Establishing the Land of Opportunity: Land Transfers and Economic Mobility in American History *

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March 2026

Abstract

Land rights are often seen as a cornerstone of economic development. Modern "land reforms" that redistribute land rights are often proposed as a way of addressing economic inequality. Despite a growing interest in the relationship between land ownership and economic mobility within and between generations in U.S. economic history, scholars have not examined the role of massive transfers of land from the public domain to private ownership during the 19th and early 20th centuries. We fill this gap by analyzing the Homestead Act and cash sales representing transfers totaling nearly 500 million acres over 1850-1940. We link individual land patent records to full-count historical censuses to provide a comprehensive account of who participated in these land transfers, how their economic standing evolved over time, and intergenerational implications. Results vary across measures of economic standing, era, and geography, but homesteaders consistently perform worse than individuals who purchased land. In contrast, the children of early homesteaders—particularly those in low-status occupations—see greater upward mobility than cash buyers and the population at large.

*Financial support for research was provided by the National Science Foundation (SES, Grant no. 2117660). The authors do not have any financial conflicts of interests to disclose. We thank Kristina Avery for excellent research assistance and several students working at the Brigham Young Record Linking Lab on this project. For helpful comments we thank Doug Allen, Leah Boustan, and seminar participants at the Mountain West Economic History Conference, Simon Fraser University, the University of Victoria, and the University of Wyoming. The errors that remain are ours alone.

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"[T]o the energetic and industrious man, without means, [the Homestead Act] has opened avenues unnumbered to independence and wealth "

-General Land Office Report, 1870

"[B]ut the promise of cheap Western land to the common laborer was as futile as a signboard pointing to the end of a rainbow"

-Fred A. Shannon, 1936

1 Introduction

In the past two decades, economists have made significant strides in measuring economic mobility over various time periods and in a number of countries. (Chetty and Hendren, 2018; Connor and Storper, 2020; Abramitzky et al., 2021; Boustan et al., 2025). This literature has also sought to understand how various factors influence economic mobility, including migration decisions (Abramitzky et al., 2014a, 2021), race (Collins and Wanamaker, 2022; Derenoncourt et al., 2023, 2024), the place where one grew up (Chetty and Hendren, 2018), wealth shocks (Ager et al., 2021), and bequests (Adermon et al., 2018). For the U.S. in particular, a common theme across many historical studies of economic mobility —implicitly or explicitly—is the ownership of land.

Most recently, scholars have document that differential rates of land ownership are an important driver of racial wealth gaps over time in the U.S. (Collins and Wanamaker, 2022; Derenoncourt et al., 2023, 2024). More broadly, the wide availability of "open" land to a wide swath of the population on the American frontier was historically unique, and so looms large in many accounts of American cultural and economic development (Turner, 1894; Donaldson and Hornbeck, 2016; Bazzi et al., 2020).[1] In the context of economic mobility, this abundance of land has been connected to relatively high rates of intergenerational mobility in the U.S. in the late nineteenth century (Long and Ferrie, 2013) and to especially high upward mobility for sons of farmers (Song et al., 2020).[2] These studies, however, treat land ownership as a *status* rather than grappling with the *process* of land acquisition and its implications for mobility. A notable exception is (Bleakley

[1]The "frontier" in American history has largely been characterized as the edge of settlement by those of European descent, but it is misleading to suggest that these lands were previously unoccupied (Feir, 2025). In fact, policies to pull non-natives towards the Western US were arguably intended to aid in extinguishing native claims to those lands (Allen, 1991; Frymer, 2017; Allen, 2019).

[2]However, more recent scholarship has questioned the overall magnitude of intergenerational mobility during this era (Ward, 2023).

and Ferrie, 2016), who study the intergenerational consequences of land acquisition using a land lottery in Georgia in the 1830s, finding limited evidence of gains for the children of winners.

Understanding the process of land acquisition is especially important in the United States, where the federal government transferred over one billion acres of the public domain to private owners between 1781 and 1940 (Vincent et al., 2020). This transfer was not a monolithic geographic phenomenon, nor was it random. Rather, it was a patchwork of specific land allocation policies that often came with conditions and induced selection. Much of the land was sold for cash, but much more was given away, both to transcontinental railroads and, most famously, to individuals via the 1862 Homestead Act (Allen, 2019).

While other countries have since attempted their own versions, homesteading in the US is perhaps the most expansive policy through which land was given to "bona-fide settlers" at a sale price of zero (Weaver, 2003). The Act transferred over 270 million acres to over 1.6 million people, implicating 93 million descendants today. Furthermore, the take-up of lands occurred across a broad range of geographies, from Florida to Michigan, Arizona to Washington, and all through the Great Plains. The homesteading era also spanned from the Civil War to World War II [3]. In other words, there is a wide range of contexts in which the settlers sought and received the free land.

However, to date, there has been no systematic analysis of how homesteading and other US land policies, relate to the distribution of land ownership and economic mobility in US history. We fill this gap by linking the universe of original land entry records ("patents") to individuals in the census over 1850–1940 to understand who obtained land, how their economic performance evolved over time, and whether their children experienced gains (or losses) as a result. First, we compare the attributes of homesteaders to cash buyers of land and those that did not secure land titles from the U.S. government. Second, we explore how the homesteaders and purchasers fared, considering numerous comparison groups across geographies and frontier migration decisions. Finally, we link homesteaders, cash buyers, and patentees to their children to explore whether they experience greater intergenerational mobility.

We overcome a sparse-data linking problem created by the fact that land patents provide no

[3] Homesteading continued through 1988 in Alaska, but was largely brought to a halt in 1934 by the Taylor Grazing Act reform which moved most of the unclaimed land under the control of the modern Bureau of Land Management

personal identifying information beyond name. We develop and deploy an algorithm to match the universe of federal land patents to the full 1850 to 1940 census counts which balances a high match rate with avoiding false matches based on common names by using the precise location of land patents to discipline matches based on distances to places identified in census records (Berkes et al., 2023). Of the 2.66 million homestead and cash land patents issued between 1860 and 1940, we match nearly 800,000, or 30 percent across 28 homestead states. The match rate is higher for homesteaders (43%), as they were more likely nearby their land at the time of the census.

Broadly, we find that there is negative selection of homesteaders in that they came from lower socioeconomic backgrounds than their peers, which we define in a variety of ways. While the performance of homesteaders relative to non-patentees varies across economic indicators and time periods, we find that cash buyers of land see larger post-patent improvements than homesteaders across every measure of economic status, comparison group, and time period. Finally, we find little evidence of strong intergenerational mobility of homesteaders, with the noted exception of the sons of low-status fathers who obtained homesteaders prior to 1900. Though necessarily descriptive, these results are broadly consistent with previous quasi-experimental findings that the Cherokee Land Lottery did not boost the lower tail of the wealth distribution (Bleakley and Ferrie, 2013) and that the Georgia Land Lottery did not result in substantial intergenerational transmission of wealth (Bleakley and Ferrie, 2016)

Our efforts here provide the most definitive descriptive account of historical land acquisition and economic mobility across the geography of the United States, with focus on land ownership as a process, rather than a static characteristic. We also unpack the "frontier mechanism" of mobility (Long and Ferrie, 2013; Bazzi et al., 2020) to show that land acquisition and the institutional terms of that acquisition matter (at least) as much as geography. Related, these insights provide nuance to the property rights literature, illuminating how the conditions attached to land redistribution policies can influence the sort of individuals that seek title and the subsequent outcomes (Galán, 2024).

Our study has several limitations. For one, our data is conditional on successfully homesteading (staking a claim and remaining on the land for at least five years to receive title). In aggregate, it is estimated that up to 66 percent of homestead entries were not, for one reason or other, proved up and title secured. This means, as a policy, the structure of the Homestead Act may have induced millions to forego other opportunities to try to homestead but come up empty-handed. Our study also focuses primarily on white (male) claimants. Black Americans received only 0.3% of successful homestead patents and faced distinct institutional barriers. Women, too, composed a small but important share of homesteading claims. These populations warrant separate, dedicated analysis.

In what follows, we first provide background on federal land policy in the US and describe mechanisms through which generous federal land transfers could facilitate economic mobility. Second, we provide a description of the data set we construct to attach personal information to land patentees and methods we deploy to measure economic status. Results are broken into three sections: descriptive statistics to assess how patentees compared to others; panel analyses to assess how land patentees fared relative to others before vs. after obtaining land; and intergenerational estimates on economic mobility of children of patentees. Finally, we conclude with a discussion of the findings and their implications for understanding the role of federal land policy for economic mobility in American History.

2 Background

2.1 Historical Land Disposal in the United States

The U.S. federal government obtained legal title to the lands stretching westward from the original thirteen colonies to the Pacific Ocean through a variety of treaties with and purchases from other colonizing nations including Great Britain, Spain, and France (Gates, 1968). The pre-existing territorial claims of the indigenous peoples living there were subsequently and systematically reduced to relatively small reservations through a series of military conquests, treaties, purchases, and outright theft by settlers and the federal government alike (Anderson and Mc Chesney, 1994; Spirling, 2012; Feir, 2025). The result was that by the middle of the nineteenth century, the U.S. federal government had *de jure*, if not *de facto* ownership of the majority of what is today the continental U.S., and could manage or dispose of this "public domain" as it saw fit.

To a large degree, the U.S. government chose the latter course, and passed nearly 1.3 billion acres into private ownership between 1781 and 1940, roughly 72% of its original 1.8 billion-acre landholdings (Vincent et al., 2020).[4] The scale of this transfer was unprecedented at the time and remains one of the single largest episodes of land privatization in history (Weaver, 2003). Through the mid-nineteenth century, federal land disposal evolved through a series of experiments with different pricing schemes, allowing purchases on credit, and varying degrees of acquiescence to "pre-emption" claims by squatters on land that was not technically available for settlement (Gates, 1968). Despite several attempts, the U.S. never managed to turn its land sales into a significant source of public revenue, instead favoring relatively generous policies geared toward promoting broad land ownership and encouraging settlement across its massive territory over which it held somewhat tenuous sovereignty (Allen, 1991; Frymer, 2017; Allen and Leonard, 2026).

The 1862 Homestead Act and its unique features stand out in historical accounts of Westward Expansion. Under the Act, settlers could obtain up to 160 acres for "free" by paying a nominal ($10–$18) filing fee, making improvements to the land, and occupying it for five years. Settlers who successfully "proved up" their homestead obtained fee simple title to the land with no further strings attached, amounting to a roughly $200 subsidy relative to the minimum sales price of $1.25 per acre (Gates, 1968). There were a variety of subsequent smaller related acts that relaxed the 160-acre restriction or the 5-year occupancy requirement but imposed other restrictions, including dictating specific uses (such as irrigation) or reserving the mineral rights to the federal government, but most settlers opted to enter land under the terms of the original 1862 Act (Allen and Leonard, 2025). Ultimately, roughly 270 million acres—an area the size of Texas and California combined—was patented under the Homestead Acts. Homesteading was divided into two major waves: the first occurred prior to 1890 and then a second, larger wave of homesteaded succeeded it over 1900–1930 (Allen and Leonard, 2025). Throughout both periods, land sales also continued at a minimum price of $1.25 per acre that was sometimes bid up via auctions for especially desirable land (Allen and Leonard, 2021), amounting to roughly 130 million acres in total.

The political discourse surrounding the passage of Homestead Act was rooted in Lockean notions of obtaining property by mixing one's labor with the land, and in the Jeffersonian democratic ideal of "yeoman farmers" as the backbone of an agrarian society (Hibbard, 1939). Contemporary writers and politicians characterized the Homestead Act as an engine of economic mobility that opened "avenues...to independence and wealth" (United States General Land Office, 1870) and

[4] 1.29 billion acres were transferred over 1781 to 2018, with 97% (or 1.26 billion acres) occurring prior to 1940.

as a means to obtain "wealth, social privileges, and political honors" (Copp, 1880). In more recent times, the Homestead Act has been lauded by leading lights across the political spectrum including John F. Kennedy who called it "Probably the greatest stimulus to national development ever enacted" (Kennedy, 1962), Barack Obama (Obama, 2009), George W. Bush (Bush, 2005), and George Will, who quipped "Rarely has a social program worked so well" (Will, 2005).

Economists have been less sanguine about the implications of the Homestead Act. Though homesteading did initially lead to a more equal distribution of land ownership, subsequent exchange and consolidation caused landholdings in heavily homesteaded counties to converge toward non-homestead counties within a matter of decades (Leonard and Kogelmann, 2022). By design, the subsidy inherent in the Homestead Act induced a "race for property rights" to push settlers West, creating potential rent dissipation in the process (Anderson and Hill, 1990; Allen, 1991). This race, coupled with the specific occupancy and improvement requirements attached to the Act, locked homesteads into a specific trajectory that resulted in relatively less productive land use patterns that persist today (Allen and Leonard, 2021). Notably absent from these analyses of the Homestead Act is an assessment of the implications of homesteading for settlers themselves. Did homesteaders successfully improve their fortunes or those of their children? Answering this question requires careful attention to the channels through which land acquisition may affect economic standing and intergenerational mobility.

2.2 Land Ownership And Economic Mobility

There are several mechanisms through which land ownership may affect economic mobility, both for landowners and for their descendants. The first channel is through the asset value of the land itself and the ability to transfer that asset to one's heirs. Intuitively, free acquisition of a valuable asset may boost one's economic prospects by overcoming credit constraints or poverty traps, unlocking new pathways for wealth creation. This is consistent with microeconomic evidence from more recent times that bequests and inheritances explain a large portion of intergenerational wealth persistence (Adermon et al., 2018), and that land titling efforts aimed at poor households can foster upward mobility for future generations (Galán, 2024).

Second, development economists have documented various channels through which secure land rights may lead to economic gains. Holders of secure property rights to land have greater incentives to *invest* in the productivity of their land, are able to leverage land as *collateral* to obtain credit, and can ultimately *realize* the value of their land through market exchange (Besley, 1995; Galiani and Schargrodsky, 2010; Dippel et al., 2025). Hence, transferring secure land rights to settlers may enable them to improve their own fortunes above and beyond the initial value of the land endowment through subsequent economic development. These gains can then be passed on to future generations.

Finally, historians and economists have emphasized the formation of a unique culture of "rugged individualism" associated with the U.S. frontier (Turner, 1894; Bazzi et al., 2020). The availability of "free" land is central to Turner's Frontier Thesis, which describes the industrious qualities that frontier life required. Social isolation and limited state capacity on the frontier attracted more individualistic people to the frontier, and engendered a culture of individualism once there (Bazzi et al., 2020). Today, counties with greater historical frontier exposure are associated with greater upward mobility, possibly as a result of cultural transmission of traits that foster upward mobility (Leonard and Smith, 2021).

The upshot is that the microeconomic evidence for the importance of secure land rights in promoting economic development and upward mobility is consistent with the idea that generous federal land policy fostered economic mobility in U.S. history. However, we are aware of only two papers that directly study the effect of historical land policy on economic mobility in this context: Bleakley and Ferrie (2013, 2016), who exploit small-scale land lotteries in Georgia and Oklahoma and find limited evidence of long-run gains for poor lottery winners or their children. However, this *unconditional* and *random* land transfer program was limited in its scope and may not generalize to the broader suite of policies that drove the *process* of federal land disposal and private land acquisition in the U.S. Generous federal land policy was not an unconditional transfer of wealth; it was a mechanism that required upfront capital, intensive labor, and a tolerance for risk.

This leaves open several key questions about the process of land acquisition in American economic history. How did federal land policies induce selection into land ownership—was federal land policy ultimately regressive or progressive? How did the socioeconomic standing of these settlers evolve over time in comparison to populations of interest, including those who stayed behind and other frontier migrants who did not acquire land from the public domain? Finally, what role did federal land transfers play in the land-based component of intergenerational economic mobility in U.S. history?

The remainder of this paper attempts to answer these questions by providing a descriptive account of i) selection into acquisition of federal land, ii) the performance of settlers overtime relative to individuals who did not acquire land from the government, and iii) intergenerational consequences of federal land disposal. We emphasize that the sweeping nature of the policies in question frustrate a causal interpretation of the patterns that we document in this analysis. Nevertheless, characterizing the performance of settlers relative to other individuals fills an important and overlooked gap in the account of land ownership and economic mobility in American history.

3 Data & Empirical Strategy

3.1 Land Patent Records

The Bureau of Land Management, Eastern States Office (BLM), has digitized over two million federal land patents originally issued by the General Land Office to private individuals. These include homesteads entered under the various homestead acts, cash sales, military scrip, railroad land grants, allotments to Native Americans, and various other grants. Figure A1 provides an example of one such patent and the information it contains. The BLM database includes information on the date, location, type, and recipient of each land patent. The digitization of these records and the ability to map their precise locations has generated a swell of recent historical research using the BLM land patent data to study modern and historical land use implications from various policies including the Homestead Act (Allen and Leonard, 2021, 2025, 2026), railroad land grants (Allen, 2019; Alston and Smith, 2022), prior appropriation water rights (Leonard and Smith, 2025), and the General Allotment Act (Dippel et al., 2025).

Each patent's location is determined by its land description within the Public Land Survey System (PLSS)—a rectangular land survey established by the Land Ordinance of 1785. The PLSS, which is depicted in Figure A2 divides (most of) the United States into uniform 6-mile by 6-mile "townships" which are then uniformly divided into 36 individually numbered "sections." Sections are further divided into aliquot parts that correspond to individual plots of land. Figure A3 illustrates this land description system. For the purposes of this paper, we match each land patent record to the corresponding PLSS section and calculate the centroid and various geographic characteristics of each section, giving us an approximation of a plot's location that is accurate to within one square mile. We use this information to facilitate a process for linking the land records to historical census records, which we describe below.

3.2 Linking Land Patents to Individual Census Records

We obtain the full count historical census records for each census from 1850 to 1940 from IPUMS (Ruggles et al., 2025).[5] Currently, we focus on males aged 20 to 65 in each year to facilitate linking across time and to focus on individuals likely to be in the workforce. A major contribution of this paper is to link the universe of individual census records to the universe of land patent records.

The first step in the linking procedure is to clean, filter, and standardize the land patent records. First, we remove any unusual characters from the patentee's names to simplify matching. Second, we limit the sample to only those patentees that have both a first name and surname. Third, we determine the location of each patent based on its PLSS description, which we use to match each patent to a square-mile "section" in the PLSS. We then assign the centroid of the section to the patent. We use these coordinates to assign each patent to the county it would have belonged to in each census year. This helps standardize the county that we match on in cases where county boundaries have changed over time. For ease of computation, we then split our data into separate files by year and county before matching.

To link land patent records to the federal census, we employ the probabilistic record linkage framework from Stata's `reclink` package. We only compare patent records to census records from the same state and county in the two closest decennial censuses (for example, a patent from 1875 is compared to the 1870 and 1880 censuses). The features that we include when using `reclink` are first name, last name, and coordinates. We gather the census coordinates using data from the Census Place Project (Berkes et al., 2023) which are based on the geographic centroid of the enumeration district or township rather than precise household location.

A major weakness of `reclink` is that it does all of its comparisons using Jaro-Winkler similarity scores. For the names, this metric reasonably accounts for orthographic variation and tran-

[5] This research uses data from IPUMS USA and the IPUMS Ancestry Full Count Data project. We gratefully acknowledge the United States Census Bureau for the original data collection, as well as Ancestry.com and FamilySearch for providing the digitized raw data that made the full-count historical databases possible.

scription errors in historical name data. However, comparing numerical coordinates through Jaro-Winkler is not ideal because it only observes different characters, not the numerical difference between those characters. We use `reclink` 's comparison is serviceable for this initial matching, but we have developed a more sophisticated matching framework for future drafts, which we will discuss later. The `reclink` algorithm identifies all the possible patent-census matches and gives each a weighted comparison score. Because coordinates are systemically different between census centroids and land coordinates, a close match on surname is weighted twice as heavily as a close match on coordinates and a given name is weighted 125% as heavily. An incorrect match on either name is weighted four times as heavily as an incorrect match on coordinates.

Because `reclink` can produce many possible matches for each land patent, we then manually cleaned the `reclink` matches to identify the best matches for any given patent. We did this by comparing `reclink` 's weighted score and the attributes of the linked census record. We drop any matches that are scored below the record's highest score or are connected to a census id with an improbable age (below the 25th percentile or above the 99th percentile). When there are still multiple matches, we drop matches attached to a census record that are geographically further away from other matches with identical names, or have improbable ages if there are other matches with more typical ages. Once the matches have been cleaned, we drop any land patent that still matches to multiple people in the census, so we only keep matches in which we are confident.

We run a similar cleaning process on census ids that matched to multiple land patents. However, because it is possible for one person to buy multiple plots of land, we do not force-drop any people who match to multiple patents. Through this process, we can initially match 16% of the land patents with individuals from the census. While this `reclink` process provides serviceable links for this initial analysis, we also developed a superior additional probabilistic record linkage framework using Splink, a Python-based implementation of the Fellegi-Sunter model. This approach estimates the likelihood that a pair of records refers to the same individual by calculating the ratio of the conditional probability of a match to the conditional probability of a non-match. The model is trained using an unsupervised expectation-maximization algorithm. The algorithm assigns weights to specific comparison levels across three primary variables:

- **String Similarity (Full Name):** We utilize Jaro-Winkler similarity scores to account for orthographic variation and transcription errors in historical name data. Comparisons are binned into five similarity thresholds (>0.92, >0.88, >0.70, exact, and null).

- **Spatial Proximity:** Each land patent record includes the geographic coordinates of the plot of land. Census coordinates are derived from the Census Place Project. Because these coordinates represent geographic centroids rather than precise household locations, we specify broad distance thresholds (>1 km, >5 km, >10 km, >20 km, >40 km, and >60 km).

- **Administrative Matching:** An exact match on County FIPS codes is used, with only two thresholds (exact match and everything else).

We train a separate model for each state/year combination to allow the model more flexibility to account for geographic variation across states, as well as the geographic distribution of land patents issued in different years within the same state. To maintain computational efficiency and mitigate the risk of false positives, we implement strict blocking rules that require candidate pairs to share a similar geographic profile before being considered for probabilistic comparison. We allow for multiple patents to be linked to a single census record, but we do not allow multiple census records to be linked to the same patent. In cases where that occurs, we keep only the link with the highest match probability. After producing the initial predictions, we post-process our set of links to eliminate low quality links produced by the model. We first filter out the worst name matches.[6] Additionally, we drop any links that do not have an exact match on county if they were produced by a blocking rule that did not require some measure of geographic similarity.

After this stage of filtering, we have matched 17.8% of all patent records. We can then increase this number by using the links that we have created to find additional links. To accomplish this, we leverage the spatial clustering inherent in census data—under the assumption that individuals appearing on the same census sheet lived in close proximity. For unlinked land patents, we identify their nearest previously linked neighbor. We then conduct a targeted search of the linked neighbor's census sheet (and the sheets immediately preceding and following). We can also use this sheet-based assumption to remove links that are unlikely to be correct.

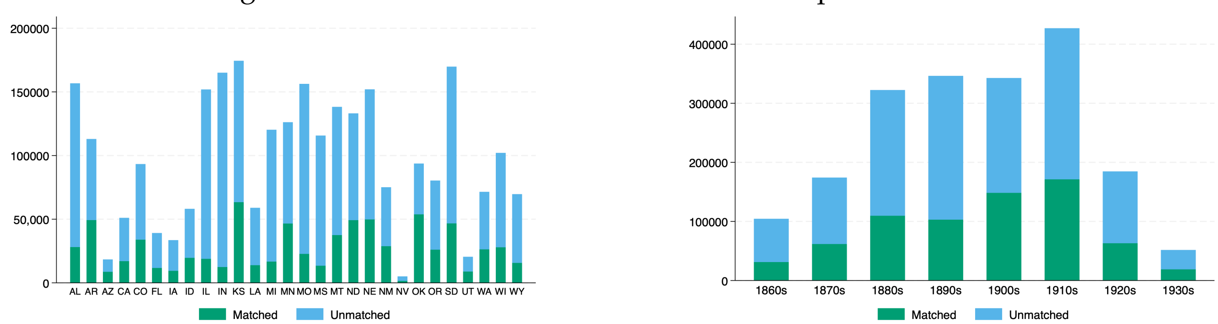
The process described above applies to *all* private land patents in the GLO/BLM database. We

[6] To accomplish this, we drop the bottom 5% of name matches by match weight, unless the 5th percentile has the same match weight as the 90th percentile. In that case we drop only the bottom 1%.

perform the initial matching across the full universe of patents issued to private individuals so that we can flag instances of double counting and so that our iterative procedure is built upon as many initial matches as possible. However, our interest in this paper is on two specific classes of patents: homesteads and cash sales.[7] Our match rates for these more standard patent types are considerably higher than for the broader universe of patents.

Figure 1 depicts the total number of matched homestead and cash sale patents by state (panel a) and by patent decade (panel b). Figure 2 depicts match rates for homesteads vs. cash sales by state and Table A1 provides a region-by-decade breakdown of match rates and total patent counts for homesteads (panel a) and cash sales (panel b). Match rates for homesteads tend to be greater than for cash sales. This is a reflection of the fact that settlers were required to actually occupy their claim for a period of 3–5 years to obtain title to a homestead, whereas cash sales could be purchased without the owner being physically present. This makes geographic blocking much more productive for matching homesteads than for cash sales. If John Smith buys a plot of land in Weld County, Colorado in 1900, he may be in that county, elsewhere in Colorado, or somewhere else entirely. Hence, finding cash buyers in the full-count census can be quite difficult.

Figure 1: Patent-to-Census Match Rates Over Space and Time



(a) Matches by State

(b) Matches by Patent Decade

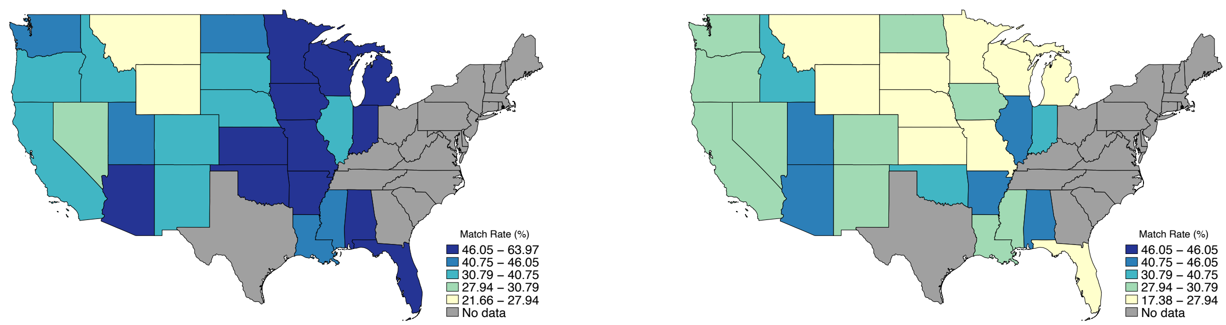
Notes: This figure depicts match rates from the land entry files to full count historical censuses across all patents issued between 1860 and 1940.

Our overall match rates for homesteads range from a low of 33.4% in the 1930s to a high of 52.5% in the 1900s. Match rates for cash sales range from a low of 16.9% to a high of 41.3%. Geographically, match rates with cash sales appear to be negatively correlated with homestead

[7] Homesteads could be entered under a variety of laws including the original 1862 Homestead Act, the Forest Homestead Act, the Enlarged Homestead Act, the Stock-Raising Homestead At, and the Reclamation Act. We group these different classes for the present analysis, but note that over 90% of all homesteads were entered under the terms of the original 1862 Act.

match rates, with the highest rates generally occurring in the Mountain West states. Figure A4 plots the precise location of each land patent that we are able to match to historical census records. Our match rates are similar to other recent studies that link historical census records to external data including USTO (invention) patents (Akcigit et al., 2017), immigration records (Alexander and Ward, 2018), military enlistment (Eli et al., 2018), and vital records (Bailey et al., 2022).

Figure 2: Match Rates by State and Patent Type



(a) Homestead Match Rate (%)

(b) Cash Sale Match Rate (%)

Notes: This figure depicts match rates from the land entry files to full count historical censuses across all patents issued between 1860 and 1940. Panel (a) depicts homesteads and panel (b) depicts cash sales. We lack data in the gray states, which were not included in the Public Land Survey System.

While our match rates are similar to those in recent literature, we face an additional complication. Because we ultimately wish to compare homesteaders and cash buyers to non-landowners, our comparison group is potentially contaminated by unmatched landowners. E.g., if John Smith bought land in Weld County Colorado in 1900 but we fail to find the relevant John Smith in the census, we misclassify him as a non-landowner (albeit potentially in a different county). To assess the scope of this problem, we calculate the share of unmatched *patents* in each county in each decade as a share of the total number of un-linked *individuals* in that county. Table A2 reports the mean, median, and 99th percentile of this ratio across counties for each decade. The right-hand panel of Table A2 also reports the breakdown of homesteads vs. cash patents that are not matched.

The mean misclassification ratio ranges from a low of 0.001 in the 1930s to a high of 0.031 in the 1890s. The 99th percentile of the ratio ranges from a low of 0.011 in the 1930s to 0.378 in the 1890s.[8] The upshot is that the ratio of unmatched records to the size of the comparison group is typically less than 1%, although certain counties exhibit high rates of misclassification in certain

[8]Matching patentees from the 1890s is especially difficult due to the unavailability of 1890 census records. Individuals who obtained land in the 1890s must be matched to the 1880 or 1900 census instead.

years. These outliers are driven by county-decade pairs with low population, suggesting they will not have an out-sized effect on our analysis, which utilizes various county-by-year fixed effects.

3.3 Linking Individual Census Records Over Time

We also link individual census records over time utilizing the links from the Census Tree (Buckles et al., 2025). We construct links for homesteaders, cash buyers, and our comparison group so that we can construct comparisons conditional on observing a given number of links over time and avoid comparing fundamentally different individuals. For the present analysis, we focus on men aged 20 to 65 when constructing links. Table A3 describes link rates across census years for homesteaders, cash buyers, and the comparison group. We report links separately for two distinct eras of settlement: 1850–1900 and 1900–1940.

3.4 Measuring Economic Outcomes

We use four approaches to overcome the lack of consistently reported measures of income or wealth over 1850–1940. Our benchmark approach is to use standard occupational scores (OccScores) obtained from IPUMS. These scores harmonize occupations over time and report the median income for each occupation in 1950. While use of OccScores to measure socioeconomic status is standard in historical studies, it is a relatively crude metric that does not vary over time or space (e.g., a farmer in Iowa in 1880 is assigned the same OccScore as a farmer in California in 1920).

Our second approach is to use an "adjusted Song score" following Song et al. (2020) and Ward (2023), which we refer to as a Ward Score. To construct the Ward Score, we calculate mean literacy rates for each region×occupation×race cell in each census year and then convert these raw means to a percentile rank from 1 to 100. Finally, we assign a percentile rank to each individual over 1840–1940 based on their occupation, race, and census region. Hence, our implementation of the Ward Score varies over time, and based on geography, occupation, and race.

Third, we follow Abramitzky et al. (2021) and Collins and Wanamaker (2022) and combine information from the 1940 and 1960 censuses to derive a predicted income measure, with specific adjustments for farmers. Following Abramitzky et al. (2021), we regress log income in 1940 for men aged 20 to 65 on a quadratic in age, plus fixed effects for occupation, region, state, and region-by-occupation fixed effects. We also follow this previous literature in making several adjustments for farmer incomes, based on the fact that many farmers are self-employed and the 1940 census did not report self employment income.[9] After obtaining these 1940 incomes as a function of occupation, region, state, and age, we predict income in each census year across 1850–1940 using individuals' characteristics in these years and the coefficients from the model estimated in 1940.

Our fourth measure of socio-economic status is an occupation-based wealth estimate inspired by Ager et al. (2021) and Collins and Zimran (2019), who estimate wealth in 1900 by calculating median wealth in the 1870 census for each occupation-county cell (for agricultural occupations) and each occupation-state cell (for non-agricultural occupations). We adopt the same approach, but use it to estimate wealth across all years in our sample. This metric assumes that the relative standing (in terms of wealth) of different occupations within a county or state is stable over time.

4 Selection into Land Acquisition

This section compares homesteaders and cash buyers of land to the rest of the U.S. population across 1850–1940. Throughout, comparisons are anchored to the first year in which we observe individuals in the census, with the restriction that this must be prior to the date they received a land patent from the federal government. The analysis is restricted to men aged 20 to 65. This range is comparable with recent historical literature on economic mobility that focuses on working-age years for linked individuals (Song et al., 2020; Abramitzky et al., 2021; Ward, 2023).

To begin, we characterize the composition and relative representation of homesteaders and cash buyers vs. the rest of the population in terms of occupation classes and birthplace. From the harmonized 1950 occupations provided by IPUMS, we construct five broad occupation classes: farm operators, farm laborers, skilled (non-farm) laborers, unskilled (non-farm) laborers, and white collar jobs.[10] Similarly, we aggregate countries into six regions: Scandinavia, Canada, Southeast Europe, U.S., German-speaking countries, and the British Isles.[11] Table 1 reports results for occupations and Table 2 reports results for birthplace.

[9] Specifically, we scale up farm managers' and farm laborers' income to adjust for in-kind transfers, and then multiply the income of farm laborers in 1940 by the ratio of earnings for farmers versus farm laborers in the 1960 census. We do this separately by region and immigration status.

[10] The 1950 Occupation Codes for each group are as follows. Farm operators: 100–123. Farm laborers: 810 and 840. Skilled (non-farm) workers: 500–595. Unskilled (non-farm) workers: 600–690. White collar workers: 0–99.

[11] The birthplace codes for each region are as follows. US-born: 1–99. British Isles: 410–414. German-speaking: 453, 454, 460. Scandinavia: 401 and 404–406. S/E Europe: 430–499. Canada: 150 and 199.

Table 1: Composition and Selection by Occupation

	Composition (%)			Relative Representation	
	Homestead	Cash Sale	Non-Owner	Homestead	Cash Sale
Panel A: 1860–1890					
Farm Operator	72.6	64.0	33.3	2.18	1.92
Farm Laborer	8.6	8.4	12.1	0.71	0.70
Skilled	5.5	7.3	14.3	0.39	0.51
Unskilled	9.6	11.5	27.0	0.35	0.43
White Collar	3.8	8.8	13.3	0.28	0.66
Panel B: 1900–1930					
Farm Operator	68.4	53.8	20.7	3.30	2.60
Farm Laborer	10.4	11.0	7.7	1.34	1.42
Unskilled	10.2	14.1	32.8	0.31	0.43
Skilled	5.1	7.3	17.0	0.30	0.43
White Collar	5.9	13.8	21.8	0.27	0.63

Notes: This table reports the occupational composition of homestead recipients, cash buyers, and non-patentees, along with relative representation (RR) ratios. Composition columns show the share (%) of each occupation class within the given patent type. RR is the ratio of a group's composition share among patent recipients to its share among non-patentees; values above 1 indicate overrepresentation. Panel A pools patents issued in the 1860s–1890s; Panel B pools patents issued in the 1900s–1930s. Occupation is classified using 1950 occupation codes from the census closest to the patent date. The 1890 decade uses the 1880 census due to the loss of the 1890 census.

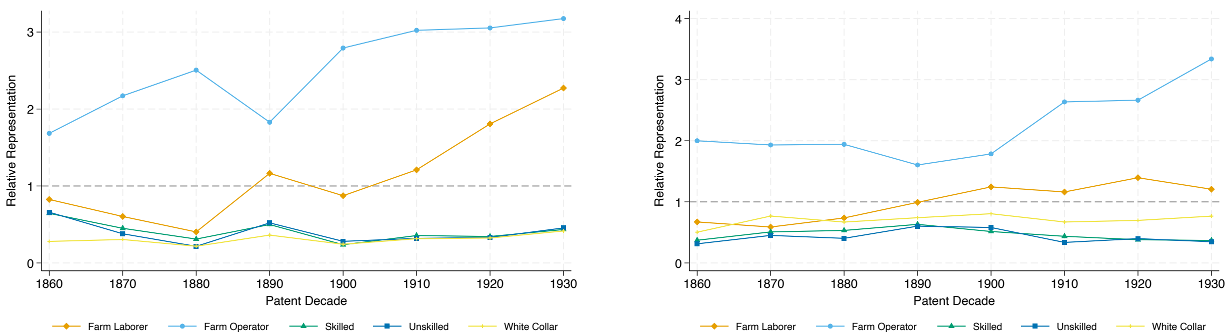
The left side of Table 1 reports the composition of homesteaders, cash-buyers, and non-owners in terms of occupation, with Panel A focused on the first wave (1850–1890) and Panel B focused on the second wave (1900–1930).[12] Over 80% of homesteaders and 70% of cash buyers were farm operators or (to a much lesser degree) farm laborers before obtaining land from the public domain, compared to roughly 45% for the rest of the population. The right panel of the table formalizes this comparison in terms of relative representation: a ratio greater than one suggests that a given occupation was over-represented among patentees compared to the rest of the population. Farm operators were twice as prevalent among homesteaders (2.18) and cash buyers (1.92) compared to the rest of the population, whereas farm laborers were actually under-represented. Non-farm occupations were all under-represented among homesteaders and buyers, and to similar degrees. Among these occupations, unskilled labor contributed the largest share to non-farm occupations comprising homesteaders and buyers.

