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

Christian Dippel† Dustin Frye‡

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Abstract

Between 1906 and 1924, the U.S. government broke up millions of acres of communally owned reservation lands under the authority of the Dawes and Burke Acts, and allotted them to individual Native American households. We exploit quasi-random variation in the legal title with which land was granted to individual households: 'fee simple' gave full ownership rights while 'in trust' status did not. We link land allotment information to the universe of Native Americans in the 1940 U.S. Census, and find that households with full property rights over their allotted land had higher wealth, income, and occupational status, and their children had higher schooling attainment. We find that these differences were more pronounced within reservations whose ancestral tribal norms emphasized private property.

Keywords: Property Rights, Indigenous Economic Development, Culture, Record Linkage
JEL Codes: N10, O1, Z1

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†University of California, Los Angeles, CCPR, and NBER.

‡Vassar College

1 Introduction

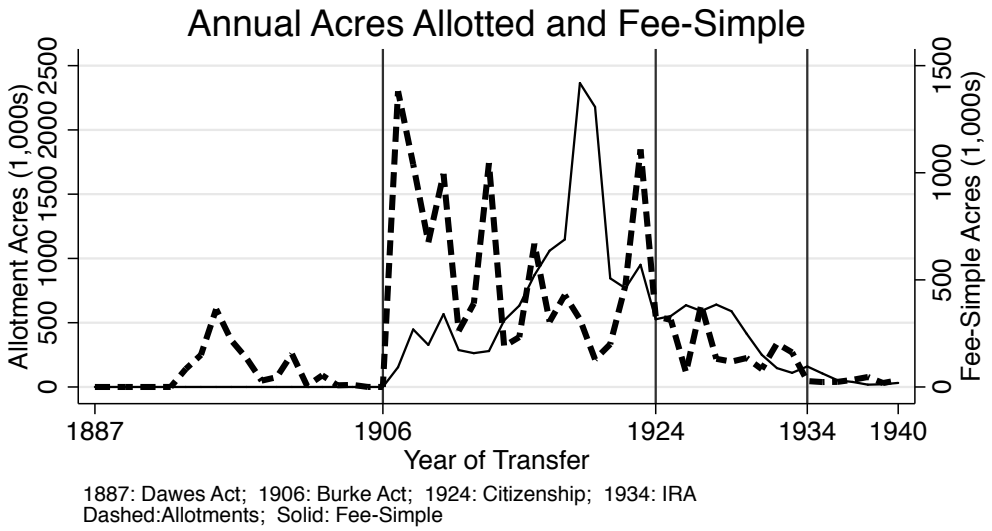
Native American reservations were formed in the U.S. in the second half of the 19th century. By the beginning of the 20th century, with the end of the Indian Wars and the closing of the frontier, the U.S. government turned its attention towards the assimilation of the Native Americans now living on reservations, and paving a path towards their citizenship. 'Indian allotment', which broke reservation lands into individually owned land allotments, was a major component of this strategy. Indian allotment had theoretically started with the passing of the Dawes Act in 1887. De facto, however, it only began in earnest with the Burke Act in 1906, which explicitly linked landownership to citizenship. All Indian allotments were first placed in a trust managed by the *Bureau of Indian Affairs* (BIA) local superintendents (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, allottees who were declared "competent" were eligible to convert their trust land to fee-simple, and coupled with this at the same time become citizens (Carlson, 1981; Banner, 2009; Otis, 2014). In 1924, at the height of the process of allotment, the Indian Citizenship Act (ICA) extended citizenship to all Native Americans, which abruptly throttled the process of allotment and transfers into fee simple. In 1934, allotment ended entirely with the passing of the Indian Reorganization (or 'Howard-Wheeler') Act (IRA). The aggregate flow of allotments and transfers is depicted in Figure 1. This paper studies the consequences for Native American households of this policy.

We link the universe of Indian land allotments to the universe of Native American households in the 1940 Full Count Census. Almost all Native Americans in 1940 lived on reservations or former reservation lands, and the focus of our analysis will be to compare households whose allotted land was transferred into fee simple to households on the same reservation whose allotted land remained in trust. Our analysis will not estimate the aggregate effect of Indian allotment on the allotted reservations. It is difficult to generate such an estimate because only about half of all reservations had been allotted when the policy ended,[1] and whether reservations were ever allotted was endogenous: Leonard, Parker, and Anderson (2018) show that reservations with better land were more likely to be allotted, ostensibly because of the second motive of the Burke Act (possibly

[1] On allotted reservations every household received an allotment.

the primary motive for some legislators) to free up the surplus tribal land left after allotment for white settlement. Although sales of un-allotted land in the end remained small in overall magnitude, they contributed considerably to this negative view.[2] Overall, Indian allotment is viewed as a harmful policy by most Native Americans today. This is unsurprising when one considers that it was a colonial policy that was forcibly imposed on tribes by Congress, and whose motivations were at best ambiguous. It should be noted that our research will only be able to estimate the differential effect of obtaining different forms of legal title under the auspices of Indian allotment, and will not be able to speak to the aggregate effects of Indian allotment on tribes as a whole.

Figure 1: Flow of Allottments and Transfers into Fee Simple



Notes: This figure tracks the flow of total acres that were allotted and the flow of acres subsequently transferred into fee simple in the BLM data.

Causal identification comes from the exogenous rotation of the Indian Agents ("allotting agents") across reservations over time, and the considerable observed variation in their propensity to transfer land into fee simple. This variation is interacted with within-reservation across-allotment variation in the date of initial issuance of an allotment, which determined its eligibility of being transferred into fee simple. This interaction mattered because an agent with a high transfer-propensity would still only transfer land into fee simple that had been held in trust for a sufficient period of time. From a practical point of view, we construct a complete reservation-year panel of In-

[2] Online Appendix Figure 1 depicts a sales ad for surplus land, which makes this point quite obvious.

dian Agents from 1887–1934, match this to the universe individual land allotments, including their date of issuance and when (if ever) they were transferred into fee simple. We then compute allotment-year specific probabilities of transfer into fee simple, and aggregate these into a cumulative probability that an allotment was ever transferred.[3]

Our core focus is on the effect that having obtained full (fee simple) title to their land had on individual Native American households when we observe them in the 1940 Census. In the baseline, we investigate the effect of having one's allotted land transferred into fee simple, comparing only across allotted households on the same reservation. We find that households whose allotted land was transferred into fee simple had higher wealth, income, and occupational status by 1940, relative to households on the same reservation whose allotted land remained in trust. We also find that the children of households whose allotted land was transferred into fee simple had obtained higher levels of education by 1940, again relative to children of households on the same reservation whose allotted land had remained in trust.

