] In practice, we find in the ICR that about ten percent of households on allotted reservations are unallotted, and we find some allotted households on unallotted reservations. We conjecture that these may be individuals with tribal membership in more than one reservation, who obtain the right to claim an allotment in one reservation but then chose an allotment inside another We omit un-allotted households on allotted reservations and allotted households on un-allotted reservations from the analysis.

effect down by whether allottees received their land in fee simple. To gain identification on the effect of the second treatment of obtaining the land in fee simple, we will pursue the IV strategy developed in Section 5.4. However, the instrument is only defined conditional on allotment so that we can only identify α_F^Y, the effect of fee simple land, in allotted reservations. Equation (3) with reservation fixed effects will therefore be the second stage estimating equation for our IV strategy.

Table 5: Effect of Allotment on Indian Farming

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
outcome:				D(HH lives on Farm)			
Allotted Household	0.371***	0.205***	0.206***	0.194***	0.259***	0.248***	0.203**
	[0.000]	[0.003]	[0.005]	[0.006]	[0.001]	[0.005]	[0.027]
R-squared	0.224	0.230	0.232	0.237	0.252	0.301	0.305
outcome:				Occupation: Farmer			
Allotted Household	0.151***	0.018	0.012	0.006	0.065	0.061	0.055
	[0.000]	[0.711]	[0.826]	[0.919]	[0.106]	[0.271]	[0.346]
R-squared	0.237	0.237	0.238	0.239	0.248	0.283	0.283
outcome:				D(Own Dwelling)			
Allotted Household	-0.001	0.140**	0.132**	0.148**	0.162**	0.183*	0.198*
	[0.952]	[0.034]	[0.040]	[0.022]	[0.017]	[0.068]	[0.059]
R-squared	0.124	0.123	0.126	0.128	0.134	0.162	0.164
outcome:				D(Own Dwelling) | D(HH lives on Farm)			
Allotted Household	-0.070***	0.098	0.091	0.109	0.110	0.127	0.152
	[0.001]	[0.162]	[0.184]	[0.109]	[0.129]	[0.203]	[0.141]
R-squared	0.164	0.167	0.170	0.171	0.177	0.210	0.213
Ctrls		+ LPA Ctrls	+Env. Pop	+Env. Agric	+Env. Income	+Env. Manuf	+Env. Finance
Observations	6,839	6,175	6,175	6,175	5,961	3,470	3,470

Notes: This table reports on an estimation of specification 1 for farming as the outcome. The regression is run on household heads only because farm-status is a household characteristic in the Census. As base-controls, We always control for age and age squared of the head, and we always include fixed effects for household size, for being a single-parent household, and for being a "young household."[35] In column 2, we add the reservation-controls from Leonard et al. (2018). In columns 3–7, we cumulatively add additional county-aggregate controls that are mostly Census-based. Coefficients on these control variables are reported in Online Appendix Table 1. Standard errors are clustered at the reservation-level, *p-values* are reported in square brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 5 reports on an estimation of specification 1 for farming as the outcome. The regression is run on household heads only because farm-status is a household characteristic in the Census. As base-controls, We always control for age and age squared of the head, and we always include fixed effects for household size, for being a single-parent household, and for being a "young household."[36] Using only these base-controls, an allotted household is 37 percent more likely to live on

[36] Young in the sense that head and spouse in the 1940 households were still adolescent children living with their

a farm, compared to households of the same tribe on an unallotted reservation. In column 2, we add the reservation-controls from Leonard et al. (2018). Their inclusion cuts the baseline estimate almost in half to 20 percent. This is consistent with the existing evidence that reservations with better land and resources were selected for allotment first. In columns 3–7, we cumulatively add additional county-aggregate controls that are mostly Census-based. The manufacturing controls are mostly missing in rural counties so that the number of observations drops in columns 6–7. The coefficient across 3–7 is fairly stable however. Given this stability, we view column 2 as our preferred specification since the additional controls in columns 3–7, especially those in columns 3–4, are likely to have themselves been affected by allotment, and are thus subject to the concern of being "bad controls."

The second panel of Table 5 presents results where the outcome of interest is a binary measure, for whether the household head's primary occupation is farming.[37] Despite the increased likelihood of living on a farm, household heads on allotted reservations were not more likely to be engaged in farming as an occupation. This is consistent with allottees leasing out their allotments as discussed by Carlson (1981).

The third panel shows evidence of allotment being associated with a higher probability of home ownership. The fourth panel shows this is mostly explained by households' higher probability of living on a farm.

Table 6 reports the effects of allotment on labor market outcomes of the household head and spouse in separate panels. Each panel presents specification 1 under three different sets of covariates. The top set of results report the baseline specification, the middle set adds a control for whether the household is living on a farm, and the final set adds the reservation-controls from Leonard et al. (2018). Our preferred specification includes both the farm control, which allows us to separate where the individual lives from their labor market experiences, and the full set of reservation-level allotment adoption controls, to address the selection into allotment. Each column presents a different labor market outcome.

For household heads, the results in columns 1–3 suggest allotment induced very minor changes to an individual's participation in the labor market. We find statistically insignificant changes in

parents in the ICR.

[37] We replicated this panel with a set of specifications based on employment industry instead of occupation and found similar results.

Table 6: Labor Market Outcomes of Parents

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
outcomes:	D(LFP)	#weeks worked	D(employed)	Weekly Wage	Occup. Income Score	Occup. Earnings score (1950 base)	Nam-Powers-Boyd Occ Status Score	Duncan Socioec. Index
Panel A: Heads	+ LAP Ctrls							
Allotted	-0.076	5.866	-0.011	3.666**	2.744**	8.487**	9.129**	7.410***
	[0.179]	[0.123]	[0.848]	[0.025]	[0.016]	[0.015]	[0.010]	[0.004]
Observations	6,175	6,175	4,228	2,394	4,167	4,167	4,167	4,167
R-squared	0.374	0.262	0.264	0.046	0.100	0.146	0.088	0.053
	+ LAP Ctrls + Farm-Ctrl							
Allotted	-0.087	5.181	0.060	4.835***	3.698***	12.328***	11.724***	8.113***
	[0.134]	[0.184]	[0.489]	[0.005]	[0.002]	[0.001]	[0.001]	[0.001]
D(Farm HH)	0.057***	3.514***	-0.477***	-3.519***	-5.025***	-20.182***	-13.660***	-3.808***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Observations	6,175	6,175	4,228	2,394	4,167	4,167	4,167	4,167
R-squared	0.376	0.267	0.435	0.058	0.198	0.284	0.178	0.068
Panel B: Spouses	+ LAP Ctrls							
Allotted	-0.083***	-2.799						
	[0.000]	[0.300]						
Observations	3,944	3,944	770	97	197	197	197	197
R-squared	0.081	0.065	0.347	0.341	0.432	0.375	0.345	0.310
	+ LAP Ctrls + Farm-Ctrl							
Allotted	-0.094***	-2.726						
	[0.000]	[0.328]						
D(Farm HH)	-0.044***	0.250	-0.196***	-3.408*	0.288	1.897	0.395	-5.119*
	[0.000]	[0.751]	[0.001]	[0.060]	[0.863]	[0.689]	[0.937]	[0.067]
Observations	3,944	3,944	770	97	197	197	197	197
R-squared	0.088	0.065	0.393	0.359	0.424	0.371	0.340	0.319

Notes: Standard errors are clustered at the reservation-level, *p-values* are reported in square brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Labor Market Outcomes of Children

outcomes:	(1) D(LFP)	(2) #weeks worked	(3) D(employed)	(4) Weekly Wage	(5) Occup. Income Score	(6) Occup. Earnings score (1950 base)	(7) Nam-Powers-Boyd Occ Status Score	(8) Duncan Socioec. Index
Panel A: Sons	+ LAP Ctrls							
Allotted	0.004	1.530	-0.194	3.449***	-0.281	1.454	0.637	0.506
	[0.902]	[0.223]	[0.195]	[0.004]	[0.889]	[0.788]	[0.866]	[0.818]
Observations	7,639	7,639	1,476	945	1,412	1,412	1,412	1,412
R-squared	0.517	0.336	0.225	0.097	0.211	0.200	0.144	0.081
	+ LAP Ctrls + Farm-Ctrl							
Allotted	-0.005	1.456	-0.192	3.643***	-0.270	1.453	0.598	0.526
	[0.926]	[0.506]	[0.201]	[0.002]	[0.887]	[0.777]	[0.872]	[0.809]
D(Farm HH)	0.028***	1.801***	-0.251***	-1.792**	-5.156***	-14.738***	-8.682***	-0.720
	[0.002]	[0.000]	[0.000]	[0.031]	[0.000]	[0.000]	[0.000]	[0.353]
Observations	7,639	7,639	1,476	945	1,412	1,412	1,412	1,412
R-squared	0.437	0.280	0.272	0.101	0.279	0.267	0.185	0.082
Panel B: Daughters	+ LAP Ctrls							
Allotted	0.002	2.081*	0.100	-4.947	15.293***	36.687***	40.044***	21.520***
	[0.923]	[0.055]	[0.731]	[0.179]	[0.000]	[0.000]	[0.000]	[0.000]
Observations	7,237	7,237	558	207	308	308	308	308
R-squared	0.150	0.116	0.348	0.383	0.218	0.204	0.221	0.246
	+ LAP Ctrls + Farm-Ctrl							
Allotted	0.013	2.543**	0.041	-5.523	14.768***	35.723***	38.425***	22.564***
	[0.604]	[0.021]	[0.884]	[0.150]	[0.000]	[0.000]	[0.000]	[0.000]
D(Farm HH)	-0.008	0.333	-0.122**	-1.147	-0.527	-1.474	-2.411	1.736
	[0.201]	[0.342]	[0.035]	[0.293]	[0.813]	[0.796]	[0.721]	[0.680]
Observations	7,237	7,237	558	207	308	308	308	308
R-squared	0.128	0.094	0.356	0.386	0.216	0.204	0.222	0.248

Notes: Standard errors are clustered at the reservation-level, *p-values* are reported in square brackets. *** p<0.01, ** p<0.05, * p<0.1.

labor force participation, the number of weeks worked, and whether the individual worked for a salary or wages. In columns 4–8, the results indicate that there was occupational upgrading following allotment. Individuals earned higher weekly wages, and consistently improved their standing across the set of occupation and income related indices. We consider four occupational rank indices provided by the census: an Occupational Income Score, and Occupational Earnings Score with a 1950 base, the Nam-Powers-Boyd Occupational Status Score, and and the Duncan Socioeconomic Index.[38] Panel B reports the same set of findings for spouses. Spouses have large and significant decreases in labor force participation, falling by over 9 percentage points. When considered relative to spousal labor force participation, this is nearly 0.5 standard deviations. Within this population, spousal labor force participation rates are extremely low, so we are cautious to over interpret results based on the sample of working spouses.

We extend the labor market findings to the children living within the set of households examined previously. Table 7 reports the labor market results separately for sons and daughters split across two panels. Similar to the household head and spouse table, within each panel we include three specifications with varying controls. Once we condition on both farm status and the allotment decision controls, we find limited affects of allotment on the labor market decisions of sons. There is some evidence of higher weekly pay, but we don't find commensurate improvements in the occupation or income indices. Daughters in allotted households experienced a slight increase in the number of weeks worked, but consistently large increases in the occupation and income indices. Given that we don't observe increased weekly wages, we interpret the increases across occupational indices as evidence of occupational upgrading.

Tables 8 and 9 explore the effect of allotment on the accumulation of education for both adults and children. For adults, we estimate equation 1 for three measures of education attainment, years of accumulated education, an indicator for completed primary school, and an indicator for attending some high school. Panel A of Table 8 presents results for household heads across for each outcome of interest across our three standard specifications. Across our three measures of educational attainment we find some evidence of higher levels of attainment on allotted reservations relative to unallotted reservations. For household heads, this effect disappears once we control for

[38]Comparing the results with and without reservation allotment controls, indicates that excluding the controls exposes a negative bias between labor markets and allotment. This is consistent with allotment being targeted to agriculturally abundant locations, which may have had less developed labor markets.

Table 8: Education of Parents

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
outcomes:	Educ. Rank (1-14)			Educ: D(>Primary)			Educ: D(High Sch)		
Panel A: Heads									
Allotted	2.143***	0.497	0.665	0.218***	-0.036	-0.017	0.065***	0.005	0.020
	[0.000]	[0.334]	[0.208]	[0.000]	[0.645]	[0.828]	[0.000]	[0.898]	[0.601]
D(Farm HH)			-0.846***			-0.081***			-0.080***
			[0.000]			[0.000]			[0.000]
Ctrls for Sel. Allotment		✓	✓		✓	✓		✓	✓
Observations	6,666	6,052	6,052	6,666	6,052	6,052	6,666	6,052	6,052
R-squared	0.360	0.347	0.357	0.271	0.268	0.273	0.140	0.140	0.148
Panel B: Spouses									
Allotted	2.775***	1.691***	1.544**	0.314***	0.124*	0.106*	0.079***	0.307***	0.292***
	[0.000]	[0.006]	[0.010]	[0.000]	[0.060]	[0.097]	[0.001]	[0.000]	[0.000]
D(Farm HH)			-0.607***			-0.061**			-0.068***
			[0.002]			[0.020]			[0.000]
Ctrls for Sel. Allotment		✓	✓		✓	✓		✓	✓
Observations	4,240	3,895	3,895	4,240	3,895	3,895	4,240	3,895	3,895
R-squared	0.381	0.362	0.368	0.290	0.279	0.279	0.153	0.154	0.160

Notes: Standard errors are clustered at the reservation-level, *p-values* are reported in square brackets. *** p<0.01, ** p<0.05, * p<0.1.

the reservation-controls from Leonard et al. (2018), which are thus again proving to be powerful determinants of household characteristics. For spouses, the reservation controls shrink the effect of allotment but do not depress them to zero. Finally, and importantly, the results are robust to including the important indicator for farm status (columns 3, 6, and 9). In Panel B, spouses on allotted reservations have an additional 1.5 years of completed education. The results in columns 6 and 9 suggest these differences are due to both a larger proportion completing primary school and entering high school.