The second wave in the early 20th century saw a slight decrease in the composition of homesteaders and buyers that were farm operators, but this decline was less sharp than what occurred in the rest of the population–the relative representation of farm operators among both classes of

[12]Here, our attention is on individual occupations *prior* to obtaining land. Later, we expand each wave forward one decade when we focus on outcomes *after* obtaining land.

patentees actually *increased* relative to the first wave. The relative representation of farm laborers also sharply increased, leading to an over-representation compared to non-patentees. The relative representation of non-farm occupations stayed fairly constant across the two waves. Figure 3 provides more detail on these trends by depicting the relative representation of each occupation class for each decade-specific cohort of homesteaders (panel a) and cash buyers (panel b) and Table B1 reports composition and relative representation for each decade.

Figure 3: Relative Representation of Occupations Among Landowners



(a) Homesteads (b) Cash Sales

Notes: This figure shows the relative representation of each occupation class among homestead and cash patent recipients compared to non-patentees across all patent decades (1860s–1930s). The dashed line marks proportional representation (RR = 1). Occupation is measured at the prior census closest to the patent date.

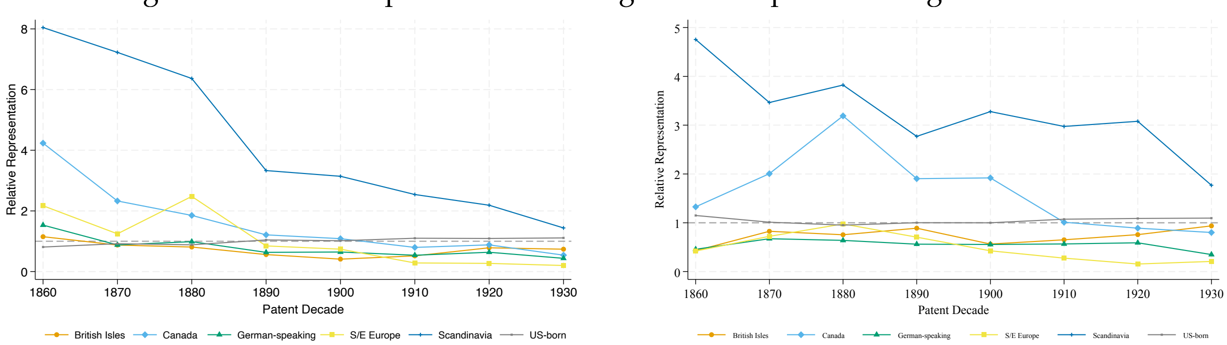
In Table 2, it is evident that the vast majority of homesteaders were U.S.-born across both waves, but that this was consistent with composition of the U.S. population as a whole. Focusing on the relative representation of non-U.S. birthplaces, Scandinavians were 6 times more prevalent among homesteads and 3 times more prevalent among cash buyers compared to non-patentees during the first era. This over-representation falls by half during the second era for homesteads, but stays relatively constant for buyers. Other origins experiencing relatively high representation in certain periods include Canada and Southeastern Europe. Figure 4 provides a breakdown of these trends for each group of patentees by decade. Figures B1 and B2 provide country-specific relative representation ratios. Table B2 reports composition and relative representation for the top 20 sending countries, while Table B3 breaks out composition for each country by decade and Table B4 breaks out relative representation for each country by decade.

Table 2: Composition and Selection by Birthplace (Regional)

	Composition (%)			Relative Representation	
	Homestead	Cash Sale	Non-Owner	Homestead	Cash Sale
Panel A: 1860–1890					
Scandinavia	7.2	3.8	1.1	6.24	3.35
Canada	3.9	4.7	2.0	1.90	2.30
S/E Europe	1.6	0.7	0.9	1.88	0.77
US-born	72.4	78.3	76.8	0.94	1.02
German-speaking	6.8	4.7	8.0	0.85	0.59
British Isles	8.2	7.9	11.2	0.73	0.70
Panel B: 1900–1930					
Scandinavia	6.0	7.2	1.9	3.09	3.69
Canada	2.0	3.2	1.8	1.06	1.72
US-born	84.0	81.6	80.1	1.05	1.02
German-speaking	3.0	3.0	3.9	0.78	0.79
British Isles	2.2	2.9	3.6	0.63	0.81
S/E Europe	2.8	2.2	8.6	0.33	0.25

Notes: This table reports the birthplace composition of homestead recipients, cash buyers, and non-patentees, along with each group's relative representation (RR) ratio. Composition columns show the share (%) of each birthplace region within the given patent type. Panel A pools patents issued in the 1860s–1890s; Panel B pools patents issued in the 1900s–1930s. The 1890 decade uses the 1880 census due to the loss of the 1890 census.

Figure 4: Relative Representation of Regional Birthplace Among Landowners



(a) Homesteads

(b) Cash Sales

Notes: This figure shows the relative representation of each birthplace region among homestead and cash patent recipients compared to non-patentees across all patent decades (1860s–1930s). Birthplace regions are defined as: British Isles (England, Scotland, Wales, Ireland), German-speaking (Germany, Austria, Switzerland), Scandinavia (Norway, Sweden, Denmark, Finland, Iceland), S/E Europe (Italy, Poland, Russia, and other Southern and Eastern European countries), and Canada. US-born includes all individuals born in the United States. The dashed line marks proportional representation (RR = 1).

Next, we compare the individual characteristics and baseline economic status of homesteaders and cash buyers to the rest of the working-age male population in each era and decade. To do so, we estimate a series of regressions of the form:

$$y_{ict} = \beta_1 \mathbb{1}(Homestead_i) + \beta_2 \mathbb{1}(Cash_i) + \lambda_c + \delta_d + \varepsilon_{ict} \quad (1)$$

where y_{ict} is the outcome of interest for individual i in county c in decade t, $\mathbb{1}(Homestead_i)$ is an indicator equal to one for individuals who eventually patent a homestead, $\mathbb{1}(Cash_i)$ is an indicator equal to one for individuals who eventually buy land, λ_c is a county fixed effect, δ_d is a census decade fixed effect, and ε_{ict} is an idiosyncratic error term. We estimate Equation 1 separately for each era, pooling across base-year outcomes in 1850–1880 for the first era and 1900–1930 in the second era. We also report decade-specific results in the appendix.

We employ different sets of county fixed effects to provide descriptive comparisons to different reference populations of interest. Our first specification omits county fixed effects to report differences relative to the rest of the U.S. population without making any geographical restrictions. The second specification uses a patent county fixed effect to compare homesteaders and cash buyers to other individuals living in the county where patentees obtained their land from the public domain. We treat patent county as a fixed, time-invariant characteristic. Hence, prior to obtaining a patent, homesteaders and cash buyers may live elsewhere, but this specification compares them to individuals living in the place where they will eventually obtain land, regardless of the year or contemporaneous location. Finally, we employ a "home county" fixed effect that identifies the county in which we first see each individual. This specification compares patentees to their baseline neighbors over time, regardless of whether they later move. We cluster standard errors by patent county throughout.

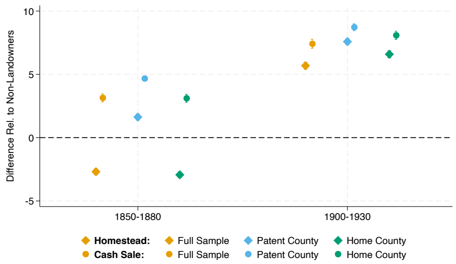
Figure 5 reports the results across patentees baseline age, literacy rate, family size, number of children, race, census-to-census link rates, immigration status, labor force participation, and father's occupational score for both the first wave and the second wave. Homestead coefficients are represented by a diamond and cash coefficients are represented by a circle. Solid markers indicate statistical significance at the 95% confidence level, whereas hollow markets indicate insignificant coefficients. Across both era and all fixed effect specifications, homesteaders tend to have larger families and more children, are more likely to be white, have fewer links across censuses, have fathers with lower occupation scores. Cash buyers are consistently older than the population at large, have larger families, more likely to be white, have fewer links, are less likely to be immigrants and have fathers of lower occupational standing.

Figure 5 also reveals important distinctions across eras and comparison groups, and between types of patentees. For instance, homesteaders in the first era tend to be younger than the population as a whole and than individuals in their home county, but older than individuals in the patent county. In the second era, homesteaders are older than all three comparison groups. Across both eras, homesteaders are younger than cash buyers, but these differences are modest compared to the divergence from the non-landowners in each comparison group. Neither class of patentees was more or less literate than the comparison group during the first era, but the second wave of patentees tended to be less literate than the population as a whole and than individuals in their home county (but not the patent county). The success of matching patentees vs. non-patentees across census years drops sharply during the second era, perhaps due to Dust Bowl-induced migration. This is consistent with the fact that relative rates of labor force participation all decline precipitously for both groups from the first era to the next.

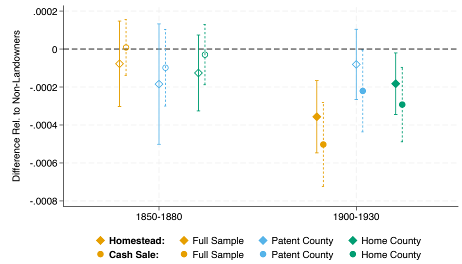
Figure 6 reports baseline differences in OccScore, Ward Score, predicted income, and occupation-based wealth using the same specifications as Figure 5. Both homesteaders and buyers in the first era began with lower OccScores than the comparison groups, but the differences are much smaller within home county or patent county. In the second era, cash buyers actually begin with higher OccScores than other individuals in their home counties and patent counties, whereas the negative comparison for homesteaders is stable across eras. Across both eras, homesteaders begin with lower occupational scores than cash buyers.

OccScores do not vary with time or geography, so it is not surprising that they are fairly stable across eras. Ward Scores, depicted in panel (b) vary within region and are calculated separately in each year. Here, we see more pronounced differences across eras. In the first era, cash buyers were no different than any of the reference groups, whereas homesteaders had lower Ward Scores than individuals in both their home counties and patent counties—a difference amounting to two percentiles in the rank distribution. Both types of patentees made relative improvements in the

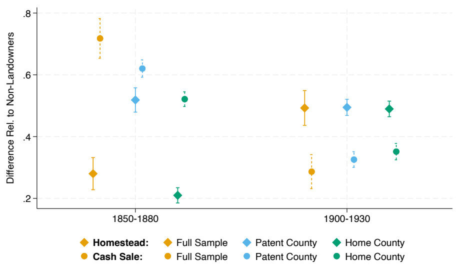
Figure 5: Selection in Land Ownership – Covariates



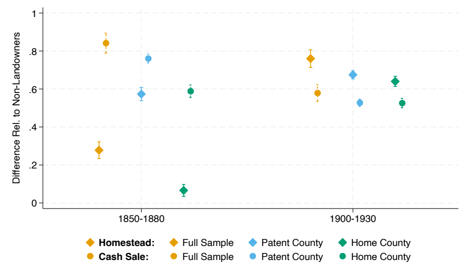
(a) Age



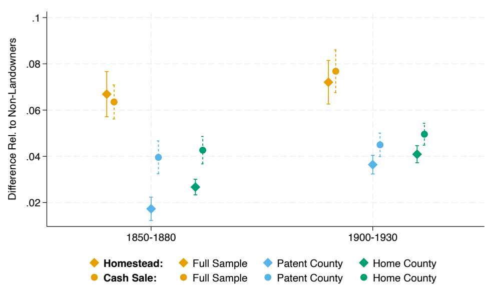
(b) Literacy



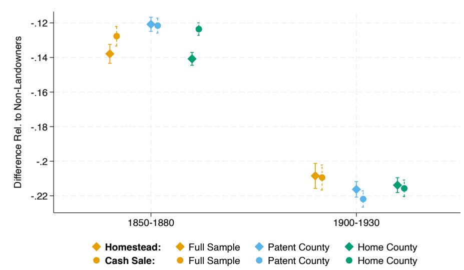
(c) Family Size



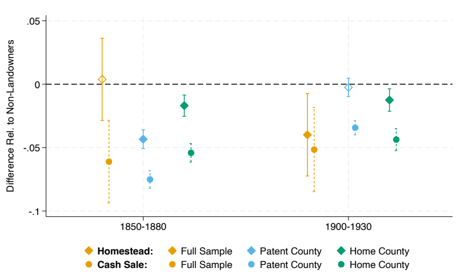
(d) Number of Children



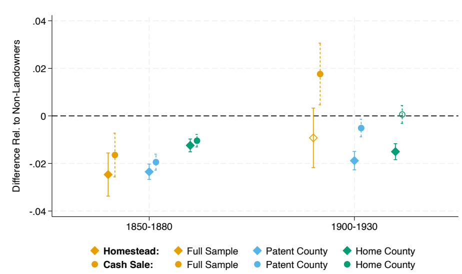
(e) White



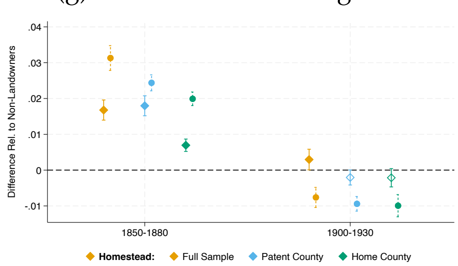
(f) Share of Possible Census Links



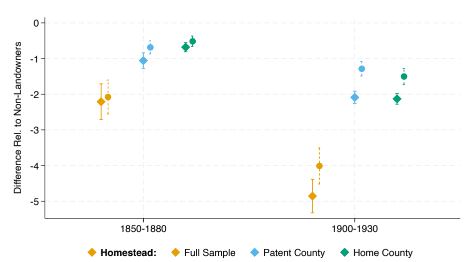
(g) 1st Generation Immigrants



(h) 2nd Generation Immigrants



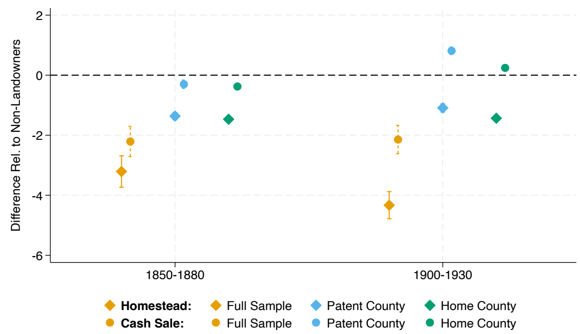
(i) Labor Force Participation



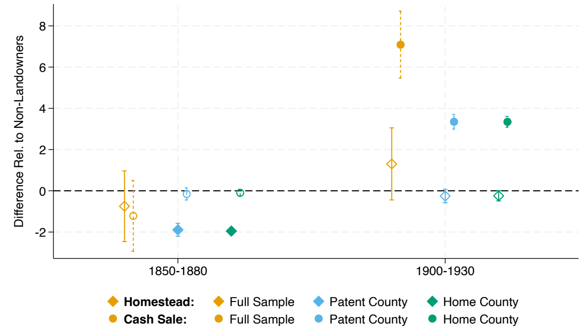
(j) Fathers' OccScore

Notes: Each panel plots coefficients from a linear probability model where the dependent variable is indicated by the panel title. The sample pools all individuals observed in the census closest to their patent date. Diamonds represent homestead recipients; circles represent cash sale buyers. Hollow coefficients are not statistically significant at the 95% confidence level. Each specification regresses the outcome on indicators for homestead and cash sale status (with non-landowners as the omitted category), estimated separately for the 1850–1880 and 1900–1930 eras. Three specifications are shown: unconditional (orange), conditional on patent county fixed effects (blue), and conditional on home county fixed effects (green). Capped bars show 95% confidence intervals based on standard errors clustered at the patent county level. The dashed line at zero indicates no difference from non-landowners.

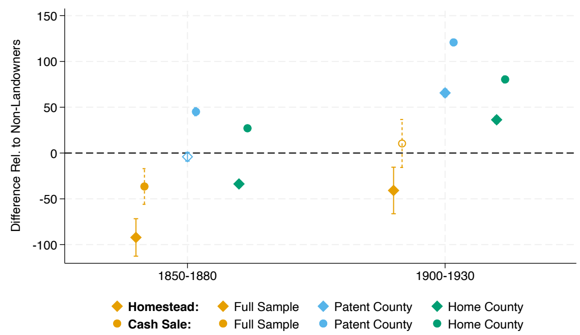
Figure 6: Selection in Land Ownership – Outcomes



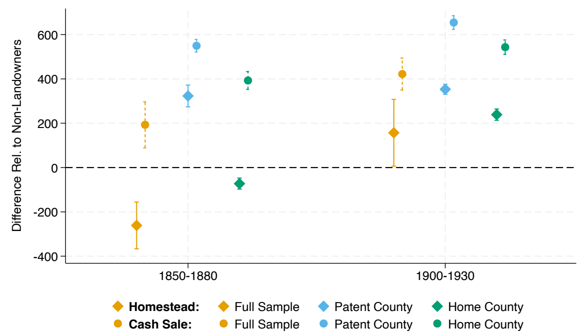
(a) OccScore



(b) Ward Score



(c) Predicted Income



(d) Occupation-Based Wealth

Notes: Each panel plots coefficients from a linear probability model where the dependent variable is indicated by the panel title. The sample pools all individuals observed in the census closest to their patent date. Diamonds represent homestead recipients; circles represent cash sale buyers. Hollow coefficients are not statistically significant at the 95% confidence level. Each specification regresses the outcome on indicators for homestead and cash sale status (with non-landowners as the omitted category), estimated separately for the 1850–1880 and 1900–1930 eras. Three specifications are shown: unconditional (orange), conditional on patent county fixed effects (blue), and conditional on home county fixed effects (green). Capped bars show 95% confidence intervals based on standard errors clustered at the patent county level. The dashed line at zero indicates no difference from non-landowners.

second era, cash buyers had higher Ward Scores than other individuals within home and patent counties, and homesteaders were indistinguishable from the comparison group.

Measures of predicted income (panel c) tell a similar story across eras, with notable improvements over time such that both buyers and homesteaders had higher baseline wealth than comparison individuals home and patent counties in the second era (the y-axis here is in log points). The trends in occupation-based wealth (panel d, also in log scale) are more nuanced. In the first era, buyers had higher baseline levels of wealth than the comparison group, but homesteaders' relative wealth was lower than comparison individuals in their home county. By the second era, homesteaders' wealth also exceeded comparison individuals in their home county as well. Across all four outcomes, both era, and all three comparison groups, cash buyers initial socio-economic status exceeded that of homesteaders.

In the appendix, we also provide comparisons by decade (Figures B3 and B4), occupation class (Figures B5 and B6), and birthplace (Figures B7 and B8). Generally speaking, differences within occupation and birthplace are smaller, but the patterns reported here remain fairly stable. Appendix B also provides tables with full regression results for the estimates in each figure.

Taken as whole, the results in this section provide the most comprehensive characterization to date of the characteristics of the individuals who established property rights to the "land of opportunity" in the late nineteenth and early twentieth centuries. These individuals were disproportionately already in farming, although land on the frontier did attract some patentees from all occupations. Patentees tended to have larger families, be more white, and come from families with lower socioeconomic status. The composition of land patentees also changed over the course of the 80-year history of major federal land disposal on the frontier.

Given these patterns, it is plausible that the abundance of "free" (or at least available) land was an important factor in upward economic mobility during this period, insofar as the newly minted landowners tended to have lower standing than other individuals. And, the Homestead Act in particular appears to have been progressive in terms of its uptake. Although the Act was not explicitly targeted or conditioned based on socioeconomic standing, across nearly every metric, homesteaders had more humble origins than those who purchased land from the public domain. Given that the $200 homestead subsidy was a relatively small share of the cost of moving to the frontier and establishing a farm (Allen and Leonard, 2021), the robustness of the homestead-cash comparison in baseline conditions is striking. Next, we examine how the individuals who utilized these federal land transfers fared over time relative to those that did not.

5 Economic Mobility for Land Recipients

This section focuses on the economic performance of homesteaders and cash buyers over time, relative to non-patentees. First, we summarize long-run occupational transitions for each group across both eras. Second, we analyze the evolution of each measure of socioeconomic status over time and compare relative differences before vs. after obtaining land from the public domain. Finally, we document heterogeneity by starting occupation and by patent location. We also examine the robustness of our core findings to a variety of alternative specifications and data constraints. While this section relies heavily on comparisons between patentees and non-patentees and utilizes progressively tighter comparisons between these groups, we emphasize caution in interpreting these estimates as causal effects of federal land policies, given the non-random selection into land ownership documented in the previous section. Rather, we view this as an important descriptive account of the performance of patentees relative to similar individuals over time that fills an important gap in our quantitative understanding of land transfers in American history.

5.1 20-Year Occupational Transitions

Table 3 reports 20-year occupational transitions for homesteaders, cash buyers, and non-owners across 1860–1900 and 1900–1940. Each cell reports the share of individuals that transitioned from each occupation reported in the first column to a given occupation reported across the first row. The final column reports the total number of individuals who began in each occupation. Having already characterized the composition of patentees' starting occupations in the previous section, the focus here is on transitions in occupation over time. We build pair-wise 20-year transitions by utilizing all individuals who we are able to link across 20-year horizons using the Census Tree. The 20-year horizon strikes a balance between allowing time for occupational transitions to emerge vs. maximizing the number of individuals that we can both link and observe during working-age years across both links.

In the first era, the majority of homesteaders transition to being farm operators, regardless of their starting status. Very few homesteaders end up as farm laborers, and very few who begin as farm laborers or operators transition to an occupation outside of farming. These trends are similar for cash buyers, albeit with lower rates of overall convergence to farm operators. The share of individuals who transition from a non-farm occupation to farm operators is much smaller for non-owners than for either type of patentee. The most striking difference when comparing the second era to the first is that the share of any given occupation that transitions to being a farm operator is much lower than in the first era across all three groups, but especially for homesteading. This suggests that the occupational dynamics of homesteading were fundamentally different during the first era than the second era, and is consistent with the fact that "late homesteading" tended to occur on more marginal lands (Allen and Leonard, 2025). To better understand these patterns, we next turn to a more detailed analysis of changes in socioeconomic standing over time.

Table 3: 20-Year Occupational Transitions

	Era 1: 1860–1900					
	End-of-Period Census					
Start-of-Period	White Collar	Skilled	Unskilled	Farm Op.	Farm Lab.	N
Homesteaders						
White Collar	0.365	0.073	0.081	0.459	0.022	1,879
Skilled	0.093	0.253	0.072	0.569	0.012	2,873
Unskilled	0.066	0.065	0.178	0.663	0.028	5,146
Farm Operator	0.065	0.042	0.064	0.807	0.022	31,222
Farm Laborer	0.050	0.043	0.083	0.778	0.046	4,574
Cash Buyers						
White Collar	0.523	0.047	0.090	0.328	0.012	2,216
Skilled	0.157	0.296	0.110	0.418	0.019	1,991
Unskilled	0.127	0.093	0.240	0.509	0.030	3,073
Farm Operator	0.087	0.048	0.076	0.765	0.024	15,355
Farm Laborer	0.098	0.062	0.131	0.666	0.043	2,381
Non-owners						
White Collar	0.631	0.089	0.133	0.135	0.012	811,837
Skilled	0.147	0.514	0.165	0.158	0.014	935,261
Unskilled	0.137	0.152	0.450	0.221	0.041	1,509,116
Farm Operator	0.076	0.049	0.091	0.751	0.033	2,361,669
Farm Laborer	0.079	0.081	0.195	0.553	0.093	729,724

	Era 2: 1900–1940					
	End-of-Period Census					
Start-of-Period	White Collar	Skilled	Unskilled	Farm Op.	Farm Lab.	N
Homesteaders						
White Collar	0.451	0.081	0.120	0.320	0.028	8,935
Skilled	0.148	0.325	0.143	0.353	0.031	7,966
Unskilled	0.106	0.100	0.272	0.456	0.067	17,017
Farm Operator	0.089	0.062	0.119	0.681	0.051	76,388
Farm Laborer	0.082	0.065	0.156	0.608	0.088	18,068
Cash Buyers						
White Collar	0.609	0.068	0.091	0.213	0.020	5,377
Skilled	0.207	0.361	0.148	0.263	0.021	2,757
Unskilled	0.170	0.121	0.296	0.366	0.047	5,569
Farm Operator	0.131	0.066	0.116	0.638	0.048	18,070
Farm Laborer	0.118	0.080	0.157	0.559	0.086	5,017
Non-owners						
White Collar	0.717	0.090	0.132	0.051	0.009	5,641,686
Skilled	0.194	0.518	0.222	0.053	0.013	4,588,878
Unskilled	0.170	0.174	0.529	0.093	0.034	7,492,804
Farm Operator	0.088	0.063	0.142	0.655	0.053	6,384,718
Farm Laborer	0.098	0.098	0.237	0.452	0.116	2,564,999

Notes: Transition shares from start-of-period to end-of-period occupation category over 20-year gaps, pooled within era. Sample restricted to males aged 20–65 linked across censuses.

5.2 The Evolution of Relative Socioeconomic Status Over Time

By definition, homesteading required individuals to take up farming as an occupation. While this is less mechanically true for cash sales, both types of land transfer had the propensity to promote farming. How did this impact socioeconomic status over time? Did individuals take a status hit in exchange for the opportunity to own land? Did their fortunes improve over time, or were they locked into farming?

To answer these questions, we create two unbalanced panels of linked individuals over time. Our first-era panel spans 1850 to 1900 and includes non-patentees as well as anyone who obtained a patent after 1850 and prior to 1900. Our second-era panel spans 1900 to 1940 and includes non-patentees and anyone who obtained a patent after 1900 but prior to 1940. In both eras, we keep only individuals that we are able to link across at least two census waves. The panels are unbalanced because they consist of individuals who are linked to 2, 3, or 4 censuses. This allows us to characterize how outcomes evolve for landowners over time without restricting our analysis to the relative small set of individuals who we can link across 30 years (4 censuses). In robustness checks described below, we discuss the sensitivity of our results to restricting the sample to balanced panels of individuals who are consistently linked across k censuses. Following Abramitzky et al. (2014b) and Collins and Wanamaker (2022), we estimate a series of link-specific logit models on the full census to estimate the probability that an individual that we first observe in year t is linked to year $t+m$ and then weight each observation in each year by the inverse of this probability so that our estimates are representative of the population as a whole.

We employ an extended two-way fixed effects (ETWFE) model (Wooldridge, 2023) to compare the evolution of outcomes over time for homesteaders vs. cash buyers vs. non-patentees. This is a fully saturated model that accounts for potential bias in traditional TWFE models by estimating cohort-specific effects that vary with time since patenting. It is more flexible and computationally efficient than other recent difference-in-difference estimators, and is more amenable to a non-causal interpretation in the sense that it simply reports average differences (relative to the final pre-treatment period) for each cohort in each time period since treatment. We estimate a series of models of the form:

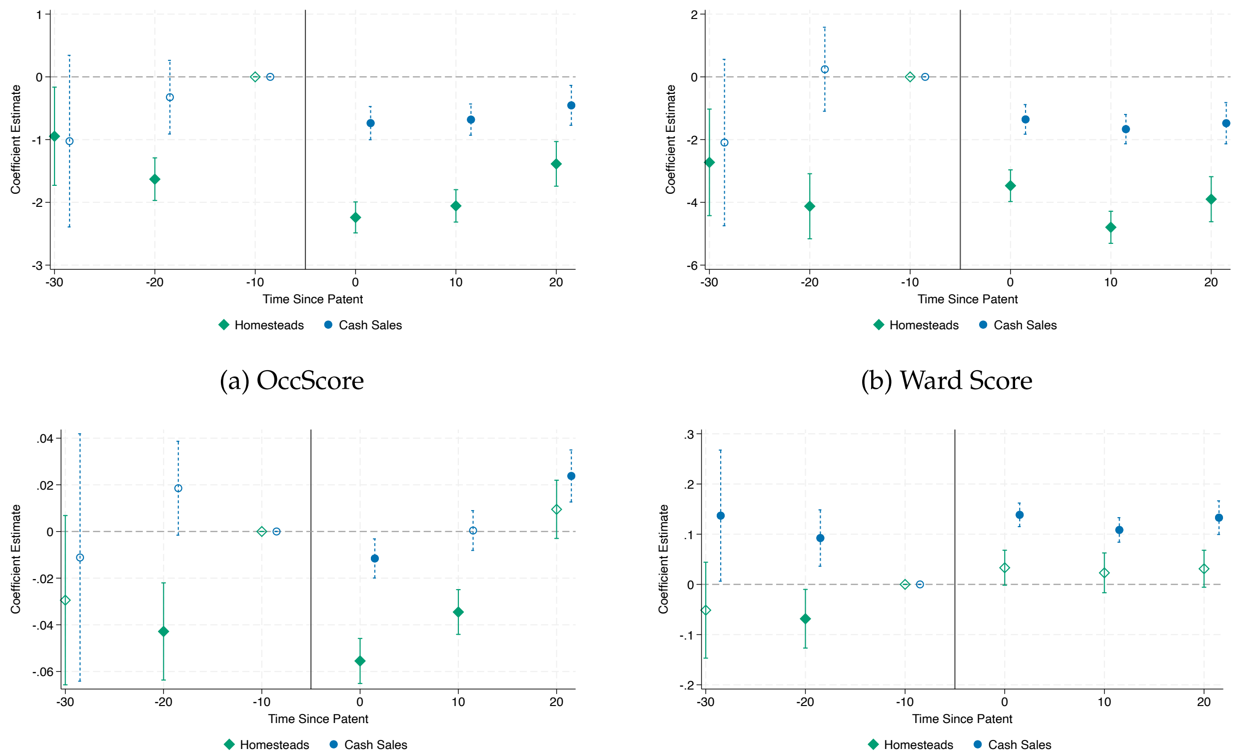
$$y_{ijt} = \alpha_j \times \tau_t + \sum_{d^h \in \mathcal{D}^h} \sum_{t \geq d^h} \beta^h_{d^h t}(H_{id^h} \times F_t) + \sum_{d^c \in \mathcal{D}^c} \sum_{t \geq d^c} \beta^c_{d^c t}(C_{id^c} \times F_t) + \mathbf{X}_{ijt}\gamma + \epsilon_{ijt} \quad (2)$$

where y_{ijt} is the outcome for individual i in county j during census decade t, α_j are county fixed effects (which vary across specification), τ_t are census decade fixed effects, $\mathbf{X}_{ijt}$ is a quadratic in age, and ϵ_{ijt} is an error term. $\mathcal{D}^h$ and $\mathcal{D}^c$ are the sets of all possible patent-decade cohorts for Homestead (h) and Cash (c) entries, respectively. Patent decade cohorts are defined by the first census decade in which an individual is "treated," so an 1867 patent would belong to the 1870 treatment cohort. H_{id^h} is a binary cohort indicator equal to 1 if individual i received their first Homestead patent in decade d^h and C_{id^c} is a binary cohort indicator equal to 1 if individual i received their first Cash patent in decade d^c. F_t is a census decade indicator. $\beta^h_{d^h t}$ and $\beta^c_{d^c t}$ are the cohort-time specific average "treatment" effects for Homestead and Cash patents, respectively. They report the difference in outcomes for homesteads (or cash sales) vs. non-patentees relative to the difference in year $t - 10$ (the census decade prior to patenting), which is normalized to zero.

We utilize several fixed effects specifications when estimating Equation 2. First, we include a patent county-by-decade fixed effect to compare the evolution of outcomes to individuals from the same patent county across different years. Second, we include a home county-by-decade fixed effect to compare the evolution of outcomes relative to non-patentees in the county in which we first see each patentee. Interacting census decades with a static county cohort allows us to compare the evolution of outcomes for patentees to non-patentees who were living in the same place at one point in time (either the base year or the patent year) but may have subsequently moved. Third, we define home-patent county pairs to construct cohorts of individuals who made the same migration decisions as our patentees, which allows us to isolate trajectories for homesteaders and buyers vs. other individuals who migrated from the same origin to the same destination, but did not obtain land. We treat this as our preferred specification. Finally, we include a specification with patent county-by-decade fixed effects and individual fixed effects to examine within-individual changes over time, relative to other individuals from the patent county. This specification absorbs any time-constant unobserved heterogeneity between individuals, but does not address shocks or trends that may be correlated with the decision to acquire land.

We aggregate the raw cohort-time specific coefficients into time-since-patent (event-study) estimates for each group and plot the results in Figures 7 and 8, which utilizes the home-patent pair fixed effect specification. Analogous plots for the other fixed effect specifications and full cohort-time coefficient estimates and standard errors are reported in Appendix C. Standard errors are clustered by the contemporaneous county for individual i in census year t.

Figure 7: Differences Relative to Non-Landowners, 1850–1900



(a) OccScore

(b) Ward Score

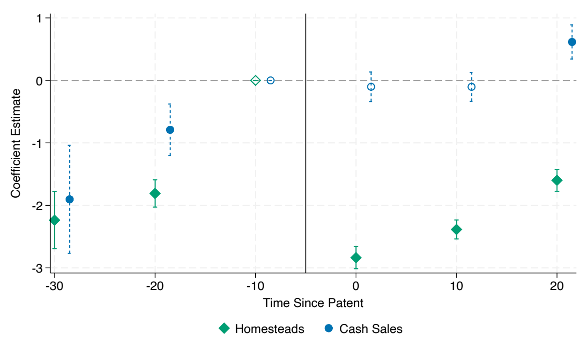
(c) Predicted Income

(d) Occupation-Based Wealth

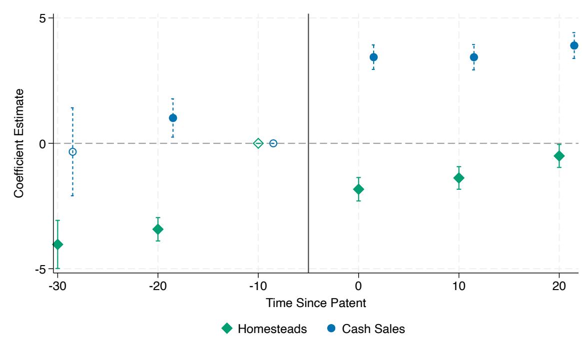
Notes: This figure plots event-study coefficients from an extended two-way fixed effects model. Treatment timing is defined as the first decade boundary following the patent date, with the omitted reference period at $t = -10$ (normalized to zero). Coefficients represent the change in the outcome relative to the omitted period for patentees compared to non-landowners. Capped bars show 95% confidence intervals based on standard errors clustered at the contemporaneous county level. All regressions include age and age-squared as controls, as well as home-patent county pair fixed effects and are weighted using inverse probability weights. Filled markers indicate significance at the 5% level.

Focusing first on the 1850—1900 estimates in Figure 7, homesteaders had consistently lower occupational scores relative to non-patentees both prior to the baseline period (the census immediately before patenting) and after obtaining their patent, though there is some evidence of an upward trend in occupational standing after patenting. In contrast, cash buyers exhibited no pre-patent differences from other frontier migrants, but suffered a persistent OccScore penalty after patenting. The results for Ward Scores are qualitatively identical. Measures of income and wealth tell a somewhat different story. For income, homesteaders have persistently lower incomes until 20 years after patenting, when differences are no longer different from zero. Again, this suggests some relative mobility for homesteaders. Cash buyers also exhibit upward mobility over time, initially taking an income hit immediately after patenting, but earning higher incomes than other migrants 20 years after patenting. Differences in wealth appear to be small and persistent over time for homesteaders, with larger (positive) stable differences for cash buyers. Cash buyers consistently outperform homesteaders both before and after patenting on every metric, with little evidence of convergence over time.

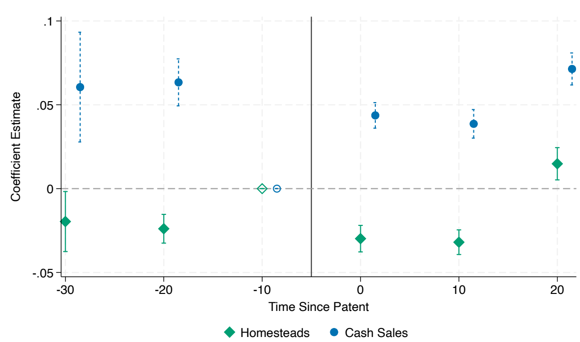
Figure 8: Differences Relative to Non-Landowners, 1900–1940



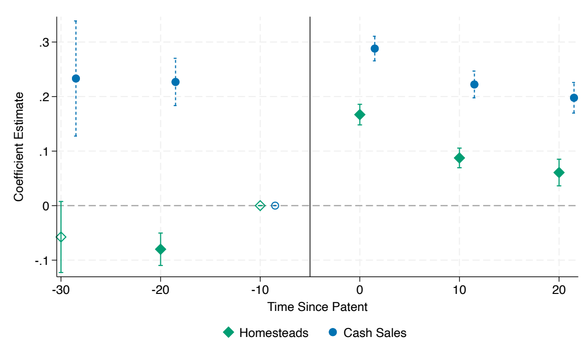
(a) OccScore



(b) Ward Score



(c) Predicted Income



(d) Occupation-Based Wealth

Notes: This figure plots event-study coefficients from an extended two-way fixed effects model. Treatment timing is defined as the first decade boundary following the patent date, with the omitted reference period at $t = -10$ (normalized to zero). Coefficients represent the change in the outcome relative to the omitted period for patentees compared to non-landowners. Capped bars show 95% confidence intervals based on standard errors clustered at the contemporaneous county level. All regressions include age and age-squared as controls, as well as home-patent county pair fixed effects and are weighted using inverse probability weights. Filled markers indicate significance at the 5% level.

