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

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Abstract

Despite their popularization as bastions of pioneer equality, America's frontier regions often exhibited highly concentrated patterns of land ownership. A patchwork of policies opened some areas to large-scale farming by absentee landlords but reserved others for settlement by small farmers. This paper studies the impacts of land concentration on the long-run development of the frontier United States using quasi-random variation in these allocation procedures. I collect a large database of modern property tax valuations and show that historical land concentration had persistent effects over a span of 150 years: lowering investment by 23%, overall property value by 4.4%, and population by 8%. I argue that landlords' use of sharecropping raised the costs of investment, a static inefficiency that persisted due to land market frictions. I find little evidence for other explanations, including elite capture of political systems. I use my empirical estimates to evaluate counterfactual policies, applying recent advances in combinatorial optimization to show that an optimal property rights allocation would have increased my sample's agricultural land values by $28 billion (4.8%) in 2017.

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1 Introduction

Land ownership is significantly concentrated in many countries[1] and economists have long debated the consequences for economic development. John Stuart Mill held a negative view of the landlords who owned most large farms, writing that they "grow richer, as it were in their sleep, without working, risking, or economizing" (Mill 1848). On the other hand, the eighteenth-century English agronomist Arthur Young observed that in France "[i]n all the modes of occupying the land, the great evil is the smallness of farms" (Young 1792). A third perspective came from Adam Smith, who believed markets could mitigate the negative effects of concentration: "[landlords]... are led by an invisible hand to make nearly the same distribution of the necessaries of life, which would have been made, had the earth been divided into equal portions" (Smith 1759).

These three historical perspectives remain relevant for a number of modern economic ideas. On the one hand, agricultural landlords face potential inefficiencies due to the weak incentives of their workers (Banerjee et al. 2002; Markevich and Zhuravskaya 2018) or from their own rent-seeking behavior (Acemoglu and Wolitzky 2011). On the other hand, concentrated ownership could enable the benefits of scale economies documented in historical studies (Allen 1988; Hornbeck and Naidu 2014; Olmstead and Rhode 2001) and modern comparisons (Paul et al. 2004). Finally, the Coase Theorem (Coase 1960) holds that under ideal conditions, productive assets will be efficiently distributed regardless of initial allocations. In practice, such results may depend on well-functioning markets and the enforceability of property rights (Bleakley and Ferrie 2014; Goldstein et al. 2018; Hornbeck 2010; Jones et al. 2019). It is thus unclear whether higher levels of land concentration should be beneficial and for how long any such effects should persist.

In this paper, I quantify the long-run effects of land concentration on economic development in the American West. Although the 1862 Homestead Act made small-scale farming the default settlement pattern in most frontier regions, I exploit a late-1800s policy known as railroad land grants, which opened specific parcels to large-scale ownership by wealthy landlords. The policy was applied in a "checkerboard" pattern, arbitrarily allowing concentration in every other square mile in large parts of the country.

[1]National Gini coefficients for land ownership in 1990 average 62 according to FAO (1990). The distribution is substantially more unequal than income which averages to 37 according to World Bank (2019).

I find that historical land concentration lowered economic development today, as measured by assessed land values. Most frontier communities were founded at the time of the policy's enactment, and my setting thus presents a rare opportunity to study the impact of initial conditions on long-run outcomes. I show that the negative development effects stem from large landlords' practice of tenant farming, particularly sharecropping, which blunted the incentives for investment. I find little evidence for any beneficial effects of scale economies in agriculture, since only in the narrow group of the lowest-quality lands does historical concentration increase assessed values. Finally, the persistence of the effects over roughly 150 years indicates that land markets fell short of the Coasian ideal, even in the financially-advanced United States.

The railroad land grant policy was an anomaly in an environment where lawmakers were increasingly concerned about high levels of land "monopolization" in frontier areas. To jump-start the construction of railroads without the difficulty of tax collection, the US government paid many railroad companies with land rather than cash. These companies typically offered their holdings for sale immediately and were exempt the provisions of the Homestead Act which reserved land for small-scale settlement. As a consequence, ownership patterns became especially concentrated in railroad areas relative to their federally-administered neighbors. In selecting these grant lands, the government relied on the principle of equal division between the railroad companies and itself. It achieved this goal by allocating alternate square miles to railroad companies, creating a pattern similar to the red and black squares alternating on a checkerboard. Essentially, neighboring square miles of otherwise identical land were subject to significantly different settlement schemes. The grant checkerboards were also drawn with arbitrary borders, stopping after a fixed distance from the railroad track. This feature of the policy allows me to use a regression discontinuity design to study the policy's spatial spillover effects on nearby federal properties.

I assemble new data sources that measure farm investment and productivity at the micro-scale, allowing me to fully exploit the natural experiments inherent to the checkerboard formula. My data cover six states in the modern period and a subset of these areas in the early 1900s. The more extensive modern data cover roughly 12 million properties in 380,000 square miles, collectively worth about $2.7 trillion in 2017. I also assemble georeferenced historical data on farms, schools, and property ownership. These data allow me to trace the land grant policy's effects back in time,

and I generally find larger impacts in historical periods. Turning to political outcomes, I measure interactions between landowners and the state by recording taxpaying behavior and office-seeking for a small sample in the 1900s.

My estimates show that land concentration led to fewer and less developed farms. Comparing neighboring squares on the checkerboard, I document a static inefficiency in which concentrated lands were used less intensively both in terms of the amount of investment received and available resident labor. Despite the passage of roughly 150 years since the policy's enactment, these basic effects remain in place. Today, areas subject to high historical concentration have about 4.4% lower assessed worth, 23% less investment according to my preferred measure, and 8% lower population. Considering land use as another form of an investment, I show that less land is cleared for crops and more is used for grazing. Using the regression discontinuity design, I show that these effects spill over into nearby areas not initially subject to concentration. Today, non-railroad areas adjacent to railroad lands have 14% less investment and 11% lower total value. Not only did markets fail to equilibrate differences in statistically identical lands, but some resales actually increased the discrepancies as landlords purchased the farms of nearby settlers. These results suggest that significant dynamic inefficiencies existed even in the advanced land markets of the United States.

Turning to mechanisms, I demonstrate that the negative effects of concentration on land value are driven by the inefficiency of sharecrop farming on output and investment. The larger owners who purchased railroad grant lands were typically unable to work their properties themselves, and, consequently, rates of tenant farming and absentee ownership rose in railroad lands and neighboring federal ones. The negative effects I find on land values primarily occur in places with high rates of sharecropping relative to other kinds of tenancy, suggesting that sharecropping's low-powered incentives drive the results. This explanation is consistent with historical literature which states that tenant farming was less efficient for long-run development as it discouraged investment and led to the allocation of land for low-investment, low-yield activities (Gates 1942). I find little evidence for other mechanisms, including the elite capture of political systems. While some studies find that landed elites are particularly effective at political capture (Acemoglu et al. 2019; Galor et al. 2009; Rajan and Ramcharan 2011), if anything the opposite holds true in my case. Compared with their neighbors, owners of the railroad sections had fewer public goods on their lands, paid

their taxes more promptly, and were marginally less likely to run for political office.

I use my empirical results to determine the optimal historical land policy under several scenarios. The spillover effects of the railroad land grant policy generate interaction effects between the individual parcel allocations, ultimately creating a binary quadratic optimization problem. These problems are in general NP-hard (Pardalos and Vavasis 1991), but state-of-the-art algorithms from the IBM CPLEX software render my case numerically tractable. I find that the optimal policy would have improved my sample's land values by about $28 billion (4.8%). The majority of these gains could have been realized without compromising US industrial policy by lowering the compensation of railroad companies. By granting the companies land with low returns to investment and retaining the best areas for homesteaders, the government could have avoided most of the negative effects of the land grant policy. Thus, railroads ideally would have exchanged high-productivity lands for more of the rugged plains of the American West.

This paper contributes to several strands of economics literature. First, there is a long tradition studying the impact of large-scale farming on historical development. Studies of government-led land and tenancy reforms form a large part of this work, though the impacts appear heterogeneous. Some reforms have increased output by strengthening the incentives of tenant farmers (Banerjee et al. 2002; Jeon and Kim 2000; Markevich and Zhuravskaya 2018; Shaban 1987) although other reforms have had mixed (Besley and Burgess 2000; Montero 2018) or even negative effects (Adamopoulos and Restuccia 2019). Similar heterogeneity can be found in the modern relationship between farm size and productivity. For the very smallest farms in developing countries, most research has found diseconomies of scale,[2] though the relationship is positive for the United States (Paul et al. 2004) and not universally acknowledged as causal (Benjamin 1995; Bhalla and Roy 1988). Other studies have emphasized the importance of scale economies generally and mechanization in particular as sources of productivity growth in agriculture (Allen 1988; Hornbeck and Naidu 2014; Olmstead and Rhode 2001). This paper contributes to these literatures by providing causally-identified evidence on the long-run impact of land concentration through its potential to foster both tenant farming and scale economies. In my setting, the inefficiencies of tenant farming discouraged investment by landlords and I find few discernible effects from scale economies or

[2]See Foster and Rosenzweig (2017) for an overview.

mechanization. Instead, large landlords tend to focus on lower-intensity, lower-yield activities like cattle ranching.

Secondly, the long-lasting effects documented in this paper join a large literature on path dependence in economic development both within the American frontier (Bazzi et al. 2017; Mattheis and Raz 2019) and more broadly (Acemoglu et al. 2001; Banerjee and Iyer 2005; Bleakley and Lin 2012; Dell 2010; Michalopoulos and Papaioannou 2013; Nunn 2009). Within this literature, my study is closest to Bleakley and Ferrie (2014), which documents the persistence of landholding patterns over a long period in the Georgia frontier. My work both builds upon theirs by focusing on micro-level data and contrasts with it by considering a policy that qualitatively changed owners from owner-operators to landlords rather than shifting farm sizes from an optimum. In the Georgia openings, regulations arbitrarily changed the grid system which mapped out land parcels. Consequently, parcel sizes were often misaligned with land quality as the effects of the demarcation took time to undo. In contrast, I explore variation within a fixed survey system that enabled potentially unlimited accumulation of land within some areas but not others. The effects I find on productivity do not stem from demarcation-driven constraints to reallocating land, but rather from the effects of an initial allocation within a given system. The increased concentration resulting from my variation further led to qualitative changes in ownership styles, in particular a rise in tenant farming. A small literature has also investigated the effect of legal and regulatory issues of the checkerboard pattern (Alston and Smith 2019; Kunce et al. 2002). The former study focuses on the economic impact of legal disputes that beset the Northern Pacific Railway lands in Montana; the latter study details the impacts of environmental regulation in oil well drilling on the Union Pacific grant areas of Wyoming. My paper, in contrast, evaluates the land grant policy broadly, studying a large number of grants and their subsequent impact on land concentration.

I structure the rest of the paper as follows. Section 2 discusses the historical background of American land policy and railroad grants. Section 3 presents a conceptual framework explaining the long-term effects of initial land allocations. Section 4 describes my data sources. Section 5 presents my main results on land values and tenancy, and Section 6 considers alternate mechanisms. Section 7 explores counterfactual land policies and Section 8 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. Territorial expansion characterized early American history, most notably with the addition of the Northwest Territories in 1783, the 1803 Louisiana Purchase, and the acquisition of Texas and parts of Mexico in 1845-48. Because these areas were largely unoccupied by its own citizens, the federal government had great latitude in crafting a national system organizing these lands. The result was the remarkably regular "Public Lands Survey System" (PLSS) which divided the new areas into an essentially square grid. The grid's main units were six-by-six mile squares called "townships," further subdivided into 36 "sections" of one square mile (640 acres). Each section was identified by a number 1 through 36 which corresponded to its location within a township. Figure 1 shows an example of this division, depicting several Nebraska townships with their numbered sections. The PLSS was widely applied in the United States with every state outside the original colonies, Texas, Kentucky, Tennessee, and Hawaii demarcated with its latticework.

By the 1860s, the default American land policy was structured to benefit small farmers. Initially, open land had only been available for sale, a system that made it difficult for people of modest means to participate in the settlement process. The standard price for federal land was $1.25 per acre and rarely varied as a function of land quality. The price was high enough to exclude the poor and credit-constrained, but it essentially subsidized the purchase of high-quality lands by wealthy buyers with access to capital. As such, "speculators" dominated the proceedings, buying up swathes of land in the hopes of reselling or contracting with tenant farmers. (Gates 1936). Though reforms to address this inequity were widely discussed, Southern policymakers were uncomfortable promoting a system of free labor and for a time they successfully opposed major changes (Goodman 1993). The onset of the Civil War, however, brought about a new political environment that enabled a drastic policy change. In 1862, Union states were thus able to pass the first Homestead Act, a law that would have wide-ranging consequences in the coming years.

Two provisions of the Homestead Act are important here. First, the law offered farmers a

"quarter section" (160 acres)[3] of land for a nominal filing fee if they agreed to settle on it for five years. Although nominally settlers were supposed to make improvements to their plots to acquire a title, this largely amounted to building a home and making a "good faith" effort to work the land, particularly since many land office inspectors found it difficult to perform detailed inspections of the vast areas in their charge (Bradsher 2012; National Archives and Records Administration 2019).[4] Second, individuals were prohibited from acquiring more than those 160 acres from the government. These changes greatly altered the American settlement process. Poor farmers without access to cash could now make a new start with land in the West. Wealthy buyers meanwhile could no longer obtain large properties in Homestead Act areas without engaging in complicated acts of fraud. Quantitatively, about 1.6 million settlers ultimately received homesteads amounting to 270 million acres of farmland although the exact numbers are disputed (Edwards 2008). Numerous other laws and programs relaxed the 160 acre cap in some areas, most notably subsequent Homestead Acts which aimed to promote settlement of low-quality land. However, by and large the federal government's transfer policies favored small owner-operator farming.

Despite the successes of its land policy in encouraging settlement, the US government carved out a number of exceptions that allowed for farms of unlimited size. Arguably the most important of these, and the focus of this paper, is that of railroad land grants. In a policy found almost exclusively in North America, US federal and state governments paid for railroads not just with money but also with unsettled land. Two factors made the land grant policy attractive to lawmakers. First, without the collection of the modern-day income or payroll taxes, the US government was relatively rich in land but poor in cash. Second, the government hoped that the land grants would give railroad companies an incentive to produce a high quality product. The more effectively railroads functioned, the more valuable the companies' lands although the relevance of this effect is disputed by historians (Rae 1952). In my sample, the earliest railroad grant was enacted in 1862, reserving Nebraska land for the Union Pacific railroad company to fund the First Transcontinental Railroad.

[3]Several other laws modified this exact quantity in some places, although the 160 acre limit was binding on many settlers. Modifications included the 1873 Timber Culture Act which allowed settlers additional land if they attempted reforestation as well as less famous Homestead Acts which increased the limit on unsettled lands a generation later.

[4]My data, depicted in Appendix Figure A1, show that around 85% of entirely unimproved land in 1912 was received for free by settlers under the Homestead Act and subsequent amendments. Even this estimate represents a lower bound as the remaining 15% could potentially have been received for free if settlers had so chosen. For instance, the Homestead Act allowed settlers to purchase land on which they lived after only a six-month, rather than five-year, period. Hence, the land improvement requirement was essentially a nominal one.

In practice, however, few land sales occurred before 1880 when settlement began in earnest.

The railroad grant exceptions to the default land policy were determined formulaically and form the natural experiment at the core of this paper. Railroad companies were awarded land near the tracks they built, but governments were reluctant to give away too much. Land nearby railroads would, after all, become the most populated and valuable, and governments wanted to retain some of it for their own policy goals. They thus settled on a formula which in principle gave railroads "every other" section (square mile) of land, ensuring that each group would retain a comparable area. This arrangement proved remarkably popular and railroad grants soon covered substantial parts of the United States as shown in Figure 2. The picture shows all the areas where railroad companies received land, although it elides differences in how much companies received in practice. In some places, railroad companies did indeed receive the fifty percent envisioned by the formula. In others, land was already in private hands meaning that the companies received nothing. Nonetheless, the amount of land transferred to railroads was enormous by any measure, with one estimate suggesting that 170 million acres or 9% of the continental US were ultimately given over to various companies (Decker 1964). Section 2.2 discusses grant formulas in more detail.

Railroad land was much more likely to become part of a large property than federal land. Although sales practices differed somewhat between companies, none imposed any of the quantity restrictions in the Homestead Acts. Indeed, as track construction was typically financed with large loans, companies were eager to recoup their costs and freely sold large blocks to individual buyers. While some people of modest means obtained railroad land in small quantities, the nearly-free federal lands were a much better option for this group. In his discussion of Kansas's settlement, Shortridge (1995) notes that immigrants from poor regions were more likely to settle outside the railroad grant boundary lines, largely due to issues of finance. As a result of the unrestricted sale size and exclusion of the poor, railroad lands typically became concentrated, a general pattern that can be seen in the details of specific historical accounts. For example, the majority of Sherman County, Nebraska was part of a railroad grant and early in its history a number of large "ranches" were created on the basis of railroad land purchases (Owens 1952). In contrast, neighboring Custer County had only a small fraction of its land given to railroads. Although in the 1870s most of the county was used for wideranging cattle grazing, by the 1880s "homesteaders had begun to arrive in

great numbers... The cattlemen saw a portion of their rangeland disappear with the arrival of each new homesteader, and vigorously opposed settlement." Ultimately "the ranchers were... driven out [these] migrations" and were replaced with the latter's smaller pastures and crop farms (Custer County 2019).

The federal government's incomplete application of the Homestead Act's provisions naturally led to high levels of land concentration and thus tenant farming in some areas. While the frontier is sometimes depicted as having been settled solely by small farmers, in fact a range of ownership structures prevailed. Indeed, Gates (1945) notes that "[t]he swift rise of tenancy is one of the most striking features of the history of the American prairies. Careful observers had no occasion to be shocked in 1880 at the publication of the first census statistics showing this rise for tenancy dated almost from the beginning of white settlement. A government land policy that permitted large-scale purchasing by speculators bears its responsibility for this early appearance and rapid growth."

Many American historians held a negative view of land concentration and the activities of landlords or "speculators."[5] Summarizing his views, Gates (1942) states that "speculator ownership and tenancy did not always result in the best use of the land. It has already been seen that speculator ownership forced widespread dispersion of population and placed heavy tax burdens upon farmers whose improved lands could be more heavily assessed than the speculators' unimproved lands." In seeking to maximize profits on their holdings, frontier landlords attempted to shift as much of the costs of development as possible onto their tenants. As such, the arrangement often discouraged investment. Gates (1945) profiles a number of frontier landlords to support this view. A particularly infamous one "purchased 160,000 acres which he... rented to tenants... He refused to make improvements upon his land himself... The result, of course, was that the buildings and fences were wretchedly poor and [his] lands came to be considered the 'most forlorn-looking estate in Illinois.'" Difficulties in determining who should make investments thus led to underdeveloped properties at the expense of both tenant and landlord alike.

[5]Earlier historical terminology frequently referred to large-scale landowners in the American West as "speculators." However, these owners need not have had transitory control over their properties. Gates (1941), for instance, relates that some "began their operations at the outset with the intention 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."

Other historians have seen frontier landlords as an even more pernicious force. They depict tenants who faced unstable and potentially coercive arrangements and a largely rent-seeking group of landlord-speculators. Stewart (1964) writes "a... disturbing consequence of land monopolization was the rapid creation of a tenancy class. The speculator, not content to hold vacant lands, had to actively seek buyers... A year or two of poor crops and the credit buyer became a tenant. The debilitating problem of landlordism in Nebraska was intimately linked to [speculation]." Such situations could also lead to underinvestment as a landlord might find it more profitable to engage in coercion to increase their rents rather than develop their property.

2.2 The Railroad Land Grant Formula

The formula determining railroad companies' lands was arbitrary in two ways. The first is that grant areas were typically determined based on sharp cutoffs with companies receiving a fraction of land within a fixed distance of the track they constructed. For instance, the Union Pacific Railroad company was allotted land within twenty miles of its Nebraska line. The second arbitrary feature is that, within a grant area, railroads were only allotted odd-numbered sections. That is, railroads received sections 1, 3, 5, 7, 9, ... , 33, 35 if those parcels were unowned. Visually, this formula led to what is known as the "checkerboard" pattern, shown in Figure 3. Although visually peculiar, the checkerboard pattern met the federal government's goal of maintaining a comparable set of lands for its own disposal and was quite simple to specify. Compliance in most areas was generally high although not perfect. Settlers who preceded the railroads were allowed to keep their claims, even if they were on odd sections. Additionally, the federal government was able to transfer most although not all of the remaining land. Exceptions could occur when the land was of low quality — as was true in some stretches of Montana and Wyoming — or part of unfarmable terrain like mountains or water bodies.

Notably, both the lands railroads received and the location of the grant area generally were arbitrary. Since they were determined solely by cartographic procedure, the odd-numbered sections should not have differed from the even-numbered ones. Similarly, because most grant area borders were set by formula, land on either side should have been broadly similar. Section 5.1 discusses my econometric strategy in more detail, transforming these qualitative insights into econometric

specifications.

3 Conceptual Framework

In this section I consider a simple model describing how historical concentration affects land investment over the long-run. Land can be owned either by small, owner-operator farmers or landlords who contract sharecropping[6] tenants. Building upon work such as Banerjee et al. (2002), the model shows that frictions in crop share agreements will discourage investment; this dynamic parallels micro-level evidence that sharecropping reduces short-run productivity (Burchardi et al. 2018). Transaction costs in land resale markets turn this static inefficiency into a dynamic one, meaning that historical allocations have persistent impacts over time. The model provides insight on why nineteenth-century government policies favoring landlords have depressed land values today.

3.1 Static Problem

The world consists of a parcel of land, its owner, and its operator who may or may not be the same agent. The owner and operator work the land for one period after which the owner sells the parcel and both retire and exit the model. Agricultural output is determined by the effort of the operator, investments made by the owner, and some amount of luck. In a "good" or "high" state of the world, output is equal to Y_H. In a "bad" or "low" state of the world, the land produces no output. Land may either be either unimproved ($I = 0$) and or improved ($I = 1$), reflecting investments to it. Improved land increases high-state productivity: $Y_H = A \geq 1$ for improved land and $Y_H = 1$ for unimproved land. If the land is unimproved, the owner may upgrade it to improved land for a cost $r > 0$. The probability of high-state output is equal to e, representing the operator's effort. Effort is costly to the operator, however, and reduces utility by a monetary-equivalent of $\frac{c}{2}e^2$.

If the owner and operator are different agents, they must agree to an output-sharing contract. Effort is non-contractible and so payments must be made only based on the realization of a high or low state, respectively h or l. Any operator has an outside option of 0 and the payments must satisfy a limited liability constraint of $h, l \geq 0$. The timing of the game is as follows: the owner chooses

[6] Although the term "sharecropping" can evoke the agricultural system of the Postbellum US South, here I use its technical meaning here to refer to a system of farming characterized by crop share agreements.

whether to invest, non-operator owners offer a take-it-or-leave-it contract specifying h and l, the operator chooses a level of effort, agricultural output is realized and any contracts implemented, and the owner sells the land. All agents are risk-neutral and investments are sold along with the land so the only relevant prices are those for unimproved and improved land, p_0 and p_1 respectively.