We repeat a similar analysis for children in Table 9. Similar to the adults table, we examine the effect of allotment on educational attainment in years, and indicators for completing primary school and attaining high school. Considering that this population may still be completing their education, we also consider a whether the child is currently attending school as a fourth education outcome. Panel A focuses on the sample of sons and Panel B restricts the sons sample to those currently attending school. Both panels show higher levels of educational attainment for sons on allotted reservations with the effects being stronger for those still in school. For the full sample in Panel A, the difference in attainment is driven by higher rates of completing primary school, but we don't see strong differences for attending high school. For the sample attending school, higher

Table 9: Education of Children

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
outcomes:		Educ. Rank (1-14)			Educ: D(>Primary)		Educ:	D(High Sch)			D(School)	
Panel A: Sons												
Allotted	1.697***	0.646*	0.602*	0.182***	0.081*	0.076**	0.152***	0.001	-0.009	0.076**	0.022	0.040
	[0.000]	[0.068]	[0.077]	[0.000]	[0.090]	[0.042]	[0.000]	[0.986]	[0.742]	[0.014]	[0.738]	[0.621]
D(Farm HH)			-0.259***			-0.020**			-0.046***			-0.052***
			[0.002]			[0.023]			[0.000]			[0.000]
Ctrls for Sel. Allotment		✓	✓		✓	✓		✓	✓		✓	✓
Observations	8,088	7,386	7,386	8,088	7,386	7,386	8,088	7,386	7,386	8,383	7,639	7,639
R-squared	0.687	0.698	0.698	0.530	0.546	0.542	0.193	0.215	0.202	0.190	0.412	0.193
conditional on being in School												
Allotted	1.302***	0.907**	1.052***	0.116***	0.034	0.053	0.108***	0.057	0.102***			
	[0.000]	[0.015]	[0.005]	[0.001]	[0.508]	[0.280]	[0.000]	[0.101]	[0.004]			
D(Farm HH)			-0.271***			-0.040***			-0.038***			
			[0.000]			[0.001]			[0.001]			
Ctrls for Sel. Allotment		✓	✓		✓	✓		✓	✓			
Observations	3,834	3,549	3,549	3,834	3,549	3,549	3,834	3,549	3,549			
R-squared	0.771	0.788	0.771	0.559	0.573	0.558	0.395	0.504	0.402			
Panel B: Daughters												
Allotted	1.659***	0.326	0.370	0.171***	0.005	0.019	0.100***	0.105***	0.127***	0.036***	0.142***	0.056*
	[0.000]	[0.210]	[0.143]	[0.000]	[0.881]	[0.525]	[0.001]	[0.002]	[0.000]	[0.000]	[0.000]	[0.077]
D(Farm HH)			-0.105			-0.013			-0.002			-0.043***
			[0.241]			[0.254]			[0.868]			[0.002]
Ctrls for Sel. Allotment			✓			✓			✓			✓
Observations	7,613	6,950	6,950	7,613	6,950	6,950	7,613	6,950	6,950	7,940	7,237	7,237
R-squared	0.697	0.716	0.715	0.533	0.554	0.548	0.212	0.249	0.222	0.207	0.436	0.220
conditional on being in School												
Allotted	1.023***	0.854***	0.763**	0.139***	0.078**	0.054	0.036*	0.117***	0.106***			
	[0.000]	[0.010]	[0.032]	[0.000]	[0.036]	[0.182]	[0.099]	[0.000]	[0.000]			
D(Farm HH)			-0.181*			-0.037**			-0.004			
			[0.070]			[0.011]			[0.672]			
Ctrls for Sel. Allotment			✓			✓			✓			
Observations	3,927	3,600	3,600	3,927	3,600	3,600	3,927	3,600	3,600			
R-squared	0.783	0.793	0.784	0.574	0.603	0.575	0.444	0.466	0.454			

Notes: Standard errors are clustered at the reservation-level, *p-values* are reported in square brackets. *** p<0.01, ** p<0.05, * p<0.1.

rates of high school attendance appear to be dictating the differences.

Panels C and D repeat the prior analysis focusing on the sample of daughters. From columns 1–3, the results for girls are very similar to boys. They show slight differences in educational attainment, with the effects being driven by the sample that is still attending school. Where differences emerge between sons and daughters is at the level of attainment where these differences are occurring. For sons, the differences were most pronounced at the level of primary education, whereas daughters experience larger differences for attending high school. When we focus our sample on daughters attending school in Panel D, we find the differences across the outcomes between allotted and unallotted reservations are very similar in magnitude between sons and daughters.

In summary, the evidence in Section 5.1 suggests that allotment failed to deliver in its objective of "turning Indians into farmers." Allottees were more likely to live on farms, but were not more likely to actually be engaged in farming as a primary occupation. The policy also had impacts on labor markets, but these impacts are only partly explained by the shift towards farming, and in the aggregate tend to have the opposite sign of what a switch towards farming would suggest: we find evidence of higher wages, and for switching towards higher-earning occupations. Controlling for farm status only strengthens these effects. These patterns are consistent with the idea that the cash transfers associated with allotment may have changed reservation wages, recalling that the first treatment arm generated leasing rents when the allottee did not work the land as a farmer, and the second treatment arm earned a large cash transfer when the allottee did not work the land as a farmer and instead sold the fee simple land.

We also find strong effects on education in Tables 8, 9. These are the most surprising because education was not targeted by allotment did not directly target education.[39]

[39] While the federal government had tried assimilationist educational policies in the form of off-reservation boarding schools, their emergence in 1879 pre-dated the thrust of allotment by almost three decades (Gregg, 2018). As early as 1891, parents had to give their "full consent" for a child to be sent to a boarding school (Adams 1995, 65, Lomawaima 1995, 36)

5.2 Education as a Signal of Assimilation?

One possible explanation for the education effects of allotment in Tables 8, 9 is that the cash transfers associated with both transfers relaxed credit constraints that impacted education. Prima face, this seems unlikely in our setting: by around 1910 primary and middle school were fully provided on reservations, and Indian parents who did not want to send their kids to a boarding high school, could instead send to a local (mostly non-Indian) public one (Hoxie, 2001, 208). Another possibility could be that education was a correlate of turning Indians into farmers, but our data shows that farming households sent their kids to school less, not more. Instead, we interpret the education effect we find as allottees signalling their assimilation in order to improve their chances of being declared competent by their local BIA agents.

This interpretation is consistent with several pieces of evidence that we observe in the data, and which we present in the following sub-sections 5.2.1–5.2.3: First, when we decompose the allotment effect into households that obtained their land in fee simple and households whose allotments stayed in trust, we find a differential effect between those two. This is reported in Section 5.2.1. This differential effect is consistent with our interpretation. It shows that higher educational attainment was associated with a higher likelihood of obtaining land in fee simple. However, we recognize that this finding is equally consistent with two other stories: For one, it may be that education was merely a correlate of other factors that increased the likelihood of competency, and that more assimilated households obtained more education for reasons unrelated to signalling. For another, it may be that obtaining the land in fee simple itself changed households' incentives to obtain more education.[40]

We have two ways to provide more direct evidence of the 'education as a signal for assimilation' hypothesis. In Section 5.2.2, we construct a repeated cross-section of the entire Native American population in the four Population Census waves 1910–1940, and verify that school attendance increased within reservations over time with the expansion of allotments (as did farming).[41] In Section 5.2.3, we peruse information on the issuance-date of individual allotments and the date when (if ever) an allotment was transferred to fee simple. This allows us to pursue a cohort anal-

[40] We will get at this second story directly when we use an IV strategy to estimate the causal effect of obtaining the land in fee simple.

[41] Educational attainment is only reported in the 1940 Census so that we can only consider school attendance in previous years.

ysis, where we identify an *age-household*-specific effect of the inventive to signal assimilation, and find that individuals within a household obtained significantly more education if their school-age years were characterized by living in an allotted household whose allotment had not yet been transferred into fee simple.

5.2.1 Decomposition of the Allotment Effect

Among adults, Table 8 documented subtle differences in educational attainment among household heads and strong increases in attainment among spouses in allotted reservations relative to unallotted reservations. By estimating specifications 2 and 3, we can decompose the allotment effect between households that obtained their land in fee simple and households whose allotments stayed in trust. Table 10 reports results for household heads and spouses in separate panels, across three different outcomes. The results for both heads and spouses signal a strong difference in educational attainment by land tenure status. Heads of household receiving fee-simple title have an additional 1.8 years of education relative to unallotted reservations. This difference is much less pronounced for heads with in-trust patents. Turning to the within reservation specifications in columns 2 and 3, we see fee-simple title is associated with over an additional year of education relative to in-trust household heads.

The spouse results in Panel B are similar. For spouses, both types of patents are associated with more educational attainment, but there is a larger gap in attainment for spouses in fee-simple households. Across both heads and spouses, we observe more individuals completing primary school and attending some high school (col 4–9).

Table 11 reports a similar set of education results for children. The structure of the table follows Table 9, but with results from specifications 2 and 3. Consistent with our findings for adults, children in fee-simple households experienced a larger increase in educational attainment relative to children in in-trust households. The point estimates are similar for sons and daughters, both in attainment and in the likelihood of completing primary school or having enrolled in high school. We find limited evidence of differences in whether children are currently enrolled in school in 1940 by allotment groups.

By distinguishing fee-simple patents from in-trust patents we can observe strong differences in the effect of each patent type of households and individuals. Focusing on the fee-simple to in-trust comparison, at the household level, we observe a strong shifts away from farming and large increases in property values. Within the household, household heads and sons, saw similar benefits in the labor market. Both groups experienced wage increases and improvements in occupational standing, but heads saw additional improvements in the number of weeks worked. Spouses and daughters also shared similar labor market changes. Both experienced large increases the number of weeks worked, and spouses saw a slight shift in employment towards wage and salary occupations. For this group, the fee-simple to in-trust comparison within reservations did not generate significant differences for labor force participation, wages, or the set of occupation and income related indices.

The most consistent set of differences associated with allotment are in educational attainment. Both adults and children experienced large increases in attainment, when comparing allotted to unallotted reservations. When we partition the allotments into fee-simple and in-trust patents, we again see large differences between the two types of patentees within the same reservation.

Table 10: Education of Parents, split by Property Rights

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
outcomes:	Educ. Rank (1-14)			Educ: D(>Primary)			Educ: D(High Sch)		
Panel A: Heads									
Fee-Simple	1.817***	1.192***	1.141***	0.105	0.123***	0.119***	0.081**	0.065***	0.060***
	[0.001]	[0.000]	[0.000]	[0.189]	[0.000]	[0.000]	[0.036]	[0.000]	[0.000]
In-Trust	0.608			-0.023			0.017		
	[0.221]			[0.762]			[0.661]		
Observations	6,052	6,679	6,679	6,052	6,679	6,679	6,052	6,679	6,679
R-squared	0.372	0.384	0.392	0.282	0.288	0.292	0.152	0.151	0.160
Panel B: Spouses									
Fee-Simple	2.498***	0.881***	0.851***	0.203***	0.085***	0.083***	0.371***	0.069***	0.066***
	[0.000]	[0.000]	[0.000]	[0.002]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
In-Trust	1.590***			0.111*			0.296***		
	[0.004]			[0.057]			[0.000]		
Observations	3,895	4,234	4,234	3,895	4,234	4,234	3,895	4,234	4,234
R-squared	0.377	0.400	0.405	0.284	0.306	0.307	0.166	0.166	0.172
Farming Ctrl	✓		✓	✓		✓	✓		✓
Ctrls for Sel. Allotment	✓			✓			✓		
reservation FE		✓	✓		✓	✓		✓	✓

Notes: This table reports on the results of estimating equations (2) and (3), where the allotment indicator is split into allotments that were transferred into fee simple and those that remained in trust. The table is structured into blocs of 3 columns in a manner equivalent to Table 8, but differs with respect to the variation in controls and fixed effects applied within bloc. Standard errors are clustered at the reservation-level, *p-values* are reported in square brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 11: Education of Children, split by Property Rights

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
		Educ. Rank (1-14)			Educ: D(>Primary)		Educ:	D(High Sch)			D(School)	
Panel A: Sons												
Fee-Simple	1.097***	0.508***	0.504***	0.126***	0.051***	0.050***	0.043	0.055***	0.054***	0.035	0.001	0.000
	[0.002]	[0.000]	[0.000]	[0.002]	[0.001]	[0.001]	[0.151]	[0.000]	[0.000]	[0.667]	[0.940]	[0.991]
In-Trust	0.606*			0.077**			-0.008			0.040		
	[0.075]			[0.042]			[0.760]			[0.621]		
Observations	7,386	8,099	8,099	7,386	8,099	8,099	7,386	8,099	8,099	7,639	8,394	8,394
R-squared	0.700	0.691	0.692	0.544	0.533	0.533	0.204	0.199	0.202	0.193	0.194	0.196
conditional on being in School												
Fee-Simple	1.442***	0.390***	0.388***	0.110**	0.060***	0.060***	0.119***	0.017	0.016*			
	[0.000]	[0.000]	[0.000]	[0.046]	[0.003]	[0.003]	[0.002]	[0.104]	[0.090]			
In-Trust	1.055***			0.054			0.102***					
	[0.004]			[0.288]			[0.004]					
Observations	3,549	3,841	3,841	3,549	3,841	3,841	3,549	3,841	3,841			
R-squared	0.773	0.775	0.777	0.560	0.564	0.565	0.403	0.401	0.403			
Panel B: Daughters												
Fee-Simple	0.875***	0.489***	0.488***	0.081**	0.060***	0.060***	0.179***	0.049***	0.049***	0.035	-0.016	-0.016
	[0.001]	[0.000]	[0.000]	[0.010]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.268]	[0.154]	[0.128]
In-Trust	0.392			0.022			0.130***			0.055*		
	[0.104]			[0.452]			[0.000]			[0.080]		
Observations	6,950	7,616	7,616	6,950	7,616	7,616	6,950	7,616	7,616	7,237	7,943	7,943
R-squared	0.717	0.701	0.701	0.550	0.538	0.538	0.225	0.222	0.222	0.220	0.214	0.215
conditional on being in School												
Fee-Simple	1.112***	0.353***	0.354***	0.100**	0.050***	0.050***	0.127***	0.022*	0.022*			
	[0.003]	[0.000]	[0.000]	[0.021]	[0.001]	[0.001]	[0.000]	[0.089]	[0.089]			
In-Trust	0.757**			0.053			0.105***					
	[0.029]			[0.179]			[0.000]					
Observations	3,600	3,928	3,928	3,600	3,928	3,928	3,600	3,928	3,928			
R-squared	0.785	0.787	0.787	0.577	0.579	0.580	0.454	0.450	0.450			
Farming Ctrl	✓		✓	✓		✓	✓		✓	✓		✓
Ctrls for Sel. Allotment	✓			✓			✓			✓		
reservation FE		✓	✓		✓	✓		✓	✓		✓	✓

Notes: This table reports on the results of estimating equations (2) and (3), where the allotment indicator is split into allotments that were transferred into fee simple and those that remained in trust. The table is structured into blocs of 3 columns in a manner equivalent to Table 9, but differs with respect to the variation in controls and fixed effects applied within bloc. Standard errors are clustered at the reservation-level, *p-values* are reported in square brackets. *** p<0.01, ** p<0.05, * p<0.1.

