Land Concentration and Long-Run Development in the Frontier United States

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Abstract

I study the long-run economic effects of land concentration on the American frontier. Using quasi-random variation in initial land allocations from a checkerboard formula, I analyze a large database of property assessments and find that historical concentration reduced modern land values by 4.5% and fixed capital by 23%. Modern effect sizes are 23%—64% of their historical equivalents, indicating significant rates of both persistence and convergence over the last 150 years. Using archival data on tenant contracts, I argue that the low-powered incentives of share agreements discouraged investment by large-scale owners with long-term effects.

Keywords: land concentration, land allocation, American frontier

JEL codes: O13, N51, Q15

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1 Introduction

Land ownership is unequal in many agricultural economies, including both developed countries historically and many developing countries today. Large landholders often play a dominant economic and political role in their societies, and a longstanding question in economics is whether this situation encourages or discourages production and growth. On the one hand, large properties could encourage investment through economies of scale or because their owners can internalize externalities over broader areas. On the other hand, large landholders large landholders face principal-agent problems when renting to tenants. Compared to owners who operate their own lands, contracts with tenants may distort incentives and discourage investment. A third possibility is that patterns of land ownership per se are unimportant for growth. If markets reach the ideals described in the Coase Theorem, land is reallocated to the most efficient owners in equilibrium (Coase, 1960).

In many settings, the largest farms tend to be the most productive (Sumner, 2014), but the long-run effects of land concentration are difficult to study for several reasons. First, this analysis would require a historical source of variation and sufficient data to examine its effects over a substantial time span. Second, land concentration is typically entangled with other factors connected to economic development including geography, government-backed land redistribution, and overall wealth inequality. Simple correlations are thus unlikely to uncover causal effects. As such, less is known about how land concentration shapes economic outcomes over longer periods of time.

In this paper, I estimate the long-term effects of land concentration using a natural experiment generated by idiosyncratic land policies in the American West. Although this region is sometimes popularized as the domain of independent pioneers, in practice many parts of it were also held by large-scale landowners who engaged in tenant farming. To capture this contrast, my work compares two major pillars of American land policy in the 19th-century: the 1862 Homestead Act and railroad land grants. The former aimed to reserve the frontier for small-scale owner-cultivators and restricted settlers from receiving more than a particular amount of land. This cap was initially set at 160 acres, roughly the average farm size in my sample areas and the US overall (Census of Agriculture, 1880). Paradoxically, the government also used millions of acres of farmland as in-kind payments to railroad companies who in turn typically sold them to wealthy purchasers without any size restrictions. Different parts of the American frontier thus sharply contrasted in whether initial settlement favored large- or small-scale ownership.

These two policies were applied in an arbitrary manner that increased land concentration in alternating square miles. Before settlers arrived, land had been divided into a square grid by the Public Lands Survey System (PLSS), and each square-mile "section" received an identification number from 1 to 36. In policy areas, railroad companies received the odd-numbered sections, while the even-numbered ones were primarily settled via the Homestead Act. This formula was often called a "checkerboard" as it resulted in an alternating pattern of ownership between small-scale owners (via the Homestead Act) and large-scale owners (via railroad land purchases). The formula balanced the quality of land in odd- and even-numbered sections, with geographic characteristics typically differing by less than 0.002 standard deviations.

I find that historical land concentration led to fewer investments and a less intense practice of agriculture for approximately 150 years after the initial allocations. Today, odd-numbered sections have 4.5% lower land value and 23% less fixed capital relative to their even-numbered neighbors. In several case studies, I use archival data to trace these differences back to the early 1900s. Odd sections had less "improved" land cleared for crop cultivation and more "unimproved" land devoted to less intensive forms of agriculture, such as cattle grazing. Modern even/odd differences are 23%–64% the size of equivalent ones in the early 1900s, suggesting that both persistence and convergence shaped the evolution of economic activity over time. Markets tended to reallocate land in a Coasian manner as the importance of the initial allocation diminished. However, this process unfolded gradually as I can still discern non-trivial differences in land use and value in 2017. Since the Homestead Act and railroad grants governed the initial settlement of approximately 25% of the continental United States, land concentration impeded growth in at least one important setting.

These results rely on original data sources that measure farm investment and productivity at the microscale, allowing me to fully exploit the natural experiment of the checkerboard formula. My modern data come from tax assessments covering 12 million properties over 380,000 square miles, with $600 billion of agricultural land in 2017. To provide evidence on mechanisms, I digitize archival records documenting farm-level ownership and operation details. I also assemble historical georeferenced data on land use and public goods. These data allow me to trace the policies' effects back in time, providing important information on the timing and sources of land concentration's effects. While these data are more limited in geographic scope, differences in modern outcomes are consistent across most subsamples of the data, suggesting the fundamental mechanisms were widespread.

My result that land concentration discouraged investment, despite plausible channels in the other direction, points to the need to establish the mechanisms. In this context, I provide evidence that land concentration's long-run impacts stemmed from tenant farming contracts, especially share contracts that split output between owners and tenants. Most directly, I use archival data to show that land concentration increased rates of non-owner operation by 10 percentage points (27%) in a 1940 survey and crop share agreements by 3.7 points (18%) in a 1965 one. The 50-100 year distance of these surveys from the initial allocation suggests earlier differences were much larger. I additionally conduct a heterogeneity analysis using county-level variation in the frequency of share agreements with tenants. Counties with high rates of share tenancy experience the largest land value loss in odd sections; counties with high rates of cash rental experience few or no losses. These differences are not driven by different state or land quality composition. The results are thus consistent with classic principal-agent theories that emphasize that share contracts reduce agents' incentives to provide costly inputs and are thus second-best solutions to contracting constraints. My results show that by shaping land use and investment, these concerns can affect economic outcomes over periods much longer than an individual contract.

I find little evidence for other mechanisms in this context. The main results are similar across all states and railroad companies in my sample, meaning that none of their individual policies explain the effects. Quantitative and qualitative discussion of the Homestead Act rules out its peculiar features as explanations. The tight zeros on land quality differences in the modern data address both concerns of an imbalance across the two groups as well as post-allocation environmental degradation. Differences in public goods provision at the square mile level are small and disappear when accounting for population differences. Since my analyses occur at the square mile level, they are not well-suited to testing whether land concentration affected political outcomes at higher levels. However, that equally implies that such mechanisms are unlikely to explain my results, which rely on variation within political units. I individually argue against a longer list of alternate mechanisms including urban growth, fragmented property ownership, and insecure property rights.

Over time, market transactions slowly reduced the importance of the initial allocation, albeit not completely. Under ideal conditions, large-scale owners could have divided and sold their properties to neighboring Homestead or similar farmers who would use it more intensively, in line with the predictions of the Coase Theorem. In practice, this was likely difficult. In my archival data, odd/even differences in property sizes diminished steadily throughout time, with no particular year or period precipitating convergence. While many market imperfections plausibly operated in this setting, I provide evidence that one barrier was the costs small-scale owners faced in raising capital. Using early 1900s tax records, I show that even-section owners took longer to pay their taxes and were more likely to do so through an intermediary. This reflects the motivation for the Homestead Act's provision of free land, aimed at attracting settlers who could not otherwise afford to purchase it.

In the final part of my analysis, I show that the impacts of the railroad land grants do not shrink at a higher level of aggregation. In theory, Homestead lands could have benefited at the expense of railroad lands, meaning that the policy simply reallocated scarce resources across farms and the even/odd differences overstated the total impact. I test for this effect by comparing non-railroad lands at the policy boundary with those just outside in a regression discontinuity (RD) framework. Surprisingly, I find that even sections within the checkerboard have lower valuations than land just outside the grant area despite being initially allocated under the same policies.

This paper contributes to several strands of literature. A very long tradition in economics has debated the relative efficiency of different modes of land ownership. Classic theories dating to Adam Smith and Alfred Marshall proposing that share contracts reduce (short-term) production by limiting tenant effort have received experimental support (Smith, 1776; Marshall, 1890; Burchardi et al., 2018). More recent theory has considered how these contracts could reach first-best outcomes (Cheung, 1969), or at least second-best ones given constraints on resources, risk sharing, and monitoring (Reid, 1977; Winters, 1974; Alston and Higgs, 1982; Eswaran and Kotwal, 1985; Naidu, 2010). In development economics, these questions are intimately related to the study of land reforms, which have had notably heterogeneous effects (Ghatak and Roy, 2007). Some research has found that stronger tenant incentives increase output (Banerjee, Gertler and Ghatak, 2002; Markevich and Zhuravskaya, 2018), but others find mixed (Montero, 2022; Besley and Burgess, 2000) or negative effects (Adamopoulos and Restuccia, 2019). Broadly, negative impacts could be due to disruptions from property confiscation or the loss of scale economies. On the question of size, the smallest farms in developing countries exhibit diseconomies of scale (Foster and Rosenzweig, 2022), though there is debate over causality (Benjamin, 1995; Desiere and Jolliffe, 2018). In the United States, size and productivity are positively correlated (Sumner, 2014). Scale economies might also foster the adoption of technologies like mechanization or facilitate the provision of public goods (Olmstead and Rhode, 2001; Allen, 1988; Dell, 2010; Hornbeck and Naidu, 2014). I contribute to this discussion by showing that the incentive problems of tenant farming dominated in the context of the American West, with land concentration reducing most measurable agricultural inputs. Public goods like schools do not increase, at least when measured locally on individual square miles.

This paper also contributes to work on the effects of property rights on economic development, particularly within the agricultural US (Hornbeck, 2010; Bühler, 2023; Dippel, Frye and Leonard, 2020; Hagerty, 2022). Several studies have documented persistent impacts from systems of surveying and delineating parcel boundaries (Bleakley and Ferrie, 2014; Libecap and Lueck, 2011). In contrast, I explore the effects of more concentrated ownership within a fixed parcel system.

Finally, this paper also contributes to work on the economic history of the American frontier (Bazzi, Fiszbein and Gebresilasse, 2017; Raz, 2021; Allen and Leonard, 2021). An important strand of this body specifically focuses on the Homestead Act, with recent work by Mattheis and Raz (2023) finding that small-scale, private ownership was superior to Homestead Act settlement. To the extent that homesteaders compared unfavorably to those who purchased land, my results understate the inefficiency of large landlords. A small literature has also investigated the effect of legal and regulatory issues of the checkerboard pattern. In particular, Alston and Smith (2022) similarly conducts a section-level analysis focused on legal controversies surrounding Montana's Northern Pacific Railroad grant. My paper adds to this literature by broadly evaluating the railroad grant policy across multiple states and companies, focusing on its effects on land concentration.

The rest of the paper is as follows. Section 2 discusses the historical background of American land policy and the railroad land grant formula. Section 3 discusses the conceptual framework. Section 4 describes my data sources, and Section 5 presents results confirming initial land concentration and land quality balance. Section 6 details my main results on land values and investment, and Sections 7–8 discuss mechanisms, frictions, and aggregate effects. Section 9 addresses alternative mechanisms, and Section 10 concludes.

2 Historical Background

2.1 American Land Policy

The rapid expansion of the United States and its dispossession of Native American peoples allowed the country to demarcate frontier areas in a highly regularized manner. Beginning in 1785, this was done via the PLSS, which divided the new areas into an essentially square grid. The grid's main units were 6-by-6 mile squares called townships, further subdivided into 36 sections of 1 square mile (640 acres). Each section was identified by a number between 1 and 36, which corresponded to its location within a township. Appendix[1] Figure A.1a shows an example of this division, depicting townships with their numbered sections. Most states created after American independence adopted the PLSS, and its grid pattern still determines many of today's parcel boundaries.

The 1862 Homestead Act markedly changed American land policy to favor small family farmers, making it a watershed moment in the country's history. Initially, the government sold land at a standard price of $1.25 per acre with few restrictions on scale which favored large-scale purchases from those with access to capital (Gates, 1936). Building off the earlier Preemption Act (1841), The Homestead Act offered farmers a maximum-sized "quarter section" (160 acres)[2] for a small fee if they agreed to farm it for five years; more eager settlers could purchase the title after just six months. Afterward, settlers received, unrestricted

[1]Appendices A-E, including tables and figures, are available online.

[2]Congress subsequently adjusted the Act's acreage limit with a number of amendments, though many of these came at the tail end of the settlement process. For example, the 1909 Enlarged Homestead Act increased the limit to 320 acres in areas of poorer quality. For simplicity, the text refers to the 160-acre standard given its prevalence, but in practice a range of allotted sizes were possible; see Section 5.3 for data.

ownership of their land.

The Homestead Act's property size cap, nearly free land, and residence requirement combined to encourage owner-cultivator settlement. Large-scale owners would have been uninterested in the smaller plots, and the residency requirement would have discouraged absentee renting. Although some "dummy entrymen" fraudulently served as placeholders for wealthier buyers (Bradsher, 2012), neither fraud nor adjustments to the Homestead Act overturned its promotion of small-scale ownership relative to alternative policies (see Section 5). With 1.6 million parcels granted nationally (Edwards, 2008), Homesteading quickly became the dominant form of federally administered settlement in my sample states. I thus use "federal" and "Homestead" settlement interchangeably.[3] I discuss further details of the Homestead Act and their relevance for my results in Section 9.2.

Paradoxically, other contemporary American land policies promoted concentrated landholding, and the most important of these were arguably railroad land grants. Beginning in 1850, federal and state governments funded railroad construction by giving companies thousands of square miles of unsettled land as in-kind payments. Companies sold these lands, notably without any restriction on purchase size or expectation that owners personally work their land (Ellis, 1946). As such, "the land policies of the railroads encouraged speculative and large-scale purchases with the result that millions of acres ... were rented or leased to incoming settlers who had expected to find free land" (Gates, 1936). This dynamic held true across many different railroad companies as "the plain fact is that basically their [sales] policies were identical" (Greever, 1951).

The Homestead and railroad grant policies were the two largest elements of American land policy at the time, making their potential impacts quite large. Homestead grants probably amounted to around 270 million acres of farmland (Edwards, 2008) and railroad land grants another 170 million (Decker, 1964). Together, these policies governed how roughly one-quarter of the continental US was settled and developed.

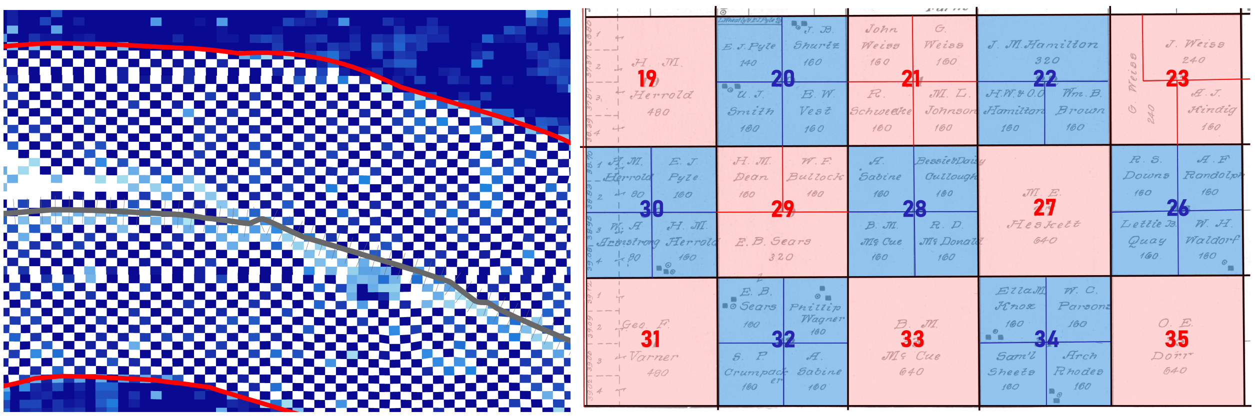
2.2 The Railroad Land Grant Formula

The allocation of lands to Homestead or railroad grants was determined formulaically, and this forms the core natural experiment of the paper. Companies were awarded land near their tracks, but the federal and state governments were reluctant to give away too much. They thus settled on a formula that gave railroads "every other" PLSS section (square mile) of land, ensuring an equal division of the policy area. This was implemented by reserving even-numbered sections (2, 4, 6...) for Homesteads and odd-numbered sections (1, 3, 5...) for railroads. In my sample states, a large majority of settled government land within the

[3] See Table 1. In other states with more pre-Homestead settlement, this would be less applicable.

grant boundaries was given to Homesteaders (see Table 1). The primary exceptions were the "education sections" administered by local government, pre-determined to be number 16 in Florida and 16 and 36 in my other five states.[4] The grant areas were typically determined based on sharp cutoffs, with companies receiving land within a fixed distance of their track.

Figure 1: Railroad Grants and Checkerboarding



(a) Federal Land Transfers, Nebraska **(b) Farm Properties 1910, Finney County, Kansas**

Notes: Panel (a) shows in blue the percentage of land transferred by the federal government to settlers according to US Bureau of Land Management records. White sections indicate no federal transfers, typically due to railroad ownership. The area is centered around the Union Pacific line in western Nebraska. Panel (b) shows 1910 farm properties ("plat map") in Finney County, Kansas with an overlaid color scheme.

Compliance with the even/odd formula was high but not perfect. Settlers of odd-numbered sections who preceded the railroad were allowed to keep their claims. Some individuals also effectively managed to purchase even-numbered sections by having accomplices fraudulently pose as Homesteaders. Because these deviations are unlikely to have been random, I use an intent-to-treat (ITT) strategy, comparing even and odd non-education sections.

Visually, the grant formula led to what is known as the alternating checkerboard pattern. Figure 1a shows in blue the fraction of each section transferred to settlers by the federal government according to US Bureau of Land Management (BLM) records. Odd sections were held by railroads and so are typically colored white, leading to a side-by-side contrast within the grant area. Figure 1b shows how this pattern manifested in the sizes of early farm properties of one township. Sections 19, 27, 31, 33, and 35 (all odd) were held by single owners, whereas 20, 21, 26, 28, 30, 32, and 34 (mostly even) were split into four standard

[4] These pre-determined lands were reserved for sale by local governments to fund education and the arbitrary locations of these sections make them an interesting topic for future research. However, they were often mismanaged by local administrators and were excluded from some key data sources; these dynamics fall outside this paper's scope. See Swift (1911); Schaede and Smith (2024).

160-acre Homestead farms. Although the map does not highlight it, odd-section owners could and did obtain multiple 640-acre sections.

2.3 Comparisons to Contemporary Farm Sizes

The two policies I study contrasted typical small-scale ownership and large-scale holdings in the context of the late 1800s US. The Homestead Act's standard 160-acre standard was fairly typical for farms of that era, with Census of Agriculture (1880) reporting an average farm size of 162 acres for my sample of six frontier states and 134 acres for all US states.[5] Although railroad land beyond a 640-acre section was connected only diagonally, this size was likely large enough to realize most contemporary economies of scale. In 1880, only 1.6% of farms in my six sample states and 2% in the US overall exceeded this size.[6] Buyers could and did purchase multiple squares, meaning individual holdings could easily be thousands of acres; see Section 5.3. In the long run, railroad land purchasers could also connect these already large squares by buying even-section plots.

3 Conceptual Framework

To structure my empirical work, I develop an illustrative model that describes how historical concentration affects land investment and ownership over the long run. A full mathematical description is given in Appendix Section A. The model compares two types of landowners with different production technologies: small-scale owners who work their own land and large-scale "landlords" who rent their properties to tenants. The static portion of the model replicates standard principal/agent theory with owner-operators and tenants who pay fixed cash rents achieving first-best outcomes. However, tenants' lack of resources can constrain some landlords to using second-best share contracts that decrease production through low-powered incentives. These owners receive lower returns from intensive use of their land and, consequently, may forgo investments that the other groups find beneficial.

I embed the static problem in a dynamic framework where small-scale owners face stochastic costs to raising capital. Consequently, the optimal reallocations of the Coase Theorem may not occur, and initial ownership has persistent effects. Initially, land may be allocated to either a small- or large-scale owner. When large-scale owners are constrained to second-

[5] A separate historical debate concerned whether the 160-acre standard was sufficiently large (Ely and Wehrwein, 1940). To the extent that this involved economies of scale, it would support the larger allotments possible under railroad grants. To the extent it reflected views over standards of living, it would be less relevant for the questions of investment and long-run productivity studied by this paper.

[6] Census-defined farms reflect operational scale rather than holdings. For this calculation, I assume a uniform distribution of farms in the census's 500–999 acre category.

best share contracts, they value improved land relatively less and are relatively less likely to purchase it. Consequently, land initially allocated to a large-scale owner is more likely to be owned by them in the future and less likely to receive investment. However, the frictions do not always prevent reallocation, meaning that patterns of investment and ownership converge over time. When cash rent is possible, convergence is immediate since both owner types act identitically in the static case.

Overall, the model predicts that when conditions result in share contracts, concentrated ownership persistently reduces investment. Coasian convergence, however, occurs asymptotically as market reallocations continually shrink the importance of the initial allocation.

3.1 Historical Support

The conceptual framework proposed in this text is in line with historical and contemporary evidence about the US agricultural economy circa 1900. Gates (1942) discusses how land concentration reduced investment on the American frontier, writing that "[large-scale] ownership and tenancy did not always result in the best use of the land." Kansas farms surveyed by Grimes (1919) specifically reported lower rates of investment for farms under share versus cash rent because "the landlord receives his share of the benefits without sharing the expense."

Despite potential disadvantages, the USDA (1923) notes that the "concentration of land ownership in large holdings is favorable to landlordism and tenancy." The ability of landlords to use cash rather than share rent was particularly limited by resource constraints: "When tenants are able to pay cash ... landlords are more likely to be willing to rent for cash than when the opposite conditions prevail." Finally, Rajan and Ramcharan (2015) note that constraints to credit were an important determinant in whether "tenants or farm workers could buy land off landlords, eliminating the agency costs associated with tenancy."