It is easy to show that the optimal contract is given by $l^* = 0$ and $h^* = \frac{1}{2}Y_H$ — the landlord and the tenant split the output evenly. This induces effort $e^* = \frac{Y_H}{2c}$ as opposed to $e^* = \frac{Y_H}{c}$ in the case of a single owner-operator. Thus a non-operator landowner would upgrade unimproved land if:

$$\frac{1}{4c}\left(A^2 - 1\right) > r - (p_1 - p_0) \tag{1}$$

In contrast, an owner-operator internalizes all of the costs and benefits of effort and investment. Lacking the contracting frictions of a non-operator landlord, they are thus more likely to upgrade land, choosing to do so if:

$$\frac{1}{2c}\left(A^2 - 1\right) > r - (p_1 - p_0) \tag{2}$$

3.2 Dynamic Problem

In this part of the model, I describe the dynamic aspects of the world including capital depreciation and land resale. After the landowner's investments and output are realized, two types of agents may purchase the land for future use. S-type agents are small farmers who act as both owner and operator, B-type agents are big landowners who require the use of tenants to work their land. Denote by O_t the owner type at time t. Due to market frictions, S-type agents face a monetary-equivalent transaction cost of purchasing the land equal to $f_t \sim F$.[7] Once this cost is revealed, many agents of each type bid for the parcel and the agent with the highest valuation pays the owner and buys the land. In $t = 0$, the land is initially unimproved and O_0 is determined by an exogenous government policy to be either S or B.

Between period t when the land is sold and period $t + 1$ when the new owner uses the land,

[7]Nothing substantive about the model changes if both agents face market transaction costs.

two events occur. First, if the land was improved, it faces a δ chance of depreciating to being unimproved. Second, productivity shocks determine the effort cost of the operator as $c_t \sim C$. Then, the owner and operator choices proceed as in the static problem. Finally, all agents discount the future at a common rate β. This leads to a characterization of equilibrium prices as shown in Appendix Section A.1. Under very general conditions described in Appendix Section A.2, the following results hold:

Proposition 3.1. *Define p_t as the price of the parcel at time t. Then, $\mathbb{E}[p_t|O_0 = B] \leq \mathbb{E}[p_t|O_0 = S]$*

This result establishes the importance of the initial owner. In my historical setting, landlord ownership induced by railroad land grants creates long-term inefficiencies. The intuition is that land investments follow a Markovian process: landlords are less likely to invest initially which lowers the probability of efficient ownership by owner-operators in the future as these agents value improved land more highly. Thus, low investment is persistent although its effects can diminish over time. Appendix Figure A2 shows land values over time for one numerical example: parcels with historical landlord ownership are perpetually lower in value but the differences slowly converge.

Proposition 3.2. *If $f_t = 0$ and resale is allowed at $t = 0$, $\mathbb{E}[p_t|O_0 = B] = \mathbb{E}[p_t|O_0 = S]$*

This result is essentially the Coase Theorem restated for my model. If resale is allowed by the initial owners and there are no market frictions, then there are no inefficiencies. The efficient owner-operators immediately purchase the land regardless of the initial owners type and there are no differences in expected future investments or land values. Thus, any gaps in these quantities for otherwise identical parcels is an indication both of the B-type's inefficiency and of market failure. The model's dynamics match those in my empirical results wherein areas with initial landlord ownership both have lower total value and lower investment historically and today.

Proposition 3.3. *Denote Y_t, e_t, r_t as owner revenue, effort, and investment costs at time t. Denote by ϕ_t the realized transaction costs f_t of the buyer. Define $\pi_{t'} = Y_t - \frac{c_t}{2}e^2 - r_t - \phi_t$, i.e. the expected owner output net of costs. Then, $p_t = \mathbb{E}_t\left[\sum_{t'=t}^{\infty} \beta^{t'-t}\pi_{t'}\right]$*

This result states that land prices reflect current and future-discounted output net of all costs, a standard result in land valuations (Borchers et al. 2014). The intuition for this result is that although there are transaction costs, otherwise bidding operates competitively and so prices

will take into account the buyer's expected profit and, iteratively, the expectation of future buyers' profits. I apply this result when evaluating counterfactual land policies in Section 7. An alternate formulation will also account for the sunk costs of historical investments, a lower bound for the total welfare loss.[8]

4 Data

To assess the impact of railroad land grants, I assemble data on a number of economic and political outcomes in several US states: Florida, Kansas, Montana, Nebraska, Oregon, and Wyoming. The majority of outcomes are at the section (square mile) level, although some key data are only available at the county level. I limit the discussion here to data sources and discuss boundary and sample construction procedures in Appendix Section B.3.

The data I collect are a mixture of a geographically broad cross-sections and several geographically narrow panels. Many modern-day outcomes such as property tax assessments can be collected for a large area and I thus observe property characteristics for the majority of the relevant portions of my states. Historical data is more difficult to collect, largely due to availability and the costs of collection and digitization. I nonetheless am able to collect historical outcomes such as land sales and ownership for individual counties or states.

4.1 Land Grant Boundaries

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. 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 have more complex boundaries. A few, for example, received land within different fixed differences at different points along their track.[9] Others contain areas included or

[8] Because the problem does not differ across time periods, in expectation the value of an investment over a fixed period of time must be positive.

[9] For example, the Atchison, Topeka and Santa Fe railroad received Kansas land within ten miles of the western portion of its track and received land within twenty miles of its eastern track.

excluded based on non-formulaic considerations. In these cases, I use a mix of historical maps, court records, and Bureau of Land Management General Land Office (BLM GLO) transfer records to determine the boundaries of the grant. For more details, see Appendix Section B.3.

4.2 Property Tax Assessments

To measure economic outcomes, I assemble a large, disaggregated database of property tax assessments. Property tax assessors aim to evaluate the total worth of a particular plot ("parcel") of land and the buildings or improvements on it for purposes of taxation. These data offer a number of advantages over alternative measures of economic development. Primarily, they are available at a very spatially disaggregated level appropriate for leveraging the railroad land grants variation. They also offer much richer information than, for example, the "night-time lights" data in which the vast majority of rural pixels are entirely dark (Bergs et al. 2018).

In their calculations, assessors either attempt to find comparable properties recently sold or estimate the net income of the property based on known characteristics. The latter case is most common for agricultural properties, with assessors estimating net income for the property based on its natural characteristics like soil quality and some human-determined characteristics like the type of land use. Thus, a cattle ranch will have its land evaluated based on the assessor's belief about the net income from cattle, even if the assessor believes wheat farming would be more profitable. The estimated income is capitalized to a net present value using a statewide capitalization rate, typically around 5%.

One of my main outcome variables is the total assessed property valuation on the PLSS section, roughly square mile, level. To calculate this value, I sum the assessed property valuations for each parcel contained within the section. The vast majority of the area in my sample is farms and total value thus matches the USDA's definition of farm real estate value (USDA 2018).[10] In a small number of cases discussed in Appendix Section B.3, parcels are spread out over more than one section. I split these parcels' valuations across sections in proportion to their common area, effectively assuming a constant density of value per square mile. Finally, in eleven counties in my sample, a large fraction of government-owned properties are missing assessment data due

[10] Appendix Section F.1 shows that towns and urban areas do not affect the results.

to their tax-exempt status.[11] In these counties, I replace the assessor's valuation with use-based figures from satellite-derived data. Based on crop prices and local productivity information, I can estimate profitability and thus valuations as I describe in Appendix Section B.5.

In addition to total value, other property characteristics are often recorded. Assessors commonly compute figures for the value of buildings and improvements on a property separately from the value of the land itself. They also frequently record data on land use, classifying the property as residential or agricultural, and noting how many total acres used for farming. Most also document the name and address of the property's owner. In the modern era, assessment is done in GIS form, allowing for very detailed information on property size and location. Some of these outcomes, notably land use metrics, are affected by the unassessed missing data issue. In these cases I either opt to use satellite-derived data as my main outcome or to drop counties with any unassessed, exempt properties as described above. I apply the latter approach to measuring grazing as satellite data cannot typically determine whether grassland is actually used to feed animals or is left undisturbed. I apply the former approach to crop farms although the latter yields similar results.

The major limiting factor in property data collection is availability. Property taxes are usually computed at the county level, although a few states have made efforts to construct a comprehensive database. I focus my data search on states where (a) railroad companies were granted land (b) property data were accessible and affordable, usually because of the existence of a statewide database. Ultimately, I collected data from a large majority of counties in the states of Florida, Kansas, Montana, Nebraska, and Wyoming. I additionally collected a small sample of property tax data from counties in Oregon. A map of covered railroad land grants and counties with sufficiently complete property data to be included in the sample is shown in Figure 4. For details on the exact sources and data procedures, see Appendix Section B.1.

[11] Specifically Montana counties Flathead, Lincoln, McCone, Missoula, Prairie, and Sanders and Wyoming counties Carbon, Laramie, Lincoln, Sweetwater, and Uinta. I identified these counties through discussions with the Montana and Wyoming departments of revenue as well as county assessors. Other counties either lack such properties or report valuations regardless of exempt status.

4.3 Land Transfer Records

In order to directly measure land concentration and sale volume, I collect information on the identity of landowners. I explore two data sources that can provide information on this topic. First, the Bureau of Land Management, through its General Land Office (GLO) records, maintains a database of federal land transfers. The federal government typically functioned as the first point of sale for the majority of non-grant land and thus these records essentially record the first purchaser but can shed no light on subsequent ones. The records contain a legal description of the land based on the PLSS (see Section 2.1), the name of the buyer, the year of transfer, the law under which the land was obtained (e.g. Homestead Act), and the total acreage of the transfer.

I supplement the BLM's records of federal transfers with archival work on railroad company transfers in Lincoln County, Nebraska. To the best of my knowledge, no comprehensive database of railroad transfers exists. Lincoln County preserved and scanned most although likely not all such records and made them available online through its Register of Deeds. These features made Lincoln the most accessible county in my search for such data, in addition to having one of the largest total areas granted to railroad companies. While it would be possible to collect similar records from other counties, the cost would have been much higher and would not substantially change the data's scope.

To analyze impacts and land sales and ownership over time, I make use of historical data recorded by county registers of deeds. For small plots of land, typically one-sixteenth section or 40 acres, the register of deeds records each transfer including the previous owner, the new owner, and the date on which the sale occurred. I obtained records for several counties from the "Nebraska Deeds Online" (NDO) website and digitized a subset of the records for Banner County, Nebraska. 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 tax records from Perkins County, Nebraska, and the 1912 assessors' records from Morrill County, Nebraska. I selected these counties based on data quality, availability, and their possession of substantial portions of land inside and outside railroad grant areas.

For some results, it is useful to link the purchasers described in these records to the named

microdata in the 1900 US Census. The linking procedure in described in more detail in Appendix Section B.4.

4.4 Population and Historical Public Goods

For modern population values, I obtain census-block level population data from the 2000 US Census. These units are fairly small and are often present at the section (square mile) level or smaller. For some states, particularly Kansas and Nebraska, the blocks were drawn with the PLSS grid in mind as shown in Appendix Figure A3. However, in other states this is not as much the case.

I intersect each Census block with the PLSS grid and allocate its total population in proportion to the area intersected. That is, I assume a constant population density in each block: a 60-person block with two thirds of its area in Section A and one third in Section B would allot 40 people to Section A and 20 to Section B. This procedure necessarily attenuates any results as the exact location of people within a block is unknown.

For historical population values, I use the remarkably detailed "enumeration district" Census maps. The 1940 versions of these maps contain the location of every rural farm, school, church, and other structures. Helpfully, the PLSS grid is superimposed on these maps, making it easy to code the total number of any building type by grid square. The number of farmsteads serves as a good proxy for the rural population as almost all would have resided in farm buildings. These maps additionally contain a good proxy for public goods by displaying the number of schools, churches, and community buildings in any given area.

Records on cities and towns contain more consistent information over time. I obtain data on them from two sources. For historical populations and locations, I use (Schmidt 2018) which codes each town as a singular point. For modern data on the precise extent of towns and cities, I use the Census TIGERLINE place shapefiles from 2000.

4.5 Geographic Characteristics and Land Use

For use as controls, placebo checks, and heterogeneity analysis I obtain a variety of geographic characteristics for each PLSS section. Elevation data are from the SRTM 250 meter resolution database. A related database from the FAO contains the terrain slope characteristic, a key agricul-

tural 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. I additionally compute the total miles of rivers and streams present in a grid square using an ESRI shapefile of all water bodies in the country.

For soil quality characteristics, I use the USDA's gSSURGO database. For crop productivity, I draw upon their "nccpi2 (all)" aggregated measure of soil productivity for different crops. gSSURGO is also the source for annual forage production of land. While gSSURGO reports soil quality on areas smaller than a PLSS section, its boundaries typically do not align with the PLSS grid. To obtain soil quality by section, I thus take the area-weighted average value gSSURGO variables and impute zero productivity if an area is unmeasured.

To obtain data on land use I turn to the USDA's CropScape "Crop Data Layer" (CDL) which imputes land use in 30 meter $\times$ 30 meter pixels from satellite images. The CDL codes land use as a specific crop, grassland or pasture, or at various levels of "development" for areas with roads and buildings. I manually match these crops to those present in the FAO GAEZ data when I require information on productivity. Finally, I obtain farm gate prices from the FAO, the USDA, and other sources where necessary for specialty crops.

5 Main Results

In this section I present results showing that land concentration lowered economic development in the long run. As suggested by the historical literature, railroad land grants did increase land concentration in otherwise similar sections. The landlords in these concentrated sections initially invested less in their properties, a difference unresolved by early twentieth-century markets. This static inefficiency became a dynamic one as low investments persisted and resulted in lower land values roughly 150 years later. Moreover, concentration and the correspondingly low rates of investment spilled over onto nearby areas. I argue that these effects stem from the use of tenancy and sharecropping which blunted the incentives of both landlord and tenant. Land concentration led to a rise in tenant farming in general and, additionally, has few impacts in areas where sharecropping was uncommon for exogenous reasons of geography. Finally, I discuss how static inefficiencies from

sharecropping became a dynamic ones by presenting evidence transaction costs which led to market failure.

5.1 Initial Land Concentration and Investments

I begin my analysis by confirming that the railroad grant policy did in fact increase land concentration. Using historical sales records from Lincoln County, Nebraska,[12] I define initial land concentration as the average acreage held by owners within a particular area and plot this measure in Figure 5. Consistent with the historical literature, the land grant policy clearly increased concentration: odd (railroad) sections are part of significantly larger properties than their even (federal) neighbors. The average log acreage is 1.3 points higher, implying that odd properties are typically 3.7 times larger than their even neighbors'. Additionally, there is no evidence of manipulation either between even and odd sections or in terms of the border placement. Outside the grant area, even and odd sections were both administered by the federal government and show similar initial property sizes and there is also little change in size for federal areas across the border. If even and odd sections somehow differed in their characteristics or if the border location was manipulated, we would in contrast have expected to see differences in property sizes as settlers reacted to differential land quality or other government policies.

To formalize these comparisons into a regression framework, I leverage the arbitrariness of railroad land assignment. Within the grant area, I simply compare even and odd sections: since the even-odd distinction stemmed from surveying decisions made many years prior to the railroad land grants, there should be no unobserved average quality differences between the two groups. I therefore run regressions of the form

$$y_i = \alpha \mathrm{RR}_i + X_i\beta + \varepsilon_i \tag{3}$$

where i is a non-education PLSS section (roughly one square mile) within a grant boundary; RR_i is a dummy variable indicating whether the section should be assigned to a railroad company according to the grant formula, i.e. whether i has an odd section number; y_i is some outcome;

[12] Comparable data are not widely available; see Section 4.3 for details.

and X_i are controls. 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. As the checkerboard pattern is regular over space, my preferred specifications use Conley standard errors to allow for the possibility of spatial correlation (Conley 2010) and specify the correlation cutoff at 100 miles. For regressions that cover only several counties, there is typically not enough data to compute these errors and I instead cluster errors at the township level.

Using these regressions, I show that concentration historically created a static inefficiency, reducing the amount of investment properties received in the early twentieth century. In theory, market reallocations could have equilibrated land use differences across equivalent lands as they were obtained by their most efficient owners. In practice, my results are more consistent with the historical work of Gates (1942) who notes that landlords tended to retain their properties even while expending fewer resources to improve them. My data cover property assessors' measures of investment in several counties in the early 1900s and the location of farms in 40 Nebraska and Kansas counties in 1940. My outcomes thus capture investment and population roughly 20 to 60 years after the land grant's effective start date. Because investment and population are fat-tailed distributions which sometimes include 0, I transform them using the "inverse hyperbolic sine" (asinh) function which allows me to interpret coefficients roughly as percentage changes (Card and DellaVigna 2017). I take a similar approach for all similarly-distributed outcomes in this paper including any measure of land values.

Table 1 performs the direct even-odd comparison and finds that land concentration led to fewer and less developed farms. Columns (1)-(2) consider 1912 tax data from Morrill County, Nebraska. The assessors' valuation of investment ("improvements") was 77% lower in railroad sections and roughly 10 percentage points less of the land is improved. Column (3) reports data from Lincoln County's 1965 property assessment; railroad sections report 26% less farm equipment. Finally, column (4) reports effects on the (asinh) number of farmsteads in each section in multiple Kansas and Nebraska counties in 1940. It finds that even 60 years after initial settlement, there were around 25% fewer farmsteads on railroad sections. Columns (5)-(8) of the table rerun the results but look at areas at least one mile outside the grant boundaries as a placebo check. These

results uncover no statistically significant placebo failures and the point estimates are generally small in absolute value.

My results so far show that the railroad land grant policy did increase initial levels of land concentration which, in turn, lowered rates of investment in the early 1900s. The data thus give preliminary support to theories which predict negative economic effects from concentration. However, in principle there might have been few dynamic implications if the effects dissipated over the subsequent century. I turn to this question in the next subsection where I explore impacts on modern land values and investment.

5.2 Modern Property Values

The low levels of investment in concentrated sections persisted into the twenty-first century and resulted in lower land values today. Land values are a natural outcome to consider as they reflect an area's economic productivity, particularly in the case of agricultural properties that form the vast majority of my sample. In historical times, obtaining ownership of land and improving it were the majors pathways of social mobility in an agrarian United States (Gray et al. 1923). Although land values may ignore the sunk costs of certain investments such as clearing farmland, these are likely small relative to the benefits, as I discuss in Section 7.

Table 2 performs the direct, even-odd comparison of sections within the grant area with total and finds that land values today are 4.4% lower in former railroad properties. The estimate is extremely stable across a number of specifications performed in columns (1)-(4) with the last of these containing the full control set. These results strongly favor the negative channels of land concentration, particularly the investment-driven ones. Column (5) estimates heterogeneous effects and demonstrates that concentration had few effects in areas of low soil quality, defined here as being in the bottom quintile of the gSSURGO productivity index.[13] Areas of low soil quality are typically only suitable for activities such as rangeland cattle grazing and thus require few improvements to the land. The lack of an effect in these areas therefore supports an investment-driven story.

Table 2 also performs two robustness checks. Column (6) replicates my preferred specification in column (4) but removes the counties which require satellite supplementation of assessors'

[13]Results are qualitatively similar using other definitions.

valuations. The results are stronger here, consistent with them being areas of poor land quality; the use of satellite data does not drive the results in columns (1)-(4). Finally in column (7) I rerun my main specification outside the grant boundary as a placebo. I find no statistically significant effect and the point estimate is small, confirming the validity of my empirical strategy.

Quantile effects of the even-odd comparison also reveal little upside for land concentration or potential scale economies. Figure 6 plots unconditional quantile effects on total property value in 5% intervals. Except at the very top and bottom of the distribution, the quantile effects are negative and significant. Although complex violations of rank-invariance could theoretically mask areas where economies of scale led concentration to increase land values, the consistent negativity of the quantile effects more plausibly suggest that concentration decreased values across a majority of the distribution.

My data show that low land values of historically concentrated properties reflect persistence in investment. Using the even-odd comparison, Table 3 shows that railroad lands' low-intensity farming style persists to the modern day across a number of different measures. My main measure of investment is 23% lower, as shown in column (1), and columns (2)-(4) show that the effect holds across different subcategories and functional forms. The column (1) point estimate is similar when restricted to Morrill County whose 1912 assessment data was analyzed in Table 1. Although some caution is of course warranted in comparing the two tables as assessment procedures have changed in the past century, comparing the two coefficients suggests about 70% of the historical gap has disappeared today. Land use patterns therefore do change, but only in a multigenerational process that is not complete today.

Historical land concentration also lowered modern-day population as estimated in columns (5)-(7) of Table 3. The raw comparison implies that population in 2000 is lower 3.4% lower. However, this figure is attenuated by the fact that census blocks are imperfectly aligned with PLSS sections, as depicted in Appendix Figure A3. Whenever a census block covers both an even and odd section, estimates of equation (3) are attenuated. Mathematically, the attenuation of the treatment effect will be invsersely proportional to the number of sections covered in a Census Block.[14] I thus sum the fraction of each census block contained in a section, top-coding the value

[14]For instance, if a census block perfectly covered an even and odd section, the population would be split evenly

at one.[15] I estimate equation (3) but with $RR_i \times [\text{Expected Attenuation}]_i$ in place of RR_i as the independent variable; $[\text{Expected Attenuation}]_i$ is included as a control. The coefficient thus represents the expected effect with no attenuation. Column (6) of Table 3 presents the adjusted results and finds an 8% drop in population. Restricting the sample to sections with 1940 farm data, the coefficient suggests a larger 15% loss as compared with the 26% loss in 1940 found in Table 1. Thus, the effects are still economically meaningful today but, as with investment, have attenuated since historical times.

The larger owners of railroad lands engaged in a low-intensive economic strategy not just in outlays of capital but also in their choice of how land was used. Converting open prairie to a soybean farm, for example, can be considered a form of investment. Table 4 considers such investments and finds that land concentration reduced investment in this category as well with the effects focused in areas of average or higher land quality. Columns (1)-(3) report effects on satellite- and assessor-derived land use patterns. Historical land concentration today leads to a more homogeneous pattern of use and one more focused on crops rather than grazing. The baseline effect of railroad land grants reduces the number of distinct land uses by about 0.1. The fraction of sections devoted to crops[16] declines by 1.7% but the fraction of sections with grazing seems to increase. As landlords faced barriers to investment, they turned their land to activities which required it less. As with the physical improvements, land use patterns proved persistent and led to the detectable although more modest differences shown today.

In the remaining columns I provide a holistic measure of land concentration's impact on land use. By combining satellite-derived data on land use, agronomic models of crop productivity, and price data I can create a purely use-based valuation for each section of land as described in Section B.5. A piece of land used for grazing but where crops could be more profitably grown would receive a lower valuation for instance, indicating use-based misallocation. Column (5) reports results on use values considering only agricultural products and column (6) reports results adding in values for

among them and only half the treatment effect could be imputed to the odd-numbered one.

[15] For example, if a Census block is spread evenly across two sections, the expected attenuation would be 0.5. If a section fully contains 10 blocks, as would occur with a town, we would expect no attenuation or an effect size of 100%.