5.2.2 Evidence from 1910–1940 Repeat Cross-Section

If our interpretation of 'education as a signal for assimilation' is correct, then schooling and farming should have expanded together as allotment was phased out across reservations. This cannot be checked in the 1940 Census no its own, since there is no information on when a household began farming or when education was attained. We can, however, construct a repeated cross-section of the entire Native American population in the four Population Census waves 1910–1940, and the choice to be a farmer and as well as children's school attendance in this data.[42]

Allotment is measured with far more noise in this panel data-set, without the ICR-link, the Population Census includes no reservation/tribe information. The best information available to assign individuals to a reservation is county.[43] Inferring reservation only from county introduces considerable measurement error. This can be seen in Online Appendix Table 2, where we re-run our core farming and schooling results and obtain estimates that are considerably less precise than in Tables 5 and 9.

With this caveat, we run the following fixed effect panel estimation,

$$Y_{i(rt)} = \alpha^{Y}_{Ar} \cdot \#\,\mathit{Allottments}_{rt} \qquad\qquad + \beta' X_i + \mu_r + \mu_t + \epsilon_{i(rt)}, \tag{4}$$

where the coefficient of interest α^{Y}_{Ar} estimates the effect of the phasing out of allotments in a reservation over time. The treatment # $\mathit{Allottments}_{rt}$ is a year-varying reservation aggregate. The outcomes $Y_{i(rt)}$ we consider are indicators for farming and for schooling in reservations.

Table 12 reports the results. First, we verify our first finding that allotments promoted Indians living on farms. This is shown in columns 1–2 In columns 3–6, we turn our attention to schooling choices, conditional on age. We find that school attendance increased within reservations over time with the expansion of allotments, conditional on either flexible age polynomials or age fixed effects. The effects are not very precisely estimated when reservation fixed effects are included, but given the noisiness of the data used in this section —demonstrated in Online Appendix Table 2— this is to be expected. As well, the point estimates with reservation fixed effects are almost identical to those with tribe fixed effects. In all columns, a doubling of the number of allotments

[42] Educational attainment is only reported in the 1940 Census so that we can only consider school attendance in previous years.

[43] See also discussion in Section 3.4.

over time increases the probability of a child between age 5 and 14 of attending school by half a percent. Cumulatively, these coefficients may explain a large part of the secular increase in school attendance documented in the BIA Annual Reports from around 1910 on; see Online Appendix Figure 3.

Table 12: Panel Estimation of Response of Farming and Education to the Allotment Process

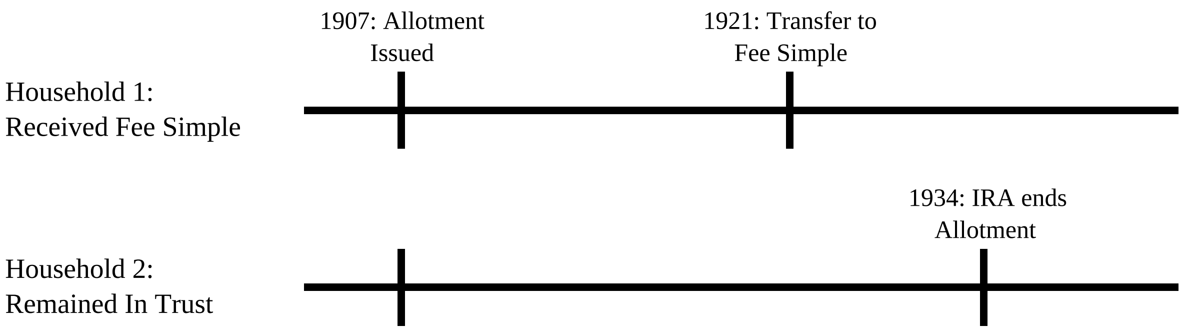
	(1)	(2)	(3)	(4)	(5)	(6)
	D(Farm)		D(school)			
Sample	HH-heads all ages		Age 5-10		Age 5-14	
ihs(Rez's #Allotments$_t$)	2.071***	2.334**	0.651***	0.654*	0.496**	0.526
	[0.000]	[0.040]	[0.004]	[0.078]	[0.035]	[0.152]
Tribe + State FE	✓		✓		✓	
Rez-FE		✓		✓		✓
Year-FE	✓	✓	✓	✓	✓	✓
Ctrl's	Age		age-FE + sex		age-FE + sex	
Observations	206,641	219,210	91,558	98,510	145,416	156,554
R-squared	0.147	0.176	0.372	0.373	0.400	0.399

Notes: Standard errors are clustered at the reservation-decade-level, *p-values* are reported in square brackets. *** p<0.01, ** p<0.05, * p<0.1.

5.2.3 Evidence from Cohort Analysis

An alternative approach to support our hypothesis that individuals used educational attainment to signal readiness for a fee-simple patent is through a direct comparison of the incentive to signal. Figure 3 illustrates a case where two households receive trust patents at the similar time, marking the period where they now have an incentive to pursued the local agent they are ready to have their patent converted to fee-simple. For Household 1, their incentive to signal stops once the fee patent is issued. Household 2 has a longer period with the incentive to signal, which stops in 1934 with the Indian Reorganization Act. If the incentive to signal is responsible for the changes in educational attainment we observed, we would expect households to accumulate more education during the periods they have the incentive to signal.

Figure 3: Cohort-Analysis of Incentive to 'Signal Assimilation'



Notes: The figure depicts two households that receive allotments in the same year. At this point, the incentive to signal assimilation begins. Household 1 is declared competent and receives their allotment in fee simple in 1921. At this point, the incentives to signal assimilation ends. Household 2's allotment remains in trust, and the incentive to signal assimilation ends only in 1934, when the IRA ends transfers into Fee Simple.

To test this, we run the following cohort analysis:

$$Y_{i(re)} = \alpha_S^Y \cdot \textit{Incentive-to-Signal}_i \qquad + \alpha_F \cdot \textit{Fee-Simple}_i + \beta' X_i + \mu_r + \epsilon_{i(r)} \tag{5}$$

$$Y_{i(re)} = \alpha_S^Y \cdot \textit{Incentive-to-Signal}_i \qquad + \beta' X_i + \mu_{HH} + \epsilon_{i(r)}, \tag{6}$$

where $\textit{Incentive-to-Signal}_i$ is an indicator variable that takes value 1 for individuals who were of school age (or of a more narrowly defined age window) at the time their household had been allotted but the allotment not yet transferred to fee simple. $\textit{Incentive-to-Signal}_i$ takes value 0 for all individuals in unallotted households, and all individuals who were of school age before their household was allotted, or after their household's allotment went into fee simple, or after 1934 when the IRA ended allotment. Specification (5) includes a reservation fixed effect μ_r, and thus compares individuals of all ages (above age 5 in 1940) and both genders within a reservation. *Incentive-to-Signal*$_i$ varies with individual i's age at the time of allotment. Specification (6) replace μ_r with a household fixed effect μ_{HH}, thus comparing only different cohorts within a household. Table 13 reports the results, for four different outcome measures. The first in each bloc of five columns estimates specification (5) with reservation fixed effects. The remaining columns 2–5 and 7–10 estimate specification (6).

In the top-two panels of the table, *Incentive-to-Signal*$_i$ is an *indicator* for whether an individual (or, at young ages, whether their parents) had the incentive to signal assimilation; i.e. whether their household had been allotted but their allotment not been transferred. In columns 1–3, we include this indicator for the year in which i was age 6, i.e. would have begun any formal schooling. In column 1, we include reservation fixed effects μ_r. We also include the core regressor of interest, whether an allotted household had received their allotment in fee simple. We include this to control for the selection on unobservables associated with the transfer to fee simple. The estimated effect α_S^Y of having an incentive to signal assimilation is economically large: it is three-quarters of the baseline estimated coefficient of being in fee simple (0.795 relative to 1.135). An alternative way of controlling for selection into education in this setup is to include household fixed effects, which we do in columns 2–5. In columns 3–5, we also include age (equivalent to year-of-birth) fixed effects (instead of an age polynomial) to better control for the mechanical relationship between age and educational attainment. In columns 4–5, we additionally include indicators for the threshold ages of transitioning to middle school (age 10) and high school (age 14). In the bottom two panels, we replace indicators for incentives at ages 6, 10, and 14 with the share of years of a certain school age range in which an individual had incentives to signal assimilation. For example, for a household which received their allotment before child i was of age 6, and had it transferred to fee simple when i was 8, 'D(Incentive to Assimilate at Primary School Age)' would be 1, and 'Share Years Incentive Assimilate: Primary School Age' would be $(8-6)/4 = 0.5$.

Across the education outcomes and different specifications, Table 13 shows a strong relationship between whether an individual has the incentive to signal and that individual having higher educational attainment. The within-household comparison with year-of-birth fixed effects in particular provides fine-grained evidence in support of the hypothesis that that households responded to allotment by sending their kids to school more.

Table 13: Cohort-Analysis: Incentive to Use Education as Assimilation Signal

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
outcomes Set 1:	Educ. Rank (1-14)					Educ: D(Any School)				
D(Incentive to Assimilate	0.795***	0.786***	0.499***	0.321***	0.349***	0.038***	0.035***	0.028***	0.015	0.017
… Primary School Age (6)	[0.000]	[0.000]	[0.000]	[0.001]	[0.001]	[0.001]	[0.002]	[0.005]	[0.169]	[0.101]
D(Incentive to Assimilate				0.336*	0.148				0.024**	0.011
… Middle School Age (10)				[0.084]	[0.257]				[0.016]	[0.244]
D(Incentive to Assimilate					0.328*					0.023
… High School Age (14)					[0.058]					[0.156]
Fee-Simple	1.135***					0.054***				
	[0.000]					[0.000]				
D(Farm HH)	-0.483***					-0.019*				
	[0.000]					[0.063]				
Gender=Female	0.212**	0.229***	0.210***	0.209***	0.212***	0.005	0.005	-0.002	-0.002	-0.002
	[0.010]	[0.003]	[0.002]	[0.002]	[0.002]	[0.499]	[0.420]	[0.701]	[0.692]	[0.718]
R-squared	0.496	0.743	0.765	0.765	0.765	0.283	0.542	0.608	0.609	0.609
outcomes Set 2:	Educ: D(>Primary)					Educ: D(High Sch)				
D(Incentive to Assimilate	0.133***	0.144***	0.065***	0.025	0.030	0.020	0.012	0.023	0.034**	0.035***
… Primary School Age (6)	[0.000]	[0.000]	[0.008]	[0.274]	[0.194]	[0.202]	[0.479]	[0.213]	[0.013]	[0.009]
D(Incentive to Assimilate				0.077***	0.041*				-0.021	-0.031
… Middle School Age (10)				[0.001]	[0.057]				[0.421]	[0.208]
D(Incentive to Assimilate					0.063**					0.017
… High School Age (14)					[0.019]					[0.226]
Fee-Simple	0.134***					0.077***				
	[0.000]					[0.000]				
D(Farm HH)	-0.047***					-0.053***				
	[0.000]					[0.000]				
Gender=Female	0.027***	0.031***	0.026***	0.025***	0.026***	0.015	0.015	0.022*	0.022*	0.022*
	[0.005]	[0.003]	[0.002]	[0.002]	[0.002]	[0.207]	[0.219]	[0.057]	[0.056]	[0.054]
R-squared	0.416	0.627	0.663	0.664	0.665	0.168	0.479	0.530	0.530	0.530
outcomes Set 1										
Share Years Incentive Assimilate:	0.862***	0.956***	0.618***	0.393***	0.487***	0.015	0.015	0.038***	0.020	0.024*
… Primary School Age (6-10)	[0.000]	[0.000]	[0.002]	[0.004]	[0.002]	[0.141]	[0.157]	[0.000]	[0.217]	[0.079]
Share Years Incentive Assimilate:				0.349	0.030				0.027	0.012
… Middle School Age (11-14)				[0.140]	[0.856]				[0.132]	[0.429]
Share Years Incentive Assimilate:					0.379*					0.018
… High School Age (15-18)					[0.090]					[0.460]
Fee-Simple	1.133***					0.046***				
	[0.000]					[0.000]				
R-squared	0.496	0.744	0.765	0.765	0.766	0.282	0.542	0.609	0.609	0.609
outcomes Set 2										
Share Years Incentive Assimilate:	0.155***	0.181***	0.095***	0.046	0.061*	0.045*	0.048*	0.020	0.035*	0.043**
… Primary School Age (6-10)	[0.000]	[0.000]	[0.002]	[0.120]	[0.068]	[0.055]	[0.060]	[0.451]	[0.092]	[0.034]
Share Years Incentive Assimilate:				0.075**	0.025				-0.023	-0.051*
… Middle School Age (11-14)				[0.012]	[0.393]				[0.360]	[0.076]
Share Years Incentive Assimilate:					0.060*					0.034*
… High School Age (15-18)					[0.086]					[0.085]
Fee-Simple	0.137***					0.084***				
	[0.000]					[0.000]				
R-squared	0.417	0.628	0.664	0.665	0.665	0.169	0.480	0.530	0.530	0.530
Age FE			✓	✓	✓			✓	✓	✓
Age-Cubic		✓					✓			
Household FE		✓	✓	✓	✓		✓	✓	✓	✓
Reservation FE	✓					✓				
Farming Ctrl	✓					✓				
Observations	21,235	20,078	20,078	20,078	20,078	21,568	20,428	20,428	20,428	20,428

Notes: This table reports on estimating specification (5) (in column 1 and 6) and specification (6). Standard errors are clustered at the reservation-level, *p-values* are reported in square brackets. *** p<0.01, ** p<0.05, * p<0.1.

5.3 Second Treatment Arm: Partial Correlations between Outcomes and Transfer to Fee-Simple

Our prior evaluation of allotment in section 5.1 considers a single treatment comparing households that received an allotment to those that did not within the same tribe. In this section, we report on estimations of equations (2) (with tribe fixed effects μ_e) and (3) (with reservation fixed effects μ_r). These alternative specifications allow us to distinguish between households that retained their allotment in trust and those that received a fee-simple patent. Within this section, we separately evaluate the effect of these two alternative property rights allocations on households and individuals.

Table 14 reports the results of both specifications on our two primary farming measures of interest. The table follows a similar structure to Table 5, where each column introduces additional controls. The results in Panel A.1 indicate both types of treatment are associated with living on a farm and the estimates are relatively stable across specifications. Our preferred model is reported in column 2, which controls for selection into allotment, but does not include contemporary county-level controls. The results suggest that relative to households on unallotted reservations, households receiving a fee-simple title are 14.6 percentage points more likely to live on a farm and households retaining their land in trust are 20.8 percentage points more likely to reside on a farm. Panel A.2 reports the results from estimating equation 3. This specification restricts the comparison to within reservation variation, allowing us estimate the effect of transferring title into fee-simple relative to retaining the trust patent. The results indicate that within allotted reservations, households receiving a fee-simple patent are over 6 percentage points less likely to live on a farm.