4 Data

4.1 Modern Outcomes

Land values are a natural outcome to study for agricultural economies and reflects the net present value of profits to current and future owners (Borchers, Ifft and Kuethe, 2014). I draw especially on land assessments by county and state governments for this project. Assessors either attempt to find comparable properties recently sold or estimate the net income of the property based on current environmental and use characteristics. Thus, a cattle ranch is evaluated based on the assessor's belief about the net income from cattle even if they believe wheat farming would be more profitable. Although each county's assessment procedures have unique elements, all comparisons in this paper are within county. This consistency alleviates concerns that individual farmers might systematically misreport characteristics of their farms in surveys (Desiere and Jolliffe, 2018).

I assemble 2017 assessment data from six US states covering 12 million parcels spread over 380,000 square miles accounting for $600 billion in agricultural land. The records detail each property's total value, the value of "improvements" (buildings and fixed capital, e.g., barns, irrigation systems), housing value, and the number of parcels. To measure the appropriateness of land use, I analyze the USDA's satellite-derived CropScape dataset that codes usage into distinct crop, grassland, and developed area categories at the 30-by-30 meter level. I then use these data to compute an estimated "use value" for land based on the expected profits from its current agricultural use. To estimate profits, I combine the CropScape data with productivity measures from the FAO GAEZ dataset and USDA and other price and profitability rates (see Appendix Section B.8). Thus, land with the same underlying geographic features can vary in its value based on its current usage. In 13 of my 322 counties, substantial areas of government land are not assessed, leading many economically active parcels to receive a $0 value. For these counties, I treat the satellite-derived use value as the total property value, though the results are essentially unchanged if these counties are instead dropped. Appendix B details the sample construction procedure and further GIS sources. Modern coverage is shown in Appendix Figure A.1b.

4.1.1 Validating Assessed Values

I validate the relevance of assessed land values by comparing them to sale prices in Florida, where the assessment data include sale amounts for 2016-2017. Appendix Table A.1 shows that assessed total values per acre are highly correlated with sale prices per acre. Aggregated to the PLSS section level as in my main analysis, the elasticity is 0.94. This result mirrors other literature, which find that assessed values are highly predictive of sales values, though other factors often have residual information (Bigelow, Ifft and Kuethe, 2020). Similarly, the usage-based values I construct are highly correlated with assessed values. Both results remain even for comparisons within the small area of a township. In both cases, valuations excluding improvements have either smaller or no additional predictive power.

4.2 Historical and Archival Sources

To elucidate the timing and mechanisms of effects from historical land concentration, I turn to archival records detailing land ownership, land use, and population from the late 1800s and early 1900s. My research either was conducted in person at the Nebraska State

Archives or used digitized records from individual counties, the Library of Congress, and FamilySearch.org (see Appendix Table A.2 for the full list of sources). Where property or sales records give owner names, I link these owners to census microdata as described in Appendix Section B.5.

Because historical land records are typically collected and held locally, historical data for this project are often only available for individual counties or states. For this reason, such analyses are limited to subsamples where key outcomes are available, and stronger assumptions of external validity are required. However, my main results on modern property values hold across a variety of state and land quality subsamples (Section 9.3). As such, the same mechanisms likely applied broadly, meaning that these smaller samples can still be informative about the full sample.

Table 1: Summary Statistics

	PLSS Sections						Counties (1940)	
	(1) Number Sections	(2) Soil Quality (z-score)	(3) Homestead (%) (Even Only)	(4) Crops (%)	(5) Value \$000	(6) # Parcels (Median)	(7) Tenant Farm (%)	(8) Share Farm (% Tenant)
Whole Sample	386,224	0	84.2	49.6	6,712	3	37.8	39.8
RR Grant Areas	132,463	-.046	86.9	47.9	2,231	2	38.8	42.8
(FL) Florida RR	1,406	-.072	93.2	37.9	30,270	70.1	13.5	21.2
(FL) Pensacola	7,857	.46	90.3	55.7	5,844	18	29.9	43.7
(KS) Atchison & Santa Fe	7,176	1.1	96.5	87.7	1,033	5	47	61
(KS) Union Pacific	12,512	1.1	91.7	85.6	895	4	43.6	51.8
(MT) Northern Pacific	62,253	-.78	80.3	29.6	1,361	1	26.9	41.2
(NE) Burlington	9,011	1.7	93.4	95.3	2,923	6	55.8	33.7
(NE) Union Pacific	14,868	.81	96.5	80.4	2,273	5	52.3	40.5
(OR) Oregon & California	1,422	1.4	26.5	41.2	16,134	10	18	19.4
(WY) Union Pacific	15,958	-.8	89.7	12	1,296	.505	15.6	36.9

Notes: The table presents summary statistics for different geographic units. Column (1) shows results for section sample size, column (2) for the gSSURGO crop productivity index (full-sample z-score), column (3) for percentages of non-railroad land transferred under the Homestead Act, column (4) for the percentage of sections with at least 1% in crops per the USDA CropScape data, column (5) for total property values, column (6) for the median number of parcels, column (7) for county-level average rates of non-owner-operated (tenanted) 1940 farms, and column (8) for county-level averages of the shares tenant farms as a fraction of all tenant farms in 1940.

4.3 Summary Statistics

Summary statistics are given for key variables in Table 1. Columns (1)–(6) show average or median statistics at the PLSS section (square mile) level, and columns (7)–(8) show averages for 1940 counties. Overall, the sample is large and features a diverse set of agricultural conditions. The main sample includes about 130,000 grant area sections, just under half of which today grow some crops. 87% of settled Homestead-eligible (even, non-education) sections used the Homestead Act or its extensions. Another 4% of land in these areas was similarly targeted toward smallholders, and only 9% were open to concentration due to settlement via public sale.[7] Finally, despite the popular image of the West as a bastion of pioneer independence, a diversity of landholding patterns prevailed. About 40% of farms operated under tenant contracts, and 40% of those used share arrangements.

Comparable statistics are given for the archival sample counties in Appendix Table A.3. These samples cover a range of areas. For most variables, there is at least one above average and one below average area.

5 Empirical Strategy

5.1 Unit of Analysis

For all regressions, the unit of analysis is a PLSS section, and all outcomes are aggregated at this level. Importantly, PLSS sections are pre-defined, natural units of area. Alternative units such as farms or parcels are formed endogenously and are shaped by the land allocation policy itself (see Figure 2b), making them more complex to analyze. Because assessors evaluate property at the parcel level, rather than the owner level, the vast majority of assessments fall within a single section; 93% of grant area sections in the modern data are formed from whole parcels. In the minority of cases where a parcel is split across multiple sections, I allocate its value uniformly by area. In the case of statistics about owners, I assign parcels the value of their owner's characteristics and compute the area-weighted average. Thus, outcomes always reflect the characteristics of a typical unit of land.

5.2 Even/Odd Regression

Within the checkerboard areas, I compare even (small-scale/Homestead) and odd (large-scale/railroad) sections. Since the even-odd distinction stemmed from surveying decisions made before the railroad grants, there should be no unobserved average quality differences between the two groups. I run regressions of the form

$$y_i = \alpha \mathrm{RR}_i + X_i \beta + \varepsilon_i, \tag{1}$$

where i is a non-education[8] PLSS section (roughly 1 square mile) within a grant boundary,

[7] Of course, even these were not necessarily concentrated. For "other small grants," I include those given for military service, to Native Americans, and through the Bankhead–Jones Farm Tenant Act of 1937.

[8] See Section 2.1. Education sections were also pre-specified by a PLSS section number, meaning they similarly should not statistically differ in quality.

RR_i is a dummy variable for a railroad (i.e., odd) section, y_i is some outcome, and X_i are controls that typically include township fixed effects.[9] In my baseline results here and in other sections, I include controls for (log) section area, mean elevation, average terrain slope, the miles of streams, average soil quality, an indicator for entirely missing or unproductive soil, and latitude and longitude by state. For fat-tailed outcomes like property valuations that sometimes include 0, I transform them using the inverse hyperbolic sine (asinh) function, which allows me to interpret coefficients roughly as percentage changes; Appendix Table A.4 explores robustness to this last choice.

Note that this is an ITT regression and likely underestimates the true effects of land concentration. I nonetheless retain the ITT design as deviations from the intended policy are unlikely to be random and because no comprehensive database of railroad land grants exists to rescale the reduced-form results by a first stage. Attenuation might also occur due to sales across section boundaries, a topic I discuss in Section 8.

Checkerboard patterns are a textbook example of spatial autocorrelation (Grekousis, 2020, Chapter 4), so by default I use Conley standard errors with a 100-mile bandwidth to allow for the important possibility of long-range spatial correlation (Kelly, 2019; Conley, 2010). Section 6 shows this approach is most conservative and results in higher standard errors relative to a range of other methods. For regressions over smaller areas (e.g., counties) where this approach is not possible, I instead cluster errors at the township level.

5.3 First-Stage Results

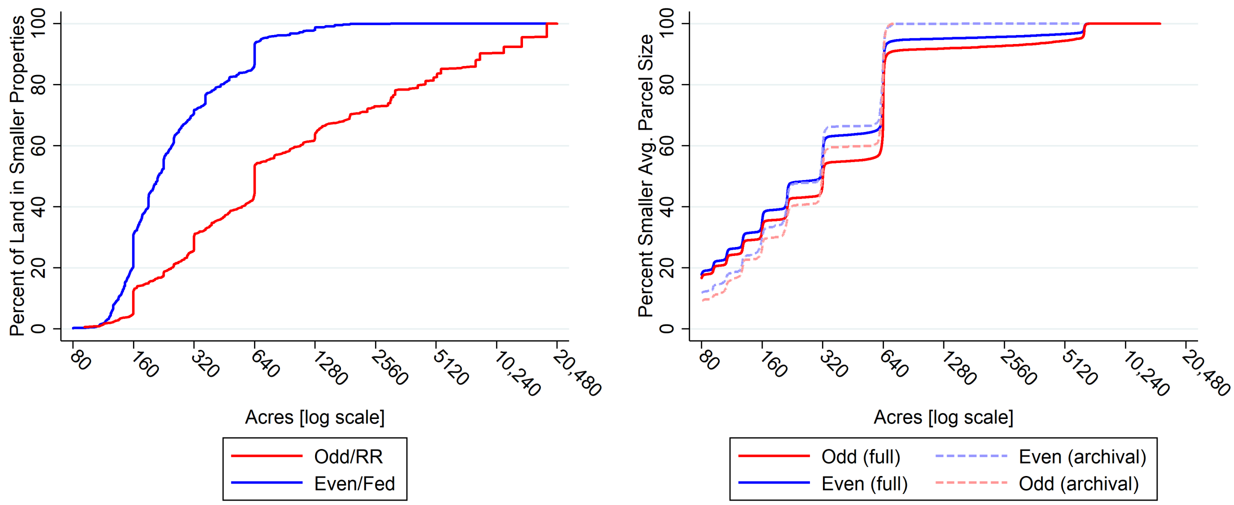
I begin my analysis by confirming that the railroad grant policy did, in fact, increase land concentration. Figure 2 presents two measures, both computed as averages at the PLSS section level. Figure 2a presents archival data from one Nebraska county about initial owners' total holdings, and Figure 2b presents data on the average size of a parcel in the full 2017 data.[10] Federal land records have been largely digitized nationwide, but comprehensive records of railroad land sales do not exist, requiring me to collect local archival data. In this case, I digitized original deeds of sale from the Union Pacific Railroad Company to individual owners and combined this information with similar federal (BLM) data. I consider the initial ownership measures in 2a the most direct and informative measure of land concentration. However, the parcel-based measures of 2b allow me to illustrate that this dynamic was general

[9] County and state × railroad grant area fixed effects are also included. However, these are effectively subsumed by the township fixed effects as townships are only very rarely split across counties, states, or company grants.

[10] Average parcel area is defined as the section's area divided by the number of parcels in the PLSS section. Parcels split among multiple sections are counted fractionally. Initial ownership is defined as the total holdings of the initial owner across all sections in the county regardless of allocation policy.

and not limited to a specific county.

Figure 2: Railroad Grants and Land Concentration



(a) Initial Owner Acres (Archival), 1800s **(b) Average Parcel Acres (2017)**

Notes: Panel (a) shows the CDF of (log) owned property sizes of initial land allocations in Lincoln County, Nebraska, based on archival data. Panel (b) shows the CDF of acres per 2017 parcel in both the full sample and in Lincoln County.

Both measures indicate that odd (railroad) sections were more concentrated. In my preferred measure from the archival allocation data, odd-section holdings are 390% larger than their even-section neighbors, driven by a shift across the distribution, including the extreme right tail. 47% of odd sections are in properties larger than one PLSS section (640 acres), and 22% are in properties over 3200 acres. In contrast, the corresponding figures for even sections are 7% and 0%, respectively.

Modern parcel data show the same dynamic for the whole sample, though they only measure historic land concentration indirectly. For administrative reasons, parcels are rarely combined, meaning that their modern boundaries partially reflect initial divisions. However, the first owners typically held many modern parcels, meaning that those differences likely attenuated over time. With this caveat, odd-section parcels are 13% larger in the county analyzed in Figure 2a and are 20% larger in the full sample. Appendix Figure A.2 shows that this result holds across a range of state and land quality subsamples, indicating that railroad land grants increased concentration broadly.

Table 2: **Balance on Geographic Characteristics**

	(1) Soil (z-score)	(2) Slopes (z-score)	(3) Streams (z-score)	(4) Elevation (z-score)	(5) log(Area)	(6) log(RR Dist)
RR Effect	-0.00047	-0.00027	-0.0014	-0.00049*	0.00011	-0.0011
	(0.00097)	(0.00035)	(0.0045)	(0.00028)	(0.00049)	(0.00078)
Sample	All	All	All	All	All	All
Grant × State FEs	Y	Y	Y	Y	Y	Y
County FEs	Y	Y	Y	Y	Y	Y
Township FEs	Y	Y	Y	Y	Y	Y
SEs / Clusters	Spatial	Spatial	Spatial	Spatial	Spatial	Spatial
N	132,463	132,463	132,463	132,463	132,463	132,463
$\mathbb{E}[y]$	-.046	1.2	.55	1.7	-.017	2.5

Notes: The table estimates the direct, even/odd comparison equation (1) on gSSURGO crop productivity in column (1), terrain slopes ("ruggedness") in column (2), miles of streams in column (3), elevation in column (4), log section area in column (5), and log distance to the railroad in column (6). Columns (1)–(4) are normalized as full-sample z-scores.

5.4 Land Quality Balance

Table 2 uses estimates of equation (1) to confirm that even and odd sections are balanced across a range of geographic characteristics, with the coefficients very small and precisely estimated. Z-scores of land quality characteristics are estimated in columns (1)–(4) and show differences of 0.0014 standard deviations or less, with the largest standard error of 0.0045. Similarly, differences larger than 0.0011 log points in section area or distance to a railroad can be ruled out. These differences are not statistically significant except for elevation at the 10% level. The tight null result on distance from the railroad highlights the fact that this paper has little to say about railroads per se: the checkerboard formula allocated land symmetrically across the two policies, and the typical section was more than 10 miles from the track.

Because land quality measures are based on modern data, these results point against environmental degradation or mismanagement as a major explanatory factor. This result is consistent with Burchardi et al. (2018), who find no soil quality change from experimental variation in land contracts. Hagerty (2022) similarly finds minimal soil differences across California irrigation districts with higher water allocations despite long-term changes in crop choice. While my tests cannot rule out environmental effects over spans larger than a PLSS section, by definition those cannot affect the section-level regressions of this paper.

6 Results: Land Values and Investments

In this section, I compare even and odd sections on dimensions of land values, investment, and other measures of inputs. I use both the full 2017 data and archival subsamples to compare the effects over time.

6.1 Land Values

Despite potential advantages from economies of scale, historic concentration ultimately lowered land values. Table 3, Panel A shows that modern assessments of odd sections are about 4.5% lower than even sections with the results significant at the 1% level across a number of standard error methodologies.[11] The columns sequentially add a rich set of controls including soil quality and township fixed effects, with column (6) including all controls as my preferred specification. The estimated differences are minor, with column (1) and column (6) differing by approximately 0.0015.

The negative effect of historic concentration is notable for two reasons. First, it demonstrates that the drawbacks larger owners faced outweighed any advantages in scale economies or access to capital. These owners also purchased their land and might have been positively selected relative to Homesteaders who obtained free land.[12] Second, the fact that an initial allocation of land had persistent effects over 150 years suggests that US land markets fell short of the ideals formulated in the Coase theorem. While multiple explanations are plausible given the results so far, they are consistent with the idea that tenancy contracts discouraged not only short-term effort but longer-term investment and forms of land use as well. I explore these mechanisms further in Section 7.

The remainder of this section provides additional support for the causality and generality of this result. First, Panel B of Table 3 performs placebo regressions of equation (1) that compare even and odd sections one or more miles from the grant areas. Assessed values of even- and odd-numbered sections differ by only small, statistically insignificant, and tightly estimated results. This is sensible as there are no reasons to think that even and odd sections should have differed except for the application of the checkerboard policy.

The reduction in land values is not driven by particular states, subsamples, or functional form but instead applies across most of the grant area. Appendix Figure A.3 runs OLS regressions separately for each state split by gSSURGO soil quality. Broadly, most of the

[11] In each column, my preferred Conley standard errors with a 100-mile bandwidth are given in parentheses. Standard errors using county clustering, township clustering, and simple heteroskedastic-robust errors follow sequentially. In all cases, the Conley errors are the most conservative and reflect the potentially spatial nature of treatment assignment, and I therefore adopt them as the default.

[12] See Mattheis and Raz (2023) for a detailed study of this question.

Table 3: Effects on (asinh) Total Property Value

	Panel A: Main Sample					
	(1) Value	(2) Value	(3) Value	(4) Value	(5) Value	(6) Value
RR Effect	-0.047***	-0.047***	-0.046***	-0.044***	-0.046***	-0.045***
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.014)
	[0.0086]	[0.0086]	[0.0085]	[0.0087]	[0.0086]	[0.0084]
	{0.0049}	{0.0049}	{0.0049}	{0.0095}	{0.0048}	{0.0048}
	⟨0.010⟩	⟨0.010⟩	⟨0.0088⟩	⟨0.0073⟩	⟨0.0069⟩	⟨0.0068⟩
State FEs	Y	Y	Y	Y	Y	Y
Grant × State FEs		Y	Y	Y	Y	Y
County FEs			Y	Y	Y	Y
Township FEs				Y	Y	Y
Geo Controls					Non-soil	Y
N	132,463	132,463	132,463	132,463	132,463	132,463
$\mathbb{E}[y]$	$2,134k	$2,134k	$2,134k	$2,134k	$2,134k	$2,134k
	Panel B: Placebo Sample					
	(1) Value	(2) Value	(3) Value	(4) Value	(5) Value	(6) Value
Odd Section	-0.000015	-0.00014	0.00012	-0.00061	-0.0016	-0.0012
	(0.0052)	(0.0051)	(0.0048)	(0.0049)	(0.0053)	(0.0050)
State FEs	Y	Y	Y	Y	Y	Y
Grant × State FEs		Y	Y	Y	Y	Y
County FEs			Y	Y	Y	Y
Township FEs				Y	Y	Y
Geo Controls					Non-soil	Y
N	230,483	230,483	230,483	230,483	230,483	230,483
$\mathbb{E}[y]$	$9,562k	$9,562k	$9,562k	$9,562k	$9,562k	$9,562k

Notes: The table reports regressions of the even/odd comparison of equation (1) on (asinh) total section property value. Panel A uses the main sample (inside the grant areas), and Panel B uses a placebo sample of areas one mile or more outside the grant area. Geographic controls are listed in Section 5.2. For Panel B, Grant × State areas are defined based on the closest actual grant area. Panel A uses Conley standard errors (100-mile bandwidth), county clusters, township clusters, and heteroskedastic-robust methods.

estimates are negative, individually statistically significant, and similar across states (conditional on quality). Alternatives to the inverse hyperbolic sine leave the estimates essentially unchanged since 0-valued squares are rare and not observably more common in either type of section (Table A.4, Panel A).

One exception to the broadness of the negative value effects is land with very low-quality soil: in approximately the bottom 20% of soil productivity across states, historic land concentration seems to have little effect on property value, as shown in Figure A.3. In these areas, the high-intensity farming necessary to feed a family on a Homestead plot was likely impractical.[13] This suggests that persistent changes in land use might drive the land value effects, as I discuss in the next section.

[13] 5% of sections with this quality grow crops, whereas 63% in the other four quintiles do. For a non-parametric version of this point, see Appendix Figures A.2-A.4.

6.2 Land Investments

Under the theory that tenant contracts reduced agricultural investment, we should expect to see fewer such investments in odd-section lands. Using additional outcomes in the modern and historic land assessment data, I explore that explanation for the reduced land values seen in Section 6.1.

Two important measures of investment available in my data are "improved land" and "improvements." "Improved land" refers to land that has been cleared and developed for agricultural purposes, primarily crop cultivation. Improving land was a substantial investment, often comparable in cost to the value of the land itself (Coffin, 1902). However, this investment could substantially boost production relative to "unimproved" acres left in their natural state. Unimproved land still had economic uses, particularly in ranching and grazing cattle on native vegetation. Assessors in the early 1900s directly tracked the number of improved acres and I compare these to satellite-based measures of crop cultivation in 2017. In the latter case, given that satellite data could be miscoded or some pixels slightly misaligned along section boundaries, I code a PLSS section as having improved land if it has at least 1% of its land devoted to crops. Assessors in both time periods also recorded dollar amounts for "improvements": the value of fixed capital such as barns and silos.