[16] Defined as at least 10% of the land being devoted to crops; other cutoffs show similar results. A cutoff is a natural outcome to consider crop usage shows significant bunching near 0%. A 10% cutoff, relative to an exact 0%, minimizes the probability of small misclassifications changing the coding.

roads and other built-up areas. The effects in both columns are similar to the impacts on assessed property value although somewhat smaller in magnitude. Pure use values are about 2% to 3% lower in most areas, but in fact if anything seem to rise in lands of marginal quality. These findings thus reinforce the earlier results, implying that concentration generally lowered land investment although it increased the extensive margin of use in some places.

There were both static and dynamic efficiency consequences from the railroad land grants policy. The landlords on concentrated sections invested less in their land and worked it with fewer laborers, instead specializing in activities such as grazing. Although this strategy was effective in areas of very low land quality, it performed poorly in most areas which required more intensive investment for optimal use. There, small-scale owner-operators dominated, being more likely to raise crops and improve their land. These depressed levels of investment are consistent with models of tenancy that emphasize how sharecropping blunts economic incentives, a topic to which I investigate in Section 5.3.

5.3 Tenancy and Ownership Structure

I argue here that low investment in concentrated lands is explained by a rise in tenant farming, particularly sharecropping. Compared with small farmers, larger owners are more likely to live too far from their holdings to personally work them. Even when not absentee, their holdings may be too large for one person or even one family to operate. Thus, such owners must find an outside source of labor. Although they could theoretically hire their workers for wages, in practice "such operation has many economic disadvantages. The most important of these are the uncertainty of the labor supply... and the difficulties of directing so adequately a large labor force in an industry so ill adapted to standardization and routine." Longer-term contracts with tenants were more expedient meaning that "the concentration of land ownership in large holdings is favorable to landlordism and tenancy." The most common type of arrangement was sharecropping in which worked in exchange for a portion of the crop at harvest. This latter group was considered historically to be on the bottom rung of the agrarian social hierarchy, having insufficient cash to pay a landlord upfront and thus only receiving a partial return on their labor (Gray et al. 1923).

The historical literature emphasizes that tenant farming lowered investment. As quoted

earlier, Gates (1942) states that "tenancy... did not always result in the best use of the land" largely because landlords did not invest as much in their properties as small, owner-operator farmers. Landlords' incentives were to minimize their share of investment and tenants had similar priorities, particularly given their frequently insecure status as workers. Thus, the lower investment of landlord properties shown in the previous subsection is plausibly an unfortunate result of landlord-tenant relations.

Measuring tenancy at the property level is difficult, necessitating the use of proxy outcomes. Because owners who lived far from their properties would be unable to work their lands personally, I use an owner's proximity to their land as a measure of tenancy. For historical data from the early 1900s, I link owner names to Census microdata as described in Appendix Section B.4. If I am unable to find a match within the same county as the property itself, I conclude that the owner is absentee and the property is worked by tenants. For the modern period, I can perform a similar analysis by calculating the exact distance between an owner's address and their land. I geocode each owner's address and compute its distance to the centroid of the PLSS section which encompasses the property. This outcome requires some care in analysis, however. In areas of the frontier with low soil quality, federal and state governments own large amounts of land which was never settled. In these cases, the governmental owners are located in Washington, D.C. or state capitals and thus far away from the properties although this does not indicate a tenanted property. As such, for my baseline analysis I restrict to townships with no governmental ownership of property.[17]

Table 5 estimates effects of land concentration on my measure of tenancy. My results show that concentration indeed changed the ownership structure of properties in the railroad lands. The initial purchasers in these sections were less likely to live on farms and more likely to come from out of state, fitting the profile of absentee landlords. Subsequent owners fit these patterns as well. In my early 1900s sample, owners of railroad sections were about 8 percentage points less likely to be matched to a county resident relative to federal section owners. Unmatched owners in my historical linking procedure likely attenuate these results, suggesting that the true differences may

[17]In some areas, railroad land grants prevent land from being retained by the government and there is thus a theoretical possibility for imbalance here. However, since data are retained at the township not section level, the potential for this bias is substantially mitigated. Dropping states with large amounts of unsettled land — namely Montana and Wyoming — yields similar results. In the remaining states, there is no connection between railroad land grants and eventual government ownership. Appendix Table A2 shows these results.

have been even larger. Owners with common names would be matched to multiple people and I am thus unable to conclude that they live in their property's county. Owners whose names were recorded incorrectly, illegibly, or with different spellings than in the Census might also have no matches. Despite attenuation from such factors, the regressions still demonstrate that early land concentration led to landowners who lived further from their plots and were thus more likely engaged in tenant farming. This pattern is still detectable today, although it is small in magnitude: in 2017 railroad section owners live about 4% further from their properties.

To provide additional support for the tenancy mechanism, I consider the pattern of heterogeneity in both the owner distance and land value effects. Based on their geography, some areas would have been more or less likely to become owner-operated. In places where the tenancy effect is small, we should also expect a smaller effect on land values. To operationalize this insight, I use data from the 1940 agricultural census[18] on the fraction of each county's farms which were owner-operated. I regress this value on the county-wide averages of geographic characteristics used in my main regressions, including state fixed effects and a linear trend in latitude and longitude by state. Using geography-predicted values alleviates concerns that this quantity is endogenous since, as shown in Panel A of Table 5, railroad land grants did increase the rate of tenancy.[19] The predictions are shown in Appendix Table A3, and in general, the highest-quality lands are less likely to be owner-operated. This fact reflects the observation from Gates (1936) that wealthy landlords were frequently able to outbid or outmaneuver homesteaders to purchase the best parcels.

The patterns of heterogeneity in the results points to tenancy as the driving mechanisms of lower land values. Panel B of Table 5 breaks the main sample into areas with predicted owner-operation above 70%, the top third, and those below that threshold. In areas of very high ownership, railroad land grants do not noticeably affect my tenancy proxy and, similarly the effect on land values becomes smaller in magnitude and statistically insignificant. In areas with more moderate levels of ownership, the impact on both tenancy and land values both become stronger. As a final piece of evidence on tenancy in general, a supplementary analysis in Appendix Section D.2 considers the impact of railroad land grants at the county level and again finds that they increased rates of tenancy. Since tenancy is directly observable at the county level, this dispenses with the

[18] The first year where all my counties appear and which has all the relevant variables for the exercise.
[19] However, the results are essentially the same using the actual rather than predicted values.

need for proxy variables although it makes use of county- rather than section-level variation.

The form of tenant agreements is also a key mechanism for the land value results. The Census classifies tenants as either "cash tenants," who rent the land but receive the full the full crop, and "share tenants" or "sharecroppers" who work the land in exchange for a fraction of the harvest. Classic economic theories suggest that the latter form of contract is inefficient since neither party receives the full benefit of any investment they make. On the other hand, labor coercion or other exploitative aspects of landlord-tenant relations could make all such relationships inefficient. To test between these theories, Figure 10 plots heterogeneity in the land value results by the predicted fraction of tenants under share agreements, replicating the methodology in Panel B of Table 5. The results indicate that most of the reduction in land values comes from places where tenants were likely to form share agreements. Moreover, the estimates become roughly 0 in areas where most tenants rent the land for cash. If most of the inefficiencies stemmed from labor coercion or features of tenancy in general, we would have expected to see a relatively flat slope of effects with respect to share tenancy. Instead, share agreements with their low-powered incentives appear primarily at fault.

Taken together, these results demonstrate that land concentration reshaped ownership structure. Lands in the railroad grant areas were more likely to be held by absentee owners and thus worked by tenants. These lands, particularly the ones worked by share tenants, received less investment which lowered their value even today. Railroad land grants thus undercut the Jeffersonian ideal of independent, small-scale farmers and fostered an agrarian system defined by sharecropping and its resulting inefficiencies, many of which are still apparent in the twenty-first century.

5.4 Spillover Effects

Beyond the persistence of low investment, the dynamic effects of land concentration spilled over onto nearby federal lands. Once a settler obtained ownership of land, the Homestead Act imposed no legal restrictions on its transfer. Given that most owners would prefer contiguous properties, the checkerboard grant pattern increased the chances that landlords could acquire the plots of small farmers by placing those large owners in close proximity to smaller ones. Appendix Figure A4 confirms this intuition, showing that expansions into adjacent sections are common, far more so

than expansion at any other distance. Comparisons between even and odd sections thus illustrate differences between "treated units" and "control units in treated areas." However, the arbitrary boundaries drawn for most railroad land grants allow me to compare "pure control" sections to control sections in treated areas. Federal sections inside the railroad grant boundary would have been surrounded by larger landlord properties whereas federal sections outside the boundary would have been uniformly owned by small-scale homesteaders. I thus employ a geographic regression discontinuity (RD) design:[20]

$$y_i = \alpha[\text{NearRR}]_i + f(d_i) + X_i\beta + \varepsilon_i \tag{4}$$

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; f is a smooth function of the running variable d_i, distance to the boundary; and X_i are controls. For my baseline results, I exclude odd-numbered sections within one mile of the boundary to prevent misclassification of railroad sections caused by any inaccurate borders. I include all controls in the direct effects regressions and add boundary fixed effects for each state $\times$ grant pair. I implement f as a local linear function on either side of the cutoff and estimate it separately for each state $\times$ railroad grant pair. I use a rectangular (uniform) kernel in my weighting procedure. My preferred specifications use Conley standard errors, both for consistency with estimates of equation (3) and to account for spatial correlations inherent in this experiment. As I am unaware of any method to estimate optimal bandwidths in the case of spatially-correlated standard errors, I use a baseline bandwidth of 5 miles showing robustness.[21] I explore robustness to bandwidth, other sample selection procedures, and standard error calculations in Appendix Figure A6 and Appendix Table A7.

[20] Resales at the border itself could mean areas just at the border would be partially affected. However, since the fraction of neighboring sections owned by railroads changes rapidly at the border, the economic effects should occur rapidly as well. Appendix Figure A5 confirms this intuition, showing that the fraction of railroad sections neighboring federal ones falls rapidly downward at the boundary. Figure 11 provides some visual evidence for partial economic effects at the border itself. One design that avoids the partial treatment issue would be a donut regression discontinuity design, dropping observations within a radius of the boundary. Appendix Table A7 considers this and other alternate designs; dropping observations near the border increases the effect size.

[21] This compares to an 8.4 mile bandwidth selected by the (Calonico et al. 2014) method when clustering by county but adding no controls. Controls would typically lower the bandwidth by reducing the outcome's variance. It is unclear how spatial errors would affect the selection procedure. Overall, lower bandwidths should generally reduce bias at the potential cost of variance.

The results of the RD estimates show that land concentration spilled over onto nearby federal sections with correspondingly negative impacts on investments and property values. Table 6 presents presents my RD estimates. Column (1) tests for balance in the initial property sizes and finds no evidence of differences across the border. Since all sections in this sample were administered by the federal government and the border placement determined by arbitrary formula, we should expect no imbalance. In my early 1900s sample, however, federal sections were 24% larger within the grant boundary. Although these sections were never given to railroads, their proximity to railroad lands led to increased concentration in them. Columns (3)-(4) show that these even sections faced similar impacts from concentration as their odd-numbered neighbors with 2017 total property value and investment lower by 11% and 14% respectively. As with the even-odd comparisons, owners in the adjacent sections live further away in both today and in the 1900s. Consistent with the idea that landlords purchased nearby lands, tenant farming spread from railroad sections to their neighbors. Additionally consistent with a story of such concentration spillovers, land use is also more homogeneous in areas neighboring railroad lands today.

For my main outcome of total property values, I explore the RD's robustness to sample and bandwidth specifications. First, Figure 11 visually depicts the effect, showing that federal properties rise in value just outside of the grant boundary. Appendix Table A7 shows robustness to sample and standard error calculations. Column (1) replicates the baseline specification. Column (2) reports standard errors clustered by county. Column (3) reports a donut regression which drops all sections within 1 mile of the boundary. Column (4) includes all odd sections to the right of the boundary. All specifications yield roughly similar results and statistical significance, with the possible exception of the donut regression which yields a substantially higher point estimate. Finally, Appendix Figure A6 shows robustness to a range of bandwidth choices.

Former landlord properties are worth less than adjacent ones today, supporting the theories that land concentration decreases economic development. That these effects have persisted for 150 years in an advanced capitalist economy is notable and indicative of significant market imperfections. More striking is that some market transactions actually increased inefficiencies: federal properties adjacent to the concentrated railroad ones have lower value than their neighbors across the grant border. The most plausible explanation is that frontier land markets were highly localized

and some participants faced significant barriers in purchasing property. As in Section 3's model, small farmers might face market frictions or credit constraints from which landlords were immune, allowing the latter to purchase properties despite their lower productivity.

5.5 Land Market Failures and Persistence

The persistence of both land use and ownership patterns over 150 years suggests significant imperfections in American land markets. The Coase Theorem predicts that, under ideal conditions, market exchange should ultimately lead to assets being owned by those who can use them most productively (Coase 1960). In this setting, one corollary is that even and odd sections should on average be held by similar owners and have similar usage once the market reaches its equilibrium. However, if early settlers faced significant transaction costs in selling land, such convergences might occur only over a long or perhaps infinite span of time. I explore this question empirically, showing the evolution of land concentration and use over time and presenting evidence for one important market friction.

Figure 7 shows that differences in land concentration between even and odd sections slowly disappear over long time scales. Indeed, the duration of the initial allocation is similar to the century-long convergence time reported in Bleakley and Ferrie (2014) for a different policy on the Georgia frontier. Due to the difficulty in digitizing the vast number of sales records potentially available, I restrict myself to a case study of the southern part of Banner County, Nebraska.[22] Average property size in railroad and federal lands show a Coasian trend toward convergence, but the process was quite slow, indicating that initial differences remained relevant for many decades. In the early days of Banner's settlement, railroad land was owned in properties roughly twice the size of their federal counterparts. Much of the railroad land was purchased by a sprawling cattle corporation, the Bay State Livestock Company. In later decades, the company went bankrupt and differences in land concentration slowly began to erode. By 1940, roughly a half-century after Banner's founding, railroad section properties were only about 14% larger. Around 1950, sharp changes in ownership occurred as properties were rapidly bought up by oil companies. As this boom was relatively short-lived, ownership patterns likely experienced large shifts in the subsequent

[22]Specifically, Banner County townships of the 17 north latitude. This area was selected as all of was contained in the railroad grant lands and Banner County could potentially provide data on both sides of the grant boundary.

decade although my data do not extend that far.

As noted by Bleakley and Ferrie (2014), one substantial market friction in this setting is the difficulty in subdividing properties. Even if it were optimal for a farmer to sell, say, half their land, it would require the buyer to have detailed knowledge of the property to understand which half they should purchase. A successful sale would also require a demarcation specifying a new border between the properties. Consistent with the importance of such costs, very few Banner County properties are split as shown in Figure 8. Considering ownership in January, 1900, I record for each plot (sixteenth section) whether it has the same owner as the largest intact part of the 1900 property.[23] Only small parts of properties are split, with about 10% of even-section properties and 20% of the odd-numbered ones breaking off. Further, there seems to be little trend toward property splitting after 1920, implying that most of the initial allocations remain persistent indefinitely.

A second form of transaction cost comes from the fact that land was not only a factor of economic production but also the home of most farm operators. Owner-operators who sold their land would thus need to move, imposing a significant cost on the transaction. Unsurprisingly, housing differences tend to be remarkably persistent. Figure 9 plots the number of rural households in Merrick County, Nebraska in even and odd sections over time.[24] In 1940, even sections have 37% more households, a ratio that only changes to 33% more by 1964. Although care should be taken when comparing this rate of change to Figure 7 as the two cover different counties, it is notable that essentially no change occurred in the household ratio over 24 years, despite the rural population shrinking steadily during this time.

These trends suggest that significant transaction costs characterized frontier land markets and made reversing concentration and land use patterns a multigeneration process still incomplete today. Other discrepancies from the Coasian ideal, such as credit constraints, may have existed as

[23]For example, if a January, 1900 property consisted of five plots and four out of five were held by the same owner in January, 1920, those four would be coded with a retention value of 1 while the fifth would be coded with a retention value of 0. Figure 8 averages retention values by even and odd sections.

[24]Data from 1925 come from the Postal Service's "Rural Free Delivery" map; 1940 - 1964 come from Census Enumeration District maps; data from 2000 come from Census Block population data of rural sections. The conversion from population to households assumes 4 people per household and top-codes households at 10 per section. Unlike the earlier maps, Census Block data do not distinguish between urban and rural households. Considering only sections without towns ameliorates the problem, but Census Blocks are not always aligned with PLSS sections, as shown in Appendix Figure A3. Urban population blocks can thus spill over to other sections. I therefore top-code these data at 10 households per section, roughly the top 3% of the distribution.

well. However, in the absence of significant transaction costs, it is unlikely that these other barriers could account for such a long period of persistence. A decade of saving or the accidental presence of unconstrained owner-operators would have reversed use discrepancies more quickly. Instead, we see gaps that have lasted for over a century. Still, extrapolating from historical trends, market reallocations appear to have the potential to eventually eliminate any differences and in the very long run they may disappear.

6 Alternate Mechanisms

In this section, I explore and find little evidence for alternate potential mechanisms which might explain the impact of railroad land grants on development. Additional mechanism checks are discussed in Appendix Section F.

6.1 Environmental Impacts

Persistent impacts on land values could come through changes in environmental characteristics. Note that these could occur in two ways. First, imbalance could occur if any of the grant areas and their federal controls differed in quality ex-ante. The most likely scenario for this would be if railroad companies manipulated the location of their tracks to obtain better land. Second, the different land usage patterns of railroad versus federal owners could plausibly have changed the soil or other features of the land. If, say, the larger landowners on railroad sections degraded the soil through neglect, land value would be persistently lower even without inefficiencies in ownership structure.

Table 7 estimates the direct and spillover effects of railroad land grants on various environmental characteristics and finds little evidence of environmental impact. All point estimates are small in magnitude and tightly estimated. On soil quality, for example, the largest point estimate is the spillover's impact of -0.0065 standard deviations, an order of magnitude below the average difference of 0.08 standard deviations between neighboring sections. One of the eight balance checks shows a statistically significant difference: the direct effects check for elevation. Because the difference is small at 3.9 inches and because there is no evidence for impact on terrain slopes, this difference is more likely due to random chance rather than some effect such as soil erosion.

Additionally, the even-odd comparison in this test would have been nearly impossible for railroad companies to manipulate as any grant area had to encompass hundreds of both even and odd sections. Thus, either the different land use patterns had little effect on land quality or impacts were spread out across a broad area.

6.2 Political Economy

One alternative explanation for the results shown thus far is that land concentration's effects stemmed from political rather than economic channels. Small farmers could have been hurt if landlords monopolized public goods for themselves or lobbied for local policies that benefited their larger properties. Indeed, Gates (1941) concludes that "to gain their objectives the speculators were forced to enter politics... They were influential in local and state governments which they warped to suit their interests." In addition to controlling public goods and public policy, landlords might have evaded taxes and thus lowered governments' budgets overall. Gates (1942) indeed asserts that "speculators were slow to pay taxes. They resisted increased levies, secured injunctions against expenditures for buildings and roads, and sometimes simply refused to pay." Some studies have provided empirical support for these claims, finding that landed elites' capture and coercion of political systems can reduce development (Acemoglu et al. 2019; Galor et al. 2009; Rajan and Ramcharan 2011). Alternately, others have found that land owning elites use their influence to solve collective action problems and increase public goods provision (Dell 2010).

Despite the plausibility of political manipulation of both sorts, I find little evidence that the typical landlord turned politics to their advantage. Table 8 presents estimates of railroad land grants' impact on public goods, taxpaying behavior, and officeseeking based on 1940 Census maps and early twentieth-century tax and election records from individual counties. Column (1) provides suggestive evidence that the number of public goods[25] in 1940 is, in fact, lower in landlord sections relative to nearby Homesteaded ones. Column (2) studies the time it took property owners to pay land taxes in 1900 and finds that land concentration in fact led to prompter payments. Finally, column (3) studies whether a property owner ran for office in 1912. The sample is small and no effects are statistically significant, but if anything owners of concentrated property are less likely

[25]Specifically, schools, churches, cemeteries, community halls, post offices, and hospitals. Schools by far appear the most commonly followed by cemeteries and community halls.

to seek office than their neighbors.[26] Columns (4)-(6) consider the same outcomes but find little evidence for any spillover effects. Notably, this finding contrasts with theories that landowning elites would want to reduce public goods broadly: the effects in column (4) suggest the negative effects are limited to landlords' properties only, although it is not tightly estimated.

These quantitative results stand in stark contrast to the claims by Gates. Landlords did not monopolize public goods for their properties and indeed had fewer located on them than their immediate neighbors. They paid their taxes more promptly and, suggestively, were less likely to run for office. Most consistent with these figures is a story of a relatively neutral political environment. Concentrated properties received public goods in rough proportion to their population and the wealthier owners of concentrated sections could pay their taxes with greater ease. Since landlords were often absentee, on average they ran less for local offices. Thus, although landlords wielded political power for their advantage in many settings, land concentration alone was not enough to lead to political capture on the frontier.

6.3 Current Owners and Farm Sizes

Although the natural experiment directly changed historical farm sizes and other owner characteristics, any remaining differences are unlikely to drive the results today. First, owner characteristics per se should have no impact on land values: once a property is sold, the previous owner cannot affect its productivity except through past investments. Assessors are cognizant of this and do not incorporate owner characteristics into their valuations.

A back-of-the-envelope calculation additionally shows that my results are too large to be caused only by persistence in farm sizes. There is some persistence in property size, with my preferred measure in Appendix Table A4 showing that railroad properties are today 4.1% larger than their neighboring federal ones. Turning to the farm size literature, only Bleakley and Ferrie (2014) estimates potentially negative effects from scale. They show evidence of an inverse-U-shaped curve for US farm sizes, indicating that there is some optimal scale. Using their highest estimate of loss from missizing, assuming that all farms in my sample are larger than this optimum, and applying

[26] In a supplementary analysis, I show in Appendix Section D.3 that counties with railroad land grants were, in fact, more likely to vote for anti-elite parties than other comparable counties.

their figure to my estimates yields only a value loss of 0.4%.[27] Even under generous assumptions, farm size persistence is an order of magnitude too small to explain my results. Consequently, the majority of the persistence in land values comes from the persistence of investments and land use patterns rather than farm size.

7 Optimal Land Policy

The previous results imply that the historical US land policy was inefficient. In this section, I consider how much land values would have improved under an optimal counterfactual policy. I detail the assumptions I need to convert my empirical results into counterfactual outcomes on land values and I discuss how I estimate sunk investment costs unobserved in property assessments. Finally, there may have been other policy barriers to assigning different lands to railroad companies which I consider both qualitatively and by imposing constraints in my optimizations.

As in result 3.3 of my model, land prices capture expected future productivity net of costs and so are a natural starting point to consider for welfare in this setting. My empirical results thus provide the backbone for assessing land policies. I consider the assignment of each PLSS section in my sample to either a Homestead policy H or a railroad policy RR. The choice for each section can thus be denoted by $p_s \in \{0, 1\}$ with a 1 denoting a railroad or market-based allocation and a 0 denoting a Homestead policy targeted toward small farmers. Land policy for a section s matters both for the value of s itself and for the value of neighboring sections due to spillover effects. The goal of the government is to assign lands so as to maximize modern property values, subtracting any sunk costs.