Panel B repeats this analysis a binary measure of farming as the primary occupation of the household head as the outcome of interest. The results are consistent with the findings in Table 5. Once we condition on reservation selection into allotment, there is no effect of either type of patent on the propensity of the household head to be working in farming. The results from within reservation variation in Panel B.2 indicate that receiving a fee-simple patent did not significantly alter the likelihood of the household head being engaged in farming. This suggests fee-simple title did not alter occupational choice, but instead reduced farming through a change in residence as fee-simple recipients were less likely to be living on farms.

Panel C revisits the effect of allotment on property values and home ownership. Both patent types are associated with higher rates of home ownership, but this effect is only precise for in-trust patents. The reservation-fixed effect specification shows that fee-simple allottees are in fact significantly less likely than in-trust allottees to own their home.

Tables 15 and 16 report the results of our partitioned treatment on labor market outcomes of adults and children. Table 15 follows the structure of Table 6, where Panels A and B separately estimate the effects for household heads and spouses. Within each panel, we estimate four models. First, we estimate the baseline model from equation 2, second we add a control for farm status, third we add the set of allotment selection controls. Finally, we include the results from estimating equation 3 with reservation fixed-effects. Our preferred specifications are the third and fourth sets of estimates within each panel.

The results in Panel A, are consistent with findings in Table 6, but highlight important distinctions in the effect of allotment. The positive effect of allotment on the number of weeks worked is stronger among the household heads that transferred their land to fee simple. Household heads with fee-simple title work an average of 8.9 additional weeks per year relative to unallotted reservations, while households retaining the land in trust have a smaller and statistically insignificant difference in weeks worked per year. If we restrict the comparison to within reservation, household heads with fee-simple title work nearly 4 additional weeks per year relative to household heads with in-trust patents. These differences do not carry over to differences in weekly wages, where both groups increase relative to unallotted reservations. Across the range of occupation and income scores reported in columns 5–8, both fee-simple and in-trust household heads experienced improvements compared to unallotted reservations, with the fee-simple household heads gaining by more according to the within reservation comparison.

The effects for spouses differ from those experienced by household heads. Spouses decreased their labor force participation, across both treatments relative to unallotted reservations (col 1). This was accompanied by subtle declines in weeks worked per year for both groups, with a more significant decrease for spouses in in-trust households relative to fee-simple households (col 2). For those spouses that stayed in the labor force, they were more likely to be employed in wage and salary occupations (col 3). We do not find effects of either form of treatment on wages or the

Table 14: Effect of Allotment, by Property Rights, on Farming and Home-Ownership

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A outcome:	D(HH lives on Farm)						
Panel A.1: Tribe-FE							
Fee-Simple	0.320***	0.146**	0.147**	0.136*	0.200***	0.177**	0.134
	[0.000]	[0.034]	[0.039]	[0.057]	[0.006]	[0.042]	[0.139]
In-Trust	0.372***	0.208***	0.208***	0.196***	0.259***	0.248***	0.204**
	[0.000]	[0.003]	[0.005]	[0.007]	[0.001]	[0.004]	[0.024]
R-squared	0.225	0.232	0.234	0.239	0.254	0.303	0.307
Panel A.2: Rez-FE							
Fee-Simple	-0.064***						
	[0.007]						
R-squared	0.240						
Panel B outcome:	Occupation: Farmer						
Panel B.1: Tribe-FE							
Fee-Simple	0.123***	-0.001	-0.008	-0.014	0.050	0.048	0.042
	[0.004]	[0.980]	[0.894]	[0.824]	[0.258]	[0.418]	[0.489]
In-Trust	0.152***	0.019	0.013	0.006	0.065	0.061	0.055
	[0.000]	[0.693]	[0.812]	[0.907]	[0.105]	[0.275]	[0.348]
R-squared	0.237	0.237	0.238	0.239	0.248	0.283	0.284
Panel B.2: Rez-FE							
Fee-Simple	-0.015						
	[0.458]						
R-squared	0.247						
Panel C outcome:	D(Own Dwelling)						
Panel C.1: Tribe-FE							
Fee-Simple	-0.074***	0.077	0.069	0.085	0.098	0.112	0.128
	[0.002]	[0.256]	[0.299]	[0.206]	[0.166]	[0.283]	[0.239]
In-Trust	0.000	0.143**	0.134**	0.150**	0.162**	0.183*	0.199*
	[0.977]	[0.031]	[0.037]	[0.020]	[0.018]	[0.065]	[0.055]
R-squared	0.128	0.126	0.130	0.131	0.137	0.165	0.167
Panel C.2: Rez-FE							
Fee-Simple	-0.064***						
	[0.001]						
R-squared	0.139						
Ctrls		+ LPA Ctrls	+Env. Pop	+Env. Agric	+Env. Income	+Env. Manuf	+Env. Finance
Observations	6,839	6,175	6,175	6,175	5,961	3,470	3,470

Notes: This table reports on the results of estimating equations (2) and (3), where the allotment indicator is split into allotments that were transferred into fee simple and those that remained in trust. The table structure is equivalent to Table 5. Standard errors are clustered at the reservation-level, *p-values* are reported in square brackets. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

different income and occupation indices, which is consistent with Table 6.

Table 16 reports the labor market effects of the partitioned treatment for children in separate panels by sons and daughters. Focusing on sons in Panel A, we find little difference in labor force participation or work behavior (col 1–3). The wage results in column 4 indicates that both fee patents and in-trust patents increased wages and the within reservation comparison indicates that the wage gains were larger within fee-simple households. The overall improvement in wages do not translate into higher occupation or income scores for either allotment category relative to unallotted reservations, but sons in fee-simple households did experience a larger increase across several indices relative to in-trust sons when comparing within reservations (col 5–8). There are several similarities in the labor market experiences for household heads and sons within the household. Both groups see strong increases in wages and occupation and income indices. Sons do not have the same changes in weeks worked, which would make sense if children were allocating more time to education.

Panel B reports the results for daughters, who experienced different labor market effects than sons. Similar to Panel B in Table 7, daughters work more weeks per year for both fee-simple and in-trust treatments relative to unallotted reservations. Daughters also see large increases across both treatments in their occupation and income indices. Focusing on the within reservation comparison, daughters in fee-simple households work 0.8 weeks per year more than daughters in in-trust households. This difference in work weeks does not carryover into differences in the indices.

Table 15: Parents' Labor Market Outcomes, split by Property Rights

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
outcomes:	D(LFP)	#weeks worked	D(employed)	Weekly Wage	Occup. Income Score	Occup. Earnings score (1950 base)	Nam-Powers-Boyd Occ Status Score	Duncan Socioec. Index
Panel A: Heads	+ LAP Ctrls							
Fee-Simple	-0.044	9.552**	0.026	4.767**	3.896***	12.025***	12.898***	9.756***
	[0.434]	[0.018]	[0.655]	[0.011]	[0.001]	[0.001]	[0.000]	[0.000]
In-Trust	-0.078	5.692	-0.016	3.566**	2.532**	7.835**	8.434**	6.977***
	[0.179]	[0.146]	[0.773]	[0.028]	[0.022]	[0.022]	[0.014]	[0.006]
R-squared	0.374	0.266	0.265	0.047	0.105	0.150	0.095	0.058
	+ LAP Ctrls + Farm-Ctrl							
Fee-Simple	-0.055	8.925**	0.064	5.670***	4.501***	14.453***	14.528***	10.225***
	[0.346]	[0.030]	[0.466]	[0.004]	[0.000]	[0.000]	[0.000]	[0.000]
In-Trust	-0.089	4.987	0.059	4.777***	3.542***	11.916***	11.179***	7.702***
	[0.133]	[0.213]	[0.492]	[0.006]	[0.002]	[0.001]	[0.001]	[0.002]
R-squared	0.377	0.271	0.435	0.059	0.200	0.285	0.182	0.073
	+ reservation FE + Farm-Ctrl							
Fee-Simple	0.035**	3.922***	0.001	0.799	0.907***	2.470***	3.231***	2.419***
	[0.017]	[0.000]	[0.941]	[0.182]	[0.001]	[0.001]	[0.000]	[0.000]
Observations	6,175	6,175	4,228	2,394	4,167	4,167	4,167	4,167
R-squared	0.385	0.278	0.427	0.083	0.202	0.282	0.186	0.077
Panel B: Spouses	+ LAP Ctrls							
Fee-Simple	-0.068***	-0.669	0.090***	-3.335	0.331	-0.374	-4.128	-1.354
	[0.002]	[0.804]	[0.006]	[0.243]	[0.811]	[0.933]	[0.452]	[0.710]
In-Trust	-0.083***	-2.705						
	[0.000]	[0.312]						
R-squared	0.082	0.068	0.355	0.351	0.432	0.375	0.346	0.310
	+ LAP Ctrls + Farm-Ctrl							
Fee-Simple	-0.080***	-0.559	0.084***	-2.895	0.635	0.298	-3.519	-1.350
	[0.000]	[0.842]	[0.003]	[0.289]	[0.664]	[0.948]	[0.536]	[0.734]
In-Trust	-0.093***	-2.620						
	[0.000]	[0.343]						
R-squared	0.089	0.068	0.400	0.367	0.424	0.371	0.341	0.320
	+ reservation FE + Farm-Ctrl							
Fee-Simple	0.011	1.865***	0.102***	-2.992	0.678	-0.179	-4.452	-1.617
	[0.223]	[0.000]	[0.000]	[0.238]	[0.640]	[0.968]	[0.428]	[0.727]
Observations	3,944	3,944	770	97	197	197	197	197
R-squared	0.120	0.085	0.442	0.363	0.466	0.407	0.372	0.321

Notes: This table reports on the results of estimating equations (2) and (3), where the allotment indicator is split into allotments that were transferred into fee simple and those that remained in trust. The table structure is equivalent to Table 6. Standard errors are clustered at the reservation-level, *p-values* are reported in square brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 16: Children's Labor Market Outcomes, split by Property Rights

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
outcomes:	D(LFP)	#weeks worked	D(employed)	Weekly Wage	Occup. Income Score	Occup. Earnings score (1950 base)	Nam-Powers-Boyd Occ Status Score	Duncan Socioec. Index
Panel A: Sons	+ LAP Ctrls							
Fee-Simple	-0.002	1.433	-0.140	5.125***	1.191	6.535	5.028	2.252
	[0.953]	[0.274]	[0.316]	[0.000]	[0.569]	[0.245]	[0.202]	[0.290]
In-Trust	0.004	1.530	-0.195	3.500***	-0.323	1.308	0.511	0.456
	[0.902]	[0.224]	[0.191]	[0.003]	[0.871]	[0.806]	[0.892]	[0.839]
R-squared	0.517	0.336	0.227	0.100	0.215	0.206	0.152	0.085
	+ LAP Ctrls + Farm-Ctrl							
Fee-Simple	-0.006	1.520	-0.159	5.260***	0.895	5.653	4.463	2.229
	[0.920]	[0.491]	[0.261]	[0.000]	[0.651]	[0.291]	[0.255]	[0.290]
In-Trust	-0.005	1.456	-0.193	3.688***	-0.308	1.318	0.474	0.472
	[0.926]	[0.505]	[0.199]	[0.002]	[0.871]	[0.795]	[0.898]	[0.831]
R-squared	0.437	0.280	0.273	0.103	0.282	0.271	0.191	0.086
	+ reservation FE + Farm-Ctrl							
Fee-Simple	-0.003	0.055	0.030	1.470***	0.960	3.663**	3.463***	1.692**
	[0.735]	[0.870]	[0.369]	[0.001]	[0.137]	[0.035]	[0.001]	[0.026]
Observations	7,639	7,639	1,476	945	1,412	1,412	1,412	1,412
R-squared	0.448	0.292	0.279	0.127	0.277	0.273	0.203	0.099
Panel B: Daughters	+ LAP Ctrls							
Fee-Simple	0.009	2.801**	0.152	-5.727	15.393***	36.493***	42.542***	22.492***
	[0.713]	[0.013]	[0.616]	[0.141]	[0.000]	[0.000]	[0.000]	[0.001]
In-Trust	0.003	2.113*	0.099	-5.100	15.299***	36.675***	40.201***	21.581***
	[0.912]	[0.053]	[0.732]	[0.169]	[0.000]	[0.000]	[0.000]	[0.000]
R-squared	0.150	0.117	0.349	0.383	0.218	0.204	0.222	0.247
	+ LAP Ctrls + Farm-Ctrl							
Fee-Simple	0.023	3.372***	0.082	-6.506	14.702***	35.208***	40.506***	23.916***
	[0.383]	[0.003]	[0.779]	[0.112]	[0.000]	[0.000]	[0.000]	[0.000]
In-Trust	0.014	2.578**	0.042	-5.722	14.762***	35.675***	38.617***	22.689***
	[0.591]	[0.020]	[0.882]	[0.139]	[0.000]	[0.000]	[0.000]	[0.000]
R-squared	0.128	0.095	0.357	0.387	0.216	0.204	0.222	0.249
	+ reservation FE + Farm-Ctrl							
Fee-Simple	0.008	0.811***	0.014	-0.572	-0.025	-0.305	2.212	1.966
	[0.141]	[0.001]	[0.832]	[0.456]	[0.988]	[0.943]	[0.606]	[0.442]
Observations	7,237	7,237	558	207	308	308	308	308
R-squared	0.136	0.101	0.427	0.432	0.270	0.246	0.266	0.291

Notes: This table reports on the results of estimating equations (2) and (3), where the allotment indicator is split into allotments that were transferred into fee simple and those that remained in trust. The table structure is equivalent to Table 7. Standard errors are clustered at the reservation-level, *p-values* are reported in square brackets. *** p<0.01, ** p<0.05, * p<0.1.

5.4 Instrumenting the Second Treatment Arm

Our identification strategy is anchored on the exogenous rotation of Indian Agents across reservations over time, and their varying propensity to transfer land from trust status into fee simple. To operationalize idea of using this varying propensity, we constructed a panel of the Indian Agents in charge on each reservation in each year reservation-year from the sources described in Section 3.5. We merged this dataset onto a complete panel of all allotments and transfers into fee simple on reservations between the Dawes Act of 1887, and the 1939, the year before the outcomes we observe in the Full-Count Census.