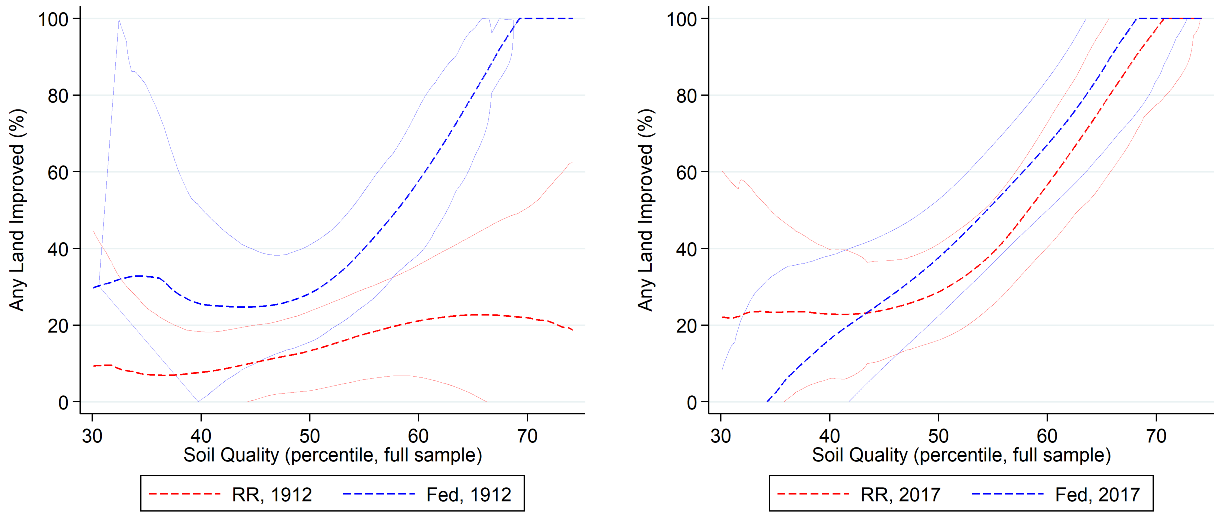
Consistent with an investment-based story, land concentration reduced historical investments in the early 1900s, and these effects are persistent (but smaller) today. Figure 3 illustrates the dynamic for one archival sample, comparing the percentage of sections with any improved cropland in 1912 and 2017. In both periods, historic concentration reduces improvements. Strikingly, the historical differences are largest in areas of high crop productivity as large owners' investments were largely unresponsive to land quality. In modern data, the differences are much smaller, and convergence has shifted investment toward the Homestead rather than the railroad pattern. The 1912 differences are statistically significant in this county though the 2017 ones are not.[14] However, Table 4 shows that the difference is significant in the full sample, with odd sections again less likely to have improved cropland.

Table 4 also reinforces these findings with a broader set of investment measures. Columns (1)–(3) report on the extensive margin of land improvement (i.e., positive improved land acres) and columns (4)–(6) on the value of improvements.[15] Both outcomes tell the same story. Land concentration lowered investments substantially in 1912, with 24 percentage point fewer sections improving any land and fixed improvement falling by 77%. In 2017,

[14] Similarly, differences in subsets of the 2017 graph are not significant given the small sample size. Using the full 2017 data, Appendix Figure A.4 studies this outcome on a variety of sumbsamples and again finds negative effects from land concentration in most states and land quality ranges.

[15] Assessed total values in the 1912 data were, unfortunately, only computed using a single rate each for improved and unimproved land compared to the more detailed procedures used today. As such, improved land alone captures all differences in 1912 assessed value.

Figure 3: Land Improved by Settlement Type (archival sample)



(a) Improved Land and Soil Quality, 1912 **(b) Improved Land and Soil Quality, 2017**

Notes: The figure shows the section-level existence of improved land for even and odd sections as a function of gSSURGO soil quality (percentile of full sample). Improved land is defined by the 1912 assessments, Morrill County, Nebraska (panel (a)) and 2017 land use (CropScape) with 1% or more land in crops (panel (b)). Both panels consider the same set of sections assessed in 1912.

the differences are substantially smaller and only statistically significant in the full sample. While the historical case study area is much smaller than the full modern sample, the results have the same sign and are not statistically distinguishable from the full sample's.

6.2.1 Could Fewer Improvements be Better?

Three aspects of these results cut against the idea that fewer improvements simply reflect larger owners economizing on the fixed costs of investment. First, property values reflect expected profits, and values are lower in historically concentrated lands. Second, while dollar-valued measures of improvements potentially incorporate fixed costs, the same pattern of results is also present in measures based on acreage usage that do not. Both the extensive margin of improved acreage and expected profits based on land use decrease (Tables 4 and 5). Appendix Table A.4 finds similar reductions across many other functional forms, including improvement value per owner. Third, land concentration historically reduced crop farming most in the best, rather than worst, croplands (Figure 3a), inconsistent with comparative advantage. Table A.4, Panel B similarly shows that the effects on land value and use intensity are small in the bottom quintile of soil productivity and are instead driven by the better quintiles. Large-scale owners thus invested less according to many measures rather than making the most out of a fixed number of improvements.

Table 4: Land Investments

	Any Improved Land (%)			(asinh) Improvement Value		
	(1) 1912 Sample	(2) 1912 Sample (in 2017)	(3) Full Sample (in 2017)	(4) 1912 Sample	(5) 1912 Sample (in 2017)	(6) Full Sample (in 2017)
RR Effect	-24.3**	-5.67	-1.48***	-0.77**	-0.25	-0.23***
	(7.76)	(6.37)	(0.42)	(0.26)	(0.42)	(0.047)
Grant × State FEs	Y	Y	Y	Y	Y	Y
County FEs	Y	Y	Y	Y	Y	Y
Township FEs	Y	Y	Y	Y	Y	Y
Geo Controls	Y	Y	Y	Y	Y	Y
SEs / Clusters	Township	Township	Spatial	Township	Township	Spatial
N	101	101	132,462	101	101	132,463
$\mathbb{E}[y]$	26%	42%	48%	\$3.2k	\$15k	\$1,277k

Notes: The table shows even/odd comparisons per equation (1) covering the extensive margin of land improvement in columns (1)–(3), as in Figure 3, and the assessed value of improvement in columns (4)–(6). The samples used are Morrill County, Nebraska in 1912 for (1) and (4); the same sections in 2017 for (2) and (5); and the full 2017 data for (3) and (6).

6.3 Other Inputs and Land Use

Data on other agricultural inputs provides additional support for the idea that land concentration reduced the intensity of economic activity relative to small-scale ownership. Although I lack survey-based microdata on most farms, I use administrative data to directly or indirectly measure labor, capital, and other land use measures. For labor, I note that farm operators typically reside on their properties, especially in the 19th and early 20th centuries. Population can thus proxy for labor inputs and I measure it in 1940 using census maps of Nebraska farm residences and 2019 using satellite data.[16] For capital, I use archival data from property assessments in one Nebraska county that measures the value of farm tools and equipment.

Consistent with lower rates of land improvement, Table 5 shows that historic land concentration also reduced observable labor and capital inputs. In 1940, roughly 50 years after the initial allocation, population in the average odd section was 25% lower with this value shrinking to 16% by 2019 (or 9.1% for the whole sample). Note that these measures are largely unaffected by towns and urban population: such population centers account for only a very small fraction of the sample. Instead, the values are shaped by the density of farming households on rural lands; see Section 9.1. For capital, use of farm tools and equipment fell by 26% in the case study county.

[16]Both sources allow me to measure population precisely, in contrast with sources like census blocks that align imperfectly with the PLSS grid. While formally the two measures differ slightly in definition, the differences are unlikely to affect the regression results. 1940 essentially measures farming residences and 2019 inferred population based on satellite imagery of residential buildings. See Appendix B.

Table 5: Land Use and Non-Land Inputs

	(1) (asinh) Farms [1940]	(2) (asinh) 2019 Pop [1940 sample]	(3) (asinh) 2019 Pop [Full sample]	(4) (asinh) Capital [1965]	(5) (asinh) Use Value [2017]	(6) # Uses [2017]
RR Effect	-0.25***	-0.16***	-0.091***	-0.25***	-0.028**	-0.069***
	(0.012)	(0.0089)	(0.014)	(0.063)	(0.014)	(0.023)
Grant × State FEs	Y	Y	Y	Y	Y	Y
County FEs	Y	Y	Y	Y	Y	Y
Township FEs	Y	Y	Y	Y	Y	Y
Geo Controls	Y	Y	Y	Y	Y	Y
SEs / Clusters	Spatial	Spatial	Spatial	Township	Spatial	Spatial
N	18,622	18,622	132,463	2,084	132,462	132,462
$\mathbb{E}[y]$	2	18	23	$13k	$380k	4.2

Notes: The tables shows even/odd comparisons per equation (1) covering (asinh) number of farms in 1940 in Nebraska in column (1); (asinh) satellite-derived populations in 2019 for the 1940 and full sample in columns (2)–(3); assessed farm equipment in 1965 in Lincoln County, Nebraska in column (4); (asinh) values based on land use, per Section B.8 in column (5); and the number of distinct land uses per CropScape in 2017 in column (6).

Agricultural land use also becomes more homogeneous and less intense according to detailed modern data. Table 5 columns (5)–(6) use the USDA's satellite-derived, 30m × 30m pixel data on land use for 2017. For agricultural activities, I code an expected profit for each pixel based on its coded usage, expected yields, prices, and costs as described in Appendix Section B.8. By this measure, historic concentration lowered use-based profits by 2.8%, comparable to the loss in land values. Finally, column (6) counts the number of distinct economic uses.[17] Odd sections exhibit fewer distinct land use choices, indicating homogeneity. This homogeneity likely reflects a mix of persistent ownership concentration and persistent patterns of land use.

Together, the last few sections illustrate that land concentration did not unlock advantages from potential economies of scale. Instead, historically concentrated lands were used less intensively as measured by inputs, investments, and valuations. While the effects attenuated over time and modern differences amount to 23%–64% of their historical magnitudes, the differences are still large enough to lower property values by 4.5% in 2017. In short, the economic changes that stemmed from concentrated land ownership endured beyond the effort involved in a single harvest and fundamentally shifted the intensity of agricultural production. These results certainly match the predictions from classic theories of tenant farming, extended to cover a very long time horizon. I explore evidence for this mechanism in the next section.

[17] Defined as a separate CropScape coding of a particular crop, pasture, or urban/developed area.

7 Mechanisms: Tenancy Contracts

I examine the relationship between the persistently low investment by railroad landowners and the agency problems associated with tenant contracts. Using archival survey data, I explore whether historic land concentration led to increased rates of tenant farming. Classical theory portrays share agreements as especially prone to agency problems compared to cash rent. I test this theory with a heterogeneity analysis to determine if land concentration had more negative effects in counties where share tenancy was more common.

7.1 Tenant Contracts: Direct Measures

To support the role of tenant farming as the link between land concentration and diminished investment, I first establish that such contracts were more common in odd-numbered sections. Geocoded information on tenant contracts is rare for the late 1800s. I thus present evidence from two local surveys: Kansas county census documents from 1940 and personal assessments and grain taxation from one Nebraska county in 1965. In both surveys, the property location, owner, and operator are given. When the owner and operator differ, I code the property as tenanted. In the 1965 survey, operator and landlord output shares are also reported, and I code properties with a positive landlord share of output as under a share agreement. Because of this survey's focus on tax liability for crops, share contracts account for a large majority of contracts reported; see Appendix B.

Table 6 shows that historical land concentration causally increased the use of tenant farming over a long time span across all sources and contract definitions. Column (1) pools both surveys and shows a 7.0 percentage point increase of a section having a tenant operator, roughly a 24% increase from the Homestead (even-section) mean. Columns (2)–(3) replicate this result for each survey individually, and column (4) replicates it for share contracts in the 1965 sample. The relative increases are similar in magnitude, with column (4) representing a 18% increase in rates of share contracting from the homestead mean. Columns (3)–(4) are significant at the 10% level, while columns (1)–(2) are significant at the 1% level.

Table 6 uses additional archival evidence to show that odd-section owners were less likely to live near their properties, indicating they were absentee landowners. I link owner names in the early 1900s Nebraska assessments to census microdata as described in Appendix B.5. Overall, odd-section owners are 8 percentage points less likely to be linked to someone in their property's county, suggesting absenteeism. While not all owners match to someone in the census, my overall success rate is similar to those in other analyses[18] and is unlikely to

[18] I uniquely match 39% of owner names to the census; a subset of these matches are located within the property's county. For comparison, Abramitzky, Boustan and Eriksson (2014) achieve a 16% match rate of people across census years. Their process differs from mine, however, as it considers a rich set of census

be differential across even and odd sections. Column (6) considers the percentile rank of owners' last name in the census. While odd-section owners have slightly less common names by about 1 percentile, this difference is not statistically significant, unlike the county-specific match difference. Railroad owners' names are typically in the 94th percentile of commonness of all census names, whereas Homestead owners' are typically in the 95th.[19]

Table 6: Tenancy and Absentee Ownership

	Tenant Agreement				Absenteeism	
	(1) Both Surveys	(2) KS Only	(3) NE Only	(4) Share Contract	(5) Owner In County (%)	(6) Last Name (percentile)
RR Effect	6.99***	10.2***	3.74*	3.74*	-8.24***	-1.15
	(2.41)	(1.62)	(2.03)	(2.02)	(2.57)	(2.24)
Sample Years	1940 1965	1940	1965	1965	1900 1912	1900 1912
Grant × State FEs	Y	Y	Y	Y	Y	Y
County FEs	Y	Y	Y	Y	Y	Y
Township FEs	Y	Y	Y	Y	Y	Y
Geo Controls	Y	Y	Y	Y	Y	Y
SEs / Clusters	Spatial	Spatial	Township	Township	Township	Township
N	1,571	738	832	832	613	581
$\mathbb{E}[y]$	33%	43%	23%	22%	12%	94

Notes: The table shows even/odd comparisons per equation (1) covering initial owners in Lincoln County, Nebraska in columns (1)–(2); owners per archival assessments of Perkins County in 1990 and Morrill County in 1912 (Nebraska) in columns (3)–(4); and 1940 farm surveys in multiple Kansas counties in columns (5)–(6). Columns (5)–(6) report on the links to the census microdata described in Section B.5.

The duration of the tenancy effect, rather than its sign, is the most notable result. Because the Homestead Act deliberately selected owners who would personally work smaller plots of land, it is unsurprising that railroad landowners more frequently employed tenant and share contracts while residing away from their properties. However, the agricultural surveys measuring tenancy studied in Table 6 cover a period roughly 70 to 100 years after the initial land allocation. Based on the attenuation of other effects shown in Section 6, it is likely that the even/odd difference in tenant farming also used to be much larger, though unfortunately I am unaware of any earlier surveys that directly measure tenancy.

The durable increase in tenant arrangements of all kinds, and share tenancy in particular, provides direct evidence for the classical mechanisms of contracting frictions as explanations for the long-term investment changes driven by land concentration. In the next section, I provide further evidence that share contracts in particular explain the results.

information to establish links, whereas I can only consider individuals' names and counties.

[19] This calculation excludes corporate ownership, where last names are not well defined.

7.2 Share Tenancy: Heterogeneity Evidence

Classical economic theory argues that the low-powered incentives in share contracts lowered agricultural output, especially compared to cash rents. In this section, I investigate whether this dynamic could extend over long periods by influencing land use and investment. Two distinct processes could contribute to this outcome. First, landowners using share agreements might be less inclined to develop their land. Second, landowners anticipating the challenges associated with share arrangements might forgo crop cultivation entirely. Instead, they would favor less intensive forms of agriculture, such as ranching, on unimproved land. As discussed in Section 3, opting for unimproved land avoids the principal/agent costs of crop share contracts but also leaves the land underutilized.

I make use of the fact that different areas of the United States varied in their propensity to use share contracts rather than cash rent. In my sample counties in 1940,[20] anywhere between 10% and 70% of farms were run by tenants, with share tenants accounting for 0%–91% of this group. If the classic incentive problem accounts for the results, counties prone to share tenancy should be most affected. A long literature in economics has explored why share contracts can remain second-best solutions in equilibrium, proposing factors of resource constraints, adjustment costs, or risk aversion (Banerjee, Gertler and Ghatak, 2002). Geographic characteristics play an important role in this, for example by influencing the stakes of monitoring tenants (Alston and Higgs, 1982).

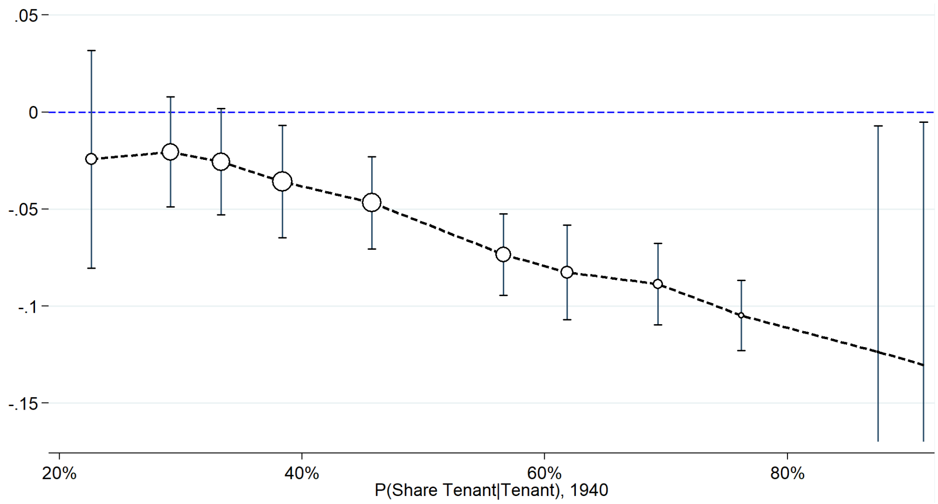
In support of the classic theories, Figure 4 presents a county-level heterogeneity analysis of equation (1) based on the 1940 fraction of tenant farms using crop share contracts. Counties with high rates of share tenancy experience the largest losses in modern property values in odd-numbered sections; counties where share tenancy is uncommon have few or no negative effects. Combined with the Table 6 result that land concentration results in more dependence on tenant farming, the larger effects in counties prone to share tenancy suggest that the incentive problems discussed by Smith and Marshall account for the lower land values today.

I perform several robustness checks on the heterogeneity analysis to rule out other explanations for this result. First, I consider whether the analysis is complicated by the fact that 1940 is a post-treatment year.[21] Note that since share tenancy prevalence is defined at the county level, individual square sections contribute very little to their own prevalence value. Appendix Figure A.5 affirms this by replacing the actual prevalence of share tenancy with predictions based on either geographic features or the share contracts' prevalence in neighboring counties. The results remain essentially the same. Figure A.5 also shows the same pattern of results across different samples split by soil quality, showing that land quality

[20] The first agricultural census year in which all my counties are present.

[21] Ideally, a pre-allocation measure of share tenancy prevalence could be used. However, because railroad land was allocated essentially at the start of settlement, no direct pre-allocation data plausibly exist.

Figure 4: Effects on Property Values by Share Tenancy and Soil Quality



Notes: The figure shows even/odd comparisons on (asinh) 2017 land value using samples defined by a county's fraction of 1940 tenants using share agreements. Each observation represents a regression of equation (1) on a subsample of the data with the estimate and 95% confidence intervals shown on the y-axis. The x-axis depicts the average share tenancy prevalence among sample observations. Controls are included as in Table 3, column (6).

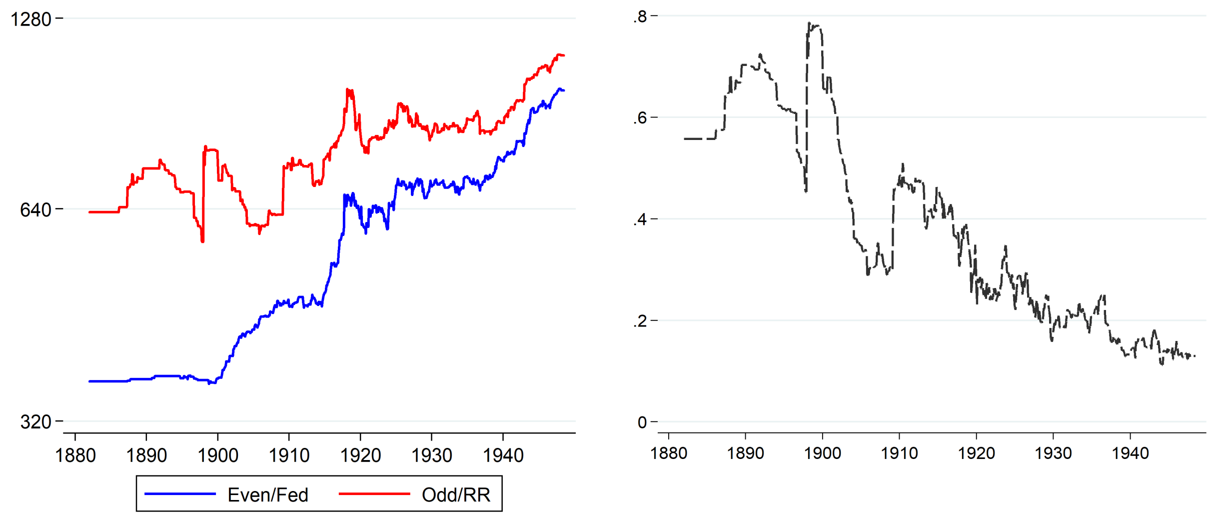
does not account for the heterogeneity in Figure 4.

In summary, direct evidence suggests that land concentration led to a long-term increase in rates of tenant farming, including share tenancy. However, the prevalent form of tenancy varied substantially across counties. Consistent with the classic theories emphasizing the incentive problems of share tenancy, counties where tenants mostly used cash rent saw few or no long-term impacts from land concentration. In counties where geographic or economic conditions predicted share tenancy would prevail, however, the negative impacts of land concentration were larger and long-lasting. The duration of this result suggests that land markets in this context fell short of the Coasian ideal where initial allocations should not have long-term impacts. In the next section I examine why Coasian reallocation has proceeded slowly.