We also investigate the interaction between households' land allotments and the ancestral cultural norms governing individual property rights in a tribe, which we glean from the *Ethnographic Atlas* (EA). Almost all reservations are uniquely associated with one ancestral tribe, and thus one set of ancestral tribal norms. We find that the differences in 1940 income and occupational status between households with fee simple and in-trust land allotments were more pronounced on reservations whose ancestral tribal norms allowed for private property in addition to communal property rights.[4,5]

One contribution of our paper is to lift the lid on the consequences of one of the largest policy interventions ever targeted at indigenous peoples. The Dawes and Burke Acts are much maligned in Indian Country to the present day, partly because it was a colonial policy that was imposed

[3] 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 dynamic duration setup.

[4] It could reasonably be argued that ancestral norms had already adjusted to European settlement to some extent when they were measured. For example, the classic treatment of property rights by Demsetz (1967) includes as a case study for the emergence of property rights exactly this argument, building on anthropological work by Speck (1915); Leacock (1954).

[5] We also find that the differences in children's 1940 educational attainment across households were more pronounced on reservations whose ancestral tribal traditions included the norm of having a 'bride price', consistent with economic theory and with previous evidence (Ashraf, Bau, Nunn, and Voena, 2019).

on tribes, and partly because of the decision to lock individual land 'in trust' indefinitely with the 1934 IRA (but effectively already in 1924). Much of this land remains in trust to the present day, under an ill-conceived design whereby all descendants of the original allottees have claims on the land, and have to agree unanimously to any activity on the land, including activities such as leasing as well as the later transfer into fee-simple. Evidence is mounting on the long-run inefficiencies created by this arrangement and the opportunity costs for Native American tribes who had been subject to allotment (Leonard and Parker, 2017; Dippel, Frye, and Leonard, 2019). In a very nice recent paper, Akee (2019) investigates the short-run effect of this by comparing households on one allotted and one un-allotted reservation in Minnesota in 1900 and 1910, and finds that allotment had negative consequences for home ownership. Our paper cannot answer the question whether Indian land allotment would have had aggregate long-run welfare benefits if all land had been transferred to households into fee simple.[6] It does, however, show that even in the relative near term that we capture with the 1940 outcomes (where the problem of multiple heirs was not important), being given land with full property rights (i.e. in 'fee simple') was far better for Native American households than being given the same land without the ability to collateralize or sell it.

A second contribution of our paper is to the literature on culture and its impact on economic choices and outcomes. This growing literature emphasizes 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).[7] 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.[8]

[6] 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, and a 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, there are today many successful efforts by tribal governments to change institutions of tribal governance to promote economic development (Regan and Anderson, 2016; Jorgensen, 2007).

[7] 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.

[8] To some extent this is the flipside of Ostrom (1990) who argued for the importance of culture in making collective

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

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.

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). Congress experimented with allotment clauses in treaties governing individual reservations starting in the 1850s.[10] These treaties formed the basis for the first general allotment acts, discussed by Congress and endorsed by the Office of Indian Affairs in the 1870s (Otis, 2014). Legislation stalled over the issues of citizenship, jurisdiction, and whether to immediately grant allotted Indians title to their land. In 1880, Senator Coke of Texas introduced a bill calling for the allotment of all reservation lands. This bill provided for the allotment of Indian lands following the designation of the president and a 2/3 majority vote in favor by the adult male members of the tribe Prucha (2014). The allocation of land varied by household type and age and allotments were inalienable for 25 years. The bill also allowed for the tribe to sell excess reservation lands to the government (Prucha, 2014).

property management function without having it deteriorate into the tragedy of the commons.

[9] Feir, Gillezeau, and Jones (2019) provide a particularly striking example of this, focusing on the bison cultures in the great plains, and the long run consequences for them of the sudden slaughter of the bison in the 1880s.

[10] By the late 1870s, nearly seventy treaties included clauses regarding the allotment of Indian lands (Prucha, 2014).

The bill found heavy support within Congress, the Office of Indian Affairs, and among reformers. The Coke bill passed in the Senate, but failed in the House due to opposition from western legislators that felt the bill was too generous to Indians (Carlson, 1981).

Henry Dawes introduced a modified allotment bill to the Senate in 1886. 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).[11] 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).[12] Proceeds from the sales of the surplus land were held in trust and appropriated at the discretion of Congress for "education and civilization" (Banner, 2009). The Secretary of Interior had the authority to establish leasing regulations for allotted lands. Leasing agreements were managed by Indian Agents and the leasing of allotted lands was widely adopted across reservations (Carlson, 1981).[13]

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 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

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

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

[13] Gradually the Dawes Act was amended to relax constraints on sales of trust lands. In 1903, the Office of Indian Affairs was authorized to sell allotments of deceased allottees with multiple heirs, and in 1907, they were authorized to sell allotments of original allottees under special circumstances.

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. The BIA agents possessed considerable authority over the assignment of allotments, even for those eligible Indians that desired an allotment. (Banner, 2009; Otis, 2014; Carlson, 1981). In cases where the Indian did not select a plot, the agent assigned an allotment. 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 years.

2.3 The End of the Allotment Era

The allotment process moved quickly. On the extensive margin, nearly four reservations per year were designated for allotment over the first two decades. On the intensive margin, the government averaged nearly 4,500 allotments per year over the same period (Otis, 2014). The rapid expansion of allotment and 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. These concerns culminated in a review of the current social and economic conditions on reservations by Lewis Meriam of the Institute of Governmental Research in 1928. 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.

2.4 Legacy of the Allotment Era

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). The policy resulted in a substantial transfer of land out of Native ownership. 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. Nearly 60 million acres were 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. Often parcels of differing tenure types are adjacent to one another, creating a checkerboard pattern on reservations. Figure 2 illustrates this pattern on the Pine Ridge Reservation.

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.[14]

[14] 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).

3.2 The Full-Count Population Census (Measuring Outcomes)

The 'rule of 72' dictates that non-anonymized individual-level Census information can be made available 72 years after a Census was published. The 1940 Full-Count US Census (FCC) includes 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 spotty, 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 the BLM data and the FCC.