5.4.1 General Setup

The land transfer data are set up as a duration-analysis data-set, because, if an allotment i was not transferred into fee simple in year t, then it may be transferred in $t+1$. An allotment's outcome in year t is an indicator $D_{i(r)t}$ that takes value 1 if allotment i tied to reservation r was transferred into fee simple in year t, and 0. The key identifying variation in our framework comes from the fixed effect $\mu_{j(rt)}$ of agent j in charge of reservation r in year t.[44]

The timing of an allotment i's transfer into fee simple would have also depended on a number of other factors. Because allotments were supposed to be held in trust for at least some time before being transferred, the time that had passed since allotment i's initial issuance in year τ_i is certain to have been a factor in the transfer decision taken in year t. we therefore include a control $(t - \tau_i)$. The process of land transfers may have been faster on some reservations than on others; we therefore include a reservation fixed effect μ_r in our estimation. The process of land transfer may also have been faster at certain times than others; we therefore include a year fixed effect μ_t n our estimation.

For illustrative purposes, consider first the following duration-style regression that does not

[44] We estimate one fixed effect $\widehat{\mu_{j(\cdot)}}$ per agent j; the notation $j(rt)$ only serves to clarify that agents rotate across reservations over time.

include any allottee characteristics[45]

$$
\begin{aligned}
D_{i(r)t} &= \mu_{j(rt)} + \mu_r + \mu_t \\
&\quad + \beta_\tau \cdot (t - \tau_i) + \epsilon_{i(r)t}.
\end{aligned} \tag{7}
$$

We expect $\beta_\tau > 0$, and $\beta_b > 0$. With the estimated coefficients $\{\widehat{\beta_\tau}, \widehat{\beta_b}\}$ and vectors of fixed effects $\{\widehat{\mu_{j(\cdot)}}, \widehat{\mu_r}, \widehat{\mu_t}\}$, we compute an estimated probability of transfer into fee simple $\mathbb{P}(\widehat{D_{i(r)t} = 1})$ for each allotment i in each year t.

The key exogenous component in equation (7) are the agent fixed effects $\widehat{\mu_{j(rt)}}$. Our identification strategy is thus akin to the strategies used in the 'judge fixed effect' literature.[46] There are three elements that need to be in place for these strategies.

The first element is that the setting gave sufficient discretion to the BIA agents to let their idiosyncratic preferences matter. The historical and institutional narrative surrounding allotment makes it clear that the BIA agents possessed considerable discretionary room over the assignment of allotments (Banner, 2009; Otis, 2014; Carlson, 1981).

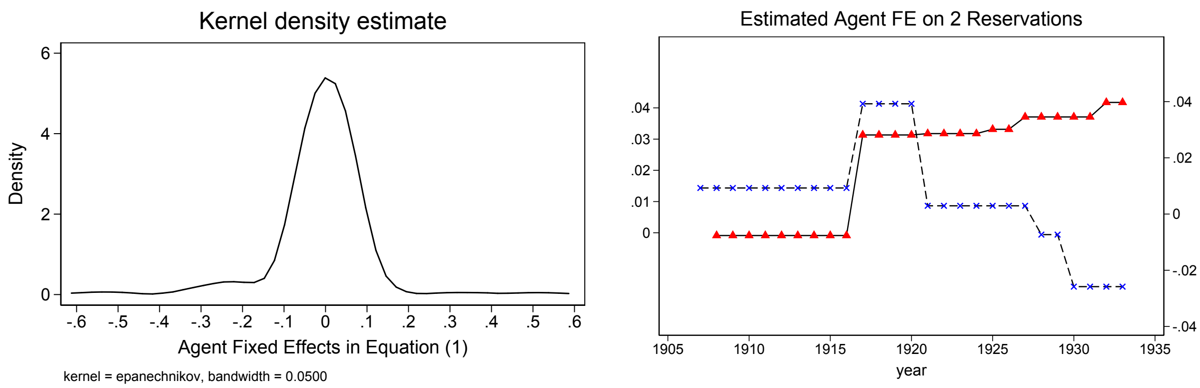
The second element is that there needs to be enough rotation of agents to gain the statistical power to estimate judges' propensity to transfer land into fee simple. In our setting, the frequent rotation of BIA agents across reservations, and the long panel of annual transfer-decisions that covers all allotted reservations allows us to separate the agent fixed effect from reservation-traits. To illustrate this fact, the left panel of Figure 4 shows the distribution of the roughly 450 agent fixed effects $\widehat{\mu_{j(\cdot)}}$ estimated in equation (7). The right panel of Figure 4 shows how the rotation of agents over time induces different time-paths in the propensity to transfer land into fee simple on two different reservations. In the initial years after the Burke Act, Salt River had an Indian Agent whose propensity to transfer land was about average, with a $\widehat{\mu_{j(\cdot)}} \approx 0$ (Charles E. Coe From 1906–1917); but from 1917 until the end of the allotment era in 1934, Spirit Lake, had a series of agents who all had higher than average propensities to transfer land into fee simple (Byron A. Sharp, 1917–1921, Frank A. Virtue, 1921–1925, Charles S. Young 1925–1927, John B. Brown 1927–

[45] Without allottee characteristics, this regression is run on BLM data combined with the agent-reservation panel, and does not include any ICR data.

[46] See, for example, Kling (2006); Di Tella and Schargrodsky (2013); Galasso and Schankerman (2014); Aizer and Doyle Jr (2015); Melero et al. (2017); Dobbie et al. (2018); Frandsen et al. (2019). Our setup departs from the standard 'judge fixed effect' setup in that our setup is naturally estimated as a duration analysis because the decision to transfer land from trust status to fee simple was taken *repeatedly*.

1932, Arthur J. Wheeler, from 1932). Salt River, by contrast, had agents with a higher propensity to transfer land to fee simple in the early years (Charles M. Ziebach 1906–1917, Samuel A. M. Young, 1917–1921), but then had a succession of three agents with a lower propensity towards the end of the allotment process (William R. Beyer 1921–1928, John S. R. Hammitt 1928–1930, and Orrin C. Gray 1930–1934).

Figure 4: Distribution of Estimated $\widehat{\mu_{j(\cdot)}}$



Notes: The left panel of this figure shows the distribution of roughly 450 agent fixed effects $\widehat{\mu_{j(\cdot)}}$ estimated in equation (7). The right panel shows how the rotation of agents over time induces different time-paths in $\widehat{\mu_{j(rt)}}$, i.e. the propensity to transfer land into fee simple, on Spirit Lake (red triangles, solid line) and on Salt River (blue crosses, dashed line)

The third element is that the assignment of judges to cases should not be endogenous to the outcome under study. In our setting, a BIA agent was in charge of all allotments during the time they were in charge on a reservation. We thus require that the assignment of a BIA agent to a reservation was conducted in a manner that was exogenous to the allotments that were considered for transfer into fee simple on that reservation.[47] From the perspective of selection, the ideal institutional setting would be one where BIA agents were rotated across reservations via a lottery. Unfortunately, the BIA did not assign agents to reservations via a lottery. One may therefore worry that the BIA allocated agents with a higher proclivity for transferring land into fee simple to reservations with certain characteristics, particularly over land. However, the historical record again suggests that this was not the case. The primary job of BIA agents was to foster education

[47] The historical record shows that the timing of rotation was anchored on the federal administration cycle: the majority of BIA agents were rotated with every when a new administration came in at the federal level every four or every eight years.On average, BIA agents managed a single reservation for approximately eight years, with the average career length lasting twelve years.

and public health on the reservations, and we argue that any selection on these characteristics would have been orthogonal to the process of allotments. We can statistically test this argument to an extent, based on the idea that if agents were chosen for the purpose of land transfer, then one might expect agent pay to correlate with $\widehat{\mu_{j(\cdot)}}$. We collected agent salary information from the Official Registers for every agent and year from 1879 to 1940. Average agent salaries were approximately $44,000 in 2018 dollars.[48] To quantify the relationship between agents' pay and the agents' estimated fixed effects, we estimate regression.

$$AgentPay_{jrt} = \mu_r + \delta_t + \beta \cdot \widehat{\mu_{j(\cdot)}} + \epsilon_{jrt}, \tag{8}$$

where $AgentPay_{jrt}$, was collected for each agent, j, located at reservation, r, in year t. Our main coefficient of interest, β, indicates whether or not agents with a higher propensity to transfer land were compensated more. We condition this specification on reservation and year fixed effects and cluster our standard errors at the reservation level. Column (1) of Table 17 reports the results of estimating equation (8). The results indicate that the agent fixed-effect is not significantly correlated with the agent salaries, which we view as evidence against selection of agents on their allotting propensity.

Table 17: Relating Estimated BIA Agent Fixed Effects to Salaries and Land Suitability

	(1) Ln(Agent Salary)	(2) Ln(Trust Land Quality)
Agent Fixed Effect	0.094	0.061
	[0.244]	[0.700]
Ln(Total Land Quality)		1.200***
		[0.000]
reservation fixed effect	Yes	
year fixed effects	Yes	
Observations	8,255	426
R-squared	0.576	0.762

Notes: In this table, we relate the estimated BIA agent fixed effects to agent salaries as well as to the quality of trust land the agent faced during their career. Column (1) reports the results of estimating equation (8). Column (2) reports the results of estimating equation (9). In square brackets are the p-values for the standard errors clustered on the reservation in column (1), and for robust standard errors in column (2); *** p<0.01, ** p<0.05, * p<0.1

We are also interested in whether agents with a higher propensity to transfer land transferred

[48]This is similar to a current federal employee paid at the General Schedule 8 grade.

lower quality land on average. To ask this, we constructed a weighted average of the land quality of reservations that agent j was ever on (weighted by number of years they were on), i.e. $TotalLandQuality_j$. We also construct the average quality of land allotted out of this pool under agent j, i.e. $TrustLandQuality_j$.[49] We then ask whether $\widehat{\mu_{j(\cdot)}}$ correlated with $TrustLandQuality_j$, conditional on the land quality of the available land pool:

$$TrustLandQuality_j = \beta \cdot \widehat{\mu_{j(\cdot)}} + \gamma \cdot TotalLandQuality_j + \epsilon_j \tag{9}$$

Column (2) of Table 17 reports the results of estimating equation (9). There is no evidence that higher land transfer propensity correlates with the quality of allotted land, relative to what land was available.

5.4.2 Adding Allottee Characteristics

To turn the results of the estimation of the duration setup into a cross-sectional instrument Z_i for the second-stage analysis, we calculate the cumulative probability that an allotment was transferred into fee simple between its issuance in year τ and the year 1934 (when the process was terminated), as

$$\begin{aligned} Z_i \quad &= \mathbb{P}(\widehat{D_{i(r),t=1934}} = 1) \\ &= \mathbb{P}(\widehat{D_{i(r),t=\tau}} = 1) \\ &+ [1 - \mathbb{P}(\widehat{D_{i(r),t=\tau}} = 1)] \cdot \mathbb{P}(\widehat{D_{i(r),t=\tau+1}} = 1) \\ &+ [1 - \mathbb{P}(\widehat{D_{i(r),t=\tau+1}} = 1)] \cdot \mathbb{P}(\widehat{D_{i(r),t=\tau+2}} = 1) \\ &+ ... \\ &+ [1 - \mathbb{P}(\widehat{D_{i(r),t=1933}} = 1)] \cdot \mathbb{P}(\widehat{D_{i(r),t=1934}} = 1) \end{aligned} \tag{10}$$

When we construct the cumulative probability of transfer in expression (10) based on estimating equation (7), we use no allottee information. It turns out that without this allottee information, the constructed instrument does not have enough statistical power once we condition on reservation fixed effects. We obtain much sharper variation when we enrich the within-reservation varia-

[49] We quantify land quality we use the FAO land suitability measure for rain fed wheat. We measure the land quality an agent faced by calculating the weighted average suitability index they faced over their career living at reservations r during years t.

tion in the estimating equation by including information on allottee i's age in year t from the ICR. Even BIA agents with a high propensity to transfer land into fee simple would have been reluctant to issue a competency certificate to an under-age allottee, and issuing such a certificate to a child must have been even rarer. Based on this logic, we bin allottee i's age in year t into three possible bins $\{\text{age}_i < 10, \text{age}_i \in [10, 18), \text{age}_i \geq 18\}$, and interact these bins with the agent fixed effects in the following estimating equation

$$\begin{aligned} D_{i(r)t} &= \mu_{j(rt)} \times D(\text{age}_i < 10) + \mu_{j(rt)} \times D(\text{age}_i \in [10, 18)) + \mu_{j(rt)} + \times D(\text{age}_i \geq 18) \\ &\quad + \mu_r + \mu_t + \beta_\tau \cdot (t - \tau_i)\epsilon_{i(r)t}. \end{aligned} \tag{11}$$

5.4.3 2SLS Estimation

As an instrument Z_i, we construct the cumulative probability (10) that an allotment was ever transferred into fee simple, predicted based on the empirical model of allotment in estimating equation (11).[50] We implement our IV procedure using standard 2SLS. When estimating 2SLS using a generated regressor like Z_i, under very weak assumptions, the point estimates are consistent and the standard errors and test statistics asymptotically valid.[51]

Table 18 reports on the resulting 2SLS estimation for our main farming outcomes. The sample in each case is household heads so that columns 1–3 share the same first stage equation reported in column 4. The instrument is a powerful predictor of the second treatment arm. When the constructed probability goes from 0 to 1, the observed likelihood of transfer into fee simple increases by 75 percentage points. The point estimate in column 4 is highly significant, and the second-stage F statistic is 14.06, above conventional thresholds where weak instruments become a worry. The 'zero-stage equation' (11) uses the interaction between allottees' birth years (expressed as ages in a given year) and agent fixed effects. Because an allottees' age could have independent direct effects on outcomes, we include it as a control variable.[52] Consistent with the historical narrative, column 4 shows that each annual increment in the birth-year of the allotment's original recipient reduces the probability of the allotment having ever been transferred into fee simple by half a percentage

[50] Online Appendix Figure 4 shows the distribution of this constructed instrument.

[51] See Pagan (1984) and Wooldridge (2010, pp116–117).

[52] The head of an allotted household in 1940 is usually the original allottee of the allotment associated with the household, but not if, for example, the household head is the heir of an original allottee. The age of the head in 1940, and the birth-year of the household's allotment's allottee are therefore not co-linear.