7.3 Reallocation Speed

The convergence of economic outcomes across even and odd sections suggests that land concentration itself should similarly converge. This prediction is partially in keeping with the Coase Theorem, albeit in a world where equilibrium adjustments might take many generations to achieve. To provide more detail about how this process unfolded, I use a case study of one Nebraska county's individual land transactions. The data cover land transfers from the county's creation in 1882 until 1947 and are available at the parcel level, representing one-sixteenth of a section (40 acres).

Figure 5: Land Concentration Over Time (archival sample)



(a) Land Concentration (Acres, Log Scale) **(b) Odd/even gap**

Notes: The figure shows monthly data on land transfers from Banner County, Nebraska. Panel (a) plots the average property sizes, and panel (b) plots the odd/even difference.

The data indicate that convergence is a slow but steady process. Figure 5 plots the average owned property size difference by month for even- and odd-numbered sections. Over the course of 65 years, the odd/even size ratio falls from a high of 2.2 to a low of 1.1. Convergence is primarily achieved as even-section properties grow in size faster than odd-numbered ones. The economic history of this period has emphasized that changes in technology such as the tractor made it more realistic for individual families to operate larger farms (Sumner, 2014; Olmstead and Rhode, 2001), and these changes likely favored size increases in even-sections over odd-numbered ones. Consistent with this growth, Appendix Section C.1 tracks the ownership of individual parcels over time to show that most convergence occurs because properties are combined into larger ones rather than split into smaller ones.

While this paper does not focus on explaining rates of convergence, several possible explanations are inconsistent with the pattern shown above. First, no specific year precipitates the dynamic. Instead, the gap plotted in Figure 5b shrinks gradually. The consistency points against any particular event or policy as a primary explanation. Second, there were no legal barriers to resale. Both Homestead and railroad landowners received full titles, and the land market was thick enough to facilitate frequent transfers. Appendix Figure A.6 shows that, in these data, even and odd sections were settled around the same time and nearly all properties changed hands at least once, often more, by 1920.

The convergence pattern is consistent with constraints or costs to smallholders of raising capital. This explanation also fits with the purpose of the Homestead Act which aimed to provide land to those who could not purchase it. Appendix Section C.7 delves further into this topic and presents archival data demonstrating that even-section owners paid their taxes less promptly than odd-section owners, plausibly reflecting reduced access to capital. Still, other factors may have contributed to the incomplete convergence.[22] As with the original Coase Theorem, many kinds market imperfections would yield the same key outcome of a persistent initial allocation. Finally, if the low-intensity use practiced under concentrated increased the cost of improving land in some parcels, it might no longer be economical to improve them. If so, this subset of lands would have experienced permanent path dependence in their usage rather than diminishing persistence.

8 Tests for Aggregate Effects

A simple back-of-the-envelope calculation suggests that led to a loss of $2.9 billion in 2017 assessed valuations within the sample area.[23] Due to the long time period between the initial allocation and my modern data, there are several reasons why the even/odd comparison may not fully reflect the policy's broader effects. Homestead lands could have obtained resources at the expense of railroad lands, implying the direct comparison overstates the total effect. Alternatively, individual landowners in each group could have obtained land in the other type of square over time, leading to an underestimate through attenuation in the first stage.

To determine whether the railroad grant policy had broader impacts beyond the direct comparisons outlined above, I compare non-railroad land in the checkerboard area to land outside the checkerboard. I exploit the fact that the grant boundaries were arbitrarily determined by formula and use a geographic RD around those borders.

8.1 RD Specification

I use an RD design to compare federal (Homestead) land within the checkerboard areas to those outside it. Because some of the borders of this grant area were determined by political features,[24] I consider only grant boundaries set formulaically by fixed distances to a railroad

[22] For important work on this topic, see Bleakley and Ferrie (2014).

[23] Top-coding at the 95$^{\text{th}}$ percentile, odd sections in the sample have a total valuation of $64 billion which I multiply by the effect estimate in Table 3.

[24] For example, the Osage Reservation (Kansas) and Crow (Montana) Reservation borders are excluded; see Appendix Section B.2 for a complete discussion.

tracks in this analysis. I run regressions of the form

$$y_i = \alpha[\text{NearRR}]_i + f(d_i) + X_i\beta + \varepsilon_i, \tag{2}$$

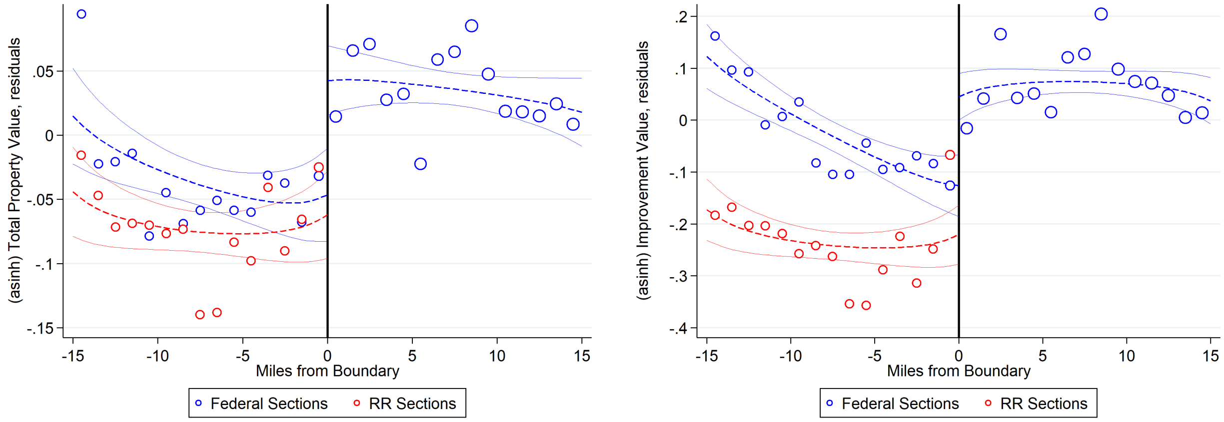
where i is a non-education, federal section within a bandwidth of the boundary; $[\text{NearRR}]_i$ is a dummy for a section being located within the railroad land grant; d_i is the distance to the boundary; f is a local linear function on either side of the cutoff, estimated separately for each state $\times$ railroad grant pair; and X_i are controls including county fixed effects, the geographic controls from the even/odd specification, and latitude and longitude by state. For my baseline results, I estimate this equation by the Calonico, Cattaneo and Titiunik (2014) methodology with a triangular kernel while clustering at the county level. For consistency, I use the optimal bandwidth for the regression in 6.47 miles across all regressions. I explore robustness to the sample and bandwidth in Appendix Figure A.7 and Table A.5.

8.2 The Checkerboard Effect

Figure 6 indicates that the aggregate effect of the checkerboard policy was, if anything, more negative than what the even/odd comparisons suggest. To compare areas with the same initial settlement policy, I focus on federally administered (Homestead) lands. The total and improvement value of these lands in 2017 sharply increase at the border, indicating that Homestead sections within the checkerboard lost value despite not being allocated to railroad owners. Combining these results with the even/odd comparisons, the best-performing groups are, in order, pure Homestead lands, Homestead lands within the checkerboard, and railroad lands. These groups correspond to pure control areas, control units in treated areas, and treated units, considering railroad allocations as a "treatment."

One possible explanation is that large-scale owners with property in the checkerboard area formed an important pool of buyers for lands adjacent to their sections, outbidding credit-constrained small-scale owners. This type of purchase across units of treatment would have reduced even/odd differences within the grant area, leading to an understatement of the policy's aggregate effects. I examine alternative explanations in Section 9.3 and Appendix Table A.5. The effects of Figure 6 are statistically significant and not due to differences in geographic characteristics at the border, which remain small and tightly estimated. A slightly higher fraction of federal land is settled, but settlers linked to census microdata also appear relatively similar.

Figure 6: RD Graphs for (asinh) Total and Improvement Value



(a) Total Value **(b) Improvement Value**

Notes: The figure shows modern data on (asinh) total and improvement value near the checkerboard border. Railroad (odd-numbered sections within the checkerboard) land is depicted in red, and federal land (all other land) is in blue. Outcomes are adjusted for the controls listed in Section 5.2.

9 Alternative Mechanisms

While previous sections have emphasized the role of tenant farming and share contracts in explaining land concentration's effects, there are other possibilities. In this section, I examine the evidence for various alternate mechanisms. I specifically consider the non-agricultural components of land value, other elements of the railroad grant policy, other elements of the Homestead Act, and land fragmentation. I also consider whether the results applied broadly or were limited to particular subsets of my data. Further channels are addressed in Appendix C, including public goods, the timing of settlement, and speculative investment.

9.1 Urban Density and Agglomeration

One major alternative story would be that the odd/even differences in land value derive from non-agricultural activities. Since the Homestead Act was designed to increase settlement, it could have fostered the creation of towns and cities whose per-acre land values vastly exceed those of farms. However, because each observation is a unit of land, agriculture is overwhelmingly the dominant economic use in the sample with urban areas present in only 0.4-3%, depending on the definition. This small part of the sample is balanced across even and odd sections and, based on a number of analyses, has a very limited impact on the final result in either direction.

Table A.6, Panel A shows that differences in the number of towns per square-mile section are small and tightly estimated. Effect estimates in columns (1)-(2) are one town per thousand square miles or less and neither is significant at the 5% level though (2) is significant at the 10% level. Since urban areas are rare on a per area basis, the relative increase could be important for other research questions. However, for this analysis, a movement of 0.1% of the sample is mechanically too small to drive the effects I find. Both coefficients are positive which, if anything, would mean odd sections benefited from having slightly more towns.

Other definitions and analyses show similar results. There are tight zeros on sections having 100+ or 1000+ people. Even sections are persistently more likely to have higher farm density, however. They have increased densities at 1+ or 10+ people per square mile, or approximately 2-3 households. I address a related question about whether the density of farms in a square mile could have agglomeration or other spillover benefits in Section C.2.

9.2 Provisions of the Homestead Act

Another explanation would be that other elements of the Homestead Act induced settlers to intensively invest, perhaps even excessively so (North, 1966). For example, if settlers were required to grow crops to receive a title, my results could simply reflect legal compulsion.

However, official regulation interpreting the Homestead Act portrays a more flexible policy where settlers were free to choose their form of agriculture. In the 1880 Luning decision, the Department of the Interior explicitly addressed the crop requirement. It ruled that crop farming was not required as “stock-raising and dairy production are so nearly akin to agricultural pursuits as to justify the allowance of entry [i.e., title]” (Department of the Interior, 1880; General Land Office, 1884). Bradsher (2012) also emphasizes that settlers primarily had to demonstrate that they intended to settle and work their land in “good faith” and “no specified amount of cultivation or improvements was required” to do so.[25]

Two pieces of quantitative evidence also suggest a flexible Homestead policy. First, the largest investment gaps occur in high-productivity areas rather than places unsuited for crops (see Figures 3 and A.3). Second, my 1912 archival assessment data show that many properties without improved acres were successfully Homesteaded. Among eligible sections, 91% of lands without assessed improvements were successfully Homesteaded; 93% of these were obtained for free.[26] These figures provide a lower bound for approval as the small

[25] Bradsher (2012) captures the spirit of the regulations, with inspectors primarily concerned with detecting fraud or other “bad faith” acts from owners who did not truly aim to work or live on the land. Like other qualitative sources, though, it does imprecisely state that “cultivation” was important. This stems from considering the law’s text without reference to the practical implementation described above. The Luning ruling made it explicit that all forms of agricultural activity would suffice. The 1880 ruling date notably precedes almost all settlement in my sample area.

[26] 79% were transferred under a typical Homestead arrangement and 5% under the 1873 Timber Act extension (granting Homesteaders free land for raising trees). 7% were sold under the Homestead Act (allowed after 6 months of residence and “improvements”). 9% were sold outside the Homestead Act. These percentages are 87%, 5%, 4%, and 5% for the whole sample of eligible sections.

portion of non-Homestead land in these areas could have been settled under other policies for reasons unrelated to the level of improvements. Even alone, the lower bound is quite high and suggests that a lack of "improved land" according to an assessor could only rarely influence titling.

9.3 Other Railroad Grant Area Policies

If not elements of the Homestead Act, perhaps other aspects of railroad companies' sales could be the primary mechanism. Notably, Alston and Smith (2022) details the legal troubles of the Northern Pacific Railroad (NPRR). The company's "violations, controversies, and investigations... had no peers" and this could have created uncertainty for those who bought NPRR land. More broadly, it could be that the actions of any particular company explain the results rather than the common theme of land concentration.

Appendix Figures A.2-A.4 show that even/odd differences point in the same direction across a large majority of state and land quality subsamples. Odd sections broadly exhibit increased parcel size, decreased rates of crop farming, and decreased land values. Figure A.8 also shows that the land value differences are similar across counties settled over a span of five decades, implying no particular year or event drove the result. The consistent pattern in odd sections across a range of states, companies, and time periods indicates the need for a similarly broad mechanism. As noted earlier, the "basically identical" policies of railroad companies consistently led to large-scale sales (Greever, 1951). A corollary of this analysis is that the legal troubles specific to the NPRR do not explain the results. Dropping the grant, in fact, slightly strengthens the land value differences; see Appendix Section C.3.

A separate concern is that the Homestead Act was implemented differently with smaller allotments in railroad grant areas. This concern affects the RD interpretation and is discussed in Appendix Section C.4. In brief, qualitative and quantitative evidence suggests that the differential application was rarely or never implemented.

9.4 Land Fragmentation

One peculiar feature of the railroad checkerboard is that exceptionally large landowners initially had to be content with diagonally connected sections (640 acres). In developing countries, fragmentation is thought to negatively impact agriculture by inhibiting scale economies or increasing workers' travel times, though in practice such costs may be modest (Ali, Deininger and Ronchi, 2019). While 640 acres is far larger than farms found in most developing countries and diagonal connections are less likely to impede travel times, the costs associated with fragmentation could be responsible for underinvestment in my setting.

However, in practice, odd sections were not particularly fragmented on diagonals and most owners in my archival data were able to obtain land connecting such holdings. Appendix Figure A.9b shows that both odd- and even-section properties retained over 90% of their contiguous sizes when removing diagonal connections. Odd sections were also part of larger holdings regardless of the metric used. Appendix Figure A.9 shows measures of total holdings, comparing total acres owned, contiguous acres owned, and non-diagonally contiguous acres. While measuring size contiguously mechanically lowers property sizes, odd sections are larger across all measures, and the dynamics across time are also similar.[27] Odd-section owners thus had greater potential for scale economies regardless of how size is defined.

Finally, it is important to note that a section of 640 acres was already at the upper end of operational sizes in the late 1800s. As noted in Section 2.3, a 640-acre farm would have been in the top 2% largest farming units in my sample states and the US overall. While many individual owners benefited from larger holdings, in practice these were operated separately according to the census.

9.5 Public Goods and Politics

Land concentration's effects could have operated through political, rather than economic, channels. Previous research has argued that land concentration reduced public goods provision through the capture and coercion of political systems (Galor, Moav and Vollrath, 2009). Other work, however, has found that landowning elites can use their influence to solve collective action problems and increase public goods provision (Dell, 2010).

On the scale I study, however, political and governmental mechanisms did not play a substantial role. While land concentration may have shaped state or local policies, these would have to have been selectively applied at the square mile level to explain my results. I test for such differences by considering two classes of outcomes at the square mile level: public goods provision and individual landowners' political activities. I code the former using 1940 census maps showing schools, churches, and community halls in rural areas as well as the modern road network. I code the second using a 1912 archival sample linking owners to the names of those running for local, subcounty offices (e.g., irrigation district commissioner) to measure officeseeking.

Appendix Table A.7 estimates the effects of the checkerboard allocation policy on both classes of outcome. Broadly, these analyses show either economically small or statistically undetectable results. In a large sample of 1940 areas in Nebraska and Kansas, odd sections

[27] Similar statistics hold in other archival data: in the early 1900s assessments, odd-section properties are 70% larger than even-section ones even when diagonal connections are ignored.

have slightly fewer public goods than their even neighbors, with a difference of one community hall per 1000 square miles and being 0.002 miles (11 feet) further from a road being statistically significant. However, these differences are not economically meaningful and unlikely to have influenced the property value results. Further, they may simply reflect a lower population on odd squares shown in Table 5. A simple per capita comparison in fact shows similar levels of public goods in the two square types.[28] For individual behavior, odd-section owners, if anything, are to be less likely to seek local political office. While the sample size is small for the latter estimate and the confidence interval includes 0, it suggestively points against frequent political activity by large-scale landowners in this context. One explanation is that the absenteeism of odd-section landowners detailed in Section 7.1 would have made it harder for them to seek local office, explaining why only 3% did so.

Even sections within the checkerboard do not appear to be statistically different than their non-checkerboard counterparts in terms of in public goods or owner political activities. In Table A.7 Panel B, estimates of equation (2) on all six outcomes are not statistically significant. On public goods, the coefficients are again small and inconsistent in sign, pointing against a broad effort to reduce or increase government spending in this area.

Overall, these results suggest land concentration in this context did not lead to political capture at the square mile level. However, it is important to note that land concentration might still have changed public policy and politics in larger units like the county, state, or nation. Because my variation occurs within such units, those differences are unlikely to explain the investment differences across even and odd sections. At higher levels, the channel remains plausible and an important avenue for future research.

10 Conclusion

Whether land is allocated to smallholders or concentrated among large landowners is a distinguishing feature of many agrarian economies. In this paper, I use a natural experiment to study the impact of that distinction on the American frontier. The effects of land concentration on agricultural development were primarily negative, with larger owners investing less in their properties due to the agency problems of tenant farming and absentee ownership. Although the effects of land concentration have partially faded over time, the process took many decades and differences in land use and value are still apparent 150 years after the initial allocation. This incomplete convergence indicates American land markets historically fell short of the Coasian ideal but have partially achieved it over many generations.

[28] Overall, odd sections have 0.051 schools, 0.0073 churches, and 0.0011 community halls per farm in the full sample compared with 0.046 schools, 0.0055 churches, and 0.0013 community halls for even sections.

The results in this paper speak to long-standing concerns about land concentration, expressed even in the early writings of Adam Smith and Alfred Marshall. This paper provides new evidence demonstrating that land concentration's effects can be long-lasting through a combination of lowered investment and the slow speed of Coasian convergence. Although this study is necessarily historical, the issue of land distribution remains pertinent to numerous policy discussions, particularly in developing countries. My findings suggest that, in certain contexts, large landholdings hinder growth instead of unlocking the advantages of scale economies. This study underscores the importance of policies that can affect land concentration given their potential to influence economic development over very long periods of time.

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Appendices For Online Publication Only

A Model Details and Proofs

This section mathematically describes and solves a model comparing owner operation to tenancy. Building off the static framework in Banerjee, Gertler and Ghatak (2002), it adds investment and resale to study long-term effects.

A.1 Static Problem

The world consists of a parcel of land, its owner, and its operator who may or may not be distinct. Owners may engage in low-intensity agriculture on unimproved ($I = 0$) land, which always produces an output of 1. Alternatively, they may engage in high-intensity agriculture on improved ($I = 1$) land. In this case, output is stochastic at $Y = A > 1$ in the case of success and is $Y = 0$ in the case of project failure. The probability of success is equal to the effort e of the operator. Owners[29] of $I = 0$ land may improve it to $I = 1$ for a cost $r > 0$. Effort and investment are costly to the operator and owner, respectively. Qualitatively, the $I = 0$ case corresponds to low-intensity uses like ranching whereas $I = 1$ corresponds to high-intensity uses like crop production.

Landowners come in two types. Small-scale owners S operate their own parcel and choose their level of investment and effort, as in the design for Homestead ownership. For high-intensity ($I = 1$) agriculture, large-scale owners L must contract with a tenant. Here, effort is unobservable, so owners of improved land are limited to offering payments based on project success or failure. Owners face a limited liability constraint based on tenants' ability to pay upfront. The contract must also offer tenants at least as much utility as their outside option, which I set to 0. All agents are risk neutral.

Effort costs the operator a monetary equivalent of $\frac{1}{2}ce^2$ and is not observable to other agents. Therefore, owners of improved land are limited to offering payments of l in a low state and h in a high state. Tenants have access to resources W which determines the limited liability constraint: $l, h \geq -W$. This situation corresponds to that of railroad owners who rented land. As discussed below, when tenants have a limited ability to pay (low W), landlords will often be constrained to offer share contracts; in the case of crop failure, fixed ("cash") rent cannot be extracted. The non-intense $I = 0$ land use similarly avoids crop share contracts, reflecting a non-crop usage.

[29] With minor adjustments, the basic results still hold if these costs are borne by the operator.

Up to this point, the $I = 1$ case is equivalent[30] to the model in Banerjee, Gertler and Ghatak (2002) and the landlord's constrained optimization thus has the same solutions:

$$(l, h-l, e) = \begin{cases} (-W, \frac{A}{2}, \frac{A}{2c}) & \text{if } W \leq \frac{1}{8}\frac{A^2}{c}, \\ (-W, \sqrt{2cW}, \sqrt{\frac{2W}{c}}) & \text{if } \frac{1}{8}\frac{A^2}{c} < W < \frac{1}{2}\frac{A^2}{c}, \\ (-\frac{1}{2}\frac{A^2}{c}, A, \frac{A}{c}) & \text{if } W \geq \frac{1}{2}\frac{A^2}{c} \end{cases} \tag{3}$$

where I have written the solution as the tenant's payment in the low state (l) and the extra payment in case of the high state ($h-l$) for ease of explication. $e = \frac{h-l}{c}$ solves for optimal tenant effort based on the first order conditions. The three cases respectively correspond to those where only the limited liability binds; where both limited liability and the outside option bind; and where only the outside option binds.