3.3 The Indian Census Rolls (Measuring Treatment)

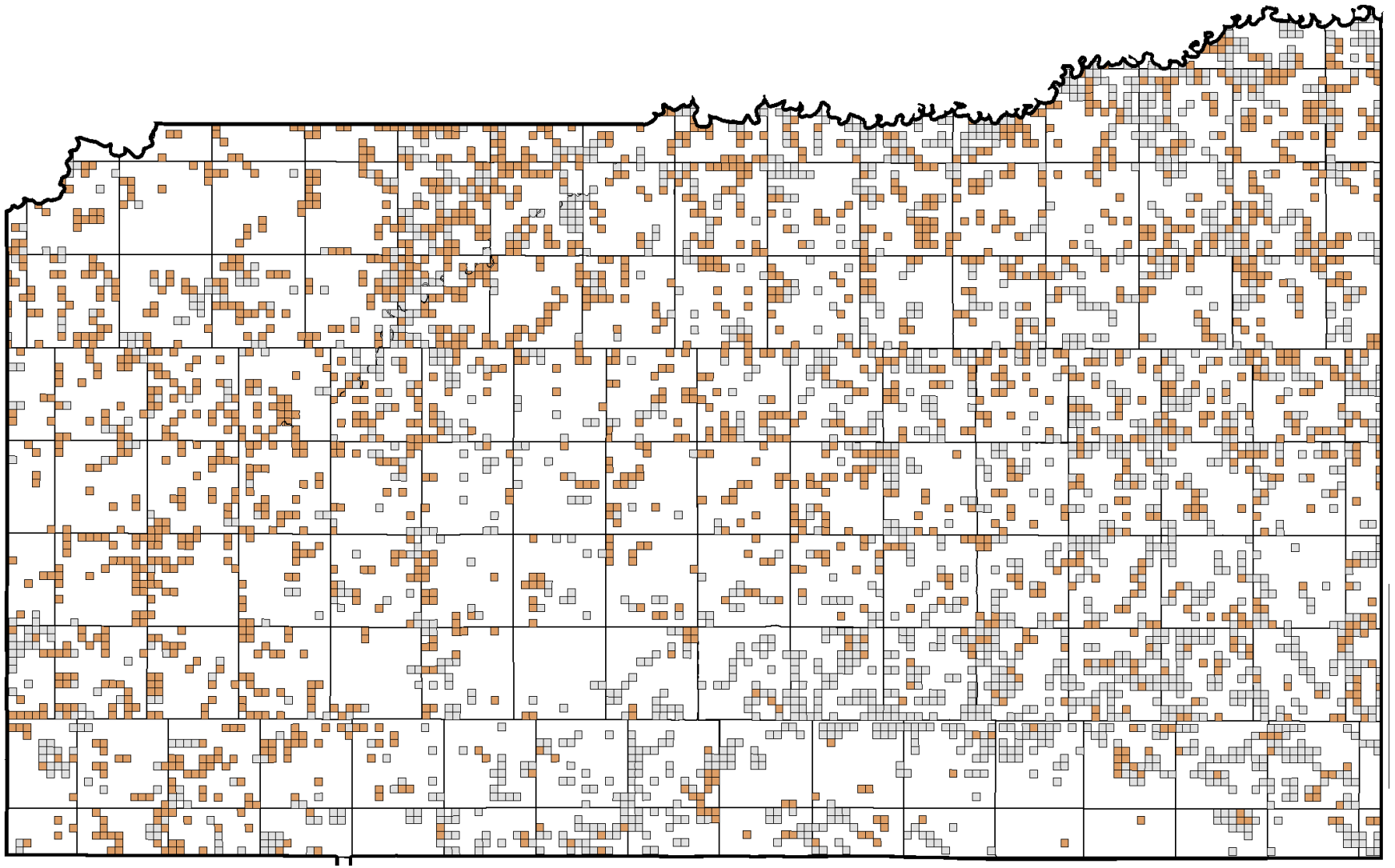
To be able to link BLM land allotment information to the FCC, we need to link both the BLM and the FCC to the *Indian Census Rolls* (ICR). The ICR were special censuses that were unrelated to the FCC and were collected on reservations in the 1920s and early 1930s. The ICC did not record any economic 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.

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 2 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 5.

3.4 The Indian Agents

Our identification strategy will combine information on when an allotment was issued (which we glean from the BLM data) with information on allottees, which we glean from the ICR data.

Figure 2: 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).

Conditional on these pieces of information, we then use the exogenous variation generated by the rotation of Indian Agents across reservations, and their varying propensity to transfer land into fee simple to estimate an exogenous probability that a given allotment will be transferred into fee simple in any given year. To do 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). The Official Registers were published biennially from 1879–1940. 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 in charge of taking the census from the beginning of each census year.

3.5 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. A second measure that we will include in our analysis when we consider the children of the original allottees is whether a tribe had ancestral norms of having a bride price, i.e. of the groom's family paying a material transfer to the bride's parents. Online Appendix Table 1 provides details on the Ethnographic Atlas's categories for each variable, which we convert into a binary indicator variable for ease of interpretation.

4 Identification Strategy

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 3.4. 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. This panel of land transfers constitutes the data underlying the aggregate time series in Figure 1.

The land transfer data are set up as a duration-analysis dataset: if an allotment i was not transferred into fee simple in year t, then it may be transferred in $t+1$. 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.[15] In addition, when allotment i was transferred into fee simple also depended on a number of other factors. On some reservations, the process of land transfer may have been faster so that we include a reservation fixed effect μ_r. At certain times, the process of land transfer was faster faster so that we include a year fixed effect μ_t. Most allotments were held in trust for some time before they became eligible for transfer so that control for $(t-\tau_i)$, the number of years that had passed since i's initial issuance in year τ_i. Whether a given allotment was transferred into fee simple may have also depended on a reservation's stock of untransferred trust allotments, which we call backlog$_{rt}$. Defining a dummy $D_{i(r)t}$ that tales value 1 if allotment i tied to reservation r was transferred into fee simple in year t, and 0, we run the following duration-style regression

$$\begin{aligned} D_{i(r)t} &= \mu_{j(rt)} + \mu_r + \mu_t \\ &\quad + \beta_\tau \cdot (t-\tau_i) + \beta_b \cdot \text{backlog}_{rt} + \epsilon_{i(r)t}. \end{aligned} \tag{1}$$

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 (1) are the agent fixed effects $\widehat{\mu_{j(rt)}}$. Other determinants of land transfer in equation (1) will be controlled for in the second stage by conditioning on reservation fixed effects and on the year τ a patent was originally issued.

The final analysis undertaken in Section 6 compares households whose allotments were transferred into fee simple to those whose allotments stayed in trust in a cross-sectional analysis across

[15] 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.