Table 18: IV Estimation: Effect of Fee Simple Rights on Farming and Home Ownership

Panel A outcome:	(1) D(HH lives on Farm)	(2) Occupation: Farmer	(3) D(Own Dwelling)	(4) Fee-Simple
Fee-Simple	-0.024	0.146	-0.085	
	(0.175)	(0.145)	(0.144)	
	[0.892]	[0.317]	[0.602]	
Instrument: Cumul Prob				0.749***
				(0.200)
				[0.000]
Allotee-Birthyear	-0.000	0.000	0.000	-0.005***
	(0.001)	(0.001)	(0.001)	(0.002)
	[0.901]	[0.856]	[0.893]	[0.006]
Observations	5,273	5,273	5,273	5,273
Kleibergen-Paap F statistic	14.06	14.06	14.06	
R-squared				0.418

Notes: This table reports on the results of an IV estimation of equation (3), with farming and home ownership as the outcome. The endogenous fee simple indicator is instrumented with the cumulative probability of transfer constructed in expression (10), predicted by the empirical model (11). Standard errors, reported in brackets, are clustered at the reservation-level; we also report *p-values* in square braces. *** p<0.01, ** p<0.05, * p<0.1.

point.

The second stage results reported in columns 1–3 of Table 18 contrast sharply with the partial correlations estimated through OLS in Panels A.2, B.2 and C.2 of Table 14. Where Table 14 shows a sharp drop in farm residence and in home ownership associated with fee simple title in the OLS, the 2SLS suggests no causal effect of fee simple title on either.

This contrast— between statistically significant partial correlates in the OLS and no causal effect estimated by the 2SLS— carries over into the labor market outcomes. Where Tables 15 and 16 show highly significant partial correlations between fee simple land and labor force engagement (participation and weeks worked) for heads, spouses, and daughters, none of these are evident Tables 19 and 20. Similarly, the OLS shows highly significant partial correlations between fee simple land and wages and occupational rank for heads and sones, but the IV estimates provide no evidence for a causal effect of fee simple on either set of outcomes.

Completing the picture, Table 21 shows no evidence whatsoever of a causal effect of fee simple land on the educational attainment of any household members.

Table 19: IV Estimation: Effect of Fee Simple Rights on Parents' Labor Market Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
outcomes:	D(LFP)	#weeks worked	D(employed)	Weekly Wage	Occup. Income Score	Occup. Earnings score (1950 base)	Nam-Powers-Boyd Occ Status Score	Duncan Socioec. Index
Heads								
Fee-Simple	-0.073	-0.439	-0.364*	-20.394	-3.353	-11.861	-7.141	-1.151
	(0.138)	(4.538)	(0.216)	(14.866)	(2.560)	(8.856)	(7.156)	(3.680)
	[0.599]	[0.923]	[0.097]	[0.176]	[0.196]	[0.186]	[0.323]	[0.756]
Allotee-Birthyear	-0.000	-0.021	-0.001	-0.145	-0.007	-0.028	-0.023	0.004
	(0.001)	(0.034)	(0.001)	(0.092)	(0.018)	(0.065)	(0.050)	(0.024)
	[0.746]	[0.540]	[0.385]	[0.120]	[0.696]	[0.672]	[0.645]	[0.858]
Kleibergen-Paap F statistic	11.89	11.89	10.70	5.271	7.623	7.623	7.623	7.623
Spouses								
Fee-Simple	-0.090	-4.320	0.049	-77.324	1.700	-7.813	32.660	52.042
	(0.067)	(3.499)	(0.193)	(137.352)	(27.319)	(59.075)	(91.010)	(60.976)
	[0.185]	[0.222]	[0.800]	[0.582]	[0.951]	[0.896]	[0.723]	[0.403]
Allotee-Birthyear	-0.001	-0.013	-0.001	-0.670	0.066	0.025	0.433	0.495
	(0.001)	(0.030)	(0.002)	(1.003)	(0.248)	(0.487)	(0.927)	(0.662)
	[0.214]	[0.664]	[0.762]	[0.515]	[0.791]	[0.960]	[0.646]	[0.463]
Kleibergen-Paap F statistic	12.13	12.13	6.395	0.371	1.875	1.875	1.875	1.875
outcomes:				First Stage				
Heads								
Instrument: Cumul Prob	0.780***	0.780***	0.905***	0.797**	0.843***	0.843***	0.843***	0.843***
	(0.226)	(0.226)	(0.277)	(0.347)	(0.305)	(0.305)	(0.305)	(0.305)
	[0.001]	[0.001]	[0.002]	[0.026]	[0.008]	[0.008]	[0.008]	[0.008]
Allotee-Birthyear	-0.005***	-0.005***	-0.005***	-0.006***	-0.005***	-0.005***	-0.005***	-0.005***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
	[0.007]	[0.007]	[0.004]	[0.002]	[0.006]	[0.006]	[0.006]	[0.006]
R-squared	0.422	0.422	0.439	0.455	0.441	0.441	0.441	0.441
Observations	5,273	5,273	3,538	2,002	3,485	3,485	3,485	3,485
Spouses								
Instrument: Cumul Prob	0.737***	0.737***	1.463**	0.148	0.353	0.353	0.353	0.353
	(0.211)	(0.211)	(0.578)	(0.241)	(0.257)	(0.257)	(0.257)	(0.257)
	[0.001]	[0.001]	[0.016]	[0.548]	[0.183]	[0.183]	[0.183]	[0.183]
Allotee-Birthyear	-0.005**	-0.005**	-0.008***	-0.008**	-0.008**	-0.008**	-0.008**	-0.008**
	(0.002)	(0.002)	(0.002)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
	[0.010]	[0.010]	[0.000]	[0.047]	[0.012]	[0.012]	[0.012]	[0.012]
R-squared	0.437	0.437	0.525	0.778	0.798	0.798	0.798	0.798
Observations	3,407	3,407	682	74	125	125	125	125

Notes: This table reports on the results of an IV estimation of equation (3), with parents' labor market outcomes as the outcome. The endogenous fee simple indicator is instrumented with the cumulative probability of transfer constructed in expression (10). Standard errors, reported in brackets, are clustered at the reservation-level; we also report *p-values* in square braces. *** p<0.01, ** p<0.05, * p<0.1.

Table 20: IV Estimation: Effect of Fee Simple Rights on Children's Labor Market Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
outcomes:	D(LFP)	#weeks worked	D(employed)	Weekly Wage	Occup. Income Score	Occup. Earnings score (1950 base)	Nam-Powers-Boyd Occ Status Score	Duncan Socioec. Index
Sons								
Fee-Simple	0.105	1.635	0.305	-11.057	8.001	21.308	12.366	2.849
	(0.117)	(3.287)	(0.191)	(8.807)	(7.621)	(22.069)	(17.314)	(7.882)
	[0.374]	[0.621]	[0.117]	[0.216]	[0.299]	[0.339]	[0.478]	[0.719]
Allotee-Birthyear	0.001	0.011	0.002**	-0.017	0.003	0.014	-0.006	-0.003
	(0.001)	(0.025)	(0.001)	(0.036)	(0.029)	(0.090)	(0.073)	(0.035)
	[0.546]	[0.660]	[0.042]	[0.639]	[0.922]	[0.880]	[0.940]	[0.934]
Kleibergen-Paap F statistic	10.61	10.61	19.78	6.058	6.474	6.474	6.474	6.474
Daughters								
Fee-Simple	0.015	1.840	0.400	-23.241	64.865	139.709	205.766	120.086
	(0.038)	(1.338)	(0.486)	(24.300)	(53.307)	(113.927)	(150.930)	(75.631)
	[0.688]	[0.174]	[0.415]	[0.350]	[0.233]	[0.229]	[0.182]	[0.122]
Allotee-Birthyear	-0.000	0.008	0.001	-0.035	0.248	0.490	0.701	0.334
	(0.000)	(0.013)	(0.003)	(0.189)	(0.392)	(0.859)	(1.203)	(0.697)
	[0.854]	[0.552]	[0.832]	[0.856]	[0.531]	[0.572]	[0.564]	[0.635]
Kleibergen-Paap F statistic	10.30	10.30	5.754	2.812	2.805	2.805	2.805	2.805
outcomes:				First Stage				
Sons								
Instrument: Cumul Prob	0.885***	0.885***	0.680***	0.515**	0.532**	0.532**	0.532**	0.532**
	(0.272)	(0.272)	(0.153)	(0.209)	(0.209)	(0.209)	(0.209)	(0.209)
	[0.002]	[0.002]	[0.000]	[0.018]	[0.014]	[0.014]	[0.014]	[0.014]
Allotee-Birthyear	-0.006***	-0.006***	-0.003*	-0.003	-0.003	-0.003	-0.003	-0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
	[0.003]	[0.003]	[0.075]	[0.186]	[0.104]	[0.104]	[0.104]	[0.104]
R-squared	0.415	0.415	0.519	0.481	0.521	0.521	0.521	0.521
Observations	6,360	6,360	1,283	812	1,231	1,231	1,231	1,231
Daughters								
Instrument: Cumul Prob	1.066***	1.066***	0.794**	0.722	0.545	0.545	0.545	0.545
	(0.332)	(0.332)	(0.331)	(0.429)	(0.325)	(0.325)	(0.325)	(0.325)
	[0.002]	[0.002]	[0.021]	[0.108]	[0.103]	[0.103]	[0.103]	[0.103]
Allotee-Birthyear	-0.006***	-0.006***	-0.005***	-0.004	-0.003	-0.003	-0.003	-0.003
	(0.001)	(0.001)	(0.002)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)
	[0.000]	[0.000]	[0.009]	[0.422]	[0.493]	[0.493]	[0.493]	[0.493]
R-squared	0.418	0.418	0.563	0.566	0.576	0.576	0.576	0.576
Observations	6,024	6,024	455	153	232	232	232	232

Notes: This table reports on the results of an IV estimation of equation (3), with children's labor market outcomes as the outcome. The endogenous fee simple indicator is instrumented with the cumulative probability of transfer constructed in expression (10). Standard errors, reported in brackets, are clustered at the reservation-level; we also report *p-values* in square braces. *** p<0.01, ** p<0.05, * p<0.1.

Table 21: IV Estimation: Effect of Fee Simple Rights on Parents' and Children's Education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Heads	Spouses	Sons	Daughters	Heads	Spouses	Sons	Daughters	Heads	Spouses	Sons	Daughters
Panel A:	Educ. Rank (1-14)				Educ: D(>Primary)				Educ: D(High Sch)			
Fee-Simple	-2.149	0.290	-1.072	-0.160	-0.198	-0.036	-0.020	0.013	-0.185	0.119	-0.144	-0.130
	(1.557)	(0.847)	(0.845)	(0.597)	(0.169)	(0.084)	(0.074)	(0.099)	(0.111)	(0.127)	(0.113)	(0.079)
	[0.173]	[0.733]	[0.210]	[0.790]	[0.246]	[0.675]	[0.788]	[0.893]	[0.101]	[0.353]	[0.208]	[0.105]
Allotee-Birthyear	-0.017	0.011*	-0.002	-0.007	-0.001	0.001	0.001	-0.000	-0.001*	0.001	-0.001	-0.001
	(0.011)	(0.006)	(0.005)	(0.005)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	[0.147]	[0.097]	[0.695]	[0.175]	[0.306]	[0.110]	[0.160]	[0.865]	[0.093]	[0.488]	[0.431]	[0.134]
Kleibergen-Paap F statisti	11.46	12.30	11.51	16.01	11.46	12.30	11.51	16.01	11.46	12.30	11.51	16.01
Panel B:	First Stage											
Z_i : Cumulative Prob	0.776***	0.733***	0.868***	0.933***	0.776***	0.733***	0.868***	0.933***	0.776***	0.733***	0.868***	0.933***
	(0.229)	(0.209)	(0.256)	(0.233)	(0.229)	(0.209)	(0.256)	(0.233)	(0.229)	(0.209)	(0.256)	(0.233)
	[0.001]	[0.001]	[0.001]	[0.000]	[0.001]	[0.001]	[0.001]	[0.000]	[0.001]	[0.001]	[0.001]	[0.000]
Allotee-Birthyear	-0.005***	-0.005***	-0.005**	-0.006***	-0.005***	-0.005***	-0.005**	-0.006***	-0.005***	-0.005***	-0.005**	-0.006***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
	[0.007]	[0.009]	[0.011]	[0.000]	[0.007]	[0.009]	[0.011]	[0.000]	[0.007]	[0.009]	[0.011]	[0.000]
Observations	0.423	0.438	0.440	0.437	0.423	0.438	0.440	0.437	0.423	0.438	0.440	0.437
R-squared	5,128	3,356	3,052	2,644	5,128	3,356	3,052	2,644	5,128	3,356	3,052	2,644

Notes: This table reports on the results of an IV estimation of equation (3), with education as the outcome. The endogenous fee simple indicator is instrumented with the cumulative probability of transfer constructed in expression (10). Standard errors, reported in brackets, are clustered at the reservation-level; we also report *p-values* in square braces. *** p<0.01, ** p<0.05, * p<0.1.

6 Interacting the Wealth Shock with Ancestral Traditions of Private Property

The results in Section 5.4.3 suggest that being exogenously treated with fee simple rights to one's allotment had no causal effect on farming, home ownership, labor market outcomes, or education. This contrasts sharply with the partial correlates estimated by in OLS Section 5.3.

This contrast strongly suggests that the OLS results are biased by selection whereby households who were more likely to obtain their allotments into fee simple were more likely to sell their farms, to earn higher wages, and to be in higher-ranked occupations. It is possible that the partial correlations in the OLS are entirely explained by pure *selection on unobservable characteristics* that made households attain more education as well as making them more likely to be deemed competent. However, the results in Section 5.2 suggest to us that the OLS coefficients are more likely to be biased by *selection on outcomes*: households who were more likely to benefit from fee simple rights to their lands were also more likely to obtain these rights, in part because they signalled their cultural assimilation to a larger extent. Another way of saying this is that obtaining fee simple rights had heterogenous effects on the treated households, and that OLS might estimate something close to the treatment effect on the treated (ToT), but that this ToT is potentially far off the population average treatment effect (ATE) because treated households were disproportionately those where fee simple would have its biggest effect.[53]

We address this possibility head-on by looking for evidence of heterogenous treatment effects along the measurable and well-studied dimension of ancestral cultural norms. If the effect of fee simple property rights was heterogenous, then it seems likely that the effect of obtaining fee simple rights (on the second treatment arm, i.e. relative to households with in-trust allotments on the same allotted reservation) would be most pronounced among populations with established ancestral traditions of private property. Fortunately, tribes' traditions of private property are measured as part of the Ethnographic Atlas (EA) dataset.

Let EA_e be an indicator for whether tribe e had such traditions. Then estimating equation (12) tests whether the effect of obtaining personal wealth with full legal title was more pronounced on

[53] By contrast, the local average treatment effect (LATE) estimated by IV may be quite close to the ATE.

reservations whose ancestral tribal traditions allowed for private property,

$$Y_{i(re)} = \alpha_F \cdot \textit{Fee-Simple}_i + \alpha_F^{EA} \cdot \textit{Fee-Simple}_i \times EA_e \qquad + \beta' X_i + \mu_r + \epsilon_{i(r)}, \tag{12}$$

where α_F^{EA} estimate the interaction between obtaining full title to the land and having ancestral traditions of private property in the EA.