Note owner-operators always achieve first-best (surplus maximizing) effort at $\frac{A}{c}$, corresponding to the last case above. In the other two cases, effort and total surplus are below the maximum for the $I = 1$ case. This derives from the fact that in the third case, tenants receive the full value of success in their contract ($h - l = A$) whereas they receive less in the other two, bottoming out at 50% in the first case. Qualitatively, the third case can thus be considered a cash rent scenario where tenants pay a fixed fee but receive the full value of agricultural production. The other two cases correspond to different versions of share contracts, with tenants receiving only part of the return on their effort.

A.2 One-period Investment Return

In the static version of the model, S and L type owners can face different returns on investment. Because of this, the derivations from hereon out depart from the effort-only focus of classic principal agent theories as in Marshall (1890); Banerjee, Gertler and Ghatak (2002). Denote by R_S, R_L the one-period net output[31] of improved land. For the L types, this equals $W + \frac{A^2}{4c}$; $W + (A\sqrt{\frac{2W}{c}} - \frac{2W}{c})$; and $\frac{1}{2}\frac{A^2}{c}$ in the three cases above. For S types, the return is always equal to R_L's value in the third case.

Lemma A.1. *$R_L \leq R_S$ with equality only in the third case*

Proof. L-type's total returns are increasing in W and so highest in the third case. Since the tenant's outside option is 0, $R_L = R_S$ in that situation. □

[30] A minor difference is that here the outside option is normalized to 0, but in general the sum of the outside option and limited liability values together form a sufficient statistic, so this has little effect. As in their model, I focus on the case where c is large enough to guarantee proper probabilities of success in equilibrium and ignore the cases where the solutions are constrained by probabilities being ≤ 1.

[31] i.e., the monetary equivalent of owning production technology for one period, equal to the expected value of output minus any effort costs.

Lemma A.1 shows that small-scale owners receive a weakly higher one-period payout from investment with equality only in the "cash rent" case.

A.3 Dynamic Problem

I now describe the multi-period aspect of the model. At $t = 0$, the parcel is initially allocated to either an S- or L-type owner with $I = 0$, reflecting Homestead and railroad distributions of frontier land. At the start of a new period, the previous owner sells the parcel and exits the model along with the operator. The landowner makes a take-it-or-leave-it offer to an agent of either type. S types, however, face stochastic costs in purchasing, reflecting their lack of access to capital.[32] The costs are distributed according to F_S. After transaction costs are determined and a sale occurs, improved land has a $0 < \delta < 1$ chance of depreciating into unimproved. Owners discount across periods at a rate of $0 < \beta < 1$.

The parameters above determine the sale prices for improved and unimproved land, p_0 and p_1. Since agents cannot foresee shocks at the time of their investment and contracting decisions, these values are fixed in each period. Finally, note that this implies that the only relevant state variable for each period.[33]

Lemma A.2. *If an L type improves land, an S type will also improve land*

Proof. From above, the one-period net output from improvement $R_S \geq R_L$. The cost r and expected land prices p_0, p_1 are the same and agents are risk neutral. □

Lemma A.2 shows that small-scale owners are weakly more likely to invest in unimproved land. For the remainder of the model description, I highlight the case without equality where S types improve land with $I = 0$ and L types do not. The case where the two types behave identically is simpler: since I is the only relevant state variable, initial ownership has no effect for $t \geq 1$. The two types can behave identically, for example, because W is high and cash rent is possible or because A is sufficiently low enough that neither type invests.

A.4 Equilibrium Characterization and Markovian Convergence

With I as the only model state variable, the dynamics simplify to a Markov chain on a 2×2 matrix. The probabilities $q_0 = p(I_t = 1 | I_{t-1} = 0)$ and $q_1 = p(I(t) = 1 | I_{t-1} = 1)$ completely

[32] Other interpretations give the same or similar results, for example, the costs of reallocation across different owners. While the homogeneity of agents within type is artificial, essentially the same results would apply under a more realistic matching process given that S types continue to value improved land relatively more than L types.

[33] Considering the end of the period to be just after the investment decision is made but before the shocks for output, depreciation, and capital costs are realized.

determine the dynamics. Then, for an agent of type j, denote by $V_{j,i}$ the willingness to pay for land given $I = i$ and after depreciation and transaction costs are paid.[34]

$$
\begin{aligned}
V_{0,j} &= \max\left(1 + \beta p_0, R_j - r + \beta p_1\right) \quad (4)\\
V_{1,j} &= R_j + \beta p_1 \quad (5)
\end{aligned}
$$

Lemma A.3. *Relative to the case where $I_{t-1} = 0$, when $I_{t-1} = 1$, period t has a weakly higher probability of S-type ownership and a weakly higher probability that $I_t = 1$ ($q_1 \geq q_0$)*

Proof. It suffices to compare the increased relative valuations $V_{i,j}$ $I = 1$ and $I = 0$ cases: $V_{1,S} - V_{0,S} \geq V_{1,L} - V_{0,L}$. Note that $V_{1,i} - V_{0,i} \leq r$ with equality when j improves unimproved land. Hence, if type S improves land (the non-trivial case for the whole model), the inequality holds because the difference is at its maximum. If type S does not improve land, neither does type L (Lemma A.2) and the differences are $R_j - 1 + \beta(p_1 - p_0)$. In this case, the inequality holds due to Lemma A.1. This establishes that S has a weakly higher valuation in the case of no depreciation or transaction costs.

Next, consider actual willingness to pay at the time of the sale. An agent of type j is willing to pay $(1 - \delta)\left(V_{1,j} - V_{0,j}\right)$ more for improved land than unimproved land. That is, depreciation attenuates the differences and transaction costs do not enter the relative difference as they are fixed. By the above, S types will pay a weakly higher premium for improved land and so are weakly more likely to purchase when $I = 1$.

Finally, note that for improved land to be unimproved in the next period, it must first depreciate. Even in that case, it is still weakly more likely to have an S-type owner (previous paragraphs) which in turn is weakly more likely to improve (Lemma A.2). □

Lemma A.3 establishes persistence: land initially owned by an S-type is weakly more likely to be improved and be held under S-type ownership in any future period, with equality holding in some cases.[35] However, when $0 < q_0 < 1$, the dynamics over time are given by the transition matrix

$$
T = \begin{bmatrix} 1 - q_0 & q_0 \\ 1 - q_1 & q_1 \end{bmatrix}
$$

[34]That is, the expected value an agent expects from the land at the start of the contracting and investment phase. This is related to, but distinct from, their willingness to pay prior to these shocks.

[35]e.g., if S and L types have identical investment behavior or F_S is distributed such that only one type or other always purchases.

which is irreducible and aperiodic, meaning convergence as $t = \infty$ to the same probabilistic distribution of investments regardless of initial S-type (with investment) or L-type (with no investment) states. Appendix Figure A.10 gives specific parameter values for such a case to illustrate the convergence. Under different conditions, absorbing states are possible, e.g., if neither type invests or if L types do not invest and have a 100% probability of purchasing $I = 0$ land.

A.5 Discussion and Predictions

In the non-trivial cases, the model predicts that higher land concentration in $t = 0$ reduces investment and increases future rates of land concentration, albeit increasingly less over time. This can be seen in the empirical results of Table 3/Figure 3 (investment with attenuation) and Figure 5 (concentration with attenuation). However, when cash rent is possible or both types avoid investing, convergence is trivial. Empirically, Figure 4 shows that there are few effects from land concentration when cash rent is common. Convergence is also trivial when A is sufficiently low that neither group prefers to invest. Appendix Table A.4 similarly shows that there are few effects in areas with very low land quality.

B Data Sources and Sample Construction

B.1 Property Tax Assessments

Florida and Montana property taxes are publicly available as GIS files. I obtained Kansas, Oregon, and Wyoming taxes through either state- or county-level tax officials. For Nebraska, I webscraped county-level data hosted by GIS Workshop, covering almost all counties. A large majority of assessments list the PLSS section (or, rarely, sections) of each property. In counties where section information was not comprehensively provided, I relied on GIS parcel maps (Florida, Wyoming) or geocoded property address (Kansas).

Some data are only reported comprehensively for specific states. Land use data including active grassland and pasturing are reported for Kansas, Montana, and Nebraska. Florida, Nebraska, Kansas, and Oregon report owner name and address. Similar data are reported partially for Montana and Wyoming, but both contain substantial unsettled lands in the public domain which are coded as owned by the federal government. These lands are typically leased to nearby farmers, meaning that ownership data has a different interpretation in these parcels compared to parcels outside the public domain. For thirteen counties in the sample, exempt government lands are absent in the dataset and for these I the CropScape-derived use value in place of total valuation.

B.2 Grant Boundaries and Sample Construction

As noted in Section 2.2, most railroad grant areas are within a pre-specified distance of the company's railroad track. For these areas, I use historical maps to find the relevant radius for the grant and draw a buffer around the railroad. In some cases, multiple effective distances applied, e.g., because companies were granted additional "miles" on some sections to substitute for excluded land elsewhere. In such cases, I choose the outermost distance as relevant. Since most railroad locations have not changed, I use modern-day GIS information from ESRI on their location as it is most precise. I confirm the grant railroad location with the 1890 railroad data from Donaldson and Hornbeck (2016).

Some grants had non-formulaic borders. For example, companies lost land that intersected with the Crow (Montana) and Osage (Kansas) reservations. In these cases, I use a mix of historical maps, court records, and Bureau of Land Management (BLM) transfer records to determine the boundaries of the grant. Maps show the rough locations of non-formulaic grants and the BLM records permit an exact mapping through the evidence of checkerboard patterns around individual PLSS Sections. In the rare cases these records are incomplete, I use the BLM Tractbooks to determine the areas railroads received grants. Using the land grant boundary lines I construct, I code any PLSS section which intersects them as being within the grant area. As noted in Section 7.3, only pure formulaic boundaries are considered as part of the RD. Borders from other political boundaries, railroad start and end points, or formula violations are not considered.

B.3 Historic Farm Microdata

For farm ownership and operational details, I draw upon 1940 "county census" documents for Kansas, preserved by Ancestry and the Kansas State Historical Society. These were used to produce (bi)annual reports on agricultural activity by the Kansas State Board of Agriculture. For each farm in 1940, the records list the operator, the PLSS section, acreage, land use, and owner information. I selected geographic coverage based both upon the presence of railroad grant land and the existence of complete records at the district level. In the long survey, some assessors chose to leave ownership blank entirely or selectively and are excluded from the analysis. Since assessors were given fixed townships, this exclusion is mechanically balanced across even and odd sections and unlikely to lead to bias.

I also include 1965 "personal assessments" from Lincoln County, Nebraska. These are essentially tax filings based on personal property and, crucially, grain production. For the purposes of this project, they include a property's location, the owner's farm equipment used on it, and a breakdown of grain output by "operator" and "landlord" shares while giving the identity of both in this case. While some respondents record a contract without a share arrangement, this is uncommon meaning the survey primarily measures share tenancy.

B.4 Land Transfer Records

I measure land concentration and sale volume with two data sources. First, the Bureau of Land Management General Land Office records offers complete coverage of initial federal transfers. To the best of my knowledge, no comprehensive database of railroad transfers exists. I thus supplement these records with archival work on railroad company transfers in Lincoln County, Nebraska, which preserved its railroad sale deeds. Historical assessment and tax records were also useful for determining land concentration's impact on investment over time. To this end, I digitized the 1900 assessment records from Perkins County, Nebraska, and the 1912 assessors' records from Morrill County, Nebraska. The Morrill records additionally record the fraction of improved land and the value of improvements.

For panel data, I digitized Register of Deeds transfer records for the 17N townships of Banner County, Nebraska available at the sixteenth section (40-acre) parcel level. I selected these counties based on data quality, availability, and their possession of substantial portions of land inside and outside railroad grant areas.

B.5 Linking to Census Microdata

I often match property owners to the most recent US Census microdata prior to the assessment/sale. Since property taxes typically only includes the owner's name, I lack key pieces of information common in other linking procedures such as an owner's age or birthplace. In all cases, I can make use of the property's county. In the case of the initial sales matching for Lincoln County, Nebraska I am also able to use a listed county of origin.

I first compute a name match score between the property owner and all Census individuals, considering only the first listed owner in the uncommon case of joint property. For both the first and the last name, I compute the Jaccard string similarity index, the fraction of unique bigrams in either name that are contained in both the owner and proposed match names. In the case of single-letter first names, I substitute a value of 90% if the two names begin with the same letter. Thus, "John Smith" would be considered a good although not perfect match for "J. Smith." I compute the overall name match as the average similarity between the first and last names.

The second element of the matching procedure is how to value location. In the case of the Lincoln County, Nebraska initial sales, I consider the owner's listed county of origin, state of origin, and finally Lincoln County itself. For historical property tax matching, I consider the property's county and state only since I lack information on the owner's origin. Taking the name match value given as above, I apply a 20-percentage point premium to the Census individual's score if they reside in the listed county of origin or property value's location; I apply a 10-percentage point premium to their score if they reside in the same state as the owner or owner's property respectively. The individual with the highest match score, including location premia, is my preferred match. To exclude false matches, by default I consider tied duplicate matches or those with string similarity below 75% as non-matches.

B.6 Population and Public Goods

I obtain 2019 population at 30 meter by 30 meter resolution from the Humanitarian Data Exchange. For historical population, I use the detailed 1940 Census "enumeration district" maps showing the location of every rural farm, school, church, and other structures. The number of farmsteads serves as a good proxy for the rural population as almost all would have resided in farm buildings. I consider schools, churches, cemeteries, and community buildings as public goods. For the modern road network, I use the Federal Highway Administration's 2015 HPMS data. For town locations, I use both the Schmidt (2018) point file and the 2000 Census TIGERLINE place polygons.

B.7 Geographic Characteristics and Land Use

Elevation data are from the SRTM 250-meter resolution database. A related database from the FAO contains the terrain slope characteristic, a key agricultural input. In the small number of areas where these data are unavailable, I impute elevation and slopes, regressing the measure on latitude and longitude in each county and using the predicted value. For river and stream length, I use data from ESRI. For soil quality characteristics, I use the USDA's gSSURGO database. To measure soil quality's inherent crop productivity, I draw upon their "nccpi2 (all)" aggregated measure of soil productivity for different crops.

B.8 Land Use Value Calculation

I construct a pure "use value" of land using satellite data on land use (USDA's CropScape), models of agricultural productivity (the FAO's GAEZ), and data on crop prices.

For pixels coded for crop use, I first consider the expected crop yield according to the FAO's GAEZ data. I use the GAEZ "high input" scenario as this most accurately reflects agricultural processes in developed countries like the United States. For the small number of crops are not listed in GAEZ data, I use USDA-reported average national yields. To compute revenue, multiply by crop farmgate prices. I primarily use FAO-reported prices, but where these are missing I use USDA prices or prices from other sources.

For pasture and grassland pixels, I use the GAEZ yield for "pasture grass" as the expected yield of forage. Following Ahola (2013), I assume an average cow weight of 1000 pounds, that each cow eats 2.6% of its weight per day, and that 30% of forage is accessible. This analysis assumes, somewhat generously, that each grassland pixel is actively grazed. For non-developed, non-agricultural pixels, I assume a value of $0 in production.

Using the USDA's Commodity Cost and Returns dataset, I estimate annual production profits and convert these into net present valuations for cattle and each major US crop. I compute the profit margin as the ratio of revenue minus operating costs, hired labor, and taxes/insurance divided by revenue. About 1% of land in my sample has crops not covered therein and for those I use a 10% profit margin. I convert estimated annual profits to valuations by discounting at 5% rate, typical for assessors, and sum within section.

My final measure of use value also includes the valuation from urban areas. CropScape classifies such developed areas into "open," "low," "medium," and "high." Since valuations from this use do not come from production, they must necessarily be imputed. I regress total valuations in counties with complete information on the total amount of land in each category of development according to CropScape, combining the last two being combined as few pixels are coded as either. I include Township fixed effects and the main geographic characteristics and round the results. This procedure estimates a $12.5 million / square mile value for open development, $125 million for low development, and $300 million for medium/high. For comparison, Omaha, NE has roughly 180,000 households typically worth $200,000 (Zillow estimate) and an area of roughly 130 square miles, yielding about $277 million in value per square mile.

C Further Results

C.1 Aggregation Versus Splitting

This section delves more into the process of convergence in land concentration discussed in Section 7.3. In theory, convergence could be achieved either by splitting up larger properties or aggregating smaller properties. By tracking the ownership of individual parcels[36] over time, this section will show that the latter process is the primary driver. In this context, splitting properties was relatively rare and short-lived in both odd and even sections. This result parallels that in Bleakley and Ferrie (2014) which notes the difficulty in perfectly subdividing properties.

To measure splitting and aggregation, I compare holdings at a particular point in time t

[36] "Sixteenth" sections of 40 acres each are tracked as essentially indivisible units in these data.

to their initial boundaries in 1882.[37] Here, a "holding" constitutes all the parcels owned by a particular entity. The extent to which an initial holding remains "unsplit" is calculated by the fraction of it held by a single owner in t. The extent to which it is aggregated is measured by the maximum fraction of a holding in t that can be traced to a single initial holding. I refer to this value as the "unmerged" amount.[38] In both cases, a value of 100% indicates no change on the relevant dimension. Lower values indicate increased merging and splitting.

Perhaps surprisingly, Appendix Figure A.11 shows that odd-section properties are rarely split into smaller pieces. Over the 65-year span of data, about 86% of the odd-numbered section parcels remain within the same 1882 property, indicating that land is mostly transferred intact to the next owner. Any splitting that occurs is typically complete by 1900, meaning that most reallocation instead happens due to parcels being combined over time. As a result, by the end period, the typical odd-numbered section property can only trace 61% of its area to a single 1882 property. This fraction shrinks over time as more land is combined into larger holdings. Since dividing large properties would have been the most direct way to reverse land concentration, its rarity points to the constraints faced by small owners in obtaining property.

C.2 Rural Density

Relatedly, perhaps the density of farms per se had positive effects on productivity. For example, farmers might have cooperated with or learned from their neighbors and odd sections do have fewer farms. However, that does not necessarily mean they had fewer neighbors. The interspersed nature of the checkerboard in fact meant that farmers on both types of section would be part of very similar communities. For example, someone living on (odd) section 23 would have had neighbors in adjacent (even) sections 22, 14, 24, and 26.

Table A.6, Panel B examines this issue empirically. For each section, it picks a point at random and measures the modern[39] population living within 1-10 miles from that point. Using this slightly broader definition, there are almost no differences in density across even and odd sections. All estimated differences are smaller than 0.5% and only one (at 3 miles)

[37] Or to their first owner if allocated in a later year. The US government and railroad companies are treated as allocating entities and not as owners for this purpose.

[38] For example, consider an initial property with three owners in t who own 70%, 20%, and 10% respectively. The first piece would be considered 70% "unsplit," and the other two 20% and 10% respectively. The area-weighted average would then be $0.7 \times 0.7 + 0.2 \times 0.2 + 0.1 \times 0.1 = 0.54$ unsplit. If those owners owned no additional land, each piece would be 100% "unmerged." If a 100-acre farm in 1900 was formed from three complete properties of 70, 20, and 10 acres, it would be considered 70% unmerged and 100% unsplit.

[39] While I use modern data due to it being available in a disaggregated form, Table 5 shows that these persistently reflect historical even/odd differences.

is significant; it would indicate odd sections had a higher rather than lower density. So, although even squares had more farms operating within their boundaries, those farmers had similar numbers of total neighbors. Unless density spillovers occurred only within the artificial boundaries of PLSS squares, they cannot explain my results.

C.3 Property Rights and Conflict

Railroad owners could have invested less in their land because they felt their ownership was not secure. The slow speed of some (but not all) companies to either build their tracks or sell their land sparked "forfeiture" movements to reclaim their unsold sections. Within my sample, the detailed overview in Ellis (1946) lists movements targeting the Northern Pacific Railroad (NPRR) and Oregon/California Railroad (OCRR) companies. The others in my sample, such as the Union Pacific, were more compliant and not targeted; see Appendix Section C.5 for evidence on settlement speed. Although individual settlers were never targeted, in principle they may still have felt uncertainty ex-ante. Alston and Smith (2022) argues the NPRR's grant was uniquely troubled by this and other legal ambiguities as the company's "violations, controversies, and investigations... had no peers" (Draffan, 1998). Thus, the NPRR and to a lesser extent the OCRR are the grants where property rights would have been most insecure.

Two analyses indicate that these forms of insecurity do not explain my results. In Appendix Table A.8, odd sections in untargeted grants experienced slightly greater land value reductions: dropping the NPRR and OCRR from the sample modestly increases the estimates' magnitude and significance. Second, I analyze the frequency of lawsuits (*lis pendens* notices) in the archival sales data from Nebraska. 28% of land in the sample experiences a lawsuit over the period, with the rate actually lower (insignificantly) in odd sections. Overall, odd sections' reduced valuations seem to result from a consistent pattern in and out of contested grants like the NPRR. Based on available data, odd-section owners in other areas did not face greater legal issues with their titles.

C.4 Homestead Implementation in Railroad Grant Areas

Some historical sources argue that the Homestead Act was implemented differently in railroad grant areas. For example, proponents of the grant policy argued that doubling federal land prices in the grant area could compensate the government's loss of half its land. Other proposals would have set the standard settler plot size at 80 acres rather than 160. If implemented, these policies would complicate the RD analysis in Section 8 as multiple variables changed at the border. The even/odd regression's interpretation would be substantively unaffected since even squares would still be reserved for individual families. However, the exact policy details would require correction.