Native American households in the 1940 Full-Count Census. To turn the results of the estimations outlined here into a cross-sectional instrument Z_i for the analysis in Section 6, 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

$$\begin{aligned} Z_i \quad &= \mathbb{P}(\widehat{D_{i(r),t=1940}} = 1) \\ &= \mathbb{P}(\widehat{D_{i(r),t=\tau}} = 1) \\ &+[1 - \mathbb{P}(\widehat{D_{i(r),t=\tau}} = 1)] \cdot \mathbb{P}(\widehat{D_{i(r),t=\tau+1}} = 1) \\ &+[1 - \mathbb{P}(\widehat{D_{i(r),t=\tau+1}} = 1)] \cdot \mathbb{P}(\widehat{D_{i(r),t=\tau+2}} = 1) + ... \end{aligned} \tag{2}$$

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

Our identification strategy is akin to the strategies used in the 'judge fixed effect' literature.[16] There are three elements that need to be in place for these strategies: one, the setting needs to allow sufficient discretion that the identity of the decision-maker matters. Two, there needs to be enough statistical power to estimate individual judges' probability of making a certain decision (like a yes/no vote or a guilty/non-guilty verdict). The third element is that the assignment of judges to cases should not be endogenous to the outcome under study. In our setting, this means Indian Agents with a higher propensity to transfer land into fee simple should not be assigned to reservations that are independently more likely to have their land transferred into fee simple. We argue that all three elements are in place in our setting.

First, the historical and institutional narrative makes it clear that the BIA agents possessed considerable discretionary room over the assignment of allotments (Banner, 2009; Otis, 2014; Carlson, 1981). Second, we are able to estimate an individual BIA agent's probability of transferring an allotment into fee simple because there was frequent rotation of BIA agents across reservations, so that we can separate the agent fixed effect from reservation-traits, and because we have

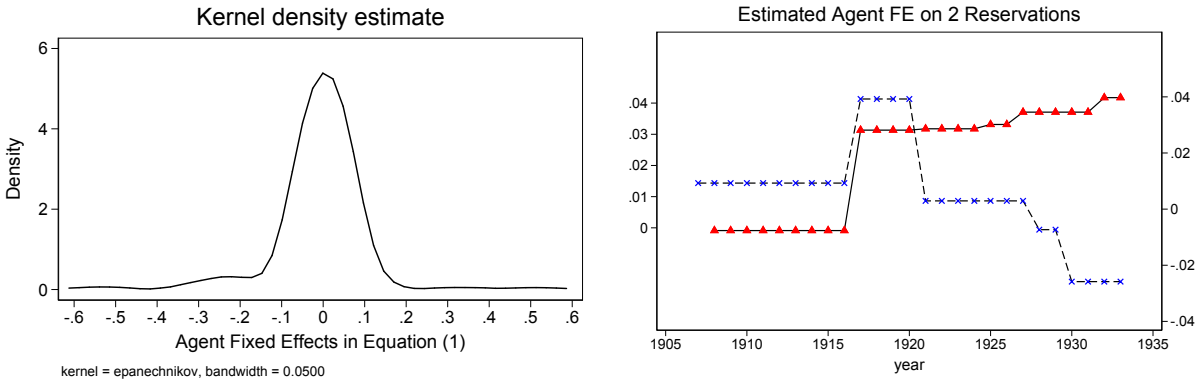
[16] See, for example, Kling (2006); Di Tella and Schargrodsky (2013); Galasso and Schankerman (2014); Aizer and Doyle Jr (2015); Melero et al. (2017); Dobbie et al. (2018); Frandsen et al. (2019). Our setup departs from the standard 'judge fixed effect' setup in that our setup is naturally estimated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

a long panel of annual transfer-decisions that covers all allotted reservations and that spans the entire allotment period. The left panel of Figure 3 shows the distribution of the roughly 450 agent fixed effects $\widehat{\mu_{j(\cdot)}}$ estimated in equation (1). The right panel of Figure 3 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 1927–1932, 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).

The key aspect is the timing in the rotation. In the early years, most allotments were relatively newly issued and therefore ineligible for transfer (captured by $\beta_\tau \cdot (t - \tau)$ in equation (1)). Even agents with a high probability to transfer land into fee simple would typically not do it when the allotment was very new. Having agents with a high $\widehat{\mu_{j(\cdot)}}$ in the early year therefore did not make a big difference. In the later years however, most allotments were eligible and the process was primarily determined by the transfer-propensity of agents. In those later years, Salt River's agents had a six percent lower probability to transfer land than Spirit Lake's (e.g. $-0.02 - 0.04$ in the right panel of Figure 3), conditional on reservation and year fixed effects as well as when an allotment was issued. As a result, almost three-quarters of the allotments on Spirit Lake were eventually transferred into fee simple, but less than ten percent were on Salt River.

The key to the instrument we construct in equation (2) is the interaction between a reservation's rotation-induced variation in $\widehat{\mu_{j(rt)}}$ over time, and an allotment's initial issuance year, which creates variation within reservations in whether an allotment was eligible for transfer into fee simple under an agent with a high or a low propensity for land-transferring. Figure 4 shows the distribution of Z_i, where i indexes an allotment and we have just under 110,000 allotments in the data. Given the logic of this identification strategy, it is important to very that Z_i is not simply explained

Figure 3: 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 (1). 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)

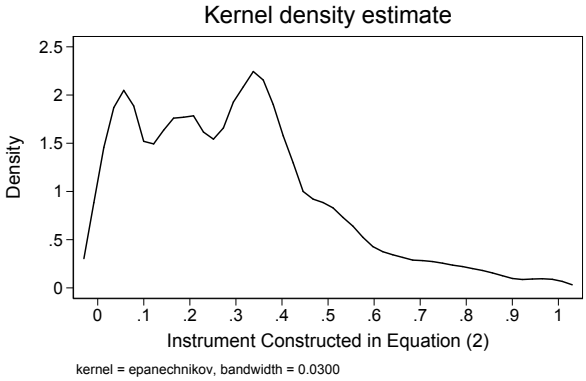
by reservation fixed effects or by the allotments' year of initial issuance. The regression

$$Z_{i(r)} = \mu_r + \beta_\tau \cdot \tau_{i(r)} + \epsilon_{i(r)t} \tag{3}$$

tests this. Results are reported in the right panel of Figure 4, with standard errors clustered at the reservation level. The R-squared of this regression is 0.55; the estimated $\widehat{\beta_\tau}$ suggest that an allotment that was issued 10 years earlier was two percent more likely to be transferred into fee simple eventually, but the effect is not significant, and the F-test for the joint insignificance of all reservation fixed effects is not rejected, with a p-value of 0.36. In combination, this evidence shows that $Z_{i(r)}$ display considerable within-reservation variation, and that this within-reservation variation cannot be explained simply by earlier allotments being more likely to be transferred into fee simple.