My empirical results identify the impact of initial ownership type on land values, providing the primary parameter I need to estimate welfare. Several assumptions, however, are needed to evaluate all counterfactual policies. First, I assume that allocating section s to railroad companies has no spillover effects on other railroad-sold neighbors. Notably this means that comparing a railroad section to a pure control Homestead requires adding both the direct effect from equation

[27] Of their estimates in logs (Table 5 Panel B), -0.202 is the largest in magnitude. I use their Figure 4 to reverse their normalization, yielding a coefficient of -1.83 on the squared gap between real and ideal sizes. With a 4.1% gap in my data, the overall effect becomes $-1.83(0.041)^2 \approx -0.3\%$. Although this calculation uses concentration differences in a different sample than the property values calculation, the non-government sample in fact shows a larger difference in valuations.

(3) and the spillover effect from equation (4); adding only the direct effect would result in a Homestead section which neighbored railroad sections. I assume that these can be added together directly, implying linear effects. Third, I assume that spillover effects on (asinh) total property value scale linearly in the fraction of neighboring sections owned by the railroad. These assumptions are not easily tested in my setting: railroad sections never have adjacent railroad neighbors and Homesteaded sections in the grant boundary have all railroad neighbors except at the very edge of the area. Finally, when evaluating counterfactual policies, I assume for computational ease that my estimates on the asinh of property values approximate percentage changes.

Another component to the optimization is how to account for historical sunk costs not accounted for in land values. For American frontier settlers, these would primarily have consisted of the costs in clearing land and constructing a dwelling. To estimate clearing costs, I conservatively use the relatively high estimates from the early 20$^{\text{th}}$ century. Coffin (1902) reports average clearing costs of \$3.50 per acre in 1900 or roughly \$100 per acre today adjusting for CPI inflation. I also account for the costs of housing construction. According to the 1930 Census,[28] the median home in rural areas of my six sample states was worth \$2000 or roughly \$30,000 today. A second question is how to evaluate the timing of sunk costs, particularly since little data exist on this at the micro level. A conservative approach is to count the costs as contemporary. Although discounting would imply larger costs if they occurred in the past, I do not similarly count historical benefits which would presumably be greater.[29] I use the figures in Table 4 to find the differential rate at which land was cleared and assume that the full section would have been cleared if it had been a crop farm. For housing, estimate the number of houses per section as one fourth the population, assuming four people per household. Because of the right-skew of these data, I top-code them at the 95$^{\text{th}}$ percentile and regress this quantity using the even-odd, attenuation-adjusted methodology in Table 3 to determine differential rates of housing construction. The results show no statistically distinguishable difference based on soil quality and I thus use the estimate of 0.089 additional houses on unconcentrated sections.

The problem becomes maximizing $\mathbb{E}[Y]$ under counterfactual scenarios with Y representing

[28] The earliest with relevant data

[29] For instance, if land was cleared in 1950 I treat the cost as though it had been cleared in 2017. My approach only considers land values in 2017, however, and so misses 67 years of the benefit of clearing. Since clearing costs were presumably smaller than benefits, valuing both for 2017 is a conservative approach.

total property value according to an assessor. Notably, the empirical results directly estimate effects on the quantity of interest: total property valuations. This fact obviates the need to separately estimate any model parameters and leads directly to a welfare formula that can be numerically optimized. With P representing the vector of policies p_s with n sections, the optimization problem becomes

$$\max_P \sum_s \hat{V}_s \left(D_s P_s + (1 - P_s) S_s P\right) - C_s P_s \tag{5}$$

$$\text{such that } P \in \{0, 1\}^n$$

The first term represents the spillover effects which can only be occur if a section s is allocated to a railroad company and a neighbor is not. S_s represents s's spillover effect percent, D_s is represents s's direct effect percent, $\hat{V}$ is the estimated counterfactual "pure Homestead" value. $\hat{V}$ can by computed in my data by taking the actual land value and subtracting any treatment effects from the railroad land grant policy. The multiplicative effects shown here ensure that the objective function is measured in dollars rather than percentages. Finally. C is a vector of sunk costs according to the previous procedure. Note that the problem is equivalent to a binary quadratic optimization. In general such problems are NP-hard (Pardalos and Vavasis 1991) but features of my problem render it tractable for specialized numerical optimization methods. Methods for problems of this form are relatively recent developments, particularly for commercially available software (Bliek et al. 2014).

I perform welfare computations under several different scenarios. In each one, I show the welfare value of an all-railroad policy ($p_s = 1$), an all-Homestead policy ($p_s = 0$), actual policy, and the optimal policy. Note that the implicit normalization above gives welfare relative to the all-Homestead policy, $P = 0_n$. In the different scenarios, I vary the source of calculations for S and D as follows:

1. (No spillovers, no sunk costs): $S = 0$, $C = 0$ and direct effects are estimated according to Table 2, column (5) which accounts for heterogeneity by land quality

2. (Add spillovers): $C = 0$, S is estimated from Table 6, direct effects are estimated as per

Model 1

3. (Add sunk costs): C is estimated according to the above discussion, S and D as in Model 2
4. (Increased sunk costs) Model 3, but with clear costs multiplied by 5 as a robustness check

Each scenario also allows me to calculate the estimated pure control value $\hat{V}$ necessary for the main calculations. In order to avoid results being driven by outliers, I bottom-code property values at \$100 per square mile and top-code them at \$25 million per square mile, the latter being roughly the top 1% of total rural land value. For consistency, I use my preferred Model 3's $\hat{V}$ although in practice the choice of $\hat{V}$ has few effects on the output.

In each scenario, I consider the value of land, adjusted for sunk costs, under five different scenarios. The historical policy serves as a baseline and I normalize welfare to 0 in that case and report differential effects from other policies. I also consider a policy of all land being allocated to railroad companies, all land being allocated to Homesteads, and next the optimal case where P can be assigned without constraint. However, in practice the federal government did need to pay railroad companies for track construction. At the time, of course, neither the government or the company could have known exactly how the land would develop. I thus regress (asinh) total property value on the geographic and railroad distance controls listed in Section 5.1 and add a constraint that the total geographically-predicted value given to railroad companies is at least as high as under a constrained optimal policy. This constraint satisfies the need to pay railroad companies with land. Although it is conceivable that paying railroad companies with land near their tracks increased their incentives for quality, in practice such effects are likely to be small. Rae (1952) argues that even the baseline monetary incentives had modest effects and, in any event, most European countries constructed high-quality railroads even in sparsely-populated areas like Russian Siberia. I thus focus my constraint on the overall value of land given to railroads and do not model any other effects.

Optimal federal land policy looked quite different from the historical one. Welfare results in billions of dollars relative to the baseline policy are shown in Table 9; Figures 12 and 13 depict the baseline allocation and the constrained optimal one under my preferred model, Model 3. In this setup, the government could have increased land values by about \$28 billion or roughly 4.8% from

the baseline. Notably this would have involved reallocating large amounts of lower-value land to railroad companies and reserving the high-quality ones for homesteaders who would work it most intensely. Thus Figure 13 shows railroad companies receiving land primarily in Florida westlands, eastern Montana, and central Wyoming, areas which often historically difficult to settle as homesteaders found small-scale farming unviable there. Including sunk costs does not substantially alter these calculations: the optimal policy's welfare falls by $0.2 billion once they are added going from Models 2 to 3. Even the raised costs going from Models 3 to 4 do not change most of the comparisons although they do change the fraction of land allocated to railroad companies substantially. These results are consistent with the historical literature which suggests that land investments cost only a fraction of their potential benefit in most areas (Lindsey 1929).

Adding the constraint that the federal government had to reward railroad companies with large amounts of land only marginally changes adjusted land values. In Model 3, welfare is lowered by $2.5 billion or about 0.4% of baseline. Finally, any welfare improvements over a pure Homestead policy would have been fairly modest, about $0.1 billion overall. Homesteaders' intensive farming methods were desirable for most areas, and landlords better for a minority of low-valued areas. These calculations thus provide support to historians such as Gates who wished the government had gone further in promoting the Homestead Act and made fewer exceptions for large owners that were codified in the railroad land grants policy.

8 Conclusion

In this paper, I have explored a natural experiment that increased land concentration in the American West. Although the concentration itself largely faded, the process took many decades and left behind changes in land use that are still apparent today. The slow speed of convergence and the permanent importance of initial land allocations in determining property borders point to failures in the land market, most likely substantial transaction costs.

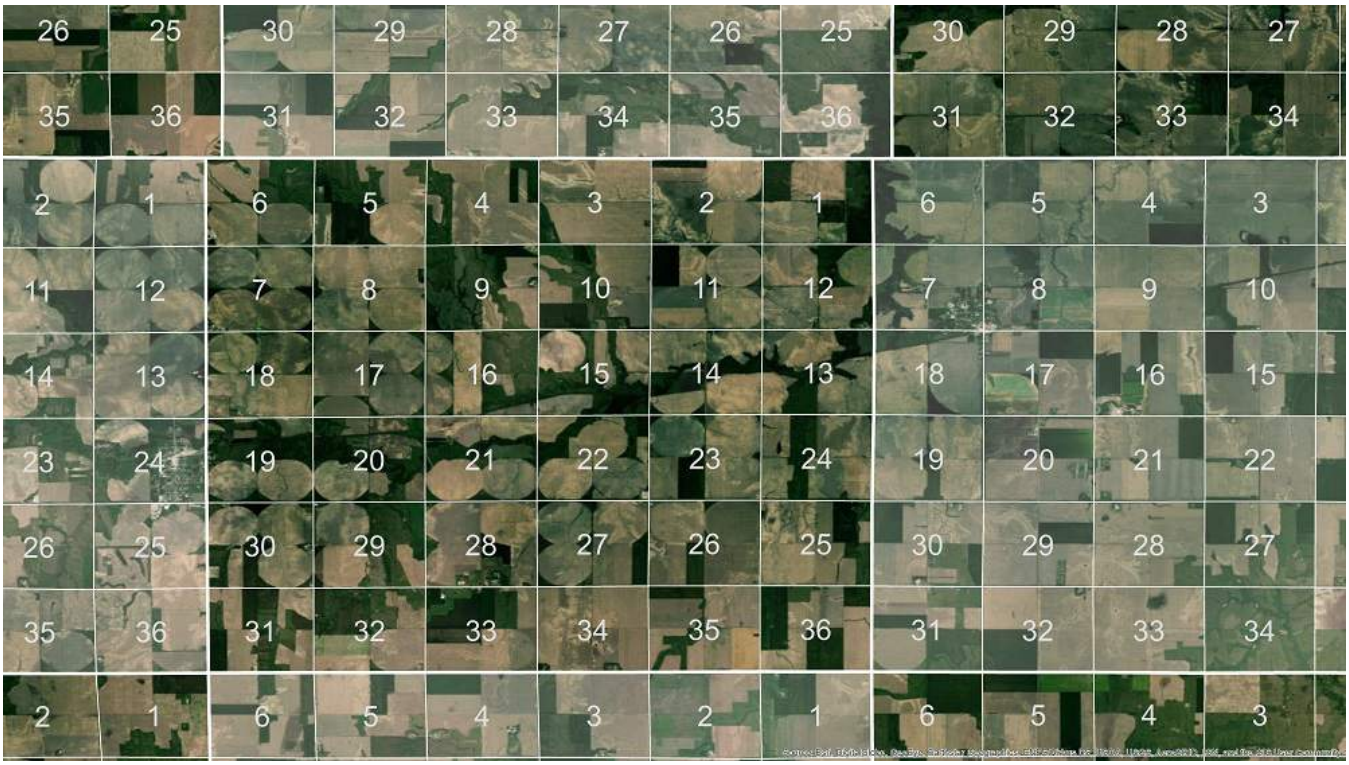
Land concentration's impact on economic development is primarily negative. The larger landowners invested less in their properties, an effect that spread as they purchased adjacent lands. In keeping with the work of Gates and other historians, tenancy generally and sharecropping

specifically were major drivers of the lowered investment. Perhaps surprisingly, larger landowners seemed to have exerted little political influence in the areas they held property. They did not monopolize public goods or seek office more than their homesteader neighbors. Contrary to the assertions of Gates and other historians, they paid their taxes on time. Since many land reforms were promulgated with the express purpose of diminishing the political power of landlords, the absence of this phenomenon in the frontier United States is strange. Plausibly, institutions other than mere economic power are required for political capture. In many countries, landlord power was built upon centuries of tradition, a supporting structure absent in the younger, democratic United States.

This study holds a number of lessons for land policy generally. In direct terms, the relative success of small farmers in developing their land supports the wisdom of the American Homestead policy and its restrictions on land speculation and accumulation. Indeed, even an optimal land policy offered only marginal improvements to land values relative to a universal application of the Homestead Act. Whether such results hold true in other settings is a question beyond the scope of this paper. However, the long-lasting effects of land concentration on development in the United States should bring renewed attention to the patterns of concentrated land ownership which are common throughout the world.

9 Tables and Figures

Figure 1: The Public Lands Survey System

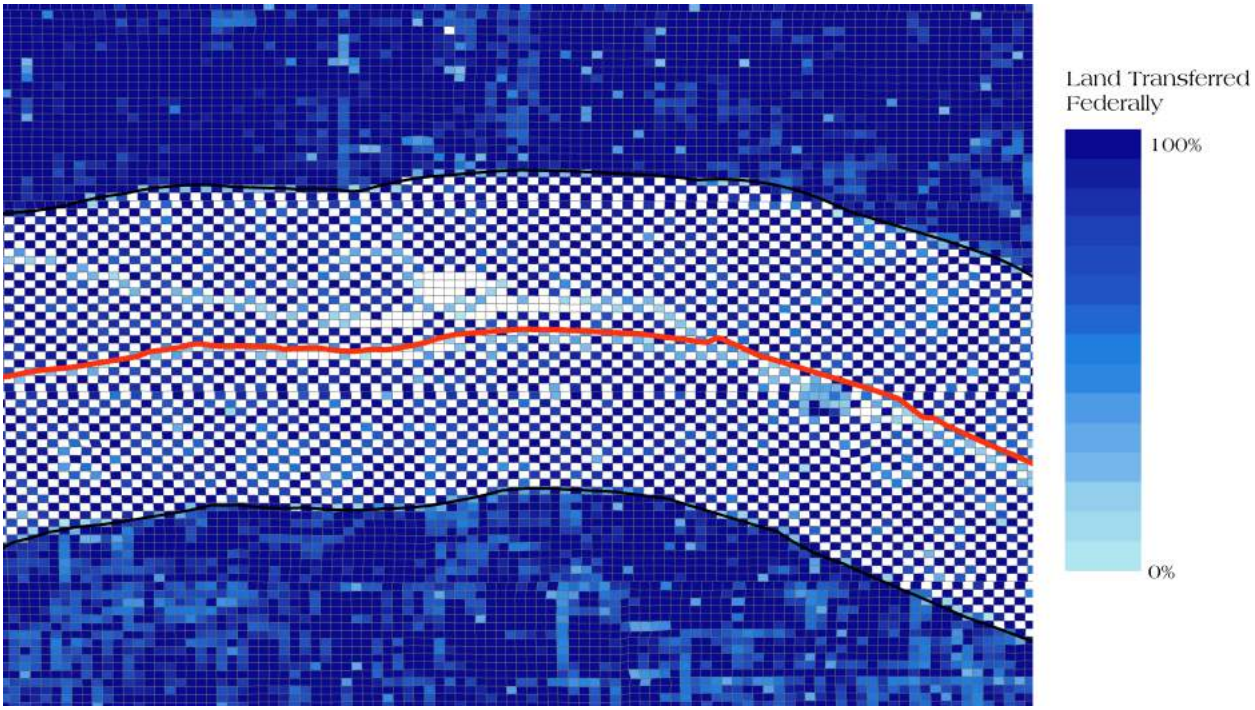


Notes: Nebraska PLSS Townships and Numbered Sections

Figure 2: Railroad Land Grant Areas

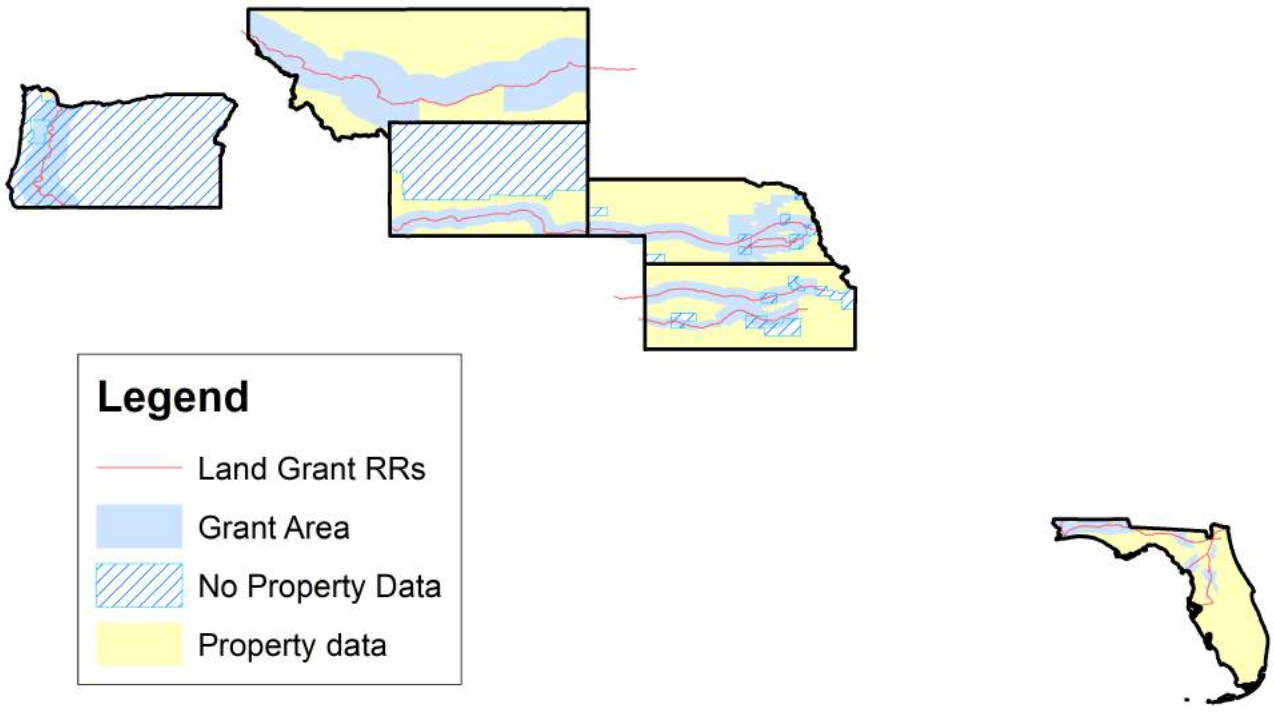
Notes: Areas Allotted for Railroad Land Grants (Source Miller and Staebler (1999))

Figure 3: The Checkerboard Pattern



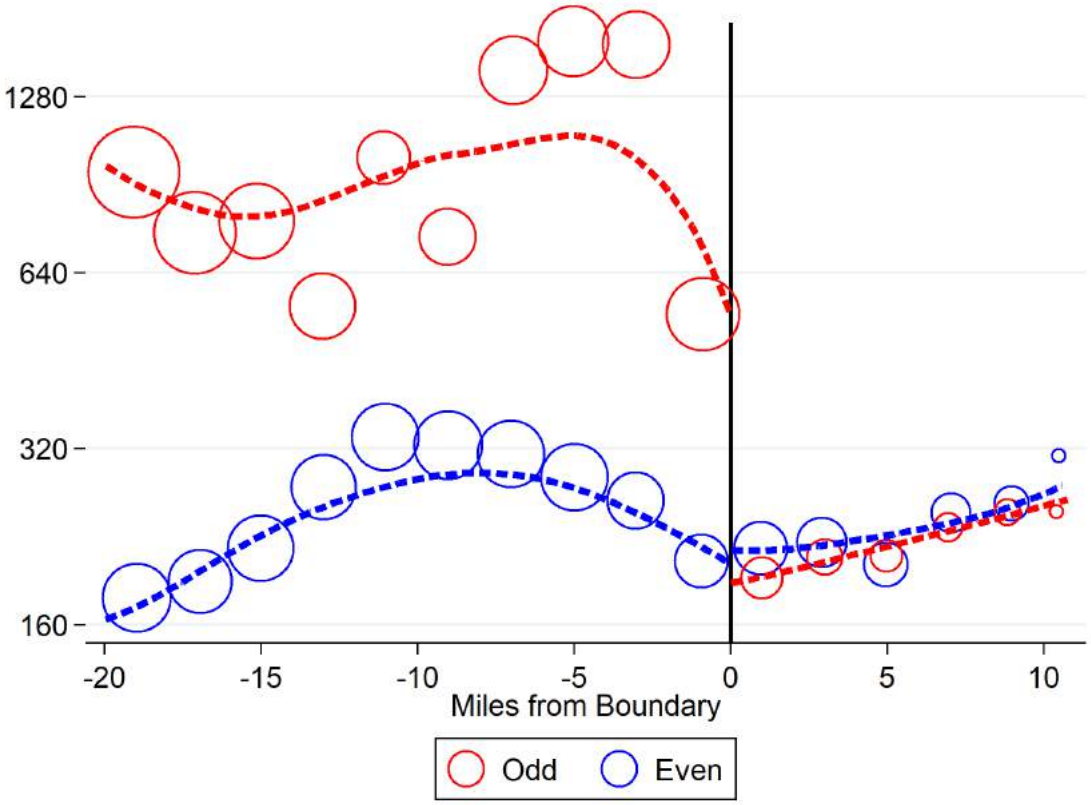
Notes: Fraction of Land (in blue) transferred by Federal Government, Western Nebraska. Lands given given by the federal government to other government entities marked fully in blue.