The 1900–1940 results in Figure 8 exhibit some interesting contrasts to the first era. Unlike in the first era, both cash buyers and homesteaders have lower occupational scores before patenting. While both show signs of improvement after patenting, cash buyers actually catch up to and then surpass the baseline difference from comparison group over time. Here, Ward Scores tell a slightly different story suggesting a steady convergence over time post-patent for homesteaders (back to their pre-patent difference), whereas cash buyers see an immediate and persistent increase in their scores post-patent. Cash buyers have consistently higher incomes and wealth than the comparison group (and homesteaders) relative to the pre-patent baseline year, suggesting a dip just prior to patenting that they swiftly recover from. Homesteads show a discrete jump in wealth after patenting that slowly declines over time.

To further summarize these results, we compute overall post-patent changes in outcomes relative to the baseline year for each patent type and report the results across each fixed effect specification in Tables 4 (first era) and 5 (second era). In the first era, homesteaders and (to a lesser degree) cash buyers see decreases in the OccScores relative to baseline differences from non-patentees in their home county, patent county, and the pairwise combination of the two. Including individual fixed effects erases these differences, however. The story is similar for Ward Scores, with the noted exception that cash buyers actually see increases in relative standing in the specification with individual fixed effects. Homesteads sustain relative declines in income across all four specifications, whereas cash buyers see increases (decreases) relative to baseline differences from individuals in their home (patent) county. Changes in wealth for homesteaders depend critically on the comparison group, exhibiting declines with home county and individual fixed effects, but relative gains with home-patent county pair fixed effects. Cash buyers see relative gains in wealth across all specifications except individual fixed effects.

Turning to the second era in Table 5, homesteaders face qualitatively similar declines in relative status compared to the first era across nearly all specifications, whereas cash buyers in the second era consistently outperform cash buyers in the first era (in terms of relative mobility). There are some exceptions worth highlighting. The most notable is for wealth, where homesteaders see relative gains in the second era in three of four specifications, and the gains for cash buyers are nearly double the first-era coefficients in three of four specifications. In both cases, however, including individual fixed effects results in an estimated decline in wealth relative to non-patentees.

Table 4: Average Post-Patent Differences Relative to Non-Landowners, 1850–1900

	(1)	(2)	(3)	(4)
Panel A: Occupational Income Score				
Homesteads	-2.313***	-1.553***	-1.967***	-0.205
	(0.075)	(0.077)	(0.092)	(0.132)
Cash Sales	-0.905***	-0.403***	-0.653***	0.056
	(0.079)	(0.075)	(0.091)	(0.120)
Observations	26,653,028	26,652,643	26,556,111	18,348,967
Fixed Effects	11,356	13,107	21,944	7,183,962
Adj. R-Squared	0.070	0.070	0.070	0.498
Panel B: Ward Occupational Score				
Homesteads	-3.605***	-4.161***	-4.132***	0.204
	(0.164)	(0.158)	(0.192)	(0.251)
Cash Sales	-1.685***	-1.749***	-1.510***	0.615***
	(0.157)	(0.151)	(0.177)	(0.229)
Observations	24,053,794	24,053,406	23,963,910	16,058,150
Fixed Effects	11,350	13,036	20,817	6,451,680
Adj. R-Squared	0.353	0.353	0.353	0.732
Panel C: Log Predicted Income				
Homesteads	-0.059***	-0.018***	-0.032***	-0.009*
	(0.004)	(0.003)	(0.004)	(0.006)
Cash Sales	-0.018***	0.006**	0.001	-0.007
	(0.003)	(0.002)	(0.003)	(0.005)
Observations	23,931,617	23,931,228	23,842,133	15,924,480
Fixed Effects	11,349	13,033	20,741	6,392,138
Adj. R-Squared	0.303	0.303	0.303	0.627
Panel D: Log Occupation-Based Wealth				
Homesteads	-0.036**	0.012	0.028**	-0.063***
	(0.018)	(0.012)	(0.014)	(0.016)
Cash Sales	0.092***	0.122***	0.125***	-0.008
	(0.009)	(0.008)	(0.009)	(0.014)
Observations	26,171,647	26,171,262	26,075,823	18,088,588
Fixed Effects	11,356	13,107	21,692	7,078,287
Adj. R-Squared	0.200	0.200	0.200	0.513
Base County FE	✓			
Patent County FE		✓		✓
Base-Patent Pair FE			✓	
Person FE				✓

Notes: This table reports average post-patent effects from an extended two-way fixed effects model estimated separately for homesteaders and cash buyers. Treatment timing is defined as the first decade boundary following the patent date. The omitted period is the decade immediately prior to treatment ($t = -10$). All regressions include age and age-squared as controls and are weighted using inverse probability weights based on the probability of an individual being linked across a given pair of census years. Standard errors, reported in parentheses, are clustered at the contemporaneous county level. Columns (1)–(3) include group-by-year fixed effects using different group definitions: the first county of appearance (Home County), the patent county (Patent County), and the home-patent county pair (County Pair). Column (4) adds individual person fixed effects to the Patent County specification. "—" indicates that the standard error is unreliable due to near-collinearity from sparse treated cells within fixed-effect groups. *p<0.10, **p<0.05, ***p<0.01.

Table 5: Average Post-Patent Differences Relative to Non-Landowners, 1900–1940

	(1)	(2)	(3)	(4)
Panel A: Occupational Income Score				
Homesteads	-2.544***	-2.094***	-2.409***	0.103
	(0.058)	(0.067)	(0.073)	(0.067)
Cash Sales	0.045	0.526***	0.082	0.500***
	(0.088)	(0.089)	(0.095)	(0.126)
Observations	91,789,176	91,789,166	91,664,498	78,365,952
Fixed Effects	15,080	15,403	36,530	28,316,354
Adj. R-Squared	0.089	0.089	0.089	0.468
Panel B: Ward Occupational Score				
Homesteads	-1.690***	-0.572***	-1.383***	-0.839***
	(0.217)	(0.204)	(0.216)	(0.138)
Cash Sales	3.275***	4.446***	3.554***	0.097
	(0.234)	(0.204)	(0.201)	(0.236)
Observations	84,249,346	84,249,335	84,132,632	70,218,812
Fixed Effects	15,078	15,399	34,736	26,029,214
Adj. R-Squared	0.247	0.247	0.247	0.708
Panel C: Log Predicted Income				
Homesteads	-0.004	-0.013***	-0.021***	-0.041***
	(0.004)	(0.003)	(0.004)	(0.003)
Cash Sales	0.047***	0.055***	0.049***	-0.021***
	(0.004)	(0.003)	(0.003)	(0.005)
Observations	81,289,120	81,289,109	81,173,057	67,108,381
Fixed Effects	15,079	15,400	34,556	24,832,847
Adj. R-Squared	0.332	0.332	0.332	0.656
Panel D: Log Occupation-Based Wealth				
Homesteads	0.113***	0.117***	0.116***	-0.128***
	(0.009)	(0.008)	(0.009)	(0.010)
Cash Sales	0.234***	0.245***	0.242***	-0.068***
	(0.010)	(0.008)	(0.009)	(0.015)
Observations	87,546,280	87,546,271	87,424,661	73,811,341
Fixed Effects	15,079	15,402	35,804	26,932,410
Adj. R-Squared	0.127	0.127	0.127	0.471
Base County FE	✓			
Patent County FE		✓		✓
Base-Patent Pair FE			✓	
Person FE				✓

Notes: This table reports average post-patent effects from an extended two-way fixed effects model estimated separately for homesteaders and cash buyers. Treatment timing is defined as the first decade boundary following the patent date. The omitted period is the decade immediately prior to treatment ($t = -10$). All regressions include age and age-squared as controls and are weighted using inverse probability weights based on the probability of an individual being linked across a given pair of census years. Standard errors, reported in parentheses, are clustered at the contemporaneous county level. Columns (1)–(3) include group-by-year fixed effects using different group definitions: the first county of appearance (Home County), the patent county (Patent County), and the home-patent county pair (County Pair). Column (4) adds individual person fixed effects to the Patent County specification. "—" indicates that the standard error is unreliable due to near-collinearity from sparse treated cells within fixed-effect groups. *p<0.10, **p<0.05, ***p<0.01.

Appendix C provides additional results related to our EWTFE estimates. Tables C1 through C8 provide cohort-time specific estimates for each specification and each outcome—these are the raw coefficient estimates from Equation 2 that we then aggregate to produce the results presented in this section. Figures C1 through C6 provide time-since-patent aggregate plots for each outcome and each fixed effect specification. Table C9 demonstrates the robustness of the first-era results to excluding patents from the 1880s, which cannot be matched to the 1890 census. Tables C10 through C13 restrict the sample to individuals who appear in at least 3 (or 4) censuses. Across each of these alternative approaches, the qualitative findings discussed in this section are unchanged.

5.3 Heterogeneity Across Occupation and Geography

Next, we provide estimates of average post-patent differences relative to non-landowners for each occupation class and for individual states in each era. Doing so allows us to shed some additional light on patterns presented in Tables 4 and 5. Specifically, including individual fixed effects has qualitatively different impacts on the estimates across the four outcome variables, attenuating the coefficients in some cases but flipping the sign in others. This may indicate that homesteaders and buyers of land systematically differ in important ways from non-patentees even within various fixed effects. However, it is also worth noting that each of our four outcomes is a different transformation of an individual's reported occupation, with differing levels of additional variation due to geography, age, and time period. Hence, estimating Equation 2 separately for each occupation class and state can reveal the extent to which results are driven variation within and across occupations (or the lack thereof) vs. variation within and across geographies.

We report results for the county-pair fixed effect specification separately for each occupation class based on an individual's *initial occupation* in Tables 6 (1850–1900) and 7 (1900–1940). In the first era, homesteaders who began as farm operators or skilled workers see relative declines in OccScore, Ward Score, and predicted income, but see gains in occupation-based wealth. Homesteaders who began as farm laborers or unskilled workers see improvements in predicted income and wealth. White collar workers see declines across the board, with varying degrees of precision. The second era is largely similar for homesteaders, except that Ward Scores see relative improvements in all occupations besides skilled and white collar workers.

For cash sales, we see relative declines in both OccScores and Ward Scores for skilled workers in the first era. Unlike homesteads, there are no significant declines across any outcome for white collar workers. Farm laborers and unskilled workers saw relative gains in Ward Score, predicted income, and wealth. In the second era, the only statistically significant decline in relative performance for cash buyers is for skilled workers' OccScores. Cash buyers over 1900–1940 across all occupations saw significant relative improvements in income and wealth, with farm laborers and operators also seeing improvements in OccScores and Ward Scores.

Table 6: Within-Occupation Differences, 1850–1900

	(1) Farm Laborer	(2) Farm Operator	(3) Skilled	(4) Unskilled	(5) White Collar
Panel A: Occupational Income Score					
Homesteads	-0.248	-0.377***	-3.467***	-1.920***	-1.519
	(0.203)	(0.082)	(0.529)	(0.340)	(1.087)
Cash Sales	0.340	-0.026	-1.800***	0.065	0.165
	(0.236)	(0.071)	(0.387)	(0.287)	(0.694)
Observations	3,120,867	8,171,909	3,544,561	5,782,636	3,137,325
Panel B: Ward Occupational Score					
Homesteads	0.287	-1.899***	-6.683***	-0.783	-5.284***
	(0.465)	(0.164)	(1.157)	(0.762)	(1.542)
Cash Sales	1.583***	-0.339**	-3.523***	1.513**	-1.491
	(0.465)	(0.148)	(0.805)	(0.595)	(0.953)
Observations	3,044,666	7,884,989	3,417,630	5,568,146	2,998,243
Panel C: Log Predicted Income					
Homesteads	0.036***	0.000	-0.040***	0.044***	-0.065***
	(0.010)	(0.003)	(0.015)	(0.016)	(0.024)
Cash Sales	0.052***	0.011***	0.004	0.045***	0.005
	(0.010)	(0.003)	(0.013)	(0.010)	(0.015)
Observations	3,040,133	7,869,687	3,397,239	5,512,624	2,979,994
Panel D: Log Occupation-Based Wealth					
Homesteads	0.123***	0.032***	0.084*	0.246***	-0.101
	(0.033)	(0.008)	(0.048)	(0.045)	(0.064)
Cash Sales	0.182***	0.050***	0.155***	0.254***	0.061
	(0.038)	(0.008)	(0.034)	(0.032)	(0.041)
Observations	3,105,590	8,152,251	3,521,409	5,735,773	3,102,331
Base-Patent Pair FE	✓	✓	✓	✓	✓

Notes: This table reports average post-patent effects from the extended TWFE model, estimated separately by baseline occupation class. Occupation class is defined based on the individual's occupation at their first observed census (entry year). All specifications include home county by patent county pair-by-year fixed effects. See Table 4 notes for additional details on the estimation approach. *p<0.10, **p<0.05, ***p<0.01.

Recall that OccScores vary only by occupation (but within the broader occupation classes reported here), Ward Scores add region-by-year variation for each occupation, predicted income adds variation based on age and makes special adjustments for self employment and farm income, and wealth varies by occupation and state (non-farm) or county (farm). The stability of the negative OccScore result for homesteaders is not surprising, given that OccScore does not vary over time or space, and that homesteading essentially required participants to become farmers.

The stability of results for certain outcomes across eras is also instructive. For instance, homesteaders who began as white collar workers see decline in relative performance across all outcomes in both eras. This reflects the stratification of high-status white collar jobs and low-status farming occupations that is large relative to regional or time-series variation in either set of occupations. On the other hand, the consistently positive wealth performance of homesteaders across the other four occupations is likely driven by the nature of the occupation-based wealth metric, which assumes a stable ranking of wealth across occupation-by-geography cells over time.

Table 7: Within-Occupation Differences, 1900–1940

	(1) Farm Laborer	(2) Farm Operator	(3) Skilled	(4) Unskilled	(5) White Collar
Panel A: Occupational Income Score					
Homesteads	-0.672***	-0.267***	-2.272***	-1.684***	-3.376***
	(0.100)	(0.044)	(0.263)	(0.137)	(0.298)
Cash Sales	0.277*	0.753***	-1.147***	0.039	0.237
	(0.159)	(0.086)	(0.360)	(0.215)	(0.320)
Observations	9,586,325	16,268,946	13,191,614	26,618,557	17,413,230
Panel B: Ward Occupational Score					
Homesteads	2.581***	0.953***	-2.092***	2.089***	-4.902***
	(0.277)	(0.128)	(0.482)	(0.366)	(0.482)
Cash Sales	4.637***	3.192***	-0.391	6.055***	0.107
	(0.393)	(0.190)	(0.580)	(0.525)	(0.433)
Observations	9,276,689	15,684,425	12,729,188	25,641,855	16,721,843
Panel C: Log Predicted Income					
Homesteads	0.041***	-0.008***	-0.012	0.033***	-0.074***
	(0.005)	(0.002)	(0.009)	(0.008)	(0.007)
Cash Sales	0.065***	0.025***	0.037***	0.080***	0.021***
	(0.007)	(0.003)	(0.012)	(0.009)	(0.007)
Observations	9,140,208	15,522,947	12,197,278	24,052,616	16,282,757
Panel D: Log Occupation-Based Wealth					
Homesteads	0.165***	0.016***	0.120***	0.218***	-0.132***
	(0.015)	(0.005)	(0.025)	(0.022)	(0.021)
Cash Sales	0.231***	0.060***	0.217***	0.305***	0.154***
	(0.022)	(0.010)	(0.035)	(0.025)	(0.024)
Observations	8,844,886	16,076,501	12,492,756	25,970,970	17,027,929
Base-Patent Pair FE	✓	✓	✓	✓	✓

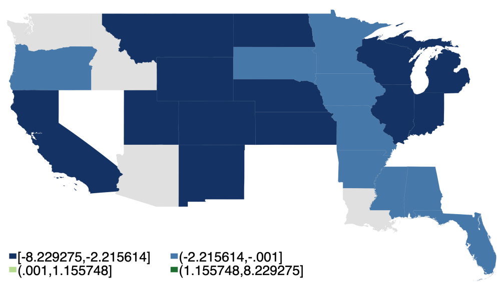
Notes: This table reports average post-patent effects from the extended TWFE model, estimated separately by baseline occupation class. Occupation class is defined based on the individual's occupation at their first observed census (entry year). All specifications include home county by patent county pair-by-year fixed effects. See Table 4 notes for additional details on the estimation approach. *p<0.10, **p<0.05, ***p<0.01.

The change in the relative Ward Score performance from 1850–1900 to 1900–1940 is likely driven by the fact that Ward Scores capture regional variation in occupational status (farmers in the Midwest may be better than farming in the Northeast), and that we re-calculate these scores annually. The change across eras implies that homesteaders in the 1900–1940 period located in regions where farming was relatively higher-status, and/or that farming was higher-status over 1900–1940 than 1850–1900. Similarly, changes in the relative impact on predicted income of skilled workers across eras likely reflects changes in the relative status of farming over time and space.

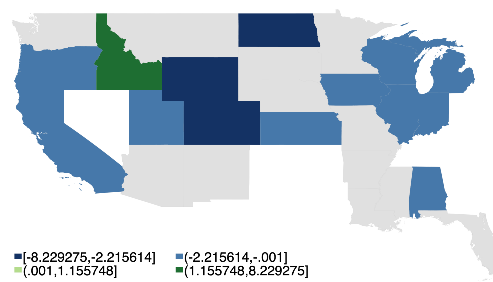
Ascertaining the relative importance of regional variation (or lack thereof) in the various outcome measures requires estimating state-specific coefficients and examining the resulting patterns.

Next, we explore geographic variation in the relative performance of patentees across outcomes and eras by re-estimating Equation 2 separately for each state, outcome, and era. For these specifications we use the location of the patent to determine the state and utilize the home-county pair fixed effect specification. We present the results in Figures 9–12, with one figure per outcome variable to force consistent scales across each map in a given figure. In each figure, we depict the estimated average post-patent relative performance for homesteads over 1850–1900 (panel a), cash sales over 1850–1900 (panel b), homesteads over 1900–1940 (panel c), and cash sales over 1900-1940 (panel d). Declines in relative performance are depicted with blue shading, while improvements are depicted in green. States with an insignificant estimate are shaded in gray, and states that lacked sufficient variation for an estimate are omitted (white).

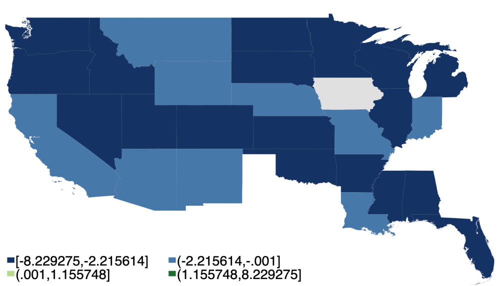
Figure 9: Relative Changes in OccScore



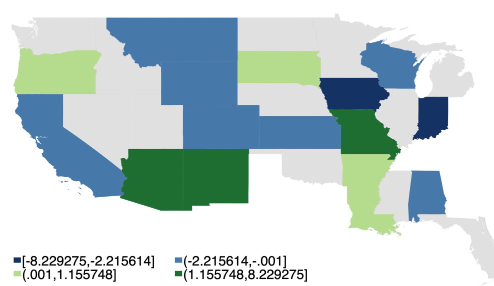
(a) Homesteads, 1850–1900



(b) Cash Sales, 1850–1900



(c) Homesteads, 1900–1940

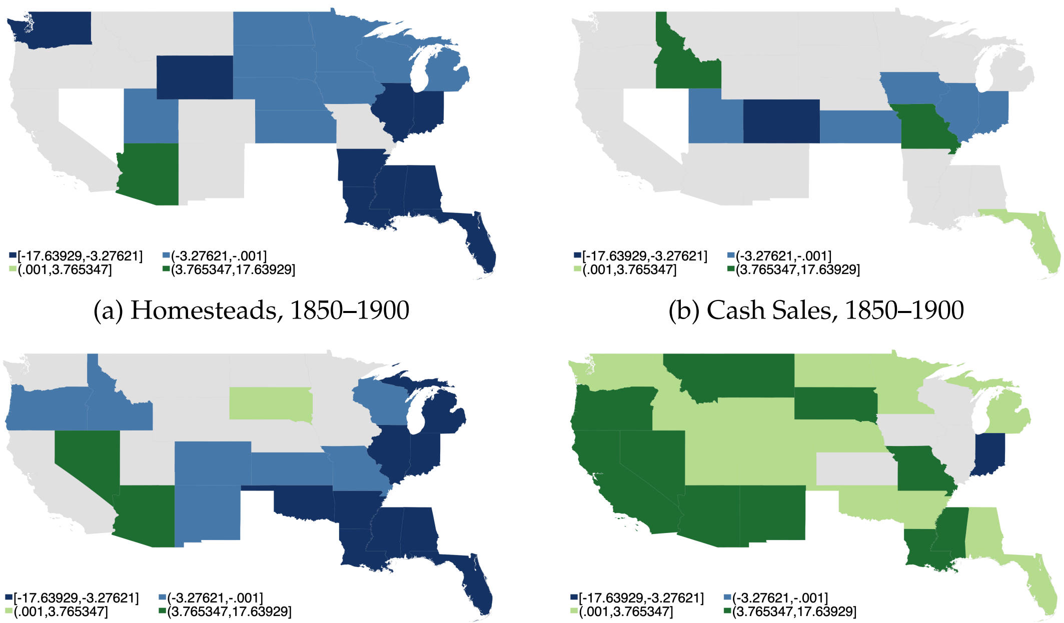


(d) Cash Sales, 1900–1940

Notes: Each panel maps the average post-patent difference in occupation scores (OccScore) between patentees and non-landowners, by patent state. Estimates are derived from state-specific extended two-way fixed effects (ETWFE) models with home-patent county pair fixed effects, weighted using inverse probability weights. The ATT for each state is the mean of post-treatment ($t \geq 0$) event-time coefficients, with the pre-patent decade ($t = -10$) normalized to zero. Standard errors are clustered at the contemporaneous county level. Dark shading indicates larger magnitude effects; gray indicates a statistically insignificant effect ($p > 0.05$); white indicates states with too few patentees to estimate (fewer than 50 treated individuals). Color scales are held constant across panels within this figure. Top row: 1850–1900; bottom row: 1900–1940. Left column: homestead patents; right column: cash patents.

Relative changes in OccScore are uniformly negative across both space and time for homesteads (Figure 9), consistent with the lack of both temporal and regional variation in this metric. There is more variation over both space and time for cash sales.

Figure 10: Relative Changes in Ward Score



(a) Homesteads, 1850–1900

(b) Cash Sales, 1850–1900

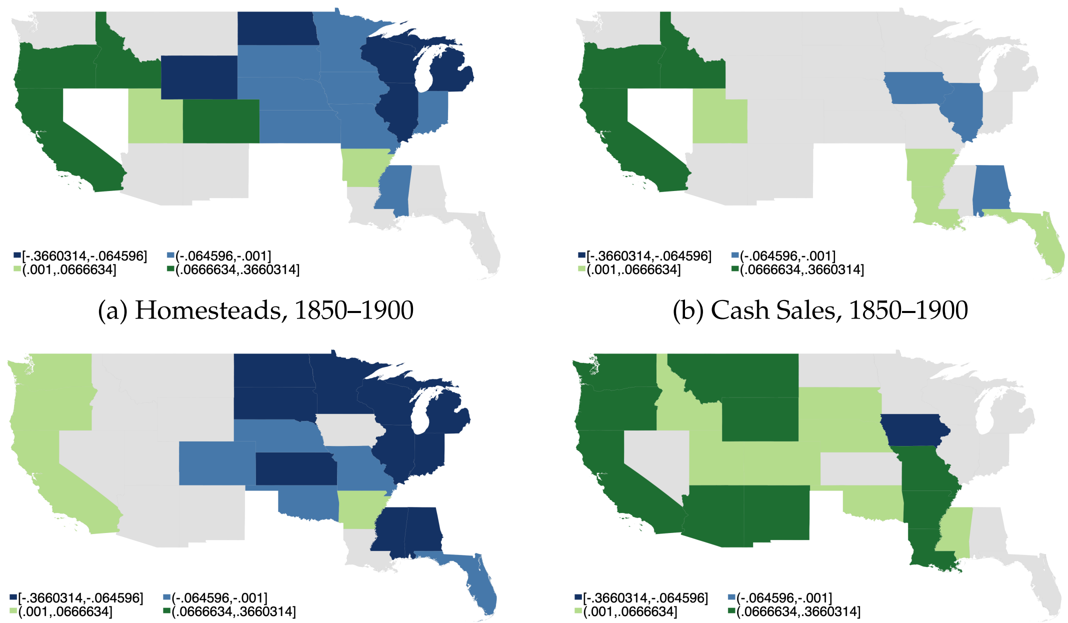
(c) Homesteads, 1900–1940

(d) Cash Sales, 1900–1940

Notes: Each panel maps the average post-patent difference in Ward scores between patentees and non-landowners, by patent state. Estimates are derived from state-specific extended two-way fixed effects (ETWFE) models with home-patent county pair fixed effects, weighted using inverse probability weights. The ATT for each state is the mean of post-treatment ($t \geq 0$) event-time coefficients, with the pre-patent decade ($t = -10$) normalized to zero. Standard errors are clustered at the contemporaneous county level. Dark shading indicates larger magnitude effects; gray indicates a statistically insignificant effect ($p > 0.05$); white indicates states with too few patentees to estimate (fewer than 50 treated individuals). Color scales are held constant across panels within this figure. Top row: 1850–1900; bottom row: 1900–1940. Left column: homestead patents; right column: cash patents.

Ward Score results in Figure 10 exhibit more geographic variation for homesteaders, especially in the second era. Several states exhibit no significant change, while several states suggest relative improvements for homesteaders, especially over 1900–1940. The regional variation here suggests that farming may have been a lower-status occupation in the Midwest and Southeast (where results are consistently negative) than in the West (where several estimates are null or positive). This is consistent with the occupational variation in Ward Score results being driven by changes over time in both where homesteaders are locating and in the relative status of different occupations—together, these findings suggest important regional variation in the relative status of farming that is not adequately captured by OccScores. The results for cash sales are starkly different across eras, with mostly relative declines over 1850–1900 and near-universal gains across states over 1900-1940. This suggests that later buyers of land (who were not locked into farming) ended up in relatively high-status occupations, regardless of where they settled.

Figure 11: Relative Changes in Log Predicted Income



(a) Homesteads, 1850–1900

(b) Cash Sales, 1850–1900

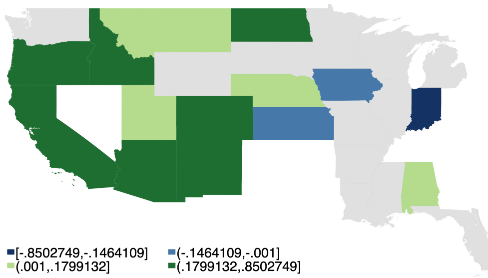
(c) Homesteads, 1900–1940

(d) Cash Sales, 1900–1940

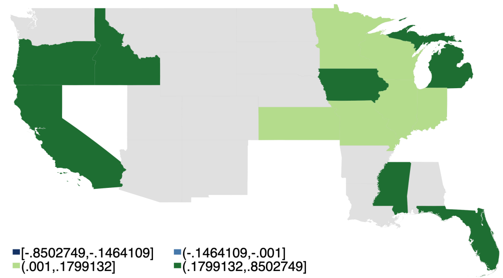
Notes: Each panel maps the average post-patent difference in log predicted income between patentees and non-landowners, by patent state. Estimates are derived from state-specific extended two-way fixed effects (ETWFE) models with home-patent county pair fixed effects, weighted using inverse probability weights. The ATT for each state is the mean of post-treatment ($t \geq 0$) event-time coefficients, with the pre-patent decade ($t = -10$) normalized to zero. Standard errors are clustered at the contemporaneous county level. Dark shading indicates larger magnitude effects; gray indicates a statistically insignificant effect ($p > 0.05$); white indicates states with too few patentees to estimate (fewer than 50 treated individuals). Color scales are held constant across panels within this figure. Top row: 1850–1900; bottom row: 1900–1940. Left column: homestead patents; right column: cash patents.

Results for predicted income in Figure 11 indicate that gains for both types of patentees were mostly concentrated in the West, especially over 1850–1900 (cash buyers see more geographically dispersed gains over 1900–1940). Here, we see alignment with the Ward Score results in some regions but not in others–homesteaders in the Midwest and Great Plains saw relative declines in Ward Scores and income across both eras, but the income results suggest large relative gains for homesteaders further West over 1850–1900. The divergence here may be explained by the farm-specific adjustments in our predicted income measure or by there being more within-group variation in the statistically predicted income metric than in the percentile-rank Ward score (many of the states with significant income improvements are simply insignificant in the Ward score specification). The latter would be consistent with the finding in Table 6—that farm operators saw relative declines in Ward scores but no net effect on predicted incomes—if different incomes effects depicted in Figure 11 net out across space, whereas the Ward Scores are more consistently negative.

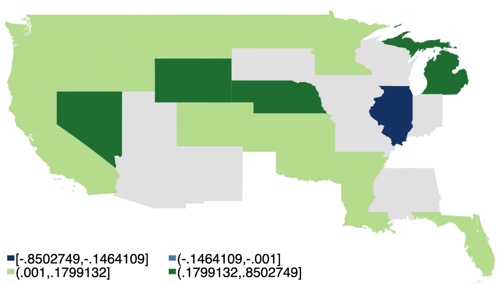
Figure 12: Relative Changes in Log Occupation-Based Wealth



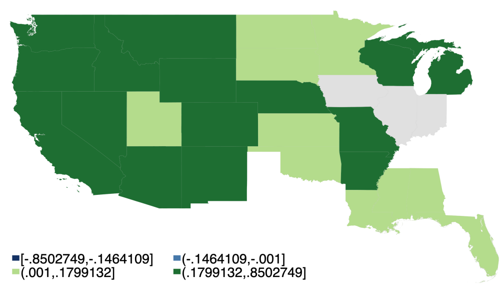
(a) Homesteads, 1850–1900



(b) Cash Sales, 1850–1900



(c) Homesteads, 1900–1940



(d) Cash Sales, 1900–1940

Notes: Each panel maps the average post-patent difference in log occupation-based wealth between patentees and non-landowners, by patent state. Estimates are derived from state-specific extended two-way fixed effects (ETWFE) models with home-patent county pair fixed effects, weighted using inverse probability weights. The ATT for each state is the mean of post-treatment ($t \geq 0$) event-time coefficients, with the pre-patent decade ($t = -10$) normalized to zero. Standard errors are clustered at the contemporaneous county level. Dark shading indicates larger magnitude effects; gray indicates a statistically insignificant effect ($p > 0.05$); white indicates states with too few patentees to estimate (fewer than 50 treated individuals). Color scales are held constant across panels within this figure. Top row: 1850–1900; bottom row: 1900–1940. Left column: homestead patents; right column: cash patents.

The occupation-based wealth estimates in Figure 12 are the most spatially uniform across patent types and eras, with mostly positive relative changes across both eras. In the first era, there is a much broader spatial distribution of gains for homesteaders than for cash buyers–with a few exceptions, homesteaders in most places saw relative improvements in wealth, whereas the gains for cash buyers were concentrated in a handful of states. The second era exhibits smaller gains for homesteaders on average, but across a fairly similar (albeit slightly broader) set of geographies than the first era. Cash buyers see large increases in relative wealth across a much larger set of states in the second era than in the first.

Taken together, the occupational and geographic decompositions reveal that variation across economic measures in Tables 4 and 5 is consistent with the differential ability if certain measures to reflect variation across geographies and over time. For instance, the persistent negative changes on OccScores for homesteaders across occupations, states, and time periods reflect the fact that farming had a lower median income than many occupations in 1950 (the year in which OccScores are defined), and that many homesteaders left some other occupation to become farmers. In contrast, changes measued in Ward scores show more variability between occupations and over space.

Cash sales exhibit significantly more variability across states and over time for OccScores and Ward Scores in particular. This difference is likely driven by the fact that homestead requirements required patentees to become "actual farmers," whereas cash sales imposed no restrictions. In other words, there is more ex-post occupational variation for cash buyers than for homesteaders (Table 3), driving more variation in Occscore and Ward score results. In contrast, the heterogeneity of wealth metrics for homesteaders across states and over time is primarily driven by the relative wealth of farmers in a given place, whereas cash buyers' changes in these metrics are less tied to the status of a particular occupation (Table 3). Income metrics show the most heterogeneity of all outcomes for homesteaders, suggesting that the farmer-specific adjustments to income measures developed by (Collins and Wanamaker, 2022) provide important refinements to previous approaches.

On the whole, the patterns presented here provide little evidence consistent with the general notion that abundant land dramatically improved the fortunes of settlers, or that the Homestead Act in particular led to massive gains for its participants. We do find some evidence of relative improvements in estimated wealth for cash buyers across both eras and for homesteaders in the second era, but we cannot rule out individual-level differences as a driver of these effects. Perhaps the most striking pattern that emerges is that homesteaders consistently perform worse than cash buyers across every single outcome, specification, and era (though not in every location).

6 Intergenerational Consequences of Land Transfers

Contemporary accounts of the Homestead Act and modern economic historians both emphasize the *intergenerational* implications of abundant land during Westward Expansion. While we see relatively modest gains for the patentees themselves across some occupations an locations (and declines in others), it is possible that generous federal land policy facilitated upward mobility for the children of patentees though the various channels discussed in Section 2. In this section, we explore whether intergenerational mobility is greater for the sons of patentees than for their peers. This serves as a direct assessment of the claims made by proponents of the Homestead Act, as well as an important exploration of potential mechanisms behind existing studies in economic history that implicitly or explicitly connect land ownership to intergenerational mobility (Long and Ferrie, 2013; Derenoncourt et al., 2023, 2024).

We follow standard practice in recent literature on economic mobility and focus on sons who are at least age 30 in the outcome year and attempt to link these sons to their fathers in a base year when the son is age 0–15. Given our sample window, this leads to the following base-outcome pairs, where the base year is the year in which we observe the son living with the father and the outcome year is the year in which we observe the son's economic status after age 30:

- 1880 $\rightarrow$ 1910, 1920, 1930, 1940
- 1900 $\rightarrow$ 1920, 1930, 1940
- 1910 $\rightarrow$ 1930, 1940

constructing these linkages involves linking sons across the base year to each outcome year using the Census Tree and using the father's information in the *base* year.

Once we have the set of fathers associated with sons that can be forward-linked from each base year, we then backward-link the fathers (via the Census Tree) to obtain at least two distinct observations of outcomes for each father. This enables us to overcome measurement error in the father's status by instrumenting their outcome in one year with their outcome in a different year (Ward, 2023). Hence, for a father in base year 1880, we would look for links across 1860–1880. An additional nuance in our setting is that both father outcomes are measured *prior* to obtaining a patent for homesteads and cash buyers. So if a father from base year 1880 obtained a patent in 1873, we would require links to the 1850 and 1860 census for that father to be included in the sample (the base years listed above may constitute one of the father's observation years if the patent is obtained after the base year).

Following Ward (2023), we construct percentile ranks for each outcome of interest—OccScore, Ward Score, predicted income, and occupation-based wealth—for both fathers and sons in each year within ten-year birth cohort bins. These percentile ranks are computed before imposing link-related sample restrictions to capture both fathers' and sons' relative standing in the population as a whole. After constructing these linkages and restricting the data to viable father-son pairs, we are left with a pooled cross section where each observation is an individual son in a specific outcome year. For each of these individuals, we observe two distinct waves of fathers' outcomes, as well as fathers' land patent status. This allows us to estimate the following:

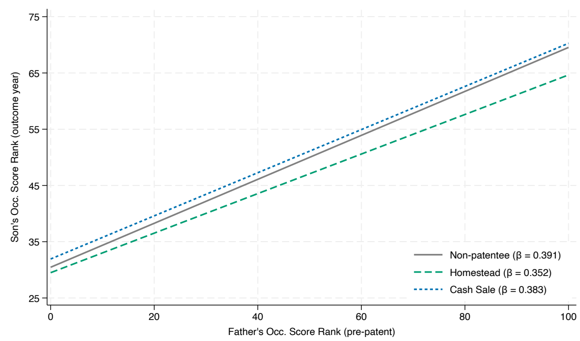
$$R_{it}^{son} = \beta_1 \hat{R}_i^{father} + \beta_2 \mathbb{1}(Homestead_i) + \beta_3 \mathbb{1}(Cash_i) + \beta_4(\hat{R}_i^{father} \times \mathbb{1}(Homestead_i)) \\ + \beta_5(\hat{R}_i^{father} \times \mathbb{1}(Cash_i)) + f(\text{age}_i) + \alpha_c + \alpha_t + \alpha_j + \varepsilon_{it} \quad (3)$$

$$R_i^{father} = \pi_1 R_i^{father,\text{prior}} + \pi_2 \mathbb{1}(Homestead_i) + \pi_3 \mathbb{1}(Cash_i) + \pi_4(R_i^{father,\text{prior}} \times \mathbb{1}(Homestead_i)) \\ + \pi_5(R_i^{father,\text{prior}} \times \mathbb{1}(Cash_i)) + f(\text{age}_i) + \alpha_c + \alpha_t + \alpha_j + \nu_{it} \quad (4)$$

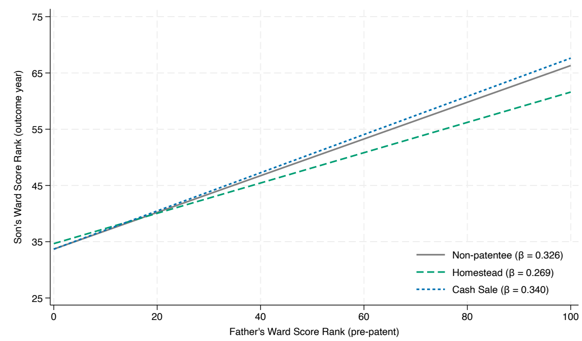
where R_{it}^{son} the percentile rank of son i in outcome year t and $\hat{R}_i^{father}$ is the father's instrumented percentile rank, defined by Equation 4. α_j are (son's) birth cohort fixed effects, α_t are outcome year fixed effects, and α_c are father's county fixed effects. $\mathbb{1}(Homestead_i)$ and $\mathbb{1}(Cash_i)$ index whether the *father* received a certain type of patent. Hence, Equation 3 estimates the persistence of occupational rank from father to sons, conditional on son's age and birth cohort, as well as father's county of residence in the base year. β_1 measures intergenerational persistence in outcomes in the comparison group. β_2 and β_3 are separate intercepts for homesteads and cash sales that allow for level differences in the outcomes of sons of homesteaders and cash buyers. The interaction terms between patent indicators and father's occupation allow the slope of the rank-rank relationship to differ for children of homesteaders and cash buyers, with these slopes captured by β_4 and β_5, respectively. We cluster standard errors by father. As before, we report results separately for patents established before vs. after 1900.