These first two elements are necessary to be able to separate the agent fixed effect from reservation-traits, but they are not sufficient for statistical identification of $\widehat{\mu_{j(\cdot)}}$ in equation (1). For this, we also require that the assignment of a BIA agent to a reservation was conducted in a manner that was exogenous to the allottees' whose allotments were considered for transfer into fee simple on that reservation. From the perspective of timing, one may worry that BIA agents were rotated only a reservation when the sale of surplus reservation lands was deemed most important by the

Figure 4: Distribution of Instrument Z_i



regression, with outcome Z_i

year(allotment issued)	-0.002
	(0.002)
reservation fixed effects	Y
Observations	108,031
R-squared	0.553
F-stat(rez fixed effects)	1.26

Notes: The left panel of this figure shows the distribution of the over 100,000 allotment-specific instruments Z_i constructed in equation (2). The right panel reports on the results of estimating equation (3).

government. However, the historical record is that 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.[17]

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 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.[18] To quantify the relationship between agents' pay and the agents' estimated fixed effects, we estimate regression.

$$AgentPay_{jrt} = \mu_r + \delta_t + \beta \cdot \widehat{\mu_{j(\cdot)}} + \epsilon_{jrt}, \tag{4}$$

[17] On average, BIA agents managed a single reservation for approximately eight years, with the average career length lasting twelve years.

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

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 1 reports the results of estimating equation (4). 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 1: 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 (4). Column (2) reports the results of estimating equation (5). 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 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. $TrustLandQuality_j$.[19] We then ask whether $\widehat{\mu_{j(.)}}$ correlated with $TrustLandQuality_j$, conditional on the land quality of the available land pool:

$$TrustLandQuality_j = \beta \cdot \widehat{\mu_{j(.)}} + \gamma \cdot TotalLandQuality_j + \epsilon_j \tag{5}$$

Column (2) of Table 1 reports the results of estimating equation (5). There is no evidence that

[19] 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.

higher land transfer propensity correlates with the quality of allotted land, relative to what land was available.

5 Linkage

The data used to construct the instrument in Section 4 makes no use of the Full Count Census FCC data which we undertake our analysis. For this, the allotment-specific instrument needs to be record-linked from the BLM-data to the Indian Census Rolls (ICR), which is straightforwardly done by allotment number, and from there to the FCC.

High variability in recorded names makes linking the ICR to the FCC is more difficult than, for example, linking non-Native American groups across Census waves. This variability in names is driven by two factors: the unusual names of Native Americans meant Census enumerators made more frequent errors in recording names. In addition, there was a strong trend of anglicization of names during the period in which we link samples, so that both first and last name of the same person were more likely to be anglicized in the FCC than in the ICR. In addition, there are lots of discrepancies in reported date of birth (the only other identifer next to sex) between the ICR and the FCC, partly because Native Americans at that time had no tradition recording year of birth, and perhaps partly because the Indian Agents that conducted the ICR were less concerned than Census enumerators of the FCC with getting accurate age information.

On the other hand, linking the ICR and the FCC is aided by two features. One is that Native Americans in the 1920s to 1940s were comparatively very immobile: the vast majority lived on or very near their tribe's reservation in both the ICR and the FCC.[20] The second key feature is that both the ICR and the FCC are organized by household and contain information on household relations (e.g. spouse, son, etc). We take advantage of this this second feature by expanding existing record linkage methods to take explicit account of household structure within our linkage algorithm.[21]

- We first used the `R`'s `RecordLinkage` package to perform an initial linkage, blocking on

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

[21] 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.0.1.

'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 between individual records was based on bigram-indexation of first and last name (using the stringsim() function), and allowing an age difference of no more than two years.

- Rather than keeping only the best possible match (which is the default), we kept all potential matches that passed a certain threshold of similarity, which was decided based on the distribution of similarity scores across the dataset.

- Records from the using data (the FCC) that have only one match are placed directly into the final output.

- For records from the using data that are matched to several different individuals in the master data (the ICR), we compared matches by calculating the similarity between the record's household in the using data, and the household of all potential records in the master data. Household characteristics include the size of the household and the gender distribution, but could be expanded (at a computational cost) to include the distribution of ages and relationships.

- We expanded this approach to account for cases where an individual match could only be true if the individual had changed households. For instance, an individual link between a 'child' in the ICR and a 'head' or 'spouse' in the FCC can only be true if the person who was a child in the 1920s had formed their own household by 1940. If this was true for the highest-score match, we placed this match directly into the final output and disregarded the others. If the highest highest-score match had a consistent household relation (e.g. 'child' in the master and using data), then we compared it only to potential matches that also had consistent household relations.

Table 2 shows examples of how this approach improves confidence in individual matches. All three families in the table have last names that are not exact matches across the two samples. The variable select in the center of the table indicates which match is chosen in the end. In all three cases, it is only the combination of seeing the households in full that generates confidence in the match

Table 2: Household-Information-Augmented Record Linkage Example

	ICR [master dataset]										FCC [using dataset]					
	individ. id	**hhid**	**birthyr**	**namelast**	**namefrst**	**relate**	**sex**	**links**	**select**	**score**	**individ. id**	**hhid**	**birthyr**	**namelast**	**namefrst**	**relate**
Family 1																
1	293557	35545638	1908	SESSPOOCH	WAUN	Head/househ	M	2	1	6.3	614-184-3	79055	1907	CESSPOOCH	JUAN	Head
	293557	35545638	1908	SESSPOOCH	WAUN	Head/househ	M	2		6.3	614-183-7	79051	1909	CESSPOOCH	CRUZ	Head
2	293558	35545638	1901	SESSPOOCH	ELLEN	Spouse	F	2	1	11.9	614-184-4	79055	1902	CESSPOOCH	ELLEN	Wife
	293558	35545638	1901	SESSPOOCH	ELLEN	Spouse	F	2		6.3	614-183-10	79053	1903	CESSPOOCH	HANNAH	Wife
3	293559	35545638	1934	SESSPOOCH	LOUIS	Son	M	5	1	11.2	614-184-5	79055	1933	CESSPOOCH	LEWIS	Son
	293559	35545638	1934	SESSPOOCH	LOUIS	Son	M	5		6.3	614-202-13	79127	1935	CESSPOOCH	DELMAR	Son
	293559	35545638	1934	SESSPOOCH	LOUIS	Son	M	5		6.3	614-184-2	79054	1936	CESSPOOCH	EDDIE	Son
	293559	35545638	1934	SESSPOOCH	LOUIS	Son	M	5		6.3	614-277-7	79461	1936	CESSPOOCH	JAMES	Son
	293559	35545638	1934	SESSPOOCH	LOUIS	Son	M	5		6.3	614-183-6	79050	1936	CESSPOOCH	JAMES	Son
4	293560	35545638	1937	SESSPOOCH	DEBOIA	Daughter	F	3		11.1	614-277-5	79461	1936	CESSPOOCH	DEVELIA	Daughter
	293560	35545638	1937	SESSPOOCH	DEBOIA	Daughter	F	3	1	6.3	614-184-6	79055	1936	CESSPOOCH	DOVELIA	Daughter
	293560	35545638	1937	SESSPOOCH	DEBOIA	Daughter	F	3		6.3	614-277-8	79461	1936	CESSPOOCH	VADA	Daughter
Family 2																
1	294094	35545903	1903	LA ROCE	TOMMY	Head/househ	M			11.0	614-217-10	79193	1903	LAROSE	THOMAS	Head
2	294095	35545903	1904	LA ROCE	MYRTLE	Wife	F			11.6	614-217-11	79193	1903	LAROSE	MYRTLE	Wife
3	294097	35545903	1929	LA ROCE	MARJORIE	Daughter	F			11.6	614-217-13	79193	1928	LAROSE	MARJORIE	Daughter
4	294096	35545903	1926	LA ROCE	FRANK	Son	M			11.6	614-217-12	79193	1924	LAROSE	FRANK	Son
5	294098	35545903	1934	LA ROCE	MONAS	Son	M	2	1	8.6	614-217-14	79193	1932	LAROSE	THOMAS	Son
	294098	35545903	1934	LA ROCE	MONAS	Son	M	2		5.9	614-217-2	79191	1933	LAROSE	JIMMIE	Son
Family 3																
1	294335	35546522	1916	MOWACHEAN	ELLEN	Wife	F			11.7	614-222-2	79209	1914	MAWOCHEAN	ELLEN	Wife
2	294334	35546522	1894	MOWACHEAN	SP**	Head/househ	M			6.1	614-222-1	79209	1896	MAWOCHEAN	SPEARS	Head
3	294336	35546522	1937	MOWACHEAN	VIRGINIA	Daughter	F	2	1	11.7	614-222-3	79209	1936	MAWOCHEAN	VIRGINIA	Daughter
	294336	35546522	1937	MOWACHEAN	VIRGINIA	Daughter	F	2		11.5	614-279-3	79461	1936	NEWOCHEAN	VIRGINIA	Daughter