Figure 4: Property Tax Sample



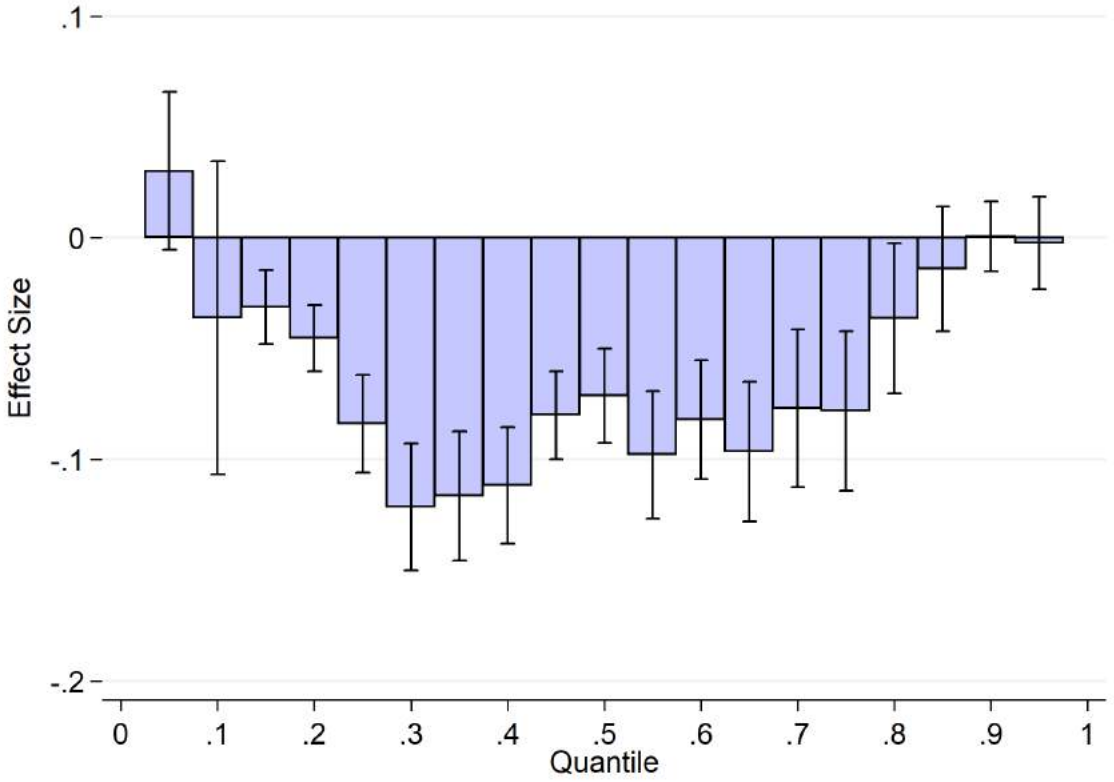
Notes: Areas covered by the property tax dataset

Figure 5: Acres Owned at Time of Initial Transfer



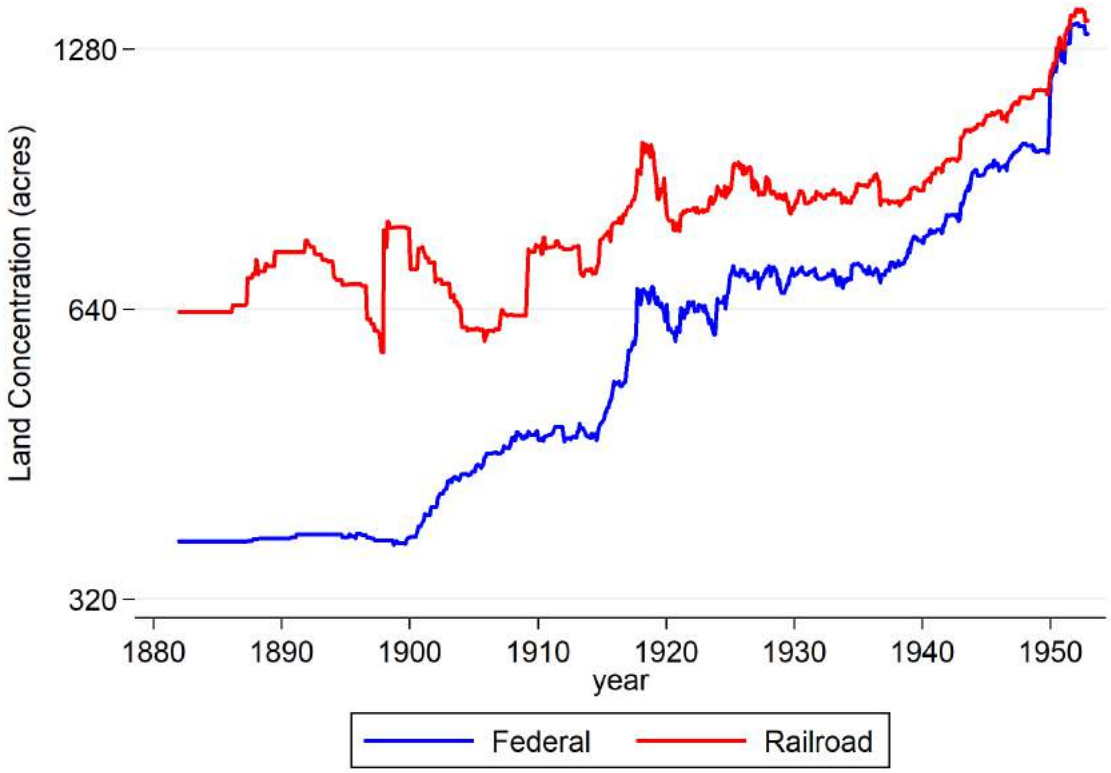
Notes: This figure depicts land concentration at the time of initial sale in Lincoln County, Nebraska. Land concentration is computed at the PLSS section level as the average log land owned at initial sale of all owners weighted by land owned in that section. These data are binned every 2 miles, with the size of each dot proportional to the number of sections within it. The left half of the figure depicts sections within the railroad land grant boundary; the right half of the figure depicts sections outside it.

Figure 6: Quantile Effects on asinh(Total Property Value)



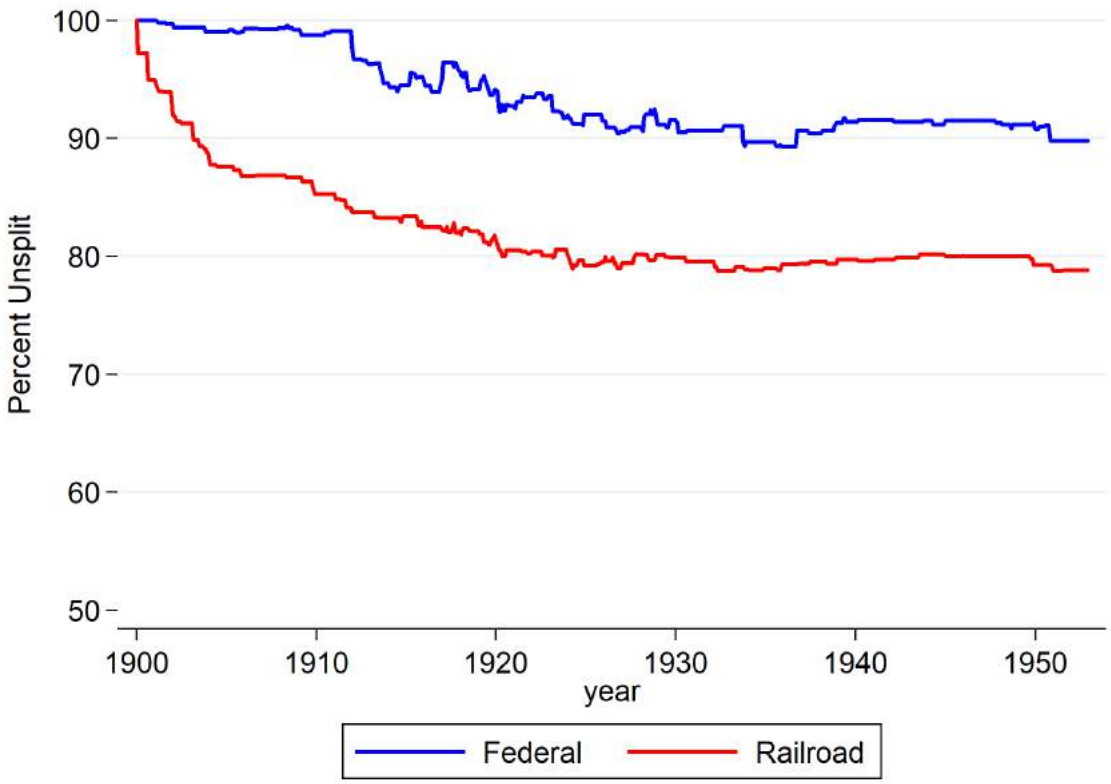
Notes: This figure depicts the quantile effects of railroad land grants on (asinh) total property value in 5% intervals according to direction, even-odd comparison equation (3) with no controls and heteroskedasticity-robust standard errors. Error bars depict a 90% confidence interval with robust standard errors.

Figure 7: Land Concentration Over Time, Banner County



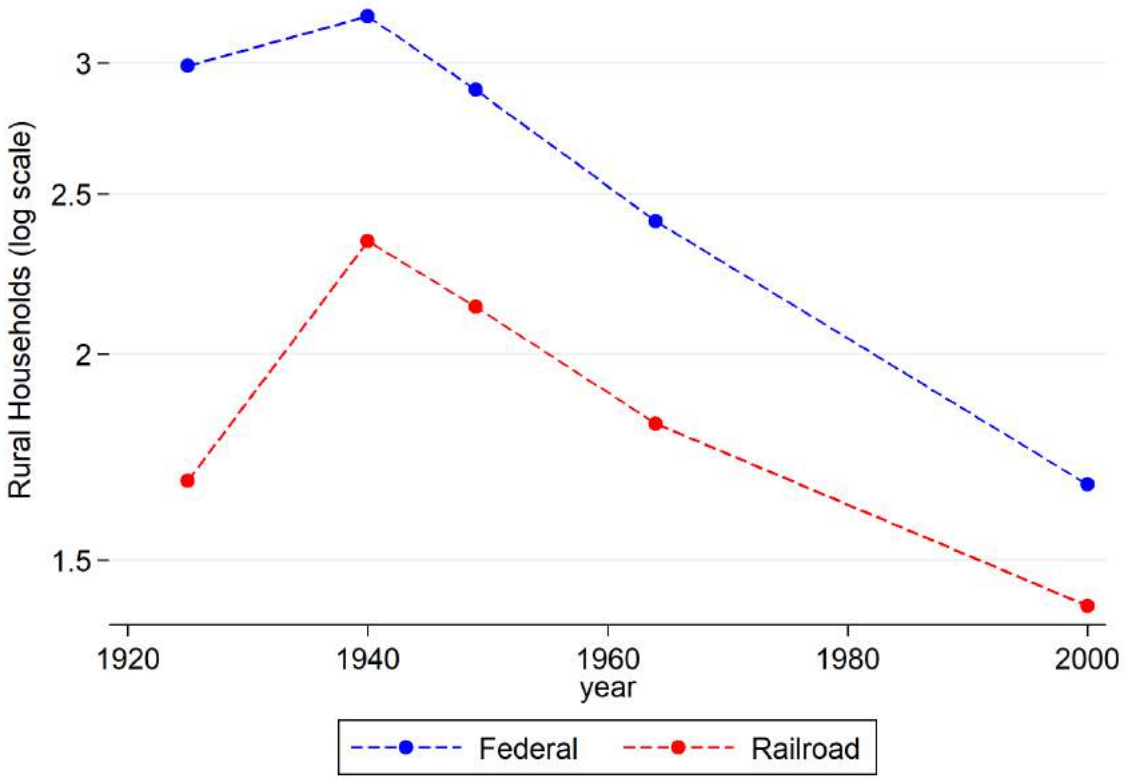
Notes: This figure shows individual land ownership concentration in the 17N townships of Banner County, Nebraska over time. For each month, I calculate the (log) amount of land owned by each individual and then note for each parcel the amount of land held by its owner. I average this quantity by month and by section parity and plot it above. For stylistic purposes, I convert the averages back to acres and use a log scale on the y-axis.

Figure 8: Banner County Unsplit 1900 Properties



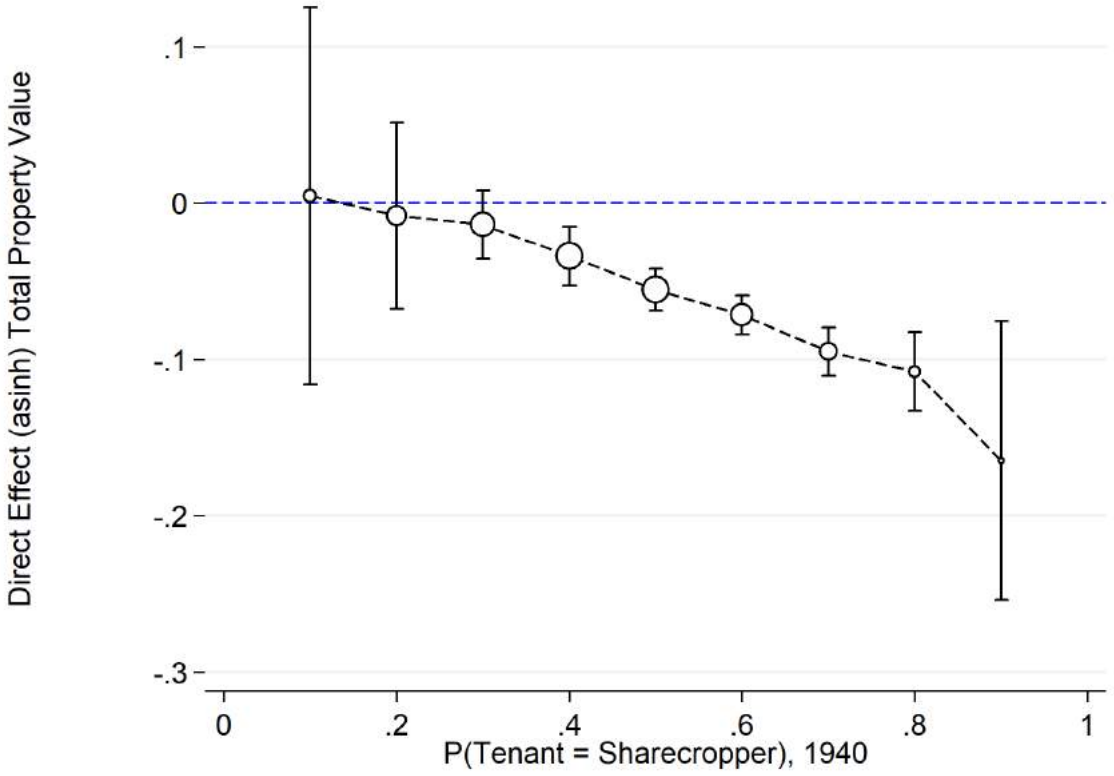
Notes: This figure depicts the average fraction of property retention of 1900 property boundaries in the 17N townships of Banner County, Nebraska. Defining a property as all the land owned by a given entity, I define retention as being equal to 1 if a parcel remains in the largest part of a 1900 property owned by one entity at some future point t. This figure report retention rates averaged across all parcels for 1900 properties by even, non-education and odd sections separately.

Figure 9: Merrick County Rural Households



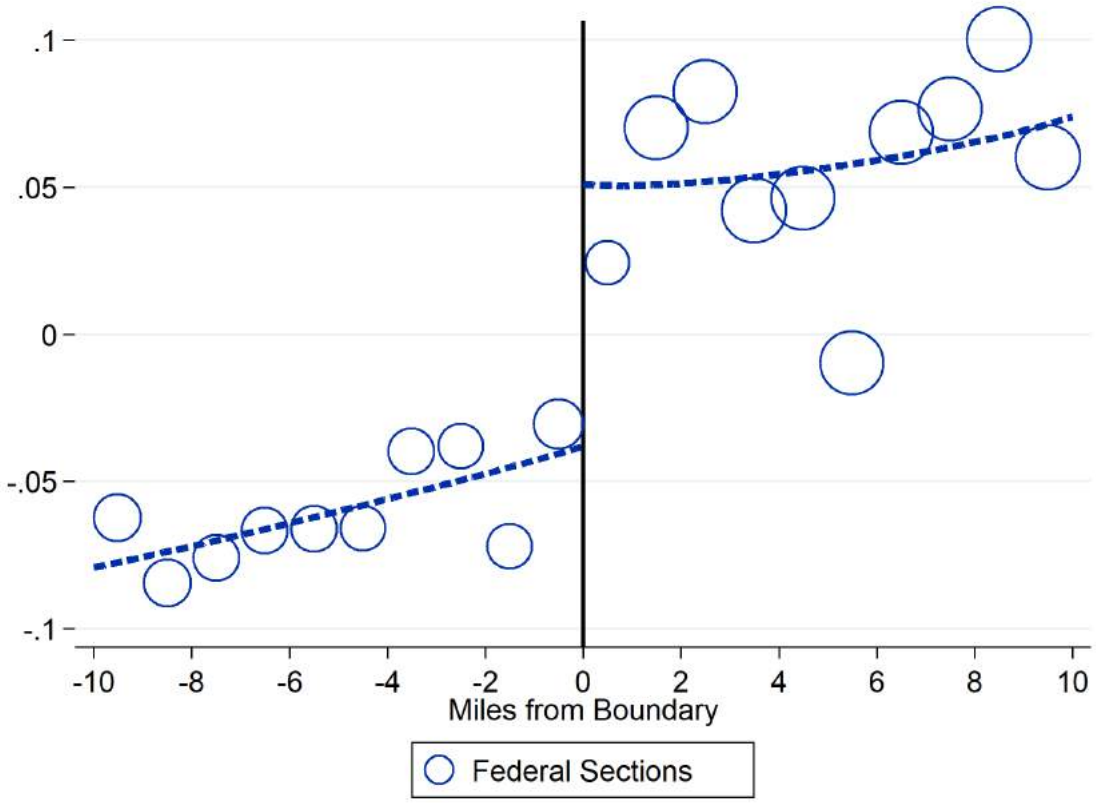
Notes: This figure depicts the number of rural households in even and odd sections of Merrick County, Nebraska. 1925 data come from the Post Office "Rural Free Delivery" map. 1940 - 1964 data come from Census Enumeration District maps. 2000 data come from Census Block data as described in Section 5.5.

Figure 10: Effects on Property Values by Predicted Share Tenancy



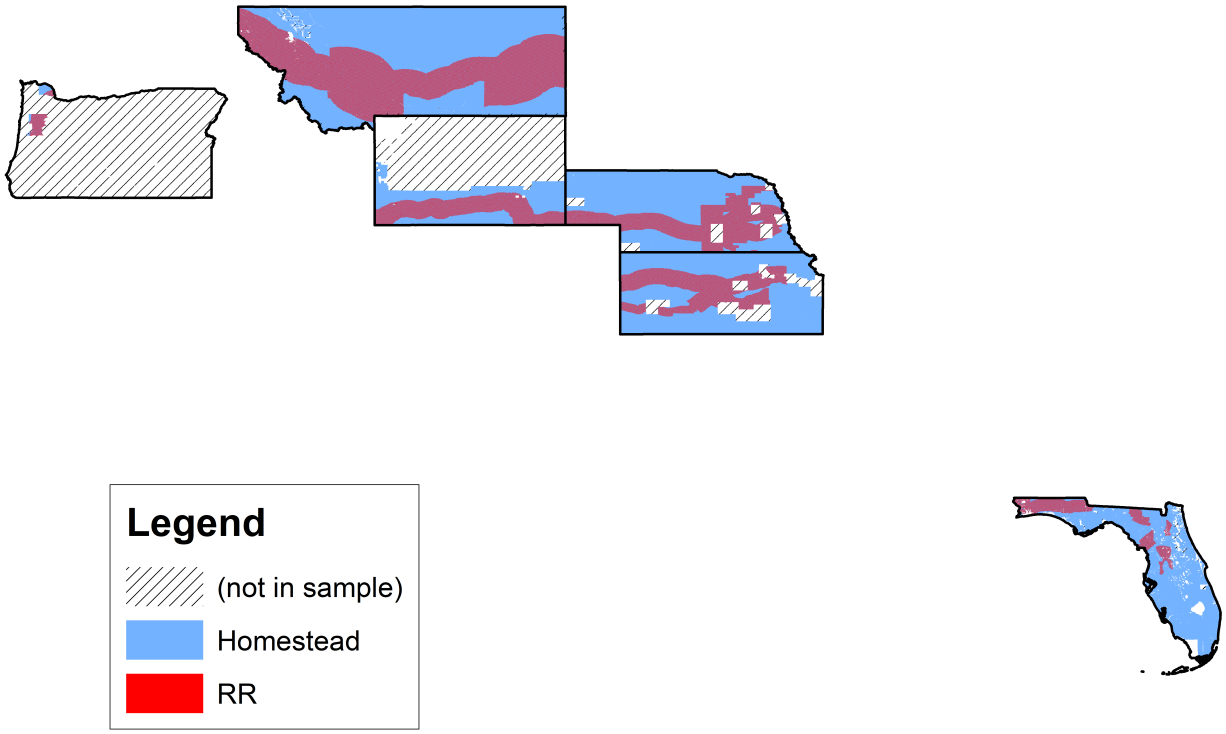
Notes: This figure reports non-parametric treatment effect estimates of the direct effect of railroad land grants on total property value with respect to the predicted fraction of tenants who are "share tenants," working for a fraction of the crop. Predicted levels of share tenancy are formed from county-level geographic characteristics as reported in Appendix Table A3. The specification uses a local linear interaction design: for a given level of predicted share tenancy, s_0, the graph reports the estimate of α_1 in the specification $y_i = \alpha_1 \text{RR}_i + \alpha_2 \text{RR}_i \times (s - s_0) + X_i\beta + \varepsilon_i$ for data within a certain bandwidth of s_0 and where s denotes share tenancy. I use a bandwidth of 20% in the above estimates. The specification essentially extends a local linear estimate of a level to estimating an interaction.

Figure 11: (asinh) Total Property Value, Residuals



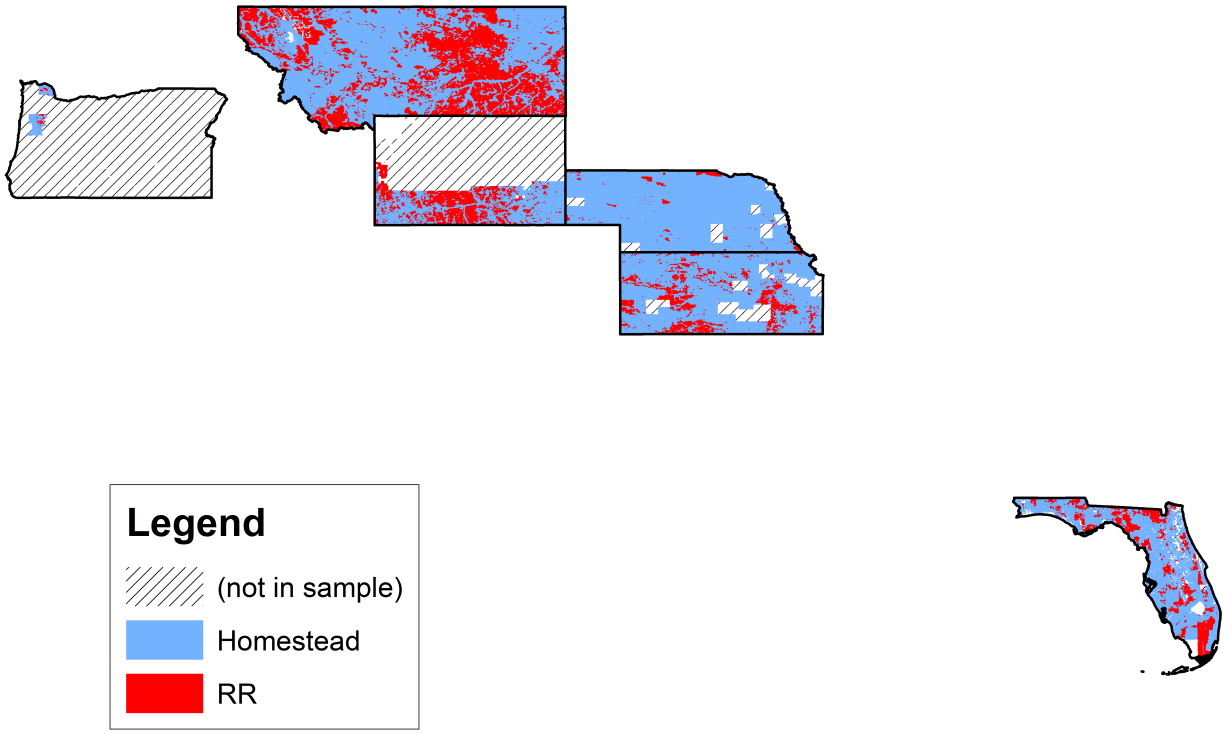
Notes: This figure reports binned averages of residuals of (asinh) total property value in one-mile bins with respect to their distance from the railroad land grant boundary. Data are restricted to non-education, federally-administered sections only: even-numbered sections always and odd-numbered sections one or more miles outside the boundary.