Based on historical and quantitative evidence, these proposals were not implemented to a significant degree in my sample. First, as noted by Gates (1954), the 80-acre rule was abandoned in 1879, preceding almost all settlement of my areas; the doubled $2.50/acre price was not meaningful given that the vast majority of settlers opted for free land under the Homestead Act. Appendix Table A.5 is consistent with this narrative. There is no detectable difference in federal land grant sizes within the border and, belying a higher price, slightly more land was transferred. The statistically insignificant 12-acre point estimate would represent a 3% decrease in contrast to the 50% implied by the 80- versus 160-acre distinction. Thus, there is little qualitative or quantitative evidence that federal settlement policy changed at the borders in my sample.

C.5 Date of Settlement

If either Homestead or railroad lands were systematically settled earlier, any differences today could simply reflect some sort of first-mover advantage or head start from the earlier group. While comprehensive data on railroad sales are unavailable across all railroad grants, the archival sales data offer one window into this question. Appendix Figure A.6b shows the fraction of land that had at least one (non-railroad, non-federal) owner by year. Railroad and Homestead land were settled around the same time in this county, with neither group consistently experiencing a faster process.

C.6 Speculation

Gates (1936) and other historians viewed some large-scale railroad land buyers to be "speculators." One interpretation of this view is that those owners held their land off the market, aiming to let it appreciate in value following population increases rather than from their own investments. My results would thus primarily represent the long-term effects of a free-riding problem rather than land concentration per se. However, Gates connected speculation with land concentration, writing that tenant farming was one "of the worst effects of the resulting large-scale ownership [from these purchases]." Many speculators had long-term ambitions of "establishing for themselves a permanent investment from which they and their descendants might draw rents as the landed aristocracy of England had done for centuries" (Gates, 1941).

Quantitative evidence also cuts against the idea of odd-sections being held idly off the market. Appendix Figure A.6a indicates that they were transferred by their owners somewhat more frequently than even sections.[40] Very simply, odd-section owners were not typically holding their properties off the market, and by 1920, 99% of odd-sections had been transferred at least once and 59% had been transferred three or more times. Delayed investments from land held off the market should also not have been more harmful in areas with high rates of share tenancy, the pattern documented in Section 7.2. Both qualitative and quantitative evidence indicates that odd-section owners were in fact working their farms, making additional use of tenant farming relative to even-section owners. This element, rather than idle land, is key to the results.

C.7 Credit Constraints

In this section, I use archival data to provide evidence that even-section owners had more limited access to capital than odd-section owners. This finding supports the hypothesis that credit constraints played an important role in the slow reallocation process discussed in Section 7.3. However, other frictions could certainly have contributed and this paper does not aim to exhaustively list them; see Bleakley and Ferrie (2014) for other important work on this topic. As with the original Coase Theorem, many possible market imperfections would lead to the same key result that the initial allocation had long-term effects.

To measure landowners' access to capital, I use an archival sample of property tax records dating from 1900. Property taxes were a substantial cash obligation in this setting, meaning that difficulties paying them reflected a general lack of access to cash. For each parcel, the records list the land's owner, the date of the tax payment, and by whom the tax was paid. Essentially, all taxes are eventually paid in this context, but on average it took 24 months, and 71% of owners used an intermediary, indicating substantial difficulties for these settlers. On average, they paid their taxes off 5 months later (t=4.32). Even-section owners were also 6.7 percentage points (t=2.05) more likely to use an intermediary to pay.[41]

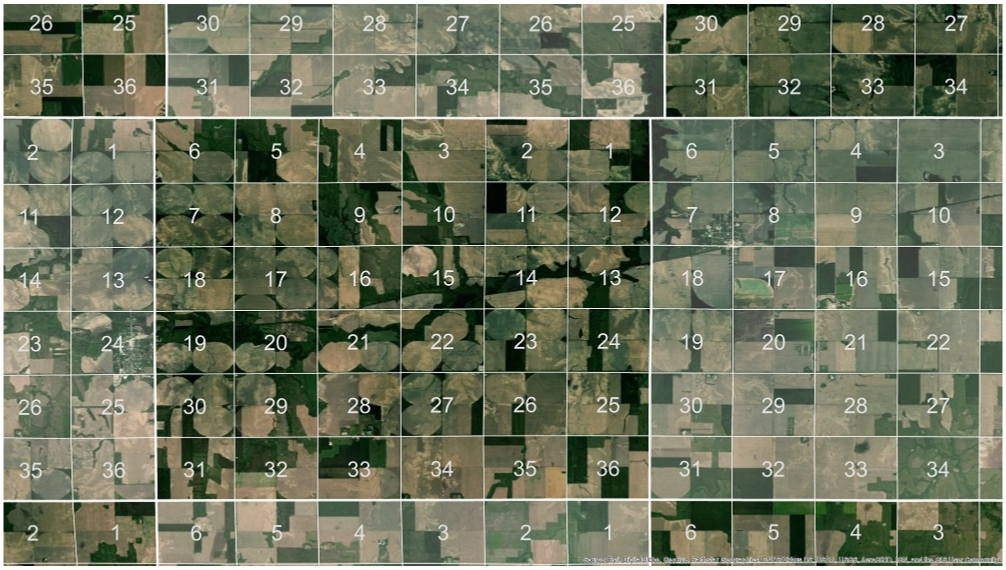
These delays and heightened reliance on intermediaries suggest that even-section owners had more limited access to capital than their odd-section counterparts. This result aligns well with the historical context: railroad land buyers were by definition capable of purchasing property, whereas the Homestead Act aimed to distribute land to individuals less able to do so.

[40] The data shown do not consider the "first" transfer from either the federal government or the railroad company in this graph.

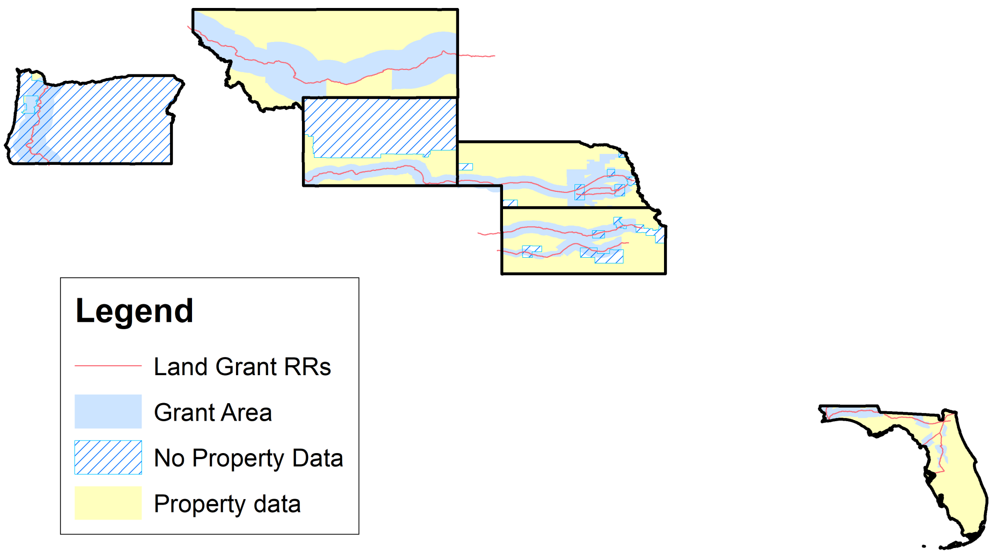
[41] Both regressions include the full set of controls as in Table 3, column (6). Since the archival dataset includes just one county, standard errors are clustered at the township level as in similar cases.

D Appendix Tables and Figures

Figure A.1: The Public Lands Survey System

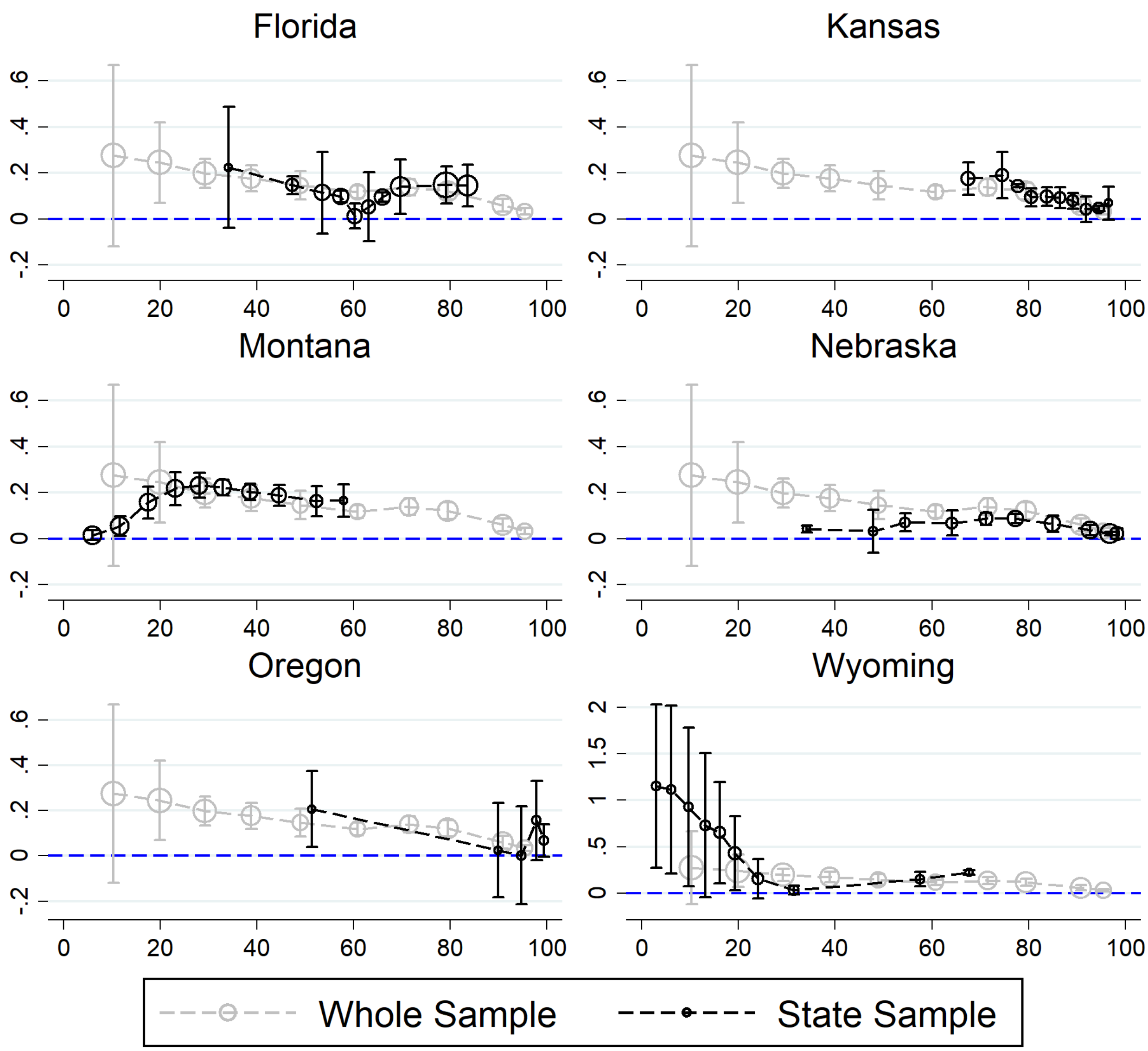


(a) Nebraska Townships and Numbered Sections



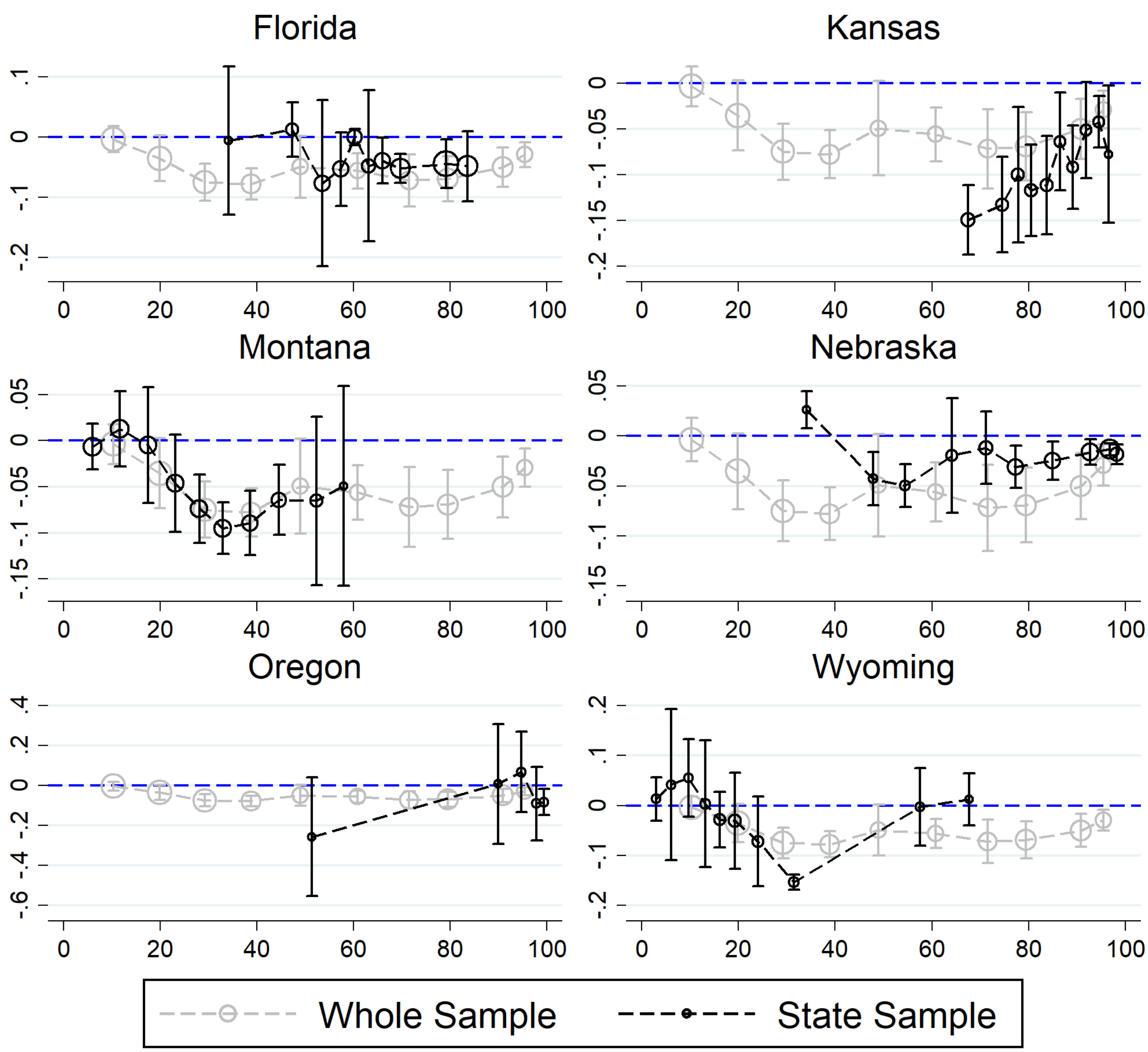
(b) Extent of Railroad Land Grants

Figure A.2: Effects on (log) Average Parcel Size



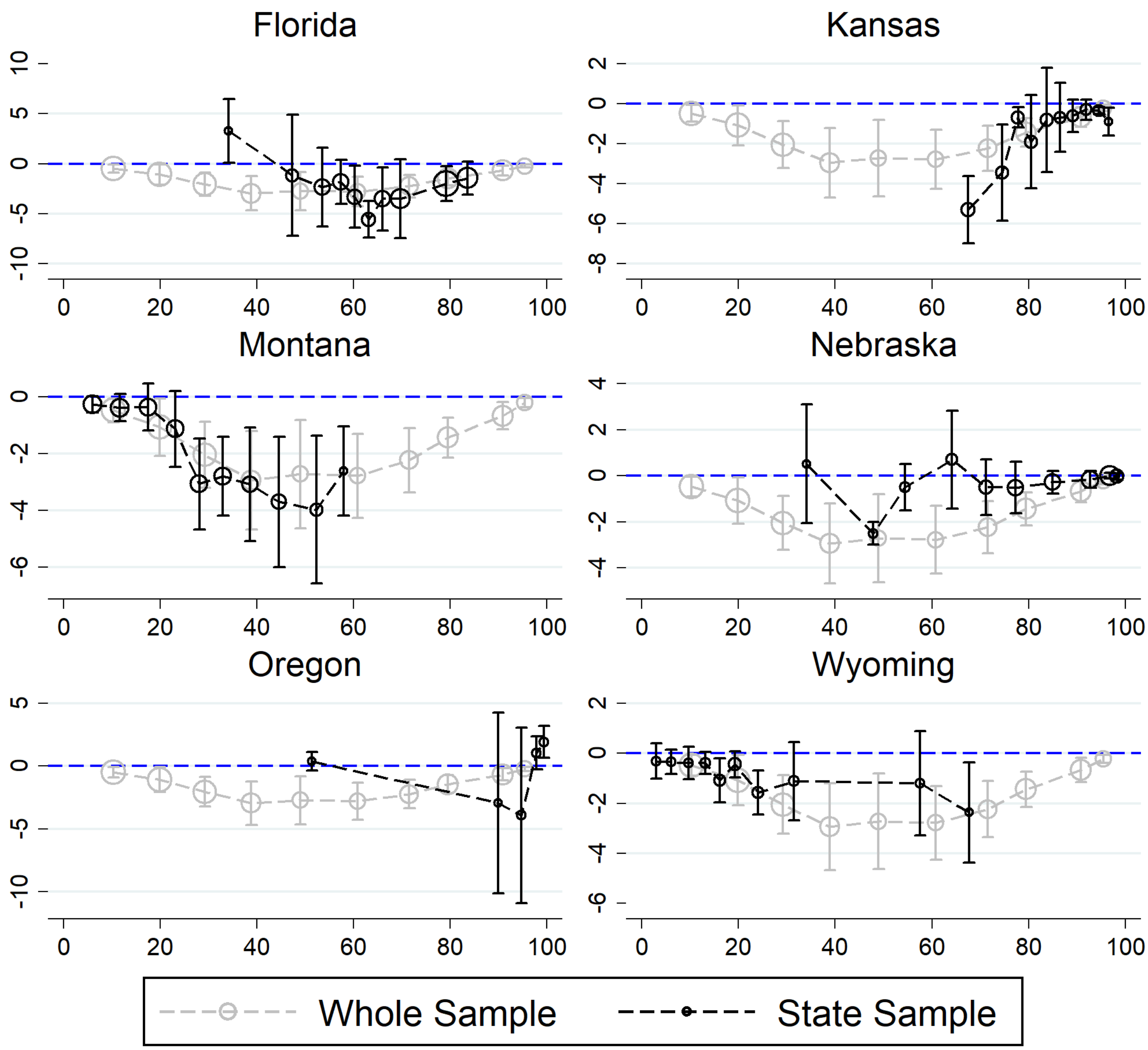
Notes: This graph shows full-sample and state-sample estimates of railroad land ownership on log(Acres / parcels) in a PLSS section as per the even/odd comparison of equation (1). Parcels that cover multiple sections are counted fractionally across each section so their total contribution sums to 1. Each dot represents a subsample of sections based on land quality according to the gSSURGO database. X-axis values reflect the average, full-sample percentile of land quality within the sample. State samples are chosen to reflect a 20-percentile range of the land quality within their state.

Figure A.3: Effects on (asinh) Land Value



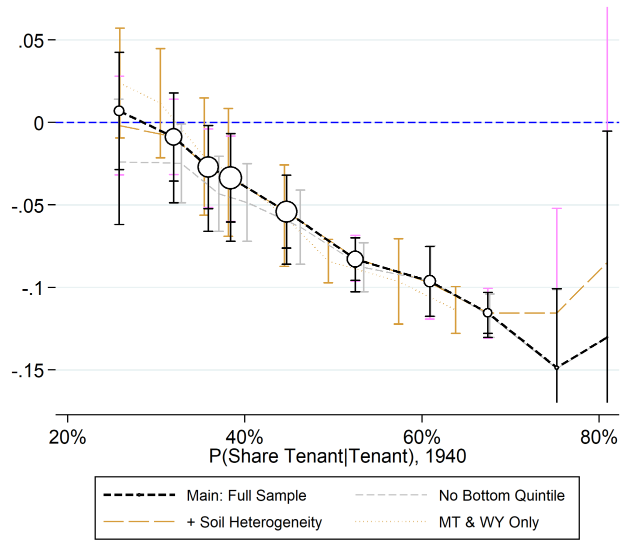
Notes: This graph shows full-sample and state-sample estimates of railroad land ownership on the (asinh) modern land value in a PLSS section as per the even/odd comparison of equation 1. Each dot represents a subsample of sections based on land quality according to the gSSURGO database. X-axis values reflect the average, full-sample percentile of land quality within the sample. State samples are chosen to reflect a 20 percentile range of the land quality within their state. Bars depict 95% confidence intervals.