Notes: This table shows how household-information can increase the confidence of individual level record linkages. The variable select in the center of the table indicates which match is chosen in the end. The hhid-cells for selected observations are also highlighted in grey. A family is defined as a household in the ICR data (master), and all possible matches from the FCC data (using) are shown for each member of each of the three ICR-households. All three families in the table have last names that are not exact matches across the two samples. Person 4 in Family 1 and Person 5 in Family 2 in particular look like poor matches when considered in isolation, but look like good matches when considered in the context of their households.

6 Results

Our focus is on the effects of Indian allotment on the treated Native American households. In principle there are two treatments one might want to consider: in-trust allotments, and fee-simple allotments. A less conservative specification might compare households that obtained no allotment to households with an allotment that either remained in trust or had been transferred into fee simple, resulting in expression 6 below, where the untreated population consists of Native Americans on the roughly half of all reservations that were never allotted. A concern with this less conservative specification is that there may be unobservable characteristics at both the household and the reservation level that correlated with whether a reservation was allotted or not. To address this concern, our favored, and more conservative, specification 7 includes reservation fixed effects, which absorb the treatment with an allotment, and leave only the comparison between having a fee-simple allotment and the omitted benchmark of in-trust allotments.

$$Y_{i(r)} = \alpha_F \cdot Fee_i + \alpha_T \cdot Trust_i \qquad +\beta' X_i + \gamma' X_r + \epsilon_{i(r)} \tag{6}$$

$$Y_{i(r)} = \alpha_F \cdot Fee_i + \qquad +\beta' X_i + \mu_r + \epsilon_{i(r)} \tag{7}$$

Specification 7 estimates the baseline effect of having a fee-simple allotment. To investigate whether the effect of obtaining personal wealth with full legal title was more pronounced on reservations whose ancestral tribal traditions allowed for private property, we estimate an interaction between the treatment variable and the ancestral traditions we coded from the EA

$$Y_{i(r)} = \alpha_F \cdot Fee_i + \alpha_{EA} \cdot Fee_i \cdot EA_r + \qquad +\beta' X_i + \mu_r + \epsilon_{i(r)}, \tag{8}$$

where EA_r is not included as a separate regressor because tribe-characteristics are absorbed by the reservation fixed effect μ_r.

Our approach is to split household-members into four categories— household heads, spouses (who are always female in the data), children in the household that are under 16 (who are unlikely to be employed), and children in the household that are 16 and over (who are likely to

be employed)— and to consider different outcome variables $Y_{i(r)}$ for these different household-groups.

Overall, we consider three types of outcome variables $Y_{i(r)}$ from the Full-Count Census. The first set of outcomes concern wealth or asset-ownership, where we consider a dummy for owning one's dwelling (`ownD`), and the value of that dwelling (`valueh`). We also include a dummy for being a farm household (`farm`), which is also a household characteristic in the census.

The second set of outcomes concern labor-market outcomes, where we consider a dummy for being in the labor-force (`labforceD`), as well as employment, measured by weeks worked in the previous year (`wkswork1`). We also consider two occupational rank indices provided by the census, both of which are ordinal and scaled to be between 1 and 100 if observed. These indices are Siegel's occupational prestige score (`presgl`), and the Duncan Socioeconomic Index (`sei`).

The third set of outcomes concern schooling, where we consider a dummy for currently attending school (`school`), and an ordinal categorical variable for years of educational attainment (`educ`), which takes 19 values: no schooling, K–12, 1–4 years of college or more than 4 years of college.

Table 3 reports on the results of estimating equation (7) for adults in the household. The top-panel reports on household-heads, the bottom-panel on wives. Asset-ownership is a household characteristic in the 1940 Census so that columns 1–3 are left blank in the wives panel. There is strong evidence for an effect of fee simple land on asset ownership, when the comparison group is Native households whose allotments remained in trust: households are 2 percent more likely own a house (although this effect is marginally insignificant), conditional on owning a house, its value is 27 percent greater. In addition, the household is also more likely to be a farm household, although it's not totally clear how to interpret this census variable because it combines working on a farm or owning a farm.

Columns 4–5 show no significant increase in the likelihood of being in the labor force or of being employed, but conditional on employment, columns 6–7 show a modest improvement in the occupational ranking of a household head. Interestingly, columns 6–7 also show a more sizeable reduction in the occupational ranking of wives, conditional on working. We have not explored this finding in more detail yet, but note that female labor force participation is very low, so that this finding needs to be taken with a grain of salt, and subject to further investigation. Column

8 reports on educational attainment (conditional on age): male household appear to have almost two-thirds of a year of additional schooling, while there does not appear to be an effect for wives. While this is subject to further investigation, one possibility is that wives are on average more likely to have completed schooling on reservations, so that a wealth shock would have been less likely to impact their schooling. Finally, column 9 reports on the likelihood of being currently in school (which is unaffected, and in any case near zero for adults).