If the effect of obtaining full fee simple title is indeed more pronounced in tribes with traditions of private property, then allotted households in those tribes should also have had stronger incentives to signal their competency in order to obtain legal title. This corollary hypothesis cannot be tested in the same within-reservation comparison, however, because all households in this comparison may have had identical incentives to for signalling assimilation. Instead, one can test the corollary hypothesis of a stronger incentive to signal by comparing allotted households who *did not* obtain their land in fee simple to un-allotted households in the same tribe, taking advantage of the many-to-one mapping from reservations to ethnographic tribes. The corresponding estimating equation is

$$Y_{i(re)} = \alpha_T \cdot \textit{In-Trust}_i + \alpha_T^{EA} \cdot \textit{In-Trust}_i \times EA_e \qquad + \beta' X_i + \mu_e + \epsilon_{i(r)}, \tag{13}$$

where EA_e does not enter as a regressor, because tribe-characteristics are absorbed by the tribe fixed effect μ_e.

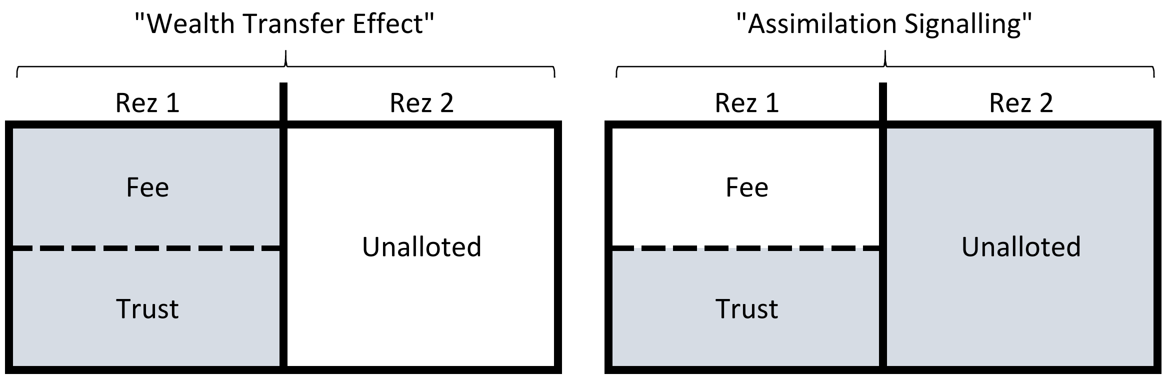
The comparisons made in the estimating equations (12) and (13) can be further clarified through Figure 5. Estimating equation (12) corresponds to a comparison of the two shaded groups in the left panel of Figure 5. , By contrast, estimating equation (13) corresponds to a comparison of the two shaded groups in the right panel of Figure 5. We recognize that this second comparison likely estimates a lower bound on the effect of traditions of private property no the incentive to assimilate, because the treated group is the "negatively selected" group of households who were not declared "competent" by the BIA agent. Nonetheless, this comparison is useful because it shuts off any potential effects of the fee simple transfer itself.

Table 22: Estimating Equation (12) for Farming & Assets

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
outcome:	D(Farm HH)		Occupation: Farmer		D(own home)				log value home $			
Fee-Simple	-0.065**	-0.065***	-0.020	-0.024	-0.053***	-0.041**	-0.062***	-0.049***	0.280***	0.292***	0.298***	0.311***
	[0.011]	[0.007]	[0.385]	[0.279]	[0.002]	[0.019]	[0.001]	[0.008]	[0.000]	[0.000]	[0.000]	[0.000]
Fee-Simple x Rights	0.009		-0.057**		-0.100*	-0.102*			0.231**	0.228**		
over Movable Property	[0.693]		[0.038]		[0.057]	[0.052]			[0.036]	[0.035]		
Fee-Simple x Rights		0.140***		-0.346**			0.226***	0.199***			-0.095	-0.223
over Land		[0.000]		[0.012]			[0.000]	[0.000]			[0.885]	[0.760]
D(Farm HH)						0.193***		0.193***		0.391***		0.392***
						[0.000]		[0.000]		[0.000]		[0.000]
Observations	6,852	6,852	6,852	6,852	6,852	6,852	6,852	6,852	5,188	5,188	5,188	5,188
R-squared	0.240	0.240	0.243	0.243	0.139	0.180	0.138	0.179	0.242	0.259	0.241	0.259
Placebo-Interactions												
Fee-Simple	-0.062***	-0.054	-0.022	-0.022	-0.063***	-0.051***	-0.051	-0.041	0.308***	0.319***	0.236*	0.251*
	[0.010]	[0.147]	[0.341]	[0.515]	[0.001]	[0.009]	[0.112]	[0.154]	[0.000]	[0.000]	[0.067]	[0.059]
Fee-Simple x Nuclear Family	-0.017		-0.026		0.028	0.031			-0.106	-0.096		
	[0.848]		[0.692]		[0.624]	[0.462]			[0.332]	[0.381]		
Fee-Simple x D(Bride-Price)		-0.013		[illegible]			-0.011	-0.009			0.079	0.075
		[0.774]		[0.928]			[0.760]	[0.801]			[0.588]	[0.608]
Observations	6,831	6,775	6,831	6,775	6,831	6,831	6,775	6,775	5,175	5,175	5,140	5,140
R-squared	0.240	0.241	0.244	0.243	0.137	0.179	0.137	0.179	0.241	0.258	0.242	0.259

Notes: All regressions include reservation fixed effects

Figure 5: Slicings of the Data



Notes: With reservation fixed effects, the within-reservation comparison of obtaining allotments in fee simple isolates the wealth transfer effect (left panel of figure). With tribe fixed effects, omitting households that obtained their allotments in fee simple isolates a lower bounds on assimilation signaling effect (right panel of figure)

Table 23: Estimating Equation (12) for Household Heads' Labor Market Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
outcome	#weeks worked		Weekly Wage		Occup. Income Score		Occup. Earnings Score		Nam-Powers-Boyd Occ Score	
Heads										
Fee-Simple	4.105***	3.959***	0.272	0.784	0.850***	0.900***	2.249***	2.460***	3.100***	3.220***
	[0.000]	[0.000]	[0.616]	[0.194]	[0.004]	[0.001]	[0.008]	[0.001]	[0.000]	[0.000]
Fee-Simple x Rights over Movable Property	-2.403		6.513***		0.690		2.652*		1.566**	
	[0.263]		[0.000]		[0.128]		[0.057]		[0.045]	
Fee-Simple x Rights over Land		-10.497		2.561***		1.996***		2.788**		2.948**
		[0.105]		[0.008]		[0.000]		[0.048]		[0.031]
D(Farm HH)	3.763***	3.765***	-3.584***	-3.594***	-4.802***	-4.802***	-19.354***	-19.352***	-12.975***	-12.975***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Observations	6,852	6,852	2,674	2,674	4,660	4,660	4,660	4,660	4,660	4,660
R-squared	0.278	0.278	0.085	0.083	0.202	0.202	0.282	0.282	0.186	0.186
Placebo-Interactions										
Fee-Simple	3.763***	3.917**	0.795	0.462	0.866***	1.133**	2.211***	4.008***	3.115***	4.208***
	[0.000]	[0.012]	[0.221]	[0.663]	[0.004]	[0.028]	[0.006]	[0.003]	[0.000]	[0.004]
Fee-Simple x Nuclear Family	1.749		0.175		0.443		2.635		1.263	
	[0.319]		[0.920]		[0.428]		[0.180]		[0.253]	
Fee-Simple x D(Bride-Price)		0.020		0.419		-0.280		-1.911		-1.206
		[0.991]		[0.749]		[0.608]		[0.194]		[0.441]
Observations	6,831	6,775	2,671	2,658	4,651	4,627	4,651	4,627	4,651	4,627
R-squared	0.278	0.276	0.084	0.084	0.202	0.203	0.282	0.282	0.186	0.186

Notes: All regressions include reservation fixed effects

Table 24: Estimating Equation (12) for Sons' Labor Market Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
outcome:	D(School)		#weeks worked		Occup. Income Score		Occup. Earnings Score		Nam-Powers-Boyd Occ Score	
Sons										
Fee-Simple	0.001	-0.002	-0.123	0.046	0.819	0.939	3.055	3.581**	3.288***	3.408***
	[0.944]	[0.868]	[0.705]	[0.891]	[0.254]	[0.151]	[0.111]	[0.042]	[0.003]	[0.001]
Fee-Simple x Rights over Movable Property	-0.056**		2.178***		1.557		6.705**		1.924	
	[0.020]		[0.000]		[0.179]		[0.038]		[0.638]	
Fee-Simple x Rights over Land		-0.408***		3.643***		3.648**		14.414***		9.614***
		[0.000]		[0.000]		[0.017]		[0.000]		[0.003]
D(Farm HH)	-0.048***	-0.048***	1.924***	1.920***	-4.636***	-4.634***	-13.474***	-13.467***	-7.907***	-7.898***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Observations	8,283	8,283	8,394	8,394	1,602	1,602	1,602	1,602	1,602	1,602
R-squared	0.197	0.197	0.293	0.292	0.278	0.278	0.273	0.273	0.203	0.203
Placebo-Interactions										
Fee-Simple	-0.009	0.002	0.037	0.096	0.998	0.624	3.676**	2.576	3.298***	1.944
	[0.525]	[0.958]	[0.916]	[0.896]	[0.126]	[0.587]	[0.037]	[0.510]	[0.002]	[0.498]
Fee-Simple x Nuclear Family	0.064		0.233		-0.588		-0.200		2.518	
	[0.203]		[0.836]		[0.848]		[0.982]		[0.671]	
Fee-Simple x D(Bride-Price)		-0.007		-0.052		0.434		1.388		1.958
		[0.844]		[0.948]		[0.757]		[0.755]		[0.530]
Observations	8,264	8,213	8,375	8,320	1,598	1,576	1,598	1,576	1,598	1,576
R-squared	0.196	0.196	0.292	0.291	0.276	0.278	0.271	0.272	0.201	0.203

Notes: All regressions include reservation fixed effects

Table 25: Estimating Equation (13)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
outcome:	D(Lives on Farm)	Occup: Farmer	Educ. Rank (1-14)	Educ: D(>Primary)	Educ: D(High Sch)	Educ. Rank (1-14)	Educ: D(>Primary)	Educ: D(High Sch)	Educ. Rank (1-14)	Educ: D(>Primary)	Educ: D(High Sch)	Educ. Rank (1-14)	Educ: D(>Primary)	Educ: D(High Sch)
Baseline			Heads			Spouses			Sons			Daughters		
In-Trust	0.370***	0.152***	2.408***	0.240***	0.100***	2.922***	0.325***	0.106***	1.708***	0.181***	0.161***	1.533***	0.156***	0.085***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.002]
D(Farm HH)			-0.788***	-0.073**	-0.087***	-0.528**	-0.052*	-0.066***	-0.035	-0.000	-0.023**	0.144	0.021	0.018*
			[0.005]	[0.011]	[0.000]	[0.040]	[0.091]	[0.000]	[0.625]	[0.988]	[0.021]	[0.220]	[0.187]	[0.074]
Observations	4,350	4,350	4,220	4,220	4,220	2,674	2,674	2,674	5,129	5,129	5,129	4,963	4,963	4,963
R-squared	0.267	0.259	0.400	0.300	0.171	0.428	0.336	0.174	0.682	0.511	0.197	0.669	0.503	0.187
x Property Rights														
In-Trust	0.091***	0.023	0.531	-0.003	0.022	1.526***	0.087**	0.186**	0.364*	0.048**	-0.000	-0.119	-0.064*	0.001
	[0.002]	[0.406]	[0.128]	[0.955]	[0.361]	[0.006]	[0.017]	[0.039]	[0.074]	[0.027]	[0.996]	[0.329]	[0.089]	[0.923]
In-Trust x Rights	0.352***	0.162***	2.378***	0.308***	0.098***	1.651***	0.281***	-0.094	1.718***	0.170***	0.206***	1.958***	0.261***	0.099***
over … Property	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.005]	[0.000]	[0.301]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]
Farm Ctrl			✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	4,350	4,350	4,220	4,220	4,220	2,674	2,674	2,674	5,129	5,129	5,129	4,963	4,963	4,963
R-squared	0.271	0.260	0.402	0.303	0.171	0.429	0.338	0.174	0.683	0.512	0.199	0.670	0.505	0.187
Placebo 1														
In-Trust	0.373***	0.151***	2.417***	0.242***	0.100***	2.914***	0.324***	0.107***	1.708***	0.181***	0.161***	1.548***	0.163***	0.085***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.003]
In-Trust x Nuclear Family	-0.391	0.206	-1.599	-0.371	-0.048	2.298***	0.333***	-0.084*				-1.598**	-0.723***	-0.035
	[0.179]	[0.232]	[0.639]	[0.348]	[0.708]	[0.000]	[0.000]	[0.086]				[0.022]	[0.000]	[0.620]
R-squared	0.267	0.259	0.400	0.301	0.171	0.428	0.336	0.174	0.681	0.511	0.195	0.669	0.504	0.186
Placebo 2														
In-Trust	0.378***	0.151***	2.478***	0.248***	0.103***	2.918***	0.324***	0.105***	1.748***	0.185***	0.160***	1.523***	0.156***	0.084***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.003]
In-Trust x D(Bride-Price)	-0.265**	0.007	-2.728**	-0.343***	-0.108	0.495	0.106	0.204***	-4.370***	-0.429***	0.014			
	[0.041]	[0.932]	[0.019]	[0.000]	[0.298]	[0.364]	[0.184]	[0.000]	[0.000]	[0.000]	[0.877]			
R-squared	0.270	0.257	0.405	0.305	0.172	0.430	0.337	0.175	0.683	0.512	0.196	0.669	0.504	0.185

Notes: All regressions include tribe fixed effects

7 Conclusion

In the early twentieth century, the U.S. government broke up millions of acres of communally owned reservation lands and allotted them to individual Native American households. Households initially received land allotments with limited property rights ('in trust'), and were incentivized to prove themselves "competent" in order to obtain full legal title ('fee simple') after a set period. Indian allotment thus had elements of a conditional transfer program aimed at assimilation. The policy was ended suddenly in 1934, locking in-trust land into its status in perpetuity. We link land allotment information to the universe of Native American households in the 1940 U.S. Census. We exploit quasi-random variation in being allotted as well as in securing the allotment in fee simple. Obtaining an allotment significantly increased the likelihood of living on a farm but not of working as a farmer, indicating that allottees leased out their land. Allotments also impacted wages and occupational rank. Surprisingly, allotment most significantly impacted educational attainment. We interpret education as a way of signalling "competency" to BIA agents. Obtaining the land in fee simple was associated with decreased likelihood of living on a farm and owning one's home, evidence that many allottees sold their land once they were deemed competent and obtained title. The fee-simple effects were more pronounced within tribes whose ancestral tribal norms emphasized private over communal property, indicating a cultural determinant in how the wealth transfer was utilized. Consistent with this, households in tribes with traditions of private property also engaged in more signalling of their assimilation.