Figure 12



Notes: This figure depicts the pattern of railroad land grants as per actual government policy. Blue indicates a Homesteaded section and red indicates a railroad land grant; purple areas thus depict places where half the sections are split between the two policies. Stripes indicate PLSS sections outside the scope of my sample data.

Figure 13



Notes: This figure depicts the pattern of railroad land grants as per the optimal constrained policy in model 3. Blue indicates a Homesteaded section and red indicates a railroad land grant. Stripes indicate PLSS sections outside the scope of my sample data.

Table 1: Direct Effects on Historic Population, Investment

	Main				Placebo			
	(1) (asinh) Investment	(2) Improved %	(3) (asinh) Equipment	(4) (asinh) Farmsteads	(5) (asinh) Investment	(6) Improved %	(7) (asinh) Equipment	(8) (asinh) Investment
RR Effect	-0.77**	-9.93**	-0.26***	-0.26***	0.042	-0.45	-0.21	-0.015
	(0.28)	(4.30)	(0.063)	(0.012)	(0.052)	(0.67)	(0.19)	(0.012)
Sample	Morrill 1912	Morrill 1912	Lincoln 1965	NE 1940	Placebo	Placebo	Placebo	Placebo
Geo Controls	Y	Y	Y	Y	Y	Y	Y	Y
County FEs	Y	Y	Y	Y	Y	Y	Y	Y
Township FEs	Y	Y	Y	Y	Y	Y	Y	Y
SEs / Clusters	Township	Township	Township	Spatial	Township	Township	Township	Spatial
N	101	101	2,084	18,999	1,229	1,229	310	8,836
$\mathbb{E}[y]$	\$3.2k	13%	\$13k	2	\$2.2k	22%	\$12k	1.5

Notes: This table estimates the effect of railroad land grants on the quality and number of farms in historical periods according to direct comparison equation (3). Columns (1)-(4) estimate the main effects within the grant areas and columns (5)-(8) estimate placebo effects outside them. Columns (1) and (5) compute the effect on (asinh) total value of improvements, measured in thousands of 2017 dollars, in Morrill County, Nebraska in 1912. Columns (2) and (6) compute the effect on the fraction of land marked as improved ranging from 0% to 100%. This outcome is also measured in Morrill County, Nebraska in 1912. Columns (3) and (7) measure the (asinh) value of farm equipment measured in Lincoln County personal assessments, 1965. Columns (4) and (8) compute effects on the number of farmhouses per section. This outcome is measured in 1940 for a sample of Nebraska counties. Geographic controls denote 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, the logarithm of distance to the grant railroad, and latitude and longitude by state. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Effects on Total Property Value

	(1) Value	(2) Value	(3) Value	(4) Value	(5) Value	(6) Value	(7) Value
RR Effect	-0.046***	-0.045***	-0.044***	-0.044***	-0.059***	-0.061***	-0.0013
	(0.015)	(0.015)	(0.015)	(0.014)	(0.012)	(0.012)	(0.0050)
RR × Low					0.058***		
					(0.012)		
log(RR Distance)				-0.40***	-0.40***	-0.24***	-0.37***
				(0.084)	(0.084)	(0.031)	(0.015)
Sample	All	All	All	All	All	Non-imputed	Placebo
State FEs	Y	Y	Y	Y	Y	Y	Y
County FEs		Y	Y	Y	Y	Y	Y
Township FEs			Y	Y	Y	Y	Y
Geo Controls				Y	Y	Y	Y
SEs / Clusters	Spatial	Spatial	Spatial	Spatial	Spatial	Spatial	Spatial
N	132,463	132,463	132,463	132,463	132,463	107,333	230,483
$\mathbb{E}[y]$	\$2,185k	\$2,185k	\$2,185k	\$2,185k	\$2,185k	\$2,321k	\$9,566k

Notes: This table estimates the impacts of railroad land grants on land value using the direct comparison equation (3) with heterogeneity by low soil quality. Low is an indicator for whether the gSSURGO soil quality index is in the bottom quintile. Columns (1)-(7) use the asinh of total property value as recorded by the assessor as the outcome. Column (6) removes the counties with satellite use value. Column (7) conducts a placebo test, only considering areas at least one mile from a railroad land grant boundary. Geographic controls are the same as in Table 1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Direct Effects on Modern Population, Investment

	(1) (asinh) Investment	(2) Investment > 0 (%)	(3) (asinh) Housing	(4) (asinh) Non-Housing	(5) (asinh) Pop	(6) (asinh) Pop	(7) (asinh) Pop
RR Effect	-0.23***	-3.68***	-0.22***	-0.16***	-0.034***	-0.083***	-0.15***
	(0.047)	(1.00)	(0.045)	(0.034)	(0.011)	(0.014)	(0.016)
Census Blocks						0.48***	0.43
						(0.045)	(.)
Geo Controls	Y	Y	Y	Y	Y	Y	Y
County FEs	Y	Y	Y	Y	Y	Y	Y
Township FEs	Y	Y	Y	Y	Y	Y	Y
SEs / Clusters	Spatial	Spatial	Spatial	Spatial	Spatial	Spatial	Spatial
N	132,463	132,463	121,906	132,463	132,460	132,460	17,713
$\mathbb{E}[y]$	\$1,277k	43%	\$1,004k	\$412k	19	19	18

Notes: This table estimates the effect of railroad land grants on population and farm quality in the modern period according to the direct comparison equation (3). Columns (1)-(4) show the impact on dollar values of improvements in 2017, measured in thousands of dollars. They respectively detail (asinh) total improvements, the percentage of sections with positive investment, (asinh) housing improvements, and (asinh) non-housing improvements. Columns (5)-(7) use the outcome (asinh) population in 2000 as derived from census blocks. Columns (6) and (7)'s coefficients come from an interacted regression that adjusts for attentuation due to census block overlap; see Section 5.2. Column (7) restricts the sample to counties that have farmstead data in 1940 for sample consistency. Geographic controls are the same as in Table 1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Impacts on Land Use

	(1) Num. Uses	(2) Crop Farm (%)	(3) Grass Farm (%)	(4) (asinh) Value Satellite (ag)	(5) (asinh) Value Satellite (all)
RR Effect	-0.093***	-1.68***	0.49	-0.027***	-0.019*
	(0.022)	(0.50)	(0.92)	(0.0094)	(0.0099)
RR × Low	0.089***	1.45***	6.53***	0.052***	0.040**
	(0.018)	(0.49)	(1.69)	(0.014)	(0.017)
Geo Controls	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	132,462	132,462	94,571	132,462	132,462
$\mathbb{E}[y]$	4.2	40%	81%	3.2%	5.5%

Notes: This table estimates the effect of railroad land grants on land use using the direction comparison equation (3) with heterogeneous effects by low land quality. Column (1) reports effects on the number of economic categories of activity reported by the CropScape satellite data. Column (2) uses an indicator for at least 10% of the land being allocated to crops according to CropScape. Columns (3) studies the percentage of land where the assessor recorded any grassland use. Columns (4)-(5) report effects on the (asinh) satellite-imputed land use values described in Appendix Section B.5. Column (4) considers only agricultural uses while column (5) adds in imputations for roads and other structures. Low land quality is defined as in Table 2. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Effects on Owner Distance to Land

	Different County (%), 1900s		(log) Distance, 2017	
	(1) Direct	(2) Spillover	(3) Direct	(4) Spillover
Panel A: Tenancy Impacts				
RR Effect	15.7***	10.6***	8.24***	0.037***
	(4.09)	(3.19)	(2.64)	(0.014)
log(RR Distance)	-3.37	4.63**	-60.3	0.099***
	(2.48)	(1.82)	(37.8)	(0.024)
Sample	Lincoln	Lincoln	2 Counties	Non-gov
Geo Controls	Y	Y	Y	Y
County FEs	Y	Y	Y	Y
Township FEs	Y	Y	Y	Y
SEs / Clusters	Township	Township	Township	Spatial
BW				N/A
N	1,239	1,591	614	34,221
$\mathbb{E}[y]$	47%	66%	88%	60 mi
	Low Predicted Ownership		High Predicted Ownership	
	(1) (log) Distance	(2) (asinh) Total Value	(3) (log) Distance	(4) (asinh) Total Value
Panel B: Heterogeneity				
RR Effect	0.045***	-0.057***	-0.043	-0.024
	(0.012)	(0.012)	(0.044)	(0.021)
Sample	Non-gov Low Own	Low Own	Non-gov High Own	High Own
Geo Controls	Y	Y	Y	Y
County FEs	Y	Y	Y	Y
Township FEs	Y	Y	Y	Y
SEs / Clusters	Spatial	Spatial	Spatial	Spatial
N	31,383	88,308	2,838	44,155
$\mathbb{E}[y]$	50 mi	\$1,873k	167 mi	\$2,949k

Notes: This table estimates the effects of railroad land grants on owner characteristics in the early 1900s and the modern period. In Panel A, columns (1)-(2) use data on the initial land sales in Lincoln County, Nebraska. Column (3) uses data from Perkins County, Nebraska in 1900 and Morrill County, Nebraska in 1912. Column (4) uses 2017 property tax data and focuses on townships with no government ownership. Panel B studies heterogeneity with respect to the predicted fraction of farms operated by tenants. Columns (1)-(2) restrict to the bottom two-thirds of the sample's tenancy rates, between 0% and 70%. Columns (3)-(4) consider the top third, 70% and above. All Panel B data are from 2017 property assessments. Geographic controls are the same as in Table 1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Spillover Effects on Property Values

	(1) (log) Acres Initial	(2) (log) Acres 1900s	(3) (asinh) Total Value	(4) (asinh) Investment	(5) Other County 1900s	(6) (log) Distance 2017	(7) Num. Uses
RR Effect	-0.13	0.24*	-0.11***	-0.14*	20.0***	0.061*	-0.100*
	(0.12)	(0.14)	(0.040)	(0.083)	(6.01)	(0.035)	(0.051)
Sample	Lincoln	2 Counties	Modern	Modern	2 Counties	Non-gov	Modern
Geo Controls	Y	Y	Y	Y	Y	Y	Y
County FEs	Y	Y	Y	Y	Y	Y	Y
SEs / Clusters	Township	Township	Spatial	Spatial	Township	Spatial	Spatial
N	384	428	23,382	23,382	428	7,834	23,381
N (clusters)	31	35	N/A	N/A	35	N/A	N/A
$\mathbb{E}[y]$	262 Ac.	651 Ac.	\$1,755k	\$978k	78%	56 mi	4.2

Notes: This table estimates the spillover impacts of railroad land grants according to the RD equation (4). The sample is federally-administered sections: all even-numbered sections within railroad grant boundaries and all odd-numbered sections at least one mile away from the boundaries. Column (1) reports effects on the log owner acreage of initial property buyers in Lincoln County, Nebraska. Column (2) reports post-settlement owner acreage in Perkins County, Nebraska (1900) and Morrill County, Nebraska (1912). Column (3) reports effects on the (asinh) 2017 total property value, measured in thousands of dollars in 2017. Column (4) reports effects on the (asinh) 2017 value of improvements, measured in thousands of dollars. Column (5) reports effects on the percent who cannot be matched to their property's county in the 1900s data from Perkins and Morrill counties. Column (6) reports effects on the (log) owner distance to their property for townships without government ownership. Column (7) reports effects on the number of distinct uses for the land as recorded by statellite data. Geographic controls are the same as in Table 1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Environmental Impacts

	(1) Soil	(2) Elevation	(3) Slopes	(4) Streams
Panel A: Direct Effects				
RR Effect	-0.00045	-0.000098**	-0.0017	-0.00075
	(0.0010)	(0.000050)	(0.0018)	(0.0021)
County FEs	Y	Y	Y	Y
Township FEs	Y	Y	Y	Y
SEs / Clusters	Spatial	Spatial	Spatial	Spatial
N	132,463	132,463	132,463	132,463
N (clusters)	N/A	N/A	N/A	N/A
$\mathbb{E}[y]$	-.046	.34	8.8	.26
Panel B: Spillover Effects				
RR Effect	-0.0065	-0.00081	0.0053	0.010
	(0.0092)	(0.0011)	(0.024)	(0.014)
Area	All	All	All	All
County FEs	Y	Y	Y	Y
Township FEs				
SEs / Clusters	Spatial	Spatial	Spatial	Spatial
N	23,382	23,382	23,382	23,382
N (clusters)	N/A	N/A	N/A	N/A
$\mathbb{E}[y]$	.026	.34	7.1	.22

Notes: This table tests for environmental imbalance in railroad sections. Panel A estimates the direct comparison equation (3) and Panel B estimates the spillover RD equation (4). Column (1) reports effects on the gSSURGO soil quality index, measured in standard deviations. Column (2) measures effects on elevation measured in kilometers. Column (3) measures effects on the average terrain slope in degrees. Column (4) measures effects on miles of streams in a section. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Impact on Political Outcomes

	Direct			Spillover		
	(1)	(2)	(3)	(4)	(5)	(6)
	Public Goods	Tax Time	Officeseeking	Public Goods	Tax Time	Officeseeking
RR Effect	-0.024*	-0.17***	-3.61	-0.0021	-0.080	-4.46
	(0.013)	(0.036)	(5.35)	(0.019)	(0.16)	(4.36)
Sample	NE & KS 1940	Perkins 1900	Morrill 1912	NE & KS 1940	Perkins 1900	Morrill 1912
Geo Controls	Y	Y	Y	Y	Y	Y
County FEs	Y	Y	Y	Y	Y	Y
Township FEs	Y	Y	Y			
SEs / Clusters	Spatial	Township	Township	Spatial	Township	Township
N	18,999	531	82	4,064	265	162
N (clusters)	N/A	24	9	N/A	19	16
$\mathbb{E}[y]$	.13	2 yrs	5.5%	.12	2.5 yrs	4.9%

Notes: This table estimates the impact of railroad land grants on public goods. Columns (1)-(3) estimate the direction comparison equation (3) and columns (4)-(6) esimate the spillover RD equation (4). Columns (1) and (4) use the total number of public goods structures in a sample of Nebraska and Kansas counties 1940 as the outcome. Columns (2) and (5) use the log time to pay property taxes in Perkins County, Nebraska in 1900 as the outcome. Columns (3) and (6) use the fraction of property owners who ran for office in Morrill County, Nebraska in 1912 as the outcome. Geographic controls are the same as in Table 1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9: Welfare Calculation Results

	Model 1	Model 2	Model 3	Model 4
All RR	-28.4	-75.5	-74.4	-69.9
Current	0	0	0	0
All Homestead	5.56	28.1	27.8	26.8
Constrained Optimum	5.06	25.4	25.4	26.3
Unconstrained Optimum	5.56	28.1	27.9	28
RR % Current	18.1	18.1	18.1	18.1
RR % Constrained	28.5	17.2	24.8	41.8
RR % Unconstrained	0	0	7.42	31

Notes: This table estimates the welfare effects of various potential US land policies relative to the historical one according to the procedures outlined in Section 7. The first five rows measure welfare changes in billions of dollars, respectively evaluating a policy giving all land to railroad companies, the actual historical policy, an all-Homestead policy, the optimal policy with the constraint that railroad companies the same estimated value of land as historically, and the optimal policy without any constraints. The last three rows report the percentage of land given to railroad companies historically, under the constrained optimum, and the unconstrained optimum respectively. Model 1 only includes direct effects, Model 2 adds in spillover effects, Model 3 adds in clearing costs, and Model 4 considers increased clearing costs.

A Model Appendix

A.1 Characterization of Prices

Define the valuations of the parcel, at the time of sale for an agent of type a and improvement status of type i as $v_{a,i}$. Define $q_{a,i}(c)$ as a's expected valuation from a parcel with improvement i and revealed operator effort cost c. Then,

$$\begin{aligned}
q_{S,1}(c) &= \beta\left(\frac{A^2}{2c} + p_1\right) \\
q_{B,1}(c) &= \beta\left(\frac{A^2}{4c} + p_1\right) \\
q_{S,0}(c) &= \beta \max\left\{\frac{A^2}{2c} + p_1 - r, \frac{1}{2c} + p_0\right\} \\
q_{B,0}(c) &= \beta \max\left\{\frac{A^2}{4c} + p_1 - r, \frac{1}{4c} + p_0\right\} \\
v_{B,0} &= \mathbb{E}\left[q_{B,0}(c)\right] \\
v_{B,1} &= \delta\mathbb{E}\left[q_{B,0}(c)\right] + (1-\delta)\mathbb{E}\left[q_{B,1}(c)\right] \\
v_{S,0} &= \mathbb{E}\left[q_{S,0}(c)\right] - f_t \\
v_{S,1} &= \delta\mathbb{E}\left[q_{S,0}(c)\right] + (1-\delta)\mathbb{E}\left[q_{S,1}(c)\right] - f_t
\end{aligned}$$

The above equations give rise to the price characterization

$$p_0 = \mathbb{E}\left[\max\left\{v_{S,0}, v_{B,0}\right\}\right] \tag{6}$$

$$p_1 = \mathbb{E}\left[\max\left\{v_{S,1}, v_{B,1}\right\}\right] \tag{7}$$

A.2 Model Proofs

First, comparing equations (1) and (2) illustrates that landlords are weakly less likely to invest at any given point in time relative to owner-operators. Further, note that landlords value investments relatively less than owner-operators. Formally, using the terminology of Appendix Section A.1,

$q_{S,1}(c) - q_{S,0}(c) \geq q_{B,1}(c) - q_{B,0}(c)$. This can be shown by noting that the owner-operator's differences in the inputs to the maximum function are either equal to the landlord's (first input) or greater (second input) and taking the maximum preserves the relative ordering.

Areas initially owned by an owner-operator is thus weakly more likely to receive investment and are in turn more likely to have a subsequent owner-operator owner. Since investments remain with probability $1-\delta$ and since the future owner is more likely to be owner-operator, the probability of investment remains weakly higher in the next period as well.

Note that the game is Markov process where the relevant state is whether land has received investment at the end of the period before land is sold. Therefore, the previous argument can be inductively applied to all subsequent periods meaning that owner-operator investment and ownership is permanently elevated for areas initially owned by owner-operators.

The land pricing result is a standard result of market-based asset pricing. Each owner values the land for the profits it gives them and for the resale value which stems from future owners' discounted valuations. Therefore, inductively, they will be willing to pay their expected profit plus the discounted stream of future owners' profits for the land.

A.3 Numerical Example

In this subsection I consider a numerical example of the model which illustrates its main points. I consider each period to last roughly a generation and so choose $\beta = 0.5$, $\delta = 0.25$. I set A and $r = 3$. Finally, I set f to be a binary variable equal to either 0 or 0.3 with equal probability; I set c to also be a binary variable equal to 2 or 6 with equal probability. Solving for the model numerically yields the following results:

1. $p_0 = 0.317, p_1 = 2.08$
2. Owner-operators invest in the "low cost" world only (50% chance), landlords never invest
3. An owner-operator always buys improved land but only buys unimproved land in the "low f" state (50% chance)
4. The probability an unimproved parcel is upgraded in the next period is 0.25, the probability

that an improved parcel remains so is 0.813

The expected land values over time for a parcel which is initially landlord vs. owner-operator owned are show in Figure A2.

B Data Sources and Sample Construction

B.1 Property Tax Data Sources

B.1.1 Florida

The Florida Department of Revenue maintains a current property tax roll database all its counties available for download from its website. From there, I obtained the 2017 version of the data. The databases include both tabular data which list PLSS section information as well as GIS shapefiles for each parcel. I typically preference the tabular records' codification of PLSS section but in a number of counties this strategy was not viable. In Citrus, Marion, Okaloosa, and Walton counties, a significant fraction of parcels had missing section information. In Clay County, a number of multi-section parcels were misleadingly coded as belonging to a single section. In these cases, I use the GIS map's codification of section and split the value equally by area in cases where multiple sections overlap with the parcel. Significantly, the Florida files document each parcel's primary use, e.g. residential, crops, grazing but unfortunately do not distinguish irrigated from dryland farming.

B.1.2 Kansas

The Kansas Department of Revenue maintains Microsoft Access databases for 94 of their counties. After some negotiation, I purchased the 2017 version of this dataset. These files do not contain GIS information but typically record the PLSS section in which each parcel is located. Most although not all cases where the section information is missing occur in cities or towns where assessors consider the PLSS less relevant. In these cases, I geocode the property address using the ESRI 2013 Composite US Address Locator, provided by MIT Libraries.

B.1.3 Montana

The Montana State Library maintains a cadastral database containing property tax information for every parcel statewide. The Montana Department of Revenue helpfully directed me to the 2017 version of these files. They contain both a GIS shapefile version with limited information and more detailed information in an accompanying SQL database. From the latter, I obtain acreages of agricultural land use and the fraction of building or improvement value devoted to housing. From the shapefile, I obtain parcel total valuation figures and ownership information.

B.1.4 Nebraska

As far as I am aware, there is no public database of Nebraska property taxes. However, a private company named GIS Workshop provides a platform for a large majority of Nebraska's counties to display their property assessment information. I programmed a webscraper that would automatically search and record information from each of the GIS Workshop county websites. Based on the search options available on these sites, it was necessary to search each county by PLSS section and record all the resulting parcels. As a result, parcels without PLSS information are omitted from the dataset. By and large this issue is limited to some properties within cities and towns. As cities and towns form an incredibly small 0.6% percent of Nebraska's sections (Schmidt data), this difference is unlikely to affect any analysis at the section level. See Section F.1 for more results on town formation.

B.1.5 Wyoming

The Wyoming Department of Revenue makes its Computer Assisted Mass Appraisal (CAMA) statewide Microsoft Access database freely available online; this paper uses the 2017 version. In addition to the assessment variables, PLSS section information is provided. However, the database suffers from two drawbacks. First, unlike most other states, a number of Wyoming parcels are quite large and cover multiple sections. Unfortunately, the CAMA database provides no PLSS information on these parcels. I consequently contacted and obtained shapefile parcel maps from the eight Wyoming counties relevant to my sample: Goshen, Laramie, Platte, Albany, Carbon, Sweetwater, Uinta, and Lincoln. These shapefiles were either publicly available online or were

generously provided for free by the assessors' offices. I link the CAMA and GIS databases, allowing me to obtain for each parcel the fraction of its area in a particular section.

B.1.6 Oregon

Although I was interested in obtaining a statewide database of property tax assessments for Oregon, unfortunately to the best of my knowledge none exists. I thus contacted each Oregon county relevant for my empirical design in an attempt to obtain the same information. Equally unfortunate, the fees for many counties prohibited the collection of any data in many cases and of GIS information in a number of others. Ultimately, I was able to obtain quality information from three Oregon counties: Benton, Columbia, and Polk. Other counties provided data with substantial data missingness as to be less than useful.

B.2 County-Level Agricultural and Political Outcomes

I obtain historical county-level data on agricultural outcomes from the US Agricultural Census as recorded in (Haines et al. 2016). I focus primarily on the 1910 agricultural census as it is the first year after the majority of settlement occurred in my sample states. I obtain election results from 1968 and earlier from (ICPSR 1999).