Table 8 presents the results for each outcome variable for patents prior to 1900 (Panel A) and after 1900 (Panel B). Figure 13 plots the slopes for each group and each outcome for pre-1900 patents and Figure 14 presents results for post-1900 patents. First stage F-statistics are strong across all outcome variables. Baseline rates of upward mobility are similar across both eras and all four outcome variables. These estimates, which range from 0.310 to 0.391, closely align with Ward (2023)'s rank-rank slope estimates for this time period.

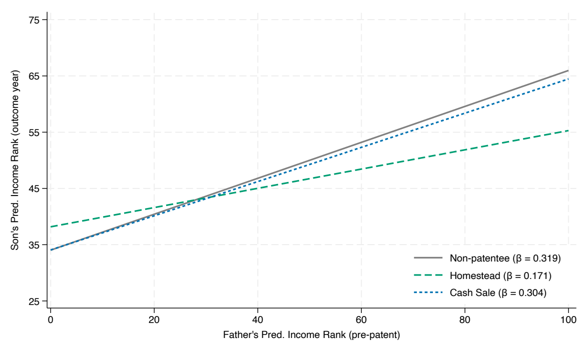
Figure 13: Intergenerational Rank-Rank Estimates (IV), Pre-1900 Patents



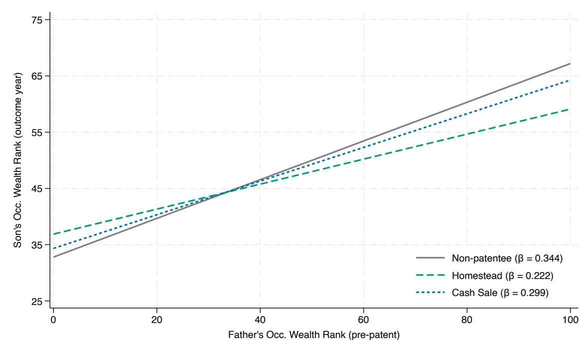
(a) OccScore



(b) Ward Score



(c) Predicted Income

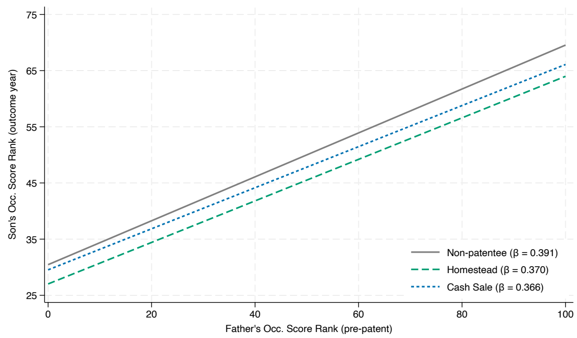


(d) Occupation-Based Wealth

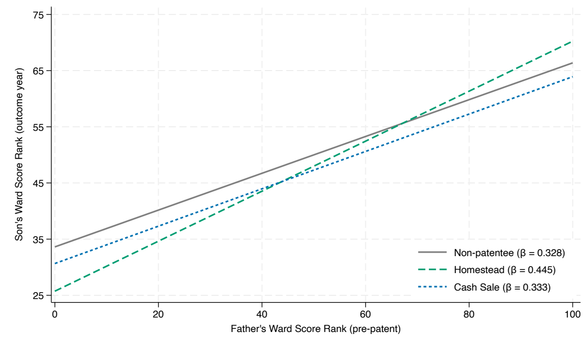
Notes: Each panel plots IV rank-rank regressions relating a son's within-birth-cohort percentile rank to his father's pre-patent percentile rank, separately by patent type. The slope (β) measures intergenerational persistence: a value of 1 implies complete persistence; a value of 0 implies complete mobility. The father's pre-patent rank is instrumented with his rank from an earlier census to address measurement error. All specifications include a quadratic in the son's age at the outcome census, birth cohort fixed effects, outcome year fixed effects, and father origin county fixed effects. Standard errors are clustered at the father level.

How did land acquisition affect the intergenerational transmission of status and wealth? The intercept coefficients measure whether the sons of homesteaders or cash buyers tend to do better than sons of patentees on average, regardless of the father's status. The interaction terms measure changes in the rank-rank slopes, which capture potential differences in the *persistence* of father's occupational status. Focusing initially on the first era (Figure 13), children of homesteaders tend to have higher incomes and wealth on average, though these estimates are of borderline statistical

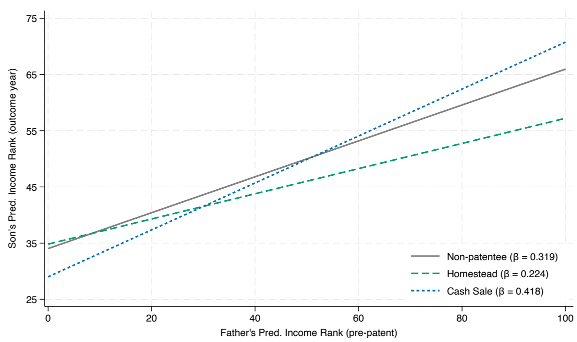
Figure 14: Intergenerational Rank-Rank Estimates (IV), Post-1900 Patents



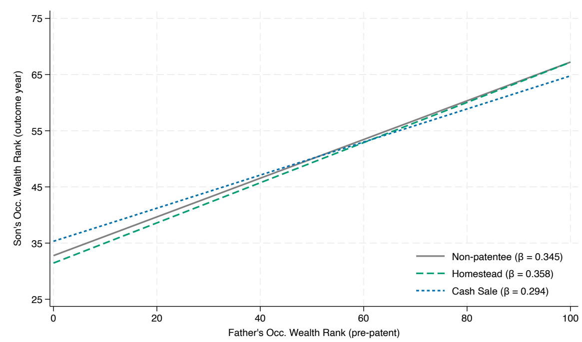
(a) OccScore



(b) Ward Score



(c) Predicted Income



(d) Occupation-Based Wealth

Notes: Each panel plots IV rank-rank regressions relating a son's within-birth-cohort percentile rank to his father's pre-patent percentile rank, separately by patent type. The slope (β) measures intergenerational persistence: a value of 1 implies complete persistence; a value of 0 implies complete mobility. The father's pre-patent rank is instrumented with his rank from an earlier census to address measurement error. All specifications include a quadratic in the son's age at the outcome census, birth cohort fixed effects, outcome year fixed effects, and father origin county fixed effects. Standard errors are clustered at the father level.

significance (Table 8). We do not find any significant average differences for the children of cash buyers. The rank-rank slope for children of homesteaders is flatter than and statistically different from the baseline slope across three of four outcomes. We do not find any differences in slopes for children of cash buyers.

The results in Figure 13 and Panel A of Table 8 imply that homesteading is associated with a leveling of outcomes across generations. Sons of homesteaders toward the bottom of the distribution tend to do better than comparable sons of non-homesteaders because they benefit from both the level boost (intercept) and the flatter slope (interaction). In contrast, sons of homesteaders in the top of the distribution inherit less advantage from their fathers on average. The upshot is that the first era of homesteading does appear to be associated with greater upward mobility for the sons of relatively disadvantaged fathers. For Ward score and income (but not for Occscore or wealth), we find that the homestead slope is significantly different from the cash slope.

In contrast, we fail to find much evidence of differential intergenerational mobility for the children of patentees who obtained patents after 1900. Qualitatively, the patterns look similar for Occscores and for income across eras, but the coefficients are less precisely estimated (despite having a comparable number of observations). Both Ward scores and wealth actually exhibit a reversal in terms of the direction of the intercept and slope effects for homesteaders—the intercept shift for Ward scores is highly significant, but the slope change is only marginally so. We find no significant differences between the implied homestead and cash slopes for any of the measures of economic standing.

In Appendix D, we provide a variety of results to demonstrate the robustness of our core findings and to explore various margins of heterogeneity. Table D1 presents results pooled across eras to compare OLS results (Panel A) to IV results (Panel B), still using the rank-rank approach. Consistent with Ward (2023), the OLS slope coefficients are attenuated relative to the IV coefficients. The OLS also exhibits an average penalty for sons of homesteaders across occupations.

Table D2 presents pooled results estimated in levels of the key outcome variables, rather than percentile ranks for both OLS and IV. Table D3 reports differential changes in the slope for children of homesteaders based on their age at the time of patenting using an OLS specification on levels. Table D4 reports differential effects based on the father's occupation, region, and nativity–these specifications specifically explore how the average change for children of homesteaders varies across father characteristics, rather than the slope. Finally, Table D5 reports separate effects for each base-outcome pair for both the levels and rank-rank specifications.

These results are a bit puzzling in the context of Section 5, where we find that later waves of homesteaders may have experienced larger relative increases in wealth and that cash buyers consistently have better relative performance than homesteaders in terms of both income and wealth. These initial (relative) gains from land titling do not appear to be transmitted across generations. We note that this is at least qualitatively consistent with the finding in Bleakley and Ferrie (2016) that the children of winners of the Georgia land lottery did not fare better than children of the losers, despite the mechanical wealth improvement for the winners. In the conclusion, we discuss several possible explanations that fit the broader context of our study.

Table 8: Intergenerational Mobility Results (Rank-Rank) by Era

	(1) Occ. Score	(2) Ward Score	(3) Pred. Income	(4) Occ. Wealth
Panel A: Pre-1900 Patents				
Father Rank	0.391***	0.326***	0.319***	0.344***
	(0.007)	(0.005)	(0.006)	(0.007)
Homestead	-0.966	0.980	4.151**	4.084*
	(2.235)	(1.086)	(2.117)	(2.132)
Cash Sale	1.468	-0.002	0.034	1.545
	(1.165)	(0.770)	(1.425)	(1.187)
Homestead × Father Rank	-0.039	-0.057**	-0.148***	-0.122**
	(0.080)	(0.027)	(0.055)	(0.053)
Cash Sale × Father Rank	-0.007	0.013	-0.015	-0.045
	(0.053)	(0.021)	(0.036)	(0.028)
Implied Homestead Slope	0.352***	0.269***	0.171***	0.222***
	(0.079)	(0.027)	(0.055)	(0.053)
Implied Cash Sale Slope	0.383***	0.340***	0.304***	0.299***
	(0.052)	(0.022)	(0.036)	(0.029)
pval(Home = Cash)	0.736	0.036	0.039	0.188
First-stage F	19.456	3839.352	36.681	368.812
	1,365,941	1,069,355	1,064,807	1,425,574
Panel B: Post-1900 Patents				
Father Rank	0.391***	0.328***	0.319***	0.345***
	(0.008)	(0.005)	(0.006)	(0.007)
Homestead	-3.430	-7.883***	0.796	-1.326
	(4.602)	(2.468)	(4.486)	(5.294)
Cash Sale	-0.922	-2.958	-5.025	2.555
	(7.575)	(4.059)	(8.179)	(6.276)
Homestead × Father Rank	-0.021	0.117*	-0.095	0.013
	(0.172)	(0.066)	(0.117)	(0.135)
Cash Sale × Father Rank	-0.025	0.005	0.098	-0.050
	(0.260)	(0.093)	(0.193)	(0.151)
Implied Homestead Slope	0.370**	0.445***	0.224*	0.358***
	(0.172)	(0.067)	(0.117)	(0.135)
Implied Cash Sale Slope	0.366	0.333***	0.418**	0.294*
	(0.260)	(0.093)	(0.192)	(0.151)
pval(Home = Cash)	0.990	0.321	0.391	0.758
First-stage F	5.733	3851.366	7.561	1055.235
	1,313,046	1,026,946	1,022,545	1,370,169
Birth Cohort FE	✓	✓	✓	✓
Outcome Year FE	✓	✓	✓	✓
Father Origin County FE	✓	✓	✓	✓

Notes: This table reports estimates of intergenerational rank-rank persistence, separately by patent era. Father and son outcomes are converted to within-birth-cohort percentile ranks (0–100). Each column corresponds to a different outcome measure. The father's pre-patent rank is instrumented with his rank from an earlier census to address measurement error. All specifications include a quadratic in the son's age at the outcome census, birth cohort fixed effects, outcome year fixed effects, and father origin county fixed effects. Standard errors are clustered at the father level. *p<0.10, **p<0.05, ***p<0.01.

7 Conclusion

This paper develops a novel, spatially weighted strategy to link historical land patents issued by the U.S. government to historical census records over 1850 to 1940, with a focus on homesteads and cash sales. To our knowledge, this is by far the most rigorous and comprehensive attempt to study the recipients of the massive and historically unique transfer of federal land to private ownership in the late nineteenth and early twentieth centuries. We provide new descriptive evidence on the characteristics of land recipients, their relative performance over time, and the implications for their children.

We find that federal land disposal—and homesteading in particular—was relatively progressive in the sense that individuals who received land tended, on average, to be of lower socioeconomic standing Hence, homesteading likely facilitated greater rates of land ownership for a swath of the population that would otherwise not have been able to purchase land. Comparing relative performance over time for homesteaders and buyers compared to non-patentees tells a mixed story that depends on how we measure economic standing and which era we focus on. Although we caution against a causal interpretation of these relationships, we wish to emphasize a striking finding: across both eras of settlement, four measures of economic standing, and four different comparison groups, homesteaders perform worse than cash buyers over time. This casts significant doubt on the narrative that homesteading was an "avenue to wealth" for the settlers themselves. Although there is little evidence that homesteaders themselves improved relative to purchasers of public land, we do find improvements in intergenerational mobility for the children of homesteaders that are absent for the descendants of cash buyers.

This presents an empirical puzzle: there is stronger evidence for improved economic performance of late homesteaders compared to the first wave, but the effects for their children are exactly the opposite. There are several possible explanations. First, previous research has emphasized that the second wave of settlement was qualitatively different, and was targeted at more marginal lands (Allen and Leonard, 2026). Hence, the income and wealth gains for patentees themselves may have been fleeting. Second, it may simply be the case that we lack sufficient power or a long enough time horizon to identify effects for the sons of fathers who patented after 1900. A third possible explanation is that higher rates of intergenerational mobility for farmers previously emphasized in the literature (Long and Ferrie, 2013; Song et al., 2020) have more to do with *farming* than with *land ownership* per se. E.g., the cultural traits of farmers (and early homesteaders in particular) may drive the upward mobility of their children more than land bequests and inheritances. This would be consistent with Bazzi et al. (2020) and Leonard and Smith (2021), insofar as early homesteaders likely had more "frontier experience"—which has been associated with intergenerational mobility—than the later wave. Disentangling these explanation is beyond the scope of the present analysis, but we note this is a fruitful area for future study.

This paper provides a comprehensive descriptive and comparative account of the individuals who obtained land from the federal government over 1850 – 1940 on relatively generous terms. Despite numerous studies emphasizing the status of "land ownership" as a mechanism for intergenerational mobility in U.S. history (Long and Ferrie, 2013; Collins and Wanamaker, 2022; Derenoncourt et al., 2023, 2024), this is the first study on economic mobility to systematically grapple with the *process* of land acquisition and the federal policies that shaped it. Federal land policies were conditional transfers of land that, by design, facilitated selection into land ownership, challenging a causal analysis. In ongoing work, we attempt to provide additional support for the descriptive results here by constructing within-family comparisons of patentees (to brothers) and their children (to cousins) and by isolating episodes of narrow, quasi-random assignment into homesteading. We hope other scholars will use the data we have assembled here to continue shedding new light on the relationship between land acquisition and economic mobility in American history.

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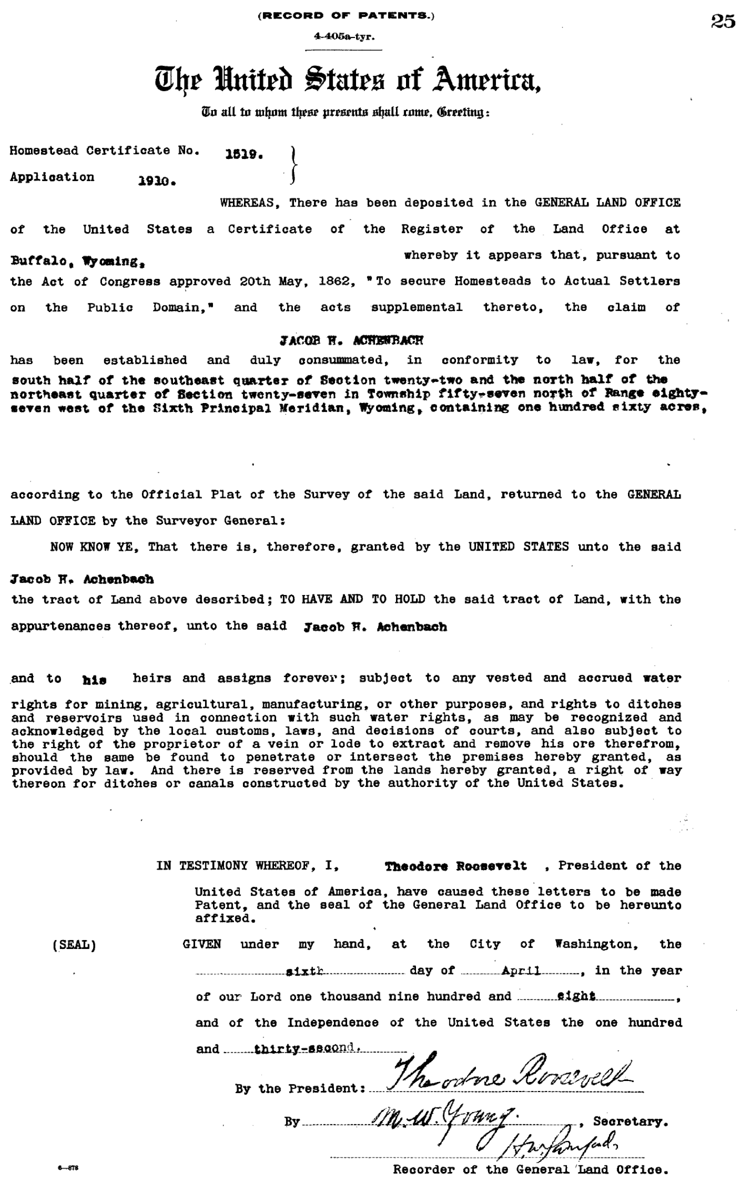
Appendix A: Data Construction and Linking

Figure A1: Example Land Patent Image



(RECORD OF PATENTS.) 25

4-405a-tyr.

The United States of America,

To all to whom these presents shall come, Greeting:

Homestead Certificate No. 1519.
Application 1910.

WHEREAS, There has been deposited in the GENERAL LAND OFFICE of the United States a Certificate of the Register of the Land Office at Buffalo, Wyoming, whereby it appears that, pursuant to the Act of Congress approved 20th May, 1862, "To secure Homesteads to Actual Settlers on the Public Domain," and the acts supplemental thereto, the claim of

JACOB H. ACHENBACH

has been established and duly consummated, in conformity to law, for the south half of the southeast quarter of Section twenty-two and the north half of the northeast quarter of Section twenty-seven in Township fifty-seven north of Range eighty-seven west of the Sixth Principal Meridian, Wyoming, containing one hundred sixty acres,

according to the Official Plat of the Survey of the said Land, returned to the GENERAL LAND OFFICE by the Surveyor General:

NOW KNOW YE, That there is, therefore, granted by the UNITED STATES unto the said Jacob H. Achenbach the tract of Land above described; TO HAVE AND TO HOLD the said tract of Land, with the appurtenances thereof, unto the said Jacob H. Achenbach

and to his heirs and assigns forever; subject to any vested and accrued water rights for mining, agricultural, manufacturing, or other purposes, and rights to ditches and reservoirs used in connection with such water rights, as may be recognized and acknowledged by the local customs, laws, and decisions of courts, and also subject to the right of the proprietor of a vein or lode to extract and remove his ore therefrom, should the same be found to penetrate or intersect the premises hereby granted, as provided by law. And there is reserved from the lands hereby granted, a right of way thereon for ditches or canals constructed by the authority of the United States.

IN TESTIMONY WHEREOF, I, Theodore Roosevelt , President of the United States of America, have caused these letters to be made Patent, and the seal of the General Land Office to be hereunto affixed.

(SEAL) GIVEN under my hand, at the City of Washington, the sixth day of April, in the year of our Lord one thousand nine hundred and eight, and of the Independence of the United States the one hundred and thirty-second.

By the President: Theodore Roosevelt

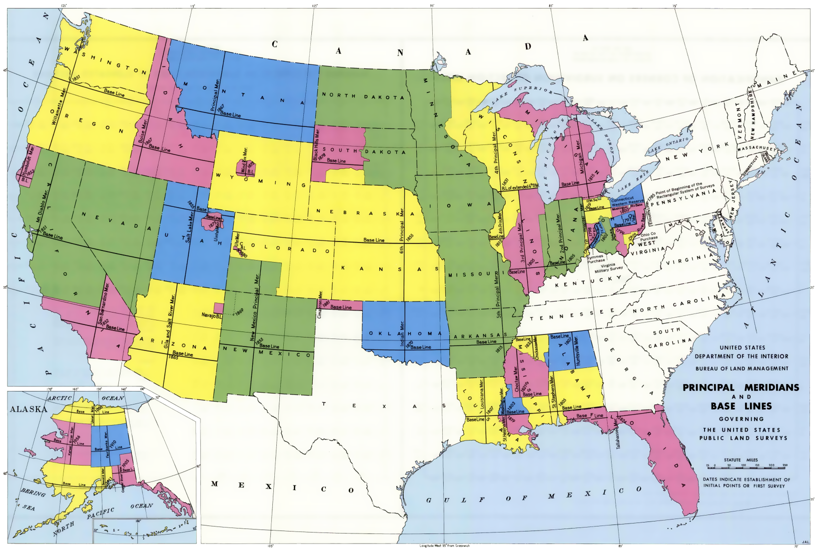
By M. W. Young, Secretary.

H. W. Sanford

Recorder of the General Land Office.

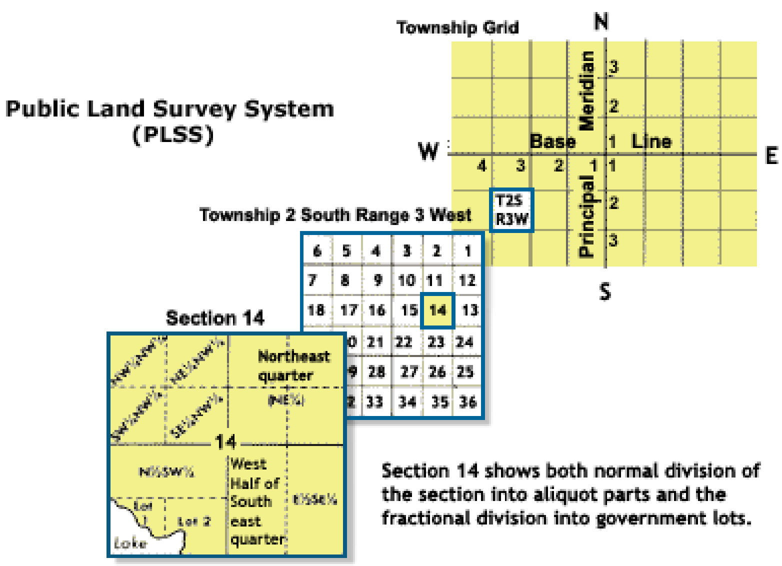
Notes: This figure depicts and example land patent, which records the date, type, location, and recipient of each land title issued by the General Land Office.

Figure A2: Spatial Extent of the Public Land Survey System



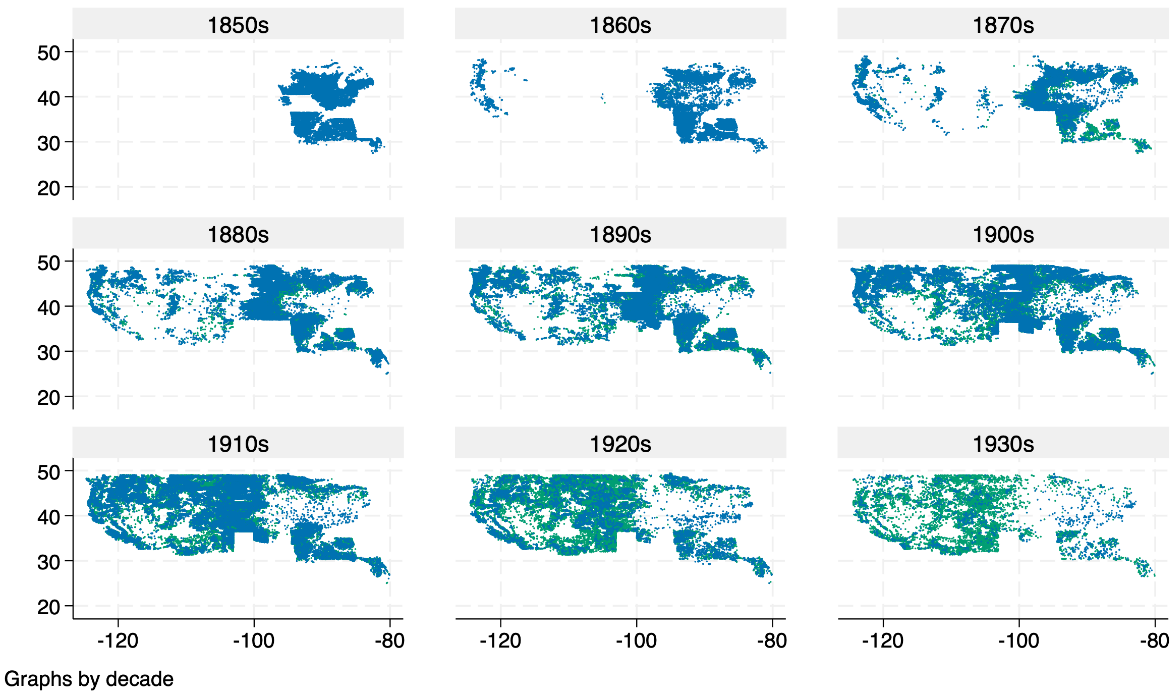
Notes: This figure depicts the spatial extent of the Public Land Survey System established by the Land Ordinance of 1785. Our land patents are all within the geographic extent of the PLSS.

Figure A3: PLSS Coordinate System



Notes: This figure depicts an example of how precise locations of individual plots are referenced withing the PLSS, which we utilize to locate landowners in space.

Figure A4: Patent-to-Census Matches



Notes: This figure depicts matches rates from the land entry files to full count historical censuses across all patents issued between 1860 and 1940.

Table A1: Patent-to-Census Match Rates

	1860s	1870s	1880s	1890s	1900s	1910s	1920s	1930s
Panel A: Homesteads								
E. North Central	48.3% (1,100)	43.7% (12,908)	44.9% (8,347)	45.0% (5,264)	58.6% (5,006)	64.6% (2,225)	65.3% (495)	55.9% (102)
W. North Central	42.9% (1,711)	31.1% (64,898)	40.6% (81,983)	32.8% (84,369)	48.9% (63,280)	45.3% (73,848)	34.8% (17,972)	34.7% (2,427)
South Atlantic	–	33.6% (1,067)	32.2% (3,534)	36.7% (5,181)	60.0% (3,210)	49.0% (3,029)	47.8% (1,181)	61.4% (223)
E. South Central	–	43.2% (1,731)	40.8% (8,429)	42.3% (19,845)	64.5% (10,572)	65.8% (5,219)	67.4% (900)	73.1% (227)
W. South Central	0.0% (1)	39.7% (5,661)	38.1% (11,984)	42.6% (27,873)	62.9% (66,446)	62.4% (24,716)	58.6% (4,511)	57.7% (1,256)
Mountain	9.1% (11)	38.8% (1,439)	28.9% (6,287)	24.7% (15,956)	31.4% (25,577)	36.7% (148,651)	30.5% (110,531)	29.8% (29,530)
Pacific	31.3% (179)	23.4% (3,315)	26.6% (7,952)	31.8% (19,951)	45.4% (15,092)	43.5% (26,754)	40.0% (17,811)	48.0% (6,013)
Overall	44.0% (3,002)	33.5% (91,019)	39.0% (128,516)	35.0% (178,439)	52.5% (189,183)	42.7% (284,442)	33.4% (153,401)	34.2% (39,778)
Panel B: Cash Sales								
E. North Central	23.8% (19,790)	8.3% (22,990)	9.2% (18,367)	27.0% (3,958)	50.0% (3,249)	49.5% (1,041)	45.8% (1,072)	38.3% (488)
W. North Central	11.8% (26,592)	20.9% (41,153)	20.5% (125,329)	15.5% (82,681)	28.9% (61,378)	32.4% (54,247)	36.4% (3,871)	27.5% (538)
South Atlantic	42.3% (1,026)	15.6% (270)	13.0% (8,645)	18.1% (950)	47.5% (1,412)	42.2% (1,441)	44.6% (707)	60.0% (70)
E. South Central	42.1% (17,355)	22.9% (319)	16.3% (7,227)	29.3% (3,163)	50.7% (2,985)	54.8% (1,751)	57.8% (870)	50.0% (254)
W. South Central	40.9% (27,681)	29.7% (1,417)	19.5% (7,193)	26.2% (9,585)	40.4% (24,241)	52.7% (6,773)	37.6% (2,797)	45.6% (360)
Mountain	5.0% (221)	26.2% (2,909)	19.0% (8,031)	17.8% (17,589)	27.1% (22,335)	33.4% (46,454)	36.2% (6,638)	41.7% (1,184)
Pacific	17.1% (5,717)	15.8% (10,114)	21.9% (11,863)	21.5% (22,749)	31.4% (25,196)	40.8% (15,670)	45.3% (3,538)	46.6% (616)
Overall	28.3% (98,382)	16.9% (79,172)	18.8% (186,655)	18.1% (140,675)	32.2% (140,796)	35.5% (127,377)	39.9% (19,493)	41.3% (3,510)

Notes: This table reports match rates between land records and the full-count historical censuses by census region and *patent* decade for homesteads (Panel A) and cash sales (Panel B), with the total number of patents in each cell reported in parentheses.

Table A2: Unmatched Land Records

Patent Decade	Unmatched-to-Pop. Ratio			Share of Unmatched	
	Mean	Median	99th Pctl	% Homestead	% Cash Sale
1860s	0.004	0.000	0.055	3.2	96.8
1870s	0.023	0.000	0.185	64.1	35.9
1880s	0.015	0.000	0.154	58.0	42.0
1890s	0.031	0.000	0.378	68.4	31.6
1900s	0.005	0.000	0.086	61.9	38.1
1910s	0.005	0.000	0.108	68.4	31.6
1920s	0.002	0.000	0.033	83.6	16.4
1930s	0.000	0.000	0.009	87.4	12.6

Notes: Each row corresponds to a patent decade. The left panel reports the ratio of unmatched patents (those not linked to any individual in the full-count census) to the total county population, at the mean, median, and 99th percentile across counties. The right panel reports the composition of unmatched patents by type: the share classified as homesteads, cash sales, or other patent types. A patent is classified as unmatched if no individual in the census closest to the patent date can be linked to the patentee name and county using the matching procedure described in Section 3.

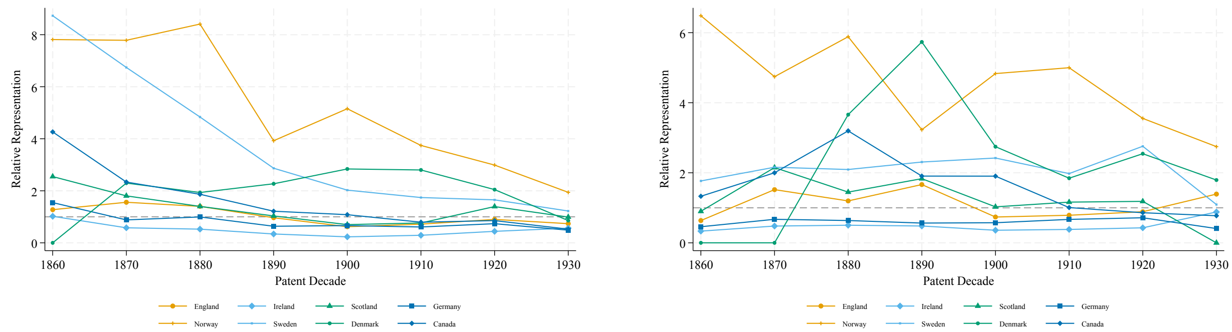
Table A3: Census-to-Census Link Rates

	Homestead		Cash Sale		Control	
Census Pair	N	Rate (%)	N	Rate (%)	N	Rate (%)
Panel A: 1850–1900						
1850 → 1860	2,682	69.0	9,120	85.1	238,461	82.8
1850 → 1870	2,682	58.3	9,120	57.0	238,461	51.8
1850 → 1880	2,682	44.4	9,120	33.5	238,461	28.8
1860 → 1870	4,690	83.1	12,132	70.7	351,958	66.9
1860 → 1880	4,690	71.3	12,132	47.0	351,958	42.4
1870 → 1880	12,560	88.4	13,925	72.3	451,806	69.6
1870 → 1900	12,560	44.2	13,925	23.6	451,806	24.8
1880 → 1900	23,480	66.6	17,590	50.6	624,523	51.8
Panel B: 1900–1940						
1900 → 1910	43,249	90.2	14,031	92.0	10,563,790	83.4
1900 → 1920	43,249	70.1	14,031	70.8	10,563,790	53.9
1900 → 1930	43,249	44.2	14,031	45.7	10,563,790	29.6
1910 → 1920	63,052	81.7	21,683	80.9	14,848,831	70.6
1910 → 1930	63,052	57.5	21,683	57.8	14,848,831	45.6
1910 → 1940	63,052	29.6	21,683	31.8	14,848,831	24.9
1920 → 1930	65,393	72.8	20,912	72.8	17,569,705	70.4
1920 → 1940	65,393	42.0	20,912	43.1	17,569,705	45.8
1930 → 1940	55,068	59.3	18,301	61.5	22,156,447	71.7

Notes: Each row reports cumulative linkage rates between an origin and target census year. N is the number of individuals observed in the origin census. Rate is the percentage of those individuals who are also observed in the target census. For non-consecutive pairs (e.g., 1850 → 1870), the rate conditions on the individual being linked in all intermediate censuses as well (e.g., also observed in 1860). "Control" includes all individuals in the analysis panel who never received a federal land patent. Linkage rates reflect the intersection of survival, census enumeration, and successful record linkage via Census Tree crosswalks. Census pairs spanning more than 30 years are omitted to match the inverse probability weighting structure. The 1880 → 1900 pair in Panel A spans 20 years due to the missing 1890 census. Differential attrition across groups is addressed via inverse probability weights following Abramitzky et al. (2014b).

Appendix B: Selection into Land Ownership

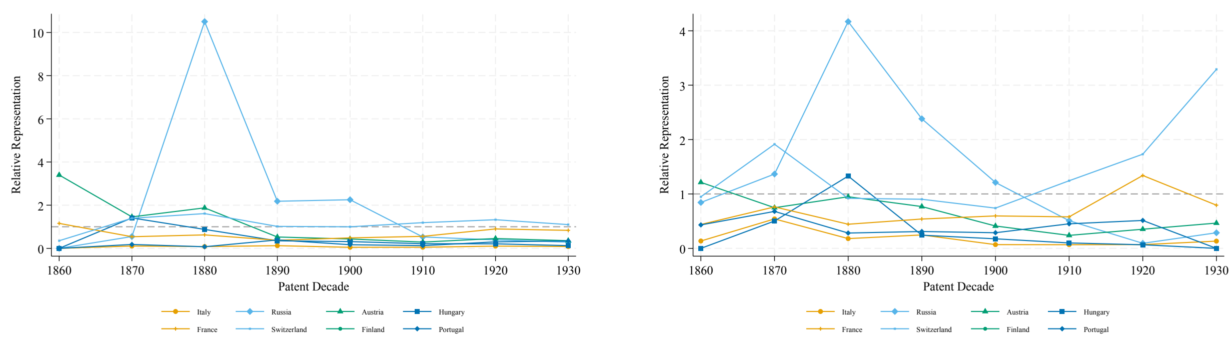
Figure B1: Relative Representation of Country-Level Birthplace Among Landowners – NW Europe



(a) Homesteads (b) Cash Sales

Notes: Each line plots the relative representation of a Northwestern European country of birth among homesteaders (panel a) and cash buyers (panel b) over patent decades. Relative representation is the share of a given birthplace among patentees divided by its share in the general population of the same census. Values above the dashed line at one indicate overrepresentation among patentees.