Figure A.4: Effects on Crop Farms

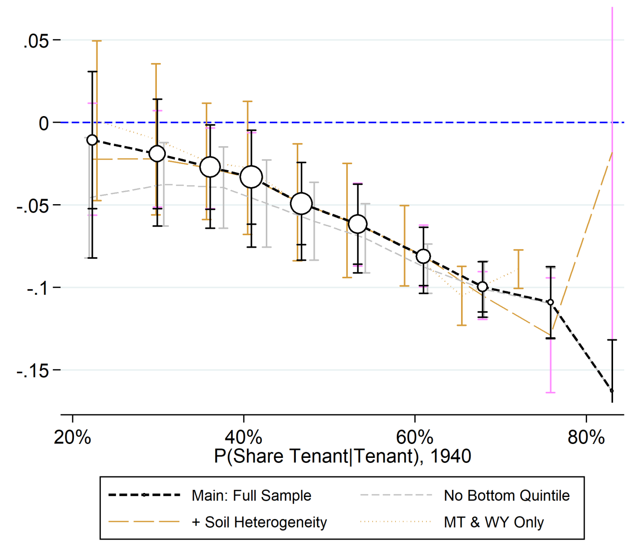


Notes: This graph shows full-sample and state-sample estimates of railroad land ownership on the percent of PLSS sections growing crops on 1% or more of their area, using the even/odd comparison of equation (1). Each dot represents a subsample of sections based on land quality according to the gSSURGO database. X-axis values reflect the average, full-sample percentile of land quality within the sample. State samples are chosen to reflect a 20 percentile range of the land quality within their state. Projected values are censored to remain within the 0-100% range. Bars depict 95% confidence intervals.

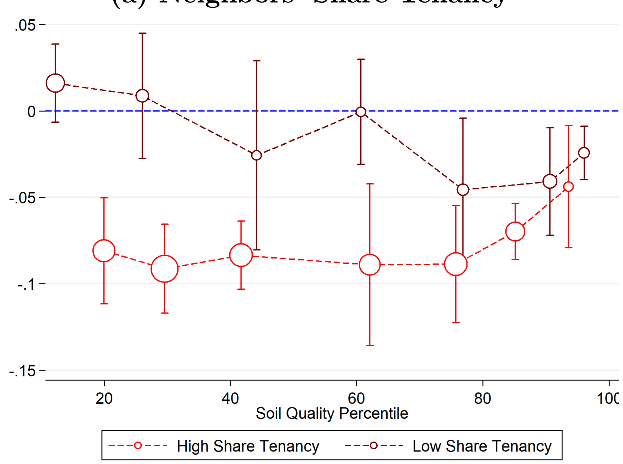
Figure A.5: Effects on Property Values by (Predicted) Share Tenancy



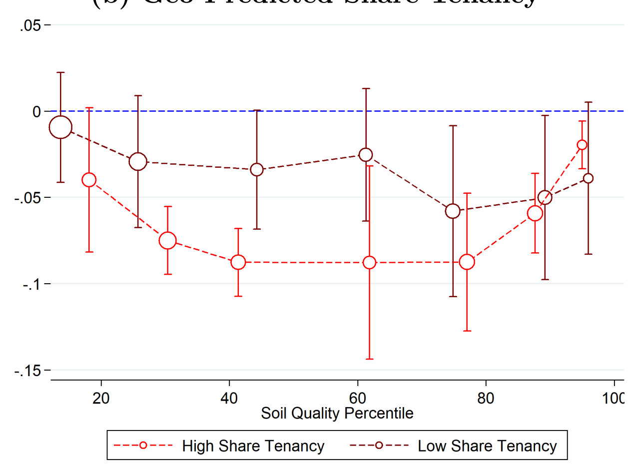
(a) Neighbors' Share Tenancy



(b) Geo-Predicted Share Tenancy



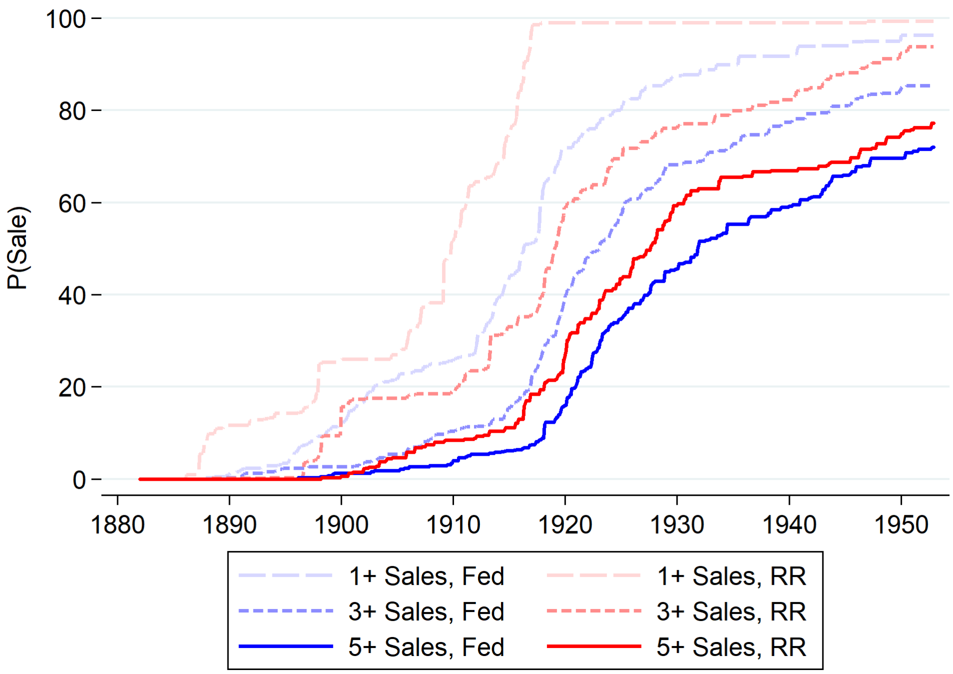
(c) Neighbors' Share Tenancy



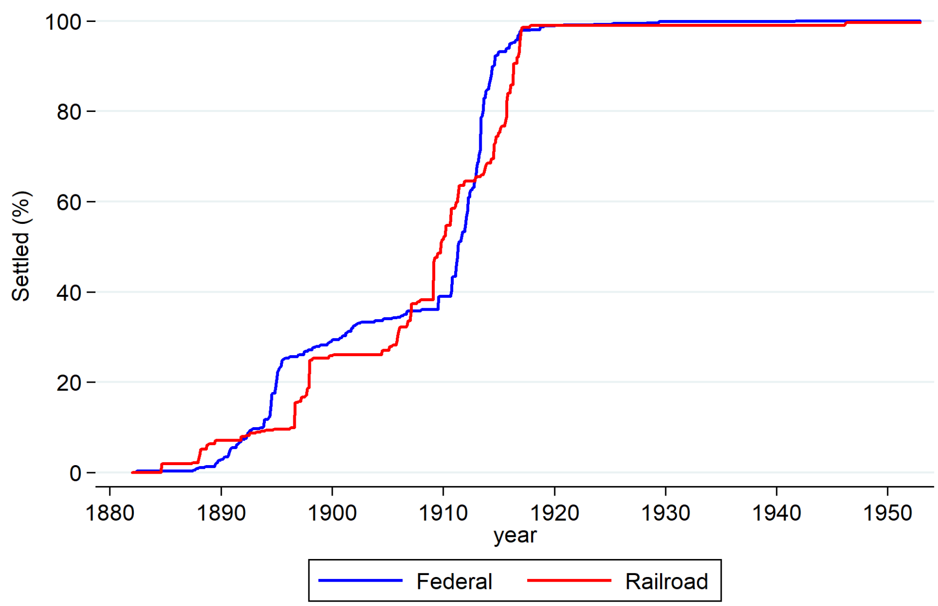
(d) Geo-Predicted Share Tenancy

Notes: This figure replicates the share tenancy heterogeneity results in Figure 4, using predicted rates of share tenancy instead of the county's own value. As such, the source of heterogeneity for any particular section is not determined by the section's own rates of share tenancy. (a), (c) use the average rate of share tenancy in neighboring counties within the same state as the prediction. (b)-(d) regress counties' share tenancy rates on the county-average values of the geographic characteristics and log(county area), using the regression-predicted values. (a)-(b) replicate Figure 4 based on the predicted levels of share tenancy. They add additional specifications that respectively drop the bottom quintile of observations based on gSSURGO soil quality; add a linear interaction between odd and soil quality; and a sample restricted to the states of Montana and Wymoing which have lowest soil quality among sample states. Panels (c)-(d) compare effects by soil quality percentile for areas above and below median rates of predicted share tenancy.

Figure A.6: Extent of Sales Over Time (archival sample)



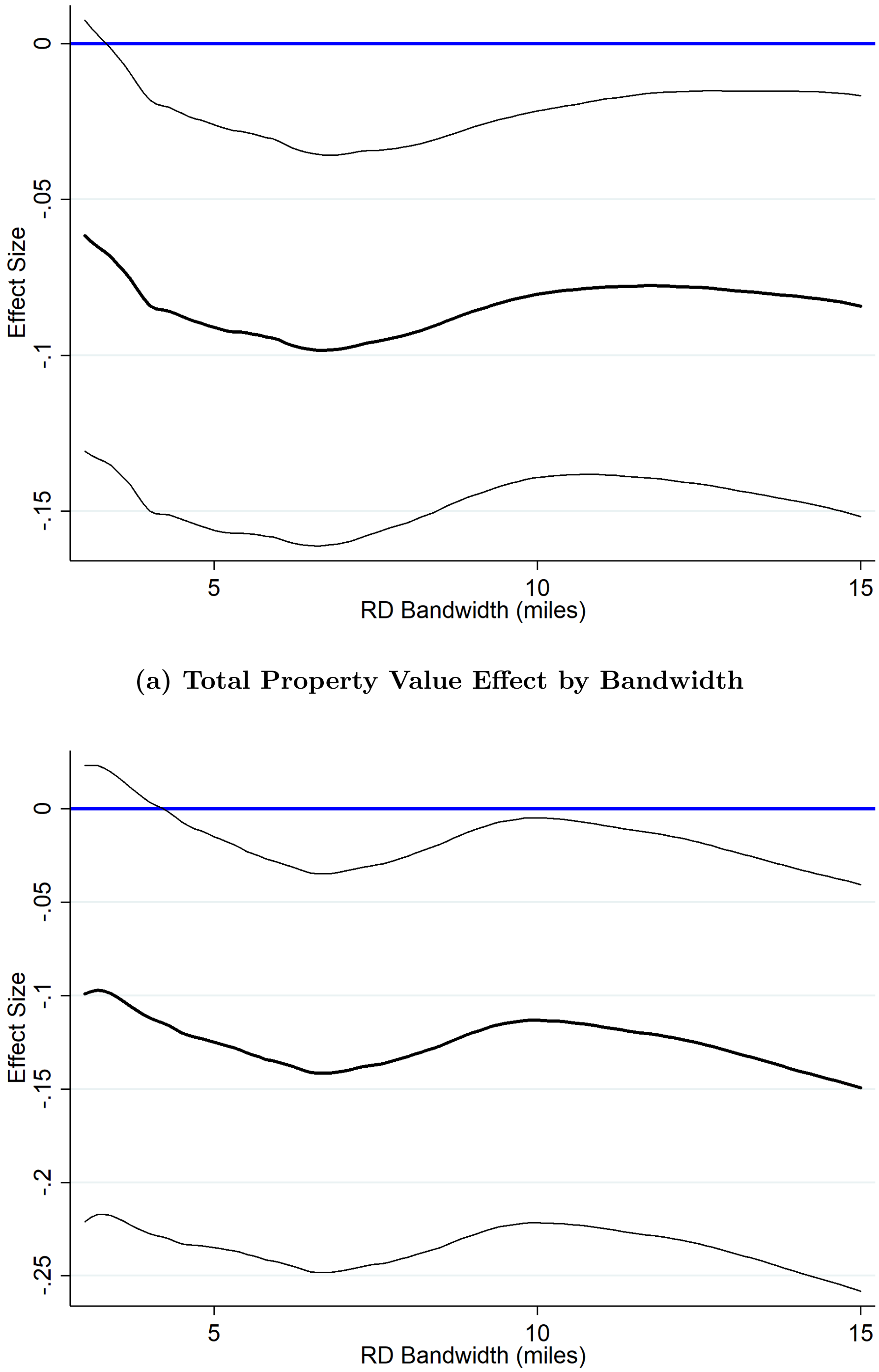
(a) Probability 1+, 3+, 5+ Cumulative Sales



(b) Ever Settled

Notes: (a) depicts the percent of railroad versus federal parcels in Banner, NE that had been transferred 1+, 3+, or 5+ times by individual owners (i.e. excluding initial transfers from either the US government or railroad). (b) depicts the fraction of land that had been transferred to its first owners (ignoring the US government and railroad companies).

Figure A.7: RD Bandwidth Robustness

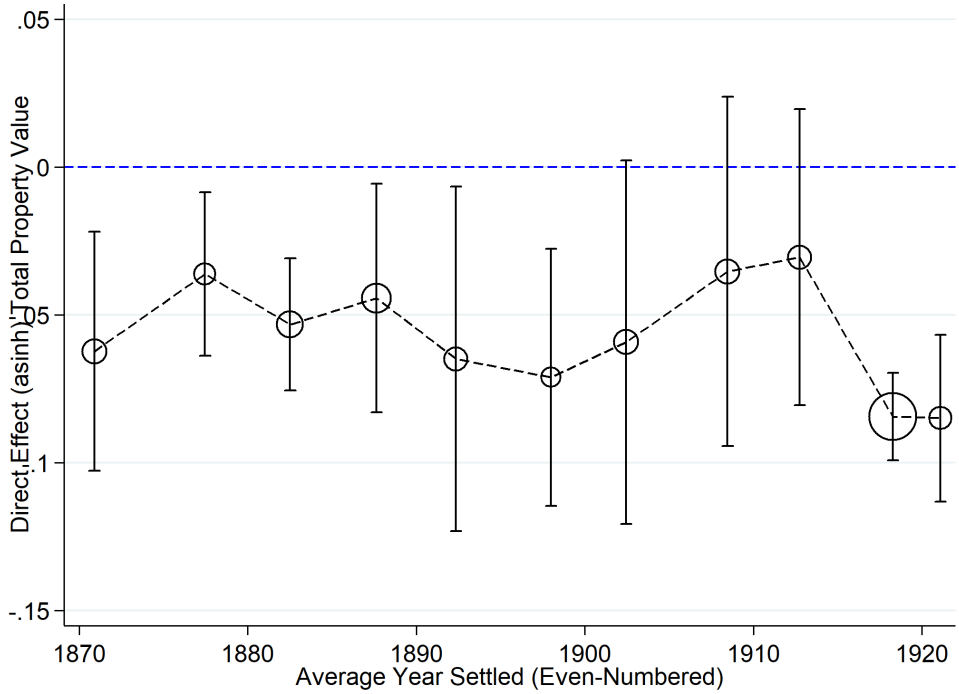


(a) Total Property Value Effect by Bandwidth

(b) Improvement Value Effect by Bandwidth

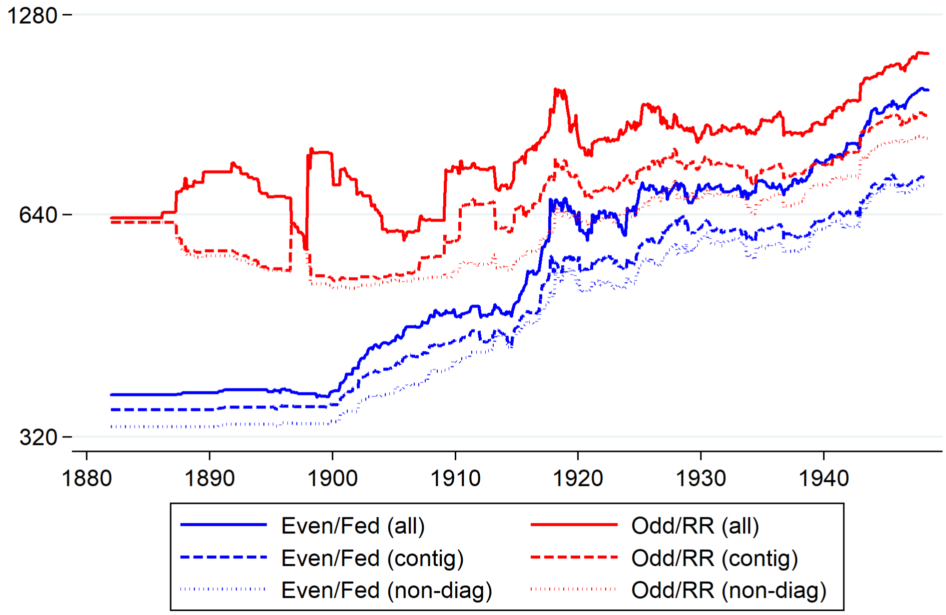
Notes: Estimates of equation (2) as a function of bandwidth, with 95% confidence intervals plotted. (a) plots (asinh) 2017 assessed total value, (b) plots (asinh) assessed improvement value.

Figure A.8: Effects by Year of Average County Settlement

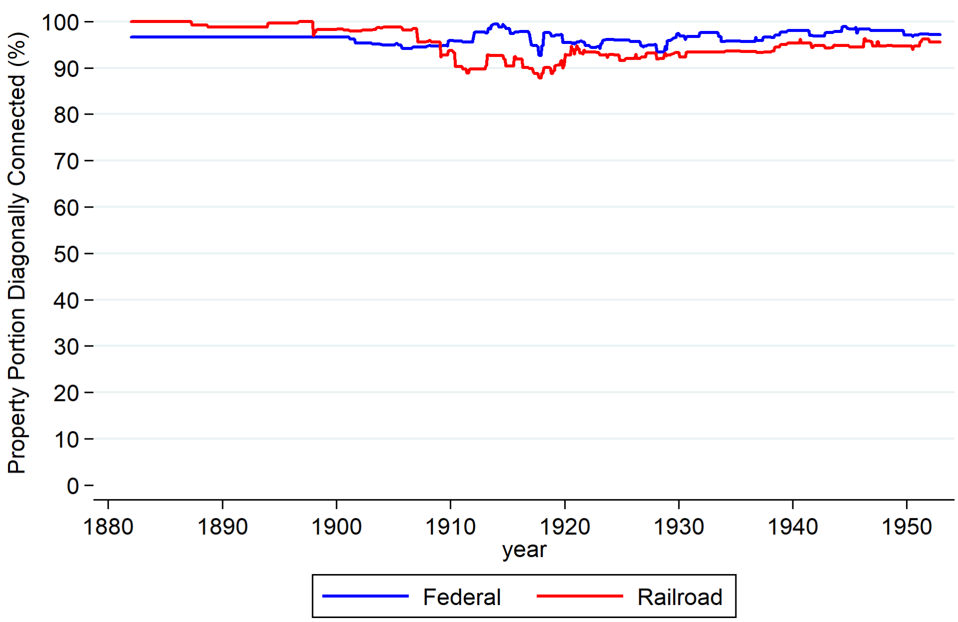


Notes: This graph shows full-sample and state-sample estimates of railroad land ownership on the (asinh) modern land value in a PLSS section as per the even/odd comparison of equation 1. Each dot represents a subsample of sections based on the average land was settled in the non-railroad lands of a county. This value is computed for each PLSS section as the average year of federal settlement for non-education, non-railroad sections within the county, excluding the section itself (i.e., "leave one out"). Bars depict 95% confidence intervals.

Figure A.9: Alternative Property Size Measures



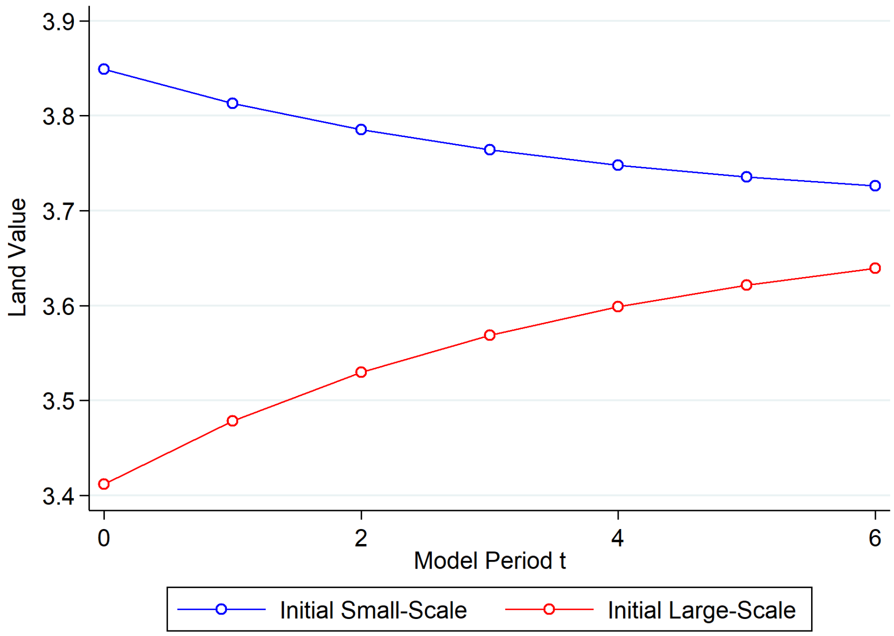
(a) Measures of Property Sizes



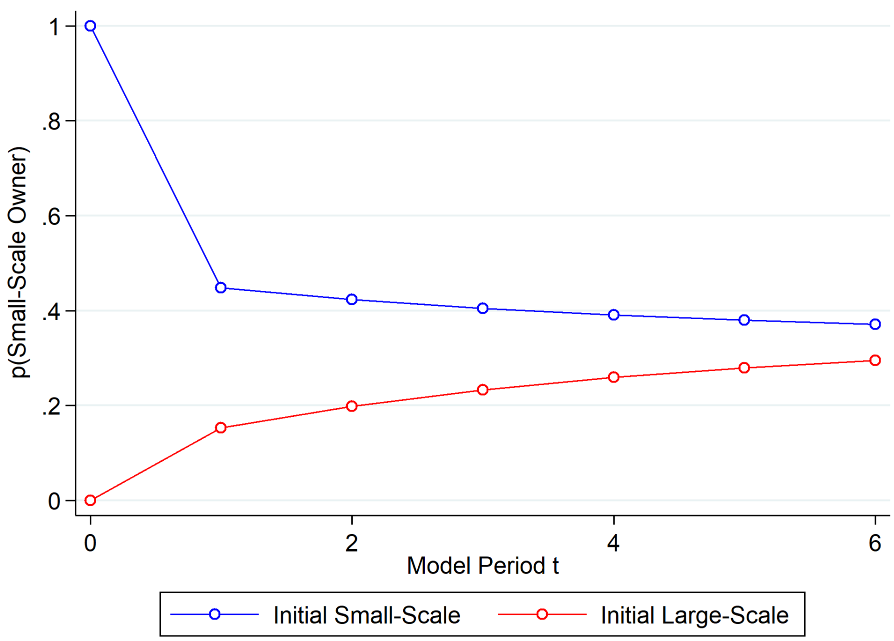
(b) Diagonal/Contiguous Property Size Ratio

Notes: property size measures over time based on archival sales data. (a) plots section-average log property sizes for even/odd sections based on an owner's entire holdings, contiguous holding, and contiguous holdings excluding diagonal connections. (b) plots the section-average ratio between the third and second of these groups.