B.3 Grant Boundaries and Sample Construction

The first consideration in constructing my sample was determining which lands were allocated to railroad companies. I limit myself to the six states in which I was able to obtain property tax data[30] and work with all railroad land grants in those states. Using the land grant boundary lines I constructed with the procedure in Section 4.1, I code any PLSS section which intersects them as being within the grant area. Of these, I code odd-numbered sections as being administrated by railroad companies and even-numbered ones as being administered by the federal government. This procedure yields PLSS sections which correspond well although not always perfectly with the federal land records.

A second consideration is that in some instances the federal government allowed railroad

[30]These states are Florida, Kansas, Montana, Nebraska, Wyoming, and Oregon.

companies some flexibility in their choice of lands. For instance, the Pensacola line in Florida gave land within six miles with compensation for land already settled being given out between the six and fifteen mile range from the railroad. Where these policies apply, I treat the outer limit of the grant as the boundary of interest. There are a few examples of railroads being given land outside distance-based formulas. Most of these involve minor deviations in limited areas, although in one major case (the Burlington grant in Nebraska) significant land was allocated without respect to distance. In these cases, I still treat even and odd sections as comparable but do not consider the outer boundaries of the grants as exogenous as the companies had some latitude to select their own lands.

Finally, the federal government reserved specific lands for local governments to fund education. Sections were reserved for this purpose by state based on their number. In my sample for instance, section 16 was reserved in Florida and sections 16 and 36 were reserved in the five other states. Because this policy was based on formula, there is no risk of differential selection in this process. Because these lands were disposed of by state and local governments, they are not be comparable to the more typical case of federal homestead land. I thus drop the so-called "education sections" in all empirical results except where otherwise noted.

This strategy relies on a few important, but reasonable, assumptions. In particular, the requirements for the regression discontinuity could be violated if railroad companies had a role in selecting the boundary for their grants. For instance, the companies might select land that had superior soil quality to any neighboring areas. In the rare cases railroad companies were able to select their lands, this concern is sensible and I thus only include boundaries that are determined formulaically from a distance to railroad tracks. Even in these cases, railroad companies selected large blocks of land at once, making it essentially impossible to invalidate the even/odd comparison. Less plausible also is the concern that railroad companies changed the locations of their tracks to obtain better land at the boundaries of their grants. Historical accounts suggest that the primary motivating factors in track location were the costs of construction and the future traffic on the line (Vance 1961). Even if the companies decided to locate their tracks so as to secure better areas, it is unlikely they would have done so in a way that discontinuously changed at the boundaries. Detailed soil surveys were generally unavailable at the time and few plots of land would have been

so productive as to merit an awkward rerouting of railroad lines to obtain them.

In most cases, my data construction procedures assume I observe the universe of properties within a county. Thus, to compute the total property value within a PLSS section, I add up all the values of properties listed as being within it. In my sample this is a reasonable assumption as most states and counties[31] assess all properties, even ones owned by the state and thus exempt from taxation. In the cases of counties which do not assess government land, I apply the following procedure:

1. If a township contains an unassessed, non-education section, replace the valuation for each section in the township with the satellite-based use valuation described in Appendix Section B.5.

2. If not, maintain the assessor's use value

Since Homestead settlement was difficult or impossible in many areas of poor land quality in Montana and Wyoming, the government retained a significant portion of land in these areas. However, it did not retain land given to railroad companies. As such, the above procedure avoids a problem of differential missing data. Selecting use data by township also maximizes the amount of the assessor's generally more detailed assessments used rather than the more limited, satellite-based measurements.

B.4 Linking to Census Microdata

In many cases, I seek to match property owners to US Census microdata. 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, gender, or race. My procedure can thus only use an owner's name and some very basic location information. 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. In all cases, however, we should expect that the match rates will be substantially poorer than in other applications. In addition to the standard issue that many names are illegible, contain spelling mistakes, or use abbreviations, many ties occur for people with

[31] With the exception of the eleven counties mentioned in Section 4.2

exact name matches.[32] Nonetheless, even in my case matching to Census microdata provides a rich source of information about property owners.

The first step in the linking procedure is determining the desired Census year on which to match. For the historical property tax records described in Section 4.3, I choose the closest Census year: 1900 for the 1900 property tax records and 1910 for the 1912 records. For the initial sales records, I first consider the Census year on or before the date of sale. If this year is earlier than 1880, I use 1880 instead to increase data compatibility. If this year is 1890, I again use 1880 due to the destruction of the 1890 Census microdata.

I next compute a measure of name matching between the property owner and all Census individuals. For both the first and the last name, I compute the Jaccard string similarity index, focusing on bigrams ($q = 2$) between the owner and proposed Census individual. This computes the fraction of unique bigrams in either name that are contained in both names and so naturally ranges from 0% to 100%. In the case of single-letter first names given by property owners, 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." Finally, if a match between the property owner's first name to the full Census first and middle name improves over the Census first name, I use this value instead. I compute the overall name match as the average similarity between the first and last names.

The final 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 also apply a 10 percentage point premium to their score if they reside in the same state as the owner or owner's property respectively.

Given the above scores, the procedure now selects between Census individuals based on the scores computed above. The individual with the highest match score, including location premia, is

[32] For instance, two people named "John Smith" who reside in the same county.

my preferred match. However, I leave the match as missing under the following conditions

1. The match score of the top individual is exactly tied with the second-highest; in the case of the Lincoln County matches I break ties in favor of residents of Lincoln County
2. The string match score of the top individual (excluding location premia) is less than or equal to 75%

Combined, these criteria return only individuals with a high probability of a match. The first criterion excludes cases of exact duplicates, e.g. two "John Smiths" who reside in an owner's county of origin. The second criterion ensures a minimum amount of name similarity. This threshold was determined by inspection: below 75%, there are few plausible name matches due to spelling variations or illegibilities.

B.5 Land Use Value Calculation

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

I begin with the CropScape data on satellite use. This dataset classifies the land use of each "pixel" — a 30 meter by 30 meter square. Pixels may be encoded as one of a number of crops, as pasture or grassland, "developed" areas such as cities or roads, and various other types of natural use such as forests or water bodies. I discuss how I convert each broad category into a use value in what follows.

For crop pixels, I first consider the expected crop yield. I draw these data from the FAO's GAEZ which produces yield estimates at a $\frac{1}{12} \times \frac{1}{12}$ degree resolution. I link each CropScape pixel to a GAEZ polygon based on its centroid and thus obtain yields for each pixel. I use the GAEZ "high input" scenario as this most accurately reflects agricultural processes in developed countries like the United States. A small number of crops[33] are not listed in the GAEZ dataset. In these cases I use USDA-reported average yields for each crop. To compute revenue, I add in data on farmgate prices. I primarily use FAO-reported prices, but in cases where these are missing I use USDA-reported prices or prices from other sources.

[33] In terms of their proportion of land use

For pasture and grassland pixels, I use the USDA ERS survey statistics for the average revenue per cow as being $666.77 per cow.[34] I use the GAEZ yield for "pasture grass" as the expected yield of forage. Following Appendix Section Ahola (2013), I assume an average cow weight of 1000 pounds and that each cow eats 2.6% of its weight per day and that about 30% of forage is available to the herd. This analysis assumes, somewhat generously, that each pasture and grassland pixel is being used for grazing purposes. In practice, satellite data cannot effectively distinguish between used and unused grassland, one advantage of using property assessment data.

For non-developed, non-agricultural pixels, I assume a value of $0 in production. In many places this should be uncontroversial as little economic activity takes place on a mountain, for example. In the case of undisturbed forests, this choice might be more controversial as they arguably provide some value as national parks or for biodiversity. However, in most property assessments they are given a value of $0 to represent the lack of production-based economic activity and I replicate this choice.

To convert each pixel's revenue to a valuation I assume a 10% profit margin for each activity and capitalize the profit stream at 5%, a similar rate to most property assessors. This gives a specific use valuation to each pixel. I link each pixel to a PLSS section via its centroid and add up the total valuation by section, computing a total agricultural use value for each section.

My main measure of use value also includes the valuation from developed areas. The CDL classifies developed areas into "open," "low," "medium," and "high." Since valuations from this use do not come from production, they must necessarily be imputed. I base my imputations on OLS slopes on the property assessors' land valuations with respect to the fraction of land in each type of development and use values of $10 million, $100 million, $500 million, and $500 million per square mile respectively.

[34] See https://www.ers.usda.gov/data-products/commodity-costs-and-returns/commodity-costs-and-returns

C Model Details

C.1 Characterizing the Equilibrium

Once costs are realized, an owner of improved land's profit from using the land as unimproved or improved are respectively: $1 - C_{0,s} + \beta P_0$ and $1 - C_{1,s} - (1 - I)U_s + \beta P_1$. Hence, before costs are realized, settler s has the following valuation for the parcel:

$$V_s(I) = \mathbb{E}\left[\max\left(1 - C_{0,s} + \beta P_0, 1 - C_{1,s} - (1 - I)U_s + \beta P_1\right)\right] \tag{8}$$

Therefore, prices are characterized by the equations:

$$P_0 = \max(0, \min\left(V_S(0), V_B(0)\right)) \tag{9}$$

$$P_1 = \max(0, \min\left(V_S(1), V_B(1)\right)) \tag{10}$$

D County-Level Analysis

D.1 Empirical Framework

For outcomes unavailable at a fine geography, I report results based on an aggregation of the section-level regressions, usually at the county level. To obtain identification here, I rely on the fact that heterogeneous county shapes and locations led to different overall exposure to railroad land grants. My main independent variable here is the fraction of the county that is given to railroad companies, controlling for the average log distance to the grant railroad itself. I use this specification for data from the agricultural census or historical voting results which are difficult or impossible to get at resolutions lower than a county. However, in some cases railroad companies did not actually receive the land promised to it by formula, for example because it overlapped with Native American reservations. Usually when companies were compensated for these losses they had significant latitude in choosing replacement areas. These deviations are therefore unlikely to be random. As such, I instrument for the fraction of railroad land within a county with the fraction

of formula-promised land in a county.

The exact estimating equations are:

$$y_i = \alpha FracRR_i + \beta \overline{\log(d)}_i + X_i\gamma + \varepsilon_i \tag{11}$$

$$FracRR_i = \pi FormulaFracRR_i + \beta_{FS} \overline{\log(d)}_i + X_i\gamma_{FS} + \varepsilon_i \tag{12}$$

where i is a geographic unit, such as a county; $FracRR_i$ is the fraction of land within i that is granted to railroad companies; $FormulaFracRR_i$ is the fraction of land promised to railroad companies by formula; $\overline{\log(d)}$ is the log distance to any grant railroad, averaged within all of i; and X_i are other controls.

D.2 Tenancy

In order to directly measure the impacts of land concentration on tenant farming, I use the fraction of land operated by non-owners in the 1910 Census of Agriculture at the county level and employ estimates of equation 11. The IV estimates use the fraction of formula-promised railroad land as an instrument for actual railroad land as discussed in Section D.1. These regressions dispose of the need to use proxy variables to measure tenancy although they make use of county-level rather than section-level variation in railroad land grants.

Estimates of equation (11) in Table A5 show that railroad land grants increase tenancy at the county level. Column (1) presents results accounting only for average log distance to the grant railroad and state fixed effects and using OLS estimation. Since deviations from land grant formulas may be non-random, columns (2)-(5) instrument for the fraction of railroad land with the fraction of the county's land which overlaps the formulaic version of the grant boundaries. Column (3) adds geographic controls, column (4) adds latitude, longitude, and (log) county area. Column (5) restricts the sample to areas within 40 miles of a grant railroad, addressing a potential concern that the regression simply compares places close to and far from railroads. All specifications indicate that railroad land grants increased tenancy. Depending on the estimate, a county that went from

no railroad land to being entirely given over to a company would experience a rise in tenancy of 14 to 32 percentage points or roughly 50% to 115% of the sample mean.

D.3 Political Economy

A natural outcome to examine when considering whether land concentration led to elite capture is voting. Historical voting statistics are generally only available at the county level and not more granularly. I thus employ estimates of equation (11) to test whether railroad land grants led to more pro-elite voting.

Classifications of major American political parties into pro- or anti-elite categories is a subjective matter, but the 1892 presidential election offers a clear distinction. This election featured the brief rise of the Populist or People's Party whose platform advocated for reducing the inequalities of America's Gilded Age. Nationally, the party supported a progressive income tax, the regulation of monopolies, and increased money supply through bimetallism. Its policies with respect to agriculture were more diverse, but small farmers' Populist support was often motivated by concerns with the tenancy system, opposition to land concentration, and other conflicts with large landowners (Holmes 1990; Rochester 1943). Table A6 presents county-level estimates of railroad land grants' impact on this party's vote share. Specifications of equation (11) are the same as those in Section 5.3. The results indicate that the presence of landlords, if anything, increased anti-elite voting; estimates indicate that a county fully given to a railroad would vote 13 to 38 percentage points more for the Populist Party. Rather than elite capture, railroad land grants seemed to have set off an elite backlash.

E Land Use Value Calculations

I quantify the overall misallocation by combining satellite data on land use with agroeconomic models of crop productivity. The USDA's Cropscape project provides satellite-based data on land use in 30 meter $\times$ 30 meter squares across the country. Combining the 2017 CropScape data, the FAO's GAEZ models of crop yields (Fischer et al. 2012), and FAO/USDA crop prices, I compute the revenue for each pixel as the product of yield, area, and price. I assume a fixed 10% profit margin for any crop and convert all income figures into a present discounted value, producing essentially a

satellite-based property assessment.

F Further Alternate Mechanisms

F.1 Agglomeration

Another explanation I rule out is that these results are caused by some form of agglomeration. There are two forms that this concern might take. The first is that the effects are driven by a choice of city and town location. That is, perhaps concentrated land ownership helped or hindered certain areas from being "first movers." In this view, the aggregate welfare effects are minimal as they merely determined the location of towns, not their total number. To test whether this is the case, I use two measures of town creation. The first is the fraction of each section's area that is part of a US Census "place" in 2000. The second is a dummy variable for whether the section contains a town in 2000 according to the (Schmidt 2018) dataset.

The effects of railroad land grants on town formation are small or nonexistent and cannot be a major driver of the main results. Less than 2% of the sample's area is part of a Census place and about 0.4% of sections contain a town in the Schmidt data. Nonetheless, to estimate the exact role town formation plays in the story, Table A8 reports estimates on town formation. The largest effect is a marginally significant positive impact on town formation from railroad grants, on the order of a 0.1% increase in the number of towns. This result is not robust to using the alternate measure of town formation and has both the wrong sign and magnitude to explain the main results.

F.2 Speculation

Another concern with the results is that they might reflect the actions of owners purely engaged in speculative investments rather than agricultural activities of any sort. These owners would have held the land off the market entirely, waiting for its value to increase before selling it. However, evidence from Banner County indicates that sales were relatively frequent and fairly similar across railroad versus federal sections. Appendix Figure A7 plots the fraction of parcels in Banner County that were transferred at least five times. Sales were quite frequent and both even and odd sections experienced many beginning in 1900. If anything, railroad lands were transferred more often

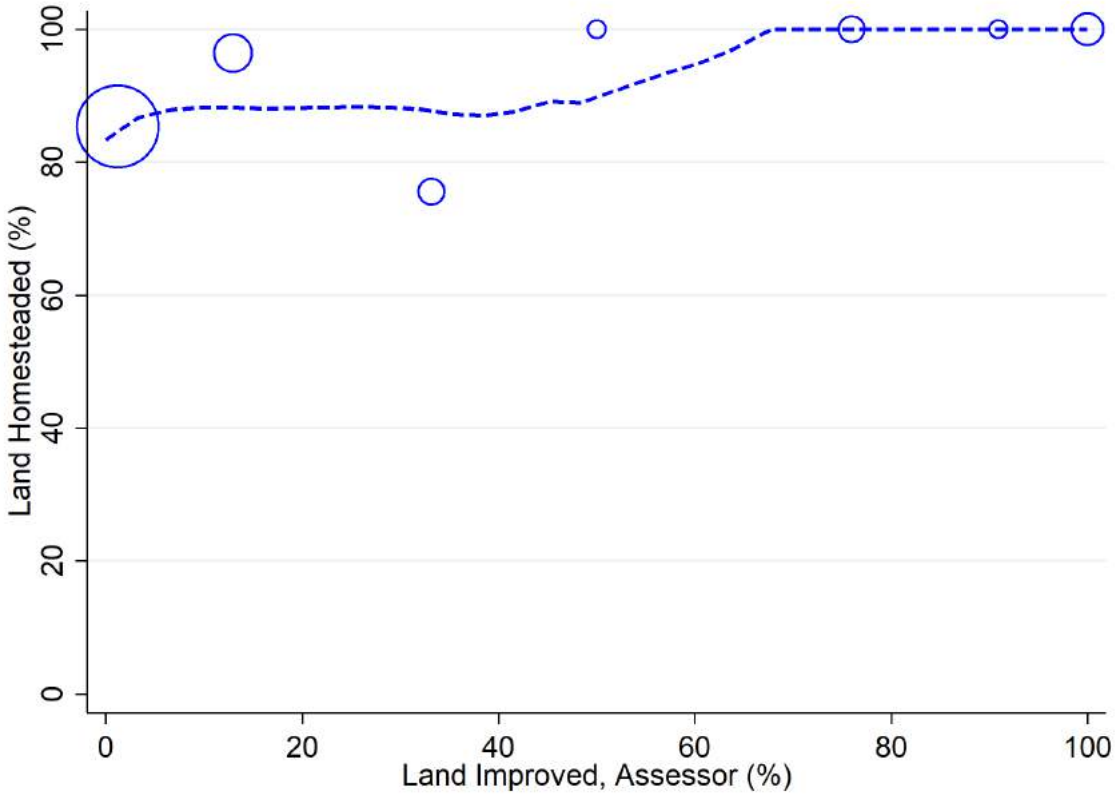
although their rate of increase in total owners eventually parallels that of the federal sections.

F.3 Federal Settler Characteristics

One potential concern with the spillover regression would be that settlers sorted themselves based on their neighbors or in response to some other policy. With respect to the latter, the federal government had hoped that the plots it retained in railroad grant areas would double in value, thus holding fixed its land value once half had been given to railroad companies. Still, in practice the government struggled to sell at these rates and, in any event, the free Homestead option was broadly available (Gates 1954). While there is no comprehensive database of offered prices of federal lands, I test for sorting of federal settlers in Table A9 which reports estimates of equation (4). In terms of their obtained acreage, dwelling type, education, and occupational income score, there is no difference across the boundary. Thus it is unlikely that self-selection or differential federal policies were the cause of contemporary differences across the grant boundary.

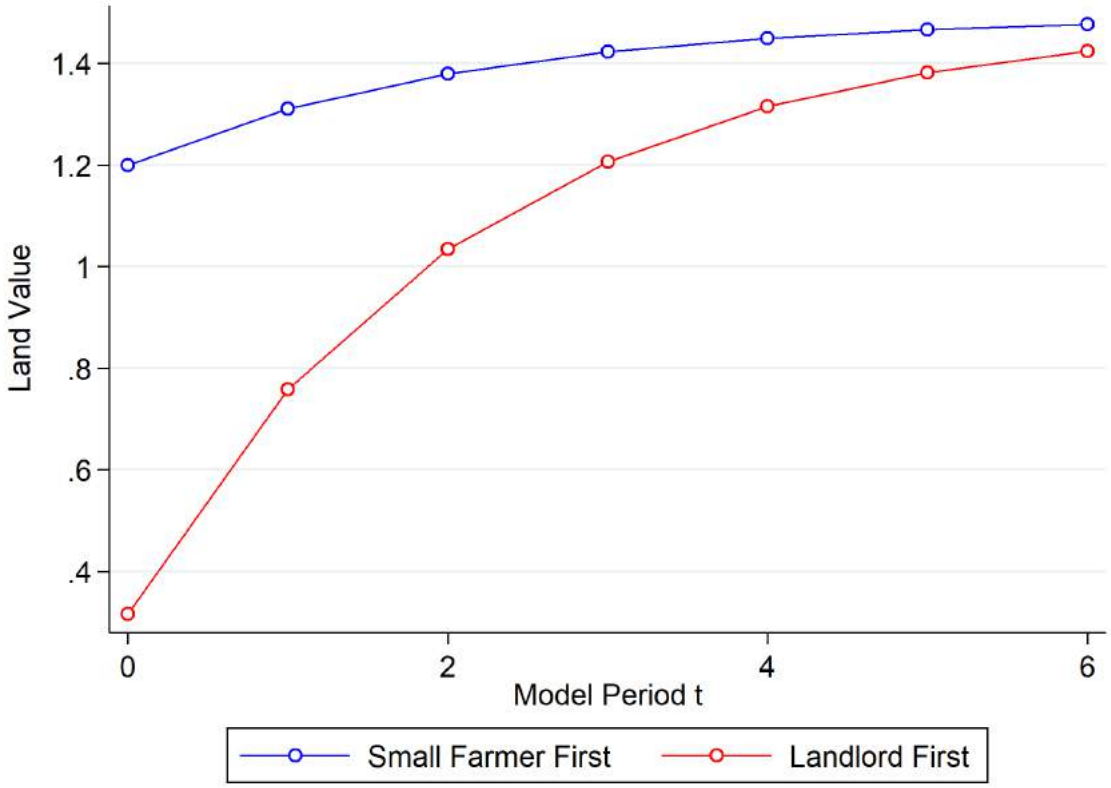
G Appendix Figures and Tables

Figure A1: Free Land and Improvements, Morrill County 1912



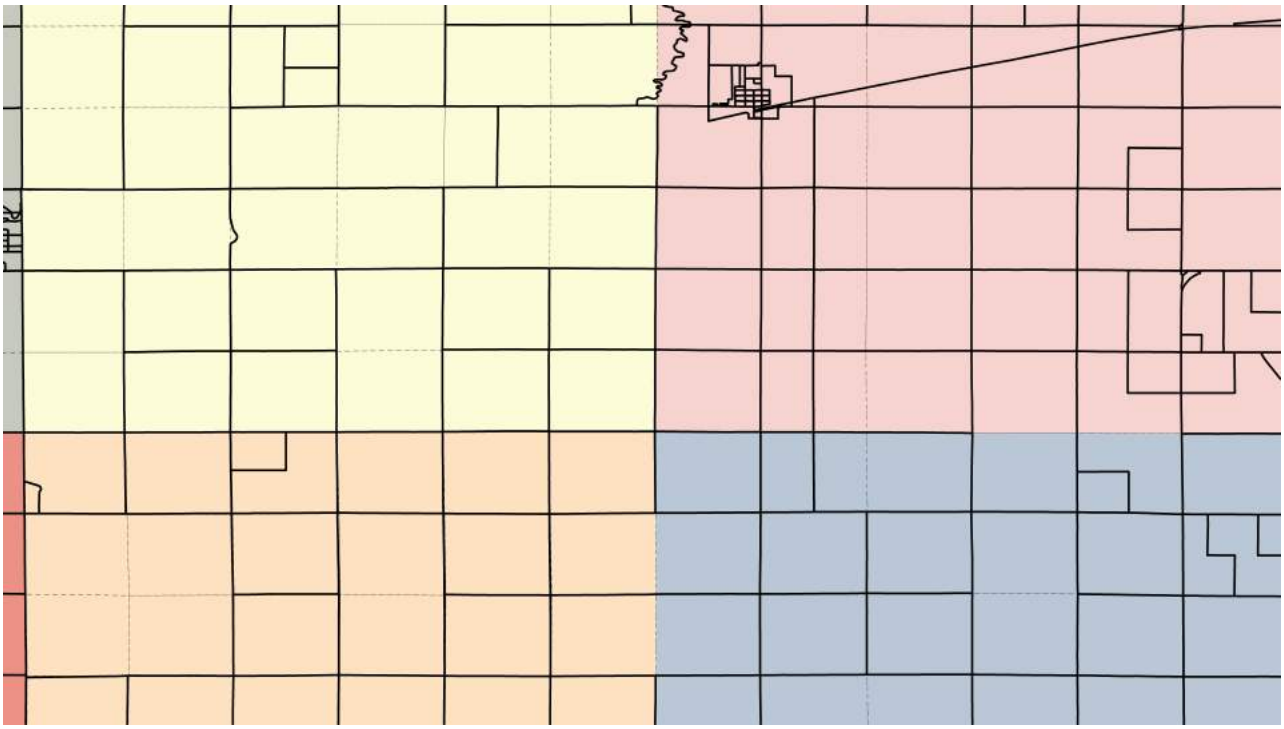
Notes: This figure graphs the fraction of land settlers in a given section received for free under the Homestead Act and odifications as a function of the fraction of land marked as improved by the property assessor. Data come from the even, non-education sections of the railroad grant area of Morrill County in 1912.