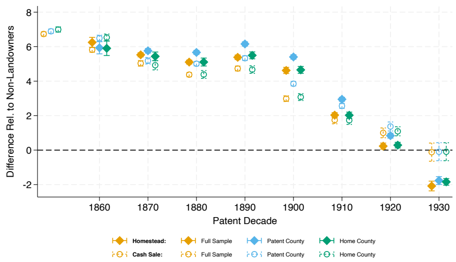
Figure B2: Relative Representation of Country-Level Birthplace Among Landowners – SE Europe



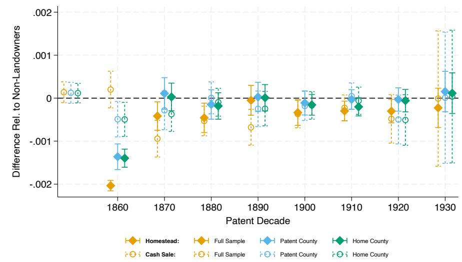
(a) Homesteads (b) Cash Sales

Notes: Each line plots the relative representation of a Southern and Eastern European country of birth among homesteaders (panel a) and cash buyers (panel b) over patent decades. Relative representation is defined as in Figure B7. Values above the dashed line at one indicate overrepresentation among patentees.

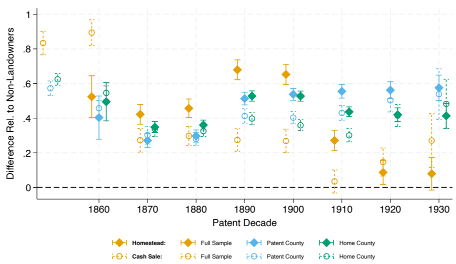
Figure B3: Selection into Land Ownership by Decade – Covariates



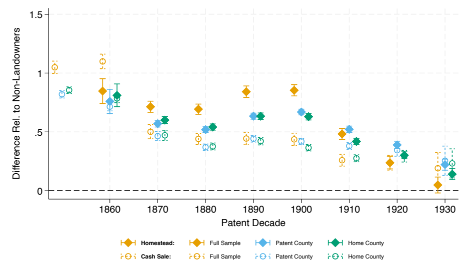
(a) Age



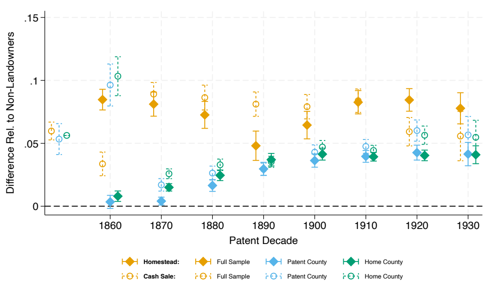
(b) Literacy



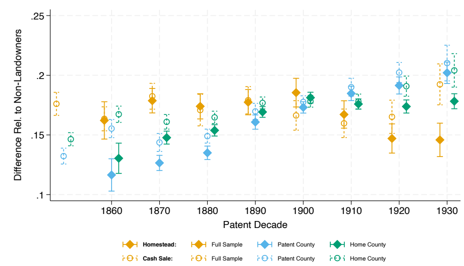
(c) Family Size



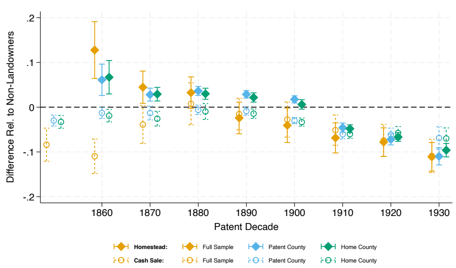
(d) Number of Children



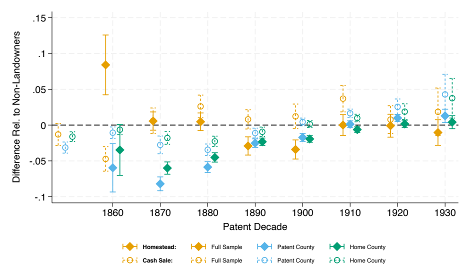
(e) White



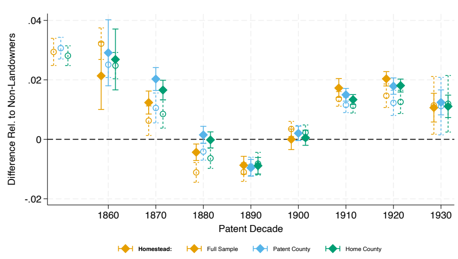
(f) Share of Possible Census Links



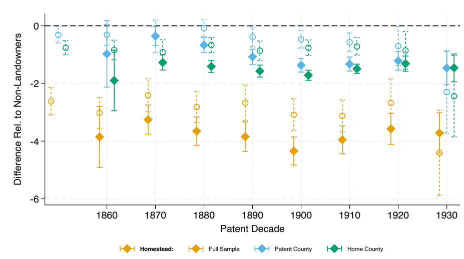
(g) 1st Generation Immigrants



(h) 2nd Generation Immigrants



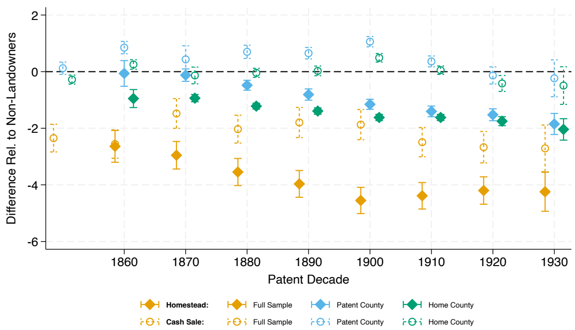
(i) Labor Force Participation



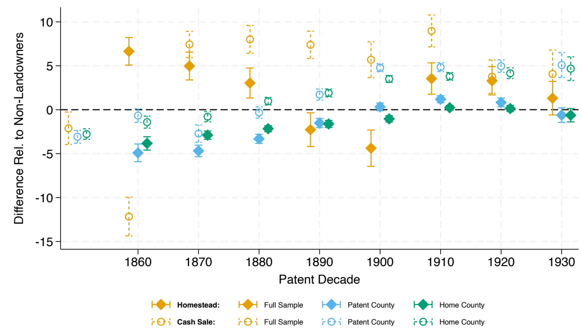
(j) Father's OccScore

Notes: Each panel plots coefficients from decade-specific cross-sectional regressions of the indicated outcome on homestead and cash sale indicators, comparing patentees to non-landowners. Three specifications are shown for each patent type: no geographic controls (Full Sample), patent county fixed effects, and origin county fixed effects. Capped bars denote 95% confidence intervals. The dashed line at zero indicates no difference relative to non-landowners. The 1890s column is inferred from adjacent census data due to the missing 1890 census.

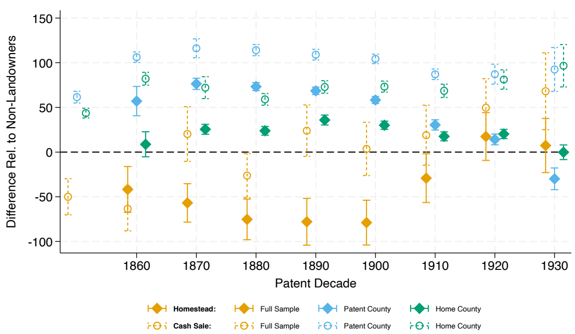
Figure B4: Selection into Land Ownership by Decade – Outcomes



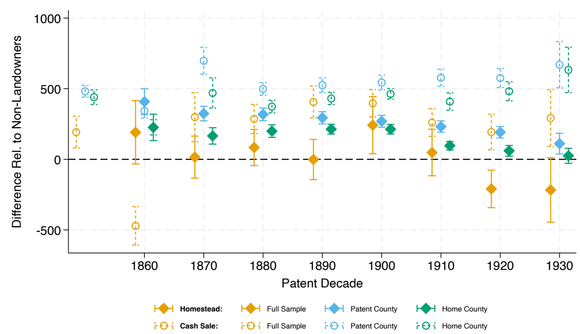
(a) OccScore



(b) Ward Score



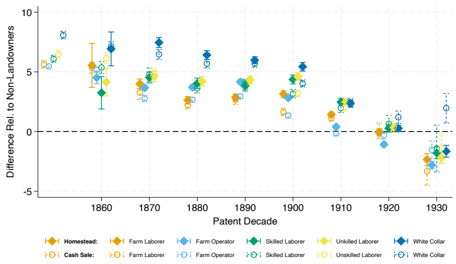
(c) Predicted Income



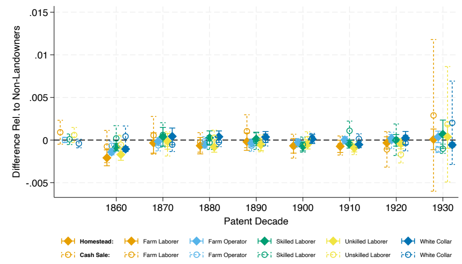
(d) Occupation-Based Wealth

Notes: Each panel plots coefficients from decade-specific cross-sectional regressions of the indicated outcome on homestead and cash sale indicators, comparing patentees to non-landowners. Three specifications are shown for each patent type: no geographic controls (Full Sample), patent county fixed effects, and origin county fixed effects. Capped bars denote 95% confidence intervals. The dashed line at zero indicates no difference relative to non-landowners. The 1890s column is inferred from adjacent census data due to the missing 1890 census.

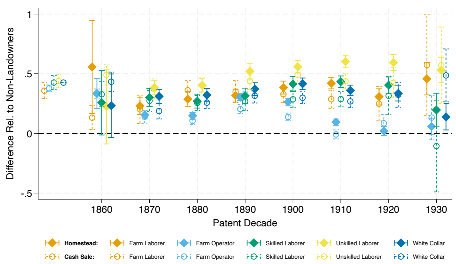
Figure B5: Selection in Land Ownership by Decade and Occupation – Covariates



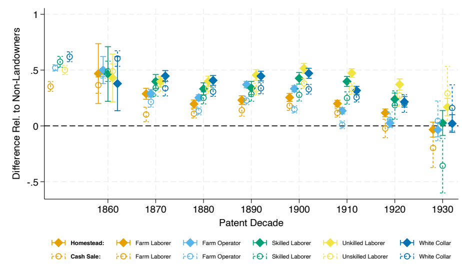
(a) Age



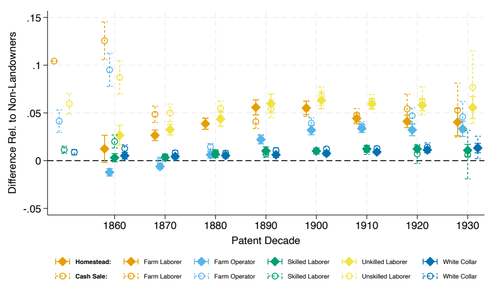
(b) Literacy



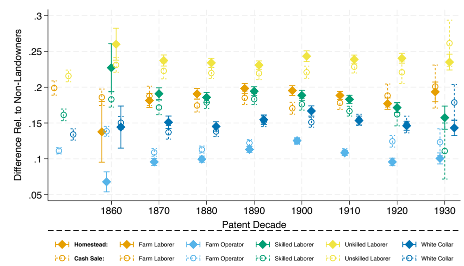
(c) Family Size



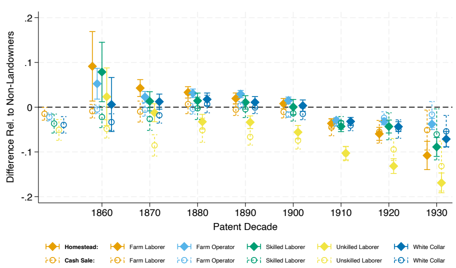
(d) Number of Children



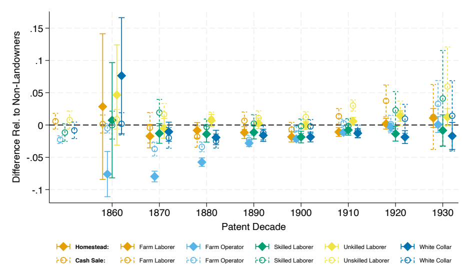
(e) White



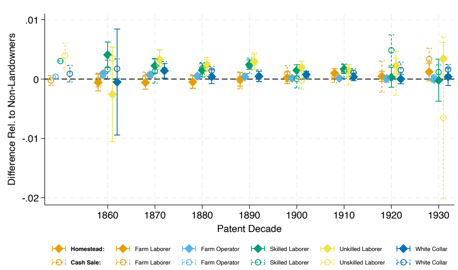
(f) Share of Possible Census Links



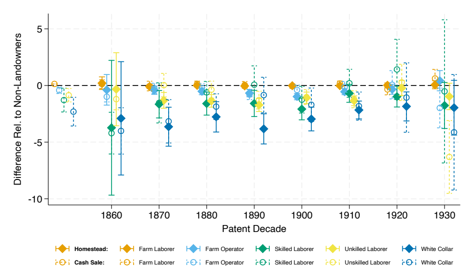
(g) 1st Generation Immigrants



(h) 2nd Generation Immigrants



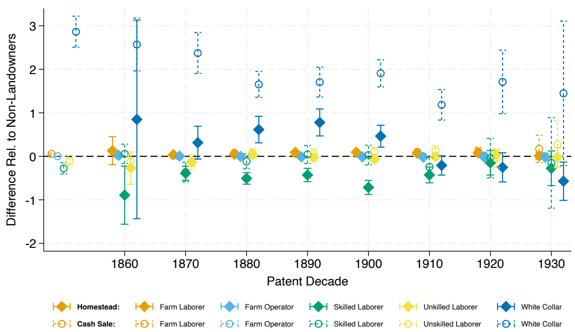
(i) Labor Force Participation



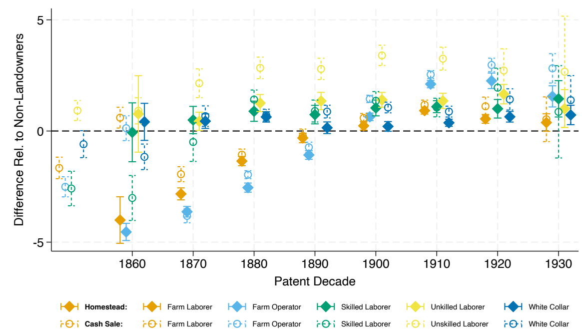
(j) Father's OccScore

Notes: Each panel plots coefficients from decade-specific cross-sectional regressions of the indicated covariate on homestead and cash sale indicators, estimated separately within each occupation class. All regressions include origin county fixed effects. Capped bars denote 95% confidence intervals. The dashed line at zero indicates no difference relative to non-landowners within the same occupation class and county.

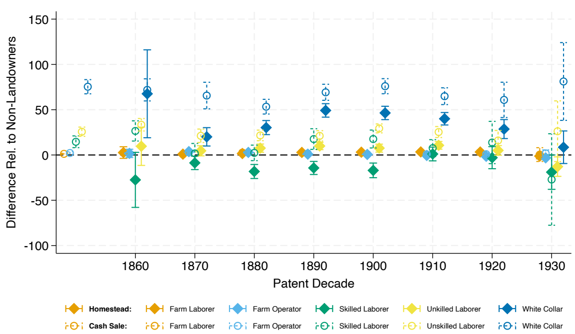
Figure B6: Selection in Land Ownership by Decade and Occupation – Outcomes



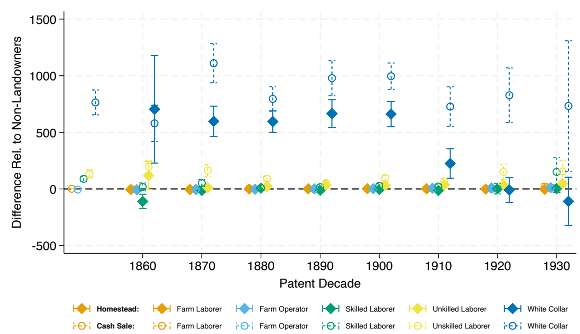
(a) OccScore



(b) Ward Score



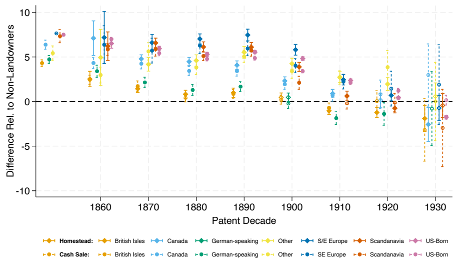
(c) Predicted Income



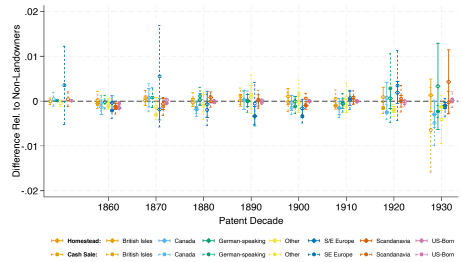
(d) Occupation-Based Wealth

Notes: Each panel plots coefficients from decade-specific cross-sectional regressions of the indicated outcome on homestead and cash sale indicators, estimated separately within each occupation class. All regressions include origin county fixed effects. Capped bars denote 95% confidence intervals. The dashed line at zero indicates no difference relative to non-landowners within the same occupation class and county.

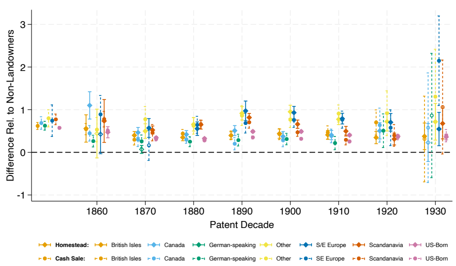
Figure B7: Selection in Land Ownership by Decade and Birthplace – Covariates



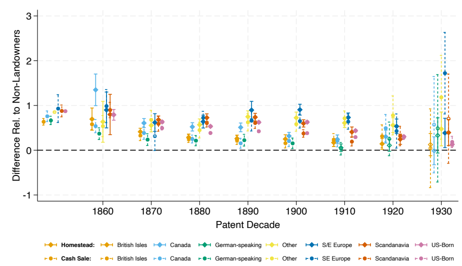
(a) Age



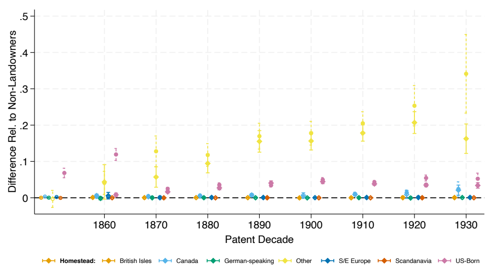
(b) Literacy



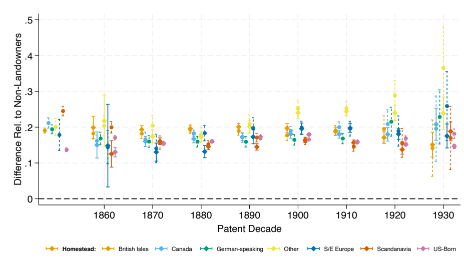
(c) Family Size



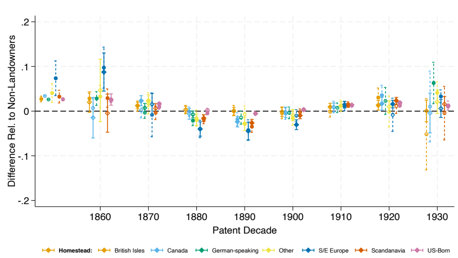
(d) Number of Children



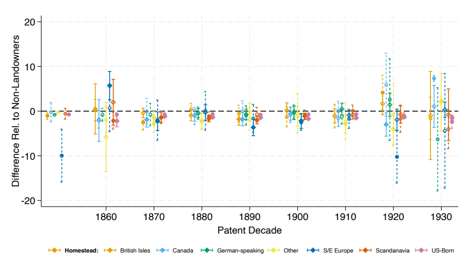
(e) White



(f) Share of Possible Census Links



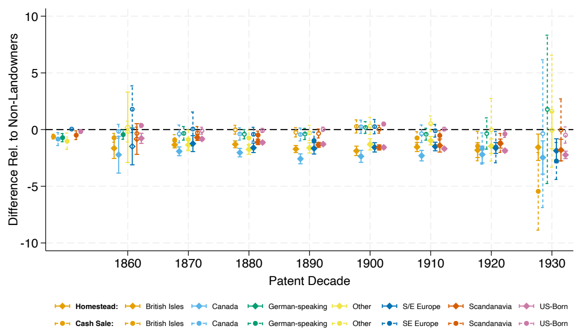
(g) Labor Force Participation



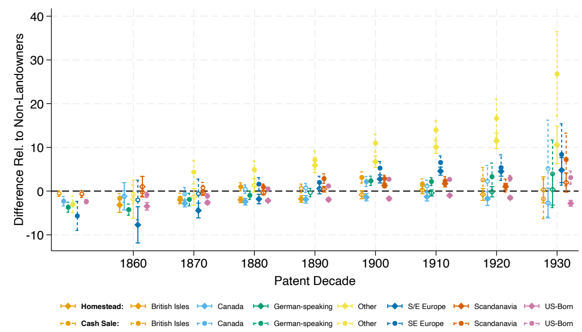
(h) Father's OccScore

Notes: Each panel plots coefficients from decade-specific cross-sectional regressions of the indicated covariate on homestead and cash sale indicators, estimated separately within each birthplace region. All regressions include origin county fixed effects. Capped bars denote 95% confidence intervals. The dashed line at zero indicates no difference relative to non-landowners within the same birthplace region and county.

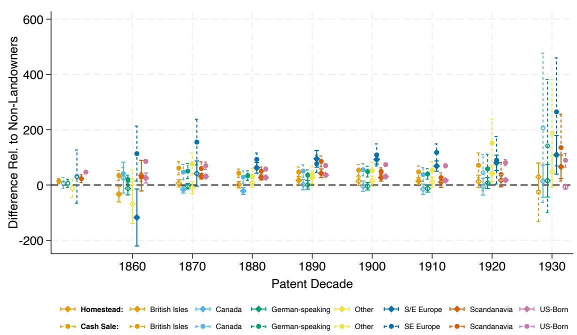
Figure B8: Selection in Land Ownership by Decade and Birthplace – Outcomes



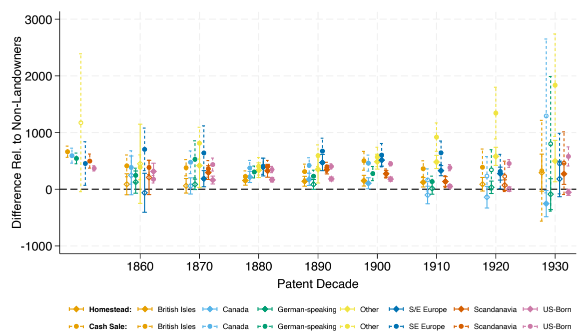
(a) OccScore



(b) Ward Score



(c) Predicted Income



(d) Occupation-Based Wealth

Notes: Each panel plots coefficients from decade-specific cross-sectional regressions of the indicated outcome on homestead and cash sale indicators, estimated separately within each birthplace region. All regressions include origin county fixed effects. Capped bars denote 95% confidence intervals. The dashed line at zero indicates no difference relative to non-landowners within the same birthplace region and county.

Table B1: Composition and Selection by Occupation and Decade

	1860s	1870s	1880s	1890s	1900s	1910s	1920s	1930s
Panel A: Homestead Relative Representation								
Farm Operator	1.68	2.17	2.51	1.83	2.79	3.02	3.05	3.17
Farm Laborer	0.83	0.60	0.40	1.16	0.87	1.21	1.81	2.27
Skilled	0.64	0.45	0.31	0.50	0.24	0.36	0.34	0.43
Unskilled	0.66	0.38	0.22	0.52	0.28	0.32	0.33	0.46
White Collar	0.28	0.30	0.22	0.36	0.24	0.32	0.32	0.42
Panel B: Homestead Composition (%)								
Farm Operator	62.4	70.5	82.0	60.1	74.6	67.1	63.6	51.4
Farm Laborer	6.2	9.9	4.7	13.4	9.0	10.3	12.4	14.8
Skilled	11.9	6.5	4.1	6.6	3.5	5.8	6.2	7.7
Unskilled	15.9	9.5	6.1	14.8	8.7	10.6	10.8	15.3
White Collar	3.6	3.6	3.0	5.1	4.2	6.3	7.0	10.8
Panel C: Cash Sale Relative Representation								
Farm Operator	2.00	1.93	1.94	1.60	1.78	2.64	2.66	3.34
Farm Laborer	0.67	0.59	0.74	0.99	1.25	1.16	1.40	1.21
Skilled	0.37	0.51	0.53	0.63	0.52	0.44	0.38	0.37
Unskilled	0.31	0.45	0.40	0.60	0.58	0.34	0.40	0.35
White Collar	0.50	0.77	0.67	0.74	0.81	0.67	0.70	0.77
Panel D: Cash Sale Composition (%)								
Farm Operator	74.1	62.7	63.6	52.7	47.7	58.5	55.5	54.1
Farm Laborer	5.0	9.7	8.5	11.4	12.9	9.8	9.5	7.9
Skilled	6.9	7.3	7.1	8.4	7.6	7.0	6.9	6.5
Unskilled	7.6	11.3	11.5	17.1	18.0	11.2	13.0	11.7
White Collar	6.5	9.0	9.4	10.3	13.8	13.4	15.1	19.8

Notes: Panels A and C report the relative representation of each occupation class among homesteaders and cash buyers, respectively: the share of an occupation class among patentees divided by its share in the general population. Values above one indicate overrepresentation. Panels B and D report the raw composition in percent.

Table B2: Composition and Selection by Birthplace

	Composition (%)			N			Relative Representation	
	Homestead	Cash	Non-Owner	Homestead	Cash	Non-Owner	Homestead	Cash
Norway	2.9	2.6	0.6	9,158	3,410	896,040	5.22	4.56
Sweden	2.3	1.8	0.8	6,951	2,210	1,417,243	3.73	2.07
Russia	0.8	0.5	1.0	3,222	754	2,211,440	2.09	1.36
Denmark	0.3	0.3	0.1	1,142	303	258,845	1.88	2.29
Canada	2.9	3.3	1.9	6,993	4,187	2,818,253	1.62	1.62
Scotland	0.8	0.7	0.6	1,557	826	750,168	1.33	1.21
Switzerland	0.3	0.4	0.3	943	358	368,853	1.12	1.46
Austria	0.3	0.2	0.7	1,097	298	1,330,720	1.10	0.64
England	2.2	2.3	2.0	5,007	2,334	2,485,868	1.02	1.10
US-born	78.2	81.5	78.7	216,715	85,791	117,970,970	0.99	1.04
Wales	0.2	0.2	0.2	475	256	258,233	0.91	0.90
Belgium	0.1	0.1	0.1	175	66	122,408	0.89	0.90
Germany	5.3	3.4	5.8	11,180	4,085	6,748,269	0.82	0.59
France	0.3	0.3	0.4	503	249	456,492	0.68	0.69
Ireland	2.9	2.2	4.9	4,309	2,313	4,893,235	0.50	0.48
Hungary	0.1	0.0	0.3	310	56	653,028	0.44	0.30
Portugal	0.0	0.0	0.1	56	38	184,966	0.20	0.37
Italy	0.1	0.1	1.3	295	105	2,847,239	0.08	0.18

Notes: "Composition" columns report the percentage of each patent type born in each country, pooled across all patent decades. "Relative Representation" columns report the ratio of a birthplace's share among patentees to its share in the general population. Values above one indicate overrepresentation.

Table B3: Composition and Selection by Birthplace and Decade (Regional)

	1860s	1870s	1880s	1890s	1900s	1910s	1920s	1930s
Panel A: Homestead Relative Representation								
Scandinavia	8.04	7.23	6.37	3.33	3.14	2.54	2.19	1.44
Canada	4.24	2.33	1.85	1.21	1.09	0.80	0.88	0.54
S/E Europe	2.17	1.25	2.48	0.85	0.74	0.29	0.27	0.20
US-born	0.81	0.92	0.88	1.04	1.02	1.10	1.09	1.11
German-speaking	1.53	0.88	0.99	0.63	0.65	0.54	0.64	0.44
British Isles	1.15	0.88	0.81	0.56	0.41	0.52	0.78	0.74
Panel B: Homestead Composition (%)								
Scandinavia	3.1	6.9	9.0	4.8	7.5	5.9	4.1	2.1
Canada	5.5	4.5	4.2	2.7	2.8	1.6	1.5	0.8
S/E Europe	1.0	0.8	2.6	0.9	3.2	2.6	2.9	1.8
US-born	60.7	69.0	68.8	81.3	80.0	84.9	87.3	92.6
German-speaking	13.4	7.6	7.4	4.8	4.0	2.8	1.9	1.0
British Isles	16.3	11.2	8.0	5.5	2.4	2.1	2.3	1.8
Panel C: Cash Sale Relative Representation								
Scandinavia	4.75	3.46	3.82	2.77	3.28	2.97	3.08	1.77
Canada	1.33	2.01	3.19	1.90	1.92	1.01	0.89	0.80
S/E Europe	0.42	0.72	0.98	0.71	0.43	0.28	0.16	0.21
US-born	1.15	1.01	0.95	1.00	1.00	1.07	1.09	1.10
German-speaking	0.46	0.67	0.64	0.56	0.56	0.57	0.59	0.35
British Isles	0.43	0.83	0.75	0.89	0.56	0.65	0.76	0.94
Panel D: Cash Sale Composition (%)								
Scandinavia	1.8	3.3	5.4	4.0	7.9	6.9	5.8	2.5
Canada	1.7	3.9	7.2	4.3	4.9	2.1	1.5	1.1
S/E Europe	0.2	0.4	1.0	0.8	1.9	2.5	1.6	1.9
US-born	86.2	76.0	74.1	78.0	78.7	82.8	87.1	91.4
German-speaking	4.0	5.8	4.8	4.2	3.5	3.0	1.8	0.8
British Isles	6.1	10.5	7.4	8.8	3.3	2.7	2.2	2.3

Notes: Each cell reports the relative representation (Panels A and C) or raw composition share (Panels B and D) of the indicated birthplace group among patentees for the indicated patent decade. Relative representation is the share of a birthplace group among patentees divided by its share in the general population. Values above one indicate overrepresentation.

Table B4: Relative Representation by Birthplace and Decade

	1860s	1870s	1880s	1890s	1900s	1910s	1920s	1930s
Panel A: Homestead Relative Representation								
Norway	7.81	7.79	8.41	3.93	5.16	3.74	2.99	1.94
Sweden	8.73	6.74	4.84	2.87	2.03	1.74	1.65	1.22
Russia	0.00	0.55	10.50	2.18	2.25	0.53	0.39	0.29
Denmark	0.00	2.30	1.93	2.27	2.84	2.80	2.05	0.86
Canada	4.26	2.34	1.87	1.22	1.08	0.79	0.85	0.53
Scotland	2.55	1.80	1.40	1.03	0.70	0.76	1.39	0.99
Switzerland	0.35	1.39	1.61	1.02	1.00	1.19	1.33	1.10
Austria	3.40	1.46	1.88	0.53	0.43	0.29	0.46	0.35
England	1.27	1.56	1.40	0.97	0.62	0.72	0.90	0.74
US-born	0.82	0.92	0.89	1.05	1.01	1.09	1.06	1.07
Wales	1.19	1.12	1.04	1.09	0.49	0.63	0.90	0.83
Belgium	0.00	1.88	1.22	0.91	0.63	0.61	0.71	1.18
Germany	1.55	0.88	1.00	0.64	0.66	0.61	0.74	0.49
France	1.16	0.54	0.62	0.37	0.49	0.56	0.90	0.83
Ireland	1.02	0.58	0.53	0.34	0.23	0.29	0.44	0.56
Hungary	0.00	1.41	0.88	0.35	0.31	0.22	0.21	0.13
Portugal	0.00	0.18	0.07	0.39	0.17	0.13	0.30	0.34
Italy	0.00	0.11	0.08	0.12	0.05	0.05	0.10	0.08
Panel B: Cash Sale Relative Representation								
Norway	6.48	4.74	5.89	3.23	4.83	5.00	3.55	2.75
Sweden	1.77	2.16	2.09	2.31	2.42	1.98	2.76	1.09
Russia	0.84	1.37	4.17	2.38	1.21	0.51	0.09	0.29
Denmark	0.00	0.00	3.66	5.74	2.75	1.85	2.54	1.79
Canada	1.33	2.00	3.20	1.91	1.90	1.01	0.86	0.77
Scotland	0.90	2.15	1.45	1.83	1.03	1.16	1.18	0.00
Switzerland	0.95	1.91	0.92	0.90	0.74	1.24	1.73	3.29
Austria	1.21	0.75	0.95	0.77	0.41	0.24	0.35	0.46
England	0.64	1.52	1.20	1.66	0.74	0.79	0.89	1.39
US-born	1.15	1.01	0.95	1.00	0.99	1.07	1.05	1.06
Wales	0.43	1.25	1.14	1.36	0.82	1.12	1.12	0.00
Belgium	2.82	1.04	1.07	0.91	0.57	0.35	0.43	0.00
Germany	0.46	0.67	0.64	0.56	0.57	0.67	0.72	0.41
France	0.44	0.76	0.44	0.54	0.60	0.58	1.34	0.79
Ireland	0.33	0.48	0.50	0.48	0.36	0.38	0.43	0.88
Hungary	0.00	0.50	1.33	0.24	0.18	0.10	0.07	0.00
Portugal	0.43	0.68	0.28	0.31	0.29	0.45	0.51	0.00
Italy	0.14	0.54	0.18	0.25	0.07	0.07	0.07	0.13

Notes: Each cell reports the relative representation (Panels A and C) or raw composition share (Panels B and D) of the indicated birthplace group among patentees for the indicated patent decade. Relative representation is the share of a birthplace group among patentees divided by its share in the general population. Values above one indicate overrepresentation.

Table B5: Composition by Birthplace and Decade

	1860s	1870s	1880s	1890s	1900s	1910s	1920s	1930s
Panel A: Homestead Composition (%)								
Norway	1.9	3.7	5.4	2.6	4.2	2.9	1.8	0.9
Sweden	1.2	3.1	3.6	2.2	2.9	2.3	1.8	1.0
Russia	0.0	0.0	1.0	0.2	2.0	1.5	1.0	0.5
Denmark	0.0	0.0	0.0	0.0	0.5	0.8	0.6	0.2
Canada	5.5	4.5	4.2	2.7	2.8	1.6	1.5	0.8
Scotland	1.9	1.3	0.9	0.7	0.4	0.4	0.5	0.4
Switzerland	0.1	0.6	0.6	0.4	0.3	0.3	0.3	0.2
Austria	0.2	0.2	0.3	0.1	0.3	0.7	0.6	0.2
England	3.7	4.3	3.4	2.4	1.2	1.1	1.1	0.7
US-born	60.5	68.7	69.1	81.2	80.4	84.7	87.7	93.0
Wales	0.4	0.4	0.3	0.3	0.1	0.1	0.1	0.1
Belgium	0.0	0.1	0.1	0.1	0.0	0.1	0.1	0.1
Germany	13.4	7.6	7.4	4.8	3.9	2.6	1.8	0.9
France	1.0	0.4	0.3	0.2	0.1	0.1	0.2	0.1
Ireland	10.3	5.1	3.4	2.2	0.7	0.6	0.6	0.6
Hungary	0.0	0.0	0.0	0.0	0.1	0.2	0.1	0.0
Portugal	0.0	0.0	0.0	0.0	0.0	0.0	0.1	0.1
Italy	0.0	0.0	0.0	0.0	0.1	0.2	0.3	0.2
Panel B: Cash Sale Composition (%)								
Norway	1.6	2.3	3.7	2.1	3.9	3.9	2.1	1.3
Sweden	0.2	1.0	1.6	1.7	3.5	2.6	2.9	0.9
Russia	0.0	0.1	0.4	0.2	1.1	1.5	0.2	0.5
Denmark	0.0	0.0	0.1	0.1	0.5	0.5	0.7	0.4
Canada	1.7	3.9	7.2	4.3	4.9	2.0	1.5	1.1
Scotland	0.7	1.5	0.9	1.2	0.5	0.5	0.5	0.0
Switzerland	0.3	0.8	0.3	0.3	0.2	0.3	0.3	0.5
Austria	0.1	0.1	0.1	0.1	0.3	0.6	0.4	0.3
England	1.8	4.2	2.9	4.1	1.4	1.2	1.1	1.4
US-born	85.5	75.1	74.0	77.7	78.7	82.6	87.1	91.5
Wales	0.1	0.4	0.3	0.4	0.2	0.2	0.1	0.0
Belgium	0.2	0.1	0.1	0.1	0.0	0.0	0.0	0.0
Germany	4.0	5.8	4.8	4.2	3.4	2.9	1.7	0.8
France	0.4	0.5	0.2	0.3	0.1	0.1	0.3	0.1
Ireland	3.4	4.2	3.2	3.1	1.1	0.8	0.6	0.9
Hungary	0.0	0.0	0.1	0.0	0.1	0.1	0.0	0.0
Portugal	0.0	0.0	0.0	0.0	0.0	0.1	0.1	0.0
Italy	0.0	0.1	0.0	0.1	0.1	0.2	0.2	0.4

Notes: Each cell reports the relative representation (Panels A and C) or raw composition share (Panels B and D) of the indicated birthplace group among patentees for the indicated patent decade. Relative representation is the share of a birthplace group among patentees divided by its share in the general population. Values above one indicate overrepresentation.