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Online Appendix

to

"The Effect of Land Allotment on Native American Households During the Assimilation Era"

Online Appendix A Online Data Appendix

Online Appendix A.1 Data Background

We compile additional county-level controls from two sources. First, we use data from the U.S. Decennial Census in 1940 constructed by Haines (2010). This series includes measures across four categories: population, agriculture, wealth and expenditures, and manufacturing. Our selected population measures include total population, urban population, native born white population, foreign born population, and population density per square mile. Within agriculture we include the total number of farms, the number of white farmers, and the total farm value. Our third category covers measures of durable good ownership, represented by the percent of the county that own radios and refrigerators, expenditures, represented by the total value of retail sales, and the wealth in housing, represented by the average value of owner occupied dwellings. The final category includes five measures of the manufacturing sector. We include controls for the number of establishments, the average number of wage earners, total wages paid, the cost of materials used in the production process, and the value of total output.

Our second county-level data series incorporates banking data from FDIC reports from 1936 (Federal Deposit Insurance Corporation, 1992). We include two measures of banking activity, the total number of deposits and the total number of banks in the county.

Online Appendix A.2 Linkage

Older record linkage methods used a smaller number of variables for matching, and often used name only, often focusing on samples of people with unusual names in order to reduce false positive matches , e.g. Ferrie (1996). Matching on names is almost always fuzzy matching, i.e. a matching algorithm that allows for typos and mis-spellings. A common approach involves splitting first and last names into substrings ('bigrams'), and to construct a similarity index over all bigrams. A commonly used similarity index is to calculate the 'Jaro Winkler index' between two names. Newer iterations of fuzzy matching have increased the flexibility to include matching on a set of numeric as well as string variables, including distance calipers on numeric variables, e.g. giving a higher match probability when two records' birth-years are one year rather than three years apart. See Abramitzky, Boustan, and Eriksson (2012, 2016) for more recent applications.

More recently, the emergence of machine learning algorithm has given a real boost to the precision of record linkage methods, as it allows for training an algorithm. See for example the method outlined in Feigenbaum (2016). There is an active and ongoing debate on the choice of methods. Bailey et al. (2017) review several of these methods and show that all algorithm may produce samples that are not fully representative of the underlying population. This includes linking records by hand, although this method is favored by Bailey et al. (2017). By contrast, Abramitzky et al. (2019) show that a range of automated linkage methods on a range of standard linkage sample perform as well as manual linkage can be expected to.[54]

Online Appendix A.3 Additional Results

[54] For another summary, Ran Abramitzky's website at https://people.stanford.edu/ranabr/matching-codes.

Figure Online Appendix Figure 1: 1910 Advertisement for Reservation Lands Left from Allotment

Notes:

Figure Online Appendix Figure 2: Sample Page of the Indian Census Rolls

5-198

South Dakota

INDIAN CENSUS ROLL

5

Census of the Crow Creek reservation of the Crow Creek jurisdiction, as of April 1, 1934, taken by James H. Hyde Acting, Superintendent.

NUMBER	NAME: Surname	NAME: Given	SEX	AGE AT LAST BIRTHDAY	TRIBE	DEGREE OF BLOOD	MARITAL STATUS	RELATIONSHIP TO HEAD OF FAMILY	Roll No	RESIDENCE: At jurisdiction where enrolled, Yes or no	At another jurisdiction, Name	Elsewhere: Post office	County	State	WARD: Yes or no	ALLOTMENT, ANNUITY, AND IDENTIFICATION NUMBERS
1	2	3	4	5	6	7	8	9		10	11	12	13	14	15	16
38	Barry	Maude May	F	5-2-07 27	Sioux	1/4	S	Alone	40	No		Pierre	Hughes	S.D.	Yes	An. 38 Al. 1162
39	Bear	Jennie K.	F	8/27/15 19	"	F	S	Alone	44	Yes					"	An. 41 Al. 1556
40	Bear	Smith/K/	M	1881 53	"	F	S	Alone	45	"					"	An. 42 Al. 585
41	Big Eagle	Benjamin E.	M	5-11-09 24	"	F	Div	Hd	46	"					"	An. 45 Al. 1370
42	Big Eagle	Henry	M	1891 43	"	F	M	Hd	49	"					"	An. 46 Al. 1153
43	" (Fire-tail)	Julia	F	1895 39	"	F	M	Wf	50	"					"	An. 47 Al. 997
44	"	Marie	F	1-23-23 11	"	F	S	dau	51	"					"	An. 48
45	"	Henry M.	M	4-1-25 9	"	F	S	son	52	"					"	An. 49
46	"	Charlotte	F	7-2-28 6	"	F	S	dau	53	"					"	An. 50
47	"	Gerald Alvin	M	9-23-33 7/12	"	F	S	son	--	"					"	--
48	Big Eagle	John	M	7-9-03 31	"	3/4	M	Hd	56	"					"	An. 53 Al. 1154

Notes:

Figure Online Appendix Figure 3: Figure 1 in Gregg (2018)

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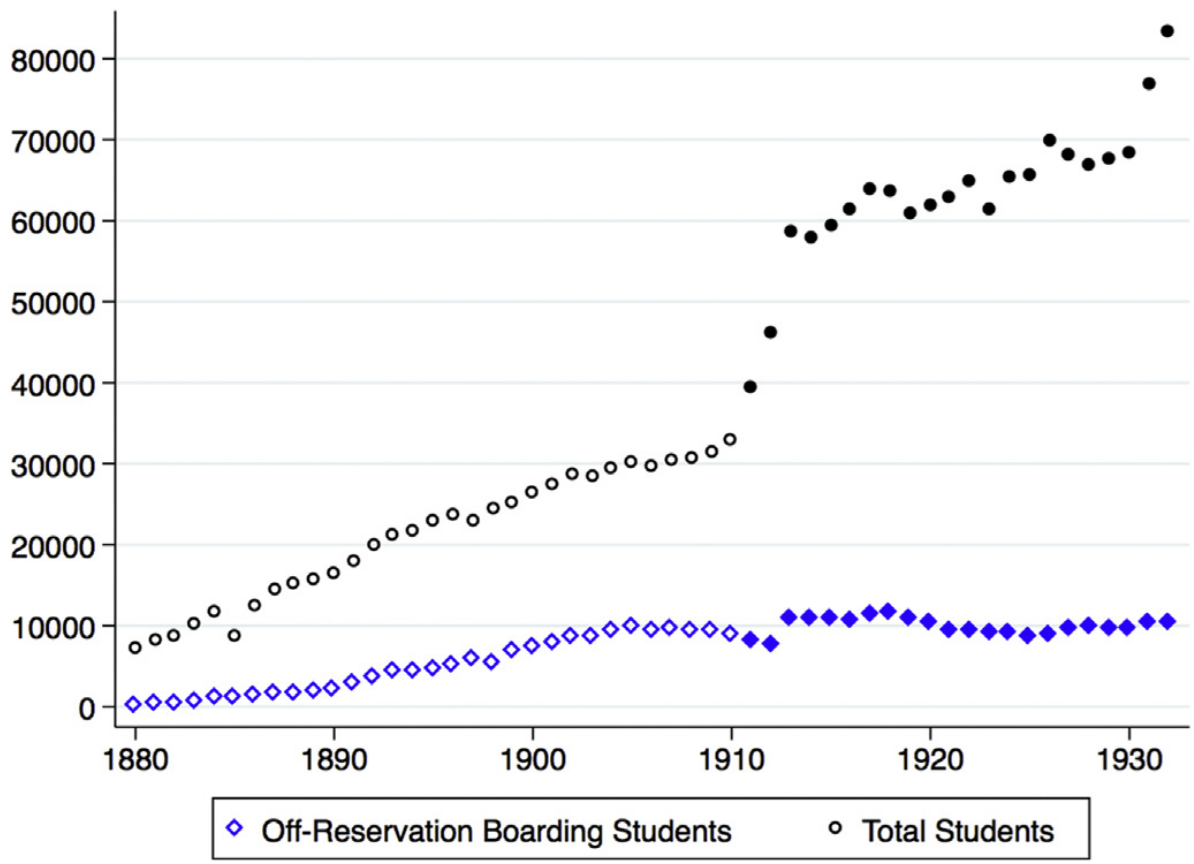


Fig. 1. Trends in School Enrollment of American Indian Children, 1880–1930. *Notes*: The black circles represent the total enrollment of school-age Indian children in all schools (i.e., off-reservation boarding schools, boarding schools, day schools, mission-run schools and public schools). The blue diamonds represent the enrollment in off-reservation boarding schools. For data availability reasons, the shaded data points reflect the years that are the focus of this paper. The calculations come from data in the Office of Indian Affairs' *Annual Reports*, 1880–1930. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

Notes:

Table Online Appendix Table 1: Coefficients on Control Variables in Table 5

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Allotted Household	0.371***	0.205***	0.206***	0.194***	0.259***	0.248***	0.203**
	[0.000]	[0.003]	[0.005]	[0.006]	[0.001]	[0.005]	[0.027]
Ruggedness		1.773***	1.797***	1.814***	1.904***	0.812*	0.833*
		[0.001]	[0.002]	[0.001]	[0.002]	[0.080]	[0.059]
Stream-Density		-153.686	-150.181	-145.540	-158.438	-24.224	7.954
		[0.139]	[0.195]	[0.186]	[0.186]	[0.841]	[0.946]
% Timber		-0.003	0.021	-0.113	-0.254	0.155	0.071
		[0.995]	[0.960]	[0.794]	[0.586]	[0.781]	[0.906]
Reservation Longitude		0.000**	0.000*	0.000	0.000	0.000	0.000
		[0.024]	[0.056]	[0.123]	[0.225]	[0.934]	[0.400]
Reservation Latitude		0.000**	0.000*	0.000	0.000	0.000***	0.000***
		[0.038]	[0.085]	[0.204]	[0.257]	[0.000]	[0.000]
Distance Fort 1880		-0.002***	-0.002***	-0.002**	-0.003**	-0.002**	-0.002***
		[0.001]	[0.009]	[0.011]	[0.039]	[0.018]	[0.007]
Total Population			0.000	-0.000	-0.000**	-0.000**	-0.000**
			[0.825]	[0.217]	[0.010]	[0.013]	[0.044]
Total Urban Population			-0.000*	-0.000	-0.000*	-0.000**	-0.000***
			[0.088]	[0.726]	[0.060]	[0.014]	[0.009]
Total Native White Pop.			-0.000	0.000	0.000**	0.000**	0.000**
			[0.963]	[0.290]	[0.012]	[0.024]	[0.042]
Total Foreign Born Pop.			0.000	0.000	0.000***	0.000***	0.000***
			[0.687]	[0.169]	[0.004]	[0.007]	[0.000]
Population Density (per sq mi)			0.000	0.000	0.000	-0.000	0.001*
			[0.861]	[0.713]	[0.570]	[0.891]	[0.052]
Total Number of Farms				0.000**	0.000**	0.000	0.000
				[0.048]	[0.012]	[0.419]	[0.506]
Number of White Farmers				-0.000*	-0.000***	-0.000	-0.000
				[0.062]	[0.010]	[0.397]	[0.430]
Total Farm Value				0.000	0.000	0.000	0.000
				[0.218]	[0.304]	[0.381]	[0.423]
Total Retail Sales					0.000*	0.000	0.000
					[0.082]	[0.239]	[0.267]
Pct. HHs with Refrigerators					-0.001	0.001	0.001
					[0.795]	[0.752]	[0.673]
Pcts. HHs with Radios					-0.008**	-0.013**	-0.014**
					[0.050]	[0.037]	[0.040]
Avg. Value of Owner Occ. Dwelling					0.000*	0.000**	0.000***
					[0.059]	[0.038]	[0.002]
Number of Manuf. Establishments						0.000	0.002*
						[0.806]	[0.061]
Avg. Number of Manuf. Wage Earne						-0.000*	-0.000**
						[0.075]	[0.021]
Total Manuf. Wages						0.000	0.000
						[0.233]	[0.171]
Cost of Materials in Manufacturing						-0.000	-0.000
						[0.510]	[0.860]
Value of Manufacturing Output						0.000	0.000
						[0.468]	[0.828]
Total Deposits (1936)							-0.000**
							[0.035]
Total Banks (1936)							-0.015*
							[0.077]
Observations	6,839	6,175	6,175	6,175	5,961	3,470	3,470
R-squared	0.224	0.230	0.232	0.237	0.252	0.301	0.305

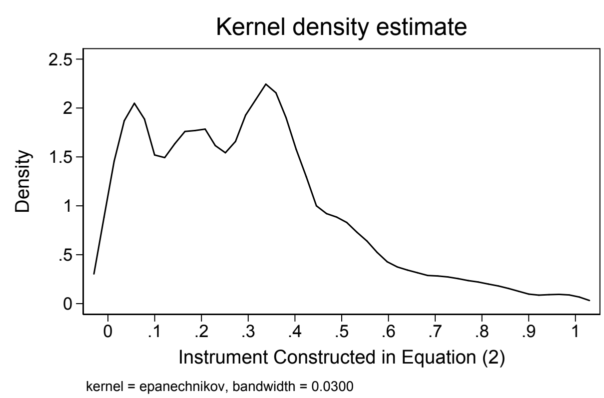
Notes: TBA

Table Online Appendix Table 2: Estimating Farming and Education Effects when Treatment is inferred from 'Predicted Reservation'

	(1)	(2)	(3)
Outcome:	D(Farm)	D(school)	
Sample	HH-heads	Age 5-20	
Allotted Rez	10.891	1.219	0.790
	[0.175]	[0.540]	[0.644]
Sex		Boys	Girls
Tribe + State FE	✓	✓	✓
Ctrl's	Age	age-FE	
Observations	56,141	37,885	36,795
R-squared	0.221	0.355	0.380

Notes: The Population Census includes no reservation/tribe information, and the finest spatial information is county; see discussion in Section 3.4. This table reports on the baseline results when reservation is predicted from county and all records predicted to be on an (un-)allotted reservation are assumed to be (un-)allotted.

Figure Online Appendix Figure 4: Distribution of Instrument Z_i



Notes: This figure shows the distribution of the over allotment-specific instruments Z_i constructed in equation (10).