Figure A.10: Sample Model Dynamics



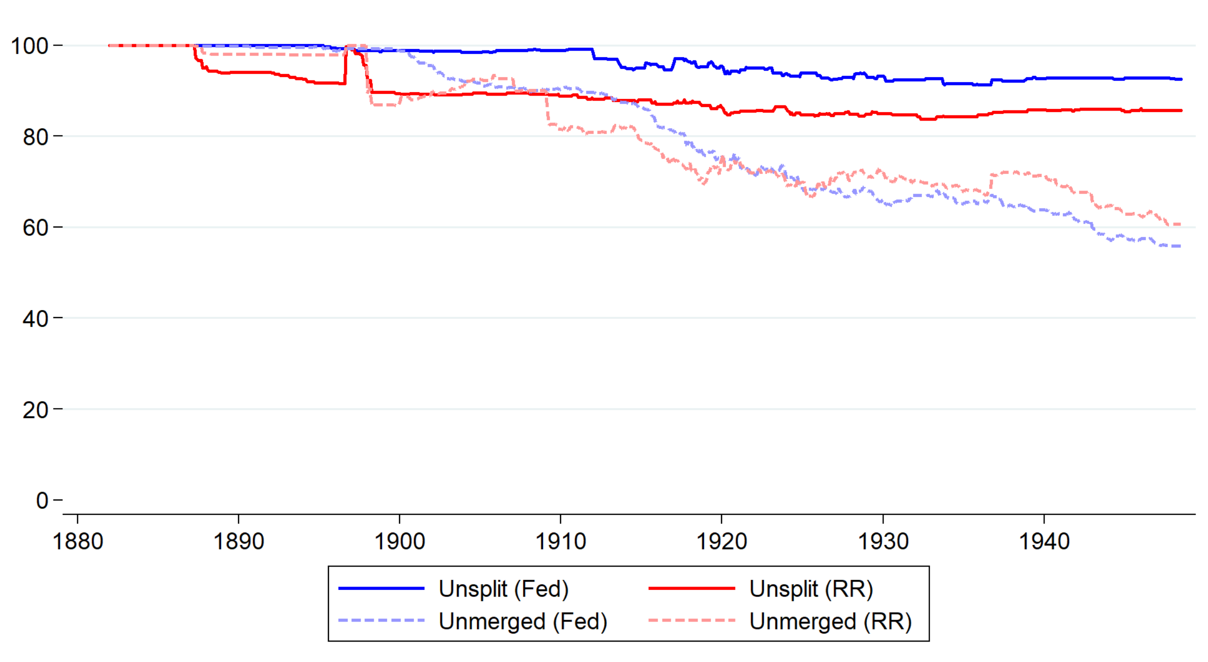
(a) Land Value



(b) Probability of Small-Scale Ownership

Notes: The figure shows expected land value and the probability of smallholder ownership for initial small- versus largeholder land. Parameter values are given by $A = 2$, $\beta = 0.7$, $\delta = 0.15$, and improvement costs of $r = 1$. Effort costs are quadratic: $\frac{1}{2}e^2$. Tenants face a limited liability and an outside option of 0, leading to an even-split sharecropping contract. S-type buyers face uniform costs on [0,2].

Figure A.11: Splitting and Merging of Initial Properties



Notes: the extent to which initial properties in the Banner, NE ownership panel remain unsplit or unmerged. See Appendix Section C.1 for definitions. A value of 100% indicates no change, lower values indicate more splitting or aggregation.

Table A.1: Sales Price and Assessed Value per Acre

	Panel A: Sales vs. Assessed Value, Section Level			
	(1)	(2)	(3)	(4)
	log(Sale Price/Acre)	log(Sale Price/Acre)	log(Sale Price/Acre)	log(Sale Price/Acre)
log(Total Val/Acre)	0.94***	0.51***	0.61***	0.68***
	(0.0080)	(0.044)	(0.038)	(0.038)
log(Land Only Val/Acre)		0.14***	0.18***	0.11***
		(0.051)	(0.034)	(0.033)
Sample	All	All	Agricultural	Agricultural
Township FEs				Y
SEs / Clusters	Township	Township	Township	Township
N	22,970	6,250	6,250	6,250
$\mathbb{E}[y]$	7.9	7.9	8.5	8.5

	Panel B: Sales vs. Assessed Value, Parcel Level			
	(1)	(2)	(3)	(4)
	log(Sale Price/Acre)	log(Sale Price/Acre)	log(Sale Price/Acre)	log(Sale Price/Acre)
log(Total Val/Acre)	0.80***	0.63***	0.60***	0.70***
	(0.020)	(0.043)	(0.046)	(0.037)
log(Land Only Val/Acre)		0.23***	0.14***	0.038
		(0.035)	(0.036)	(0.028)
Sample	All	All	Agricultural	Agricultural
Township FEs				Y
SEs / Clusters	Township	Township	Township	Township
N	913,886	850,494	11,104	11,104
$\mathbb{E}[y]$	9	8.9	8.6	8.6

	Panel C: Use vs. Assessed Value, Section Level			
	(1)	(2)	(3)	(4)
	asinh(Value)	asinh(Value)	asinh(Value)	asinh(Value)
asinh(Use Value)	0.42***	0.46***	0.25***	0.23***
	(0.0057)	(0.0087)	(0.0040)	(0.0045)
asinh(Ag. Use Value)		-0.066***		0.10***
		(0.0080)		(0.0038)
Sample	All	All	All	All
Township FEs			Y	Y
SEs / Clusters	Township	Township	Township	Township
N	339,482	339,482	339,482	339,482
$\mathbb{E}[y]$	7,434	7,434	7,434	7,434

Notes: This table correlates sales, assessed, and use values per acre. Sales data come from Florida tax records from 2016-17. It considers properties sold in 2016-17 with a positive sales price and reported acreage. Both the total property valuation and the valuation excluding "improvements" (buildings) are considered. Panel C correlates author-generated values based on land use with assessed values for the sample of property tax counties (regardless of railroad grant status); see Appendix Section B.8. Panel B uses data at the property level. Panels A and C aggregate values to the PLSS section level, as in the paper's main regressions for equation (1).

Table A.2: Archival Sample Description

Area (year)	Source	Variables	Where Used
Lincoln County, NE (1800s)	First individual owners (Federal: BLM records Railroad: deeds of sale)	Owner name, county of origin, property description	Figure 2, Table 6
Morrill County, NE (1912)	Land assessment, local elections	Owner name, property description, improved land, improvement value, officeseeking	Figure 3, Table 4, Table 6, Table 8, Table A.4, Table A.7
Nebraska (1940)	Census enumeration district maps	Number of farms, schools, and other public goods by PLSS section	Table 5, Table A.7
Lincoln County, NE (1965)	Personal property assessment	Farm equipment, (share) tenancy by PLSS section	Table 5, Table 6
Perkins County, NE (1900)	Land assessment	Owner name, property description	Table 6, Table 8
Kansas (1940) 30 townships Barton, Dickinson, Harvey, Pottawatomie counties	State agricultural survey	Operator name, property description, owner name	Table 6
Banner County, NE (1882-1948)	Land transfer records	Owner name, recipient name, property description, deed type	Figure 5, Figure A.6a, Table A.8

Notes: Descriptions of archival samples used in this paper. Kansas 1940 survey samples include only townships with complete lists of owners and operators. In many cases, the list of owners was left blank. Banner County, NE land records cover the 17N townships.

Table A.3: Summary Statistics (Archival Samples)

	PLSS Sections						Counties (1940)	
	(1) Number Sections	(2) Soil Quality (z-score)	(3) Homestead (%) (Even Only)	(4) Crops (%)	(5) Value \$000	(6) # Parcels (Median)	(7) Tenant Farm (%)	(8) Share Farm (% Tenant)
RR Grant Areas	132,463	-.113	86.9	47.9	2,231	2	38.8	42.8
Lincoln, NE (1800s, 1965)	2,084	-.107	99.3	44.4	1,078	2	49.2	35.2
Morrill, NE (1912)	101	-.273	93.3	41.6	307	1	55.7	63.3
Nebraska (1940)	18,622	.92	95.9	82.8	2,296	5	51.5	39.8
Perkins, NE (1900)	537	.345	98.5	98.9	1,398	4	54.4	55.9
KS State Census (1940)	738	1.45	83.6	96.5	2,626	7	42.3	43.6
Banner, NE (1882+)	204	-.278	96.6	72.5	338	2	42.7	71.7

Notes: The table presents summary statistics for different geographic units, focused on the archival data samples of Table A.2. Column (1) shows results for section sample size, column (2) for the gSSURGO crop productivity index (full-sample z-score), column (3) for percentages of non-railroad land transferred under the Homestead Act, column (4) for the percentage of sections with at least 1% in crops per the USDA CropScape data, column (5) for total property values, column (6) for the median number of parcels, column (7) for county-level average rates of non-owner-operated (tenanted) 1940 farms, and column (8) for county-level averages of the shares tenant farms as a fraction of all tenant farms in 1940.

Table A.4: Land Values — Functional Form and Heterogeneity

	Panel A: Functional Form (Total Property Value)				
	(1) asinh(x) (baseline)	(2) ln(1+x)	(3) ln(max(1,x))	(4) $x > 0$ (%)	(5) $x >$ median (%)
RR Effect	-0.045***	-0.045***	-0.046***	-0.015	-1.49***
	(0.014)	(0.013)	(0.013)	(0.026)	(0.39)
Grant × State FEs	Y	Y	Y	Y	Y
County FEs	Y	Y	Y	Y	Y
Township FEs	Y	Y	Y	Y	Y
SEs / Clusters	Spatial	Spatial	Spatial	Spatial	Spatial
N	132,463	132,463	132,463	132,463	132,463
$\mathbb{E}[y]$	\$2,134k	\$2,134k	\$2,134k	1.0e+02%	50%

	Panel B: Functional Form (Investments)				
	1912 Sample			Full 2017 Sample	
	(1) Imp. Value > 0 (%)	(2) (asinh) Imp. Value / owners	(3) Acres Imp. (% Section)	(4) Imp. Value > 0 (%)	(5) (asinh) Imp. Value, non-home
RR Effect	-24.1**	-1.14***	-9.93**	-3.68***	-0.16***
	(8.01)	(0.19)	(4.09)	(1.00)	(0.034)
Grant × State FEs	Y	Y	Y	Y	Y
County FEs	Y	Y	Y	Y	Y
Township FEs	Y	Y	Y	Y	Y
Geo Controls	Y	Y	Y	Y	Y
SEs / Clusters	Township	Township	Township	Spatial	Spatial
N	101	82	101	132,463	132,463
$\mathbb{E}[y]$	23%	\$2.7k	13%	43%	\$412k

	Panel C: Heterogeneity				
	(1) (asinh) Value Total	(2) (asinh) Value Ag.	(3) Crop Farm (%)	(4) (asinh) Value Improvements	(5) (asinh) Pop
RR Effect	-0.059***	-0.045***	-1.85***	-0.30***	-0.11***
	(0.013)	(0.013)	(0.49)	(0.038)	(0.011)
RR × Low	0.052***	0.064***	1.38***	0.25***	0.078***
	(0.012)	(0.016)	(0.46)	(0.038)	(0.012)
Grant × State FEs	Y	Y	Y	Y	Y
County FEs	Y	Y	Y	Y	Y
Township FEs	Y	Y	Y	Y	Y
Geo Controls	Y	Y	Y	Y	Y
SEs / Clusters	Spatial	Spatial	Spatial	Spatial	Spatial
N	132,463	132,462	132,462	132,463	132,463
$\mathbb{E}[y]$	\$2,231k	\$380k	48%	\$1,277k	23

Notes: This table extends Table 6.2 with alternative functional forms for the outcomes and heterogeneity. Panel A focuses on functional form. (1) and (4) considers the extensive margin of improvements. (2) studies the (asinh) value of improvements divided by the number of individual owners who own land in the section. (3) studies the fraction of a section's land marked as improved. (5) focuses on the value of improvements excluding homes and dwellings. Panel B considers an interaction effect with low land quality, defined as a gSSURGO quality measure in the bottom 20% of the sample. All data are from the full modern sample and respectively use modern total property value, imputed use value based on satellite data, the extensive margin of crop farming, the value of improvements, and population.

Table A.5: Checkerboard Area Effects

	Panel A: Main Estimates					
	Baseline			Drop 1-Mile Donut		
	(1) (asinh) Total Value	(2) (asinh) Value Improvements	(3) (%) Any Improved	(4) (asinh) Total Value	(5) (asinh) Value Improvements	(6) (%) Any Improved
In Checkerboard [Even]	-0.098**	-0.14**	-1.28	-0.18***	-0.18*	-1.46
	(0.038)	(0.065)	(0.97)	(0.066)	(0.095)	(1.73)
Grant × State FEs	Y	Y	Y	Y	Y	Y
County FEs	Y	Y	Y	Y	Y	Y
Geo Controls	Y	Y	Y	Y	Y	Y
SEs / Clusters	County	County	County	County	County	County
N	32,511	32,511	32,510	27,214	27,214	27,213
N (clusters)	162	162	161	162	162	161
$\mathbb{E}[y]$	\$1,674k	\$998k	51%	\$1,674k	\$998k	51%

	Panel B: Land Quality Balance					
	(1) Soil (z-score)	(2) Slopes (z-score)	(3) Streams (z-score)	(4) Elevation (z-score)	(5) log(Area)	(6) log(RR Dist)
In Checkerboard [Even]	-0.012	0.0062	0.024	0.0018	0.0032	-0.0058*
	(0.014)	(0.0063)	(0.025)	(0.0059)	(0.0034)	(0.0033)
Grant × State FEs	Y	Y	Y	Y	Y	Y
County FEs	Y	Y	Y	Y	Y	Y
Geo Controls	N	N	N	N	N	N
SEs / Clusters	County	County	County	County	County	County
N	39,825	39,825	39,825	39,825	39,825	39,825
N (clusters)	162	162	162	162	162	162
$\mathbb{E}[y]$	.059	.88	.47	1.6	-.017	3.2

	Panel C: Federal Settler Characteristics					
	(1) Acres Granted	(2) Ever Granted (%)	(3) Public Land 2017 (%)	(4) Occ. Income	(5) Farm Home (%)	(6) Owns Home (%)
In Checkerboard [Even]	-12.3	1.28*	-0.84	-0.11	2.18	-1.66
	(9.72)	(0.76)	(0.71)	(0.40)	(1.76)	(3.31)
Grant × State FEs	Y	Y	Y	Y	Y	Y
County FEs	Y	Y	Y	Y	Y	Y
Geo Controls	Y	N	Y	Y	Y	Y
SEs / Clusters	County	County	County	County	County	County
N	24,122	32,511	31,754	7,106	8,156	2,965
N (clusters)	157	162	161	135	137	104
$\mathbb{E}[y]$	396 ac	58%	27%	14%	56%	71%

Notes: RD comparisons of federal sections per equation (2). Panel A considers (1)-(2) 2017 total and improvement value (3) owned acreage in 1900s assessments (4) lack of census microdata link to 1900s owner (5)-(6) number of distinct CropScape land uses and extensive margin of crop farming. Panel B considers the geographic characteristics analyzed in Table 2. Panel C considers (1) average acres per grant, top-coded at the 95$^{\text{th}}$ percentile (2) the percentage of land ever granted (3) the percentage of public land in 2017 (4)-(6) consider the characteristics of settlers linked to census microdata in the decade before their grant.

Table A.6: Distribution of Population Effects

	Panel A: Town Outcomes, Even/Odd					
	(1)	(2)	(3)	(4)	(5)	(6)
	# Towns CDPs	# Towns Schmidt (2018)	Pop $\geq$ 1 (%)	Pop $\geq$ 10 (%)	Pop $\geq$ 100 (%)	Pop $\geq$ 1000 (%)
RR Effect	0.00029	0.0010*	-3.63***	-1.02***	-0.046	0.0085
	(0.00024)	(0.00059)	(0.66)	(0.30)	(0.054)	(0.019)
Sample	All	All	All	All	All	All
Grant × State FEs	Y	Y	Y	Y	Y	Y
County FEs	Y	Y	Y	Y	Y	Y
Township FEs	Y	Y	Y	Y	Y	Y
Geo Controls	Y	Y	Y	Y	Y	Y
N	132,463	132,463	132,463	132,463	132,463	132,463
$\mathbb{E}[y]$	.024	.0039	33%	11%	3%	.58%

	Panel B: (asinh) Population Within X Miles					
	(1)	(2)	(3)	(4)	(5)	(6)
	1 mile	2 miles	3 miles	4 miles	5 miles	10 miles
RR Effect	0.0012	-0.0040	0.0044***	0.0032	-0.0013	0.0012
	(0.0033)	(0.0034)	(0.0012)	(0.0025)	(0.0018)	(0.0033)
Grant × State FEs	Y	Y	Y	Y	Y	Y
County FEs	Y	Y	Y	Y	Y	Y
Township FEs	Y	Y	Y	Y	Y	Y
Geo Controls	Y	Y	Y	Y	Y	Y
N	132,463	132,463	132,463	132,463	132,463	132,463
$\mathbb{E}[y]$	74	292	648	1,140	1,774	74

Notes: This table tests for effects of railroad land grants on town formation. Panels A-B use PLSS-section level data on the fraction of land in a census place, the Schmidt (2018) number of towns, and the satellite-based population. Panel A explores the even/odd comparison from equation (1). Panel B studies the (asinh) population in 2019 within specified distances of a random point within each section.

Table A.7: Public Goods and Political Behavior

	Panel A: Even/Odd				
	(1)	(2)	(3)	(4)	(5)
	Schools	Churches	Community Halls	Road Distance	Owner Seeks Office (%)
RR Effect	-0.014	-0.00022	-0.0010***	0.0021***	-3.61
	(0.0100)	(0.00078)	(0.00035)	(0.00076)	(5.05)
Sample	NE & KS 1940	NE & KS 1940	NE & KS 1940	All 2015	Morrill 1912
Grant × State FEs	Y	Y	Y	Y	Y
County FEs	Y	Y	Y	Y	Y
Township FEs	Y	Y	Y	Y	Y
Geo Controls	Y	Y	Y	Y	Y
N	18,622	18,622	18,622	132,463	82
$\mathbb{E}[y]$	.096	.013	.0025	1.1 mi	5.5%
	Panel B: In Checkerboard (Federal Only)				
	(1)	(2)	(3)	(4)	(5)
	Schools	Churches	Community Halls	Road Distance	Owner Seeks Office (%)
In Checkerboard [Even]	-0.011	0.00067	-0.00060	0.051	-5.21
	(0.012)	(0.0047)	(0.0011)	(0.042)	(3.45)
Sample	NE & KS 1940	NE & KS 1940	NE & KS 1940	NE & KS 1940	Morrill 1912
Grant × State FEs	Y	Y	Y	Y	Y
County FEs	Y	Y	Y	Y	Y
Geo Controls	Y	Y	Y	Y	Y
N	18,514	18,514	18,514	296,289	525
$\mathbb{E}[y]$	.086	.01	.00054	1.3 mi	4.9%

Notes: This table studies the presence of public goods, tax records, and officeseeking on PLSS sections. Panel A compares even and odd sections using equation (1). Panel B considers even (Homestead) sections within the grant area differ from those outside using equation (2). Columns (1)-(3) count the number of schools, churches, and community halls according to 1940 census enumeration district maps. (4) measures the distance from the section's centroid to the closest road in 2015. (5) uses an archival case study from Perkins, NE in 1900 which counts the number of months owners took to pay their property tax bill. (6) uses an archival case study from Morrill, NE in 1912 and computes the fraction of owners in a section who ran for county and sub-county elected office.

Table A.8: Property Rights and Legal Disputes

	(1) asinh(Value) Baseline	(2) asinh(Value) No NPRR	(3) asinh(Value) No NPRR, OCRR	(4) Recorded Lawsuit (%)
RR Effect	-0.045***	-0.049***	-0.047***	-2.63
	(0.014)	(0.014)	(0.014)	(5.54)
Grant $\times$ State FEs	Y	Y	Y	Y
County FEs	Y	Y	Y	Y
Township FEs	Y	Y	Y	Y
Geo Controls	Y	Y	Y	Y
SEs / Clusters	Spatial	Spatial	Spatial	Township
N	132,463	70,210	68,788	204
$\mathbb{E}[y]$	\$2,231k	\$3,003k	\$2,732k	28%

Notes: The table shows even/odd comparisons per equation (1). (1)-(3) replicate Table 3, Panel A. (2) drops the Northern Pacific Railroad grant and (3) additionally drops the Oregon and California Railroad grant. (4) analyzes the Banner County sales data with the outcome being the fraction of land in a section that ever experienced a lawsuit (*lis pendens* notice) during the period.