Table B6: Selection into Land Ownership: Family Size

	1860s	1870s	1880s	1890s	1900s	1910s	1920s	1930s
Panel A: Homestead Entry								
Full Sample	-0.126.	0.069.	-0.029.	0.180.	0.137*	-0.230**	-0.361***	-0.333**
	(0.273)	(0.068)	(0.060)	(0.052)	(0.053)	(0.045)	(0.055)	(0.075)
	2,038,736	*2,963,416*	*4,097,182*	*4,097,192*	*6,380,787*	*8,394,488*	*9,583,487*	*11,212,791*
Patent County FE	0.645.	0.696***	0.306.	0.543***	0.406***	0.395***	0.094**	0.010*
	(0.273)	(0.075)	(0.058)	(0.069)	(0.044)	(0.039)	(0.041)	(0.066)
	2,038,683	*2,963,374*	*4,097,132*	*4,097,131*	*6,380,759*	*8,394,478*	*9,583,480*	*11,212,790*
Origin County FE	0.179.	0.337***	0.192**	0.219**	0.326***	0.283***	0.063*	0.003*
	(0.250)	(0.059)	(0.051)	(0.045)	(0.040)	(0.031)	(0.041)	(0.064)
	2,038,712	*2,963,402*	*4,097,174*	*4,097,184*	*6,380,783*	*8,394,488*	*9,583,485*	*11,212,791*
Panel B: Cash Sale								
Full Sample	0.421***	-0.209.	-0.075*	0.001.	-0.117*	-0.484***	-0.244.	-0.106.
	(0.100)	(0.099)	(0.072)	(0.082)	(0.063)	(0.072)	(0.099)	(0.206)
	2,038,736	*2,963,416*	*4,097,182*	*4,097,192*	*6,380,787*	*8,394,488*	*9,583,487*	*11,212,791*
Patent County FE	0.535**	0.634***	0.468***	0.564***	0.384.	0.263***	0.095.	0.133.
	(0.092)	(0.098)	(0.083)	(0.089)	(0.050)	(0.051)	(0.089)	(0.194)
	2,038,683	*2,963,374*	*4,097,132*	*4,097,131*	*6,380,759*	*8,394,478*	*9,583,480*	*11,212,790*
Origin County FE	0.440**	0.343***	0.252.	0.236***	0.246***	0.179***	0.060.	0.137.
	(0.084)	(0.078)	(0.059)	(0.070)	(0.045)	(0.046)	(0.087)	(0.192)
	2,038,712	*2,963,402*	*4,097,174*	*4,097,184*	*6,380,783*	*8,394,488*	*9,583,485*	*11,212,791*

Notes: Each column reports the coefficient on a homestead (Panel A) or cash sale (Panel B) indicator from a separate cross-sectional regression estimated on the full-count census for the indicated patent decade. "Full Sample" includes no geographic fixed effects. "Patent County FE" adds fixed effects for the county in which the patent was issued. "Origin County FE" adds fixed effects for the individual's county of residence at first observation. Standard errors in parentheses; observation counts in italics. *p<0.10, **p<0.05, ***p<0.01.

Table B7: Selection into Land-Ownership: Number of Children

	1860s	1870s	1880s	1890s	1900s	1910s	1920s	1930s
Panel A: Homestead Entry								
Full Sample	0.154.	0.139**	0.217***	0.129**	0.237**	0.018***	-0.050.	-0.051.
	(0.196)	(0.053)	(0.046)	(0.041)	(0.037)	(0.030)	(0.040)	(0.056)
	2,038,736	*2,963,416*	*4,097,182*	*4,097,192*	*6,380,787*	*8,394,488*	*9,583,487*	*11,212,791*
Patent County FE	0.623*	0.523***	0.491***	0.338***	0.297**	0.235***	0.057.	0.002.
	(0.198)	(0.053)	(0.040)	(0.045)	(0.033)	(0.024)	(0.029)	(0.047)
	2,038,683	*2,963,374*	*4,097,132*	*4,097,131*	*6,380,759*	*8,394,478*	*9,583,480*	*11,212,790*
Origin County FE	0.337.	0.274***	0.390***	0.146.	0.266**	0.188***	0.056*	0.008.
	(0.191)	(0.047)	(0.038)	(0.035)	(0.032)	(0.022)	(0.029)	(0.046)
	2,038,712	*2,963,402*	*4,097,174*	*4,097,184*	*6,380,783*	*8,394,488*	*9,583,485*	*11,212,791*
Panel B: Cash Sale								
Full Sample	0.377.	-0.089.	-0.173.	-0.209.	-0.066.	-0.134.	0.045.	0.044.
	(0.071)	(0.067)	(0.052)	(0.052)	(0.037)	(0.043)	(0.071)	(0.161)
	2,038,736	*2,963,416*	*4,097,182*	*4,097,192*	*6,380,787*	*8,394,488*	*9,583,487*	*11,212,791*
Patent County FE	0.428**	0.432***	0.281***	0.183.	0.111.	0.146***	0.134.	0.104.
	(0.065)	(0.064)	(0.045)	(0.055)	(0.034)	(0.033)	(0.067)	(0.156)
	2,038,683	*2,963,374*	*4,097,132*	*4,097,131*	*6,380,759*	*8,394,478*	*9,583,480*	*11,212,790*
Origin County FE	0.387**	0.206.	0.097.	-0.026.	0.051*	0.114***	0.124.	0.109.
	(0.062)	(0.062)	(0.042)	(0.049)	(0.031)	(0.031)	(0.066)	(0.152)
	2,038,712	*2,963,402*	*4,097,174*	*4,097,184*	*6,380,783*	*8,394,488*	*9,583,485*	*11,212,791*

Notes: Each column reports the coefficient on a homestead (Panel A) or cash sale (Panel B) indicator from a separate cross-sectional regression estimated on the full-count census for the indicated patent decade. "Full Sample" includes no geographic fixed effects. "Patent County FE" adds fixed effects for the county in which the patent was issued. "Origin County FE" adds fixed effects for the individual's county of residence at first observation. Standard errors in parentheses; observation counts in italics. *p<0.10, **p<0.05, ***p<0.01.

Table B8: Selection into Land-Ownership: White

	1860s	1870s	1880s	1890s	1900s	1910s	1920s	1930s
Panel A: Homestead Entry								
Full Sample	0.020***	0.092***	0.081***	0.055***	0.068**	0.086***	0.088***	0.086***
	(0.002)	(0.008)	(0.008)	(0.008)	(0.006)	(0.005)	(0.005)	(0.008)
	2,038,736	*2,963,416*	*4,097,182*	*4,097,192*	*6,380,787*	*8,394,488*	*9,583,487*	*11,212,791*
Patent County FE	0.032.	0.006.	0.010.	0.022.	0.028*	0.023***	0.021***	0.022***
	(0.021)	(0.004)	(0.004)	(0.005)	(0.004)	(0.003)	(0.003)	(0.005)
	2,038,683	*2,963,374*	*4,097,132*	*4,097,131*	*6,380,759*	*8,394,478*	*9,583,480*	*11,212,790*
Origin County FE	0.012**	0.012.	0.015.	0.028**	0.031**	0.024***	0.023***	0.024***
	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.002)	(0.003)	(0.005)
	2,038,712	*2,963,402*	*4,097,174*	*4,097,184*	*6,380,783*	*8,394,488*	*9,583,485*	*11,212,791*
Panel B: Cash Sale								
Full Sample	0.015*	0.108***	0.102***	0.097***	0.085***	0.087***	0.061*	0.058.
	(0.003)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.009)	(0.018)
	2,038,736	*2,963,416*	*4,097,182*	*4,097,192*	*6,380,787*	*8,394,488*	*9,583,487*	*11,212,791*
Patent County FE	0.003.	0.015.	0.021.	0.032***	0.035***	0.035***	0.033**	0.030.
	(0.002)	(0.005)	(0.004)	(0.007)	(0.005)	(0.006)	(0.008)	(0.014)
	2,038,683	*2,963,374*	*4,097,132*	*4,097,131*	*6,380,759*	*8,394,478*	*9,583,480*	*11,212,790*
Origin County FE	0.005.	0.022.	0.026***	0.033***	0.035***	0.032***	0.035***	0.034.
	(0.002)	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)	(0.007)	(0.014)
	2,038,712	*2,963,402*	*4,097,174*	*4,097,184*	*6,380,783*	*8,394,488*	*9,583,485*	*11,212,791*

Notes: Each column reports the coefficient on a homestead (Panel A) or cash sale (Panel B) indicator from a separate cross-sectional regression estimated on the full-count census for the indicated patent decade. "Full Sample" includes no geographic fixed effects. "Patent County FE" adds fixed effects for the county in which the patent was issued. "Origin County FE" adds fixed effects for the individual's county of residence at first observation. Standard errors in parentheses; observation counts in italics. *p<0.10, **p<0.05, ***p<0.01.

Table B9: Selection into Land-Ownership: 1st Generation Immigrants

	1860s	1870s	1880s	1890s	1900s	1910s	1920s	1930s
Panel A: Homestead Entry								
Full Sample	0.132.	0.071.	0.096.	0.002.	0.003.	-0.051***	-0.065**	-0.092***
	(0.068)	(0.023)	(0.020)	(0.018)	(0.022)	(0.019)	(0.016)	(0.016)
	2,038,736	*2,963,416*	*4,097,182*	*4,097,192*	*6,380,787*	*8,394,488*	*9,583,487*	*11,212,791*
Patent County FE	0.070.	0.009.	0.023**	0.014.	0.002.	-0.052**	-0.054***	-0.075***
	(0.062)	(0.014)	(0.010)	(0.009)	(0.006)	(0.007)	(0.008)	(0.012)
	2,038,683	*2,963,374*	*4,097,132*	*4,097,131*	*6,380,759*	*8,394,478*	*9,583,480*	*11,212,790*
Origin County FE	0.048.	-0.006.	0.021**	0.003.	-0.004.	-0.053***	-0.057***	-0.079***
	(0.052)	(0.012)	(0.010)	(0.009)	(0.006)	(0.006)	(0.008)	(0.012)
	2,038,712	*2,963,402*	*4,097,174*	*4,097,184*	*6,380,783*	*8,394,488*	*9,583,485*	*11,212,791*
Panel B: Cash Sale								
Full Sample	-0.082*	0.011.	0.054.	0.015.	0.018.	-0.031***	-0.051.	-0.082*
	(0.027)	(0.028)	(0.026)	(0.020)	(0.024)	(0.021)	(0.024)	(0.029)
	2,038,736	*2,963,416*	*4,097,182*	*4,097,192*	*6,380,787*	*8,394,488*	*9,583,487*	*11,212,791*
Patent County FE	-0.001.	-0.024.	-0.033.	-0.023.	-0.037**	-0.057**	-0.041.	-0.041.
	(0.011)	(0.015)	(0.011)	(0.013)	(0.008)	(0.008)	(0.014)	(0.025)
	2,038,683	*2,963,374*	*4,097,132*	*4,097,131*	*6,380,759*	*8,394,478*	*9,583,480*	*11,212,790*
Origin County FE	-0.008.	-0.045**	-0.023.	-0.026*	-0.037**	-0.055***	-0.041.	-0.041.
	(0.011)	(0.013)	(0.011)	(0.010)	(0.008)	(0.008)	(0.014)	(0.025)
	2,038,712	*2,963,402*	*4,097,174*	*4,097,184*	*6,380,783*	*8,394,488*	*9,583,485*	*11,212,791*

Notes: Each column reports the coefficient on a homestead (Panel A) or cash sale (Panel B) indicator from a separate cross-sectional regression estimated on the full-count census for the indicated patent decade. "Full Sample" includes no geographic fixed effects. "Patent County FE" adds fixed effects for the county in which the patent was issued. "Origin County FE" adds fixed effects for the individual's county of residence at first observation. Standard errors in parentheses; observation counts in italics. *p<0.10, **p<0.05, ***p<0.01.

Table B10: Selection into Land-Ownership: Labor Force Participation

	1860s	1870s	1880s	1890s	1900s	1910s	1920s	1930s
Panel A: Homestead Entry								
Full Sample	0.008*	0.010*	0.008*	0.005**		0.005***	0.005.	0.005.
	(0.004)	(0.002)	(0.001)	(0.001)		(0.001)	(0.001)	(0.002)
	2,038,736	*2,963,416*	*4,097,182*	*4,097,192*		*8,394,488*	*9,583,487*	*11,212,791*
Patent County FE	0.010.	0.004.	0.003.	0.001.		0.004**	0.005.	0.005.
	(0.005)	(0.001)	(0.001)	(0.001)		(0.001)	(0.001)	(0.002)
	2,038,683	*2,963,374*	*4,097,132*	*4,097,131*		*8,394,478*	*9,583,480*	*11,212,790*
Origin County FE	0.007**	0.005.	0.004.	0.002.		0.004**	0.005.	0.004.
	(0.003)	(0.001)	(0.001)	(0.001)		(0.001)	(0.001)	(0.002)
	2,038,712	*2,963,402*	*4,097,174*	*4,097,184*		*8,394,488*	*9,583,485*	*11,212,791*
Panel B: Cash Sale								
Full Sample	0.013.	0.008.	0.005**	0.004.		0.004.	0.005*	0.002*
	(0.003)	(0.002)	(0.001)	(0.002)		(0.001)	(0.001)	(0.004)
	2,038,736	*2,963,416*	*4,097,182*	*4,097,192*		*8,394,488*	*9,583,487*	*11,212,791*
Patent County FE	0.011.	0.001.	0.000.	-0.000.		0.004.	0.004.	0.001*
	(0.002)	(0.001)	(0.001)	(0.002)		(0.001)	(0.001)	(0.004)
	2,038,683	*2,963,374*	*4,097,132*	*4,097,131*		*8,394,478*	*9,583,480*	*11,212,790*
Origin County FE	0.011.	0.002.	0.002.	0.001.		0.004.	0.004.	0.001*
	(0.002)	(0.001)	(0.001)	(0.001)		(0.001)	(0.001)	(0.004)
	2,038,712	*2,963,402*	*4,097,174*	*4,097,184*		*8,394,488*	*9,583,485*	*11,212,791*

Notes: Each column reports the coefficient on a homestead (Panel A) or cash sale (Panel B) indicator from a separate cross-sectional regression estimated on the full-count census for the indicated patent decade. "Full Sample" includes no geographic fixed effects. "Patent County FE" adds fixed effects for the county in which the patent was issued. "Origin County FE" adds fixed effects for the individual's county of residence at first observation. Standard errors in parentheses; observation counts in italics. $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$.

Table B11: Selection into Land-Ownership: Father's Occupational Income Score

	1860s	1870s	1880s	1890s	1900s	1910s	1920s	1930s
Panel A: Homestead Entry								
Full Sample	-0.187.	-1.257.	-2.343.	-1.211.	-1.778.	-1.984.	-1.334.	-3.484.
	(2.515)	(0.684)	(0.725)	(0.568)	(0.492)	(0.428)	(0.590)	(1.198)
	204,106	*293,503*	*584,744*	*584,745*	*785,368*	*949,136*	*1,101,754*	*1,297,174*
Patent County FE	-0.502.	-0.645.	-1.657*	-1.000.	-0.903.	-1.307**	-0.328.	-2.744.
	(3.684)	(0.992)	(0.789)	(0.756)	(0.532)	(0.463)	(0.569)	(1.233)
	232,582	*293,370*	*584,664*	*584,642*	*785,312*	*949,098*	*1,101,734*	*1,297,159*
Origin County FE	0.135.	-0.578.	-1.592*	-0.860.	-0.960.	-1.090*	-0.307.	-2.687.
	(2.721)	(0.717)	(0.695)	(0.524)	(0.464)	(0.404)	(0.553)	(1.127)
	204,002	*293,387*	*584,691*	*584,692*	*785,333*	*949,118*	*1,101,738*	*1,297,160*
Panel B: Cash Sale								
Full Sample	-2.901.	-1.432.	-0.492.	-0.782.	-0.704.	-1.446.	-1.330.	-2.833.
	(0.872)	(1.121)	(0.590)	(0.733)	(0.577)	(0.709)	(1.438)	(2.259)
	204,106	*293,503*	*584,744*	*584,745*	*785,368*	*949,136*	*1,101,754*	*1,297,174*
Patent County FE	-1.386.	-1.125.	-0.034.	-0.551.	-0.032.	-0.535.	-0.120.	-1.761.
	(1.004)	(1.407)	(0.746)	(0.899)	(0.631)	(0.766)	(1.385)	(2.210)
	203,990	*293,370*	*584,664*	*584,642*	*785,312*	*949,098*	*1,101,734*	*1,297,159*
Origin County FE	-1.793.	-1.463.	0.041.	-0.434.	-0.028.	-0.476.	-0.230.	-1.481.
	(0.835)	(1.124)	(0.585)	(0.706)	(0.567)	(0.681)	(1.332)	(2.160)
	204,002	*293,387*	*584,691*	*584,692*	*785,333*	*949,118*	*1,101,738*	*1,297,160*

Notes: Each column reports the coefficient on a homestead (Panel A) or cash sale (Panel B) indicator from a separate cross-sectional regression estimated on the full-count census for the indicated patent decade. "Full Sample" includes no geographic fixed effects. "Patent County FE" adds fixed effects for the county in which the patent was issued. "Origin County FE" adds fixed effects for the individual's county of residence at first observation. Standard errors in parentheses; observation counts in italics. *p<0.10, **p<0.05, ***p<0.01.

Table B12: Selection into Land-Ownership: Occupational Income Score

	1860s	1870s	1880s	1890s	1900s	1910s	1920s	1930s
Panel A: Homestead Entry								
Full Sample	-0.954.	-0.761**	-0.588.	-0.629**	-0.721**	-1.066**	-1.093***	-0.773.
	(0.797)	(0.186)	(0.178)	(0.182)	(0.138)	(0.140)	(0.158)	(0.220)
	2,038,736	*2,963,416*	*4,097,182*	*4,097,192*	*6,380,787*	*8,394,488*	*9,583,487*	*11,212,791*
Patent County FE	-0.521.	-0.720*	-0.287.	-0.303.	-0.398.	-0.600.	-0.607.	-0.327.
	(0.965)	(0.182)	(0.140)	(0.163)	(0.113)	(0.093)	(0.117)	(0.194)
	2,038,683	*2,963,374*	*4,097,132*	*4,097,131*	*6,380,759*	*8,394,478*	*9,583,480*	*11,212,790*
Origin County FE	-0.353.	-0.597*	-0.309.	-0.324.	-0.387.	-0.589.	-0.599.	-0.332.
	(0.714)	(0.134)	(0.123)	(0.126)	(0.101)	(0.078)	(0.109)	(0.181)
	2,038,712	*2,963,402*	*4,097,174*	*4,097,184*	*6,380,783*	*8,394,488*	*9,583,485*	*11,212,791*
Panel B: Cash Sale								
Full Sample	0.242.	0.174*	-0.190.	-0.178.	-0.008.	-0.437**	-0.424*	0.221.
	(0.249)	(0.217)	(0.186)	(0.215)	(0.160)	(0.179)	(0.273)	(0.557)
	2,038,736	*2,963,416*	*4,097,182*	*4,097,192*	*6,380,787*	*8,394,488*	*9,583,487*	*11,212,791*
Patent County FE	0.056.	0.127.	0.160.	0.053.	0.169.	0.046**	0.198.	0.721.
	(0.195)	(0.206)	(0.159)	(0.194)	(0.130)	(0.124)	(0.231)	(0.543)
	2,038,683	*2,963,374*	*4,097,132*	*4,097,131*	*6,380,759*	*8,394,478*	*9,583,480*	*11,212,790*
Origin County FE	0.016.	0.134.	0.049.	0.043.	0.152.	0.014.	0.145.	0.754.
	(0.184)	(0.172)	(0.132)	(0.162)	(0.108)	(0.111)	(0.230)	(0.548)
	2,038,712	*2,963,402*	*4,097,174*	*4,097,184*	*6,380,783*	*8,394,488*	*9,583,485*	*11,212,791*

Notes: Each column reports the coefficient on a homestead (Panel A) or cash sale (Panel B) indicator from a separate cross-sectional regression estimated on the full-count census for the indicated patent decade. "Full Sample" includes no geographic fixed effects. "Patent County FE" adds fixed effects for the county in which the patent was issued. "Origin County FE" adds fixed effects for the individual's county of residence at first observation. Standard errors in parentheses; observation counts in italics. *p<0.10, **p<0.05, ***p<0.01.

Table B13: Selection into Land-Ownership: Ward Score

	1860s	1870s	1880s	1890s	1900s	1910s	1920s	1930s
Panel A: Homestead Entry								
Full Sample	3.308.	4.651***	4.683***	0.450*	0.540**	7.925***	7.812**	6.516.
	(0.962)	(0.739)	(0.720)	(0.785)	(1.049)	(0.873)	(0.747)	(0.891)
	1,818,368	*2,822,371*	*3,996,792*	*3,996,801*	*5,911,897*	*8,069,452*	*9,152,384*	*10,727,338*
Patent County FE	0.358.	-0.931.	-0.670.	-0.297*	0.443.	0.231*	-0.182.	-0.016.
	(1.315)	(0.268)	(0.217)	(0.312)	(0.292)	(0.225)	(0.248)	(0.405)
	1,818,315	*2,822,329*	*3,996,743*	*3,996,740*	*5,911,868*	*8,069,442*	*9,152,377*	*10,727,337*
Origin County FE	-0.248.	-0.107.	-0.126.	-0.044.	0.015.	0.512.	0.168.	0.242.
	(0.729)	(0.187)	(0.177)	(0.183)	(0.192)	(0.161)	(0.225)	(0.403)
	1,818,343	*2,822,356*	*3,996,784*	*3,996,794*	*5,911,892*	*8,069,452*	*9,152,382*	*10,727,337*
Panel B: Cash Sale								
Full Sample	-10.245***	5.948***	6.985***	5.593***	6.703***	10.408***	6.179*	5.910.
	(1.022)	(0.797)	(0.676)	(0.696)	(0.872)	(0.992)	(1.164)	(1.755)
	1,818,368	*2,822,371*	*3,996,792*	*3,996,801*	*5,911,897*	*8,069,452*	*9,152,384*	*10,727,338*
Patent County FE	-0.110*	0.194.	0.326.	0.354.	1.395**	1.765***	1.703*	1.729.
	(0.320)	(0.419)	(0.274)	(0.448)	(0.270)	(0.353)	(0.501)	(1.163)
	1,818,315	*2,822,329*	*3,996,743*	*3,996,740*	*5,911,868*	*8,069,442*	*9,152,377*	*10,727,337*
Origin County FE	-0.303.	0.721.	0.770*	0.767*	1.341*	1.912.	1.812*	2.001.
	(0.263)	(0.307)	(0.204)	(0.292)	(0.200)	(0.285)	(0.467)	(1.129)
	1,818,343	*2,822,356*	*3,996,784*	*3,996,794*	*5,911,892*	*8,069,452*	*9,152,382*	*10,727,337*

Notes: Each column reports the coefficient on a homestead (Panel A) or cash sale (Panel B) indicator from a separate cross-sectional regression estimated on the full-count census for the indicated patent decade. "Full Sample" includes no geographic fixed effects. "Patent County FE" adds fixed effects for the county in which the patent was issued. "Origin County FE" adds fixed effects for the individual's county of residence at first observation. Standard errors in parentheses; observation counts in italics. *p<0.10, **p<0.05, ***p<0.01.

Table B14: Selection into Land-Ownership: Predicted Income

	1860s	1870s	1880s	1890s	1900s	1910s	1920s	1930s
Panel A: Homestead Entry								
Full Sample	-38.034.	-50.773*	-59.602***	-61.554***	-53.141***	-12.943**	47.660**	94.292*
	(38.384)	(10.732)	(10.694)	(10.642)	(10.845)	(11.843)	(12.398)	(16.649)
	1,800,518	*2,780,189*	*3,919,504*	*3,919,512*	*5,806,786*	*7,810,279*	*8,582,109*	*10,063,262*
Patent County FE	36.990.	43.483***	47.156.	27.343**	29.018**	9.261.	-8.552*	17.729.
	(37.286)	(7.742)	(5.663)	(8.147)	(4.788)	(4.343)	(5.199)	(8.675)
	1,800,465	*2,780,146*	*3,919,454*	*3,919,452*	*5,806,757*	*7,810,269*	*8,582,102*	*10,063,261*
Origin County FE	-2.395.	-10.070.	17.185.	-5.330.	11.117.	5.823.	-2.040.	20.466.
	(30.702)	(4.893)	(4.383)	(4.383)	(3.935)	(3.351)	(4.496)	(7.667)
	1,800,493	*2,780,173*	*3,919,496*	*3,919,504*	*5,806,782*	*7,810,278*	*8,582,108*	*10,063,262*
Panel B: Cash Sale								
Full Sample	-95.281***	2.908.	-49.594.	-24.449*	-12.526.	1.972*	50.307**	73.311.
	(14.394)	(16.930)	(12.224)	(13.265)	(13.460)	(17.520)	(18.670)	(32.084)
	1,800,518	*2,780,189*	*3,919,504*	*3,919,512*	*5,806,786*	*7,810,279*	*8,582,109*	*10,063,262*
Patent County FE	29.037***	52.747*	45.424**	13.384.	17.941**	31.222**	34.261.	48.640.
	(6.490)	(8.928)	(7.395)	(10.290)	(5.750)	(5.204)	(9.960)	(24.001)
	1,800,465	*2,780,146*	*3,919,454*	*3,919,452*	*5,806,757*	*7,810,269*	*8,582,102*	*10,063,261*
Origin County FE	16.679.	19.871.	10.743.	-2.535.	11.060.	25.666.	35.360.	52.058.
	(5.721)	(7.234)	(5.037)	(6.202)	(4.433)	(4.771)	(9.533)	(24.075)
	1,800,493	*2,780,173*	*3,919,496*	*3,919,504*	*5,806,782*	*7,810,278*	*8,582,108*	*10,063,262*

Notes: Each column reports the coefficient on a homestead (Panel A) or cash sale (Panel B) indicator from a separate cross-sectional regression estimated on the full-count census for the indicated patent decade. "Full Sample" includes no geographic fixed effects. "Patent County FE" adds fixed effects for the county in which the patent was issued. "Origin County FE" adds fixed effects for the individual's county of residence at first observation. Standard errors in parentheses; observation counts in italics. $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$.

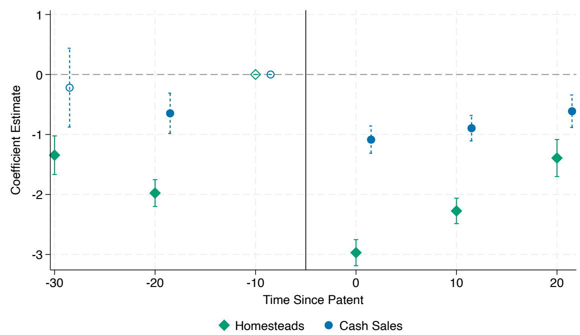
Table B15: Selection into Land-Ownership: Occupation-Based Wealth

	1860s	1870s	1880s	1890s	1900s	1910s	1920s	1930s
Panel A: Homestead Entry								
Full Sample	-176.189.	-391.281.	-303.460.	-388.614*	-58.586*	-84.739.	-202.093*	-78.648.
	(256.572)	(76.382)	(78.057)	(62.475)	(83.198)	(72.788)	(76.340)	(137.107)
	2,038,734	*2,963,416*	*4,097,160*	*4,097,170*	*6,214,996*	*8,237,872*	*9,353,676*	*10,709,997*
Patent County FE	253.870.	73.180.	93.784**	80.002.	22.076.	-12.335.	-17.533.	48.123.
	(273.362)	(65.330)	(33.279)	(56.268)	(28.524)	(26.662)	(28.548)	(50.593)
	2,038,681	*2,963,374*	*4,097,110*	*4,097,108*	*6,214,968*	*8,237,862*	*9,353,669*	*10,709,996*
Origin County FE	-10.795.	-97.462*	34.329.	-48.799.	31.569.	-22.013*	-24.406.	48.544.
	(200.272)	(36.120)	(29.031)	(28.364)	(22.385)	(20.332)	(26.156)	(39.699)
	2,038,710	*2,963,402*	*4,097,152*	*4,097,162*	*6,214,992*	*8,237,872*	*9,353,675*	*10,709,997*
Panel B: Cash Sale								
Full Sample	-742.188***	-104.586.	-187.682.	-140.271.	-47.726.	65.259**	55.911.	304.744.
	(99.615)	(101.651)	(66.447)	(74.405)	(54.486)	(70.643)	(88.984)	(155.555)
	2,038,734	*2,963,416*	*4,097,160*	*4,097,170*	*6,214,996*	*8,237,872*	*9,353,676*	*10,709,997*
Patent County FE	204.112.	336.876*	212.328*	163.897*	137.662*	200.160***	255.480.	547.137.
	(50.244)	(72.069)	(41.028)	(61.673)	(37.849)	(34.804)	(64.142)	(131.105)
	2,038,681	*2,963,374*	*4,097,110*	*4,097,108*	*6,214,968*	*8,237,862*	*9,353,669*	*10,709,996*
Origin County FE	129.540.	189.917.	105.788.	58.644.	91.341.	190.731*	243.527.	537.101.
	(39.301)	(49.908)	(30.193)	(37.838)	(24.748)	(31.625)	(59.309)	(129.014)
	2,038,710	*2,963,402*	*4,097,152*	*4,097,162*	*6,214,992*	*8,237,872*	*9,353,675*	*10,709,997*

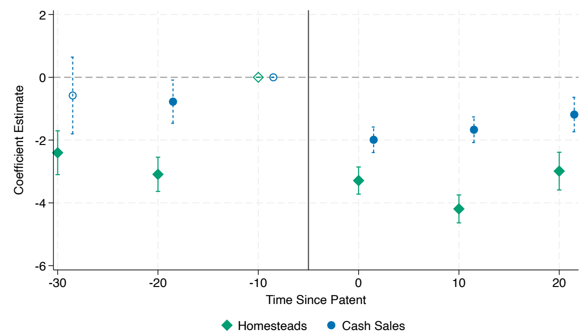
Notes: Each column reports the coefficient on a homestead (Panel A) or cash sale (Panel B) indicator from a separate cross-sectional regression estimated on the full-count census for the indicated patent decade. "Full Sample" includes no geographic fixed effects. "Patent County FE" adds fixed effects for the county in which the patent was issued. "Origin County FE" adds fixed effects for the individual's county of residence at first observation. Standard errors in parentheses; observation counts in italics. *$p<0.10$, **$p<0.05$, ***$p<0.01$.

Appendix C: Mobility for Landowners

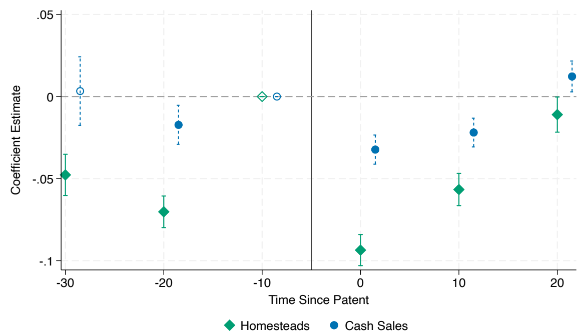
Figure C1: Differences Relative to Non-Landowners, 1850 – 1900 (Home County FE)



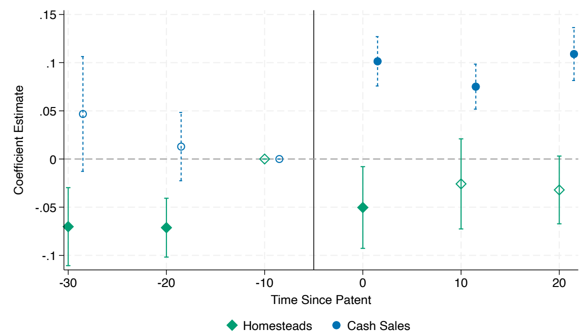
(a) OccScore



(b) Ward Score



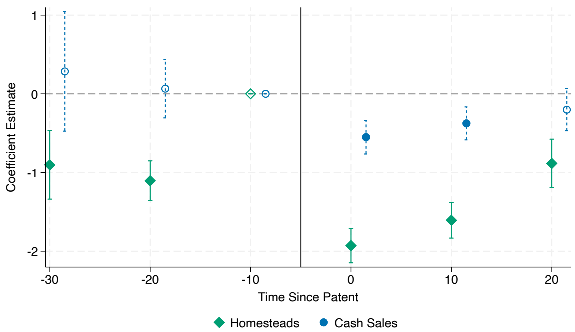
(c) Predicted Income



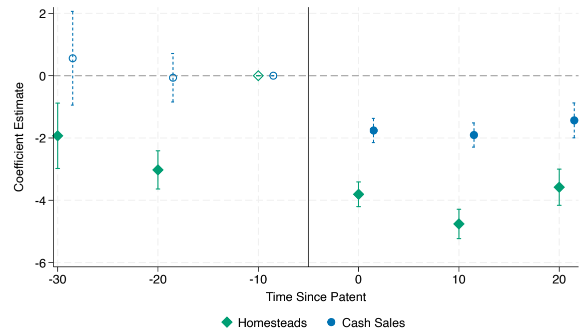
(d) Occupation-Based Wealth

Notes: This figure plots event-study coefficients from an extended two-way fixed effects model. Treatment timing is defined as the first decade boundary following the patent date, with the omitted reference period at $t = -10$ (normalized to zero). Coefficients represent the change in the outcome relative to the omitted period for patentees compared to non-landowners. Capped bars show 95% confidence intervals based on standard errors clustered at the contemporaneous county level. All regressions include age and age-squared as controls, as well as home county fixed effects and are weighted using inverse probability weights. Filled markers indicate significance at the 5% level.

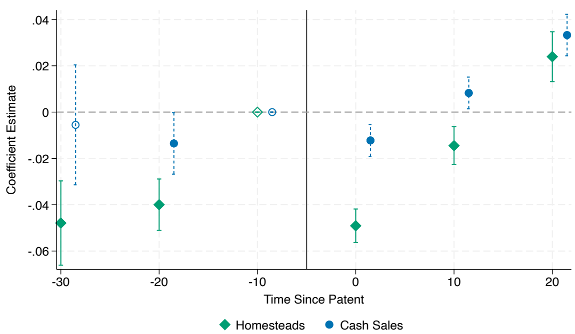
Figure C2: Differences Relative to Non-Landowners, 1850 – 1900 (Patent County FE)



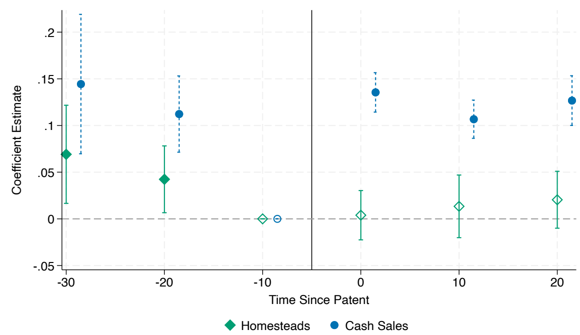
(a) OccScore



(b) Ward Score



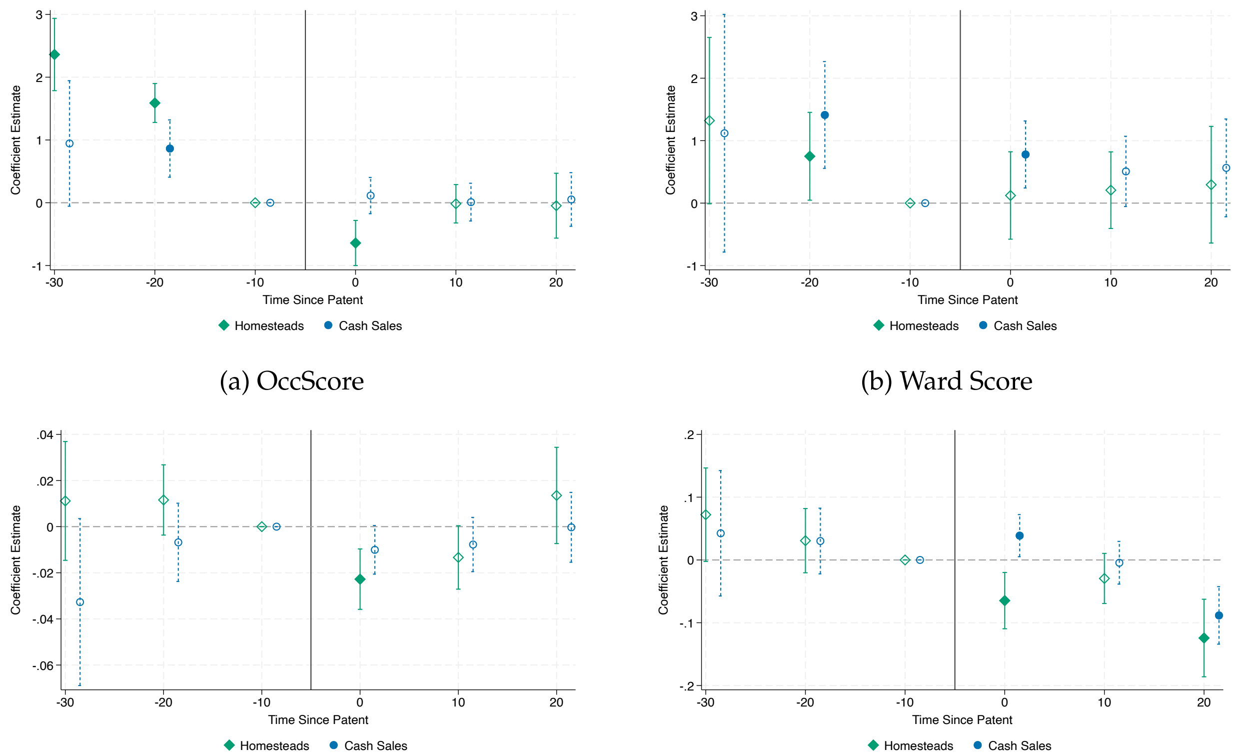
(c) Predicted Income



(d) Occupation-Based Wealth

Notes: This figure plots event-study coefficients from an extended two-way fixed effects model. Treatment timing is defined as the first decade boundary following the patent date, with the omitted reference period at $t = -10$ (normalized to zero). Coefficients represent the change in the outcome relative to the omitted period for patentees compared to non-landowners. Capped bars show 95% confidence intervals based on standard errors clustered at the contemporaneous county level. All regressions include age and age-squared as controls, as well as patent county fixed effects and are weighted using inverse probability weights. Filled markers indicate significance at the 5% level.