Table 4 reports on the results of estimating equation (7) for children in the household. The top-panel reports on boys, the bottom panel on girls. Across columns, the table reports on variables about labor market outcomes (columns 1–4), and education/schooling (columns 5–6). Column 6 has the largest number of observations because it is estimated over all boys/girls. Labor force participation (columns 1–2) can only be investigated conditional on not being in school. Educational attainment (columns 5) is also investigated conditional on not being in school, because for children in school it is almost entirely a function of age. Finally, occupational rank (columns 3–4) is investigated conditional on working. All regressions include age and age squared as controls. Column 6 suggests that conditional on age, children of households with fee simple land stayed in school longer. This effect is more pronounced for boys, consistent with the explanation offered above that educational attainment for adult men was more affected because women were on average more likely to finish school. Column 5 shows that, once out of school, the average educational attainment of the children of households with fee simple land was higher. Columns 1–2 show a positive effect on labor force participation for girls out of school, but not boys. Conversely, columns 3–4 show a positive effect on occupational rank for boys out of school, but not girls, the same gender pattern we see for adults.

Table 5 follows the same structure as Table 3, but estimating equation (6) without reservation fixed effects. Table 6 follows the same structure as Table 4, but estimating equation (6) without reservation fixed effects.

Table 3: Effect of Fee-Simple Land Allotments on the Treated

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	asset-ownership			labor-market outcomes				education	
	ownD	valueh	farm HH	labforceD	wkswork1	presgl_	sei_	educrank	school
Panel A: Household-Heads									
fee-allotment	0.028*	0.195***	0.007	-0.011	0.033	0.820**	0.676	0.640***	0.001
	[0.085]	[0.000]	[0.510]	[0.521]	[0.970]	[0.043]	[0.181]	[0.000]	[0.711]
Observations	31,309	22,758	31,309	31,075	31,075	25,459	25,464	29,607	31,075
R-squared	0.163	0.192	0.245	0.276	0.225	0.122	0.023	0.391	0.012
Panel B: Wives									
fee-allotment				-0.002	0.013	-3.801***	-5.373**	0.152	0.002
				[0.725]	[0.971]	[0.006]	[0.011]	[0.255]	[0.326]
Observations				27,024	27,024	4,465	4,466	25,835	27,024
R-squared				0.211	0.153	0.118	0.191	0.437	0.008

Notes: This table reports on the results of estimating equation (7), with no reservation fixed effects. This table reports separately on household-heads in the top-panel and wives in the bottom-panel. The table reports on asset-ownership (col 1–3), labor market outcomes (col 4–7), and education (col 8–9). In square brackets are p-values for standard errors clustered on the reservation; *** p<0.01, ** p<0.05, * p<0.1

Table 4: Effect of Fee-Simple Land Allotments on the Treated's Children

	(1)	(2)	(3)	(4)	(5)	(6)
	labor-market outcomes				schooling	
	labforceD	wkswork1	presgl_	sei_	educrank	school
Panel A: Boys						
fee-allotment	0.009	0.376	0.821	1.494***	0.584***	0.034***
	[0.404]	[0.361]	[0.160]	[0.008]	[0.000]	[0.005]
Observations	7,602	7,602	2,486	2,486	7,028	13,458
R-squared	0.629	0.387	0.095	0.055	0.736	0.157
Panel B: Girls						
fee-allotment	0.019**	0.849**	-0.698	-2.433	0.577***	0.021*
	[0.030]	[0.020]	[0.552]	[0.281]	[0.000]	[0.074]
Observations	7,575	7,575	453	453	6,876	14,188
R-squared	0.159	0.115	0.181	0.178	0.751	0.140

Notes: This table reports on the results of estimating equation (7), with no reservation fixed effects. This table reports separately on boys in the top-panel and girls in the bottom-panel. Across columns, the table reports on variables about labor market outcomes (col 1–4), and education/schooling (col 5–6). labor force participation (col 1–2) and education attainment (col 5) are investigated conditional on not being in school, occupational rank (col 3–4) is investigated conditional on working. All regressions include age and age squared as controls. In square brackets are p-values for standard errors clustered on the reservation; *** p<0.01, ** p<0.05, * p<0.1

Table 5: Effect of Fee-Simple Land Allotments on the Treated

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	asset-ownership			labor-market outcomes				education	
	ownD	valueh	farm HH	labforceD	wkswork1	presgl_	sei_	educrank	school
Panel A: Household-Heads									
trust-allotment	0.097***	-0.125*	0.044	-0.014	-3.146**	-0.042	-0.832**	0.342	-0.002
	[0.000]	[0.057]	[0.106]	[0.121]	[0.041]	[0.929]	[0.027]	[0.354]	[0.269]
fee-allotment	-0.043**	0.266***	-0.009	0.004	2.295**	0.929*	1.352**	0.549*	0.002
	[0.030]	[0.000]	[0.693]	[0.768]	[0.033]	[0.086]	[0.017]	[0.070]	[0.399]
Observations	31,309	22,760	31,309	31,075	31,075	25,460	25,465	29,607	31,075
R-squared	0.142	0.141	0.183	0.230	0.174	0.097	0.009	0.352	0.005
Panel B: Wives									
trust-allotment				-0.078***	-3.199***	0.688	1.470	0.316	-0.000
				[0.001]	[0.001]	[0.460]	[0.314]	[0.460]	[0.728]
fee-allotment				0.052***	2.190***	-4.126***	-7.158***	0.119	0.001
				[0.006]	[0.005]	[0.003]	[0.002]	[0.736]	[0.600]
Observations				27,024	27,024	4,474	4,475	25,835	27,024
R-squared				0.181	0.122	0.077	0.134	0.386	0.002

Notes: This table reports on the results of estimating equation (6), with reservation fixed effects. This table reports separately on household-heads in the top-panel and wives in the bottom-panel. The table reports on asset-ownership (col 1–3), labor market outcomes (col 4–7), and education (col 8–9). In square brackets are p-values for standard errors clustered on the reservation; *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 6: Effect of Fee-Simple Land Allotments on the Treated's Children

	(3)	(4)	(3)	(4)	(5)	(6)
	labor-market outcomes				schooling	
	labforceD	wkswork1	presgl_	sei_	educrank	school
Panel A: Boys						
trust-allotment	-0.003	-0.876	0.819	-1.043	-0.095	-0.029
	[0.885]	[0.466]	[0.411]	[0.330]	[0.805]	[0.507]
fee-allotment	0.008	0.062	0.707	1.214**	0.606***	0.026**
	[0.488]	[0.897]	[0.173]	[0.028]	[0.000]	[0.026]
Observations	7,607	7,607	2,495	2,495	7,033	13,461
R-squared	0.621	0.373	0.055	0.019	0.729	0.148
Panel B: Girls						
trust-allotment	-0.022	-1.454	-1.604	-1.146	-0.110	-0.055
	[0.554]	[0.381]	[0.230]	[0.521]	[0.803]	[0.310]
fee-allotment	0.028***	0.821**	-1.220	-3.695	0.618***	0.015
	[0.002]	[0.022]	[0.374]	[0.156]	[0.000]	[0.241]
Observations	7,582	7,582	465	465	6,883	14,192
R-squared	0.145	0.100	0.077	0.063	0.745	0.130