Figure A2: Land Values in Model Example



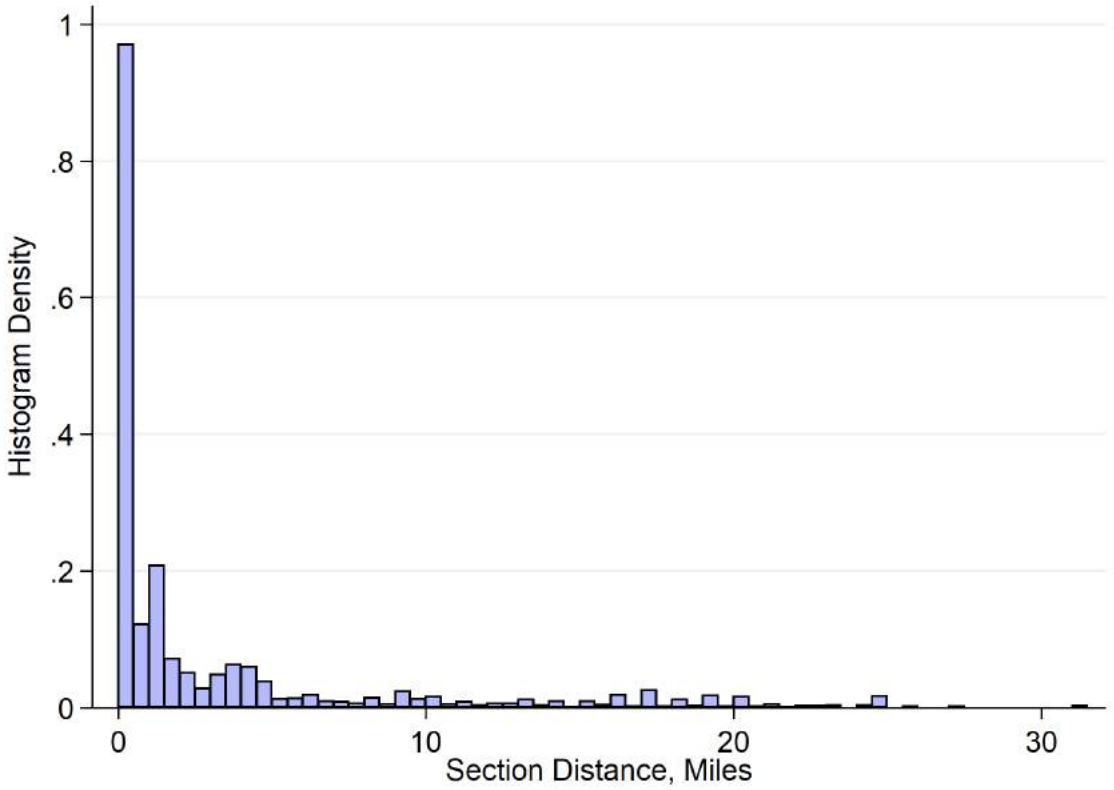
Notes: This figure graphs the expected land value for a parcel initially owned by a small farmer versus one owned by a landlord in the context of the numerical example of the model described in Appendix Section A.3.

Figure A3: US Census Blocks and the PLSS



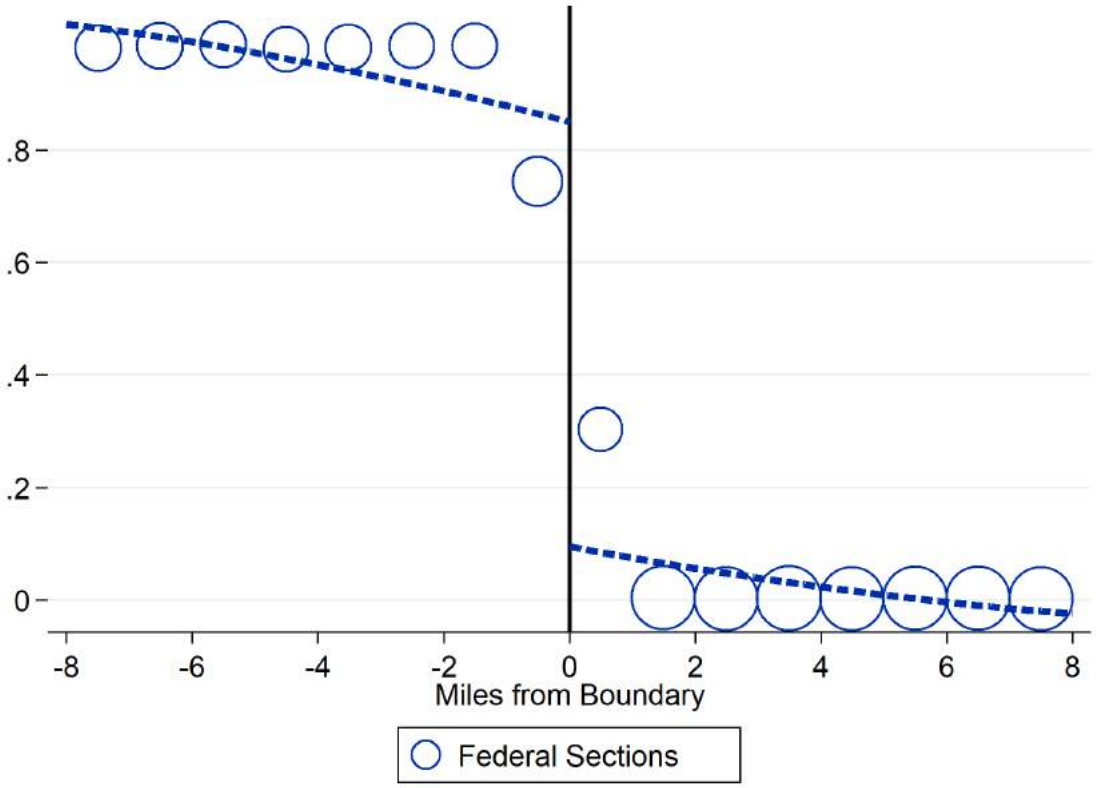
Notes: 2000 US Census Blocks (bold) overlaid with PLSS Grid, Nebraska

Figure A4: Distance of Property Expansions, Banner County



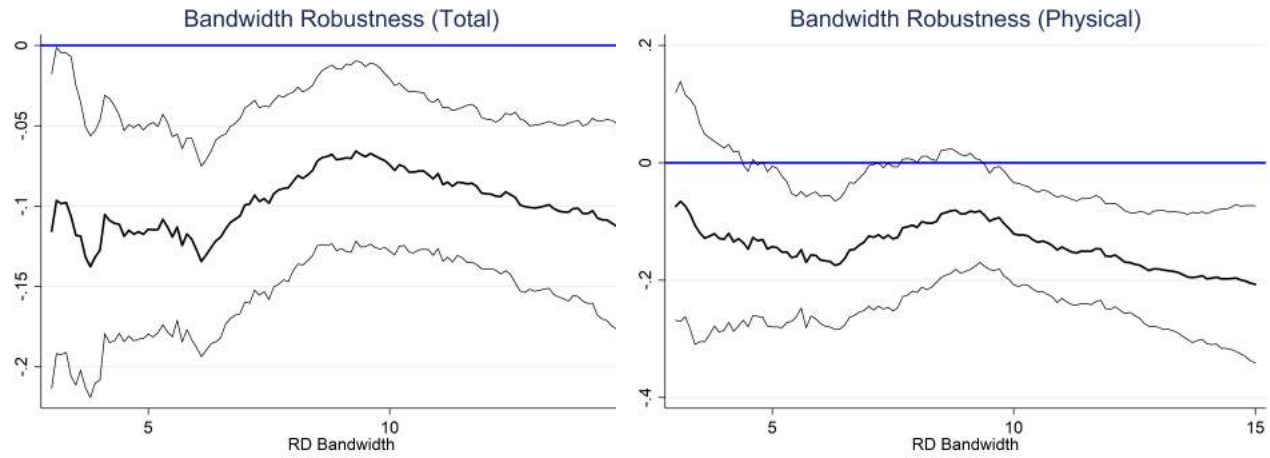
Notes: This figure illustrates the distance between the PLSS sections of an owner's original property and an owner's subsequent expansions. The leftmost bar represents cases in which an owner expanded into sections adjacent to those containing their original property. Data come from the Banner County, Nebraska recorder of deeds.

Figure A5: Neighboring Concentrated Land



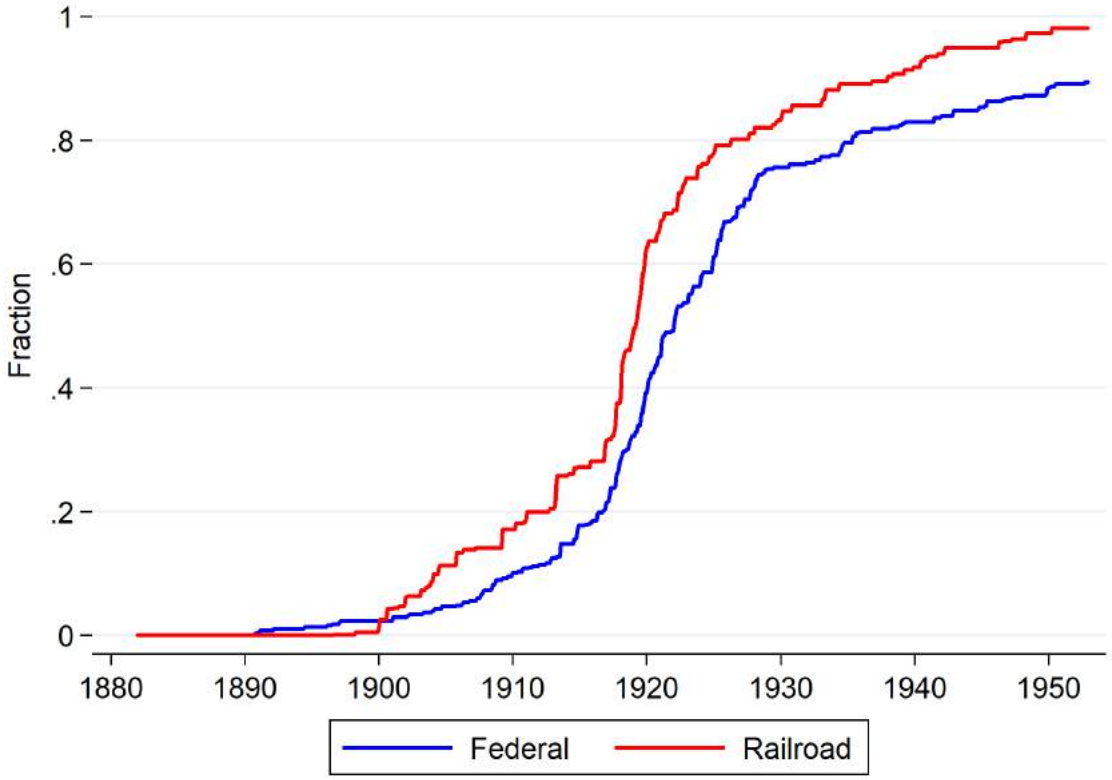
Notes: This figure illustrates the implicit first stage of the spillover RD design. Considering only federally-administered sections, it shows the fraction of neighboring PLSS sections subject to concentration due to railroad administration.

Figure A6: Bandwidth Robustness



Notes: This figure shows the robustness of estimates of the spillover RD equation (4) to bandwidth choice, displaying (a) impacts on (asinh) total property value and (b) (asinh) investment with a bandwidth range of 3 to 15 miles.

Figure A7: Fraction of Parcels with 5+ Transfers, Banner County



Notes: This figure depicts the fraction of railroad versus federal lands in Banner County, Nebraska that were transferred at least five times up to that point.

Table A1: Initial Buyer Characteristics

	(1) (log) Acres	(2) Farm Home	(3) Literate	(4) Occ. Score
RR Effect	1.37***	-15.7***	5.87**	4.87***
	(0.16)	(4.09)	(2.89)	(1.58)
log(RR Distance)	-0.13**	3.37	0.20	-0.85
	(0.062)	(2.48)	(2.96)	(0.85)
Area	RR	RR	RR	RR
Geo Controls	Y	Y	Y	Y
Township FEs	Y	Y	Y	Y
SEs / Clusters	Township	Township	Township	Township
N	1,591	1,239	622	880
N (clusters)	67	67	65	67
$\mathbb{E}[y]$	6	53	92	14

Notes: This table estimates equation (3) to test for differences between the initial buyers of railroad and federal land for Lincoln County, Nebraska. Data on buyers were linked to the most recent census prior to the purchase, except when the purchase occurred before 1880 in which case they were linked to the 1880 census. Column (1) reports differences in (log) acreage, column (2) reports the percentage living on a farm, column (3) reports the percentage that could both read and write, and column (4) reports their average occupation income score (IPUMS occscore). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A2: Owner Distance and Settlement Sparsity

	Consistent Settlement		Sparse Settlement	
	(1) Government (%)	(2) (log) Distance	(3) Government (%)	(4) (log) Distance
RR Effect	0.17	0.059***	-14.2***	-0.097
	(0.11)	(0.0061)	(5.22)	(0.064)
Unsettled (%)	4.4%	4%	33%	24%
Geo Controls	Y	Y	Y	Y
County FEs	Y	Y	Y	Y
Township FEs	Y	Y	Y	Y
SEs / Clusters	Spatial	Spatial	Spatial	Spatial
N	54,039	52,222	77,504	64,584
$\mathbb{E}[y]$	3.3%	42 mi	33%	168 mi

Notes: The table shows the effect on government ownership and owner distance based on the level of settlement in the states. Columns (1)-(2) cover states where unsettled sections are rare: Florida, Kansas, Nebraska, Oregon. Columns (3)-(4) cover states where large areas were never settled: Montana and Wyoming. All columns use the even-odd comparison of equation (3). Columns (1) and (3) report effects on the percentage of land legally owned by government entities. Columns (2) and (4) report effects on the (log) owner distance to their property. The table also reports the percentage of even sections within the sample which were never settled according to BLM records. Conley standard errors are in parentheses and geographic controls are the same as in Table 1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3: Geographic Predictions of Tenancy

	(1) Farms Owned	(2) Share Tenants
Soil Quality	-0.072***	0.13***
	(0.015)	(0.026)
Soil Quality, Fraction Lowest	-0.20**	0.078
	(0.096)	(0.095)
log(RR Distance)	0.0042	-0.022**
	(0.0044)	(0.0096)
Elevation (km)	0.054	0.42
	(0.14)	(0.32)
log(Area)	-0.28***	0.22
	(0.081)	(0.20)
Terrain Slope	0.0029	-0.0097***
	(0.0029)	(0.0037)
Stream Miles	0.023	-0.043
	(0.075)	(0.13)
State FEs	Y	Y
State × X,Y	Y	Y
SEs	Robust	Robust
N	311	311
$\mathbb{E}[y]$	.61	.4

Notes: This table predicts county-level tenancy characteristics in 1940 with geographic ones. In column (1) the outcome is the 1940 fraction of farms operated by their owners. In column (2) the outcome is the fraction of all tenants who operate as share tenants. Predicting variables are the same as for the main specifications, including a linear function in latitude and longitude by state. These characteristics are averaged by PLSS section across the county. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Modern Owner Characteristics

	(1) (asinh) Land Owned	(2) (asinh) Land Owned	(3) (log) Owner Distance	(4) Company (%)	(5) Government (%)
RR Effect	-0.15	0.041***	0.037***	6.46***	-8.27**
	(0.20)	(0.014)	(0.014)	(2.41)	(3.49)
Sample	All	Non-gov	Non-gov	All	All
Geo Controls	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	131,543	35,670	34,221	131,543	131,543
$\mathbb{E}[y]$	262 mi^2	11 mi^2	60 mi	27%	21%

Notes: This table reports differences in modern owner characteristics according to the direct comparison equation (3). Columns (1)-(2) use the (asinh) number of parcels owned, adjusted for approximate section size. Specifically, it treats each parcel as the same fraction of a PLSS section and records the aggregates total number of fractional sections owned by each owner. Column (1) uses the full sample and column (2) uses the sample of townships without government ownership. Column (3) reports the average (log) distance of an owner to the section's centroid. Columns (4) and (5) report differences on the percent of land owned by corporations or the government. Geographic controls are the same as in Table 1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: Fraction Farmland Non-Owner Operated 1910

	(1) OLS	(2) IV	(3) IV	(4) IV	(5) IV
RR Area	13.6**	17.9	28.1***	20.5**	31.7***
	(6.58)	(11.8)	(10.2)	(9.25)	(10.1)
(log) RR Dist	1.02	1.65	4.82***	2.99**	4.48**
	(1.34)	(2.01)	(1.61)	(1.50)	(1.87)
Sample	All	All	All	All	< 40 miles
SEs	Robust	Robust	Robust	Robust	Robust
State FEs	Y	Y	Y	Y	Y
Geo Controls			Y	Y	Y
X,Y, area				Y	Y
N	264	264	264	264	167
$\mathbb{E}[y]$	28	28	28	28	28

Notes: This table estimates the effect of railroad land grants on tenancy at the county level in 1910. The independent variable is the fraction of land in a county allocated to railroad companies. Controls are the average (log) distance to a grant railroad in a county, [geographic] mean elevation, average terrain slopes, miles of stream, soil quality, [other] county latitude, longitude, and (log) area. Column (1) reports an OLS regression. Columns (2)-(5) instrument the railroad land fraction using the fraction of land in a county that overlaps the railroad grant area if the formula had been perfectly applied. The baseline sample is all counties in the main regressions, i.e. restricted to cases where I have property tax information. Column (5) further restricts to counties which are on average 40 or fewer miles from a grant railroad. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6: Fraction Vote for Populist Party, 1892

	(1) OLS	(2) IV	(3) IV	(4) IV	(5) IV
RR Area	16.1***	15.2*	12.7	15.6**	37.8***
	(5.50)	(8.56)	(8.34)	(7.83)	(8.23)
(log) RR Dist	1.82	1.70	0.96	1.77	7.28***
	(1.11)	(1.40)	(1.33)	(1.28)	(1.71)
Sample	All	All	All	All	< 40 miles
SEs	Robust	Robust	Robust	Robust	Robust
State FEs	Y	Y	Y	Y	Y
Geo Controls			Y	Y	Y
X,Y, area				Y	Y
N	247	247	247	247	157
$\mathbb{E}[y]$	39	39	39	39	39

Notes: This table estimates the effect of railroad land grants on the Populist Party's 1892 presidential vote share. The independent variable is the fraction of land in a county allocated to railroad companies. Controls are the average (log) distance to a grant railroad in a county, [geographic] mean elevation, average terrain slopes, miles of stream, soil quality, [other] county latitude, longitude, and (log) area. Column (1) reports an OLS regression. Columns (2)-(5) instrument the railroad land fraction using the fraction of land in a county that overlaps the railroad grant area if the formula had been perfectly applied. The baseline sample is all counties in the main regressions, i.e. restricted to cases where I have property tax information. Column (5) further restricts to counties which are on average 40 or fewer miles from a grant railroad. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A7: Spillover Effects on (asinh) Property Values

	(1) Base	(2) County	(3) Donut	(4) All odds
RR Effect	-0.11***	-0.11***	-0.17**	-0.095***
	(0.040)	(0.044)	(0.074)	(0.034)
Geo Controls	Y	Y	Y	Y
County FEs	Y	Y	Y	Y
SEs / Clusters	Spatial	County	Spatial	Spatial
N	23,382	23,382	19,845	25,142
$\mathbb{E}[y]$	\$1,755k	\$1,755k	\$1,806k	\$1,773k

Notes: This table shows robustness of the estimates of the spillover RD equation (4) to specification. Column (1) reports the baseline specification. Column (2) reports standard errors clustered by county. Column (3) reports a donut regression which drops all sections within 1 mile of the boundary. Column (4) includes all odd sections to the right of the boundary. Column (5) restricts to areas with low soil quality. Geographic controls are the same as in Table 1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A8: Effects on Town Formation

	Direct		Spillover	
	(1) Places	(2) Towns	(3) Places	(4) Towns
RR Effect	0.00034	0.0010*	0.0045	0.00062
	(0.00024)	(0.00059)	(0.0034)	(0.0016)
Sample	RR	RR	All	All
Geo Controls	Y	Y	Y	Y
County FEs	Y	Y	Y	Y
Township FEs	Y	Y		
SEs / Clusters	Spatial	Spatial	Spatial	Spatial
N	132,463	132,463	23,382	23,382
$\mathbb{E}[y]$	0.0	0.0	0.0	0.0

Notes: This table tests for effects of railroad land grants on town formation. Columns (1)-(2) estimate the direct comparison equation (3). Columns (3)-(4) estimate the spillover RD equation (4). Columns (1) and (3) use the fraction of a section's area that is part of a Census Place in 2000. Columns (2) and (4) use the number of town centroids within the section as the outcome. Geographic controls are the same as in Table 1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A9: Initial Federal Settler Characteristics Spillover

	(1) (log) Acres	(2) Farm Home (%)	(3) Literate (%)	(4) Occ. Score
RR Effect	-0.056	2.10	2.28	0.035
	(0.049)	(3.37)	(1.73)	(0.27)
Geo Controls	Y	Y	Y	Y
County FEs	Y	Y	Y	Y
SEs / Clusters	Spatial	Spatial	Spatial	Spatial
N	17,383	5,912	2,151	5,125
$\mathbb{E}[y]$	5.8	56	91	14

Notes: This table estimates the spillover impacts of railroad land grants according to the RD equation (4). The sample is federally-administered sections: all even-numbered sections within railroad grant boundaries and all odd-numbered sections at least one mile out of the boundaries. Column (1) reports effects on the log owner acreage of initial property buyers. Column (2) reports effects on the percentage of owners whose home is a farm. Column (3) reports effects on the percentage who are literate and column (4) reports on the average occupational income score. Geographic controls are the same as in Table 1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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