Figure C3: Differences Relative to Non-Landowners, 1850 – 1900 (Person + Patent County FE)



(a) OccScore

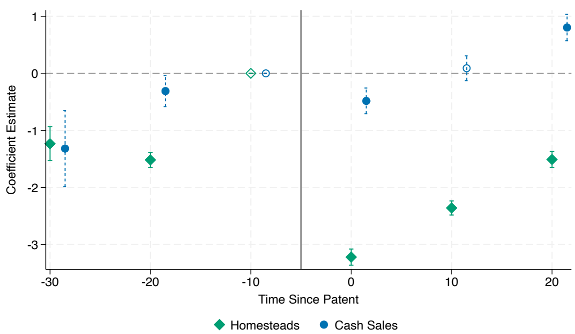
(b) Ward Score

(c) Predicted Income

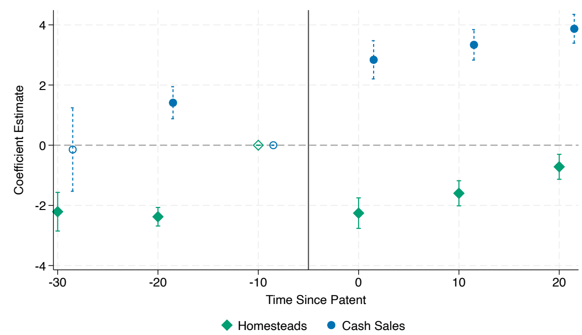
(d) Occupation-Based Wealth

Notes: This figure plots event-study coefficients from an extended two-way fixed effects model. Treatment timing is defined as the first decade boundary following the patent date, with the omitted reference period at $t = -10$ (normalized to zero). Coefficients represent the change in the outcome relative to the omitted period for patentees compared to non-landowners. Capped bars show 95% confidence intervals based on standard errors clustered at the contemporaneous county level. All regressions include age and age-squared as controls, as well as patent county and person fixed effects and are weighted using inverse probability weights. Filled markers indicate significance at the 5% level.

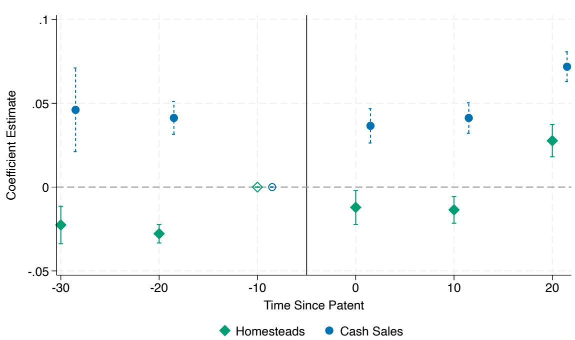
Figure C4: Differences Relative to Non-Landowners, 1900 – 1940 (Home County FE)



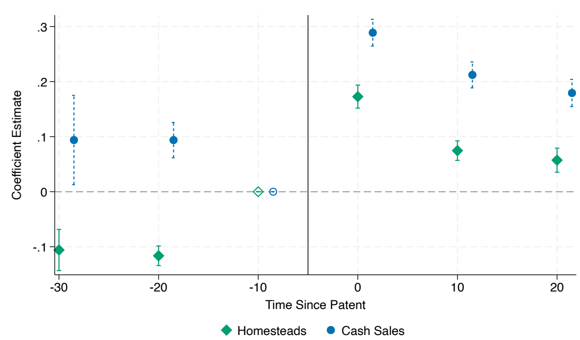
(a) OccScore



(b) Ward Score



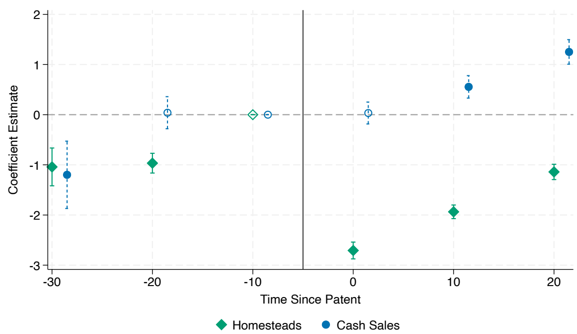
(c) Predicted Income



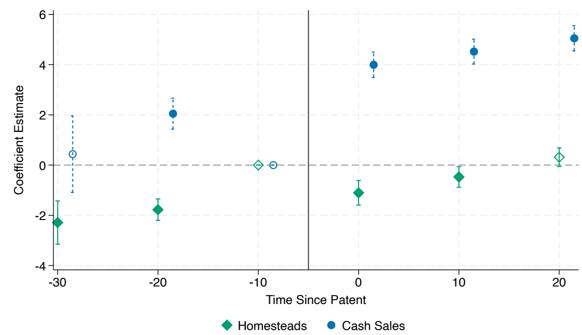
(d) Occupation-Based Wealth

Notes: This figure plots event-study coefficients from an extended two-way fixed effects model. Treatment timing is defined as the first decade boundary following the patent date, with the omitted reference period at $t = -10$ (normalized to zero). Coefficients represent the change in the outcome relative to the omitted period for patentees compared to non-landowners. Capped bars show 95% confidence intervals based on standard errors clustered at the contemporaneous county level. All regressions include age and age-squared as controls, as well as home county fixed effects and are weighted using inverse probability weights. Filled markers indicate significance at the 5% level.

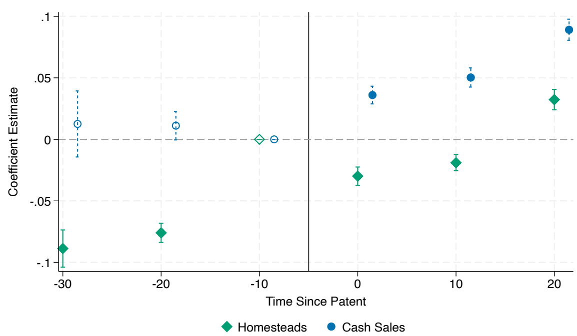
Figure C5: Differences Relative to Non-Landowners, 1900 – 1940 (Patent County FE)



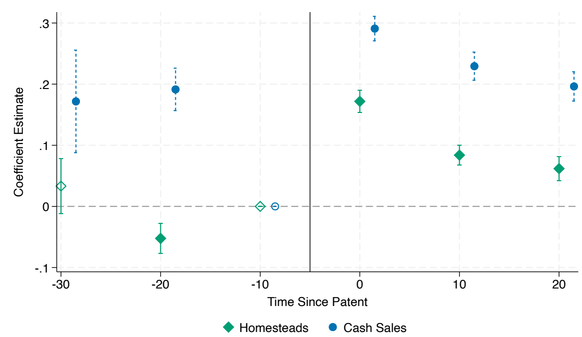
(a) OccScore



(b) Ward Score



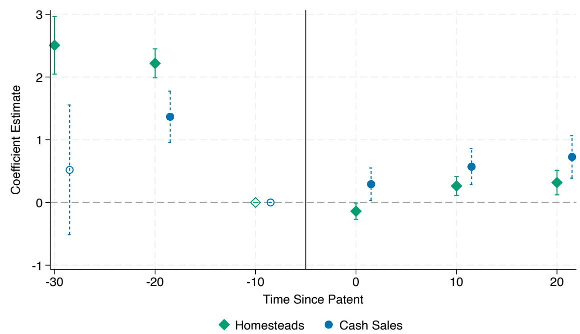
(c) Predicted Income



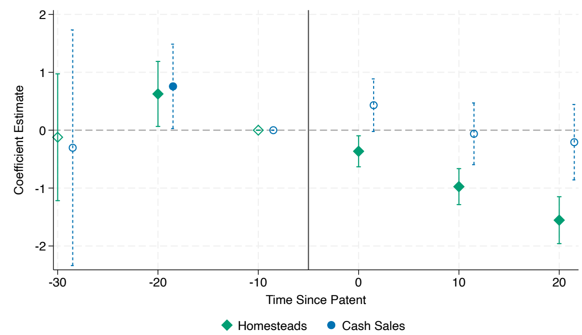
(d) Occupation-Based Wealth

Notes: This figure plots event-study coefficients from an extended two-way fixed effects model. Treatment timing is defined as the first decade boundary following the patent date, with the omitted reference period at $t = -10$ (normalized to zero). Coefficients represent the change in the outcome relative to the omitted period for patentees compared to non-landowners. Capped bars show 95% confidence intervals based on standard errors clustered at the contemporaneous county level. All regressions include age and age-squared as controls, as well as patent county fixed effects and are weighted using inverse probability weights. Filled markers indicate significance at the 5% level.

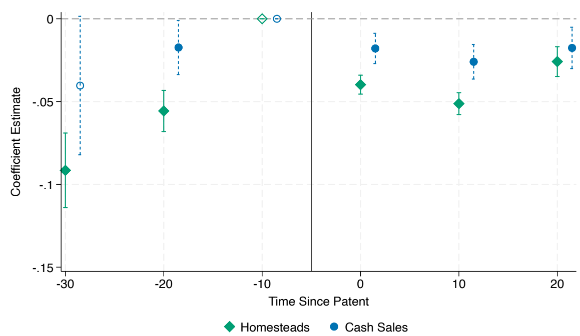
Figure C6: Differences Relative to Non-Landowners, 1900 – 1940 (Person + Patent County FE)



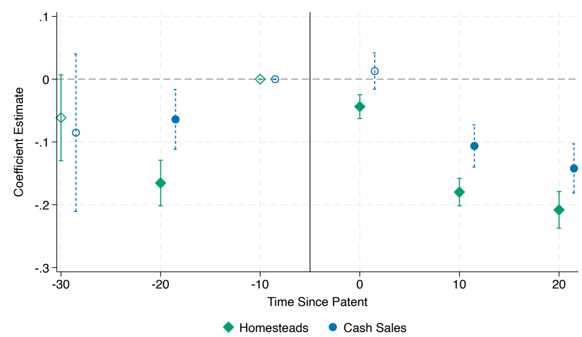
(a) OccScore



(b) Ward Score



(c) Predicted Income



(d) Occupation-Based Wealth

Notes: This figure plots event-study coefficients from an extended two-way fixed effects model. Treatment timing is defined as the first decade boundary following the patent date, with the omitted reference period at $t = -10$ (normalized to zero). Coefficients represent the change in the outcome relative to the omitted period for patentees compared to non-landowners. Capped bars show 95% confidence intervals based on standard errors clustered at the contemporaneous county level. All regressions include age and age-squared as controls, as well as patent county and person fixed effects and are weighted using inverse probability weights. Filled markers indicate significance at the 5% level.

Table C1: Full Cohort-by-Event Time Coefficients, Occupation Score 1850 – 1900

		Homesteads				Cash Sales			
Cohort	Event Time	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1860	t = 0					-1.224***	-0.798***	-0.877***	0.072
						(0.152)	(0.147)	(0.172)	(0.176)
	t = 10					-1.251***	-0.926***	-1.115***	-0.227
						(0.137)	(0.132)	(0.144)	(0.199)
	t = 20					-0.946***	-0.554***	-0.752***	-0.175
						(0.160)	(0.157)	(0.179)	(0.224)
1870	t = -20	1.009	0.384	-2.143	1.280	-0.539**	-0.166	-0.513	0.621
		(0.903)	(1.049)	(1.747)	(1.520)	(0.245)	(0.283)	(0.395)	(0.416)
	t = 0	-3.691***	-3.106***	-3.239***	-2.644**	-0.915***	-0.503**	-0.463	0.252
		(0.408)	(0.467)	(0.566)	(1.307)	(0.225)	(0.225)	(0.307)	(0.370)
	t = 10	-2.449***	-1.615**	-2.252***	-1.168	-0.564**	-0.081	-0.420	0.330
		(0.924)	(0.816)	(0.542)	(1.444)	(0.248)	(0.246)	(0.310)	(0.419)
1880	t = -30	-0.479**	-0.130	-0.358	2.485***	0.322	0.425	0.222	0.915
		(0.219)	(0.368)	(0.722)	(0.510)	(0.465)	(0.639)	(0.969)	(0.911)
	t = -20	-2.080***	-0.907***	-1.910***	1.358***	-1.293***	-0.152	-1.258	0.905
		(0.244)	(0.250)	(0.440)	(0.309)	(0.433)	(0.533)	(0.811)	(0.642)
	t = 0	-3.040***	-1.922***	-2.236***	-0.573***	-1.087***	-0.287	-0.496	0.117
		(0.118)	(0.122)	(0.140)	(0.180)	(0.260)	(0.246)	(0.334)	(0.376)
	t = 20	-1.393***	-0.885***	-1.387***	-0.047	0.155	0.609**	0.232	0.576
		(0.157)	(0.157)	(0.182)	(0.264)	(0.293)	(0.299)	(0.362)	(0.492)
1890	t = -30	-2.018***	-1.386***	-1.243**	2.355***	-0.605	-0.162	-2.605***	0.826
		(0.262)	(0.317)	(0.601)	(0.417)	(0.516)	(0.577)	(0.809)	(0.723)
	t = -20	-1.911***	-1.240***	-1.503***	1.802***	-0.430	0.096	0.205	1.169***
		(0.146)	(0.172)	(0.220)	(0.207)	(0.309)	(0.341)	(0.562)	(0.369)
	t = 10	-2.270***	-1.607***	-2.052***	0.007	-0.661***	0.059	-0.348	0.131
		(0.108)	(0.116)	(0.133)	(0.156)	(0.186)	(0.190)	(0.227)	(0.262)
1900	t = -30	-1.329***	-1.236**	-1.416*	2.091***	-0.170	1.360	1.213	1.369
		(0.447)	(0.513)	(0.746)	(0.548)	(0.908)	(0.891)	(2.787)	(1.053)
	t = -20	-2.233***	-1.065***	-1.597***	1.274***	-0.676	0.757	-0.270	0.420
		(0.255)	(0.284)	(0.344)	(0.375)	(0.490)	(0.479)	(0.739)	(0.631)
	t = 0	-2.664***	-1.822***	-2.145***		-0.587	0.333	-0.821*	
		(0.269)	(0.266)	(0.304)		(0.426)	(0.419)	(0.463)	
Avg. Post-Patent		-2.313***	-1.553***	-1.967***	-0.205	-0.905***	-0.403***	-0.653***	0.056
		(0.075)	(0.077)	(0.092)	(0.132)	(0.079)	(0.075)	(0.091)	(0.120)
Observations		26,653,028	26,652,643	26,556,111	18,348,967	26,653,028	26,652,643	26,556,111	18,348,967
Base County FE		✓				✓			
Patent County FE			✓		✓		✓		✓
Base-Patent Pair FE				✓				✓	
Person FE					✓				✓

Notes: This table reports cohort-specific event-study coefficients from an extended two-way fixed effects model for Occupatin Scores. Each row corresponds to a treatment-timing cohort (defined by the decade of patent receipt) at a given event time relative to the patent decade. The omitted reference period is $t = -10$ (the decade immediately prior to treatment). Columns (1)–(4) report estimates for homesteaders and columns (5)–(8) report estimates for cash buyers, with each set of four columns corresponding to different fixed effect specifications. All regressions include age and age-squared as controls and are weighted using inverse probability weights. Standard errors, in parentheses, are clustered at the contemporaneous county level. "—" indicates an unreliable estimate due to near-collinearity in sparse fixed-effect cells. The "Avg. Post-Patent" row reports the cohort-size-weighted average of post-treatment ($t \geq 0$) coefficients. *p<0.10, **p<0.05, ***p<0.01.

Table C2: Full Cohort-by-Event Time Coefficients, Ward Score 1850 – 1900

		Homesteads				Cash Sales			
Cohort	Event Time	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1860	t = 0					-2.842***	-2.326***	-2.275***	0.363
						(0.246)	(0.232)	(0.272)	(0.310)
	t = 10					-2.543***	-2.384***	-2.527***	0.294
						(0.258)	(0.251)	(0.285)	(0.334)
	t = 20					-1.473***	-1.548***	-1.813***	0.176
						(0.302)	(0.296)	(0.347)	(0.365)
1870	t = -20	0.521	0.562	-12.479**	2.712	-1.752***	0.047	-1.177*	2.548***
		(1.894)	(2.434)	(5.190)	(3.543)	(0.460)	(0.575)	(0.669)	(0.769)
	t = 0	-5.621***	-4.908***	-4.548***	-1.628	-2.077***	-1.247***	-1.001*	1.777**
		(0.821)	(0.871)	(1.221)	(2.447)	(0.474)	(0.467)	(0.577)	(0.746)
	t = 10	-2.570*	-2.814***	-2.642**	1.087	-0.629	-0.129	-0.042	2.173***
		(1.406)	(1.039)	(1.272)	(2.830)	(0.478)	(0.467)	(0.575)	(0.790)
1880	t = -30	-1.046**	0.369	-1.322	2.674**	-0.120	2.178	-0.584	2.299
		(0.502)	(0.923)	(1.268)	(1.220)	(0.783)	(1.347)	(2.520)	(1.707)
	t = -20	-2.328***	-2.743***	-3.692***	0.696	-1.828*	-0.942	-0.966	1.773
		(0.547)	(0.630)	(1.213)	(0.730)	(0.958)	(1.073)	(1.738)	(1.162)
	t = 0	-2.883***	-3.312***	-3.199***	0.182	0.045	-0.544	0.703	1.167
		(0.238)	(0.219)	(0.279)	(0.361)	(0.508)	(0.517)	(0.680)	(0.714)
	t = 20	-2.989***	-3.581***	-3.899***	0.294	-0.525	-1.174*	-0.705	1.459
		(0.306)	(0.297)	(0.366)	(0.477)	(0.598)	(0.646)	(0.750)	(0.994)
1890	t = -30	-3.228***	-3.345***	-3.514**	0.800	-0.777	-0.448	-3.686**	0.790
		(0.523)	(0.735)	(1.392)	(0.878)	(0.995)	(1.101)	(1.512)	(1.353)
	t = -20	-3.554***	-3.356***	-4.540***	0.798*	0.393	0.388	2.134*	1.305*
		(0.358)	(0.421)	(0.701)	(0.464)	(0.610)	(0.669)	(1.278)	(0.712)
	t = 10	-4.225***	-4.800***	-4.839***	0.189	-1.175***	-2.043***	-1.382***	0.150
		(0.228)	(0.244)	(0.264)	(0.314)	(0.342)	(0.328)	(0.403)	(0.490)
1900	t = -30	-3.060***	-2.992***	-3.602	-0.241	-0.929	0.275	-0.370	-0.330
		(1.087)	(1.042)	(2.396)	(1.065)	(1.787)	(1.917)	(4.246)	(2.138)
	t = -20	-2.968***	-2.633***	-2.890***	0.546	-0.778	-0.510	-0.742	-1.187
		(0.553)	(0.529)	(0.707)	(0.709)	(1.072)	(1.134)	(1.851)	(1.282)
	t = 0	-4.347***	-5.291***	-4.227***		-0.482	-1.492*	-0.259	
		(0.465)	(0.448)	(0.611)		(0.803)	(0.770)	(0.992)	
Avg. Post-Patent		-3.605***	-4.161***	-4.132***	0.204	-1.685***	-1.749***	-1.510***	0.615***
		(0.164)	(0.158)	(0.192)	(0.251)	(0.157)	(0.151)	(0.177)	(0.229)
Observations		24,053,794	24,053,406	23,963,910	16,058,150	24,053,794	24,053,406	23,963,910	16,058,150
Base County FE		✓				✓			
Patent County FE			✓		✓		✓		✓
Base-Patent Pair FE				✓				✓	
Person FE					✓				✓

Notes: This table reports cohort-specific event-study coefficients from an extended two-way fixed effects model for Ward Scores. Each row corresponds to a treatment-timing cohort (defined by the decade of patent receipt) at a given event time relative to the patent decade. The omitted reference period is $t = -10$ (the decade immediately prior to treatment). Columns (1)–(4) report estimates for homesteaders and columns (5)–(8) report estimates for cash buyers, with each set of four columns corresponding to different fixed effect specifications. All regressions include age and age-squared as controls and are weighted using inverse probability weights. Standard errors, in parentheses, are clustered at the contemporaneous county level. "—" indicates an unreliable estimate due to near-collinearity in sparse fixed-effect cells. The "Avg. Post-Patent" row reports the cohort-size-weighted average of post-treatment ($t \geq 0$) coefficients. *p<0.10, **p<0.05, ***p<0.01.

Table C3: Full Cohort-by-Event Time Coefficients, Log Predicted Income 1850-1900

		Homesteads				Cash Sales			
Cohort	Event Time	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1860	t = 0					-0.057***	-0.036***	-0.032***	-0.021***
						(0.005)	(0.004)	(0.005)	(0.007)
	t = 10					-0.036***	-0.019***	-0.019***	-0.017**
						(0.005)	(0.005)	(0.005)	(0.007)
	t = 20					0.003	0.019***	0.012**	0.000
						(0.005)	(0.005)	(0.006)	(0.008)
1870	t = -20	0.007	-0.008	-0.187**	0.034	-0.010	-0.029***	-0.001	-0.009
		(0.031)	(0.041)	(0.093)	(0.065)	(0.008)	(0.011)	(0.012)	(0.016)
	t = 0	-0.172***	-0.115***	-0.109***	-0.053	0.018*	0.016*	0.013	0.027**
		(0.016)	(0.017)	(0.022)	(0.041)	(0.011)	(0.009)	(0.012)	(0.013)
	t = 10	-0.130***	-0.079***	-0.091***	-0.023	0.014	0.017*	0.003	0.013
		(0.024)	(0.020)	(0.023)	(0.052)	(0.010)	(0.009)	(0.011)	(0.014)
1880	t = -30	-0.017**	-0.015	-0.005	0.028	0.020	-0.018	0.007	-0.097***
		(0.009)	(0.016)	(0.024)	(0.023)	(0.014)	(0.024)	(0.047)	(0.035)
	t = -20	-0.068***	-0.033***	-0.067***	0.018	0.006	-0.014	0.019	-0.045**
		(0.009)	(0.011)	(0.023)	(0.014)	(0.015)	(0.017)	(0.026)	(0.021)
	t = 0	-0.098***	-0.055***	-0.060***	-0.022***	-0.006	0.017*	0.022*	-0.011
		(0.005)	(0.004)	(0.005)	(0.007)	(0.014)	(0.010)	(0.012)	(0.014)
	t = 20	-0.011**	0.024***	0.010	0.014	0.034***	0.066***	0.051***	-0.002
		(0.005)	(0.005)	(0.006)	(0.011)	(0.010)	(0.010)	(0.012)	(0.017)
1890	t = -30	-0.061***	-0.068***	-0.039	0.005	0.001	-0.004	-0.024	0.001
		(0.010)	(0.013)	(0.030)	(0.016)	(0.016)	(0.019)	(0.029)	(0.025)
	t = -20	-0.079***	-0.050***	-0.039***	0.003	-0.025**	-0.006	0.032*	0.012
		(0.006)	(0.008)	(0.013)	(0.010)	(0.010)	(0.011)	(0.018)	(0.014)
	t = 10	-0.055***	-0.013***	-0.033***	-0.013*	-0.020***	0.032***	0.018**	-0.006
		(0.005)	(0.004)	(0.005)	(0.007)	(0.008)	(0.006)	(0.007)	(0.010)
1900	t = -30	-0.080***	-0.065***	-0.057	-0.010	-0.025	0.015	-0.011	-0.002
		(0.018)	(0.018)	(0.039)	(0.023)	(0.034)	(0.030)	(0.101)	(0.037)
	t = -20	-0.049***	-0.020*	-0.011	0.028*	-0.042**	-0.004	0.020	-0.006
		(0.011)	(0.011)	(0.016)	(0.016)	(0.018)	(0.016)	(0.029)	(0.022)
	t = 0	-0.071***	-0.021**	-0.035***		-0.023*	0.026**	0.007	
		(0.010)	(0.008)	(0.011)		(0.013)	(0.012)	(0.014)	
Avg. Post-Patent		-0.059***	-0.018***	-0.032***	-0.009*	-0.018***	0.006**	0.001	-0.007
		(0.004)	(0.003)	(0.004)	(0.006)	(0.003)	(0.002)	(0.003)	(0.005)
Observations		23,931,617	23,931,228	23,842,133	15,924,480	23,931,617	23,931,228	23,842,133	15,924,480
Base County FE		✓				✓			
Patent County FE			✓		✓		✓		✓
Base-Patent Pair FE				✓				✓	
Person FE					✓				✓

Notes: This table reports cohort-specific event-study coefficients from an extended two-way fixed effects model for Log Predicted Income. Each row corresponds to a treatment-timing cohort (defined by the decade of patent receipt) at a given event time relative to the patent decade. The omitted reference period is $t = -10$ (the decade immediately prior to treatment). Columns (1)–(4) report estimates for homesteaders and columns (5)–(8) report estimates for cash buyers, with each set of four columns corresponding to different fixed effect specifications. All regressions include age and age-squared as controls and are weighted using inverse probability weights. Standard errors, in parentheses, are clustered at the contemporaneous county level. "—" indicates an unreliable estimate due to near-collinearity in sparse fixed-effect cells. The "Avg. Post-Patent" row reports the cohort-size-weighted average of post-treatment ($t \geq 0$) coefficients. *p<0.10, **p<0.05, ***p<0.01.

Table C4: Full Cohort-by-Event Time Coefficients, Log Occupation-Based Wealth 1850 – 1900

		Homesteads				Cash Sales			
Cohort	Event Time	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1860	t = 0					0.101***	0.121***	0.127***	0.052**
						(0.015)	(0.013)	(0.014)	(0.021)
	t = 10					0.115***	0.126***	0.137***	0.005
						(0.016)	(0.016)	(0.017)	(0.024)
	t = 20					0.122***	0.116***	0.140***	-0.062**
						(0.015)	(0.015)	(0.018)	(0.024)
1870	t = -20	0.003	0.160	-0.750**	0.349**	0.009	0.085***	0.041	0.034
		(0.091)	(0.127)	(0.359)	(0.177)	(0.023)	(0.029)	(0.031)	(0.046)
	t = 0	-0.000	0.089**	0.106	0.279**	0.129***	0.177***	0.159***	0.077**
		(0.050)	(0.043)	(0.070)	(0.109)	(0.028)	(0.027)	(0.034)	(0.037)
	t = 10	-0.010	0.021	0.029	0.172	0.071***	0.097***	0.082***	-0.060
		(0.056)	(0.045)	(0.068)	(0.123)	(0.027)	(0.026)	(0.032)	(0.038)
1880	t = -30	-0.043	0.133***	0.002	0.166***	0.083**	0.169***	0.170	0.102
		(0.028)	(0.043)	(0.073)	(0.061)	(0.041)	(0.064)	(0.111)	(0.087)
	t = -20	-0.097***	0.018	-0.081	0.002	0.056	0.108*	0.198***	-0.013
		(0.028)	(0.033)	(0.061)	(0.040)	(0.047)	(0.057)	(0.063)	(0.066)
	t = 0	-0.085***	-0.021	0.024	-0.076***	0.047	0.150***	0.189***	-0.043
		(0.026)	(0.015)	(0.019)	(0.023)	(0.044)	(0.028)	(0.033)	(0.041)
	t = 20	-0.032*	0.020	0.031*	-0.124***	0.079***	0.152***	0.116***	-0.148***
		(0.018)	(0.016)	(0.019)	(0.031)	(0.029)	(0.028)	(0.036)	(0.054)
1890	t = -30	-0.068**	0.038	-0.061	0.055	0.056	0.134**	0.158**	0.048
		(0.029)	(0.038)	(0.071)	(0.054)	(0.046)	(0.054)	(0.068)	(0.071)
	t = -20	-0.085***	0.063**	-0.083**	0.076*	0.001	0.178***	0.102**	0.130***
		(0.020)	(0.026)	(0.041)	(0.039)	(0.032)	(0.035)	(0.052)	(0.043)
	t = 10	-0.026	0.013	0.023	-0.034	0.037*	0.091***	0.090***	0.005
		(0.024)	(0.017)	(0.021)	(0.021)	(0.021)	(0.016)	(0.020)	(0.030)
1900	t = -30	-0.139**	0.015	-0.145	-0.092	-0.055	0.126	0.003	-0.100
		(0.063)	(0.062)	(0.138)	(0.084)	(0.098)	(0.106)	(0.276)	(0.129)
	t = -20	0.001	0.007	0.038	-0.091*	0.000	-0.018	0.035	-0.219***
		(0.039)	(0.037)	(0.048)	(0.050)	(0.053)	(0.058)	(0.098)	(0.080)
	t = 0	0.056	0.075***	0.055		0.153***	0.124***	0.083**	
		(0.036)	(0.028)	(0.041)		(0.039)	(0.034)	(0.040)	
Avg. Post-Patent		-0.036**	0.012	0.028**	-0.063***	0.092***	0.122***	0.125***	-0.008
		(0.018)	(0.012)	(0.014)	(0.016)	(0.009)	(0.008)	(0.009)	(0.014)
Observations		26,171,647	26,171,262	26,075,823	18,088,588	26,171,647	26,171,262	26,075,823	18,088,588
Base County FE		✓				✓			
Patent County FE			✓		✓		✓		✓
Base-Patent Pair FE				✓				✓	
Person FE					✓				✓

Notes: This table reports cohort-specific event-study coefficients from an extended two-way fixed effects model for Log Occupation-Based Wealth. Each row corresponds to a treatment-timing cohort (defined by the decade of patent receipt) at a given event time relative to the patent decade. The omitted reference period is $t = -10$ (the decade immediately prior to treatment). Columns (1)–(4) report estimates for homesteaders and columns (5)–(8) report estimates for cash buyers, with each set of four columns corresponding to different fixed effect specifications. All regressions include age and age-squared as controls and are weighted using inverse probability weights. Standard errors, in parentheses, are clustered at the contemporaneous county level. "—" indicates an unreliable estimate due to near-collinearity in sparse fixed-effect cells. The "Avg. Post-Patent" row reports the cohort-size-weighted average of post-treatment ($t \geq 0$) coefficients. *p<0.10, **p<0.05, ***p<0.01.

Table C5: Full Cohort-by-Event Time Coefficients, Occupation Score 1900 – 1940

		Homesteads				Cash Sales			
Cohort	Event Time	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1910	t = 0	-3.001***	-2.727***	-2.712***	-0.767***	0.141	0.594***	0.734***	-0.025
		(0.092)	(0.095)	(0.100)	(0.103)	(0.172)	(0.160)	(0.178)	(0.202)
	t = 10	-2.470***	-2.223***	-2.430***	-0.388***	0.271	0.608***	0.143	0.061
		(0.099)	(0.103)	(0.109)	(0.112)	(0.169)	(0.168)	(0.183)	(0.221)
	t = 20	-1.519***	-1.174***	-1.555***	0.119	0.947***	1.397***	0.846***	0.406
		(0.121)	(0.124)	(0.132)	(0.137)	(0.173)	(0.174)	(0.195)	(0.248)
1920	t = -20	-1.215***	-0.717***	-1.668***	2.430***	-0.011	0.345*	-0.537**	1.628***
		(0.078)	(0.118)	(0.138)	(0.146)	(0.155)	(0.187)	(0.239)	(0.237)
	t = 0	-3.423***	-2.741***	-2.862***	0.038	-0.947***	-0.343**	-0.724***	0.622***
		(0.097)	(0.117)	(0.130)	(0.105)	(0.149)	(0.148)	(0.159)	(0.198)
	t = 10	-2.381***	-1.786***	-2.447***	0.555***	0.037	0.648***	-0.282*	1.158***
		(0.087)	(0.099)	(0.109)	(0.114)	(0.155)	(0.158)	(0.163)	(0.210)
	t = 20	-1.504***	-1.116***	-1.633***	0.466***	0.649***	1.096***	0.368*	1.069***
		(0.086)	(0.093)	(0.112)	(0.132)	(0.159)	(0.170)	(0.193)	(0.230)
1930	t = -30	-1.212***	-1.017***	-2.233***	2.481***	-1.334***	-1.239***	-1.937***	0.686
		(0.155)	(0.197)	(0.245)	(0.242)	(0.350)	(0.350)	(0.451)	(0.539)
	t = -20	-2.046***	-1.381***	-2.058***	1.836***	-1.568***	-1.247***	-1.813***	0.400
		(0.134)	(0.148)	(0.169)	(0.168)	(0.322)	(0.330)	(0.399)	(0.398)
	t = 0	-3.156***	-2.605***	-3.005***	0.526***	-1.279***	-0.899***	-1.141***	0.273
		(0.125)	(0.141)	(0.147)	(0.133)	(0.322)	(0.324)	(0.375)	(0.400)
	t = 10	-2.100***	-1.780***	-2.145***	0.746***	-0.690*	-0.373	-0.463	-0.085
		(0.124)	(0.127)	(0.146)	(0.161)	(0.362)	(0.361)	(0.415)	(0.505)
1940	t = -30	-1.684**	-1.526*	-2.319**	3.033***	-1.043	-0.475	-1.297	-2.446
		(0.804)	(0.818)	(0.948)	(0.991)	(1.558)	(1.645)	(2.034)	(2.356)
	t = -20	-3.137***	-2.713***	-2.530***	1.433**	-1.178	-0.768	-2.434	-2.131
		(0.538)	(0.555)	(0.584)	(0.725)	(1.442)	(1.443)	(2.169)	(2.539)
	t = 0	-2.307***	-2.242***	-2.806***	1.054	-0.749	-0.775	-0.692	-3.803**
		(0.626)	(0.618)	(0.690)	(0.745)	(1.414)	(1.371)	(1.771)	(1.921)
Avg. Post-Patent		-2.544***	-2.094***	-2.409***	0.103	0.045	0.526***	0.082	0.500***
		(0.058)	(0.067)	(0.073)	(0.067)	(0.088)	(0.089)	(0.095)	(0.126)
Observations		91,789,176	91,789,166	91,664,498	78,365,952	91,789,176	91,789,166	91,664,498	78,365,952
Base County FE		✓				✓			
Patent County FE			✓		✓		✓		✓
Base-Patent Pair FE				✓				✓	
Person FE					✓				✓

Notes: This table reports cohort-specific event-study coefficients from an extended two-way fixed effects model for Occupatin Scores. Each row corresponds to a treatment-timing cohort (defined by the decade of patent receipt) at a given event time relative to the patent decade. The omitted reference period is $t = -10$ (the decade immediately prior to treatment). Columns (1)–(4) report estimates for homesteaders and columns (5)–(8) report estimates for cash buyers, with each set of four columns corresponding to different fixed effect specifications. All regressions include age and age-squared as controls and are weighted using inverse probability weights. Standard errors, in parentheses, are clustered at the contemporaneous county level. "—" indicates an unreliable estimate due to near-collinearity in sparse fixed-effect cells. The "Avg. Post-Patent" row reports the cohort-size-weighted average of post-treatment ($t \geq 0$) coefficients. *p<0.10, **p<0.05, ***p<0.01.