Notes: This table reports on the results of estimating equation (6), with reservation fixed effects. This table reports separately on boys in the top-panel and girls in the bottom-panel. Across columns, the table reports on variables about labor market outcomes (col 1–4), and education/schooling (col 5–6). labor force participation (col 1–2) and education attainment (col 5) are investigated conditional on not being in school, occupational rank (col 3–4) is investigated conditional on working. All regressions include age and age squared as controls. In square brackets are p-values for standard errors clustered on the reservation; *** $p<0.01$, ** $p<0.05$, * $p<0.1$

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Online Appendix

to

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

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



Notes:

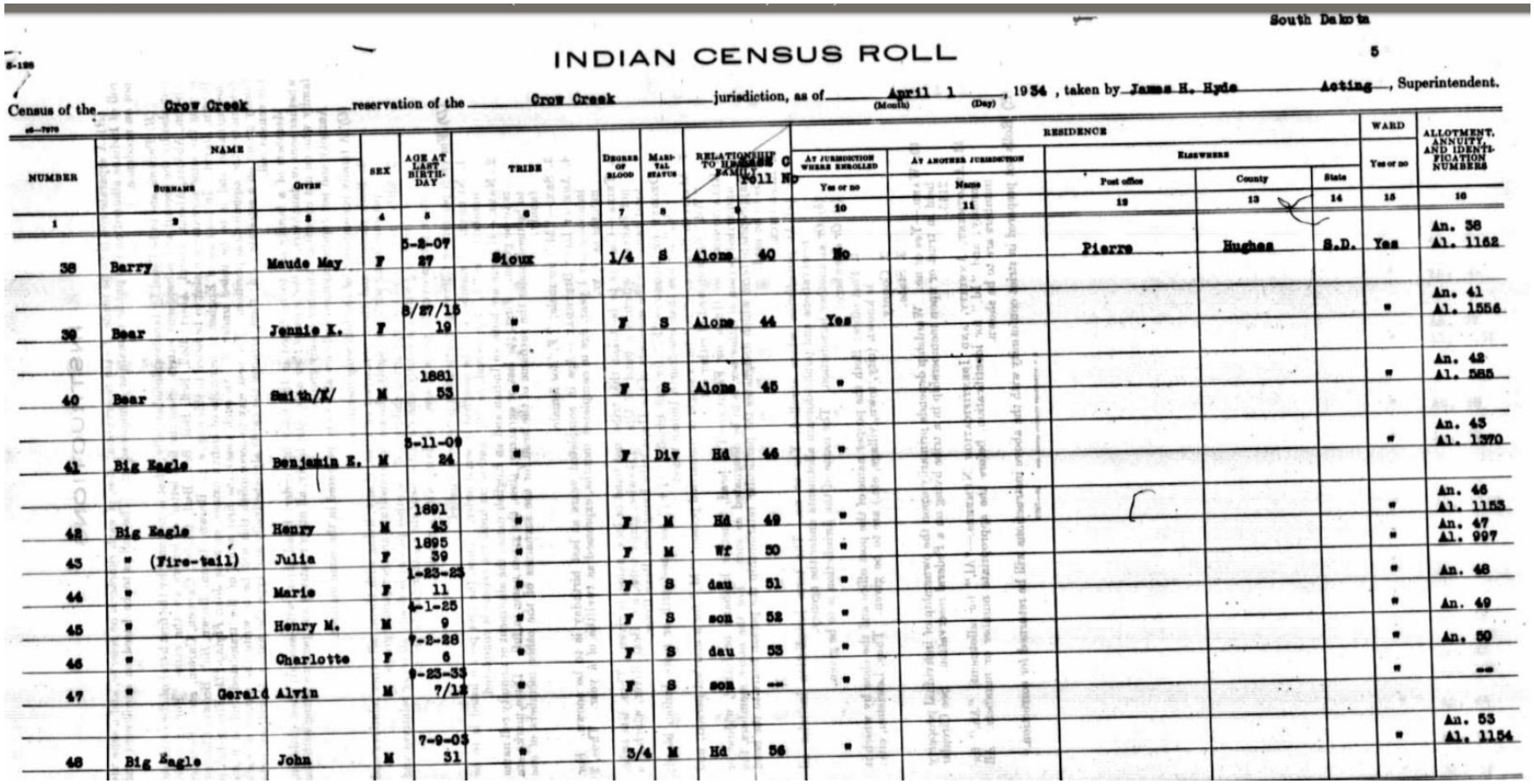
Online Appendix A Online Data Appendix

Online Appendix A.0.1 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

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



South Dakota

5

INDIAN CENSUS ROLL

Census of the Crow Creek reservation of the Crow Creek jurisdiction, as of April 1 (Month) (Day), 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)	RESIDENCE: At another jurisdiction (Name)	RESIDENCE: Elsewhere: Post office	RESIDENCE: Elsewhere: County	RESIDENCE: Elsewhere: 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	5-8-07 27	Sioux	1/4	S	Alone	40	No		Pierre	Hughes	S.D.	Yes	An. 38 Al. 1162
39	Bear	Jennie K.	F	8/27/13 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	3-11-09 24	"	F	Div	Hd	46	"					"	An. 43 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:

Online Appendix – Not for Publication

Table Online Appendix Table 1: Ethnographic Atlas Variables 'Property Rights' and 'Bride Price'

D(Property Rights)		EA: 76. Inheritance Rule for Movable Property	D(Bride Price)		EA: 6. Mode of Marriage (Primary)
0	1	Absence of individual property rights or rules	1	1	Bride Price or wealth, to bride's family
1	2	Matrilineal (sister's sons)	1	2	Bride Service, to bride's family
1	3	Other matrilineal heirs (e.g., younger brothers)	1	3	Token bride price
1	4	Children, with daughters receiving less	0	4	Gift Exchange, reciprocal
1	5	Children, equally for both sexes	0	5	Sister or female relative exchanged for bride
1	6	Other patrilineal heirs (e.g., younger brothers)	0	6	Absence of consideration
1	7	Patrilineal (sons)	0	7	Dowry, to bride from her family

Notes: TBA

outlined in Feigenbaum (2016). There is an active and ongoing debate on the choice of methods. Bailey, Cole, Henderson, and Massey (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, Boustan, Eriksson, Feigenbaum, and Pérez (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.[22]

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