Table C6: Full Cohort-by-Event Time Coefficients, Ward Score 1900 – 1940

		Homesteads				Cash Sales			
Cohort	Event Time	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1910	t = 0	-3.800***	-2.721***	-2.791***	-1.218***	2.888***	4.219***	4.233***	0.803**
		(0.389)	(0.310)	(0.324)	(0.244)	(0.535)	(0.323)	(0.338)	(0.381)
	t = 10	-3.052***	-2.121***	-2.504***	-1.540***	2.840***	3.868***	3.195***	-0.008
		(0.311)	(0.252)	(0.266)	(0.253)	(0.377)	(0.326)	(0.352)	(0.410)
	t = 20	-1.717***	-0.661**	-1.281***	-1.847***	3.473***	4.734***	3.713***	-0.180
		(0.318)	(0.278)	(0.330)	(0.285)	(0.337)	(0.338)	(0.344)	(0.470)
1920	t = -20	-2.691***	-1.676***	-3.510***	1.180***	1.338***	2.008***	0.907**	0.907**
		(0.169)	(0.264)	(0.278)	(0.371)	(0.298)	(0.352)	(0.447)	(0.420)
	t = 0	-1.576***	-0.376	-1.280***	0.050	2.911***	3.937***	2.891***	0.203
		(0.291)	(0.279)	(0.253)	(0.206)	(0.323)	(0.360)	(0.335)	(0.305)
	t = 10	-0.779***	0.590**	-0.773**	-0.775***	3.768***	5.150***	3.689***	-0.165
		(0.257)	(0.282)	(0.301)	(0.213)	(0.330)	(0.366)	(0.358)	(0.364)
	t = 20	0.029	1.044***	0.078	-1.336***	4.293***	5.383***	4.098***	-0.237
		(0.228)	(0.221)	(0.268)	(0.262)	(0.348)	(0.372)	(0.404)	(0.414)
1930	t = -30	-2.223***	-2.275***	-4.146***	-0.089	-0.082	0.483	-0.198	0.185
		(0.334)	(0.453)	(0.514)	(0.567)	(0.713)	(0.774)	(0.911)	(1.034)
	t = -20	-1.766***	-1.980***	-3.308***	-0.461	1.734***	2.217***	1.518**	0.471
		(0.319)	(0.356)	(0.418)	(0.412)	(0.663)	(0.663)	(0.727)	(0.773)
	t = 0	-1.156***	0.057	-1.384***	0.126	2.078***	3.039***	2.034**	-0.026
		(0.423)	(0.483)	(0.489)	(0.296)	(0.728)	(0.744)	(0.816)	(0.717)
	t = 10	-0.946***	-0.069	-0.808**	-0.419	3.588***	4.440***	3.244***	0.267
		(0.313)	(0.321)	(0.380)	(0.346)	(0.823)	(0.809)	(0.885)	(0.869)
1940	t = -30	-1.950	-2.534	-1.688	-0.793	-1.251	-0.392	-2.831	-9.002
		(1.614)	(1.828)	(2.197)	(2.075)	(4.043)	(4.450)	(4.466)	(6.384)
	t = -20	-2.149*	-1.483	-2.381	0.425	1.276	2.171	-0.346	-7.217*
		(1.287)	(1.338)	(1.490)	(1.460)	(2.863)	(2.651)	(3.510)	(3.714)
	t = 0	-1.575	-1.545	-3.600**	1.065	3.668	3.676	5.859	-7.829**
		(1.382)	(1.454)	(1.659)	(1.550)	(3.285)	(3.175)	(3.591)	(3.900)
Avg. Post-Patent		-1.690***	-0.572***	-1.383***	-0.839***	3.275***	4.446***	3.554***	0.097
		(0.217)	(0.204)	(0.216)	(0.138)	(0.234)	(0.204)	(0.201)	(0.236)
Observations		84,249,346	84,249,335	84,132,632	70,218,812	84,249,346	84,249,335	84,132,632	70,218,812
Base County FE		✓				✓			
Patent County FE			✓		✓		✓		✓
Base-Patent Pair FE				✓				✓	
Person FE					✓				✓

Notes: This table reports cohort-specific event-study coefficients from an extended two-way fixed effects model for Ward Scores. Each row corresponds to a treatment-timing cohort (defined by the decade of patent receipt) at a given event time relative to the patent decade. The omitted reference period is $t = -10$ (the decade immediately prior to treatment). Columns (1)–(4) report estimates for homesteaders and columns (5)–(8) report estimates for cash buyers, with each set of four columns corresponding to different fixed effect specifications. All regressions include age and age-squared as controls and are weighted using inverse probability weights. Standard errors, in parentheses, are clustered at the contemporaneous county level. "—" indicates an unreliable estimate due to near-collinearity in sparse fixed-effect cells. The "Avg. Post-Patent" row reports the cohort-size-weighted average of post-treatment ($t \geq 0$) coefficients. *p<0.10, **p<0.05, ***p<0.01.

Table C7: Full Cohort-by-Event Time Coefficients, Log Predicted Income 1900 – 1940

		Homesteads				Cash Sales			
Cohort	Event Time	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1910	t = 0	-0.044***	-0.042***	-0.041***	-0.075***	0.042***	0.047***	0.058***	-0.017**
		(0.004)	(0.003)	(0.004)	(0.005)	(0.007)	(0.005)	(0.006)	(0.008)
	t = 10	-0.041***	-0.036***	-0.039***	-0.089***	0.030***	0.040***	0.033***	-0.043***
		(0.004)	(0.004)	(0.004)	(0.005)	(0.007)	(0.006)	(0.006)	(0.008)
	t = 20	0.025***	0.039***	0.028***	-0.037***	0.072***	0.092***	0.076***	-0.020**
		(0.006)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.009)
1920	t = -20	-0.036***	-0.080***	-0.031***	-0.042***	0.034***	0.002	0.061***	-0.019*
		(0.003)	(0.005)	(0.006)	(0.008)	(0.005)	(0.007)	(0.008)	(0.010)
	t = 0	-0.005	-0.035***	-0.033***	-0.024***	0.025***	0.021***	0.025***	-0.021***
		(0.008)	(0.005)	(0.006)	(0.005)	(0.007)	(0.005)	(0.005)	(0.007)
	t = 10	-0.009	-0.018***	-0.041***	-0.031***	0.044***	0.054***	0.037***	-0.015*
		(0.006)	(0.005)	(0.005)	(0.005)	(0.007)	(0.006)	(0.006)	(0.008)
	t = 20	0.030***	0.027***	0.005	-0.017***	0.072***	0.086***	0.067***	-0.015*
		(0.006)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.009)
1930	t = -30	-0.024***	-0.090***	-0.022**	-0.091***	0.052***	0.018	0.065***	-0.029
		(0.006)	(0.008)	(0.009)	(0.012)	(0.013)	(0.014)	(0.017)	(0.022)
	t = -20	-0.013**	-0.069***	-0.011	-0.082***	0.074***	0.051***	0.075***	-0.006
		(0.006)	(0.006)	(0.007)	(0.009)	(0.013)	(0.012)	(0.014)	(0.017)
	t = 0	0.027***	0.003	-0.002	-0.015**	0.074***	0.068***	0.073***	0.004
		(0.008)	(0.007)	(0.007)	(0.006)	(0.013)	(0.012)	(0.013)	(0.015)
	t = 10	0.026***	0.011*	0.002	-0.033***	0.088***	0.089***	0.087***	0.005
		(0.007)	(0.006)	(0.007)	(0.008)	(0.015)	(0.014)	(0.016)	(0.019)
1940	t = -30	0.010	-0.061*	0.030	-0.112**	-0.066	-0.089	-0.018	-0.250**
		(0.030)	(0.033)	(0.041)	(0.044)	(0.069)	(0.068)	(0.077)	(0.114)
	t = -20	0.020	-0.042*	0.007	-0.085***	0.053	0.014	0.072	-0.140**
		(0.023)	(0.023)	(0.026)	(0.030)	(0.051)	(0.049)	(0.067)	(0.068)
	t = 0	0.033	0.009	-0.014	-0.058**	0.063	0.036	0.111	-0.168**
		(0.026)	(0.025)	(0.029)	(0.028)	(0.060)	(0.058)	(0.073)	(0.080)
Avg. Post-Patent		-0.004	-0.013***	-0.021***	-0.041***	0.047***	0.055***	0.049***	-0.021***
		(0.004)	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)	(0.003)	(0.005)
Observations		81,289,120	81,289,109	81,173,057	67,108,381	81,289,120	81,289,109	81,173,057	67,108,381
Base County FE		✓				✓			
Patent County FE			✓		✓		✓		✓
Base-Patent Pair FE				✓				✓	
Person FE					✓				✓

Notes: This table reports cohort-specific event-study coefficients from an extended two-way fixed effects model for Log Predicted Income. Each row corresponds to a treatment-timing cohort (defined by the decade of patent receipt) at a given event time relative to the patent decade. The omitted reference period is $t = -10$ (the decade immediately prior to treatment). Columns (1)–(4) report estimates for homesteaders and columns (5)–(8) report estimates for cash buyers, with each set of four columns corresponding to different fixed effect specifications. All regressions include age and age-squared as controls and are weighted using inverse probability weights. Standard errors, in parentheses, are clustered at the contemporaneous county level. "—" indicates an unreliable estimate due to near-collinearity in sparse fixed-effect cells. The "Avg. Post-Patent" row reports the cohort-size-weighted average of post-treatment ($t \geq 0$) coefficients. *p<0.10, **p<0.05, ***p<0.01.

Table C8: Full Cohort-by-Event Time Coefficients, Log Occupation-Based Wealth 1900 – 1940

		Homesteads				Cash Sales			
Cohort	Event Time	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1910	t = 0	0.234***	0.205***	0.187***	-0.025	0.351***	0.310***	0.304***	0.093***
		(0.013)	(0.011)	(0.011)	(0.017)	(0.018)	(0.014)	(0.018)	(0.024)
	t = 10	0.126***	0.107***	0.109***	-0.174***	0.230***	0.213***	0.201***	-0.066***
		(0.011)	(0.011)	(0.011)	(0.018)	(0.016)	(0.015)	(0.017)	(0.025)
	t = 20	0.098***	0.083***	0.090***	-0.201***	0.202***	0.196***	0.202***	-0.095***
		(0.015)	(0.014)	(0.015)	(0.019)	(0.017)	(0.015)	(0.019)	(0.028)
1920	t = -20	-0.143***	-0.085***	-0.100***	-0.196***	0.061***	0.170***	0.201***	-0.067**
		(0.010)	(0.016)	(0.018)	(0.023)	(0.017)	(0.019)	(0.025)	(0.028)
	t = 0	0.175***	0.170***	0.166***	-0.042***	0.235***	0.273***	0.266***	-0.052**
		(0.014)	(0.011)	(0.012)	(0.015)	(0.016)	(0.015)	(0.017)	(0.020)
	t = 10	0.061***	0.074***	0.073***	-0.185***	0.188***	0.235***	0.229***	-0.151***
		(0.012)	(0.011)	(0.013)	(0.016)	(0.017)	(0.018)	(0.018)	(0.024)
	t = 20	0.027**	0.046***	0.039***	-0.213***	0.155***	0.197***	0.193***	-0.193***
		(0.012)	(0.012)	(0.015)	(0.020)	(0.017)	(0.018)	(0.020)	(0.026)
1930	t = -30	-0.111***	0.029	-0.070**	-0.063*	0.108**	0.188***	0.238***	-0.058
		(0.020)	(0.024)	(0.035)	(0.036)	(0.043)	(0.044)	(0.056)	(0.065)
	t = -20	-0.069***	0.006	-0.047*	-0.108***	0.239***	0.286***	0.338***	-0.035
		(0.018)	(0.018)	(0.026)	(0.027)	(0.042)	(0.040)	(0.045)	(0.053)
	t = 0	0.064***	0.123***	0.140***	-0.077***	0.256***	0.292***	0.316***	-0.042
		(0.020)	(0.020)	(0.023)	(0.021)	(0.037)	(0.036)	(0.038)	(0.044)
	t = 10	0.012	0.064***	0.084***	-0.177***	0.258***	0.295***	0.307***	-0.066
		(0.017)	(0.016)	(0.019)	(0.024)	(0.040)	(0.039)	(0.043)	(0.054)
1940	t = -30	0.005	0.124	0.200	-0.028	-0.160	-0.112	0.150	-0.575*
		(0.103)	(0.096)	(0.139)	(0.134)	(0.198)	(0.215)	(0.290)	(0.343)
	t = -20	-0.015	0.075	0.053	-0.107	0.050	0.103	0.211	-0.421*
		(0.073)	(0.076)	(0.089)	(0.096)	(0.137)	(0.140)	(0.187)	(0.217)
	t = 0	-0.058	0.014	-0.044	-0.215**	0.265	0.262	0.655***	-0.414*
		(0.092)	(0.087)	(0.106)	(0.099)	(0.207)	(0.199)	(0.188)	(0.230)
Avg. Post-Patent		0.113***	0.117***	0.116***	-0.128***	0.234***	0.245***	0.242***	-0.068***
		(0.009)	(0.008)	(0.009)	(0.010)	(0.010)	(0.008)	(0.009)	(0.015)
Observations		87,546,280	87,546,271	87,424,661	73,811,341	87,546,280	87,546,271	87,424,661	73,811,341
Base County FE		✓				✓			
Patent County FE			✓		✓		✓		✓
Base-Patent Pair FE				✓				✓	
Person FE					✓				✓

Notes: This table reports cohort-specific event-study coefficients from an extended two-way fixed effects model for Log Occupation-Based Wealth. Each row corresponds to a treatment-timing cohort (defined by the decade of patent receipt) at a given event time relative to the patent decade. The omitted reference period is $t = -10$ (the decade immediately prior to treatment). Columns (1)–(4) report estimates for homesteaders and columns (5)–(8) report estimates for cash buyers, with each set of four columns corresponding to different fixed effect specifications. All regressions include age and age-squared as controls and are weighted using inverse probability weights. Standard errors, in parentheses, are clustered at the contemporaneous county level. "—" indicates an unreliable estimate due to near-collinearity in sparse fixed-effect cells. The "Avg. Post-Patent" row reports the cohort-size-weighted average of post-treatment ($t \geq 0$) coefficients. *p<0.10, **p<0.05, ***p<0.01.

Table C9: Robustness to Dropping 1890 Cohort (1880s patents)

	(1)	(2)	(3)	(4)
Panel A: Occupational Income Score				
Homesteads	-2.331***	-1.494***	-1.869***	-0.194
	(0.097)	(0.094)	(0.108)	(0.194)
Cash Sales	-0.954***	-0.497***	-0.709***	0.095
	(0.085)	(0.079)	(0.096)	(0.137)
Observations	26,654,334	26,653,976	26,556,255	18,350,501
Adj. R-Squared	0.070	0.070	0.070	0.498
Panel B: Ward Occupational Score				
Homesteads	-3.138***	-3.632***	-3.539***	0.302
	(0.202)	(0.183)	(0.233)	(0.366)
Cash Sales	-1.788***	-1.663***	-1.512***	0.774***
	(0.169)	(0.162)	(0.192)	(0.256)
Observations	24,055,012	24,054,650	23,964,047	16,059,613
Adj. R-Squared	0.353	0.353	0.353	0.732
Panel C: Log Predicted Income				
Homesteads	-0.061***	-0.021***	-0.030***	-0.005
	(0.004)	(0.003)	(0.004)	(0.007)
Cash Sales	-0.018***	0.001	-0.003	-0.006
	(0.003)	(0.003)	(0.003)	(0.005)
Observations	23,932,836	23,932,474	23,842,270	15,925,949
Adj. R-Squared	0.303	0.303	0.303	0.627
Panel D: Log Occupation-Based Wealth				
Homesteads	-0.043**	0.010	0.031**	-0.078***
	(0.018)	(0.012)	(0.014)	(0.023)
Cash Sales	0.104***	0.128***	0.132***	-0.008
	(0.010)	(0.008)	(0.010)	(0.016)
Observations	26,172,953	26,172,595	26,075,967	18,090,122
Adj. R-Squared	0.200	0.200	0.200	0.513
Base County FE	✓			
Patent County FE		✓		✓
Base-Patent Pair FE			✓	
Person FE				✓

Notes: This table reports average post-patent effects from an extended TWFE model after excluding individuals who received patents during the 1880s (the "1890" treatment-timing cohort). This cohort is excluded because the missing 1890 census means these individuals have no observation at their treatment date ($t = 0$); their first post-treatment observation occurs at $t = 10$, creating an asymmetry relative to other cohorts. All other estimation details follow Table 4. Standard errors are clustered at the contemporaneous county level. "—" indicates an unreliable estimate due to near-collinearity. *p<0.10, **p<0.05, ***p<0.01.

Table C10: Robustness to Dropping Observations with Less than 3 Links, 1850 – 1900

	(1)	(2)	(3)	(4)
Panel A: Occupational Income Score				
Homesteads	-2.938***	-2.104***	-2.325***	0.011
	(0.122)	(0.135)	(0.179)	(0.170)
Cash Sales	-1.307***	-0.867***	-1.043***	0.069
	(0.107)	(0.104)	(0.129)	(0.144)
Observations	10,138,617	10,138,114	10,096,992	10,138,099
Panel B: Ward Occupational Score				
Homesteads	-3.362***	-3.157***	-3.662***	0.180
	(0.258)	(0.252)	(0.382)	(0.321)
Cash Sales	-2.786***	-2.420***	-2.339***	0.602**
	(0.197)	(0.188)	(0.245)	(0.265)
Observations	9,257,495	9,257,001	9,218,805	9,210,038
Panel C: Log Predicted Income				
Homesteads	-0.089***	-0.040***	-0.048***	0.004
	(0.005)	(0.005)	(0.007)	(0.007)
Cash Sales	-0.036***	-0.014***	-0.014***	-0.011**
	(0.004)	(0.003)	(0.004)	(0.005)
Observations	9,217,421	9,216,927	9,178,893	9,165,651
Panel D: Log Occupation-Based Wealth				
Homesteads	-0.151***	-0.053***	-0.019	-0.076***
	(0.022)	(0.013)	(0.019)	(0.021)
Cash Sales	0.042***	0.076***	0.087***	0.002
	(0.011)	(0.009)	(0.011)	(0.017)
Observations	10,089,576	10,089,071	10,048,106	10,089,054
Base County FE	✓			
Patent County FE		✓		✓
Base-Patent Pair FE			✓	
Person FE				✓

Notes: This table reports average post-patent effects from an extended TWFE model, restricting the sample to individuals (both treated and control) observed in at least 3 census years. This tests robustness to the unbalanced panel structure by progressively requiring more complete observation histories. Treatment timing is defined as the first decade boundary following the patent date, with the omitted period at $t = -10$. All regressions include age and age-squared as controls and are weighted using inverse probability weights. Standard errors are clustered at the contemporaneous county level. "—" indicates an unreliable estimate due to near-collinearity. *p<0.10, **p<0.05, ***p<0.01.

Table C11: Robustness to Dropping Observations with Less than 4 Links, 1850 – 1900

	(1)	(2)	(3)	(4)
Panel A: Occupational Income Score				
Homesteads	-3.093***	-1.986***	-2.626***	0.782**
	(0.251)	(0.296)	(0.517)	(0.367)
Cash Sales	-1.423***	-0.835***	-0.922***	-0.074
	(0.181)	(0.176)	(0.250)	(0.216)
Observations	3,740,681	3,739,879	3,727,070	3,739,879
Panel B: Ward Occupational Score				
Homesteads	-3.725***	-3.639***	-2.720*	0.206
	(0.537)	(0.639)	(1.517)	(0.713)
Cash Sales	-3.061***	-2.466***	-2.331***	0.543
	(0.298)	(0.282)	(0.403)	(0.376)
Observations	3,408,921	3,408,138	3,396,470	3,404,891
Panel C: Log Predicted Income				
Homesteads	-0.110***	-0.042***	-0.045***	0.036***
	(0.010)	(0.010)	(0.016)	(0.013)
Cash Sales	-0.048***	-0.017***	-0.015**	-0.017**
	(0.006)	(0.005)	(0.006)	(0.007)
Observations	3,402,725	3,401,937	3,390,279	3,398,119
Panel D: Log Occupation-Based Wealth				
Homesteads	-0.314***	-0.121***	-0.083	-0.009
	(0.029)	(0.028)	(0.050)	(0.043)
Cash Sales	0.009	0.054***	0.067***	0.005
	(0.014)	(0.012)	(0.017)	(0.022)
Observations	3,730,398	3,729,599	3,716,809	3,729,599
Base County FE	✓			
Patent County FE		✓		✓
Base-Patent Pair FE			✓	
Person FE				✓

Notes: This table reports average post-patent effects from an extended TWFE model, restricting the sample to individuals (both treated and control) observed in at least 4 census years. This tests robustness to the unbalanced panel structure by progressively requiring more complete observation histories. Treatment timing is defined as the first decade boundary following the patent date, with the omitted period at $t = -10$. All regressions include age and age-squared as controls and are weighted using inverse probability weights. Standard errors are clustered at the contemporaneous county level. "—" indicates an unreliable estimate due to near-collinearity. *p<0.10, **p<0.05, ***p<0.01.

Table C12: Robustness to Dropping Observations with Less than 3 Links, 1900 – 1940

	(1)	(2)	(3)	(4)
Panel A: Occupational Income Score				
Homesteads	-2.916***	-2.519***	-2.760***	0.039
	(0.068)	(0.081)	(0.083)	(0.065)
Cash Sales	-0.175*	0.241**	-0.135	0.482***
	(0.096)	(0.095)	(0.101)	(0.127)
Observations	51,158,318	51,158,289	51,059,841	51,158,287
Panel B: Ward Occupational Score				
Homesteads	-2.861***	-1.981***	-2.607***	-0.904***
	(0.229)	(0.229)	(0.233)	(0.135)
Cash Sales	2.339***	3.269***	2.561***	-0.056
	(0.254)	(0.217)	(0.215)	(0.243)
Observations	47,314,556	47,314,522	47,221,792	47,107,296
Panel C: Log Predicted Income				
Homesteads	-0.015***	-0.028***	-0.035***	-0.041***
	(0.005)	(0.004)	(0.004)	(0.003)
Cash Sales	0.039***	0.042***	0.039***	-0.020***
	(0.004)	(0.003)	(0.003)	(0.005)
Observations	45,757,388	45,757,353	45,665,181	45,362,892
Panel D: Log Occupation-Based Wealth				
Homesteads	0.068***	0.063***	0.068***	-0.125***
	(0.009)	(0.008)	(0.009)	(0.010)
Cash Sales	0.197***	0.197***	0.202***	-0.064***
	(0.010)	(0.009)	(0.010)	(0.015)
Observations	49,266,941	49,266,911	49,170,675	49,180,061
Base County FE	✓			
Patent County FE		✓		✓
Base-Patent Pair FE			✓	
Person FE				✓

Notes: This table reports average post-patent effects from an extended TWFE model, restricting the sample to individuals (both treated and control) observed in at least 3 census years. This tests robustness to the unbalanced panel structure by progressively requiring more complete observation histories. Treatment timing is defined as the first decade boundary following the patent date, with the omitted period at $t = -10$. All regressions include age and age-squared as controls and are weighted using inverse probability weights. Standard errors are clustered at the contemporaneous county level. "—" indicates an unreliable estimate due to near-collinearity. *p<0.10, **p<0.05, ***p<0.01.

Table C13: Robustness to Dropping Observations with Less than 4 Links, 1900 – 1940

	(1)	(2)	(3)	(4)
Panel A: Occupational Income Score				
Homesteads	-3.248***	-2.920***	-3.142***	-0.043
	(0.081)	(0.097)	(0.101)	(0.077)
Cash Sales	-0.380***	-0.017	-0.438***	0.518***
	(0.123)	(0.122)	(0.136)	(0.137)
Observations	25,301,656	25,301,624	25,233,806	25,301,624
Panel B: Ward Occupational Score				
Homesteads	-3.850***	-3.321***	-3.714***	-1.063***
	(0.261)	(0.251)	(0.271)	(0.153)
Cash Sales	1.654***	2.336***	1.823***	-0.075
	(0.291)	(0.255)	(0.260)	(0.270)
Observations	23,421,370	23,421,336	23,357,313	23,398,382
Panel C: Log Predicted Income				
Homesteads	-0.024***	-0.045***	-0.048***	-0.047***
	(0.005)	(0.004)	(0.004)	(0.003)
Cash Sales	0.034***	0.032***	0.034***	-0.014**
	(0.005)	(0.004)	(0.004)	(0.005)
Observations	22,926,797	22,926,763	22,863,136	22,837,356
Panel D: Log Occupation-Based Wealth				
Homesteads	0.036***	0.023***	0.037***	-0.149***
	(0.010)	(0.009)	(0.011)	(0.012)
Cash Sales	0.166***	0.161***	0.175***	-0.073***
	(0.012)	(0.010)	(0.012)	(0.017)
Observations	24,439,957	24,439,924	24,373,565	24,433,855
Base County FE	✓			
Patent County FE		✓		✓
Base-Patent Pair FE			✓	
Person FE				✓

Notes: This table reports average post-patent effects from an extended TWFE model, restricting the sample to individuals (both treated and control) observed in at least 4 census years. This tests robustness to the unbalanced panel structure by progressively requiring more complete observation histories. Treatment timing is defined as the first decade boundary following the patent date, with the omitted period at $t = -10$. All regressions include age and age-squared as controls and are weighted using inverse probability weights. Standard errors are clustered at the contemporaneous county level. "—" indicates an unreliable estimate due to near-collinearity. *p<0.10, **p<0.05, ***p<0.01.

Appendix D: Intergenerational Mobility

Table D1: Pooled Rank-Rank Results

	(1) Ward Score	(2) Occ. Score	(3) Pred. Income	(4) Occ. Wealth
Panel A: OLS				
Father Rank	0.036***	0.037***	0.023***	0.021***
	(0.001)	(0.001)	(0.001)	(0.001)
Homestead	-1.052***	-1.982***	-1.223***	0.151
	(0.347)	(0.329)	(0.354)	(0.316)
Cash Sale	0.396	-0.696***	0.330	0.878***
	(0.259)	(0.231)	(0.270)	(0.228)
Homestead × Father Rank	-0.018***	-0.016**	-0.015**	-0.019***
	(0.007)	(0.008)	(0.007)	(0.006)
Cash Sale × Father Rank	-0.015***	0.004	-0.014**	-0.018***
	(0.006)	(0.007)	(0.005)	(0.004)
N	1,489,865	1,714,840	1,485,151	1,789,249
Panel B: IV				
Father Rank	0.329***	0.391***	0.322***	0.349***
	(0.005)	(0.007)	(0.006)	(0.007)
Homestead	-0.794	-1.206	3.552*	3.059
	(1.008)	(1.959)	(1.919)	(2.031)
Cash Sale	0.040	1.475	0.168	1.589
	(0.753)	(1.117)	(1.382)	(1.177)
Homestead × Father Rank	-0.022	-0.042	-0.139***	-0.097*
	(0.026)	(0.070)	(0.050)	(0.051)
Cash Sale × Father Rank	0.010	-0.010	-0.019	-0.046
	(0.020)	(0.050)	(0.035)	(0.028)
First-stage F	11798.8	7539.2	8631.0	6186.9
N	1,072,711	1,370,245	1,068,144	1,429,970

Notes: This table reports OLS estimates of intergenerational rank-rank persistence. Father and son outcomes are converted to within-birth-cohort percentile ranks (0–100). Panel A reports OLS estimates from the pooled sample. Panel B instruments the father's pre-patent rank with the father's rank from a second pre-patent census to address classical measurement error, following Ward (2023). The sample in Panel B is restricted to fathers linked to both pre-patent censuses. All specifications include a quadratic in the son's age at the outcome census, birth cohort fixed effects, outcome year fixed effects, and father origin county fixed effects. Standard errors are clustered at the father level. *p<0.10, **p<0.05, ***p<0.01.

Table D2: Intergenerational Mobility Results in Levels

	(1) Ward Score	(2) Occ. Score	(3) Pred. Income	(4) Occ. Wealth
Panel A: OLS				
Homestead	-0.287	-0.431**	0.103	-0.010
	(0.545)	(0.198)	(0.065)	(0.014)
Cash Sale	1.131**	-0.465**	0.159***	0.033***
	(0.453)	(0.193)	(0.053)	(0.010)
Father Score	0.112***	0.074***	0.061***	0.000***
	(0.002)	(0.002)	(0.001)	(0.000)
Father Score × Homestead	-0.044***	-0.052***	-0.022**	-0.000***
	(0.010)	(0.011)	(0.010)	(0.000)
Father Score × Cash Sale	-0.030***	0.009	-0.026***	-0.000***
	(0.009)	(0.012)	(0.008)	(0.000)
N	1,489,865	1,714,840	1,485,151	1,789,249
Panel B: IV				
Homestead	-1.127	-0.575	0.519	0.010
	(1.397)	(1.956)	(0.398)	(0.129)
Cash Sale	-0.829	1.308	-0.456	-0.024
	(1.082)	(1.371)	(0.356)	(0.082)
Father Score	0.464***	0.615***	0.548***	0.000***
	(0.006)	(0.013)	(0.008)	(0.000)
Homestead × Father Score	-0.020	-0.040	-0.085	-0.000
	(0.030)	(0.133)	(0.062)	(0.000)
Cash Sale × Father Score	0.014	-0.078	0.066	0.000
	(0.022)	(0.090)	(0.055)	(0.000)
First-stage *F*	20045.6	4194.0	11175.3	272.4
N	1,072,711	1,370,245	1,068,144	1,429,970

Notes: This table reports level regressions of son outcomes on patent type, controlling for the father's pre-patent score and its interactions with patent type. Unlike the rank-rank specification in the main table, outcomes are in levels rather than percentile ranks. Panel A reports OLS estimates; Panel B instruments the father's pre-patent score with the father's score from a second pre-patent census following Ward (2023). The "Moved" outcome (column 5, Panel A only) indicates whether the son resides in a different county than the father's patent county. All specifications include a quadratic in the son's age at the outcome census, birth cohort fixed effects, outcome year fixed effects, and father origin county fixed effects. Standard errors are clustered at the father level. *p<0.10, **p<0.05, ***p<0.01.

Table D3: Intergenerational Mobility and Age at Patenting

	(1) Ward Score	(2) Occ. Score	(3) Pred. Income	(4) Occ. Wealth
Panel A: Continuous				
Homestead	-2.114***	-1.111***	-0.039***	-0.080***
	(0.438)	(0.217)	(0.008)	(0.020)
Cash Sale	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Homestead × Age at Patent	-0.026**	-0.004	-0.000	0.001
	(0.012)	(0.006)	(0.000)	(0.001)
Cash Sale × Age at Patent	-0.025**	-0.003	0.000	0.001
	(0.012)	(0.006)	(0.000)	(0.001)
Panel B: Binned (Homestead)				
Born After Patent	1.790***	0.480	-0.008	0.013
	(0.689)	(0.326)	(0.012)	(0.031)
Age 0–5 at Patent	1.631**	0.319	-0.011	-0.058*
	(0.726)	(0.352)	(0.013)	(0.033)
Age 6–10 at Patent	0.593	0.182	-0.021*	-0.048
	(0.727)	(0.336)	(0.012)	(0.032)
Age 11–15 at Patent	0.780	0.002	-0.015	-0.022
	(0.725)	(0.343)	(0.012)	(0.034)
Birth cohort FE	✓	✓	✓	✓
Outcome year FE	✓	✓	✓	✓
Father origin county FE	✓	✓	✓	✓

Notes: This table tests whether the intergenerational effects of patenting depend on the child's age when the father received his patent. The sample is restricted to patentee families. Panel A interacts patent type with a continuous measure of the child's age at patent. Panel B bins age at patent into discrete categories; homestead interactions are shown (cash sale analogues omitted for space). The omitted category is children aged 16 and older at the time of the patent. All regressions control for the father's pre-patent ward score and a quadratic in the son's age at the outcome census, with birth cohort, outcome year, and father origin county fixed effects. Standard errors are clustered at the father level. *p<0.10, **p<0.05, ***p<0.01.

Table D4: Intergenerational Mobility by Father's Occupation and Birthplace

	Wardscore		Occscore		Predcited Income		Occupational Wealth	
	Homestead	Cash Sale	Homestead	Cash Sale	Homestead	Cash Sale	Homestead	Cash Sale
By Father's Occupation Class								
Farm Laborer	0.541	-3.201**	0.334	-0.486	0.026	-0.010	0.008	0.041
	(2.976)	(1.560)	(1.651)	(0.585)	(0.044)	(0.026)	(0.132)	(0.075)
Farm Operator	-1.432***	0.372**	-0.580***	0.122	-0.019***	0.006*	-0.036***	0.011
	(0.263)	(0.187)	(0.125)	(0.104)	(0.005)	(0.003)	(0.012)	(0.009)
Skilled	-2.495***	-0.436	-1.611***	-0.636**	-0.048***	-0.011	-0.000	-0.054*
	(0.925)	(0.646)	(0.409)	(0.312)	(0.016)	(0.011)	(0.042)	(0.028)
Unskilled	2.690	0.482	0.641	-0.154	0.046	0.030	-0.050	0.096
	(4.828)	(3.094)	(1.593)	(2.153)	(0.070)	(0.050)	(0.204)	(0.129)
White Collar	-1.912	-1.797**	-2.417***	-0.280	-0.048**	-0.015	-0.049	-0.053
	(1.303)	(0.755)	(0.662)	(0.485)	(0.021)	(0.013)	(0.057)	(0.034)
By Father's Region								
Northeast	-2.660***	0.155	-1.128***	-0.091	-0.033***	0.007	-0.060**	0.012
	(0.629)	(0.500)	(0.260)	(0.233)	(0.009)	(0.007)	(0.026)	(0.019)
Midwest	-2.297***	-0.310	-1.273***	-0.498***	-0.035***	-0.011***	-0.032**	0.015
	(0.331)	(0.210)	(0.166)	(0.125)	(0.006)	(0.004)	(0.015)	(0.010)
South	-1.429***	-0.229	-1.015***	-0.265	-0.027***	-0.002	-0.003	-0.006
	(0.480)	(0.464)	(0.239)	(0.222)	(0.009)	(0.007)	(0.023)	(0.020)
West	-4.094***	-0.290	-2.173***	-0.053	-0.065***	0.005	-0.145***	-0.031
	(0.901)	(0.493)	(0.419)	(0.319)	(0.016)	(0.010)	(0.044)	(0.024)
By Father's Nativity								
Native	-2.410***	-0.304	-1.335***	-0.366***	-0.035***	-0.006*	-0.037***	0.007
	(0.276)	(0.192)	(0.128)	(0.103)	(0.005)	(0.003)	(0.012)	(0.008)
Immigrant	-1.515***	0.331	-0.898***	-0.322	-0.033***	-0.000	-0.032	0.030*
	(0.483)	(0.360)	(0.276)	(0.241)	(0.010)	(0.007)	(0.026)	(0.018)

Notes: Each cell reports the coefficient on the indicated patent type from a separate OLS level regression estimated on the indicated subsample. Subsamples are defined by the father's characteristics at his base-year census: occupation class, Census region, or nativity. All regressions control for the father's pre-patent ward score, a quadratic in the son's age at the outcome census, and include birth cohort, outcome year, and father origin county fixed effects. Standard errors are clustered at the father level. *p<0.10, **p<0.05, ***p<0.01.

Table D5: Intergenerational Results by Decade

	1880→1910	1880→1920	1880→1930	1880→1940	1900→1920	1900→1930	1900→1940	1910→1930	1910→1940
Panel A: Levels (Wardscore)									
Homestead	-2.693***	-1.892***	-2.105***	-2.030**	-2.508***	-2.511***	-3.768***	-1.878***	-2.040***
	(0.391)	(0.399)	(0.442)	(1.011)	(0.733)	(0.749)	(0.784)	(0.571)	(0.595)
Cash Sale	-0.042	-0.104	-0.124	-0.971	-0.188	-0.015	-0.451	-0.491	-0.439
	(0.226)	(0.238)	(0.264)	(0.625)	(0.714)	(0.718)	(0.769)	(0.677)	(0.674)
Panel B: Rank-Rank (Wardscore)									
Father Rank	0.044***	0.041***	0.041***	0.047***	0.029***	0.027***	0.026***	0.044***	0.045***
	(0.001)	(0.001)	(0.001)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Homestead	-1.493***	-1.022**	-0.993*	-2.346	-2.757***	-2.226**	-2.952***	-1.590**	-1.197
	(0.504)	(0.513)	(0.584)	(1.748)	(0.860)	(0.937)	(0.925)	(0.752)	(0.793)
Cash Sale	0.469	0.714**	0.401	-0.609	0.084	-0.127	-0.766	1.571*	0.556
	(0.316)	(0.339)	(0.380)	(1.073)	(0.936)	(0.929)	(0.982)	(0.942)	(0.905)
Home × Rank	-0.020**	-0.016	-0.024*	-0.004	0.023	0.004	-0.012	-0.001	-0.011
	(0.010)	(0.011)	(0.012)	(0.034)	(0.017)	(0.019)	(0.019)	(0.014)	(0.015)
Cash × Rank	-0.020***	-0.029***	-0.018**	-0.004	-0.000	0.006	0.012	-0.039**	-0.021
	(0.007)	(0.007)	(0.008)	(0.025)	(0.019)	(0.019)	(0.020)	(0.018)	(0.018)
Observations	410,071	366,057	296,217	50,420	65,773	64,982	59,210	89,192	87,332

Notes: Each column represents a separate regression estimated on a single base year → outcome year pair, testing whether the intergenerational effects of patenting are stable across cohorts. Panel A reports OLS level regressions of sons' ward scores on patent type. Panel B reports rank-rank persistence estimates with patent type interactions. All regressions include birth cohort and father origin county fixed effects and a quadratic in the son's age at the outcome census. Standard errors are clustered at the father level. *$p<0.10$, **$p<0.05$, ***$p<0.01$.